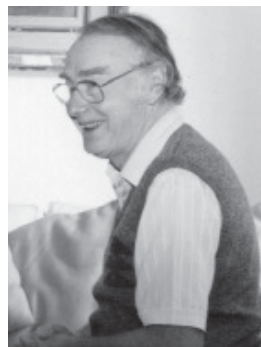


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# Commemorating **Paul Stirling** 25 Years After He Passed Away



edited by

**Emine Onaran İncirlioğlu**  
**Gabriele Rasuly-Paleczek**

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# Emine Onaran İncirlioğlu and Gabriele Rasuly-Paleczek

## Introduction

*I personally hold that truth is better than falsity, and that now – whatever may have been the case in the past – credulity is undesirable. Why do I hold that? I do not know. Just because I am an academic and not a salesman? Perhaps my younger colleagues will disagree, precisely because with the current admiration for images, presentation, virtual reality, and the need to make a commercial success of everything, deception is more important than truth. Somehow, I do not think that that is the point which ‘we’ have reached.*

Paul Stirling, “Credulity,” Unpublished article<sup>1</sup>

Paul Stirling has been an important figure for each of the authors in this collection, albeit in different ways. When Emine Onaran İncirlioğlu decided to put together this volume, Gabriele Rasuly-Paleczek eagerly accepted to be a co-editor and suggested to publish it in 2023 to commemorate the 25<sup>th</sup> year after he passed away. We sought contributions from family, friends and colleagues whose lives Paul had touched, and eventually fourteen of us joined together by submitting papers.

It took us almost three years from the first letter of invitation we wrote to the contributors, to the completion and publication of this online journal. In the process of compiling the book, the table of contents took different forms. Our initial attempt was to organize it in a fourpartite structure: In Part One, we would gather articles that discussed Paul Stirling’s contribution to social anthropology in general and the anthropology of Turkey in particular, including theoretical critiques “a quarter century” after he passed away. Part Two and Part Three would focus on his relations and involvement with the University of Kent and the Middle East Technical University, respectively. We particularly wanted to dwell on the three traces Paul Stirling left at the University of Kent in Canterbury: The Annual Stirling Lectures; the Stirling Library at Marlowe Building; and his Ethnographic Data Archive, “45 Years of the Turkish Village.” Finally, Part Four would be devoted to articles describing “Paul Stirling, the man”.

The process, however, did not develop as we anticipated. We somewhat changed the structure of the volume partly because the multi-layered contributions that we received would not fit into a clear-cut subtitle, and partly because, unfortunately, some of the prospective contributors who submitted their abstracts did not follow up and withdrew from the project for various personal reasons. Some authors took their time and submitted their articles much later than the deadline, and we the editors were also slow to revise and send our remarks. Timing also had an effect on

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<sup>1</sup> [https://era.anthropology.ac.uk/Era\\_Resources/Era/Stirling/Papers/CredulityASA/credulity1\\_1.html](https://era.anthropology.ac.uk/Era_Resources/Era/Stirling/Papers/CredulityASA/credulity1_1.html)

this, as it, first coincided with the pandemic, and then with the disheartening earthquake in South-eastern Turkey on February 6, 2023.

Thus, in the resultant table of contents, Part One combines articles that focus on Paul Stirling at both the University of Kent and the Middle East Technical University, in terms of both his work and his person. In the first article, “Paul Stirling and the Making of Anthropology at Kent,” Roy Ellen discusses Paul’s place in the history of British Social Anthropology, his relations with his contemporaries, and both academic and institutional changes he has introduced at Kent, while noticing his personal qualities, such as his “self-deprecating informality and wry ironic sense of humour.” Krishan Kumar presents a personal account in his article, “Paul Stirling: Chair, Colleague, Friend,” where he provides examples “to illustrate the breadth of Paul’s interests and his openness to areas normally ignored or frowned upon by sociologists. And Sencer Ayata narrates excerpts from his memories as he bridges the two universities in “Paul Stirling as a Supervisor, Colleague and Friend: The UK-Turkey Dimension.”

In their articles in Part Two, Chris Hann, David Shankland, Lale Yalçın-Heckmann and Gabriele Rasuly-Paleczek focus on Paul Stirling’s contribution to the anthropology of Turkey. Chris Hann’s article, “Paul Stirling, the Turkish Nation-State, and the Chimera of Modernity” is a slightly updated version of the Stirling lecture he gave in Canterbury in November 2012 that “retains its title, informal style, and plentiful personal memories.” Discussing the post-Stirling developments during the AKP period, Hann questions the turn Kemalist modernity has taken in Turkey. In “Forty-five years of modernisation in Turkish Villages: What Stirling might have written” David Shankland reflects on Stirling’s longitudinal research in the Kayseri villages, and attempts, as hinted in his title, “to speculate on what the book might have contained, and indeed to assess how close he was to the goal of the account of social change that he hoped to produce.” Paul Stirling’s seminal work *Turkish Village* was instrumental in Lale Yalçın-Heckmann’s contribution. In her article “Is There a Kurdish Village? Thinking with Stirling and Reflecting on Kurdish Rural Settlements of the Last Century,” based on her study of Kurdish rural settlements and tribal relations, she traces “Stirling’s work to re-construct and re-assess” what she has learned from him and where their “main differences lie”, as well as questioning “such denotations as ‘Turkish’ or ‘Kurdish’ while identifying villages.” Gabriele Rasuly-Paleczek’s, “Turkish Village - a blind spot in contemporary field research: What’s going on in rural Turkey today?” offers a brief overview of the social science studies that have dealt with the dramatic transformation processes in rural Turkey since the death of Paul Stirling. In this regard, she advocates a resumption of detailed ethnographies, as Paul Stirling has done in the past.

Similarly in Part Three, although Bahattin Akşit and Kayhan Delibaş have contributed “academic” articles in their research areas pertaining to the study of social change in Turkey, both included Paul Stirling’s initial work, either explicitly or implied between the lines. Akşit, compares his findings in the research project “Paths of Rural Transformation and Seasonal Migration” in which he had taken part, with that of Stirling’s, in “Rural/Agricultural Transformations and/or Reproductions in Turkey: Thinking with / against Paul Stirling,” in terms of land ownership, the quality and quantity of land cultivated, changing gender relations and marriage patterns, among various other aspects. And Delibaş, in “The Driving Factors of Turkey’s Foreign Policy Shifts from West to East: Turkish Politics and Society after Paul Stirling,” examines “the multidimensional change Turkey has witnessed under the political Islamist government and its impact on relations with the Western world.” Both authors use

qualitative methods such as participant observation and face-to-face interviews, in addition to “macro level” analyses.

Part Four, “Paul Stirling as A Supervisor” includes two articles, namely “Ethnography and the History of Violence in the Spanish Civil War” by John Corbin, and “When the Anthropology of Women was Revolutionary – A Memoir,” by Nancy Lindisfarne-Tapper neither of which, strictly speaking, are from Paul Stirling’s research areas. Both authors, however, benefited from Paul’s advice, Corbin as his supervisee first at the London School of Economics, then at the University of Kent, while Lindisfarne met Stirling when he acted as her external examiner for her M. Phil at the School of Oriental and African Studies, then as a “a kind and generous mentor.” Lindisfarne also recounts Stirling’s visit during her and Richard Tapper’s field research in Turkey.

Part Five is devoted to three articles that locate Paul Stirling in the two Kayseri villages, Sakaltutan and Elbaşı. Hülya Demirdirek met Stirling as her professor at the Middle East Technical University where she was a student of sociology. In her article “From ‘Prof. Stirling’ to ‘Paul’” she traces her memories with him as they relate in different capacities. For Abdulvahap Taştan, as a young academic appointed by the Erciyes University, Paul was a “foreign” scholar who had to be monitored in the field—under the chain of command, so to speak, of the Turkish government. However, in his article, “At the Field with Paul Stirling: Through the eyes of his *mihmandar*,” Taştan relates their interactions at different layers beyond his “official” duty. Finally, Emine Onaran İncirlioğlu, in “Paul Stirling, the man: Pink shirt, missing toe and deep wells of depression,” narrates Paul Stirling’s unique role in her life as he treated her as his equal and introduced her as his “research collaborator” in spite of her junior position.

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No matter how Paul Stirling was related to each of us authors – whether we were his students, supervisees, colleagues, friends or any combination of these roles – he was very much concerned about taking responsibility in our written and oral communication. He, himself, was not a person of monologues, or dialogues of the deaf. Relating to and being in harmony with the people he addressed, and speaking the same language with them was extremely important for him. What he had written in 1986 in the introduction of a paper, was typical of him: “I have a list of general things that I want to say. But can I get them clear? Do others want to read them?”<sup>2</sup>

As Shankland stated in his article in this volume, we all agree that “clarity of style” was a “remarkable characteristic of Stirling’s work.” Corbin, as his student, noted that, although “Paul had no special interest in conflict and violence in Spain,” which was Corbin’s research topic, he “did care strongly about language and would promptly return drafts with comments and suggested rephrasing.” His marks in green ballpoint pen were famous. Following Paul Stirling’s footsteps, we were concerned about assembling this volume, yet ironically, editing it fell to two non-native speakers of English. We did try to get help from a native speaker, but for various reasons, it didn’t work out.

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<sup>2</sup> From the first draft of paper for ASA Migration Conference, April 1986, “Migration: Mixing Things Up.” İncirlioğlu, personal collection.

We cannot pass without mentioning and thanking our friends who supported us during these three years. First of all, we are grateful for all the contributors who accepted to submit papers that make up the volume, and diligently followed up through the editing process. Hülya Demirdirek, Vincent Nunney, Rita Bowles and Stuart Duckward read parts of the text and made comments, for which we are indebted. Special thanks go to Alper Yurtseven who was kind enough to design the cover of the ASSA special issue, and to Catharine Stirling, who provided us with some of her parents' photographs that enrich the collage on the cover. We would also like to thank Patricia Zuckerhut and Brigitte Fuchs – the editors of the “Austrian Studies in Social Anthropology” for agreeing to include this volume as a special issue of the ASSA Journal.

There is no conclusion to this volume; we gave the last word to Paul Stirling himself, under the subtitle “no conclusion” and included the “skeleton for a talk” that he gave in February 1980 in the Social Anthropology Research Training Seminar, on “Writing a thesis”.

# **Part One**

**Paul Stirling at the University of Kent  
and  
Middle East Technical University**



## Roy Ellen

### Paul Stirling and The Making of Anthropology at Kent

#### Introduction

Under the aegis of Sir Edward Evans-Pritchard, Paul Stirling was in that group of post-war social anthropologists which first undertook serious fieldwork in the Mediterranean basin. The theoretical significance of this for anthropology was to be radical, all his peers having been – to use the words of John Davis (1977: 241) – “pitched into fields which had histories”. His monograph, *Turkish Village* (published in 1965) was a model of lucidity, and was to revolutionise the study of the Turkish countryside. In turn, Stirling was to contribute to the establishment of Mediterranean anthropology in the next generation, supervising the research of the philosopher and anthropologist Ernest Gellner, John Davis (later to become Warden of All Souls College Oxford), John and Marie Corbin, Margaret Kenna, Nevill Colclough, amongst others. He was an influential figure for young Turkish social scientists, of whom he trained a good many to PhD level,<sup>1</sup> and was Visiting Professor at the Middle East Technical University in Ankara between 1983 and 1986. Stirling read philosophy and classics at Oxford and went on to study social anthropology there with Evans-Pritchard. After defending his doctorate, he moved to the LSE to work with Raymond Firth, and in 1965 was appointed to a foundation professorship at the then new University of Kent at Canterbury. He remained at Kent beyond his official retirement in 1984 until his death.

Although we now have the Paul Stirling archive (in part lodged at the Royal Anthropological Institute, and virtually at <http://era.anthropology.ac.uk>), apart from the obituaries written in haste around the time of his death, there is little considered appreciation, correspondence or recorded interviews that might shed light in significant detail on his early intellectual life, and so the account presented here of how Stirling’s appointment to Kent shaped the way anthropology developed there has been pieced together from references in biographical and autobiographical accounts of others (see e.g. Anon 1987, Ellen 1998, Hann 1988, Kumar 2011, Stirling 1995) and from personal recollections (Kumar 2020). Paul was not a person you would expect to spend much time curating his back catalogue of ephemera.

#### Stirling and British Social Anthropology before Kent

After graduating in philosophy and classics at Oxford, although Stirling was single-minded in his determination to become an anthropologist, he felt that he was initiated into subject by Evans-Pritchard with immoderate haste. By April 1949 he was in Turkey, on his way to Kayseri and the villages of Sakultatan and Elbaşı. He felt little prepared for what he was to encounter

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<sup>1</sup> 1970 Altan Eserpek, 1982 Sencer Ayata, 1984 Ayşe Güneş-Ayata, 1986 Yıldız Ecevit, 1995 Hediye Sibel Kalaycıoğlu. In addition, Kayhan Delibaş worked as his research assistant but was supervised by Frank Furedi (2001), later working with Michael Fischer on the Paul Stirling archive.

in the field, and much concerned about the adequacy of his Turkish language skills (Shankland 2011: 18), as indeed he remained in later life. Nevertheless, he finished his PhD in 1951 within the time schedule stipulated by Oxford expectations of this period, and returned to Turkey with his young wife Margaret. While at Oxford he had amongst his fellow research students John Barnes, Clyde Mitchell, John Peristiany, Freddie Bailey and Mary Tew (Douglas), names that would recur during the Kent years because of a common background and shared interests.

In January 1952 Paul Stirling joined the anthropology teaching staff of the London School of Economics. He was to stay there for 12 years. In his personal recollections of the LSE, Stirling tended to emphasise the lowliness of his status there and the hierarchy of staff relations in the Department of Anthropology: a pyramid with Raymond Firth at the top. A story he repeated numerous times concerned his role in the production process of the LSE Social Anthropology monograph series before this was taken over by Athlone. His role was to wrap-up monographs in brown paper for distribution under the supervision of Firth, though Paul was later to go on to edit and manage the series himself (Firth 1966: 8). Coming from a very different angle, Paul's young colleague Robin (J.R.) Fox more overtly revealed his discomfort with the formalities of social relationships within the department, where "chaps" would call each other with false modesty 'Fox', 'Stirling' or 'Freedman' with its implicit nod to hierarchy and misogyny. Stirling was later also fond of pricking the pomposity of these senior colleagues, and deflating the intellectual status of those in his own peer group who would later become famous, such as Mary Douglas. But Paul's disdain for others was reciprocated in the reputation he himself acquired at the LSE for being slow to form an opinion. According to Robin Fox (2017: 299) ... "Paul Stirling was in a constant dither about whether to call the feuding units in his Turkish village 'lineages'," or not. However, in his defence, it is very clear from his published interview with David Shankland that the character of the Anatolian "lineage" was something that intellectually puzzled him, as did what constituted "groups", or why Radcliffe-Brown should insist that it was always the reality of "structure" that mattered. He remained highly sceptical of the generalisations made by sociologists of the X to Y kind, mainly because he always saw the heterogeneity in his own data (Shankland 2011: 11, 21). From philosophy, Stirling had acquired not only a concern for precise definitions regarding the meaning of analytical concepts, but also a passionate interest in causality and the role of knowledge in social systems; from Evans-Pritchard he had learned the importance of history. His insistence that one should seek to identify causal links, and his acknowledgement of the formidable problems in doing so, made him equally sceptical of simple-minded scientism and post-modern approaches. Although he was, in essence, a proponent of interdisciplinarity and "the project of social science", he was quick to recognise the limitations of both – as we shall see.

Apart from the formal relationships Stirling established with his teachers, two crucial and enduring associations formed during the LSE period. The first was with Ernest Gellner. Unlike his relationship with other LSE colleagues this cut across the formal hierarchy. Gellner had made friends with Paul, who introduced him to social anthropology at the LSE. Here Gellner "found something of a home" (Hall 2003: 155) as a doctoral student, working on the Berbers of Morocco under the joint supervision of Paul and Raymond Firth. What appears to have appealed to Gellner about this arrangement was that it did not require him to split his sensibilities as a philosopher from those of being an anthropologist (Hall 2003: 160). The shared regard Stirling and Gellner had for both subjects does much to explain the way in which social anthropology took shape at Kent. In an interview with John Davis (1991: 66), Gellner stresses his indebtedness to Paul and reflects on his relationship with the two subjects:

And the paradox, the joke, is, having escaped from philosophy to anthropology partly, certainly not totally but partly, to escape from linguistic philosophy, I find in my old age that the thing I was escaping from is now almost dominating anthropology: the hermeneutic plague, as I call it, which is partly inspired by Wittgenstein.

Both Gellner and Stirling shared a robust antagonism towards linguistic philosophy in particular following their Oxford encounters as undergraduates. A little later in the same interview, we find the following reminiscence:

I remember a seminar at which Somalis and sanctity were discussed and someone present said, “It’s interesting that there seems to be much more inwardness about Somali Islam than about Berber Islam”, and Paul Stirling said, “It’s nothing to do with Somalis, it’s to do with the fact that the informant about Somalis is [B. W.] Andrzejewski and not Gellner”, and this I think is true. (Davis 1991: 71)

The other key relationship was with John Davis. When Davis moved to the LSE to become a graduate student, he met three individuals who were to mould his enduring preoccupations: Raymond Firth, Lucy Mair and Paul Stirling, who as we have already noted was at the time undertaking pioneer work in Mediterranean ethnography. In addition to his Turkish work, which continued over the years and developed into a classic longitudinal case study, Paul had also been working in southern Italy on land reform. Under Stirling's supervision Davis also undertook fieldwork there, while Mair was later to oversee the writing-up. Through an introduction from Paul, John was adopted and mentored by Manlio Rossi-Doria, an economist in Naples and Italian senator with considerable clout in cultural circles there (Dresch and Ellen 2018: 123). Paul found John a job as research assistant, which was better than the alternative of working part-time for the Post Office to make ends meet. John had been ill, but got a letter from Stirling:

It said in effect that he had no respect for people from Oxford since they thought themselves far too clever; he had no high expectations of anyone who had no training in anthropology; he didn’t really want to employ anyone who had only a second-class degree – but he needed someone who could speak Italian to be his research assistant, and he was (typical Paul Stirling) anxious to help someone who was sick and hospitalised. (J. Davis, Memoir A, p. 8. Quoted in Dresch and Ellen 2018: 124)

So, when Davis embarked on his anthropological work in Italy in 1963 it was with a six-month stint as Paul Stirling’s research assistant. Stirling, whom Davis liked a great deal, was hugely important to his subsequent progress. This was a decade in which the Mediterranean drew interest from anthropologists elsewhere. Monographs such as Julian Pitt-Rivers’ *People of the Sierra* (1954) had forced English-speaking anthropologists to pay attention. Although Stirling had worked in Turkey in the late 1940s, he was slow to publish: his *Turkish Village* would still have been in draft, although close to finished, when John was his assistant (Dresch and Ellen 2018: 127).

John and Paul became distant in later life, after Paul’s retirement and on John’s move from Kent to Oxford. While John studiously sought to impress and had an impeccable hand-writing generally executed with a fountain pen, Paul was scruffy, his hand-writing was barely readable and during his later years he produced endless illegible rough notes on scraps of paper. John came to be less tolerant of Stirling’s weaknesses, and to wonder more openly about his strengths. Typical was his imagining of an eventual Stirling festschrift or perhaps a work of

reminiscences which – he cruelly suggested – might be entitled “Doubt and Delay”. In a lengthy note, written in later life, Davis says much about the virtues of his old mentor and patron: “And yet he never made it to the top: he was not FBA; he was not an officer of the ASA or RAI, not of any international academic organisation, apart ... from an Anglo-Turkish association of one kind or another” (J. Davis, *Memoir B*, p. 40: quoted in Dresch and Ellen 2018: 138). This “getting to the top” plainly interested John far more than it had Paul. In 1989 John applied to Oxford, and then became President of the Royal Anthropological Institute. In short, he accomplished all the things he felt Paul failed to achieve but which Stirling himself probably thought irrelevant (Dresch and Ellen 2018: 142).

### **The founding of the University of Kent and the expansion of social science in the UK**

Kent was one of the ‘new universities’ founded during the nineteen-sixties in the United Kingdom, the so-called “plate-glass” universities rather than the “civic” or “red-brick” universities that had emerged to counter the dominance of Oxbridge. These included Sussex (1961), York and the University of East Anglia (1963), Lancaster (1964), Kent, Essex and Warwick (1965), Stirling (1967) and Ulster (1968). Strathclyde, Heriot-Watt and Dundee were to follow later. The red-brick universities had been parodied in the novels of Kingsley Amis and David Lodge, and in turn the plate-glass universities were to be satirised by Malcolm Bradbury. The redbrick universities were often accused of failure in a number of key areas: curricula were over-specialised, their departments were inflexible silos, they lacked a sense of community and suffered from poor staff-student relations (see e.g., Collini 2021). This was very much an Oxbridge view. The new plate-glass universities were part of a Labour government attempt to implement the recommendations of the Robbins Report, the expansion of UK higher education being in part a response to the post-war “baby boom”. However, at the same time, it was an opportunity to innovate new subjects and teaching methodologies. As part of this mid-1960s growth spurt in UK university provision, sociology and the social sciences in general were particularly favoured, although except in a few existing institutions (such as the LSE) sociology itself was still struggling to find an academic foothold.

In 1945 LSE was the only British university with a Department of Sociology, and was a principal source of trained sociologists. Anthropology pre-existed and was seen by some to occupy the intellectual and pedagogic space that might otherwise be reserved for sociology, and especially within the older universities of Oxford and Cambridge there had long been opposition to its expansion. However, the end of the Second World War, the national political mood for social engineering and radical reform, changed all that, and departments began to appear at Leeds, Leicester, Birmingham, Liverpool and Hull, with staff many of which had been trained at the LSE (Albrow 1989: 202).

Both anthropology and sociology were small subjects, and anthropologists and sociologists often had complex identities at this time, associating equally with both subjects depending on the context. Raymond Firth and Meyer Fortes had sponsored the formation of the British Sociological Association (BSA) in 1950 (Mills 2011: 63), while Paul Stirling later became a member of its education committee. Lucy Mair and Maurice Freedman were for a long time on the editorial board of the *British Journal of Sociology*, while Ronald Frankenburg for many years edited the *Sociological Review*. Firth also had a role in establishing the Social Science Research Council in 1965, and both he and Max Gluckman sought interdisciplinary dialogue

and looked to cooperate with other social sciences (Frankenberg 1988: 108). To some extent, and for some purposes, the subjects were accepted as interchangeable and overlapping. Michael Banton, for example, had been trained in LSE anthropology but went to a chair in sociology at Bristol. Indeed, in addition to his prominent role in editing the first four monographs of the Association of Social Anthropologists (ASA) Banton later became president of the Royal Anthropological Institute. Joint departments of Sociology and Anthropology were established, for example at the School of Oriental and African Studies, employing individuals such as John Peel who was equally at home in both subjects. At Manchester, Max Gluckman had sought to form a joint department with himself holding the chair in social anthropology, while appointing Peter Worsley (another anthropologist) to a chair in sociology. This arrangement was not to last and the department eventually split into its two component subjects (Mills 2011: 93, 106).

Despite – perhaps also because of – this disciplinary fluidity and the pressure at government level to see sociology expand as the more relevant subject for British social policy, some anthropologists were anxious that their separate identity should not be lost. This was especially because within the existing anthropology departments and learned societies, the subset “social anthropology” was only now beginning to coalesce in distinction to physical anthropology, museum studies of material culture and prehistory. The Association of Social Anthropologists had just been established in 1946. As David Mills (2011: 17-18, 26) reminds us, “British” social anthropology in the 1950s was very much “an imagined scholarly community” and its practitioners were in several minds as to what best characterised its theory and practice (Gluckman and Eggan 1965). Indeed, distinct disciplines within the social sciences more widely had barely emerged when there was anxiety about their boundaries.

Kent took up the challenge to develop sociology (along with places like York, Sussex, Essex and Stirling), but at the same time sought to “liberate the subject silos” (Pellew and Taylor 2020: 4), to be radical, utopian and hybrid. In contrast to most other places, Kent adopted a collegiate system along Oxbridge lines, which in part no doubt hankered to the academic roots and sentiments of those who initially drew up the plans. This had some curious and reactionary consequences. When senior members of Eliot College held its first “high table” – arranged on a raised dais in front of a picture window giving views of Canterbury Cathedral – junior members sitting at tables in the main body of the dining hall stood up without prompting in deference to the procession of senior members. Traditions like this were instantly invented. So, although Kent sought to be part of a wider experiment in higher education, the attempt to mimic Oxbridge in customary behaviour and even architecture, reinforced some very conservative values. It was highly convenient that the college system could be intellectually justified by claiming that the cross-currents between subjects possible through multi-subject colleges (with their shared accommodation, shared dining, and junior and senior common rooms) were a fertile breeding ground for modern interdisciplinarity. And there is little doubt that it provided an environment that was simultaneously familiar and invigoratingly new, where anything seemed possible, where freedom to innovate was positively encouraged. Kent was later to modify the collegiate system, as financial problems kicked-in. With duplicated infrastructures, such as dining-halls and common rooms, colleges were expensive to maintain. Although it subsequently moved to a more conventional academic structure (“subject concentration” and later departmentalisation) interdisciplinarity remained a stated objective and had been important to Paul Stirling in his interpretation of his role and the development of teaching and research. Paul, no doubt at the time, but certainly much later, could see the irony in all of this.

## The beginnings of anthropology at Kent

Anthropology arrived at the University of Kent with the appointment of Paul Stirling as Professor of Sociology in 1965. After a brief sabbatical he started work in April that year, among other things appointing three other members of staff before the first students arrived in September (Shankland 2011: 13). This first batch included Nevill Colclough (like John Davis, an Italianist), Ray Pahl and Derek Allcorn. John Davis joined a year later. They were part of a group of other LSE staff and students who were to form the nucleus of a board of studies, later to become the Department of Sociology and Social Anthropology. From his own recollections, it sounds as though during this phase at least Paul did not much relish the challenge of establishing a subject and courses from scratch, given the absence of the essential resources for so doing (such as teachers and books). This goes against the conventional wisdom harking back to the halcyon days of the “new universities” when it is assumed a group of new institutions at the “peak of their confidence” between 1961 and 1967 shared in the exhilaration of common purpose. Stirling’s dislike of the turgid LSE formality and hierarchy no doubt influenced his style at Kent, where he adopted a self-deprecating informality and wry ironic sense of humour.

Whereas the older universities had been slow to establish sociology, senior anthropologists within these same institutions (such as Raymond Firth) had also been reluctant to endorse the establishment of anthropology elsewhere. This was on the grounds that social anthropology was essentially a graduate subject, because it was considered (an elitist view) that expansion might somehow sully what was best in the subject, and because it was thought that the perceived demand could be met within those universities where it was already found. However, a shortage of senior sociologists to occupy advertised positions in the new universities, and an apparent surplus of well-qualified social anthropologists, led to the appointment of a series of anthropologists as professors of sociology: Max Marwick in Stirling, Peter Worsley in Manchester, Ronnie Frankenburg at Keele, Jaap van Velsen in Aberystwyth, and Bill Epstein and Freddie Bailey at Sussex. John Barnes went to Cambridge and Paul Stirling to Kent. On Stirling’s own admission Kent had difficulty finding a sociologist, and the first resigned after only a few months, leaving a way for his own appointment (Shankland 2011: 13). Moreover, it has been suggested that the position as head of sociology at Kent had first been offered to Robin (J.R.) Fox, who is supposed to have turned-down the position. Had he accepted, Kent anthropology might have been a very different creature than it subsequently became. In the late 1960s Fox was developing new courses at the LSE that drew strongly on animal ethology, Darwinism and human evolution, involving Michael Day and John Napier. He eventually left LSE for a post at Rutgers, where he continued to develop his style of evolutionary anthropology that anticipated later developments more generally in British anthropology, and which were to influence developments at Kent in the late 1990s and during the two decades of the 21st century.

The movement between anthropology and sociology was much in keeping with the ethos of British social anthropology at the time, in defining itself as that branch of sociology that specialized in non-industrial societies, while maintaining its distinctiveness in terms of methodology. Lucy Mair (1972: 1) in her *Introduction to Social Anthropology* had said quite explicitly that social anthropology was “a branch of sociology”, and at the LSE anthropology was long taught as an optional pathway in the BSc Sociology. There was also provision for social anthropology within a University of London intercollegiate degree in Anthropology, but within LSE it could also be taken as an option within the BSc (Economics) programme. It was therefore able to advertise its social science credentials, and demonstrated how the subjects might be integrated (Firth 1966: 6). The kind of social anthropology introduced by Paul to Kent

was therefore perfectly compatible with a particular strand of late 1960s sociology: comparative social systems with a main regional focus on the complex societies of southern Europe and the Mediterranean, and overlapping areas of pertinent theory and methodology. His work on Turkey and later Italy had been a core part of the post-war expansion in the social anthropology of complex societies with long histories. Both John Davis and Nevill Colclough had first degrees in history, and together with a researcher (Robert Rowlands) and a group of doctoral students (John Corbin, Paul and Barbara Littlewood, Margaret Kenna and Michael Lineton), were part of this ethnographic vanguard, later joined by Marie Corbin. Thus, Paul Stirling was remarkably successful in the manner in which he engineered an accommodation between sociology and social anthropology at Kent, and subsequently permitted both to flourish. One mark of this success was that by the end of the 1960s all of the first cohort of Kent social anthropology research students and researchers occupied positions as social anthropologists in mixed departments.

Stirling, as we have noted, had been trained by Firth and therefore had no direct connection with a broader holistic anthropology and was now predisposed to the “social science projects” of the LSE. However, the crisis of identity in British Social Anthropology was closely linked to the position of anthropology in general education, and as early as December 1964 Paul had taken the lead in organising a Wenner-Gren funded conference at the LSE on “The place of anthropology in general education”. Moreover, there were persistent arguments about who was qualified to be admitted to the Association of Social Anthropologists: should ASA membership be restricted to those trained as social anthropologists in the British (and by extension Commonwealth, i.e., colonial) tradition, or might the Association be opened up to the Americans? The debate was partly about disciplinary stewardship versus popularisation, which Paul memorably characterised as the “mandarin versus missionary positions”: should social anthropology be only for professionals or for the masses (Mills 2011: 150, 167)? It was argued, amongst other things, that social anthropology needed to be represented in teaching other than as a part of sociology. This activity was followed by the establishment of a ‘Committee on Anthropology in General Education’ which Stirling chaired, and which advocated close cooperation with the BSA. These were concerns that Stirling took with him to Kent, and which undoubtedly influenced the way in which he was to envisage developing the subject.

Meanwhile at Kent the Stirling group formed common purpose with the sociologists, some of whom had a specific interest and respect for anthropology: Ray Pahl (Cambridge then LSE, appointed 1965), Frank Parkin (appointed 1965, who had been taught anthropology by Paul at the LSE) and Derek Allcorn (Cambridge anthropology, PhD from Manchester, appointed 1966). All collaborated in the joint teaching of social theory. A good proportion of these new sociology members of staff had LSE connections even if they had no previous training in anthropology, for example Krishan Kumar (Political sociology at LSE, appointed Kent 1967) and Mary Evans (politics at LSE). Pahl and Davis were later to share an interest in the study of the informal economy of the UK. The courses taught at this time were part of integrated sociology degrees, with courses such as “non-industrial societies” and “peasant societies” being convened by the anthropologists. The links between social anthropology and sociology were also fostered through the appointment of external undergraduate examiners who had to demonstrate competence in both subjects. As a result, during this period Kent established close links with John Barnes, C.W. Williams, J. Clyde Mitchell, Joe Loudon and others, all teaching in sociology departments. Marie Corbin worked on research projects with both Ray Pahl and Derek Allcorn between 1968 and 1970, and both John and Marie Corbin had office space in the newly established CRISS, the Centre for Research in the Social Sciences. From the

beginning research students were part of the mix, some having transferred from the LSE. Some of the early PhD theses were registered as “Sociology”. By 1974, however, Kent was conferring social anthropology PhDs on candidates who had been registered as sociology students, the first of whom was John Corbin.

This early period, from 1966 through to about 1970, was one of intellectual fluidity and innovation at Kent. George Homans was a visiting professor during this period, and Marshall Sahlins spent time there during his Parisian sojourn, while he was working on themes that were to be published as *Stone Age Economics*. The transfer from LSE of a professor, two lecturers and a group of four PhD students all specialising in the Mediterranean immediately established Kent as a centre of research excellence in this field. This is a reputation it has maintained, though the actual numbers of Mediterraneanists have much fluctuated, and at one time seemed on course for extinction. A similar group at Sussex, specialising in the anthropology of Europe, under the tutelage of Freddy Bailey, were frequent guests at Kent seminars, and there early developed a fruitful “Kent-Sussex axis”. By 1966 Mediterranean anthropology was in full flow, still a newcomer to the discipline, bolstered by Stirling, John Davis, Pitt-Rivers, Freddy Bailey, Michael Kenny, Emrys Peters and John Peristiany. As Dresch and Ellen (2018: 125) put it, “Peristiany’s conferences in Athens were the highlight of the academic year for Mediterranean anthropologists, but Kent became a central place, and Mediterranean anthropology a kind of travelling house party”, a road show moving between dinners and social conferences in different evocative locations. In a tribute to John Davis, Michael Gilsenan observed how there was:

a real intellectual flowering and a kind of collective endeavour. Kent was the perfect setting ... Kent formed a kind of hub.

I don’t think that I have ever quite recaptured the atmosphere and stimulation of those conversations, meetings and conferences in Kent, Rome, Zaragoza, Galicia, so many places. Ernest Gellner added enormously to the engagements and felt a real link with Kent, Sydel Silverman and Eric Wolf became friends. Provocation, argument, and lots of food and drink, those were the rules. (M. Gilsenan, Memorial address, Oxford, 24 June 2017, p. 10: quoted in Dresch and Ellen 2018: 125)

The “hub” about which Gilsenan enthuses was arguably the initial creation of Stirling at Kent and Bailey at Sussex, connecting their respective Mediterraneanist and Europeanist research groups, but the organisational energy and zest for hospitality provided by John Davis made it work all the better. All told, the period initiated in 1965 at Kent might be said to represent the heyday of Mediterraneanist anthropology.

This then was the organisational and intellectual core, but developments in other parts of the Faculty of Social Sciences at Kent were to augment this in interesting ways. John Kesby was appointed in 1971, having completed his D.Phil with Edwin Ardener at Oxford and having served three years teaching in Cambridge, for Kings’ and Newnham colleges. Kesby was the first appointment of an anthropologist (though still as a Lecturer in Sociology) who had experience of working in a tribal society, and in Africa, and who was not a Mediterraneanist. Having retired from the LSE several years previously, Lucy Mair also joined the team in 1971, teaching political anthropology, which she continued to do until her “second retirement” in 1980, largely co-teaching with first Davis and then Roy Ellen. It was partly in honour of “Lucy” that the Anthropology computing server was so named in 1986.



The decision by the Faculty of Social Sciences to develop interdisciplinary studies, consistent with the vision of the founding fathers, brought Henry Bernstein, a Marxist sociologist who had worked on Mau-Mau in Kenya. Another decision, to establish Southeast Asian studies, saw the arrival in 1971 of Jeremy Kemp. Kemp was another product of LSE, who specialised in rural social organisation in Thailand. Southeast Asian studies had been formally established several years earlier with the appointment of Dennis Duncanson, a Vietnam counter-insurgency expert, on the recommendation of Maurice Freedman, who had succeeded Firth as head of the Anthropology department at the LSE. This was in line with – though not funded by – the UK government Hayter scheme for strengthening regional studies. Bernstein and Kemp held half positions in sociology in addition to their specialist areas.

Not only was there a strong ethos of interdisciplinarity, there was also a great deal of cross-teaching which facilitated friendships with non-anthropologists. Although, to begin with, the anthropologists taught sociology courses that included components on non-industrial societies, the Board of Studies in Sociology (first established in 1971) began to teach separate social anthropology courses in 1972: Social Anthropology 1 (political and economic anthropology) and Social Anthropology 2 (ritual and belief). The Board officially became a Board of Studies in Sociology and Social Anthropology (BOSSA) in 1973, and this coincided with the creation of specific degrees and courses in Social Anthropology and the establishment of more posts. Stirling initially chaired the BOSSA, followed by Nevill Colclough and John Davis, until Richard Scase took over in 1989. Given that the structure of the university gave these boards little strategic power or ability to allocate resources, the position of chairperson was widely regarded as a chore that research active senior members of staff might wish to avoid.

## Consolidation

Roy Ellen was appointed in 1973, Nanneke Redclift in 1974 and Jerry Eades in 1976, all to positions in social anthropology rather than sociology. The ethos of the founding fathers at Kent had been to form broad faculties as administrative and budgetary units. Within these were loose “Boards of Study” which organised teaching, and with staff distributed across the colleges in an interdisciplinary mix – each “a microcosm of the whole University” (Martin 1990: 130). With the expansion of Kent this became increasingly inconvenient, and Paul Stirling with the help of its then Master, Alec Whitehouse, engineered the gradual transfer of sociology and anthropology staff to Eliot College from the other colleges. The operative phrase was “subject concentration”.

These years were the golden period of cross-subject collaboration. Specific courses were designed and co-taught in research methods (with Ellen, Pahl and later Will Tyler, Marie Corbin and Nadia Lovell), urban anthropology and sociology (with Eades, Pahl, and Chris Pickvance), medical anthropology and sociology (with John Corbin and David Morgan) and development (Bernstein with various anthropologists). John Jervis, a sociologist whose postgraduate research had been on structuralism, regularly taught the course on ritual and belief during the late 1970s and 1980s. The Board of Studies also pioneered bridge courses with history (Davis, Colclough and Andrew Butcher) and with philosophy – “Understanding other cultures” (Davis, Anne Sellars and John Bousfield, but the brainchild of Paul Stirling). At the same time, the permissive teaching framework allowed for specialist curiosities such as John Davis’s “l’Année Sociologique” (required reading: *l’Année Sociologique*). Major research initiatives such as Ray Pahl’s project on the informal economy of Sheppey brought together

both anthropologists and sociologists (such as Nikki Goward and Claire Wallace), and the group displayed substantial evidence of serious scholarship and research across its specialist interests.

There was also time for levity. The intellectual gravity of all those efforts in subject-building were tempered by the occasional joint research seminar where colleagues were entertained by Derek Allcorn presenting advanced re-interpretations of Marx's *A contribution to the Critique of Political Economy* in which "small-holding peasants" effortlessly became "Small-minded pedants", while Ernest Gellner, then editor of the *European Journal of Sociology*, could hold the press deadline to accommodate a faux-Marxisante analysis of Beatrix Potter's *Peter Rabbit and the Grundrisse*, by "Rosa and Charlie Parkin", for which it was claimed there could be no such thing as "an innocent reading". Frank Parkin, Rosa and Charlie's father, was later to draw on a caricature of the Kent anthropology scene when writing his comic novel *Krippendorf's Tribe* (1985), subsequently turned into a film, a story that incorporates characters loosely based on some of his erstwhile colleagues. John Davis also devised the simulation game "Potlatch" during the 1980s which instructively amused several cohorts of students. It was widely accepted at this time that Kent comprised a happy crew, had established a distinctive style, and had in some small way "made interdisciplinarity work". This was largely down to Paul's relaxed, generous and intelligent management style.

The retrenchment of the 1980s and the Thatcher years brought both advantages and disadvantages for anthropology at Kent. The Centre for Southeast Asian studies closed in 1988. This was largely due to the drop in the number of students wishing to study the subject, and withdrawal of government and university support as strategic interest in the region as a site of Cold War confrontation receded. Closure of the Centre brought Kemp wholly within the anthropology group, which also extended a welcome to Bill Watson. Watson had been appointed to Southeast Asian Studies in 1981 on the strength of his cross-disciplinary credentials, and an interest in history and literature, but thereafter brought these skills into anthropology. Meanwhile, cuts being made elsewhere in the university sector were to be of benefit. Kent anthropology performed well in the first national Research Assessment Exercise (RAE), which allowed it to successfully bid for staff transfers under the Thatcherite rationalisation programme. This led in 1989 to the return of John and Marie Corbin from the University of East Anglia. John transferred into a permanent position, while Marie chose a part-time option. Both sides of the Board of Studies expanded despite retrenchment because of their excellence in research, and to that extent the historic accommodation between social anthropology and sociology set in motion and overseen by Paul Stirling in the early years was still seen to be operating.

Another major development during these years was the establishment of the Centre for Computing and Social Anthropology (CSAC) in 1986 by John Davis, which for a while placed Kent at the forefront of innovations in computing applications that have now become standard throughout academia. Paul had for some time been interested in the potential of computing, but always reckoned that his own use of computers had been "a mess" (Shankland 2011: 20). Kent Anthropology had early (1969-72) initiated ground-breaking work in the area of computer applications, in an SSRC funded project "Computer applications to the analysis of local census materials in Southern Spain", in which Marie Corbin and Paul Stirling on the back of an SSRC grant had used the facilities of the Atlas Computer Centre at Didcot to reconstruct family and kinship data from census records. A decade later John Davis spent a year in Berkeley where he had been much impressed by the work of the Language Behavior Laboratory under the direction of Brent Berlin. He followed through this interest by appointing Michael Fischer in

1985 as Lecturer in Computing and Anthropology, tasked with putting Kent anthropology on the map of anthropological IT. During this period Kent Anthropology pioneered applications, both within the university and more broadly, that were later to become commonplace (bibliographic databases, email communication, text production) as well as specialist programs for handling kinship data. The first internet service began in November 1986 (three years before Tim Berners-Lee's invention of the World Wide Web), and the first web site was launched in May 1993, one of the first 400 web sites in the world. Much of this early work was reported in BICA, a "Bulletin of Information on Computing and Anthropology" (1984-1992), and CSAC was also to be involved in electronic publishing initiatives, such as the CSAC Monograph series (1990-2001). With the departure of Davis in 1990, Fischer succeeded as director of CSAC (later to be appointed Professor of Anthropological Sciences), and the work of the Centre was much invigorated with the arrival of David Zeitlyn in 1995. At this time, CSAC led a consortium of UK universities developing the HEFCE-funded "Experience-rich Anthropology" (ERA) project to enhance the teaching and learning of the subject through the provision of a variety of online materials and applications. In hindsight it is difficult to grasp the significance of these initiatives, which were considered obscure and nerdy by many during the mid-1980s, but are now taken so much for granted.

Paul Stirling himself was not a natural when it came to the new technologies of scholarship, but his sound judgment of their potential curiously placed him in the vanguard. His films for the Open University were exemplary of their kind, not aesthetically modulated "ethnographic films", but an extremely effective use of the medium to produce clear pedagogic instruments. We have seen how, in the early seventies, he collaborated with Marie Corbin on the computer analysis of census materials; and after he retired, he worked increasingly closely with Michael Fischer on the Stirling CD-ROM. Inspired by Firth's assertion of the importance of the 'anthropological record', Stirling took this one step further to produce a fully-contextualised multi-media package of his entire Turkish output: films, photographs, fieldnotes and published work. The finished result will be a technically innovative and eminently fitting memorial.

In retrospect, one of Stirling's major contributions to the analysis of rural society was in relation to social change and what we might now call "complexity theory". This had long been an interest of his, relating to his problems in locating "structure" and causality, but it was not until the dust had settled after his work in establishing sociology and anthropology at Kent, and when he had the time for more fieldwork in 1970, that he was in a position to reflect on the speed of change in his Turkish villages, and the new set of analytical issues that this engendered (Shankland 2011: 15). These he explored further in joint fieldwork with Emine İncirlioğlu and Mehmet Arıkan in 1985 and through reflections which were to pre-occupy him for the rest of his life, which were rehearsed in a key essay published by him in a festschrift to honour Lucy Mair (Stirling 1974), itself the leitmotif for a volume of celebratory essays edited by Chris Hann (1994). But Paul's interest in social change was never purely academic. It translated, almost instinctively — catalysed by a humanist temperament and self-identification as a non-Marxist "left wing" "do-gooder" — into a concern for applied anthropology. This may in part have hampered his preferment professionally. Though not a career development consultant, he insisted on forging links between policy-makers, planners and anthropologists, even when it was politically incorrect to do so. He was the energy and moral presence behind the professionalisation of applied anthropology in the UK, founding and inspiring the Group for Anthropology in Policy and Practice (GAPP) in the early eighties (e.g., Stirling 1983), and overseeing its transformation in 1988 into the British Association for Anthropology in Policy and Practice (Mills 2011: 128). An abiding concern of Paul's was with anthropologists unable to find academic posts. GAPP provided a focus for revitalising old debates about application

and nurtured new sub-disciplinary fields (see e.g., Grillo 1994, Pink 2006, Wright 2006). Given these examples, we should perhaps note that both Ralph Grillo and Sue Wright had long been close associates of Stirling, while Sarah Pink was a student and researcher at Kent working with John Corbin and Ray Pahl.

### **Changing departmental structures: cracks in the Stirling model**

Paul Stirling retired in 1984 and died in 1998. During this period the growth of both anthropology and sociology, staff departures and the tragic and untimely deaths of close sociology colleagues (Derek Allcorn, Steven Box and Christine Marsh), as well as new arrivals, began to fundamentally alter the character of Kent Anthropology. Frank Parkin moved to Magdalene College Oxford to teach political sociology with Stephen Lukes, Ray Pahl moved to Essex as a research professor, Richard Scase into the new Kent Business School, and Henry Bernstein to the School of Oriental and African Studies and thereafter to the Open University. In 1990 John Davis, who had by then been elected to a Fellowship of the British Academy, left to succeed Rodney Needham as Professor of Anthropology at Oxford and to head the Institute of Social Anthropology. Davis, who later went on to become Warden of All Souls College, claimed to have been “made at Kent”. At an institutional level too, 1960s interdisciplinarity was beginning to look tired. It was stretching at the seams, and was out of kilter with a renewed political emphasis on disciplines required as part of emerging forms of audit culture in higher education nationally. Kent, therefore, began a process of full “departmentalisation”. Interdisciplinary studies at Kent, as a consequence, finally collapsed as a subject and as a pedagogic ideal, not under the weight of its inner intellectual contradictions and conflicting virtues (methodological or otherwise), but because students were not attracted to its superficial amorphousness and perceived deficit of “rigour” (would it get them a job?). At the same time, interdisciplinary studies defied convenient administration at the university level, while for the UK government it was problematic given the straight-jacket imposed by politically-driven research assessment (Reason 1977, Strathern 2004).

The Board of Studies of Sociology and Social Anthropology was replaced in 1993 by a Department of Sociology and Social Anthropology, and its foundation more-or-less coincided with the arrival of John Davis’ professorial successor – Chris Hann. These developments immediately provided the basis for new teaching and research initiatives. Hann, an admirer of the Stirling legacy, sought its continuation, and indeed in 1994 the Department launched the inaugural Stirling lecture series in Paul’s honour. The 1990s also saw the establishment of undergraduate degrees involving exchanges with universities in Spain, the Netherlands, Italy and Germany, reflecting the university’s claim to being the UK’s “European University”. In a different vein, Roy Ellen’s work in ecological and environmental anthropology during the 1980s, and the appointment of Laura Rival in 1994, provided the capacity to initiate new graduate programmes in this area, and research focussed on the Centre for Biocultural Diversity, especially in ethnobotany through collaboration with the Royal Botanic Gardens at Kew. The establishment of the Durrell Institute of Conservation and Ecology (DICE) in 1989 led to some joint teaching, and this would provide one of the rationales for the subsequent merger of the two groups.

The work of Ellen in human ecology, and the strong “four field” interests of Michael Fischer and John Corbin also led to a momentous decision during the 1990s to broaden the scope of Kent anthropology, by introducing a BSc programme. Ellen had been teaching a course on

“Ecology and evolution” since 1987, and the combination of Fischer’s teaching of quantitative and scientific approaches and the development of an entirely new Part 1 course on “The foundations of human culture” served as a plausible basis to get this off the ground. Available teaching competence was augmented by part-time staff, in much the same way as Robin Fox had introduced part-time teachers such as Michael Day and John Napier at the LSE in the late 1960s. However, this programme was not on a completely firm footing until the appointment of Sarah Elton in 1999. Indeed, it was a struggle to staff this programme in the early years, but there was a major step-change in 2003 when although Kent lost Sarah Elton to the York-Hull Medical School and then Durham, it was able to make more appointments in the area of evolutionary and biological anthropology.

Changes in the composition of the Department had by 1997 created tensions between sociology and anthropology, and a place that had previously acquired a strong reputation as a “happy” department was now less so. By this time Hann was on leave of absence at Halle where he would take up the position as Director of the Max Planck Institute for Social Anthropology, and a disappointing performance in the 1997 RAE provided another opportunity for a radical change. This came in the form of the departure of the sociology group to form a new department with Social Policy, and a merger of the Durrell Institute of Conservation and Ecology (DICE) with the Anthropology group, to create an entirely new and innovative entity, in the form of the Department of Anthropology. Thus, although at a stroke this undermined a major plank in the Stirling vision, for the first time since its appearance at Kent in the mid-1960s Anthropology had the visibility of an independent organisational unit. In the years preceding this move, the development of a strong interest in environmental issues gave a natural overlap with the mission of DICE. DICE’s founding ethos accepted that effective conservation initiatives necessitated a role for social science, as well as sound biological groundwork. At the same time, there was a shared strong fieldwork ethos in both units, an orientation that had strained DICE’s relations with Biosciences (of which it had previously been part).

There were changes too within social anthropology at this time. The brief incumbency of John Clammer as a replacement for Chris Hann was swiftly followed by the appointment of Roger Just, who moved to Kent from Melbourne. With the arrival of Just, Southern Europe and the Mediterranean returned as a significant area of interest. As part of other organisational rationalisations within the university, Glenn Bowman moved into Anthropology in 1998 from Humanities, where he had been a lecturer in communications and image studies since 1990.

During the first decade of the twenty-first century Kent anthropology consolidated its position in terms of three broad groupings: social and cultural anthropology, ethnobiology and environmental anthropology, and biological anthropology. By 2013 these had morphed into three organisational “pillars”: socio-cultural anthropology, human ecology (to link with the conservation social science emphasis of DICE) and biological anthropology. Significant investments had been made in other areas, for example in establishing a cross-disciplinary Centre for Ethnographic Research. Academic staff expansion continued. The arrival of Dimitrios Theodossopoulos, Matt Hodges, David Henig, and subsequently João de Pina-Cabral from Lisbon in 2012 as Professor of Social Anthropology and Head of School, reinforced further the position of Kent in the field of Southern European anthropology. The Mediterranean tradition was maintained, but by then this core focus in the Department had ended.

As a founding father of the University of Kent, Paul Stirling was in the enviable position of being able to more-or-less create sociology and anthropology in his own image. As we have seen, the history of Kent anthropology is largely the history of decisions made by Paul in the

early days and their consequences. Indeed, his vision of humane, sceptical and empirically-based enquiry shaped the subjects as they developed at that institution. However, although the composition and ethos of early anthropology at Kent reflected Stirling's vision and personal qualities, there were inner contradictions. The consequences of his own open-minded approach to appointments and tolerance of opposing views might be seen as a weakness, especially when combined with the trajectory whereby new generations seemingly devour those who appoint them. At the same time, the vision was obstructed by periodic top-down university organisational restructuring in response to financial and academic accounting. This ultimately led to the creation of a very different kind of anthropology, but nevertheless one which could respect the values and norms that made it possible.

In this paper, as well as paying homage to the work and influence of Paul Stirling, I have tried to show how a minority and fringe subject could develop within the atmosphere of the new British universities of the 1960s, and how networking and personal influence could modulate the character of a department and academic community. It is difficult to imagine how much of this would be possible today in an era of audit culture, large faceless appointment committees, and university bureaucracies bound by rules of ethical procedure to mitigate bias, such as deliberate and routine anonymisation. Stirling's academic world (arguably appropriate for a social anthropologist researching a small-scale society) was itself to a considerable extent a small-scale oral community and uncompromisingly negotiated through personal interaction. Deprecating in the extreme and full of doubts about his own research and writing, and suffering bouts of depression, Stirling was a conscientious intellectual and personal mentor to staff he appointed, but might have found it difficult to survive the newest UK university dispensation himself. Scrupulously honest, he speaks to us today with a candour rare amongst those who have reputations to protect, and this honesty may well have become a liability which biased the judgment of those around him and hampered his own academic career. Always a healthy sceptical presence in seminars, he condemned dogma of all kinds and was a fully paid-up member of the awkward squad. More than anything else perhaps, he constantly reminded us that there was a real world outside the Academy. It might seem strange that someone like myself has written this account, perhaps especially because I have subsequently moved into fields of enquiry in which Stirling asserted his incompetence — cultural cognition on the one hand and human ecology on the other — but I am grateful to Paul for providing me with a comfort-zone in which I could explore my own doubts at a time when I was establishing my own career.

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## References

- Albrow, M. 1989. Sociology in Britain after the Second World War. In: Genov, N. (ed.). *Building National Traditions in Sociology after World War II*. London: Sage, pp. 194-219.
- Anon. 1987. Obituary: Derek Allcorn. In: *Anthropology Today* 3 (3): 19.
- Collini, Stefan. 2021. Review of *Utopian Universities: A Global History of the New Campuses of the 1960s*. In: *Reviews in History* (Review no. 2434), DOI: 10.14296/RiH/2014/2434. Accessed: 1.4.2021.
- Davis, John. 1977. *People of the Mediterranean: An Essay in Comparative Social Anthropology*. London: Routledge and Kegan Paul.
- Davis, John. 1991. An Interview with Ernest Gellner. In: *Current Anthropology* 32 (1): 63-72.
- Dresch, Paul and Roy Ellen. 2018. John Horsley Russell Davis, 9 September 1938 - 15 January 2017. In: *Biographical Memoirs of Fellows of the British Academy*, 17: 121-143.
- Ellen, Roy. 1998. Obituary: Paul Stirling: Understanding the Real World Outside. *The Guardian*, Thursday 2 July.
- Ellen, Roy. 2014. A short history of Anthropology at Kent, 1965-2014. <http://www.kent.ac.uk/sac/about/index.html>.
- Firth, R. [R.F.] 1966. A brief history (1913-1963). Department of Anthropology, The London School of Economics and Political Science, pp. 1-9.
- Fox, Robin. 2017 (2004). *Participant: Observer: A Memoir of a Transatlantic Life*. Second Edition. London: Routledge.
- Gluckman Max and Fred Eggan 1965. Introduction. In: Michael Banton (ed.). *The Relevance of Models for Social Anthropology*. ASA Monographs, 1. London: Tavistock Publications, pp. ix-xl.
- Grillo, Ralph. 1994. The Application of Anthropology in Britain, 1983-1993. In: C.M. Hann, Christopher M. (ed.). *When History Accelerates: Essays on Rapid Social Change, Complexity and Creativity*. London: Athlone, pp. 300-316.
- Hall, John R. 2003. Ernest André Gellner 1925–1995. In: *Proceedings of the British Academy* 120: 151-172.
- Hann, Christopher M. (ed.). 1994. *When History Accelerates: Essays on Rapid Social Change, Complexity and Creativity*. London: Athlone.
- Hann, Christopher M. 1998. Obituary: Paul Stirling. *The Independent*, 25 June.
- Kumar, Krishan. 2011. Frank Parkin Obituary: Sociologist who also wrote campus satires. *The Guardian*, 9 November.

Kumar, Krishan. 2020. The New and the Old: The University of Kent at Canterbury. In: Pellew, Jill and Miles Taylor (eds.). *The Utopian Universities: A Global History of the New Campuses of the 1960s*. London: Bloomsbury, pp. 143-158.

Mair, Lucy. 1972 (1965). *An Introduction to Social Anthropology*. Second Edition. Oxford: Clarendon Press.

Martin, Graham. 1990. *From Vision to Reality: The Making of the University of Kent at Canterbury*. Canterbury: University of Kent at Canterbury.

Mills, David. 2011. *Difficult Folk? A Political History of Social Anthropology*. Oxford, New York: Berghahn.

Parkin, Frank. 1985. *Krippendorf's Tribe*. London: Collins.

Pellew, Jill and Miles Taylor. 2020. Introduction. In: Pellew, Jill and Miles Taylor (eds.). *The Utopian Universities: A Global History of the New Campuses of the 1960s*. London: Bloomsbury, pp. 1-18.

Pink, Sarah (ed.). 2006 *Applications of Anthropology: Professional Anthropology in the Twenty-first Century*. Oxford: Berghahn.

Reason, David. 1977. The Red Queen's estate; or, grounds for interdisciplinarity. *Studies in Higher Education* 2 (2): 203-209.

Shankland, David. 1999. An interview with Professor Paul Stirling. In: *The Turkish Studies Association Bulletin* 23(1): 1-23.

Stirling, Paul. 1974. Cause, Knowledge and Change: Turkish Village Revisited. In: Davis, John. (ed.) *Choice and Change: Essays in Honour of Lucy Mair (LSE Monographs on Social Anthropology, 50)*. London: Athlone, pp. 191-230.

Stirling, Paul. 1983. The ASA Decennial Conference. *GAPP Newsletter* 5: 3-5.

Stirling, Paul. 1995. Obituary: Ernest Gellner. *The Daily Telegraph*, 9 November.

Strathern, Marilyn. 2004. *Commons and Borderlands: Interdisciplinary, Accountability and the Flow of Knowledge*. Wantage: Sean Kingston Publishing.



## Krishan Kumar

### Paul Stirling: Chair, Colleague, Friend

One cold spring morning in 1967 I arrived at the University of Kent at Canterbury (UKC)—as it was then called—for a job interview. I was in pursuit of my first academic job. The job advertised was for an Assistant Lecturer in Sociology, in the new Department of Sociology and Social Anthropology (the University itself, one of the so-called “New Universities” which had been founded in the 1960s, had only opened in 1965; see on this Kumar 2021).

I had not yet completed my Ph.D. in Sociology, for which I was registered at the London School of Economics (LSE). I did not in fact have any formal qualifications in Sociology at all. My first degree, my BA, was in History, from the University of Cambridge. I had then gone to LSE to pursue studies in Sociology, but had somehow been enrolled in the new MA degree in Political Sociology, largely taught by political scientists in the LSE Government Department. This suited my interests but it meant that I had little background in general Sociology, classical or modern. After getting my M.Sc. (Econ.) in Political Sociology I then enrolled in the Ph.D. programme in Sociology, but as was common at that time that did not entail taking taught courses, you simply attended those courses that caught your interest. In my case that meant attending Karl Popper’s graduate seminars in philosophy, a graduate seminar in the Sociology of Development led by Ronald Dore and Emmanuel de Kadt, Ernest Gellner’s undergraduate lectures that were the basis of his book *Thought and Change* (1966), Michael Oakeshott’s lectures on the history of political thought, and various seminars in social and political theory, including one on sociological theory taught by Percy Cohen. Beyond that I devoted myself to work on my doctoral thesis, on “Darwinism and Sociology: With Special Reference to the Empires of the Ancient World”, for which my supervisor was the anthropologist Robin Fox (no sociologist at LSE wanted to have anything to do with Darwin).

It was with this eclectic background, in which formal training in Sociology was conspicuously absent, that I confronted the selection committee at UKC on that cold spring morning. The committee was chaired by the Dean of the Social Sciences, Walter Hagenbuch, and included a political scientist, Maurice Vile. But the two people who were clearly going to make the decision on the appointment were Paul Stirling, as Chair of the Department of Sociology and Social Anthropology, and the person who was currently the most senior sociologist in the Department, Derek Allcorn, Senior Lecturer in Sociology. I was rather nervously aware that it was Allcorn that I would have to satisfy, as the position I had applied for was in Sociology and he was the only sociologist on the committee. Even worse was that the position advertised was in the field of the Sociology of Organizations, in which I had no expertise whatsoever (though I had hastily read up a few things on it before coming for my interview). Nevertheless, they had shortlisted me for the appointment, so clearly my lack of a formal background in sociology had not proved an insuperable obstacle. Why was that?

The answer to that puzzle was speedily resolved. After a few general questions from Hagenbuch and Vile, the interview was handed over to Allcorn. He put some penetrating questions about organizations, and about what kind of sociology courses I might be able to

teach. Luckily my reading of some recent articles in the field of organizations allowed me to give at least coherent answers, and I even managed to show the relevance of my interest in Darwinism to the study of organizations. But, given that there were other candidates for the job – including Michael Mann! – more qualified in this specialized field, I doubt if I would have been appointed if Allcorn alone had had the last word on the decision.

This is where Paul Stirling's intervention was crucial, and also served to reveal much that was central to Paul's way of thinking. It was Paul who now began to question me about my interests, and in particular what it was about Darwinism that had made me want to apply its concept to social processes. I mentioned my intention to look particularly at the development of bureaucratic rule in the ancient empires of Egypt, Mesopotamia, and Rome, and of how Darwinian processes of selection and adaptation had made them at first highly successful in competition with other forms, but had in the later stages arrested their development through their overspecialized adaptation to an environment that was in the process of change, not least through the working of the very practices that had made them so successful in the earlier periods (for more on this, see Kumar 1994). I was able here to call upon my study of classical history at Cambridge, and of my subsequent reading in the history of the ancient world undertaken for my Ph.D. work.

Paul then revealed that it was his wish to establish at Kent a course in the "Sociology of Intermediate Civilizations", by which he meant the civilizations that had succeeded, or at least were different from, the kinds of simpler societies generally studied by anthropologists, as well as the modern civilizations generally studied by sociologists. He had in mind the great ancient civilizations of Eurasia – Chinese, Indian, Persian. Mesopotamian, Mediterranean – which he felt had fallen between the cracks in the regular curricula of anthropology and sociology departments. Here, he felt, was an enormous tract of human experience and social organization that should not be simply left to classicists and ancient historians but should be incorporated in the thinking and teaching of anthropologists and sociologists. As a unified Department of Sociology and Anthropology, Kent's was in a good position to initiate this development, which had been neglected by most of the new as well as the older universities. I think that behind this idea was also Paul's memory of the multidisciplinary course on "Comparative Social Institutions" that had once been taught at LSE, and that featured the ancient empires as well as feudalism, nomadism, slavery, etc. LSE had discontinued the course before I arrived there; I learned later that Paul had been one of the people, during his time at LSE, who had opposed its abolition.

Paul himself had studied "Greats" – the Greek and Roman classics – at Oxford before turning to anthropology, and subsequently taking up a position in the Anthropology Department at LSE. What was interesting was his choice of Turkish villages in which to do his field work for his doctorate (his book *Turkish Village* was of course the product of this work). He chose in other words an area that had been part of the old Ottoman Empire as well as the modern Turkish state, and his work showed him fully aware of that dual inheritance. When he was appointed the first Professor of Sociology and Social Anthropology at the University of Kent, he determined that the anthropological work that would be pursued at Kent—what would make it a distinctive school of "Kent Anthropology"—would be largely in Mediterranean societies. Hence the earliest graduate students to undertake doctoral work at Kent went out to do fieldwork in southern Italy, Sicily, Spain, Greece and of course Turkey.

All these regions had histories – which were still living presences – in precisely those "intermediate civilizations" that Paul wished to make central to the Department at Kent.

Hence, though I was even less an anthropologist than I was a sociologist, my work appealed to him as falling within the general area that he wished to develop at Kent. I think he saw my kind of historical sociology as a sort of bridge between classical anthropological work, focused on “simpler” societies, and the studies of most sociologists which, focused very much on contemporary societies, often were indifferent to historical processes.

And so, thanks largely I’m sure to Paul’s advocacy, I was appointed to the position (luckily there were two jobs available, and another very qualified candidate was chosen for the Sociology of Organizations position, so Allcorn too was satisfied). But what the whole episode made clear to me was the very unusual quality of Paul as Chair of a joint Department of Sociology and Social Anthropology. Paul was not bound by any rigid or professional definition of what constituted Anthropology or Sociology. For him both disciplines were concerned with the study of human societies, in all their variety. This meant looking to the East and South as well as the West and North, to the past as well as the present. It also meant being open to influences and findings in a number of different disciplines, history, politics, philosophy, literature, as well as those of formal anthropology and sociology. I think Paul’s own background in the classics had led him in that direction, as classicists normally range over many disciplines—history, philosophy, literature—and attend to many objects, from literary texts to archaeological findings and the material remains of ancient civilizations. This was what I think also made him receptive to my own multidisciplinary background. My lack of specialization in professional sociology was not for him an important consideration. What mattered more were my interests and the way they drew on a varied set of academic specializations. He was interested in the courses I pursued, with Karl Popper and others, at LSE. Even my studies in evolutionary biology, in which I was guided by Robin Fox—a former colleague of Paul’s in the LSE’s Anthropology Department—were to Paul a possible source of insight into the nature of human social evolution (even though, like me, he was resistant to the more extreme claims of “socio-biology”).

It is significant, I think, that very few of the early appointments in the Department were of people with an original background in Anthropology or Sociology. Most had first studied history, or philosophy, or English literature, or classics. Many were products of Oxford and Cambridge, followed often by graduate work at LSE. That of course had been Paul’s own itinerary; and while one might see this as reflecting a certain “Establishment” view of the educational hierarchy in the United Kingdom, for Paul I think the significant thing was the breadth of academic experience they brought to their work as anthropologists and sociologists. He did not want people who had *only* been trained in sociology or anthropology.

This openness to intellectual influences from outside the strict disciplines of anthropology or sociology became evident again a few years later, when I had settled into the Department as a colleague. For various reasons, we never did establish a course on the “Sociology of Intermediate Civilizations”, though I was able to introduce topics relating to ancient empires and civilizations in some of my other courses, especially Political Sociology. But, going with the general air of experimentation that was current in all the “New Universities”, I became involved with the attempt to set up a new interdisciplinary, inter-departmental degree in Sociology and English. This turned out to be quite divisive, particularly as regarded the core course that was meant to bridge the disciplines. Some of the sociologists wanted to treat literature as a storehouse of sociological data, available for analysis and categorization in terms of purely sociological concepts such as class, status, forms of social mobility and of social conflict.

The members of the English Department were quite opposed to what they saw as a form of sociological reductionism, treating literature simply as raw data, akin to the statistical data collected by sociologists. They were certainly interested in social approaches to literature, but wanted to give literature its due, with its own ways of reflecting and commenting on society. I, together with some of the other sociologists, was sympathetic to this view. As Chair of the Sociology Department, Paul was drawn into this debate, and it was probably his intervention on our side that made the degree possible at all, for the English Department would have refused to participate on any other terms. As a result, with equal participation from members of the two Departments we were able to establish a joint degree in Sociology and English that was unique in English universities at that time, and that for years attracted some of the best students from both Sociology and English. Paul's part in this was critical; I was too junior a member at that time to have convinced a majority of my colleagues that a genuine sociology of literature would have to recognize literature as an at least partly autonomous realm, with its own forms and traditions. In Paul's understanding of what sociology was or could be, this was not a problem at all.

One further example might serve to illustrate the breadth of Paul's interests and his openness to areas normally ignored or frowned upon by sociologists. Together with my colleague Ray Pahl I had developed a course called "Sociological Theory and Social Practice", in which we sought to show the possible practical and policy implications of various forms of social theory. While Ray concentrated on actual, real-life, examples in new forms of community and work organization, I chose to look at imagined worlds, visions of alternative arrangements in which social theory was put to work to imagine new ways of living and working. Some of this could be found in the new literature of "futurolology"; but I found far better examples in the longstanding tradition of utopian fiction, from Thomas More to Edward Bellamy, William Morris, H. G. Wells, B. F. Skinner, and the newer form of the "ecotopia" of Ernest Callenbach and Ursula Le Guin.

Once more, some of my colleagues were disturbed by this incursion of imaginative fiction into the real-world sociology. This time, moreover, it was not in the service of the new degree in English and Sociology, where it might have found a place, but in one of the courses that was explicitly part of the Sociology curriculum. Sociologists have tended to look askance at utopias, except as expressions of social discontent, or perhaps in the form of "utopian communities" set up within the body of modern urban societies as partial "escapes" or withdrawal from modern life. Fully-fledged utopias, whole societies imagined and designed on particular principles, such as socialism or science, have seemed to most sociologists improper objects for sociological investigation. After all, they are not real; what possible principles could they be the test of, or provide reliable evidence for?

Paul knew his Plato, and Plato's *Republic*; he knew of a whole Greek tradition of writing utopias as a way of examining certain fundamental political and ethical principles. He was also familiar with Thomas More's *Utopia*, and More's humanistic revival of the Greek tradition of speculation through the imagination of ideal worlds. It was not at all difficult to convince him of the value of utopias as a field for sociological analysis — one, moreover, where the literary quality of the works made them highly attractive vehicles for the exploration of different varieties of social theory. He was fully supportive of this addition to our Sociology offerings — and equally supportive of my decision to spend several years delving deeper into the utopian tradition as a research project, one that eventually resulted in my book, *Utopia and Anti-Utopia in Modern Times* (1987).

I hope these few examples serve to give some impression of Paul as Chair and colleague, one whose vision—and, I would say, humanity—led to a distinctive and highly successful “Kent school” of Sociology and Social Anthropology (the later division into two Departments, which Roy Ellen will have more to say about, was one I think that Paul regretted. He always saw the two disciplines as related, each enriching the other). For most of the 1970s and 1980s, Kent regularly scored very highly in the various “Research Excellence” exercises conducted by the Government. Paul must take a good deal of the credit for this outcome. His selection of a very varied group of teaching staff, his support for unorthodox and unusual areas of teaching and research, and his ready availability—to the sacrifice, I feel, of his own research—to discuss new ideas and initiatives, were the life supports of the Department that he founded and oversaw for many of the crucial early years. A striking testimony to this was the personal loyalty that was so evident among the staff, and the fact that so many remained in the Department, often for several decades, despite good offers from elsewhere. Kent under Paul was a very good place to be.

A few words, finally, on Paul as a friend. I cannot say that I developed a strong personal relationship with Paul; our ways of life, family commitments, and circle of friends were very different. But what I can certainly attest to is the warmth of feeling that existed between us, and my conviction that I could always go to Paul to discuss any matter, personal or pedagogic, that troubled me. Here too what mostly stood out was Paul’s openness, and degree of tolerance. He had his own strong beliefs and values; but there were few people I knew who were so ready and willing to listen to other points of view, and to offer helpful advice even if in certain respects it went against his own preferred course of action. Paul was an anthropologist, of course, and anthropologists are supposed to be more open to diversity than most. We all know that is not always the case. For Paul, I think it was simply his own credo, his own acceptance of humanity in its many guises. Paul was in many ways a philanthropist; both his Christian and his classical sympathies perhaps had something to do with this. But I also think his engagement with anthropology and sociology, and his broad and humane understanding of both, deepened this commitment to exploring the human condition in all its plenitude. In this 25<sup>th</sup> year commemorating his untimely death, and at a time when the social sciences along with the humanities are under intense scrutiny as to their meaning and purpose, it is well to remember his example.

## References

Kumar, Krishan. 1987. *Utopia and Anti-Utopia in Modern Times*. Oxford: Blackwell’s.

Kumar, Krishan. 1994. *The Evolution of Society: A Darwinian Approach*. In Chris M. Hann (ed.). *When History Accelerates: Essays in Honour of Paul Stirling*. London: The Athlone Press, pp. 23-53.

Kumar, Krishan. 2021. *The New and the Old: The University of Kent at Canterbury*. In Pellew, Jill and Miles Taylor (eds.). *Utopian Universities: A Global History of the New Campuses of the 1960s*. London: Bloomsbury Academic, pp. 141-155.

**Sencer Ayata**

## **Paul Stirling as a Supervisor, Colleague and Friend: The UK-Turkey Dimension**

I first met Professor Paul Stirling on a cold winter day in early January of 1974 in his office at Eliot College. During that and later meetings, he occasionally wanted to continue the conversation in Turkish but had difficulties remembering various Turkish words. As I heard him saying so many times afterwards, he said his Turkish got much better after a little more practice in Turkey. That was the beginning of a long lasting and very genuine relationship between a supervisor and his student and later a colleague and good friend. Now, as I think about the twenty-five years that we have known each other and about the numerous times we have gotten together, I cannot remember any specific time when Paul really hurt me, though we often had disagreements, arguments, and disputes. When he said something to offend me, both my wife and I knew very well that he would call shortly thereafter and say: “that was very rude of me”. This was an uncommon behaviour from a senior academic, at least during Turkish university life at that time.

After leaving England to teach in Turkey, we met less frequently but for longer periods, and he later became a colleague of mine at the Middle East Technical University (METU) Department of Sociology. During some of his visits, Paul stayed with us. For instance, in the summer of 1984, we shared the same house for weeks when my wife was away in England. We talked, walked, and cooked together. For a short while, I had to leave Ankara, telling Paul that besides his bedroom, he could use another available room as his study and the living room to entertain his guests while I was away. When I came back, I saw that, with the exception of one room, almost all of the space in the house was covered with his photocopies, files, notebooks, and books. Not to offend him, I said nothing about the general state of the house. Instead, I told him: “so this room was not of much use for you”. He looked little embarrassed and responded: “that’s my gymnastics room”. We were indeed so close; Paul was a very good friend of the family, me, my wife and our daughter Zeynep.

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While waiting for an appointment at the British Council in Ankara, a friend of ours saw one of Paul Stirling’s notebooks left on a table. He opened it to look for some kind of identification and came across this sentence: “This morning I woke up worrying about Ayşe’s thesis”. Paul’s name was on the cover of the notebook. This was about Ayşe, my wife. Paul had always been an exceedingly concerned and responsible supervisor. His students from Turkey, especially those who had government grants, frequently had problems with the Turkish Cultural Attaché in London regarding extensions, research leave, or progress reports. In addition to patiently writing numerous letters to support his students’ cases to satisfy the Turkish bureaucracy, Prof. Stirling invested time and energy establishing personal relations with various authorities in the Turkish Ministry of Education to help them develop a better understanding of the conditions linked to completing a PhD in social sciences in England. Indeed, until then, the Ministry had

almost no such experience. On one occasion, he invited the Turkish Cultural Attaché to Canterbury for a weekend so that he could meet the students, learn what they were doing, and socialize with them. His efforts proved useful in helping us overcome our various difficulties with the bureaucracy. He was even more helpful to his Turkish students from Kent, five of whom were teaching at METU on their return from England in 1981 and 1982. Those were the years when Turkish universities were under the control of administrations appointed by the military authorities. Paul Stirling was well connected with many influential professors in the university circles, including some in the Higher Education Council, the supreme authority of the university system. He tried to take good care of the nascent and highly vulnerable METU Department of Sociology, where he himself was teaching as a foreign professor. He was also involved in dealing with problems that its individual members had with the broader faculty and university authorities i.e., delayed promotions, difficulties in getting passports, arbitrary appointments of department members to other universities. When somebody approached, he seemed almost always reluctant at the beginning, but we knew that within a day or two he would either call or pay a visit to say, “I’ve been thinking about this. Look, what I can do...”.

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In Turkey, rural–urban migration has been a subject studied both intensively and extensively (i.e., family, kinship, neighbourhood, social networks, clientelist ties, labour markets, housing, political participation, customs, religion) by sociologists, economists, and city planners. What is common to these studies is their observation of the migration process through the urban realm—that is, from the perspective of migrants in cities. At the other end, there is the tradition of village studies that goes back to the early 1940’s. What is certainly very rare are any sociological/ anthropological studies that look at rural–urban migration from the rural end. In this sense, Paul Stirling’s longitudinal study of the villages of *Sakaltutan* and *Elbaşı* is almost unique in that it provides background information about persons and families long before they had moved to cities. Paul Stirling’s studies enable a closer look at what conditions existed in the village and at what kind of opportunities in the cities had made villagers to move there. His work also dealt in great detail with the later course of interaction between migrants and their village. Here, the newcomer to the city becomes a person who we are already familiar with, someone we already know well.

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Paul Stirling and J.K. Campbell were PhD students in Oxford during the same period and both studied with Evans Pritchard. Stirling’s “Turkish Village” (Stirling 1965) and J.K. Campbell’s “Honour, Family and Patronage” (Campbell 1964) were two books based on extensive fieldwork carried out in Turkey and Greece. What I heard from Paul Stirling himself was that these two works were pioneering studies in British social anthropology on what was at the time called “historical societies”.

Nevertheless, I think that “Turkish Village” did not make the kind of impact that it deserved on sociological and political studies in Turkey. One obvious reason was the very small size of the academic social science community at the time and the almost negligible number of those social scientists who could read in English. The early sociological studies on rural Turkey, carried out by young scholars at Ankara University, attempted to expose a realistic portrayal of social and economic conditions in the villages. Almost a decade later, in the 1960s, rural research tended to proliferate with growing academic and political interest in issues of development and underdevelopment. Academic and intellectual interest on village life and

rural society gained a big momentum in Turkey in the 1970s with the rise of competition among political parties for the overwhelming rural vote, as well as the vigorous debates on the “question of peasantry” among Marxist academics and intellectuals (Öztürk 1991). These studies focused mainly on rural economic change and class structure. Other aspects of village life and society, such as kinship, community, customs, social networks, values and culture were only of secondary importance. Hence, they somewhat diverged from Paul Stirling’s work, which was particularly rich in such areas. I think this was the second important reason why “Turkish Village” had not become a major source of reference at a time when discussions and debates on rural issues had peaked. From the 1980’s onwards, as the rural population moved massively to cities and as the agrarian sector lost its economic prominence, interest in rural issues and agrarian problems became less salient.

A decade later, Turkey’s political and intellectual agenda shifted to issues of cultural divides and cultural identity, similar to sociological debates and trends in many other countries. This had brought the so-called centre-periphery paradigm to the forefront as a major source of conflict in Turkish politics. The core of the periphery, the Central Anatolian village, was seen as the center and starting point for the burgeoning of the Islamist movement and its progeny’s (the Justice and Development Party; AKP) rise to power. The Islamist movement and the AKP, in their challenge of the modern secular centre, emphasized the historical and societal divide between the overwhelming Islamic rural population and the Westernized urban elites in Turkey, and often exaggerated the antagonism and the scope of the clash between the two.

Reading through “Turkish Village”, it is difficult to come across cases revealing serious antagonism and intense conflict between the centre and periphery as put forward by the proponents of this point of view. On the contrary, what one finds is village life significantly isolated from both other parts of society and the state apparatus. As Stirling often hinted at, due to its lack of capacity or because of inefficiency, the state and the early Republican reforms hardly penetrated the village at all. In other words, the village remained by and large untouched by both the government and changes in urban life and institutions. Life in the village went along just as it had before, without any significant restrictions imposed by government authorities. For instance, there was neither a government appointed religious functionary (*imam*) nor a teacher in the village for a long time. Both functions were undertaken by poorly educated local people. The situation somewhat changed during the war years as the government needed to introduce new taxation on agriculture because of the urgent need to support a large army. The village population came under dual pressure as young men were recruited to the military and those left behind had to overcome the tax burden. Otherwise, according to Paul Stirling, what really characterized the approach of the centre towards the periphery during the early Republican period was a mix of insufficiency and ineffectiveness rather than insensitivity, cruelty, and oppression. I think it was a misfortune that the centre–periphery debate was overwhelmingly based on secondary sources or, even more so, on journalistic writings rather than on studies that provided first-hand, detailed, and accurate accounts of the village and its relations with the outside world—works such as “Turkish Village”.

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Social anthropologists who studied the same locality and the same people over a very long period—as Paul Stirling did—are indeed quite rare. I do not know of anyone in Turkey who has done so. Stirling visited Sakaltutan during different intervals for nearly forty years, writing articles on “revisiting revisits”. In his first visit in 1949, he stayed in the village for more than a year. Sakaltutan was a poor isolated Anatolian village located almost at the very centre of the



country, 33 km away from Kayseri, the nearest city. His wife, Margret Stirling, joined him during the early part of his fieldwork. At the time, the village lacked all basic services including water, electricity, a school with a teacher, a medical centre, and regular transport services. There was no tractor, let alone any other motor vehicle, in the entire village. To my knowledge, until that time, and indeed hardly after that, no native researcher or any other sociologist had stayed in a Turkish village for such a long period doing interviews and full-time participant observation.

As I often heard from Paul, life in Sakaltutan was generally hard and challenging but even more so during the very cold winter months. He said that rats used to get into the bed in order to get warm, and both he and Margret used to grab their tails to throw them out of the bed. On such cold winter days, Paul got seriously ill and could have died had he not been taken to the hospital. On one particular occasion, one of his close associates in the village, a man called Bilal Çavuş, carried him on his shoulders to the nearest medical center, which was 17 km away; this happened during a blizzard when the ground was covered with snow nearly half a meter deep. When I went to the village with Paul for the first time in the summer of 1974, Paul visited Bilal's wife to offer his condolences for her late husband, who had died between his two visits. Paul went alone when there was nobody else in the house. In a deeply conservative and religious traditional village, where women were strictly forbidden to talk to non-relative male adults, this visit seemed simply incomprehensible to me. The explanation was that *Pol Emmi* (Uncle Paul), as those younger than Paul used to call him was an exceptional person, one that was very highly respected and trusted by everyone in the village. After all, the villagers knew that her husband was the person who had saved Paul's life.

Paul was highly devoted to "his village". He was jubilant when the "Open University" decided to make a documentary film about Sakaltutan as a case study on issues of agrarian development and social change. The village—with its landscape, houses, people and their "Uncle Paul"—was introduced to a wide academic community.

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I went to Sakaltutan with Paul Stirling twice. We took a night bus from Ankara to Kayseri. I remember him, under dim light, looking at some notebooks while stopping from time to time as though he was memorizing something. As a matter of fact, he was going over his previous visit notes. When we went to the village, I found that most of the time he had difficulty remembering who a particular person was. However, when someone whispered him the name, he took everyone by surprise, showing that he knew a plethora of information about the individual and his/her family. This could be indeed anything, i.e., the father's previous heart attack, the purchase of a tractor, a cousin who had opened a shop in Antalya, the extension of a house, or crop failure in a particular year. On one such occasion, a villager whispered to me, "this man's memory is unforeseen, he remembers everything that has happened to us better than we do". Indeed, even during those few days that we spent in the village, Paul Stirling found somewhere to be alone to write down what he saw and heard in every possible detail. During this first visit, he told me that, "Even if there is no one to talk to or nothing to see interesting around, take time to describe what the place or the landscape looks like or if indoors, the rugs, furniture and even the colour of the walls". I repeated these words to every post-graduate student going out to the field for research. For Paul Stirling, the utmost criteria for decent social anthropological work had always been "good, honest recording".

Paul Stirling was not a prolific writer. I think one important reason for this was his perfectionism and philosophical scepticism. In comparison with the enormous size and the very high quality of his data, he indeed published little. Nevertheless, this invaluable data has not been lost. In his last years, he devoted himself to making his data available for everyone by putting it on the internet. When I met him for the last time, I remember how this endeavour made him so excited. This was typical of him, a person who always tried to do a good deed.

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Sociology as a discipline—and as a departmental chair—was introduced to Turkey almost a century ago by Ziya Gökalp, also known as the ideologue of modern Turkish nationalism. With his ideas, he not only influenced the founders of the Turkish Republic but also Turkish sociology. The core subjects of Gökalp's sociology were culture, civilization, nations, social solidarity, and corporations (Bulut 2005). Because of his work, his followers made more use of secondary, historical, and folkloric sources in their studies. A divergent project in Turkish sociology was initiated in the late 1940s by a group of young scholars who focused on Turkey's social and economic problems and looked closely at the overwhelming issues related to rural society; this kind of work was also based on fieldwork data (Arslan 2019). One of their former students, Mübeccel Kıray, who later continued her studies in anthropology in the US, was able to bring together nearly a dozen sociologists, social psychologists, and political scientists in the department of Social Sciences at METU. Kıray and her young colleagues, who also possessed a Western education, attempted to bring together fieldwork-based micro level sociological analysis with broader theoretical frameworks in their studies on various dimensions of social, structural, and organizational changes in Turkey.

Kıray's impact on the department has been mainly on two areas: prioritizing empirical research and, as different from the Gökalp tradition, diverting the main subject matter of research to issues of everyday life and society (Kıray 1998 and 2000). There have always been other department members who took interest in historical and philosophical aspects of sociology, especially after the rise of hermeneutics. Therefore, for Paul Stirling's students from Turkey, his persistence on the importance of fieldwork was not something new and unfamiliar. Yet, there was still some difference in methodology; unlike Kıray's essentially survey-based data collection method (though in-depth interviews were also part of it), Stirling, in addition to this, also emphasized long periods of stay as well as participant observation. His Turkish students took a middle course: they did not stay in the field as long as Paul did but used participant observation and carried out in-depth interviews.

Over the years, the Social Science Department (later Sociology Department) sent many graduates to study abroad, while also developing one of Turkey's most renowned post-graduate programs. At present, individuals with METU Sociology PhD degrees are teaching in dozens of universities in Turkey. In addition, Stirling's former students who teach at METU have transmitted this "deepened" understanding of field practice to their own students. I think that in this particular sense—along with others in Turkey who share similar methodological views—Paul Stirling has made a lasting impact on sociological research in Turkey.

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When we were in Kent, Paul Stirling frequently said that there was enormous social change taking place in Turkey, though the number of social scientists interested in studying it were very few. However, beginning in the 1970s, academic interest in issues of social change in

Turkey began to increase both in Turkish as well as in foreign universities. These studies followed two main paths: one was inspired by modernization theories and debates and the other mainly by Marxism. Studies on Turkey's modernization experience tended to focus on Republican modernization reforms and its aftermath, consisting of such issues as the rise of entrepreneurship, religion, populist politics, and the tensions between tradition and modernization. As to studies inspired by Marxism, a distinction should be made between those focusing on the stages of historical evolution of societies and the relatively few empirical studies on development, capital accumulation, industrialization, and changes in class structure, i.e., transformations related to peasants, the urban poor, and the working class. Stirling's social anthropological approach was closer to this second trend, not in terms of its theoretical orientation but in its interest in micro-level empirical analysis of social change. Despite his emphasis on going to the field with an empty baggage, his students were more familiar with studies that approached the subject of study with a strong theoretical disposition. In a way, it was a lot like first putting a piece of gum in one's mouth and then learning to chew it instead of learning to chew-without gum. I think his students again tended to sort out a middle way by laying equal emphasis on explanatory theoretical framework and going to the field with an open mind to become open to all of the challenges in the field. In my view, they tended to avoid the "verificationist" style adapted by some social researchers in Turkey.

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As far as I could see, Paul Stirling had very little to share with the rising theoretical orientations of the 1970s and 1980s such as post-modernism, post-structuralism, post-colonialism, and neo-Marxism. In personal talks and in seminar discussions, his comments and remarks were generally critical. Unlike some other powerful critiques of "critical sociology", Stirling's objections were fundamentally based on his sceptical philosophical outlook rather than on a strong ideological opposition to particular kinds of theory. Therefore, he often disagreed with many of us regarding the role, relevance, and place of theoretical discussions in the dissertation. Indeed, later during my academic work in Turkey, while examining numerous applicants for doctoral degrees or even for professorships, I came across many cases in which what someone said on theory made up three quarters of the text without any direct relevance to the original research. Though Paul Stirling made critical comments on such general statements in the text, in his overall evaluation of the work the kind of theory adopted did not matter much. What he wanted to see was whether these general theoretical statements had any direct relevance to other parts of the dissertation. His usual remark would be "if not, why say it?". This is one of the most important things I learned from Prof. Stirling.

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Paul Stirling was already in his mid-fifties when, almost all of a sudden, he ended up supervising five students from Turkey as result of a program supported by the Turkish Government to extend scholarships for students to study abroad. I doubt that he had supervised any student from Turkey before. Later, he supervised more, but from the first group of five students, all five including myself (Mehmet Ecevit, Yıldız Ecevit, Sibel Kalaycıoğlu, Ayşe Ayata) later joined METU faculty. It was there that the previous tutorial relations turned into collegial, friendly relationships.

During his stay in Turkey in the early 1980s, Stirling's modest, friendly, and supportive attitude was well received and appreciated not only by the Kent group and members of the Sociology Department but also by many others in the university. In the Department Paul Stirling took on

many responsibilities: acting as both our advisor and guardian. He had friends and acquaintances among rectors and deans in the universities. He tried to protect and promote the department, explaining and outlining our concerns, problems, and future plans. In the department itself, Paul tried to create a spirit of friendship, solidarity, and cooperation. He strived to be friendly with everyone around him regardless of political, ideological, or academic differences. I personally began to see well that what really makes the exchange of ideas, cooperation and, eventually, the accumulation of knowledge in a department was tolerance, rather than finding the right people to defend the right point of view. As it is still very common in Turkish universities, there has always been a strong tendency to create ideologically homogenous departments at the expense of unjustly getting rid of others. Both the country's military regimes in 1971 and 1980—and later the AKP government in 2016—have all dismissed hundreds of academics from universities on similar grounds. As a matter of fact, the Social Science Department at METU was itself a victim of such an attempt in 1971, when the military took the upper hand in Turkish politics without undertaking direct rule. Mübeccel Kıray and other young colleagues were made to resign as they were under pressure from rival colleagues in the department who were favoured by influential university and martial law authorities.

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As a supervisor, Prof. Stirling considered good expression and writing skills to be as important as the quality of the content itself. I remember him scrutinizing essays, chapters and indeed any piece of academic writing that I wrote for him, not only making corrections in almost every single sentence but also explaining why a particular way of expression was right or wrong. Every time, he reiterated the importance of clarity in writing to make sure that each sentence, as well as the whole text, was well understood. He put equal emphasis on the organization of ideas, often showing which sentence was to follow the next, how the paragraphs should be divided, what to begin an essay with, and how to end one. In my first year, I submitted an essay that was to be submitted to the postgraduate studies committee. He wrote in his report that “the essay is indeed rich in ideas but otherwise it is a muddle, indeed an appalling muddle”. He expected a plain style that is more expository, descriptive and persuasive. Paul Stirling strongly advised me and, I assume all his students, to avoid gilded expressions, very long sentences, and redundant words. His comments often included the note “redundant word”; for instance, the way the word “process” was used: why say “industrialization process” as the word industrialization itself denotes a process. Paul used a green pen to make comments on paper instead of the usual red; when he returned my essays, especially in the first year, I often found that most pages would be completely painted green. His educational background in Philosophy and Classics at Oxford must have played a part in such emphasis on accuracy, clarity and perfectionism. Yet, against all odds, I always thought Paul's advice was extremely valuable; later, I repeated his words hundreds of times to my own students: “always think of the reader”.

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When I was in Kent, I noticed that Paul Stirling had good relations with almost everyone around him; he had good friends among a faculty with divergent political views and ideological dispositions. One obvious example was my teacher, co-supervisor, and intellectual guide and—ever since the beginning—my very good friend Prof. Henry Bernstein. Henry remained committed all his life to the political economy approach in his academic work and to socialist views in politics. In Turkey at that time—even more so now—it was considered normal for people of opposing political views to be even on non-speaking terms. In England, we also knew

faculty members who often tried to totally dismiss each other. Curious about this, I asked Henry why he and Paul got on so well even when discussing politically touchy subjects like student protests, boycotts, and occupations. His answer was inspiring: “This, unlike what you may think, is not very common in England either, but Paul, you see, is a genuine liberal”.

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At one point during a late evening conversation, when Paul was staying with us in Ankara, I asked him what he thought about demise. First, there was sadness on his face and then tears in his eyes. I felt deeply sorry; it was a thoughtless and uncalled for question. As I was thirty years younger, I thought Paul, as a sort of elder, would come up with a kind of philosophical remark about death or provide answers to existential questions that I wondered about. Instead, after reflecting for a while, he said, “I think I have done enough good in this world”. I think he has done more than enough.

## References

- Arslan, Hakan. 2019. Türkiye’de Kır Sosyolojisi Araştırmaları. Ankara: Siyasal Kitabevi.
- Bulut, Yücel. 2005. Türkiye’de Sosyoloji Sınıflamaları ve Ziya Gökalp. In: İstanbul Üniversitesi Sosyoloji Dergisi 3 (10): 91-103.
- Campbell, John K. 1964. Honour, Family and Patronage: A Study of Institutions and Moral Values in a Greek Mountain Community. Oxford: Clarendon Press.
- Kıray, Mübeccel. 1964. Ereğli. Ağır Sanayiden Önce Bir Sahil Kasabası. Ankara: Başbakanlık Devlet Planlama.
- Kıray, Mübeccel. 1998 (1972). Örgütlenemeyen Kent: İzmir. İstanbul: Bağlam Yayıncılık.
- Öztürk, Nihal. 1991. Türkiye’de Tarımın Yapısı Üzerine. In: Gelenek, 33. <https://gelenek.org/turkiyede-tarimin-yapisi-uzerine>. Accessed: 10.06.2023.
- Stirling, Paul. 1965. Turkish Village. London: Weidenfeld and Nicholson.

## **Part Two**

### **Paul Stirling's Contribution to the Anthropology of Turkey**

**Chris Hann**

## **Paul Stirling, the Turkish Nation-State, and the Chimera of Modernity<sup>1</sup>**

### **Introduction**

Paul Stirling was born in 1920, just a few years before the Kemalist republic that he devoted his life to studying. When he died in 1998, that state was on the brink of a great transformation. In the eyes of his many detractors abroad and at home, the dominance of Recep Tayyip Erdoğan in the present century has pushed the country away from Europe, into a new system of conservative authoritarian rule and oligarchical corruption. Critics cite the draconian measures that followed the failed coup of 2016 as evidence of the regime's anti-democratic tendencies. By contrast, Erdoğan sympathizers argue that the "moderate Islamists" in power since 2002 have taken the country into a new era of political stability and economic prosperity. The best evidence to support this diagnosis is the fact that Turkey was hardly touched by the financial crisis that had massive consequences around the world after 2007. But economic indicators have deteriorated since then, along with the political climate, and the republic is deeply divided as it approaches its centenary.

This would not come as a surprise to Paul Stirling. When he began his pioneering fieldwork in central Anatolia in 1949, the massive changes imposed "from above" in the 1920s had not yet had much impact on the villages and small towns where the majority of the population lived (Stirling 1965: Ch. 12). His passing half a century later coincided with the passing of the era of Kemalist *modernity*. So, we can place Paul's work in a clearly demarcated historical context: he was the outstanding anthropologist of the making of a *modern* Turkey, of the processes of *modernization*. That is the cluster of terms I wish to problematize in this paper – modern, modernity, modernization.

In his first fieldwork, supervised in Oxford by Evans-Pritchard, Paul documented a peasant world that was about to disappear in the course of the systematic development policies of the Democratic Party, which triumphed in the landmark general election of 1950. The

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<sup>1</sup> I met Paul Stirling for the first time in summer 1982 at Eliot College, University of Kent, Canterbury. Ildikó Bellér-Hann and I were preparing our first fieldwork in Turkey and we needed expert advice. Paul was generous from the beginning. I came to know him well during the 1990s, when I was a newly appointed professor in Canterbury. He was a regular visitor as an Emeritus and for several years we shared an office in the heart of Eliot. Alongside Jack Goody and Ernest Gellner, contemporaries of Paul whom I knew well in Cambridge, he had a huge impact on my own thinking. I was grateful for many incisive comments, both orally and scribbled by hand with a green biro on my juvenile drafts. All three scholars formed attachments to the country of their field research, but Paul Stirling lived and breathed for Turkey to the end of his life. The others wrote more books; but only Paul created a new institution, one that became a powerhouse for both sociology and social anthropology, in Britain and internationally. I was deeply saddened by his premature passing. After many years in Germany, it was a privilege to return to Canterbury in November 2012 to give the Stirling lecture in his honour. The present text is a lightly revised version of that lecture that retains its title, informal style, and plentiful personal memories. The main text has not been significantly updated and does not pretend to offer a comprehensive survey of all the pertinent literature, not even of the materials readily available in English.

anthropological moment among the wheat-growing farmers of central Anatolia was not so different from that of Bronislaw Malinowski a generation earlier among the yam cultivators of the Trobriand Islands: “Ethnology is in the sadly ludicrous, not to say tragic, position, that at the very moment when it begins to put its workshop in order, to forge its proper tools, to start ready for work on its appointed task, the material of its study melts away with hopeless rapidity.” (1922: xv)

Unlike Malinowski, Paul Stirling returned to his field sites repeatedly. He followed the migrants of Sakaltutan to Adana on the Mediterranean coast, and as *Gastarbeiter* in Baden-Württemberg, Germany. At the University of Kent, in the intellectual community of anthropologists and sociologists which he founded and led from 1965, he supervised numerous doctoral dissertations by Turks about Turkey. Many of these students, and the colleagues with whom Paul cooperated most closely in Ankara, were influenced by the Marxism of those decades. They were critical of exploitative relations, both those which persisted in quasi-feudal forms in the countryside and those which took newer forms in capitalist factories. Paul empathized with the morals driving this critique, but found the analyses and policy prescriptions of the neo-Marxists dogmatic and simplistic.

From today’s perspective, both the bourgeois proponents of capitalist modernization and their Marxist critics had much in common: above all a rather linear view of progress. How could it have been otherwise when Anatolian worlds were being so radically transformed before their very eyes? There was room to argue about the precise mechanisms of change and the social justice of the outcomes, but the statistics for literacy, education, nutrition, employment and mortality seemed to tell an unambiguous story. The end-point of this linear teleology was none other than Europe: after all, Turkey was not just a full member of the NATO military alliance but a prospective member of Western Europe’s supranational political community, initially known as the Common Market, and still known as the European Community when Turkey formally applied for membership in 1987.

Erdoğan’s Justice and Development Party (AKP - *Adalet ve Kalkınma Partisi*) has remained nominally committed to the goal of EU membership. This party grew out of earlier “religious” parties suppressed by secular power holders (ultimately by the generals in successive military interventions). The AKP has tinkered with many details, not least by boosting the power of the presidency, the office held by Erdoğan since 2014, but modifications of this order are not my topic in this paper. I want to consider continuity and change from an anthropological perspective in three spheres: the political, the economic and the religious. I discuss the criteria for “modernity” in each one of these spheres (the separation of which is an archetypal modernist social scientific exercise). I ask if we should recognize a second modernity in the post-Stirling decades, i.e., the Erdoğan era, or if it is perhaps high time to jettison this slippery term altogether. Before concluding I turn to the less familiar social science vocabulary used by Paul Stirling himself to characterize the society to which he devoted his professional career – terms such as “information explosion” and “cognitive proliferation” (Stirling 1993: 12-3); and finally, to the question of values.

## A Homogenous Polity

The collapse of the Ottoman Empire and its replacement by myriad nation-states, the republic of Mustafa Kemal being one of them, is commonly taken to exemplify the advent of political



modernity in the Middle East. According to Ernest Gellner's theory of nationalism an efficient industrial society requires a uniform "high culture" that can only be assured through a homogenizing educational system controlled by the state (Gellner 1983). Whereas the Ottoman Empire had epitomised the multicultural polities of *Agraria*, the Kemalist republic inaugurated the modern world of *Industria*, the social cohesion of which depends upon strong national sentiment.<sup>2</sup> Long after former subject peoples of the empire such as Greeks and Bulgarians had consolidated national identities corresponding to new territorial borders (albeit imperfectly – on the ground the fit was never as exact as the Gellnerian "congruence" of culture and polity suggested), most speakers of Turkish across Anatolia had no clear sense of their national identity. The Kemalists set out to change this in the early decades of their rule. They began dramatically by expelling many of the non-Turks in the course of what was euphemistically termed an "exchange of populations" with Greece (Lausanne Treaty, 1923). They continued by writing a new national history, creating new symbols (many of them focused on Kemal himself, who adopted the name Atatürk, "father of the nation"), and a new capital city, and purifying the language (even going so far as to change the script in which it was written). All of this has been well documented by historians (Lewis 1961, Zürcher 1993). Less well documented are the institutional mechanisms through which the new state disseminated a national consciousness (not only through schools but also through the *halk evleri* and the *köy enstitüleri*, both influenced by parallel innovations in the Soviet Union). The strength of Turkish national identity, indeed its continuous rise during decades in which the strength of English/British national identity was evidently waning, fascinated Paul Stirling, though he did not address this topic directly. In short, from the 1920s the Kemalist nation-state emerged as the uncontested "container" of a new Turkish society, thus fulfilling a criterion that is as central to sociologist Anthony Giddens' (1987) vision of "modernity" as it is to the Gellnerian philosophy of history.

However, even after implementation of the Lausanne Treaty, the ethnic cartography of Anatolia was just as complex as that of the Balkans. The Kemalist republic was only prepared to recognise minorities according to the principle applied by the Ottomans, i.e., religion. Relatively small communities of Jews and Eastern Christians (mostly Greeks and Armenians) have persisted, concentrated in Istanbul. After many decades of suppression, it is nowadays gradually becoming possible to uncover more details of the history of non-Islamic communities in other parts of the country. I shall return to religion later: Islam dominates across Anatolia, but of course it is by no means homogenous. Meanwhile secular diversity takes many ethnic forms. The Kurds form by far the largest minority, but in the inventory of Peter Alford Andrews

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<sup>2</sup> It seems to me quite likely that Gellner was indebted to Stirling, five years his senior, in formulating his general model of nationalism. The two studied linguistic philosophy together in Oxford after the Second World War, before Stirling decided in 1947 to switch to anthropology. Later they were colleagues for many years at the London School of Economics, where Stirling was Gellner's adviser during his doctoral research in Morocco, which led to the monograph *Saints of the Atlas* (Gellner 1969). Paul Stirling declared frequently that *Turkish Village* would never have seen the light of day in 1965 but for the pressure exerted by his friend, who co-edited the series for Weidenfeld and Nicolson in which it appeared. But I don't think they were personally close. Respectful both of Gellner's philosophical acumen and his literary panache, the older scholar shared his "enlightenment puritan" sympathies vis-à-vis the relativists. He nonetheless identified inconsistencies and ambiguities, some of them inherent in Gellner's style of argumentation and his penchant for metaphors and jokes. Paul Stirling was dissatisfied with simplistic ideal types such as "tyranny of cousins" and "fundamentalist." He argued instead for a more differentiated approach to the distribution of knowledge in society. (Stirling 1996-7; this short contribution to a memorial issue published shortly after Gellner's passing is a marvellous example of Paul Stirling's searching intelligence, clearly influenced by his teachers in Oxford prior to 1947, as well as his chaotic style of presentation—digressions and incomplete references, his obsessions with precise language and causality, and his moral concerns.)

there are some fifty other ethnic groups (Andrews 1989).<sup>3</sup> In contrast to the religious minorities, none of these minorities enjoys any legal recognition and protection from the state. Millions of Kurds live in conditions of “agonistic pluralism” and the influence of left-wing populism remains strong (Tekdemir 2021). Within that putative nation, some activists identify separate sub-groupings on linguistic criteria (notably Zaza). In other, smaller minority communities, many members deny the classifications of Andrews. For example, Hemşinli are sometimes loath to be associated with Armenians, despite linguistic and historical evidence (Simonian 2007). Some members of minority groups acknowledge the affiliation, while insisting that they are nonetheless Turks in a full, national sense as well (i.e., not merely citizens of the Turkish state, but *Turks*). This is the case with the Laz of the east Black Sea coast. To some observers in Europe, these positions are simply misguided, the result of several generations of nationalist brainwashing. For such critics, it is self-evident that Lazuri speakers constitute a distinct ethnic group, closely related to other Mingrelian speakers in Georgia and not to any Turkish ethnic or ethno-linguistic group. It can be countered that the very concepts of ethnicity and nation-state are modern constructions, radically different from the religious basis of group identity in Ottoman days, when Laz migrants could already communicate in Turkish and probably felt a complex, context-sensitive collective identity (for further discussion, see Bellér-Hann and Hann, 2001).

When Ildikó Bellér-Hann and I carried out fieldwork among these minorities on the east Black Sea coast, the subject of ethnicity was sensitive, almost a taboo. We pursued our enquiries discreetly, concluding that there were strong pressures in the direction of assimilation and that “ethnic identity” was not a very salient issue in the region (and certainly not as salient as activists outside the country would have liked it to be). Much has changed since we completed our research in the 1990s. Laz artists have become well known on the national stage, the language can be heard regularly on the regional radio; it can even be read in books and other publications. Such *de facto* recognition is not enough to satisfy activists (some of them based in Germany), who insist that a modern state must recognise its minorities *de jure*, and then follow up with measures to create Laz schools, revert to original Laz settlement names, etc. But few of the Laz we knew showed any inclination to support such demands. Meanwhile for many Turkish citizens, with a very strong patriotism inculcated in them thanks to the nation-state container, prescriptive interventions from outside the country are perceived as unwarranted attempts to undermine national unity, regardless of whether the group in question consists of 100,000 Laz or 15 million Kurds. This mainstream also has problems with Western calls for declaratory recognition of the 1915 genocide against Armenians. A large majority endorses the earlier model of modernity, the nation-state model imported from Europe in the form of the Wilson doctrines after the First World War, even if Europe itself seems now to have moved on from that model and to be trying to impose another, based on some version of multicultural recognition. Normative criteria for the constitution of states and modernity appear to be shifting globally. Within Turkey, there is no consensus on how to move forward in the twenty-first century.

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<sup>3</sup> The project of Peter Alford Andrews was funded by the German Research Council and based on painstaking enquiries among migrants in Germany. It would not have been possible to conduct this research in Turkey, either then or now.

## A Market Economy

Of course, neither political nor economic modernization began with a *tabula rasa* in the 1920s. Significant changes took place in the last decades of the empire which paved the way for the bourgeoisie that emerged after its collapse. But can the term bourgeoisie be used at all, in a context in which the commanding heights of the economy were now controlled by the state according to the principle of *étatisme*? The policies of the early republic were greatly influenced by the Soviet Union, in economic planning as in many other spheres (Keyder 1983). But when Paul Stirling first went to Anatolia in 1949, the forces of production remained backward and social relations were little changed since Ottoman times. This was the “peasant” world immortalized in his monograph (Stirling 1965; cf. Meeker 2003 for a historical account of local-level continuities on the Black Sea coast).

The modifications launched in the era of multi-party politics after 1950 (a “watershed” according to Stirling 1974: 198-200) expanded the market principle, but the state did not relinquish control. This was particularly so in the rural sector, where purchasing prices for agricultural commodities and essential inputs were determined by the ministries in Ankara. These policies, in combination with protection against foreign imports and the provision of credit through cooperatives, did much to raise the standard of living of those who remained in the villages, without significantly diminishing the incentives to migrate to new, more lucrative and more exciting employment opportunities in the industrial sector, both at home and abroad. Paul Stirling documented the consequences of these policies in his important article “Turkish village revisited” (1974). Some years later, he made an ethnographic film for the Open University that took him back to Elbaşı and Sakaltutan but also to the migrants in Adana and Germany (*A Time of Change*, 1981). These analyses of the blurring of the boundary between the village and the national society were a continuation of the trends outlined in Chapter 12 of *Turkish Village* (“The village and the world”).

A more far-reaching embrace of the market principle took place following a period of military rule in the early 1980s, after Turgut Özal’s Motherland Party had won the elections of 1983. Özal deserves to be up there with Margaret Thatcher and Ronald Reagan in the political pantheon of neoliberalism. Well trained as an economist in Texas, he knew more about market theory than his Anglo-Saxon contemporaries. However, constrained by the axioms of the republic and the generals, he had to proceed cautiously as he dismantled the earlier model of economic modernity. Özal did not abolish the support prices, nor the state enterprises which dominated production in sectors such as tobacco, sugar (Alexander 2002) or tea (Hann 1990). But he did open these sectors up to private competition, and overall, he succeeded in shifting the national economy away from import-substitution towards the global market. With these steps, he prepared the way for a more thoroughgoing neoliberalism by the governments of Erdoğan after 2002. After waves of privatization, private capital is nowadays ascendant and need no longer live in fear of the state. This is also visible in the rural sector, where support prices have been abolished and there is little to disturb the formation of capitalist property relations, previously unknown in Anatolia (see Aydın 2005, 2010). If Paul Stirling had lived to witness these most recent developments, he would probably bemoan the paucity of ethnographic investigations of the contemporary countryside.

Economic growth rates in the new century have remained impressive. Should we conclude that Turkey, in this era of market dominance, has accomplished a new modernity in this sphere, superior to the levels accomplished in the mixed, state-led economy of the earlier Kemalist-Stirling model of modernity? For Eren Duzgun, from the standpoint of “political Marxism”,

what we have witnessed under AKP rule is “more the consolidation of a relatively novel capitalist project than a mere transition to another form of modernity” (2012: 144). He argues that familiar Western models of the bourgeoisie are inappropriate for comprehending its rise in Turkey, where the economy was historically embedded in the political sphere, and from which it could hardly escape in the early decades of the republic. Only now under Erdoğan has this separation been accomplished.

But not all observers agree that this fundamental detachment has taken place. Roy Karadağ (2010) sees Turkish neoliberalism as an “oligarchic capitalism” in which entrepreneurs continue to rely on political linkages (“cronyism”, “corruption”). In any case, it is clear that, in the economy as in the polity, the criteria for modernity have been shifting: away from the rationality of state centralization towards the coordinating capacity of the market, appropriately lubricated by personal ties and networks. For Duzgun, the tendency is politically deplorable, but it is still somehow progressive. According to the criteria of Transparency International, however, the brave new Turkey of the twenty-first century is becoming increasingly corrupt.<sup>4</sup>

## A Secularized Society

So far, I have argued with respect to the polity and the economy that the criteria of Kemalist modernity have been called into question in the Erdoğan era, which is also the post-Stirling era. Neither the homogenous nation-state nor the state-led economy seem to be viable options in the epoch of multiculturalism and neoliberalism, and the future is uncertain. Let us turn now to religion, a sphere in which the criteria for modernity are ostensibly more robust. In modern societies, according to the conventional social science theories, religion is one sphere of the social system, separate from other spheres or sub-systems such as politics and economics. According to most versions of secularization theory, religious faith, if it persists at all, becomes a matter of private conviction that can have no place in the public sphere. From this angle, French secularism in our time is the logical fulfilment of Enlightenment ideals. As is well known, these aspirations and the master narratives of the modern social sciences had a profound impact on the Turkish republic.

Mustafa Kemal abolished the Caliphate, instituted secular law codes based on those of Switzerland, and created new state institutions to administer religion and ensure that it did not stray outside its proper domain (*Diyanet İşleri Bakanlığı* – see Shankland 1999). These legal regulatory measures cut deep into life-worlds, including (most conspicuously) gender relations. No woman could work as a teacher or civil servant so long as she wore the traditional headscarf. Such restrictions on the dominant religion contributed greatly to the success of non-Kemalist parties once competitive elections were allowed. The agency of parties with an explicit commitment to bringing Islam back into the public sphere was at least partly responsible for provoking the military interventions of the late twentieth century, only for those parties to be banned in their wake. The pendulum, however, never swung back to its previous position. Contrary to the usual assumptions that modernization implies secularization and a weakening of religious observances, religion in the Kemalist republic has shown no sign of withering. Recep Tayyib Erdoğan himself (b. 1954) had a strongly religious education in Istanbul,

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<sup>4</sup> In 2020 Turkey was ranked 86<sup>th</sup> out of 180 countries in the “Corruption Perceptions Index”, its performance having declined significantly since the methodology was improved in 2012. See <https://www.transparency.org/en/countries/turkey>. Accessed: 17.10.2021.

graduating from an *imam hatip* high school before proceeding to business studies at Marmara University (the details of his enrolment and qualifications have long been contested by his enemies). These high schools (nominally for the training of state-controlled *imams*) were greatly strengthened in the 1980s under the governments of Turgut Özal, who combined his neoliberal economics with ostentatious demonstrations of his faith.

As in the case of the economy, the changes of recent years in the religious domain did not come from nowhere: they have precedents in the policies of Özal's Motherland Party (*Anavatan Partisi*). I remember amplification of the *ezan* increasing steadily during visits to the Black Sea coast in the 1980s and 1990s, as an aural barometer of the faith of the nation. Numerous anthropologists investigated this surge and the changing nature of public life in a range of local contexts, together with the diverse currents of a new Islamic intelligentsia (Tapper 1991, Navaro-Yashin 2002). It nevertheless seemed to many Turks like a revolution when, after religious leaders had yet again been imprisoned and banned from politics by the soldiers, Erdoğan's new AKP was allowed to win a decisive victory at the general election of 2002. After finally assuming power in March 2003, no pictures were more symbolic than those showing his wife and daughters in public wearing their headscarves. The former Mayor of Istanbul has softened some of his policies and retained the basic structures of the *Diyanet İşleri Bakanlığı*, but the public climate has undergone a sea change.

It is instructive to follow the commentaries on these developments, both at home and abroad. The Turkish intelligentsia has more than one voice. One increasingly influential response since the 1980s has been to insist that veiling is not inherently incompatible with modernity (Göle 1996). This connects with a wider trend to recover elements of the heritage of the Ottoman empire and even to open up the previously secret histories of dark episodes in the republican period (the ethnic cleansing of Christian communities, massacres of Kurdish rebels, etc.). Outside Turkey, the reforms introduced by the first Erdoğan government found numerous admirers. Some European leaders were so impressed by measures to strengthen the rule of law and modernize administration that they urged reactivating the endlessly stalled project of EU accession. The Islamism of Erdoğan's party was considered to be moderate and a valid expression of Turkish cultural traditions, not inherently any less liberal than the equivalent Christian traditions of several EU member states, and preferable to the dogmatic, nationalist secularism of the militant Kemalists.

Again, such shifts in Western attitudes cause confusion inside Turkey. Those loyal to the Kemalist legacy ask: how is it possible that Europe, locus of the Enlightenment and modernity par excellence, can now say to us that it is right that our teachers and ministers should wear the headscarf, because that is our cultural tradition? Veterans of the early Kemalist decades collect memorabilia of that period and feel "nostalgia for the modern" (Özyürek 2006) that they feel to be slipping away.<sup>5</sup>

It is not only the established secular elites who feel threatened. As noted above, Islam in Anatolia is by no means homogenous. Heterodox Alevi communities, whether Turkish, Kurdish or Zaza speakers, feel that the country's expanding "religious freedom" is a threat to their autonomy and security. Numerous measures have provoked criticism from Western leaders, but also from diaspora Alevis. The state has not extended its sympathy for the market model to the sphere of religion and continues to ban Christian proselytising. But for all this, anthropological work in Istanbul has documented a dynamic world of Islamic NGOs forming

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<sup>5</sup> Let it be noted in passing that Esra Özyürek (like Nilüfer Göle) is the daughter of a prominent Kemalist politician.

a civil society of a kind that would hardly have been possible in Paul Stirling's lifetime (Walton 2017). In short, significant changes have taken place in the post-Stirling religious sphere, as in every other sphere, and here too, the criteria for what is to count as modern have become thoroughly muddled.

## Social Cognition/Knowledge

What would Paul Stirling say about all these developments? I have emphasized topics on which he does not actually have such a lot to say, at least not at first glance. Stirling tells us almost nothing about the Armenian minority communities which had been uprooted barely a generation before his fieldwork in the immediate vicinity of the settlements he studied. He wrote about the peasant economy in meticulous detail, but the system of support prices was not yet in place when he began his fieldwork; in later work he addressed only its indirect effects, through migration and wealth creation. Paul did not see himself as an economic anthropologist, let alone a political economist. As for religion, again there is little in his published works to indicate that this anthropologist, though raised in English Protestantism and surely familiar with the Roman Catholic sympathies of his Oxford supervisor, was seriously interested in grappling with this domain.<sup>6</sup> After revisiting the Kayseri villages in 1970 and 1971, he noted in a few perfunctory paragraphs that while formal expressions of religion were "booming", it was becoming less "central" to village life as the society as a whole modernized and became more secular (Stirling 1974: 228-9). This was very much what a modernization theorist would predict.<sup>7</sup>

If Paul Stirling did not contribute significantly to specialist knowledge of Kemalist Turkey in any one of the three domains I have been talking about, I think he did something rather more ambitious. He was after all trained in social anthropology, where the conventional social science demarcations are subordinated to holistic assessment. Apart from this training, Paul was intuitively interested in how what he came to call "social knowledge" expanded in the course of modernization and what we nowadays term globalization or "transnational networks". At the same time, his early training in philosophy also left its mark. Paul maintained strong interests in the theory and epistemology of anthropology; he fretted endlessly about appropriate concepts, and indeed what qualified as a concept (Stirling 1996-7). Stirling was sympathetic to Gellnerian (i.e., Popperian) notions of openness, in science as in society. Yet it was hardly possible to transfer a "hard science" positivist approach to social anthropology. What really mattered in this discipline was not a "unique truth" but rather the knowledge that the community you were studying took for granted and thus considered to be true ("subjective" truth, as he termed it in Stirling, 1974). Without grasping this local social knowledge, it would hardly be possible to intervene constructively to improve the well-being of the community. This was of no concern to Gellner but it was enormously important to Stirling, on the basis of everything he observed over decades in Turkey. He devoted a lot of energy in his later years to

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<sup>6</sup> Colleagues who worked alongside Paul Stirling in Canterbury from the mid-1960s have confirmed that they knew him as a fundamentally secular man, though his wife Margaret had active links to her local church. His funeral and the celebration of his life and work at Eliot College in 1998 were both entirely secular (Roy Ellen and Krishan Kumar, personal communications). Yet it is interesting to note a slippage in his late engagement with the work of Ernest Gellner: whereas Gellner referred to himself as an "Enlightenment puritan," Stirling seems to prefer "Enlightenment Protestant" (Stirling 1996-7: 69).

<sup>7</sup> Much later, briefly and hesitantly, he rationalised "Islamic revival" and fundamentalist claims in terms of "making sense of their new world in terms of their old cosmos" (Stirling 1993: 13).

“applied anthropology”.<sup>8</sup> I think Paul Stirling often found it difficult to reconcile his personal sympathy with the enlightened goals of the Kemalists with the anthropological injunction to privilege the local, “native point of view”. But by operationalizing a concept of social knowledge, he found a way to reconcile his epistemological concerns (stemming from his training in Oxford philosophy) with his humanist sociological impulse to understand social change and potentially even to shape it by influencing policymakers.

The attempt to formulate a holistic theory of accelerating social change is clearest in the famous box diagram which accompanied his contribution to Lucy Mair’s *Festschrift*, in which box number 11, “knowledge, beliefs and skills”, is by far the largest below the level of the national level changes in the economy and in politics (boxes 1 and 2) (Stirling 1974: 203). Of course, this box permeated everything else in the diagram, since “every change must have intrinsic symbolic and cognitive aspects” (1974: 207). The expansion of mundane “stocks of knowledge” was bound to have an impact on basic beliefs and cause “cognitive changes” (1974: 208), as well as a decline in “cultural consensus” and “social control”. In later years, stimulated by the arrival in Kent of Michael Fischer, a specialist in computing techniques for anthropologists, he developed this interest in cognition as the most appropriate term to synthesize the diverse forms of social knowledge he had observed and participated in during the decades of modernization. Social cognition encompassed the total knowledge that people had of their social worlds. As those village worlds were transformed with the arrival of the first tractors and the departure of the first migrants, and as new worlds were created in the cities, Paul wanted to understand and explain the flux of social cognition. Which bits of knowledge would be retained and by whom? Which might, perhaps after a lag, eventually facilitate adaptations in the new contexts (cf. Fischer 1994)? Paul was sure that innovative methods would be needed to answer such questions. For example, it would be necessary to work in multiple sites and to employ quantitative techniques to verify if the propensity to marry a first cousin would rise in the city, where migrants could not take the risk of forging alliances with complete strangers. He also probed into the sources of new knowledge, and he asked what knowledge might disappear for good. Overall, he was convinced that modernization brought a massive expansion of social cognition, and that, even if cohesion diminished and inequalities increased, this was emancipating. I think he retained this optimism to the end of his life.

Was he right? Some observers of the modernization processes were less sanguine at the time, and perhaps today most would suggest numerous qualifications. For example, recent studies of Anatolian migrants in Germany have documented darker aspects of urban adaptation (Schiffauer 2000, Sutterlüty 2010). The prevalence of cousin marriage turned out not to be a temporary blip. Crimes of “honour” persist in Berlin Kreuzberg, where Turkish and Kurdish women continue to wear the headscarf, to acquiesce in patriarchal structures that discriminate against their daughters, and to occupy narrower life-worlds than the first generation of *Gastarbeiter* in the sense that they interact only with members of their own community (in so-called “parallel societies”, much debated in the German mass media). I think Paul would have been extremely concerned by these developments. He would have sought to understand them in wider contexts of life in Germany. If religious enthusiasms and “traditional” forms of behaviour are nowadays more conspicuous in Kreuzberg than in Kayseri, might this be a consequence of the deep sense of discrimination and alienation experienced by the second and third generation, which was halting the “natural” expansion of cognition? Might we, on closer inspection, find that some quarters of Kayseri, Istanbul and Ankara are not so different from

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<sup>8</sup> Paul Stirling was the driving force behind GAPP (“Group for Anthropology in Policy and Practice”) in the 1980s. He justified this investment laconically with the argument, “We need to do all we can to ensure that our students find jobs”.

Berlin and Cologne in these respects? At this point, Paul might have suggested that, since we simply do not know the answers, we urgently need more empirical investigation of the facts on the ground. But sheer ethnographic description would hardly suffice: there would have to be a *model* (perhaps he took this with him throughout his career from Evans-Pritchard 1940). Beyond this, it was necessary to specify causal chains. Stirling was fully aware that the social science could never match the natural sciences in this respect. But you had to make the effort, as he himself did most explicitly in his ‘Turkish village revisited’ (Stirling 1974). The challenge is to comprehend distributed social cognition holistically: to explain what we are more likely nowadays to term the “subjectivities” of individuals and groups in terms of sociological processes, specifying all the relevant variables of the model as rigorously as possible. The ultimate goal was truth, but Stirling was more interested in a truthful rendering of social cognition than in pursuing the elusive “unique truth” of Ernest Gellner. Needless to say, this defence of positivism was deeply unfashionable among social scientists in the late twentieth century.

## Concluding Reflections

Although anthropologists have echoed other social scientists in developing “functionalist” theories predicated on the demarcation of sub-systems, in our discipline the holistic impulse has generally been the stronger one. Questioning those demarcation lines makes us suspicious of “modernity” and all its cognates, and more sensitive to continuities with older “embedded” political economies and belief worlds. Having said this, the case of republican Turkey lends itself well to a modernization approach, since this was the top-down agenda of new elites in the decades in which Paul Stirling investigated the consequences of their interventions at the micro-level. The pace of change in the various spheres was different, and even the basic directions could change. While the embrace of market economics seems unambiguous and irreversible, toleration of multiculturalism seems more limited and ambivalent. Only in the new century has the revival of Islam in the public sphere become securely entrenched, but Stirling did not live to observe this.

In the light of post-Stirling developments, the Kemalist version of modernity looks like a failure. That has been the democratic verdict of voters. Has the AKP under Erdoğan forged forward to some new model of modernity, in which capitalist political economy and Islamic civil society can realize an unprecedented harmony? Closer inspection suggests that what we find under the presidency of Erdoğan is no more stable a package than the secularist modernity he sought to overcome. The increased assertiveness of Sunni Islam threatens the religious heterodox, but it also inhibits the expression of many secular forms of diversity, including ethnicity. In some urban pockets, both at home and abroad, where social networks revert to the size of the old village networks and the values of the village are asserted more vigorously than they ever were by the peasants, we may have the impression that time’s arrow has gone into reverse.

Is Turkey more or less modern today, now that the wives of the President and government ministers are free to go about in headscarves? Where is the causality here? There is no box for headscarf or veil in Stirling’s famous diagram! Most anthropologists have an implicit tendency to what the late Alfred Gell called “temporal cultural relativism” (Gell 1992). At some level, they know that time marches on, that neither headscarves nor neo-Ottomanism can undo the changes brought by Atatürk’s modernity. Rather, both are to be understood as consequences of



this modernity. But if their informants tell them that it is now modern (*çağdaş*) to grow beards and wear veils, anthropologists also feel under an obligation to respect this information, and even to prioritise it ahead of their own “analytic” definition.

In any case, *is* there a robust analytic definition to be had in the case of a term such as modernity? The Ancient Greeks considered themselves modern. Objectively, they certainly achieved significant breakthroughs in their time (though arguably no greater than the parallel phenomena of the Axial Age in India and China). After first going to the field around the same time, Jack Goody, a contemporary of Paul Stirling at Oxford, gave up his later studies of postcolonial modernization in Ghana in favour of *longue durée* East-West comparisons across Eurasia. For him, modernity is always a “shifting target”, as East and West alternated in technological and scientific leadership over the centuries (Goody 2004).<sup>9</sup>

Yet it seems hard to deny the extent and speed with which massive changes affected the entire population of the new republic in Paul Stirling’s lifetime. This was the Kemalist mission, Turkey’s “first modernity”, as we might label it. As for the country’s “second modernity”, it is held together by an increasingly fragile economy and by what Jenny White calls “Muslim nationalism” (White 2012). Patriotic sentiment is strong and shamelessly manipulated by the authorities. Negotiations with the EU proceed on the back-burner, but many Turks nowadays question whether this goal, for so long the ultimate yardstick of their successful arrival in the modern world, is actually a place where they would want to be.

Paul Stirling’s sharp modernist mind had a strong preference for linear, cumulative development, in both society and in scholarship. I recall a lecture he gave in Cambridge in the late 1980s on social anthropology as a cumulative body of knowledge. The audience, familiar with Ernest Gellner’s lectures but already exposed to the “writing culture” debates, was polite but sceptical. Stirling was no more sympathetic than Gellner to fundamentalism or postmodernism (which I think worried him more in his final years than the economic reductionism and political dogmatism he had combatted among some of his students and colleagues in an earlier generation). But I think he would have been flexible and sufficiently non-linear to engage seriously with recent “multiple modernities” theorists. He would have read with interest the works of those now uncovering hidden memories, perhaps even in the villages he himself had studied. He would have acknowledged the validity of anthropological studies of consumption and the media. He might even have swallowed his scorn for fundamentalism and been ready to pay more attention to religion. But all of this would have to remain subordinate to the encompassing goal of rigorously specifying “social cognition”, his primary tool for tracing the lives of people becoming ever more mobile and exposed to ever more diverse sources of knowledge.

Paul left us a very clear statement of his scientific epistemology and his values in the final paragraphs of his introduction to *Culture and Economy*, the volume he edited just a few years before his death (Stirling 1993).<sup>10</sup> After noting that many Turks, both intellectuals and village

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<sup>9</sup> More recently, in the course of a critique of Weberian “disenchantment” theorizing, Hans Joas has criticized modernization along with differentiation and rationalization as “dangerous nouns of process”: by lumping too many diverse phenomena together, such abstractions impede historical understanding (Joas 2021).

<sup>10</sup> These concluding remarks and the quotation are taken from the final paragraphs of my Introduction to the Festschrift I edited to honour Paul Stirling shortly after my move to Canterbury in the early 1990s. See Hann 1994.

informants, had become increasingly “suspicious of modernism and progress” during the decades in which he had been studying these processes, he concluded:

But I do not want to reverse it all. I would not want my best village friend's *gelin*, who was saved by caesarean in the Kayseri University hospital, to have died with her child, as she would have in 1950. Nor do I want people to be walking in the snow without shoes, shivering because their supply of cattle dung cakes has run out before the end of winter, nor seeing their children weedy from malnutrition. The extollers and the denouncers of modernisation, or capitalism, are both highly selective. How can anyone make an overall moral judgement on all these processes of change? They have happened: the results are there.

As an anthropologist, I am part of a collective effort to understand them. I find the complexity incredibly difficult to analyse. As a moral person, I have my own firm views and I hold that understanding social processes is relevant both to judgements, and to framing successful policies at all levels. But I also hold that understanding – achieving ‘truth’, that is, less misleading models of social processes – is a separate and morally neutral task. (Stirling 1993: 14-5)

## References

- Alexander, Catherine. 2002. *Personal States. Making connections between people and bureaucracy in Turkey*. Oxford: Oxford University Press.
- Andrews, Peter A. (ed.). 1989. *Ethnic Groups in the Republic of Turkey*. Wiesbaden: Reichert.
- Aydın, Zülküf. 2005. *The Political Economy of Turkey*. London: Pluto Press.
- Aydın, Zülküf. 2010. Neo-liberal transformation of Turkish agriculture. In: *Journal of Agrarian Change* 10 (2): 149-87.
- Beller-Hann, Ildikó and Chris Hann. 2001. *Turkish Region. State, Market and Social Identities on the East Black Sea Coast*. Oxford: James Currey.
- Evans-Pritchard, Edward E. 1940. *The Nuer. A description of the modes of livelihood and political institutions of a Nilotic people*. Oxford: Clarendon Press.
- Duzgun, Eren. 2012. Class, State and Property. Modernity and capitalism in Turkey. In: *European Journal of Sociology* 53 (2): 119-148.
- Fischer, Michael D. 1994. Modelling complexity and change: social knowledge and social processes. In: Hann, Chris M. (ed.). *When History Accelerates. Essays on rapid social change, complexity and creativity*. London: Athlone, pp. 75-94.
- Gell, Alfred. 1992. *The Anthropology of Time*. Oxford: Berg.
- Gellner, Ernest. 1969. *Saints of the Atlas*. London: Weidenfeld and Nicolson.

Gellner, Ernest. 1983. *Nations and Nationalism*. Oxford: Blackwell.

Giddens, Anthony. 1987. *The Nation-State and Violence*. Berkeley: University of California Press.

Göle, Nilüfer. 1996. *The Forbidden Modern. Civilization and Veiling*. Ann Arbor: The University of Michigan Press.

Goody, Jack. 2004. *Capitalism and Modernity. The great debate*. Cambridge: Polity.

Hann, Chris. 1990. *Tea and Domestication of the Turkish State*. Huntingdon: Eothen Press.

Hann, Chris (ed.). 1994. *When History Accelerates. Essays on rapid social change, complexity and creativity*. London: Athlone.

Joas, Hans. 2021. *The Power of the Sacred. An alternative to the narrative of disenchantment*. Oxford: Oxford University Press.

Karadag, Roy. 2010. *Neoliberal restructuring in Turkey: from state to oligarchic capitalism*. Max Planck Institute for the Study of Societies, Discussion Paper 10/7.

Keyder, Çağlar. 1987. *State and Class in Turkey. A study in capitalist development*. London: Verso Books.

Lewis, Bernard. 1961. *The Emergence of Modern Turkey*. Oxford: Oxford University Press.

Malinowski, Bronislaw. 1922. *Argonauts of the Western Pacific: An account of native enterprise and adventure in the Archipelagoes of Melanesian New Guinea*. London: George Routledge & Sons.

Meeker, Michael. 2003. *A Nation of Empire. The Ottoman legacy of Turkish modernity*. Berkeley: University of California Press.

Navaro-Yashin, Yael. 2002. *Faces of the State: Secularism and Public Life in Turkey*. Princeton: Princeton University Press.

Özyürek, Esra. 2006. *Nostalgia for the Modern. State secularism and everyday politics in Turkey*. Durham, N.C.: Duke University Press.

Schiffauer, Werner. 2000. *Die Gottesmänner: Türkische Islamisten in Deutschland; eine Studie zur Herstellung religiöser Evidenz*. Frankfurt am Main: Suhrkamp.

Shankland, David. 1999. *Islam and Society in Turkey*. Huntingdon: Eothen Press.

Simonian, Hovann H. (ed.). 2007. *The Hemshin. History, society and identity in the highlands of northeast Turkey*. London: Routledge.

Stirling, Paul. 1965. *Turkish Village*. London: Weidenfeld & Nicolson.

Stirling, Paul. 1974. Cause, Knowledge and Change. Turkish village revisited. In: Davis, John (ed.). *Choice and Change. Essays in honour of Lucy Mair*. London: Athlone, pp. 191-229.

Stirling, Paul. 1993. Introduction. Growth and changes: speed, scale, complexity. In: P. Stirling (ed.) *Culture and Economy. Changes in Turkish villages*. Huntingdon: Eothen Press, pp. 1-16.

Stirling, Paul. 1996-7. *Ménage à Trois on a Raft*. In: *Cambridge Anthropology* 19 (2): 59-69.

Sutterlüty, Ferdinand. 2010. In *Sippenhaft. Negative Klassifikationen in ethnischen Konflikten*. Frankfurt am Main: Campus.

Tapper, Richard. (ed.). 1991. *Islam in Modern Turkey: Religion, Politics and Literature in a Secular State*. New York: St Martin's Press.

Tekdemir, Omer. 2021. *Constituting the Political Economy of the Kurds. Social embeddedness, hegemony, and identity*. London: Routledge.

Walton, Jeremy F. 2017. *Muslim Civil Society and the Politics of Religious Freedom in Turkey*. Oxford: Oxford University Press.

White, Jenny B. 2012. *Muslim Nationalism and the New Turks*. Princeton: Princeton University Press.

Zürcher, Erik J. 1993. *Turkey. A modern history*. London: I.B. Tauris.

## **David Shankland**

### **Forty-five years of modernisation in Turkish Villages: What Stirling might have written**

Paul Stirling has a sure place in the history of social anthropology. He authored the standard work on a Turkish village (1965). He was the founder of Anthropology at the University of Kent. He was one of the first anthropologists to conduct a systematic restudy of their earlier material, researching the changes which had taken place since his initial fieldwork in the late 1940s. He then many years before this was to become widespread placed a large proportion of his material: articles, scanned copy of his monograph, PhD thesis and fieldwork notes on the internet as open access publications (Zeitlyn 2000).<sup>1</sup>

Over and above this, he was an excellent tutor, a careful and assiduous mentor to generations of students. He was a kind and careful doctoral supervisor too, notably to many researchers who were sent to his department from Turkey on scholarships, and became a life-long friend of many. To this, we need to add what are in Malinowski's phrase the "imponderabilia of everyday life" which are less easy to codify; his dedication to scholarship, his scepticism of grand ideas without proper support, his insistence upon clarity when writing, his readiness to edit work sent to him with a green pen, often put to very good use, his careful advice to colleagues as they were planning their careers, his equal care when making new appointments, and his contribution to academic discussion.

After his retirement from Kent, Stirling was for many years chair of a weekly lunch-time seminar devoted to Turkish studies at the School of Oriental and African Studies in London. There, in a pleasant setting – the Director's Committee Room on the first floor next to the Senior Common Room in the Old Building - he presided over an extra-ordinary range of papers, often given by researchers at the outset of their careers but also by members of the seminar themselves, notably amongst them Andrew Mango, Geoffrey Lewis, Brian Beeley, William Hale, John Norton, David Barchard, and Clement Dodd, who had founded the Turkish Studies Master's programme at SOAS after his retirement from the University of Hull. Courteous, sceptical, incisive and detailed in his interventions, Stirling would draw upon the decades of experience that he had gained as a teacher and researcher to great effect. They were occasions that we keenly looked forward to from week to week.

A further remarkable characteristic of Stirling's work is his clarity of style. Stirling himself, I feel, would put this down to his undergraduate training in philosophy, which encouraged him to think exactly what he meant when using any particular word. Indeed, though by all accounts he was not particularly close to Evans-Pritchard, Stirling's writing style is congruent with his supervisor in its literary quality, sense of responsibility when trying to describe social life and

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<sup>1</sup> Stirling's fieldnotes, many of his articles and a scanned version of *Turkish Village* remain available on that site [http://era.anthropology.ac.uk/Era\\_Resources/Era/Stirling/index.html](http://era.anthropology.ac.uk/Era_Resources/Era/Stirling/index.html).

reluctance to litter the text with extraneous references. The following quotation from the outset of his career, in the preface to his DPhil thesis is revealing in this regard:

When I say that my object has been straightforward description, I mean that I have not attempted to apply directly to the material any theoretical notions, nor, for the most part, to discuss theoretical problems. But the achievement of a straightforward description is by no means a simple matter. It requires first of all a great deal of skill in writing, a struggle with words. Skill in this art is not a matter of embellishment, it is a matter of ability to do the job which one has set out to do. I make no claim to have been victorious in this struggle; I can only say that I am at least aware of constant failure to say exactly what I wished to say. (Stirling 1950: iii)

Yet, this struggle came at a cost. His sometimes-lacerating self-criticism meant that he would be prepared to hold back or even discard drafts if they did not meet his very high standards (Hann 1994: 17). This same reticence affected too the publication of *Turkish Village*, which came out to his great regret later than he should have wished, some sixteen years after the end of his initial fieldwork. He told me once that he would attempt to write drafts of that book in the summer vacations, and then by the time the next vacation had come round would be dissatisfied, would tear them up and start again.

Toward the end of his life, Stirling equally reflected upon how complex the material was that he had collected for his restudy. Again, he encountered the same problem in that he found it very difficult to piece everything together as a continuous text that he felt was good enough to be published. Nevertheless, he did continue to work at it, and he did write a number of articles. In this piece, I shall try to speculate on what the book might have contained, and indeed to assess how close he was to the goal of the account of social change that he hoped to produce.

At the outset, it may be helpful to outline the way that I encountered Stirling and his work. After I arrived in Cambridge to take up my doctoral studies, I was determined to work on Turkey. My supervisor, Ernest Gellner was a North African specialist and decided that I would benefit from a close encounter with Paul Stirling, who was one of his oldest friends. For this reason, in 1986 I was sent to meet Paul at Kent, where he still had an office in the department. We spoke for many hours on that first occasion, and almost imperceptibly fell into the habit of meeting frequently.

Though I knew his writings, particularly his articles on the early Republic, it was really after I arrived in Turkey and became established in a village that I came to engage with *Turkish Village* on its own terms. My annotated, now rather battered photocopy of that volume which I had beside me in the village where I worked, became an extraordinarily useful *vade mecum*, beautifully structured and full of insight in almost every line. It remains a corner-stone of our understanding of Turkish village life, and has long provided me with a point of comparison as I continue to work with the Turkish Alevi villages (Shankland 1994).

Half-way through my fieldwork, in 1989, Stirling came to stay with me in Ankara, where I was also at that time teaching at the Middle East Technical University, in an apartment kindly lent to me by Lale Yalçın. Stirling's aim was to complete his second study, begun some years before, on the changes that Sakaltutan and Elbaşı, the two villages where he had worked since the late 1940s, had experienced.

As he explains in an interview conducted in the 1990s, shortly before his death (Shankland and Stirling 1999), Stirling's first study took place in 1949-1952, when he had obtained a Scarborough Fellowship in order to conduct fieldwork in Turkey. He obtained a lectureship at the LSE quickly having finished his doctorate. His early articles established his reputation, and he began to attend interviews to obtain a chair around the country, eventually being successful at Kent. Initially it was designated as a professorship in Sociology, but later Stirling changed the name to Social Anthropology. He was fond of recounting, perhaps half-joking, that he did this simply by changing the heading on his departmental letter paper.

Rather later than he had hoped, his *Turkish Village* appeared (by his account in 1964 though it has a publication date of 1965). Caught up with planning a new department and now with a young family, he was able to go back to Turkey only very briefly at the beginning of the 1970s, that is more than twenty years after his initial research. That brief visit stimulated him enormously. Social anthropology then, and indeed until very recently, was not accustomed to thinking of rural communities in terms of the changing process of social life over time. As may be seen from its title, "The Social Structure of Turkish Peasant Communities", his doctoral study was written in a department still under the influence of Radcliffe-Brown, and gave great importance to those aspects of social relations that tend to perpetuate themselves. Later, a good deal of the analysis in *Turkish Village* was given over to thinking in these terms; the way that the high death rate and low birth rate kept the village population fairly constant; the way that property division inheritance mitigated against the emergence of a landed hierarchy; the way that labour availability constrained the opening of new fields; the way that individual economic enterprise was restricted by the need to supply credit to family and friends, and so on.

Stirling was aware of the impact of the Republic, and the Kemalist reforms, and emphasised their importance both in his early articles, and his later writings. Clearly then, he did not think for a moment that social life was static. He did, though, seem to feel that it would take time for the reforms to reach the village where he worked, and was particularly insistent that change tended to be introduced from outside rural communities. It would not be misleading then, to say that from the very outset he was working with two contrasting models: one of social change and one of social stability, and that it took him the intervening decades to work through the implications of the way that these two came into juxtaposition with one another.

We can see an instance of this contrasting preoccupation quite explicitly in an article that derives from a second period of fieldwork that he conducted in Italy in the late 1960s. This fieldwork did not work out as fruitfully for Paul as his Turkish experience, though occasionally he used to refer to it in conversation. At that time, it will be recalled, there was a renewed interest in the anthropology of the Mediterranean, and Stirling gave this paper within the framework of one of a series of conferences that were attempting to consider the comparative possibilities of anthropology in the region (see Ellen in this volume). There, drawing from his field-site in southern Italy, he reflects on the cultural impact on local communities of a growing centralised bureaucracy.

The creation or sudden expansion by government action of a bureaucracy does not by any means prevent people from continuing to think and behave in terms of a personal morality. Office holders may be seen not as servants of a system of rules but as holders of the power to distribute benefits (or to refrain from imposing losses and sufferings). As such they are subjected to the full weight of informal pressures to serve the interest of their kith and kin, their friends, their clients and patrons. The formal, theoretically impartial rules must be stretched, interpreted, ignored or broken in order to make this

possible. This state of affairs we call corruption, though whether it is the system established by the formal rules of the legislator that has been corrupted, or the character of the administrator who has allowed personal duties to interfere with his public duties, I am not clear.

South Italy is a society in transition, galloping transition. The pre-war social structure survived in a large measure except for increased poverty and increased population pressure, until about 1950. Since then, the pace of change has been steadily accelerating... (Stirling 1968: 51-52).

Stirling developed these ideas further in a well-known article written after his 1970s revisit to Sakaltutan. It was first published in a festschrift for Lucy Mair, and then reprinted in the collection *Aspects of Modern Turkey*, edited by William Hale (1976) who was at that time still at the University of Durham. In it, he illustrates his Turkish material by drawing on Lucy Mair's understanding of social change.

The difficulty is a general one, because we cannot write of talk without words. Like others, I too, felt I needed some kind of classification or summary or framework in terms of which to think out the changes I observed. The schema that follows arose directly from attempts at description: it is an extension of Professor Mair's definition. I propose four types of 'changes.' The four types are not mutually exclusive; and they each imply the others. i) changes in social relationships, ii) change in knowledge and belief iii) changes in values, iv) changes in the general characteristics of the society. (Stirling 1976: 76)

Later on, however, after he began writing up articles from his second period of intense fieldwork, that is from the late 1980s until his death, he became less and less convinced that even these abstractions were useful, and tried as much as possible to describe in plain language what he thought was occurring.

To say that he sought to use plain language does not imply any lack of thought (see his comments on truth: Stirling 1996/97). Rather, he was increasingly concerned that the language that anthropologists employ serves to obscure rather than reveal the rightful aim of the anthropological inquiry which was, to him, to investigate reality. Again, we should clarify that he was aware of the arguments surrounding relativism and positivism that have been so prominent in anthropology for decades: they had formed indeed already a part of his undergraduate studies in philosophy. However, he did not find them terribly useful. His reasoning appears to have been two-fold: first that, though of course communities differ greatly in their customs and understanding of the world, the level of incommensurability implied by a starkly relativist approach was greatly exaggerated: that in fact humans, wherever they may come from, have no difficulty in sharing a basic appreciation of the world in which we live: that the sun rises in the morning and sets in the evening; that we need food and water to survive, that we get cold when the temperature is below freezing, that we may need shelter from the elements and from danger, and so on.

Secondly, he felt that his main research question was indisputably valid. It was for him evidently the case that something transformational happened in Turkey between 1923 and 1988, the period that he was researching. He wished now to find out as much as he could about that change. Or to put this more broadly, given that the task of anthropology is to study reality,



exploring the modernisation of a rural community and its wider implications was his chosen contribution to help anthropology in that endeavour.

He set about this task with a sense of caution, planning the research meticulously. Underlying his approach, however, was the idea that social life is linked together through a highly complex series of macro and micro causal processes which interact with each other, and that even if difficult, we can with careful research discern something about these processes, indeed we can hardly escape doing so. For example, in his introduction to *Culture and Economy*, the results of a conference on Turkish villages that he convened with Clement Dodd at SOAS, he writes:

These papers then confirm ... that the main task, and which all of us take seriously, is modestly to establish specific causalities: that is that a factor X has at least some influence on an outcome Y; of that an outcome Y is in part a result of a factor X. Yet authors avoid using the word 'cause' itself, using instead an endless variety of euphemisms ... (Stirling 1993: 5)

Again:

I argue that as a matter of testable ethnographic fact, overtly or covertly, most anthropologists most of the time imply and are profoundly interested in some kind of causal connection – things are as they are because of something else, usually a large collection of interacting something elses. (Stirling 1998: 2)

## The Restudy

When Stirling came to design his study, his prime aim was to gain data that would help identify the micro processes and changes consequent upon the modernisation of Turkish villages. In order to so, he began with his initial survey material from the 1940s, and then incorporated new data that he would collect in the field. However, his study encountered severe methodological difficulties. He very soon found himself handling huge quantities of data, which he found very difficult to process efficiently, causing him some distress. Partly, this was due to the sheer amount of material generated, and partly because of the way that in those pre-internet days data-base programmes were less intuitive to use than they later were to become. At the Royal Anthropological Institute, indeed, in our archives we have samples of the complex computer inputting that was needed in order to store his data.

Nevertheless, he and his team did collect a great deal of material, and he did write about this on several occasions, from which it is possible to discern the way that his approach was taking shape (Stirling 1988, 1991, 1992a, 1992b, 1993). Before looking at this in more detail, though, it is helpful perhaps to consider certain wider influences on his thought that shaped the inquiry. He explains in the interview that he gave describing his life in anthropology that he was motivated by empathy with the poor of this world, in particular as illustrated by sociological literature of the period.

... I was a radical – I got very left wing in the sixth form. ... I was told to write a paper about nutrition in Britain. I suppose it must have been 1937, and my elder brother sent me the Left Book Club literature to read for this paper. I was absolutely bowled over and appalled to read the ethnography, so to speak, though it was not written by

anthropologists, of Durham miners: two unemployed Durham miner families living in one room with their wives and children because that was all they could afford and kicking their heels on a village street all day, because the mine had closed down... I also read Seeborn Rowntree and discovered that 40% of the citizens of York could not afford enough to eat. I was absolutely shattered by this kind of detail so although I never joined the party – the Communist Party – I have been a bleeding heart ever since. (Shankland and Stirling 1999: 3)

When he moved to studying at the Anthropology Institute in Oxford, as well as Evans-Pritchard he had as his teachers, John Barnes, Max Gluckman, Meyer Fortes and was fellow students with Mary Douglas and Freddie Bailey. If, however, the emphasis at that time on social structure influenced him in forming his early work,<sup>2</sup> by the time that he had developed his second inquiry, the Malinowski influence appears to be coming to the fore.

Here we should pause momentarily to consider why this might be an important distinction. It will be recalled that, though Malinowski is rightly regarded as a pioneer in terms of his great influence in standardizing fieldwork, he did not subscribe to the rather abstract emphasis on social relations that came to dominant social anthropology in the 1940s and subsequently. Instead, Malinowski emphasised the empirical importance of ethnography, the necessary close attention to minute detail, the small aspects of everyday life that enable researchers to appreciate the dilemmas that the people with whom they are living experience from day to day.

This fierce attention to empirical detail has sometimes been traced back to the Empiricism of Mach, whom Malinowski studied for his doctoral researches. Whatever the truth of this, there is I think, a clear link between Stirling's emphasis on the meaning of words, the difficulty of describing adequately what we see, and the Malinowskian prescription that we have to experience, and write down as carefully as we can what we find.

There is however a further way that Malinowski is close to the way that Stirling eventually decided to develop in his work. Malinowski regarded himself as a functionalist in a very specific way in that he emphasised the way that societal institutions develop around the biological needs of human beings. For him, the need for food, shelter, and reproduction is universal, and though the way that this will be done varies across different communities, it will invariably affect the way that societal institutions come into being, and how they change. Malinowski's approach, seemingly adopted from his teacher Westermarck at the LSE (Shankland 2019), has not become prominent in later anthropology, which was almost immediately after his departure taken over by Radcliffe-Brown's more structural position. However, it was certainly part of Stirling's thinking when planning his later work: in other words, there is no doubt that he felt that one function - in Malinowski's sense - of modernity was to achieve greater protection, health and comfort to the mass of humanity than has ever been the case before in human history.

This merges indeed, with Stirling's personal empathy for the difficulty that so many persons globally experience in obtaining the basic needs of life. It might seem surprising that this needs stressing at all, but social anthropologists have seldom been comfortable discussing the apparently positive consequences of the astonishing transition to modernity over the last centuries. The reasons for this are not entirely clear: partly, it is presumably a reaction against

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<sup>2</sup> 'The Oxford Department at that time studied structure and these idiots, I was told, in the LSE used the word culture. I remember Meyer Fortes actually saying in a lecture that it is the structure that matters.' (Shankland and Stirling 1999: 11)

the nineteenth century evolutionists and Victorian ideals of progress. Partly, too it is a result of the synchronistic emphasis on understanding how different societies fit together. Another factor too, is the sense of loss that has been felt by anthropologists when they see the way that pre-modern life is changing. Yet again, partly this may be the result of the politics of academic life: the emergence of sociology as a distinct discipline, one whose very existence was predicated upon the study of the emergence of modernity, left the field open for anthropologists to emphasise their distinct specialisation in the diversity of human cultures.

Whatever the exact reason may be, it is this consciousness that modernisation has not been entirely negative, that made Stirling very hesitant to condemn or criticise the Republic. It was not only that he admired, as did almost everyone of that generation, the extraordinary way that a new nation had emerged from the destruction of the Ottoman Empire, nor indeed was he immune to its faults. It was rather that he felt that he had seen with his own eyes the astonishing changes in the village where he had worked. These could be experienced in multiple ways: in terms of the life expectancy, which had risen from forty to something like seventy during the course of his researches, the provision of adequate housing and medical structures in the village, or the escape from the tremendously hard grind of everyday subsistence agriculture. It was in part at least these changes, in minute detail that he hoped to chart through his ESRC project, which he had begun in 1984, an endeavour that was to preoccupy him until his death. Thus, when Stirling wished to revisit and summarise his material it was through this basic lens that he wished to interpret what he found.

Turkey became a nation state in 1923. From then on, it grew prodigiously. In round figures, by 1986, the population had multiplied by 4, from about 12.5 to over 50 million, and Gross National Product by about 20. So the GNP per cap., that is, in theory the average wealth created annually by each separate citizen, grew five times – 500 per cent – in just over sixty years. True that this internationally established econometric construction is seriously misleading; that it ignores distribution, that it is a sieve of statistical loopholes and that it in no way measures material welfare, let alone subjective sense of well-being. But an increase of five times in the value of things produced on average by every individual, achieved within sixty years – one life time, two and a bit generations – is not just surprising, it is fabulous; what other superlatives can I use? (Stirling 1993: 1)

There is one further strong influence. Stirling remained close to sociological thinking throughout his career. It will be recalled that his chair was initially conferred in Sociology. He gave one of his final papers to the British Sociological Association, and he told me that he regretted the debate concerning the administrative separation into two of his department at Kent that came about after his retirement, and which he thought might lead to an unnecessary dilution of academic potential (see Ellen in this volume). However, he possessed a strong dislike for modernisation theories which were pitched at an abstract level, and cited this as one reason that he decided to go back to Turkey:

I still think the inequalities, even within the industrial countries, let alone in the world, are absolutely appalling and so in that sense I had been a fellow traveller at Oxford. But it seemed to me that the whole Marxist model of social change didn't tell me anything about what was happening in the village. It was far too macro, there was nothing which explained what I found in the village in the theories of social change that I knew so, I was almost back to square one. (Shankland and Stirling 1999: 15)

Equally, he was steeped in the thought and writings of his friend Ernest Gellner, the great writer on the transition to modernity, for whom he had enormous affection and regard. Gellner had become his pupil at the LSE when looking for a supervisor to study for his doctorate, and Stirling was fond of reciting the way that he had listened to Gellner's description of Zawiya Ahansal, the village where he had come to research, and told him to go off and read *The Nuer* (Evans-Pritchard 1940), overruling his protestations. According to Paul, Gellner returned to him and confirmed that this indeed was relevant – the rest, as they say, is history as Gellner became one of the most determined defenders of segmentary lineage theory thereafter, indeed almost the only person who was prepared to do so publicly in his generation in the UK (Gellner 1981). Gellner, to Paul's regret, was taken from him as a supervisee by Firth, who recognised his brilliance. Paul incidentally, never shared the later adulation of Firth, feeling that he was rather too dominant as head of department.

Stirling's admiration of Gellner however, manifested itself in a curious way, almost as an antithesis. Stirling was convinced that Gellner's question was valid; that it is a fundamental part of the social science inquiry to seek the roots of the emergence of modernisation and the modern world. However, he was dissatisfied with Gellner's answers, and indeed with the rapidity with which Gellner made up his mind. He felt that Gellner was too reliant on models that had not been systematically demonstrated, and therefore remained sceptical. In a way, it would not be fanciful to see his wishing to look at the micro-details of modernisation as a counterpart to Gellner's approach. Indeed, sometimes in conversation, he would compare his own style of reading to Gellner's: Paul himself would read carefully, teasing out ideas and thinking them through before moving onto the next page, often even closing the book and setting it to one side in order to give him the chance to develop his thoughts. Gellner, on the other hand, according to Paul would maintain that the successful academic is one who could throw the book into the air and have it open as it hit the ground on exactly the key idea in the text.

We can discern then a number of intellectual influences, both positive and negative, shaping the way that Stirling approached the project. Taking into account too his writings, and the various research papers that he published before his death, what might the draft table of contents have looked like?

#### Forty-Five Years of Modernisation in Turkish Villages

Paul Stirling

#### Possible Table of Contents

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Chapter Five:	Modernisation and Organisational Change
Chapter Six:	Reflections
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The order of contents above is drawn from Stirling's published output, the unpublished material that he circulated (much of which is now on-line) and from the conversations that he had about the project. However, it was characteristic of Paul that he only published or wrote down his material after thinking about it very carefully, and I think that certainly in this case too, he

would have devised a more elaborate and sophisticated framework as the book took shape, with many unexpected diversions and insights that we obviously cannot now tell. An instance of this may be seen in one of his final papers, entitled ‘Credulity’, written for the ASA conference “Indirection” which is uploaded onto the Paul Stirling archive (Stirling 1998). This highly original article consists of a sparkling series of reflections and speculations, each paragraph taking a more unexpected turn than the last.

### **Introduction: When History Accelerates**

The title of the introduction ‘When History Accelerates’ is taken from the *festschrift* of that name dedicated to Stirling edited by Chris Hann (1994). It captures the essence of the question that Paul wished to study, and such initial reflections on rapid modernisation would surely have been part of any opening statement. Stirling also left various descriptions of how he went about his endeavour, wherein he covers his early fieldwork experience, as well as the genesis of the later research (e.g., Stirling 1992b). It is highly likely that he would have included this autobiographical historical overview in the introduction. He would have also, I am sure, included a caveat covering the technical difficulties of the work, which embarrassed and frustrated him.

Stirling’s difficulty here to my mind, stems from the radically different methodology that his second fieldwork adopted. His first ethnographic research was conventional. He once told me that he followed the Malinowskian fieldwork paradigm during that first research as closely as he could, indeed that he felt that it was the ideal way to conduct fieldwork. He lived in the village, only going to stay with the missionaries in Talas for a short time when he was too ill to remain. He spoke Turkish with them. He filled notebooks with qualitative data, and the only statistics he collected covered a household census, genealogies, and sufficient data on crops in order to understand the village household-economy. In effect, an anthropologist in this situation is looking for patterns: once they have conducted household survey, they do not need material from the whole village in depth, but rather need sufficient knowledge and examples of everyday life in order to illustrate their vision, a vision that they can reconfirm every time they leave their front door. Though they need a few close friends in the village in order to clarify and explain what is not obvious, they do not need to gather material from everyone, nor does everyone need to be equally articulate or ready to engage with the visiting anthropologist. In a way, we can say that this technique is fairly close to everyday life as envisioned in the work of Robin Dunbar (2010), in as much as we all have a few close friends, accompanied by an expanding circle of acquaintances of whom we demand much less.

By contrast, when he set up his new study, Stirling committed himself to a completely different type of inquiry, one that relied upon close to hundred per cent participation from the by now much enlarged village and its diaspora. He also required fairly detailed information from them, details that he then recorded. This explains why the amount of data thereby collected was so enormous, quickly running into many thousands of data points. To say that this is quantitative data would be slightly misleading, because he had no intention of using this material in a complex statistical way: rather he felt that collecting this would improve the possibilities of descriptive analysis, as well as allow others to search the data-base that would eventually result.

Nevertheless, several points should be made immediately in any introduction in defence of the project, and of his own doubts. It did, in spite of everything, achieve the vast proportion of its goals in terms of data collection. Not only was Stirling himself profoundly committed to the work, he was also ably supported, not least by Emine İncirlioğlu (1993), who wrote her thesis

on the project as part of his team, and by Mehmet Arıkan, Vahap Taştan, and Hülya Demirdirek of whom he also spoke highly. At Kent, he was also assisted by many colleagues including Michael Fischer who helped him with the computerisation of his material.

Stirling was also welcomed by the villagers, with whom he had an excellent rapport. His Turkish academic colleagues both during his first, and later stay admired and liked him. *Turkish Village*, for example, gained a detailed review when it came out, which praised it highly (Fındıkoğlu 1965). Aydın Yalçın and Nilüfer Yalçın equally were amongst his oldest friends, and their daughter Lale herself became a distinguished anthropologist. Later, when he was at the Middle East Technical University, he seemed to have been equally esteemed, even to the extent of being invited to become head of department: certainly, he would often talk about his friends and often former pupils there, amongst them Bahattin Akşit, Sencer and Ayşe Ayata, and Mehmet and Yıldız Ecevit. I remember keenly too, his visiting me in the early 1990s, when I was working at the British Institute of Archaeology at Ankara, and taking me to meet the Undersecretary of State for Culture, who was an old friend, and who greeted him with extraordinary warmth. Certainly, then, we can say that he worked well and was comfortable in the Republic.

Further, and this is something that I myself experienced to my wry regret when looking at the way that the Alevis move to Germany, that if one is looking at the way a group of villagers migrate from a single source the methodology that Stirling devised is both sensible and logical, even if it takes one into pastures new in terms of one's would-be professional competence (Shankland and Çetin 2006). More broadly, it might even be said to mark the contrast between rural and urban fieldwork, or between single-sited and multi-sited ethnography. In other words, again, we should see this as a pioneering project with inevitable teething problems, not as a failure, however disappointed he himself sometimes was.

## **Chapter One: The Kemalist Revolution and the Founding of the Republic**

A key question that would need to be decided at the outset is the extent to which Paul would have attempted a diagrammatic presentation of his overall vision of the mechanisms of social change, much as he did in his 1974 article. There is no doubt that the single-page visual presentation of his ideas gives a helpful illustration of the way that cause and effect may work together in multiple ways. However, toward the end of his life he felt that such diagrams were misleading, not only because they gave a misplaced sense of order where none necessarily exists, but also because there was no realistic way that a weighting could be attached to any of the causal factors that he had identified vis a vis any other. He also was acutely conscious of the way that metaphors such as 'mechanisms of social change' could give a misplaced sense of concreteness, and I can envisage his green pen striking through that phrase as I write.

Instead, he appeared to be working toward succinct paragraph by paragraph summaries and numbered lists of points in order to convey his ideas. We can see this in the presentation of the Kemalist reforms, which he returned to repeatedly throughout his written work, both early and late. Here is an example offered to the British Sociological Association, where he writes of 'eleven revolutions.'

### **The Eleven Revolutions**

No words are fully satisfactory. What constitutes a change? Or one unit of change? Or a revolution? ... I identify eleven main clusters of changes or revolutions. Arbitrary? Yes. ...

1. A new kind of State; Nation replaces Empire
2. Political and legislative westernisation and secularisation: Atatürk's reforms
3. Population growth; four times in sixty years, and still growing strong
4. Economic growth. An increase in GNP of over twenty times; and in GNP per cap of around 5 times in sixty years
5. Massive changes in occupational structure
6. Massive rural-urban migration
7. A massive increase in the number, complexity and effectiveness of organisations
8. A massive change in the national stock of knowledge; especially the knowledge embedded in the constant talk of people; ordinary people as well as expert people; 'social knowledge'
9. Changes in Islam
10. Changes in family and kinship
11. Changes in gender. (Stirling 1992a: 3)

Yet, though numbered points work well-enough for an article, they are difficult to make consistent throughout a whole monograph, and I think that he would have had returned to the careful prose that he used to such good effect in *Turkish Village* and related articles. Indeed, another way of stating his great challenge – the one that made him most depressed - would have been the difficulty of changing these different, isolated points into a sustained narrative.

He would have started, I am sure, with the Kemalist revolution. In any rewriting or representation of his understanding of the founding of the Republic, there are two starting points in particular that, I think, he would have brought out in a new version. The first is that he increasingly felt that the transformation from empire to a republic was fundamental because it gave the opportunity for Turkish citizens to identify with the new national unit in a way that had simply not been possible during the Ottoman Empire. It is sometimes pointed out that modernisation in Turkey has a much longer history than the creation of the Republic and the Kemalist reforms, that the Ottoman Empire made all sorts of attempts in this direction. But this is not quite what Stirling meant: his point was rather that the Republic was created in great part to achieve greater economic wealth for its citizens, to create an affluent country, rather than to serve a particular religion or dynasty, and all citizens were invited to be part of this endeavour (see also Hann 1990).

Secondly, toward the end of his life he felt that the economy grew gradually throughout the Republican period. This point, which he was fond of making in conversation, refers to a common discussion amongst Turkish specialists as to the contrasting economic success of the CHP governments which were in power from the founding of the Republic until the election of Menderes in 1950 with those that followed thereafter. Though there can be no doubt that Menderes pursued the characteristic debt-led development policies that were dominant until the Özal era and after, Paul felt that the converse claim, that the Republic had not developed up until that point was not true. Indeed, he eventually came to the conclusion that in fact the economic development under the early Republicans, partly because they were starting from such a low base, was roughly comparable to the Democrats that followed even if their ostensive approach was different.

This idea of modernisation was of course largely borrowed from the outside, in emulation of those states that appeared to have achieved or were on the way to achieving this. Gellner (1988) was interested in the Big Bang, the conditions that led the first cumulative economic development to take place. For him, the question was why it happened at all. Stirling's

empirical question was rather different: given that the goal of economic development, integral to the Republic from the outset, was adopted and it was substantially successful, how did it in fact come to be realised?

## **Chapter Two: Migration, Social Change and the Economy**

The difficulty in the next chapter would have been that which is so common in Social Anthropology, which is where to start when everything in a community appears to be so mixed up, so related to everything else. This dilemma is all the more the case for Stirling, as he is so conscious of the complexity of the causal processes that he is trying to disentangle. Nevertheless, after rereading his surviving articles, I think that after he had outlined in the first chapter the establishment of the macro infrastructure that was absolutely necessary to achieve modernisation: that is roads, water, electricity, factories, schools, credit, alongside the expanded bureaucracy, he would have started his village account with a description of migration, something that he had written up or presented on several occasions as he began to publish the research. Further, his historical perspective is clear, in that he eventually came to realise that emigration from the village for labour probably took place earlier than he had first appreciated, so that already by the 1940s, the village had to a substantial extent become a dormitory community. Then, however, men would go alone to work, leaving their families in the village. He called this 'pendular' migration, as distinct from the permanent or the household migration that took place subsequently. The point of his 1974 article in turn is that this migration brought not only increased funds to the village, but had profound effects on its economy, society and culture.

Though no doubt this would have formed the kernel of the chapter, a more general point that he would have made, one that links the first and second chapters I feel would have been that to his mind, modernity requires the creation of larger towns and cities that were hitherto contemplated, and therefore migration and urban life is an essential concomitant of economic development. Perhaps this is one area where he could have pointed out that there appeared to have been a misconception during early Republican times: it appears to be the case that in planning the new Republic, it was assumed that the cities would grow only gradually, enough to produce the civil servants who would run the country, whilst the country-side would be developed in situ through the development of rural business and trades through the village institutes, which would help with the modernisation of agriculture. The villages in turn would be serviced by factories who would procure the basic needs of the population; sugar, flour, textiles, and paper that essential need of an administrative bureaucracy. In fact of course, what happened was that alongside the great expansion in Turkey's population, Istanbul, Ankara and regional towns such as Adana, Konya, and Antalya very quickly became very large cities, an expansion that shows no signs of slowing today. This, alongside the expansion of hitherto smaller settlements into towns, means that the proportion of rural to urban population is continually falling. Today, it is usual when talking with the villagers where I work that they say that this is one change that did happen with Menderes, who removed the restriction on movement that until then had impeded migration to the cities.

A popular image of such migration after controls are removed is that it is uncontrolled: that the number of people flooding into the cities causes enormous stress on the public infrastructure and availability of land, creating large areas of slum dwellings. This indeed did happen to some extent. But the great value of Stirling's work is to show in detail the patterns that govern this migration, which are far more nuanced than this popular impression would permit.



One of the things I want to say is that the whole illusion of the *gecekond* [shanty-town] that there are all these poor villagers going in and being exploited and ending up at the bottom of the urban pile is absolute rubbish. Nearly all the prosperous people in the towns are in fact ex-villagers, and the number of people who were there in the first place and stayed there is quite a small proportion at any time. (Shankland and Stirling 1999: 22)

Stirling identifies several linked reasons for this rapid integration of the rural into the urban population: one of the most important of these is that it was possible for the villagers to move to the outskirts of the towns, build temporary structures, and then obtain title over the land that they had occupied. Then, they would build a single-storied structure which eventually could have further floors built on top, until, ultimately when the land become incorporated in the expanding city, could be entirely rebuilt as a multi-floored apartment block in which the villagers could receive several floors in return for making over the other floors to the contractor to sell. This meant that they would have a foothold, quite literally, in the towns from near the outset of their emigration.

The villagers of Sakaltutan in turn were integral part of this process for, from very early on in the Republic, they came to specialise in building work. Though some remained labourers, most would develop in skill and rise in the profession, particularly as plasterers. Thus, they would begin as apprentices, then become skilled workers, then foremen, then sub-contractors and – the most successful – contractors in their own right. This meant that there emerged simultaneously new forms of social differentiation: a few, those unable to negotiate social networks necessary to obtain work or work at all, would remain or become poor. Most however would benefit greatly, whilst a few would become wealthy by international standards. In this way, the integration of the village into the Republic went hand in hand with the expansion of the urban environment.

Migration overseas appears to have been part of this rapid transition, in that some villagers did move to Germany as guest workers (indeed Paul visited them and made a film for the Open University), and even more so to the Arab countries of the Middle East. He describes, for example, the way that enterprising villagers on the haj would seek out work opportunities in construction, and bring across their relatives if the possibility emerged. Overall, though, it appears that as the Sakaltutan economy improved such overseas migration became an impetus to their further success, rather than a substitute for it.

### **Chapter Three: The Household, Kinship and Marriage**

The careful study of the household and its economy made up a substantial part of *Turkish Village*, including a description of the household cycle, whereby three or even four generations may share their resources in common, then split as the sons leave with their wives, only to form again in turn. As his longitudinal researches continued Stirling noted that households tend to become smaller, just as many studies have found, but he also described in his final articles (eg 1988) the way that the urban economy became incorporated into the village household. The remittance money that flowed back into the village enabled them to buy fields, to invest in agricultural products, but also to invest in the towns. In turn, though it would have been entirely normal in the pre-modern period for a household to have some small diversification of income, households more than before began to possess multiple cash sources: such as income from selling milk obtained from milch cows bought after oxen had been replaced with tractors; girls carpet weaving from home, increased crop yields and so on.

At the same time, marriage patterns remained surprisingly constant. In pre-modern times, it would have been normal for a family to look around for a suitable spouse amongst relatives and local villages, drawing on both the husband and the wife's relatives. However, as the number of surviving children rises it becomes easier, rather than more difficult, to find a spouse in this way, hence the number of cousin marriages rose rather than fell. There also has emerged a gradual change whereby women are now desirous to move to the towns rather than stay in the village. Paul became conscious during his restudy that he had not been able to research gender relations with the sensitivity that he would have liked (on this see also İncirlioğlu 1993). Eventually, however, he came to the conclusion that it would have been difficult for him to do otherwise because of the patriarchal basis of village society. Nevertheless, one point that he frequently made is that the place of women had improved greatly.

No doubt, if Stirling had lived to work through the data, and to write in longer fashion about these household changes there are many other trends that he would have drawn out and presented. These however are those that he wrote about most often in his final papers.

#### **Chapter Four: Culture and Knowledge**

As Stirling worked on the project over the years, he became increasingly pre-occupied with what he referred to as the cultural changes that have taken place in the village, which he regarded as highly complex and intertwined. As he included social structure within his definition of culture (Stirling 1993), in a way any part of the book could be included under this rubric. Nevertheless, when he wrote specifically about the relationship between economy and culture, he often remarked of a series of interconnected changes that could be drawn out into a single chapter, such as a vast expansion of individual networks, the growth of the education system, an increased knowledge of the world, and changes in belief and religion.

It will be recalled that Gellner (1981) maintained that Islam was, in effect, the beneficiary of modernisation, that its belief structures could be supported by modern infrastructure, and components of religion which demand literal faith could be made compatible with modernity in various ways. Though there is ample evidence, and has been for many decades, that the Turkish experience is in accord with the theories on Islam proposed by Gellner, I do not think that Stirling would have made this a foundation of his monograph. I talked about this issue with him on many occasions. He was certainly interested in the reforms and their impact upon religious practice, but he thought that the fundamental transition to a western perspective was extremely deep-rooted, and difficult to change. Over and above this though he did not deny the eagerness with which the villages grasped the possibilities for counter-reform that were offered by Menderes as he campaigned in the late forties, he felt that Turkish politics has to be understood in terms of competing interest groups and patronage networks which would lead, in any case, to a bifurcation of political parties into different networks. Locally, therefore, he felt that the villages would equally well have voted for the *CHP* as the *DP* if a particular candidate would serve their immediate interests.

Nevertheless, he noted the way that religion was expanding to become a greater part of the public sphere, and the various ways that it appeared to be the beneficiary of the wider social changes that are taking place; such as the increasing ease of printing religious material, transmitting religious ideas and rules of religious conduct by modern media, or making the pilgrimage to Mecca. As he died before the victory of the *AKP* in the national polls in 2002, we cannot tell how he would have incorporated this into his manuscript. Given his underlying approach I think, that he would have still considered the rise of Islam as a component, but not

a dominant component of social change. By this, I mean that his emphasis on concentrating as much as possible on the way society as a whole develops and modernises would lead him to consider this as the starting point of his inquiry, and the religious characteristics of the village as important but incidental.

## Chapter Five: Modernisation and Organisational Change

A chapter on organisational and occupational change would have given Stirling a chance to restate one of his main themes, which is the difficulty of achieving such a rapid modernisation at a large scale, one that required both a huge increase in occupational diversity, and administrative capacity. Indeed, this takes us into the only area where Stirling experienced an occasional tension in Turkey. His insistence that the cities were largely inhabited by those who had newly arrived, perhaps led him to fail to realise until near the end of his life that there was a well-established urban class who could trace their descent back to generations of civil servants, or alternatively from the remnants of the urban *millet* populations, such as the Jews or Armenians (though see Stirling 1996). But, of course, he was viewing his material through a rural perspective so this can be comprehended.

In one of his most important papers, Stirling describes the way that the mutual violent conflict between left and right, which culminated in the 1980 coup, could be interpreted as resulting from the difficulties of adjusting and absorbing the expanding urban population into a new infrastructure and new forms of social control. This argument, which can certainly be traced back to Durkheim, he presented as head of the British delegation to celebrate the hundredth year of Atatürk's birth. It appears in the volume of the proceedings (1982), and has been translated into Turkish. However, Stirling told me that he had to fight very hard to say something intellectual at a meeting that was regarded as being largely ceremonial. Occasionally, his approach appears to have been misunderstood and at one point about this time, he was denounced to the authorities. Albeit temporarily, Stirling had his research permit removed. This was unfortunate in itself, but unfortunate too, because in fact much of Stirling's research pointed in the opposite direction; that migration is more ordered than commonly supposed. It was, however, a sensitive period. Eventually he was able to retrieve his research permission.

## Chapter Six: Reflections

It is difficult, perhaps even impossible, to know whether he would have included a concluding discussion. Presumably it would have depended on whether he, or the publisher, felt that the message that he wished to convey was clear enough from the main text. If he had done so, however, it would have given him the change to explore themes that he was increasingly becoming preoccupied by with regard to anthropology as a whole. For Stirling, one could not really consider the emergence of modernity without taking into account an equivalent expansion of rational thought, however complex the reception of that rationality might be in practice. This meant, as we have seen in that paper for the ASA entitled 'Credulity' he

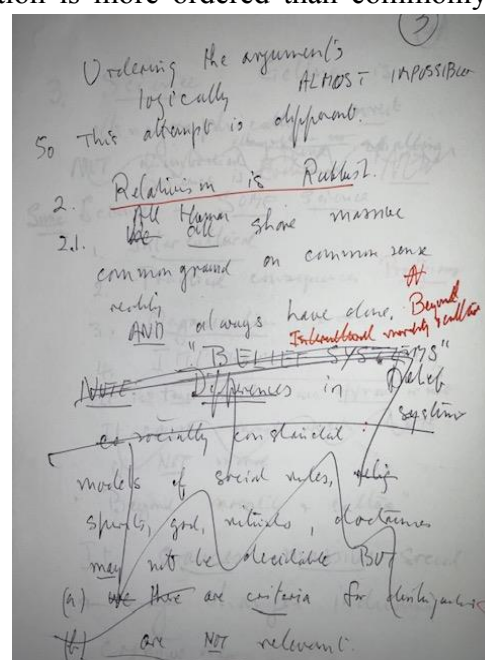


Figure: Paul Stirling's notes for the "Credulity" paper (RAI Archive)

was becoming interested in the growing post-modernist trends within anthropology for which he had increasingly little patience, as we can see in his notes for that paper, some of which are in the RAI archive [see figure]. Perhaps, then, this would have been an appropriate place for him to reaffirm his position: that there is a basic unity to human kind, an argument that certainly would be entirely congruent with his sympathy for Malinowski. Equally, it would have been a way that he could have outlined in some detail his thoughts on universal knowledge and its relationship with the emergence of modernity and the role of anthropology in our investigation of that endeavour.

### **Appendices: Tables and summaries of data analysis**

Finally, there would have been, I think, a readiness to add an appendix, which would have included the kind of methodological explanation that he wrote when describing his on-line material. I am conscious that the one lacking feature in this suggested text is a sense of the variety of questions that could be asked of Stirling's accumulated data-base, presumably because he was still working through these as he began to write up his articles. We perhaps would have seen a variety of these questions and explorations in tabular form, which would have acted as suggestions as to the way that the project could be taken forward.

### **Conclusions**

Even though we have only scattered articles, we can see how unique Stirling's work was. Though we see all around us the results of Turkey's modernisation, the way that this has been realised has never been written about at the level of detail that he achieved. His project, even though twenty-five years have now passed since his untimely death, remains as original as ever. We can also see, in spite of his oft-stated sense of failure, just how close it was to being completed. There are amongst those extensive final papers numerous, fluent restatements of his general themes, and if he had lived longer surely the data would have become easier to handle, all the more so as computing power expanded so quickly and data-bases became routine. This means that in all likelihood, the book would have been finished. Indeed, if upon rereading the material for this article I was struck once more by its first-rate quality, I was equally surprised to realise just how close he was to reaching his goal, closer perhaps than he himself realised.

In terms of its content, it is likely, I think, his profound understanding of the complexities inherent within his material would have led him to a more intricate treatment than the preliminary contents outline above provides for. It is worth stressing that each time Stirling presented his work, he appears to have discovered new, and often increasingly sophisticated ways of analysing and understanding his material. Toward the end of his life, for example, he presented a full-length paper to a workshop in Vienna (Stirling 1996) that represents a clear elaboration on his approach to the relationship between culture and economic change. He begins, as he would often do in his own mind, with ruminations on Gellner's understanding of the roots of nationalism, and then looks specifically at the way that social knowledge may have changed in three periods; the late Ottoman Empire; the early Republic, and later in the two villages where he worked as they modernised in the twentieth century. He then considers, in each case, the relationship between groups, mutual interaction and the expansion of social knowledge. The above scheme for his monograph would surely have included extensive discussions along these lines; so that, in all likelihood, a more dynamic flow would have emerged than my outline implies, an interwoven discussion of the links between macro and

micro social change; migration and cognition, stability and organisational change that often would take the reader into new and unexpected paths.

Given though, that my encapsulation is not widely off the mark in outline at least, what would have been the book's importance? I think it would have quickly become a classic, an ideal complement to his *Turkish Village* monograph. Just as that text, it would have been understated. It would not have been full of polemics, nor made wild claims. However, it would have been clear and accessible. It quickly would have been accepted as a book with a huge amount of useful material presented in a coherent and suggestive way, useful for those not just who work on modern Turkey, but for anyone interested in the emergence of the industrialised, twentieth century world. Above all, it would have been original; it is not just that he had the benefit of seeing these changes over a period of forty years, but rather that he lived through them, recognised their importance, and collected data to demonstrate them as they were achieved. His example serves to remind us of the overwhelming desire that human-beings have to be comfortable, to be affluent, and its social consequences. It perhaps needed someone such as Stirling, with his scepticism of grand theory, to remind us anthropologists to take this fundamental desire into account once more as we devise our research.

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### References

- Dunbar, Robin. 2010. How many friends does one person need? Dunbar's number and other evolutionary quirks. Cambridge, Mass: Harvard University Press.
- Evans-Pritchard, Edward Evan. 1940. The Nuer: A description of the modes of livelihood and political institutions of a Nilotic people. Oxford: Oxford University Press.
- Fındıkoğlu, Ziyaeddin. 1965. Prof. P. Stirling'in "Türk Köyü" Eseri Hakkında [On Prof. Paul Stirling's *Turkish Village*]. In: İş ve Düşünce Türkiye İktisadi ve İhtimai Araştırma Mecmuası 252 (December): 2-10.
- Gellner, Ernest. 1981. Muslim Society. Cambridge: Cambridge University Press.
- Gellner, Ernest. 1988. Plough, Sword and Book. London: Collins Harvill.
- Hann, Chris. 1990. Tea and the Domestication of the Turkish State. Huntingdon: Eothen Press.
- Hann, Chris (ed). 1994. When History Accelerates: essays on social change, complexity and creativity. London: Athlone Press.
- Hann, Chris. 1998. Obituary: Paul Stirling. In: The Independent, 25 June.

İncirlioğlu, Emine Onaran. 1993. Marriage, Gender Relations and Rural Transformation in Central Anatolia. In: Stirling, Paul (ed.). *Culture and Economy: Changes in Turkish Villages*. Huntington: Eothen Press, pp. 115-125.

Shankland, David. 1994. Social Change and Culture Responses to Modernization in an Alevi Village in Anatolia. In: Hann, Chris (ed.). *When History Accelerates: essays on social change, complexity and creativity*. London: Athlone Press, pp. 238-254.

Shankland, David and Atila Çetin. 2006. Culturalism and social mobility: an Alevi village in Germany. In: Shankland, David. (ed.). *Structure and Function in Turkish Society*. Istanbul: Isis, pp. 167-184.

Shankland, David. 2019. Social Anthropology and its History. In: *Zeitschrift für Ethnologie* 144: 51-76.

Shankland, David and Paul Stirling. 1999. An Interview with Professor Paul Stirling. In: *Turkish Studies Association Bulletin* 23(1): 1-23.

Stirling, Paul. 1950. *The Social Structure of Turkish Peasant Communities*. Unpublished DPhil Thesis. Oxford. [https://era.anthropology.ac.uk/Era\\_Resources/Era/Stirling/Thesis/Stirling\\_Thesis\\_1.html#Page5](https://era.anthropology.ac.uk/Era_Resources/Era/Stirling/Thesis/Stirling_Thesis_1.html#Page5). Accessed 17.1.2023.

Stirling, Paul. 1965. *Turkish Village*. London: Weidenfeld and Nicholson.

Stirling, Paul. 1968. Impartiality and Personal Morality. In: Peristiany, John. (ed.). *Contributions to Mediterranean Sociology*. Paris and The Hague: Mouton, pp. 49-64.

Stirling, Paul. 1974. Cause, Knowledge and Change: Turkish Village revisited. In: Davis, John (ed.). *Choice and Change: Essays in Honour of Lucy Mair*. London: Athlone, pp.191-229.

Stirling, Paul. 1982. Social change and social control in Republican Turkey. In: *Papers and Discussions: Türkiye İş Bankası International Symposium on Atatürk*. Ankara: İş Bankası, pp. 565-600.

Stirling, Paul. 1988. Labour Migration and Changes in Anatolia. Edited version of unpublished paper delivered to the Conference on Mediterranean Migrations, University Al Charif Al Idrissi, Morocco, 11<sup>th</sup> -14<sup>th</sup> July. [http://era.anthropology.ac.uk/Era\\_Resources/Era/Stirling/Papers/AnatoliaMigration/anatolia\\_mig\\_1.html](http://era.anthropology.ac.uk/Era_Resources/Era/Stirling/Papers/AnatoliaMigration/anatolia_mig_1.html). Accessed 17.1.2023.

Stirling, Paul. 1994. Labour Migration in Turkey: Thirty-Five years of Changes. (Original paper presented to the International Scientific Conference; the Problems of Migration, Moscow 17<sup>th</sup> - 21<sup>st</sup> June 1991.) In: Altuntek, Serpil et al. (ed.). *HUMANA: Bozkurt Güvenç'e Armağan*. Ankara: Ministry of Culture. [http://era.anthropology.ac.uk/Era\\_Resources/Era/Stirling/Papers/YearsLabourMigration/35years\\_1.html](http://era.anthropology.ac.uk/Era_Resources/Era/Stirling/Papers/YearsLabourMigration/35years_1.html). Accessed 17.1.2023.

Stirling, Paul. 1992a. An Agrarian Society Industrialises: Turkey's Revolutions. Unpublished paper presented to the British Sociology Association conference. In the RAI archives.

Stirling, Paul. 1992b. Two Turkish Villages and their Emigrants: 1950-1986. Comments and Advice. Blurb by Stirling describing the research project. [http://era.anthropology.ac.uk/Era\\_Resources/Era/Stirling/blurb.html](http://era.anthropology.ac.uk/Era_Resources/Era/Stirling/blurb.html). Accessed 17.1.2023.

Stirling, Paul (ed.). 1993. *Culture and Economy: Changes in Turkish Villages*. Huntington; Eothen Press.

Stirling, Paul. 1996. Social Processes in the History of the Republic. Unpublished draft paper given at the Institut für Völkerkunde der Universität Wien, May 1996. In the RAI archives.

Stirling, Paul. 1996/97. Ménage à trois on a raft. In: *The Cambridge Journal of Anthropology*, special issue in Memory of Ernest Gellner 19 (2): 59-69.

Stirling, Paul. 1998. Credulity. Unpublished paper prepared for the ASA Conference "Indirection." [https://era.anthropology.ac.uk/Era\\_Resources/Era/Stirling/Papers/CredulityASA/credulity1\\_17.html#Section6](https://era.anthropology.ac.uk/Era_Resources/Era/Stirling/Papers/CredulityASA/credulity1_17.html#Section6). Accessed 17.1.2023.

Zeitlyn, D. 2000. Archiving Anthropology. In: *Forum Qualitative Sozialforschung* 1(3). <https://www.qualitative-research.net/index.php/fqs/article/view/1034>. Accessed 17.1.2023.

**Lale Yalçın-Heckmann**

## **Is There a Kurdish Village? Thinking with Stirling while Reflecting on Kurdish Rural Settlements**

Paul Stirling's *Turkish Village* has been seminal for anthropologists studying rural life in Turkey in the second half of the 20th century. I still have my copy of *Turkish Village*, dated with my father's hand writing in it, "Ocak 1965" (January 1965). As the publication date is also 1965, I assume Stirling must have given this copy himself to my father, Aydın Yalçın, whom he thanks in the Preface. In my childhood Paul Stirling was known to me as "Paul amca" (uncle Paul in Turkish) on his various visits to our Ankara home. The book was on my father's shelf and he must have given it to me when I started studying anthropology in London. The copy hence has my hand-writing and notes in it too, some of them from the time I read it for my own research, but some notes are for my teaching anthropology in Ankara and at various German universities. In this article, I re-visit Stirling's work to re-construct and re-assess what I have learned from him when I was studying Kurdish villages and where our main differences lie. Finally, I want to re-think such denotations as "Turkish" or "Kurdish" while identifying villages, whether these terms suggest any common patterns about the economy and social organization of rural life at the time and beyond.

Paul Stirling's *Turkish Village* has been a classic and must-read, since its publication, for anyone studying Turkish rural life. It has been a model for long stay ethnographic research, for understanding the villagers' lives and worlds, and for thinking about Turkish villages in the specific political and historical conjuncture of the late 1940s, early 1950s. Stirling has been of course also important for his re-study in the following decades, for tracing the lives of the villagers, writing on development and social change, examining these changes not only with macro-economic and demographic indicators but also thinking through cognition, knowledge systems and economic opportunities in internal and international migration (Stirling 1974, 1987, 1993). His work has been a starting point for many later Turkish and international anthropologists and sociologists (see for example Akşit 1985, Delaney 1991, İncirlioğlu 1993, Rasuly-Paleczek 1987, Schiffauer 1987, Shankland 1993, Sirman 1990, Strasser 1995, Yalçın-Heckmann 1991, among others). Stirling himself was aware of the existing rural research in Turkey (especially of İbrahim Yasa) and of the depictions of rural life in the works of the novelist Mahmut Makal. He was engaging with social science scholars and students as well as being involved in debates and critiques of the then existing models of rural change, as Bahattin Akşit (1993) documents in his overview. In this article, I reflect on several aspects of his arguments, methodology and findings and how these have had an impact on my own work.

### **Issues of Typicality and Representativeness**

Stirling recounts in the book how he came to Turkey and what kind of anthropological questions he had in mind. His theoretical background reflects an interest in questions related to



social organization, solidarity, lineage systems, and disputes—themes well developed in those years around the works of Durkheim by Africanists like E.E. Evans-Pritchard, M. Fortes and M. Gluckman, and by Middle Eastern and Mediterranean researchers like E. Peters and J. Pitt-Rivers. His research can be well placed in the era where focus in anthropological research changed from tribal systems to peasant studies (cf. Harris 2005); he was exploring kinship and lineage systems as well as peasant economy, embedded into national and international economies and political systems, and within the context of nation-state building and modernization models.

He writes that he wanted to find a “small and relatively closely-knit society” (Stirling 1965: 3). To what degree such a location would be representative of the whole was an important issue for him: “Although it was not possible to choose a ‘typical’ village, because no such thing exists, it was at least possible to avoid choosing villages with obvious peculiarities. I set out to find an orthodox Muslim, Turkish-speaking village of modest size, fairly far away from the direct influence of the cities, on the plateau which forms the largest part of Anatolia” (ibid.).

This issue of “typicality” is a point which he comes back to at the end of Chapter 2 (ibid.: 25). Here Stirling resists the pressure of having to say something about the typicality of the villages he studied, and defends himself by stating that he was on his own and could not have studied five (or twenty-five) different villages. This is the sociological rule of thumb about the size of the sample and its analytical strength. Anthropologists, as Stirling says, try to offer “a model of social structure of a community” (ibid.) and the model can throw light on other similar communities. All this follows well the theoretical debates and problems in British anthropology of those decades (see Barth 2005: 22-31).<sup>1</sup>

In the 1980s, when I initially intended to study the “Kurdish political identity and tribal structures,” I was also concerned about how to choose a field site for my research. I wanted to stay in a village in Hakkari, following the common-sense logic of anthropological methods at the time, where I was expecting to “cover a community” as fully as possible. Hakkari as a region was commonly referred to as the least assimilated region into Turkish politics and culture, due to its physical landscape, underdevelopment and peripheral location bordering Iraq and Iran. Security was another point of concern for choosing the field site. In the late 1970s Turkey was going through extreme political polarization and there was armed conflict between various militant political fractions of left and right. So, I wanted to stay in a place for at least a year – in the Malinowskian tradition – which would be least effected by the conflict. A mountain village seemed more suitable than a plains one or the town of Yüksekova, as it would be less frequented by and accessible to non-villagers, political activists or state authorities. It took some months before I had an open invitation to stay in the mountain village of Sisin, and the circumstances of settling in the village certainly shaped my access to and extent of knowledge on, tribal politics, kinship relations and village economy.

Comparatively speaking, therefore, Stirling’s and my reasons for choosing the rural settlement for the Turkish and Kurdish rural lives show similarities as well as differences. For selecting

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<sup>1</sup> Interestingly enough these decades marked also a change to, and interest in, the “ethnographic moment” in Turkey, as Ali Sipahi (2021: 385) writes. If the Republican era wanted to “enlighten” the peasants, the new era (of multiparty political system) aimed to “grasp the reality of peasants through fieldwork.” Niyazi Berkes, Behice Boran, İbrahim Yasa and Mübeccel Kıray were the pioneers of these early fieldwork-based peasant studies, followed by Paul Stirling as the first foreign social scientist to carry out such a study (ibid.). Nevertheless, Sipahi adds, peasants had already been a subject of study by folklorists since the 19<sup>th</sup> century, as they were in European countries.

the particular villages of Sakaltutan and Elbaşı, Stirling adds the “unscientific” reason that the villages were close to the American school and clinic in Talas for eventual support (Stirling 1965: 3). Hence, ethnographic conventions and theories of the time, common-sense logic and security reasons as well as serendipity all added to the particular choice of the village for both Stirling and myself.

## **The Integration of the Center and the Rural Periphery**

Stirling stayed in Sakaltutan from November 1949 to August 1950, his wife Margaret joining him in March. In 1951 both were in Elbaşı from August to November (see Stirling 1965: 3).

In the first chapter Stirling embeds the rural field site in history and theoretical arguments, and presents a rough overview of the historical development of Turkey from the Ottoman Empire to the modern Republic. His emphasis is on the changes which were abrupt, fundamental and top-down for the villagers. He is particularly attentive to the social, political, and cognitive distance of the villagers to the city and how the administrators were all educated yet alienated from the villagers. The physical distance (especially Sakaltutan’s remoteness, access being hindered due to snow for two months in winter) was also a protective aspect for the villagers from the control and imposition from the center of power. Stirling summarizes this in his last sentence of the chapter: People did not oppose the reforms of Atatürk because they “did not know, or did not understand, or did not care what the central government was doing” (ibid.: 13). Hence, he connects the physical distance and remoteness of the village to broader issues like how integrated they are into the new republican state as well as how much chance they had for cognitive development.

My reasons for choosing a mountain village with restricted access also implied limited access of villagers to state institutions and services, as well as unevenly distributed knowledge about the Turkish state and its political system. The village Sisin was accessible via a footpath, which took about an hour’s walking. The path started at the end of the main vehicle road, but avalanches and deep snow cut off the road from the main town and from other hamlets for some weeks and months in winter. Gender and age differences caused the uneven knowledge about the Turkish state. Men acquired knowledge and learned Turkish primarily through the military service. Women hardly left the tribal area and its villages; they rarely went to town, at most – and mostly older women – for visiting the hospital in case of severe illness. For instance, all births took place in the village. Villagers did not have any health services; I learned how to vaccinate the children and do injections and offered this help for the villagers during the last months of my stay.

## **The Materiality of Village Life: Houses and Environment**

Stirling describes the topography of Sakaltutan and Elbaşı, the situation of cultivable land and climatic conditions, all helping the reader understand the organization of the rural economy, as well as settlement patterns and work lives. His description of the houses is striking for illustrating the level of development in general. The houses which had an open *tandır* oven in the middle resemble the houses described for plains Kurdish villages in Hakkari of the 1970s and 80s, as I frequently heard during my research. The household goods, chores, gendered

division of labor and the arrangement of the inner rooms all resonate my experiences of the village households and life during my fieldwork in Hakkari in the 1980s. I also described at length the physical conditions of the mountain village house, its construction, the material goods in it, as well as its spatial and temporal use, mostly following gender, age and social hierarchies, the variations following the semi-nomadic pattern of animal herding and agricultural economy (see Yalçın-Heckmann 1991, chap. 5: “The village and the village household”).

One important difference relates to the demography and topography, which are in the background of Stirling’s discussion and in my work: The average household population and the use of space within the house differed substantially between the Turkish and Kurdish cases. In “my” Kurdish village case, the households had a higher average population and in none of the ordinary houses did I come across a “guest room” (*oda* in Stirling’s Turkish village case) which was used solely or primarily by men and guests. In the 1980s, Kurdish mountain-village households had only one main room that was used by both household members and guests; the usage time, manner and occasion was dependent on the number, gender and rank/prestige of guests. Only wealthier and higher status households (as I saw in a few other mountain villages, such as the house of the tribal leader, *ağa*, of the *muhtar*, or of a religious leader like a *seyyid*) had a separate guest room. I once visited, and was hosted by, the former tribal leader’s son in Oramar—a larger village which formerly had been a district center. His wife prepared the guest room for me which had armchairs, a table and a bed, something I had not seen in any other village house until then. The room was apparently for the use of “respected” guests alone, all the furniture was covered by cloth, to keep them clean when unused. Commonly, guests of an “average” household would be hosted in the main room of the house, sharing it with the household head for socializing and taking meals during the day, and with all the other household members for spending the night.

## What is a Village? Aspects of Integration into the Modern State

Stirling comments on the founding of Sakaltutan based on the oral memory of elderly people; that they can remember up to six generations above them (Stirling 1965: 23). Here, he estimates some 200 years to be approximately the time since the founding of the village; Elbaşı apparently being older. Stirling concludes the chapter by underlining that Sakaltutan was “still completely a village” (ibid.: 24), meaning people’s life-worlds were composed of the village, even if some had seen and been to other places. The effect of education (only primary school, almost only men, with some three years of attendance) was still negligible. Elbaşı was again somewhat different, more sophisticated (ibid.).

The mountain village Sisin, where I spent most of my fieldwork time hence resembles Sakaltutan in these aspects as well; but there was no school, village men had learned Turkish and some reading and writing during their military service and no village women spoke Turkish, nor had been to school. Again, unlike Sakaltutan or Elbaşı, there had been no labor migration from Sisin until the time of my research, a situation which changed dramatically since the 1990s. Stirling, having done fieldwork in two villages, contrasts Sakaltutan with Elbaşı in terms of infrastructural and developmental indicators and concludes that the effects of the variation are not significant; on the contrary, “Elbaşı’s greater wealth, education and outside contact seemed to make surprisingly little difference to the way of life of the majority of its population, and the two villages were for the most part remarkably alike” (ibid.). Again,

this puzzle is linked to the modernization and development models and discussions of the time. Stirling is cautiously probing: When do the villagers become cognitively modern citizens of an economically developed and modern nation-state?

During my research I collected quantitative data only in Sisin. I had however similar queries about the possible effects of lacking infrastructure and state institutions in the settlement. Were Sisin villagers more distant to the Turkish state because of the lack of any state institution there? Was it because they had no experience of labor migration outside the small tribal region? This was at least the opinion of many outsiders; other co-tribals, other villagers and townsmen often referred to Sisin villagers as being naïve and/or ‘untouched’ (*saf* in Turkish, meaning unassimilated, “original” or “authentic”, depending on the speaker’s perspective). Often the villagers’ conspicuous religiosity was the ground for this depiction; Sisin villagers all followed the rules of religious practice, from five-time daily prayers to fasting during Ramadan. This reputation of religiosity and remoteness (in the eyes of the non-tribal and local outsiders) combined protected them from the central state’s close scrutiny and control of political insurgency until the 1980s.<sup>2</sup> Hardly any local state official came to the village during my stay. Only after the 1980 military coup the political situation changed also in the village, a development I was able to observe the beginnings of and describe in my book with the story of “The case of Sisin’s guns” (Yalçın-Heckmann 1991, chap. 1, pp. 27-38).

Stirling pays much attention to the village economy and agricultural “mode of production.” Accordingly, in Sakaltutan (and less so in Elbaşı) subsistence economy was dominant,<sup>3</sup> albeit with some degree of integration into the state and market led economy, especially in terms of interdependency with the surrounding towns (see Stirling 1965, chapter 4: “The village economy”). In Sisin, pastoral herding economy was the dominant mode of production and I discussed this economy at length in chapter 6 (Yalçın-Heckmann 1991). Similar themes of domestic production, inheritance, gendered division of household labor, reciprocity and exchange within the village as well as between villages dominate both Stirling’s discussion and my own work. Nevertheless, Stirling seems to be more interested in themes of change and mobility, even if he was recapitulating how villagers were identifying themselves with the village as no other social unit.

In my research, I was only partly attentive to this kind of village life-world orientation. People, men and women seemed to have a rural orientation and self-identification as “a village of honest people,” especially when their collective identity had to be defended against the outsiders, be it other co-tribals, other tribesmen or Turkish officials. But internally, and looking inwards, some households and lineages were seen as having a weaker or less collective sense. This was the background and the explanation for the existence of two *zomas* (camp units, in Kurdish) and two corresponding collective herds within the village. The lack of collective will or solidarity, however, was not over-stressed; the demographic pressure on pastures used by this village, with household population growing since the 1960s was also counted as an

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<sup>2</sup> Remembering that the militant political fights were going on in Turkey of late 1970s almost everywhere, the few political insurgency cases relevant for Hakkari at the time involved Kurdish guerrilla fighters from Northern Iraq, supporters of Talabani and Barzani factions. These had a military clash within the Turkish borders, not far away from Sisin. However, PKK or other Kurdish factions within Turkey had not yet any significant political or military activity in Hakkari at the time.

<sup>3</sup> Stirling states that the “villagers exaggerated this self-sufficiency, and liked to emphasize their independence; in their still traditional view of the world, the land gave an assurance of survival which such unreliable sources of income as a government salary did not” (1965: 79).

important reason for dividing the *zomas* as the size of the herd increased.<sup>4</sup> The division into two *zoma* groups also followed the lineage alliances and affinal relations.

What I did not explore at any length at the time was the fact that Sisin had formerly been a Nestorian settlement,<sup>5</sup> and that the first Oramari settlers received state supported settlement documents around the 1950s with the right for permanent land usage. The village lands belonged to the category of “unused and abandoned settlement” (*metruk*), a legal concept often used for formerly Christian property. The history of such former Christian (Armenian and Nestorian) settlements and their legal context have been studied in the late 1990s and 2000s (see Wiessner 1997, Üngör and Polatel 2011). I did not dwell on the possible effects of this state allocation of land, how this might have influenced the internal and subsequent relations of hierarchy and economy. Which household had which land within the topography of the village did not seem to matter too much at the time, other than being close to or distant from the fresh water spring, upon which there were occasional quarrels between women.

Another issue for comparative thinking is the size of the rural settlement; that Stirling’s villages were at the time classified as village (*köy*), whereas many Kurdish rural settlements belonged primarily to the administrative category of hamlets (*mezraa*), like Sisin. This, for me, partially explained the lack of state administrative authorities in Sisin, as a settlement. There was no village head (*muhtar*), no council of elders (*köy ihtiyar heyeti*), no guard or watchman (*bekçi*), no village school or any other administrative and infrastructural person, place or institution. The embeddedness of Sisin as a hamlet within the larger tribal space and organization led me pay more attention to economic, political, kinship and practical relations beyond Sisin.

## Concluding notes

Stirling’s analysis of the village entails an analytical tension and unresolved ambivalence between being “typical” for Turkish villages yet distant “from the center,” being “far away” from urban centers and urban effects and yet integrated into the nation-state polity. In my own research on the Kurdish village there was a similar tension and ambivalence between being a typical Kurdish village or rural hamlet and accounting for its distance from the political center. There were other aspects to consider as far as the relationship between the political center and the village was concerned. Mainly because it is in a border region (hence of concern of sovereignty/control from the perspective of the Turkish central state), but also a tribal village (i.e., inhabited by tribal villagers and seen as belonging to a tribally claimed territory), the question of its typicality becomes complicated.<sup>6</sup> The latter factor enabled Sisin to be loosely integrated into the state polity and yet to maintain significant political ties within and beyond the tribe. The tribal framework meant that the villagers would engage in tribally and traditionally claimed and legitimized – state acknowledged – rights to hay cultivating areas, tribal pastures and tribal decision-making processes. Nevertheless, at the local (hamlet) level

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<sup>4</sup> The village of Sisin was re-settled by some seven households of the Oramari tribal Kurds around the mid-1940s. Around 1975 there must have been a division of the camp units into two, making two *zomas*. I interpreted this as an outcome of population growth and the need for managing a larger herd, hence two *zomas* instead of one would make a more efficient use of the pastures around the village.

<sup>5</sup> Villagers knew that there had been a small Nestorian chapel in the place of the small mosque there.

<sup>6</sup> For more on this issue, see Chapter 4 of my book *Tribe and Kinship among the Kurds* (1991).

the tribal system could still be challenged and re-defined on the basis of being a village community.

In the last four decades the embeddedness of the village in a nation-state polity or a tribal political landscape have not been themes for intensive ethnographic research. Rather, the decline of peasantry and traditional agriculture in general (Keyder and Yenal 2013, Öztürk 2012) and the evacuation of villages and deportation of villagers in Kurdish areas (Jongerden 2007, Kurban et.al. 2007) have dominated the literature. With that, discussing the typicality of rural life and village structures seems to have become – to my regret – irrelevant.

The question of similarities between Turkish and Kurdish villages remains open for further research; the deportations, however, seem to mark the major difference between them. Finally, even if deportations of Kurdish villagers are the major reason for the “end of Kurdish rural settlement” in this region, paradoxically the outcome of urbanization, migration and modernization is very similar to the fate of Turkish villagers as Stirling commented in his later work.

## References

- Akşit, Bahattin. 1985. *Köy, Kasaba ve Kentlerde Toplumsal Değişme*. Ankara: Turhan Kitabevi.
- Akşit, Bahattin. 1993. *Studies in Rural Transformation in Turkey 1950-1990*. In: Stirling, Paul (ed.). *Culture and Economy: Changes in Turkish Villages*. Huntingdon: Eothen Press, pp. 187-200.
- Barth, Fredrik. 2005. *Britain and the Common Wealth: Malinowski and Radcliffe-Brown, 1920-1945*. In: Barth, Fredrik, André Gingrich, Robert Parkin and Sydel Silverman (eds.). *One Discipline, Four Ways: British, German, French, and American Anthropology*. Chicago, London: University of Chicago Press, pp. 22-31.
- Delaney, Carol. 1991. *The Seed and the Soil: Gender and Cosmology in Turkish Village Society*. Berkeley: University of California Press.
- Harris, Mark. 2005. *Peasants*. In: Carrier, James (ed.). *A Handbook of Economic Anthropology*. Cheltenham, UK: Edward Elgar, pp. 423-438.
- İncirlioğlu, Emine Onaran. 1993. *Marriage, Gender Relations and Rural Transformation in Central Anatolia*. In: Stirling, Paul (ed.). *Culture and Economy: Changes in Turkish Villages*. Huntingdon: Eothen Press, pp. 115-125.
- Jongerden, Joost. 2007. *The Settlement Issue in Turkey and the Kurds: An Analysis of Spatial Policies, Modernity and the War*. Leiden: Brill.
- Keyder, Çağlar and Zafer Yenal. 2013. *Bildiğimiz Tarımın Sonu, Küresel İktidar ve Köylülük*. İstanbul: İletişim Yayınları.

Kurban, Dilek, Deniz Yüksek, Ayşe Betül Çelik, Turgay Ünal, and Tamer A. Aker (eds.). 2007. *Coming to Terms with Forced Migration: Post-Displacement Restitution of Citizenship Rights in Turkey*. İstanbul: TESEV.

Öztürk, Murat. 2012. *Agriculture, Peasantry and Poverty in Turkey in the Neo-liberal Age*. Wageningen: Wageningen Academic Publishers.

Rasuly-Paleczek, Gabriele. 1987. *Der Wandel der traditionellen Wirtschaft in einem anatolischen Dorf*. Wiener Beiträge zur Ethnologie und Anthropologie, Horn-Wien: Berger.

Schiffauer, Werner. 1987. *Die Bauern von Subay: das Leben in einem türkischen Dorf*. Stuttgart: Klett-Cotta.

Shankland, David. 1993. *Alevi and Sunni in Rural Anatolia*. In: Stirling, Paul (ed.). *Culture and Economy: Changes in Turkish Villages*. Huntingdon: Eothen Press. pp. 46-64.

Sipahi, Ali. 2021. *Ethnographic Authority and Public Culture in Turkey in the 1950s*. In: *History and Anthropology* 32:3 381-403.

Sirman, Nükhet. 1990. *State, Village and Gender in Western Turkey*. In: Finkel, Andrew and Nükhet Sirman (eds.). *Turkish State, Turkish Society*. London: Routledge, pp. 21-51.

Stirling, Paul. 1965. *Turkish Village*. London: Weidenfeld and Nicolson.

Stirling, Paul. 1974. *Cause, Knowledge and Change: Turkish Village Revisited*. In: Davis, John (ed.). *Choice and Change: Essays in Honour of Lucy Mair*. London: Athlone Press, pp. 191-229.

Stirling, Paul. 1987 [1971]. *A Turkish Village*. In: Shanin, Theodor (ed.), *Peasants and Peasant Societies: Selected Readings*. Oxford: Basil Blackwell, pp. 36-44.

Stirling, Paul. 1993. *Introduction: Growth and Changes – Speed, Scale, Complexity*. In: Stirling, Paul (ed.). *Culture and Economy: Changes in Turkish Villages*. Huntingdon: Eothen Press. pp. 1-16.

Strasser, Sabine. 1995. *Die Unreinheit ist fruchtbar: Grenzüberschreitungen in einem türkischen Dorf am Schwarzen Meer*. Wien: Wiener Frauenverlag.

Üngör, Uğur Ümit and Mehmet Polatel. 2011. *Confiscation and Destruction: The Young Turk Seizure of Armenian Property*. London: Continuum.

Wiessner, Gunnar. 1997. *Hayoths Dzor - Xavator: ethnische, ökonomische und kulturelle Transformation eines ländlichen Siedlungsgebietes in der östlichen Türkei seit dem 19. Jahrhundert*. Wiesbaden: Reichert.

Yalçın-Heckmann, Lale. 1991. *Tribe and Kinship among the Kurds*. Frankfurt/M.: Peter Lang Verlag.

## **Gabriele Rasuly-Paleczek**

### **Turkish Village - a blind spot in contemporary field research: What's going on in rural Turkey today?**

Unlike most of the contributors to this volume, I did not have the privilege of knowing Paul Stirling well or working with him for any length of time.

Originally, I knew Paul only through his publications, in particular through his seminal monography "Turkish Village" published in 1965. Studying his research on the two central Anatolian villages Elbaşı and Sakaltutan, was a must for any anthropologist preparing field research in rural Turkey, so also for me. By reading Paul's work I received many inspirations for developing the focus of my doctoral thesis. The latter eventually dealt with the impact of labour migration on the economy of a rural community situated in the mountainous region of the Bursa Province in western Anatolia (cf. Paleczek 1987 and Rasuly-Paleczek 1993).

I first met Paul personally in 1982 at the conference "Society and Politics in Turkey" organized by the Institute for Comparative Social Research in Berlin. Our second encounter took place when I attended the "Culture and Economy: Changes in Turkish Villages" conference Paul had organized in June 1990 at the School of Oriental and African Studies (SOAS), University of London.<sup>1</sup>

It was not until the mid-1990s that a closer collaboration with Paul finally came about. This cooperation dated back to the third EASA Biennial Conference in Oslo in 1994. At Paul's initiative, several researchers working on Turkey at the time met, including, if I remember correctly, David Shankland and Hülya Demirdirek.

During this joint dinner, we discussed current politics in Turkey and the impact of Kemalist reform policies. Paul suggested the organization of a multidisciplinary conference or workshop to investigate the link between Kemalism and the processes of social transformation in the history of the Turkish Republic.

David Shankland and myself got excited about the idea and started raising funds to turn this idea into reality. Luckily, we were both successful and so in 1996 two meetings took place. One in May in Vienna and the other one in December in Lampeter, where David was then teaching at the University of Wales.<sup>2</sup>

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<sup>1</sup> The results of that conference were later published by Paul with the same name (Stirling 1993 a).

<sup>2</sup> The Vienna Meeting on "Social Processes in the History of the Turkish Republic" took place from May 17<sup>th</sup> to 19<sup>th</sup> 1996. Paul and myself had planned to publish a co-edited volume of the papers presented at that conference. However, his sudden death in May 1998 stopped this endeavor. Following Paul's death Michael Fischer from the Centre for Social Anthropology and Computing at Canterbury University, United Kingdom expressed his willingness to collaborate in publishing the workshop papers. Yet, despite his commitment the enterprise met a number of obstacles. The most crucial of all was that no final version of Paul's contribution to the volume or other notes authorized by him for publication were at our disposal. As Margaret, Paul's widow, informed me, he had



Both conferences dealt with a topic that, as far as I can gather from the correspondence with Paul at the time, was burning under his nails. Over the years he had developed a deep interest in analysing the tremendous processes of transformation that had taken place since the Turkish Republic had been founded in 1923, in particular those that were triggered by the mass emigration of its rural citizens.

In contrast to most other scholars who studied the social history of the Turkish Republic from a top-down perspective, Paul advocated for a bottom-up perspective. While they researched Kemalism, Atatürk's reform policies, the institutional framework of the Turkish government, etc., Paul was more interested in the perceptions and reactions of those that had become the target of Atatürk's modernization efforts. Therefore, he focused on questions like: What exactly did the people (e.g., the villagers) know about the Kemalist modernization project? How did they talk about it? What kind of strategies were developed by ordinary Turkish citizens to deal with the rapid changes they were confronted with? Did the Kemalists possess the necessary human resources and institutional capacities to promote their modernization project in the rural areas of the country?<sup>3</sup>

A second topic that occupied him and is closely related to the first, was his great interest in studying the effects of these transformation processes on the affected villagers through detailed empirical investigations. Long before the "multi-sited ethnography approach" was popularized by authors such as Marcus (1995) and other anthropologists, Paul was following "his villagers" from Elbaşı and Sakaltutan, to their new abodes.<sup>4</sup>

In order to study both topics – the transformation processes at large in Turkey as well as the changes that occurred in villages he knew – he had outlined his ideas in several papers and developed a kind of grid to study them. To this effect, he advocated comprehensive and detailed empirical data collection, preferably by social anthropologists (for details cf. Stirling 1974, 1981, 1993 b, 1994, 1996).

Our interests overlapped as we were both particularly interested in studying the impact of emigration on rural areas of Turkey. Like Paul, I had an interest in ethnographic studies in Turkish villages. Like him, I was particularly interested in examining the transformation processes that were triggered by emigration in the rural communities of Turkey.

But while Paul undertook his first fieldwork in the late 1940s and early 1950s and his re-studies in the early 1970s and mid 1980s, I didn't follow up my initial fieldwork until decades later. By the time I started my first field research in the mid-1970s in a so called *orman köy* (forest

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still been working on his paper briefly before he died. However, the workshop and the preparation of the edited volume had heavily relied on his inspiration and his persistent questions on the nature of social transformations in Turkey, particularly the Kemalist reform model. So finally, to our great regret we had to give up the plan to publish the contributions of the Vienna conference.

The Lampeter Conference on "New Approaches to Contemporary Turkey" took place on December 7<sup>th</sup> and 8<sup>th</sup>, 1996. Both meetings were attended by anthropologists, political scientists, historians, economists and geographers.

<sup>3</sup> Paul had posed several of these questions in a letter that had been sent to all participants of the Vienna conference. For further details on his thoughts see Stirling 1974, 1981, 1988, 1994 and 1996.

<sup>4</sup> Following his long-term field research between 1949 and 1952 together with his wife, Paul revisited his original field sites again, in the early 1970s, albeit only for short term visits. In 1985 and 1986 he conducted another long-term field research, this time in cooperation with Emine Onaran İncirlioğlu, Hülya Demirdirek, Abdulvahap Taştan and Mehmet Arıkan. For details on the research results cf. Stirling 1974, 1988, 1994 and -İncirlioğlu 1993 and respective chapters in this publication.

village) in the western Anatolian province of Bursa, the rural exodus was already clearly visible.<sup>5</sup> Of the original 115 farmsteads only 72 were still permanently inhabited in the late 1970s and early 1980s. 14 were used by part time migrants who cultivated their village land. Another 27 buildings were vacant and 2 had already become uninhabitable (cf. Paleczek 1987:34 and Rasuly-Paleczek 1993:106).

In 2012, when I began my re-visits the village was almost deserted, at least from fall to spring.<sup>6</sup> While there were 314 permanent residents and another 55 semi-permanent residents in the village in 1980, who were still engaged in agriculture, animal husbandry and silk cocoon production, their number had dwindled to some 30 to 50 people, mostly elderly women and retired couples that lived permanently in the village using their house gardens and some of their irrigated land for food production (mostly vegetables), while their former grain fields remained untended /uncultivated.<sup>7</sup>

Many more former farmsteads had in the meantime fallen apart. Numerous house gardens – once intensively cultivated with vegetables and mulberry trees – were overgrown with bushes and shrubs. The forests surrounding the village, which used to be heavily cut, have recovered and now cover much of the former pastureland. Somehow the village gave the appearance of a vanishing place.

What struck me most during my re-visits were the many new concrete houses with balconies, thermo-windows and satellite dishes that had been erected or were under construction. The interior of the village houses had also changed. All houses now had electricity and running water, indoor toilets and showers, modern furnished kitchens with refrigerators, microwave ovens, washing machines etc., and bedrooms with real beds instead of mattresses folded away during daytimes.

Some people had even built new cattle sheds or had made investments to cultivate their land (e.g., buying tractors, installing automatic interval timer irrigation systems or hoses for irrigating their house gardens or vegetable plots). While some garden plots and fields were uncultivated, others were intensively used to grow vegetables or alfalfa and other fodder to feed a growing number of milk cows.<sup>8</sup>

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<sup>5</sup> The term *orman köy* refers to villages situated in areas covered by forests, often impassable and mountainous. According to Talım (1985: 6) approximately 17.000 rural communities were categorized as *orman köy* in the 1980s. Their roughly 9 million inhabitants belonged to the most underdeveloped segment of Turkish society. These rural communities constituted the main emigration areas of Turkey at the time.

<sup>6</sup> Since 2012 I have re-visited the village and its migrants in Bursa and Gemlik several times for brief periods. My last visit took place in the fall of 2019. So far, my own re-study is a work in progress. I have presented some preliminary results at various conferences and in a short paper (Rasuly-Paleczek 2019).

<sup>7</sup> Emigration has also provoked alterations in the composition of households and the division of labor. During my first visits some 314 persons were permanently living in the village, constituting 72 households. Another 55 people were residing in 14 semi-permanent households. 31 percent of the 72 permanent village households consisted of married couples only (mostly elderly couples whose children had all migrated). 20 percent were nuclear families and 7 percent were married couples with other unmarried relatives. Various forms of extended households constituted another 22 percent and 11 percent were fragmented households of widows, bachelors etc. (Paleczek 1987:72-74 and Rasuly-Paleczek 1993:105-107).

<sup>8</sup> Keeping small crowds of milk cows is a rather new phenomenon. It started around 2013 when several returning migrants bought Dutch cows on state promoted credits.

What also puzzled me was the constant flow of people to and from the village – at least from spring to fall - some commuting between Bursa or Gemlik and the village on a daily basis or on weekends, others residing several weeks or months in a row.

This mobility is facilitated by infrastructural investments that had taken place during the last years. While the community had been a remote place in the past, often inaccessible due to lack of means of transportation (only two minibuses, often out of order and one privately owned pick-up truck) or landslides that had destroyed the gravel road after heavy rains in the past, it was now easy to travel to and from the village. Several new asphalted roads had been built (the last one in 2015) to connect the village with the outside world. Most villagers and migrants owned cars. In addition, a shuttle bus offered services three times a day from Gemlik to the village and back to town allowing people to just come to the village for a short visit, e.g., to irrigate their garden plots.

How can these somehow contradictory phenomena – decreasing number of permanent residents, ongoing construction of new houses, investment in modern agricultural tools etc. – be understood? What does the village mean to its residents and migrants? Is it still a place to make a livelihood or is it a place of recreation or retirement? Is it the nostalgic place of people's youth, a place of belonging and individual and collective identities? And finally, is what I have observed in that village specific to this village only or is it typical for rural Turkey today? These are some of the questions that bother me and that might also have been of interest for Paul.

I cannot offer any detailed answers yet as my own research is still a work in progress. However, the demographic data for Turkey and some recent studies (Kavak 2012, Jongerden 2022; Öztürk, Jonderden and Hilton 2014a, Öztürk, Jonderden and Hilton 2014b, Öztürk, Jonderden and Hilton 2018a, Öztürk, Topaloğlu, Hilton and Jongerden 2018 b and Öztürk, Jonderden and Hilton 2022) suggest that many other rural communities must have been exposed to similar developments.

With currently around 76 % of her total population of 84,7 million living in urban areas the demographic structure is now upside down compared to the beginning of the Turkish Republic.<sup>9</sup> Moreover, proportionally more older people live in the villages than in the urban regions.

Despite these dramatic changes, hardly any detailed studies – let alone anthropological monographs – exist, that could offer concrete information about the effects of this development in the rural areas. And since only very a limited number of village re-studies were conducted – especially not during the last few years – we lack data on concrete examples of the extensive transformation processes that have taken place in recent years and decades. In other words, rural Turkey became a blind spot of research, in particular of social anthropological studies.

So, the question arises, what is going on in the villages of Turkey? How has the emigration of large parts of their population affected agricultural production? What significance do the villages have today for their current and former inhabitants? And many other questions that Paul – if he were still alive – would certainly have asked.

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<sup>9</sup> According to the 1927 census data, the first published after the founding of the republic, Turkey had a population of 13,6 million. Of these roughly 76% lived in rural areas and only 24% in urban centres. Cf. figures for 1927 from T.C. Başbakanlık Devlet İstatistik Enstitüsü 1992: 36, figures for 2021 from O'Neill 2023.

Before I turn to the question of how the current transformation processes in the rural areas – in particular their depopulation – can or should be examined, I would first like to give a brief overview of how social scientists, in particular social anthropologists, have dealt with Turkey's rural areas and the effects of emigration in the past.<sup>10</sup>

The first village studies were conducted in the 1930s, 1940s and early 1950s, among them the pioneering works by Niyazi Berkes (1942), Behice Boran (1945), Nermin Erdentuğ (1956 and 1959), İbrahim Yasa (1957) and Paul Stirling (1949 – 1952, published in 1965). They were followed by many more studies in the years to come.

Originally, many of these monographies – as was common in social anthropology at the time – had a holistic approach, i.e., they depicted a village in all its aspects (i.e., kinship, marriage, material culture, religious ideas and practices as well as other local customs, relations to the outside world, etc., cf. Stirling 1965, Erdentuğ 1956 and 1959, Yasa 1957).

Eventually these holistic monographies were replaced by more specific ones. Some scholars began to examine in greater detail a single complex within the context of the overall culture and society in a particular village or region (e.g., Meeker 1970, on “honour and shame”, Aswad 1971, on social strategies for property control by women). Others combined a general theory or model with their specific field data, as e.g., Schiffauer (1987) who used Marcel Mauss's concept of “gift exchange” to explain the social relations in the village he studied. As a result of the greater influence of feminist approaches, increasing attention was also paid to the gender aspect (e.g., Delaney 1992).

In view of the social and economic changes in rural Turkey more problem oriented studies were also conducted. Often a particular village or region was chosen to address a specific problem (e.g., effects of mechanization cf. Hinderink and Kiray 1970, judicial conflicts over land e.g., Starr 1978, to mention just a few).

With growing mass migration that started in the late 1950s and reached a first peak in the mid-1960s, rural migrants in the towns and cities, later on also in foreign countries, became the major focus of research. Most earlier studies dealing with emigration focused on the reasons for emigration, the process of migration (e.g., seasonal, permanent, chain migration etc.) and the problems rural migrants were confronted with in their new places of residence, either in urban areas of Turkey or abroad (cf. Erman 1993 and 1997, Abadan-Unat 2009). In particular, the so-called *gecekondu* became a major subject of research.<sup>11</sup>

Until quite recently many of these studies viewed the rural migrants in cities through the prism of an urban-rural or modern-traditional dichotomy (cf. Birkalan-Gedik 2011: 11 and 14; 2018: 6224). Rural migrants are often seen as aliens who cannot integrate into modern urban life (cf. Erman 1993 and 1998, Fliche 2004, Kaya 2015). In this context, reference is often made of the

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<sup>10</sup> This is not the place to present a detailed list of all the work done or offer detailed information on how rural communities and their connections to the city are being addressed. For links, cf. Birkalan-Gedik 2011 and 2018, Erdentuğ and Magnarella 2001, Magnarella and Türkdoğan 1976, Olson 1986. In the following, I will only go into more detail on those studies that are relevant to my argument.

<sup>11</sup> According to Birkalan-Gedik (2011:16) and Fliche (2007:15), the *gecekondu* studies increasingly replaced village monographs from the 1960s onwards. For details on the *gecekondu* cf. Karpas 1976, Wedel 2004 and Erman 2001.

ruralization, “Anatolization” or “villagerification” of Turkish cities, especially İstanbul (cf. Planck 1974 and Fliche 2007).

While the large number of *gecekondu* and other emigration studies provide us with ample information on the migration process and the live of rural migrants in Turkish cities and abroad, the impact of this mass exodus on the migrants’ places of origin remained a stepchild of research in the past.

Only a very limited number of studies dealt in detail with the effects of internal or external migration on the villages or regions of origin of the migrants (cf. Abadan-Unat, Keleş, Penninx, Van Renselaar, Van Velzen and Yenisey 1976, Magnarella 1979, Paleczek 1987, Rasuly-Paleczek 1993, Struck 1984 and 1988, Stirling 1974, 1988 and 1994; and more recently Fliche 2007).

Many of the few existing studies focused on the importance of socio-economic networks that link the neo-urban population with their former village (e.g., mutual support, flow of resources, brides married off to the city) (cf. e.g., Stirling 1994:33 f, Çelik 2009: 26-28, 37-51, Fliche 2005 and 2007). Much less attention was paid to the effects of emigration on the agricultural production in the migrants’ village of origin (exceptions are e.g., the studies by Olson 1995, Magnarella 1995, Struck 1988, Paleczek 1987, Rasuly-Paleczek 1993).

What is more or less completely absent are in-depth re-studies of rural communities that would allow us to grasp the effect of emigration from a historic comparative perspective. One of the few available sources are several articles Paul published following his re-visits to Sakaltutan and Elbaşı of the Kayseri province, in the early 1970s and mid-1980. Unfortunately, they do not offer a detailed analysis of the changes that had taken place since his first visits in the late 1940s and early 1950s. Paul rather summarized some of the changes he found most noteworthy to mention, such as improvement of infrastructures (e.g., roads, schools, health services), extensive links between villagers and migrants, changes in worldview, social control and gender roles, greater mobility of village population etc.<sup>12</sup>

Due to the focus of most studies on the reasons for emigration and the challenges the new migrants in Turkish cities and abroad were confronted with, the impact of emigration on the rural areas itself remained a neglected topic.

It was only since the 2010s when the impact of neoliberal agrarian policies became increasingly visible, leading amongst others to a renewed large-scale emigration of the rural population, that a re-focusing on the country’s villages took place (cf. Öztürk 2012), yet on a rather limited scale, in particular with regard to social anthropological studies.<sup>13</sup>

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<sup>12</sup> Other available restudies include Yasa’s restudy of Hasanoğlu (1969; his first study was published in 1955) and Olson’s restudy of 1995 of two villages she had studied between 1964 and 1966 in the province of Burdur.

<sup>13</sup> Since Paul passed away the rural areas of Turkey have gained new momentum. Some researchers now refocus on the role of the village institutes (*köy enstitüleri*) (cf. Ünal 2019), others deal with the representations of Turkish peasants in literature (cf. Karaömerlioğlu 2002 or Yazıcı Yakın 2007), Kemalist efforts to modernize rural communities (cf. Sezer 2022), rural social movements (cf. Ribeiro 2020) or rural and eco-tourism as a way to improve the life of the rural population (cf. Akkuş, Ç and Akkuş, G. 2018). So far, most studies have been conducted by sociologists and agrarian sociologists. Their studies focus primarily on the impact of neoliberal policies in rural Turkey, on rural poverty and on the strategies of the rural population to survive (cf. Kavak 2012, Öztürk 2012, Öztürk, Jongerden, Hilton 2022).

In my opinion, there are several reasons for this neglect:

First: detailed village monographies based on months of field research, as practiced by Paul and other anthropologists in Turkey and elsewhere, have fallen out of fashion in cultural and social anthropology. Additionally, the paradigm shift has resulted in most European and American anthropologists now studying transnational Turkish communities.

Second: a shift in focus has taken place in anthropology, away from holistic studies (e.g., the “classical village monographs”) towards the treatment of specific topics (e.g., the construction of identity of ethnic and religious minorities, sense of belonging etc.).

Third: The need to publish articles in peer-reviewed journals rather than monographs, which young researchers in particular are subject to in order to advance their academic work, means that even if detailed studies have been carried out, they can only be published in the highly compressed form of these journal articles.

To my knowledge only a few monographies have been published in recent years by social anthropologists and other social scientists. They all focus on rather specific topics such as love and marriage (Hart 2005), village religiosity (Hart 2013), healing practices (Onder 2007), village tourism (Tucker 2003), village women (Karkıner (2009), emigrants to the city (Grabolle-Çeliker 2010; Kaya 2014 and 2015) or the emigrants’ relations to their place of origin (Fliche 2007). So far, no single social anthropological study exists that goes into detail about the current lives of village residents themselves and how decades of emigration have changed their lives, in particular, with regard to agricultural production, social relations, material culture etc.

Following the transnationalism paradigm, which plays a dominant role within migration studies today, most authors deal with the relationships between migrants and their village of origin from the perspective of the emigrants. Special attention is here paid to the study of remittances and their use – in particular the so-called remittance houses (cf. Alyanak 2015, Van der Horst 2015) – as well as to the role of hometown organizations (*hemşehrilik* association) (cf. Çelik 2009, Fliche 2005). Other studies deal with the various forms of remigration (e.g., retirement and root migration of the second generation, cf. Wolbert 1995, Fliche 2007 and 2011, Alyanak 2015, King and Kılınç 2014).

In line with the focus on sense(s) of belonging and space and place making, also dominant topics in current social anthropology, several newer studies make references to the current meanings of the village for its former inhabitants, now migrants in urban areas of Turkey or abroad. In my opinion, Fliche (2007:201-219 and 2011) has so far examined this aspect most explicitly. He underlines that the village – now connected to a vast array of heterogeneous spaces (sub-urban areas of European cities, *gecekondu* quarters or metropolitan regions in Turkey) – has tremendously changed its function. According to Fliche (2011:15 seq., 19) the village is now perceived as one resource among many others, and not – as in the past – as a milieu that had to be mastered by its inhabitants by either staying put or migrating. For some of its former residents the rural community still represents a resource that can be tapped in times of economic hardship (e.g., sending family members to work the village land in order to produce supplies for consumption in town or by lending plots in exchange for crop shares) (Fliche 2011: 17). But for many others the village has acquired a number of new functions. For them it is now primarily a place of recreation and holidays or retirement. (cf. Fliche 2007 and 2011, Alyanak 2015, Çelik 2009). It may also function as a spiritual resource, where migrants

and residents can pursue their heteropractical rituals without interference of state authorities (cf. Fliche 2011: 17).<sup>14</sup> And finally, as Fliche (2011:15, 18 seq.) states, the village has also turned into an object of [nostalgic] desire, amongst others reflected by the explosion of internet sites dedicated to villages, the foundation of *Hemşehrilik* associations, the diffusion of neo-rural aesthetics and the depiction of the village as a “place of order, beauty, luxury, calmness and sensuousness” (my translation; Fliche 2011:19).<sup>15</sup> Among the many new functions that have been attributed to the village, its function as holiday site and / or place of retirement has so far been most extensively analyzed.<sup>16</sup> Thereby, most studies, primarily focus on the remittance house, its architectural characteristics and its functions.<sup>17</sup> According to Alyanak (2015) the remittance house is not only a place for spending holidays or retirement, it also functions as a symbolic site for the united family, as a connection with the ancestors (“baba ocağı”) and as embodiment of national belonging and honor.

While these studies are interesting and offer us insights into the changing meanings of the village, we learn little about the socio-economic changes in recent Turkish agriculture. It was not until the 2010s that socio-economic aspects were taken into greater consideration again. At this point, the impact of the neoliberal restructuring of Turkey’s agricultural sector was becoming increasingly evident.<sup>18</sup> Rising production costs, reduced income from the sale of their products (e.g., in tobacco and sugar beet production) as well as fewer employment opportunities and widespread unemployment led to an increasing impoverishment of parts of the rural population and set in motion a renewed mass migration from rural areas.<sup>19</sup>

Here it was above all agricultural sociologists (e.g., Sinem Kavak, Joost Jongerden, Andy Hilton and Murat Öztürk) who began to study the survival strategies of the rural population.<sup>20</sup>

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<sup>14</sup> This statement refers to Alevi villages. Similar observations have been made by Shankland (2003:45) and Çelik (2009:86).

<sup>15</sup> Regarding the nostalgic desire many migrants highlight the “fine weather and clean water” (köyün havası güzel, suyu temiz, Çelik 2009:84; cf. also 94 and 107) as outstanding features of rural life they are missing in the cities. Similar statements have been made by migrants from the village I have been studying. On the nostalgia for the village in Turkey, cf. Young 2020.

<sup>16</sup> Another function of the village – less mentioned in these studies – is its importance as a “shelter for the poor” and a “place for survival and protection”, that is for those who are too poor to migrate or for those who live in precarious circumstances in the cities and are forced to return to their former place of residence (cf. Öztürk 2012: 139 and Kavak 2012: 109 f).

<sup>17</sup> On details on different styles and building materials of remittance houses cf. Van der Horst (2010) and Alyanak (2015).

<sup>18</sup> The agricultural reform program from 2001 to 2006 included the abolition of state price controls, cheap seed and credit. At the same time, it facilitated land transfer and access of international capital to Turkey’s agricultural sector. For details cf. Öztürk 2012 and Öztürk, Jongerden and Hilton 2014a: 344.

<sup>19</sup> “... between 2000 and 2007, the total rural population of Turkey dropped from around 24 to 21 million, some half a million per year... this devastation of rural depopulation has continued, at a rate of around 1% annually over the for years 2008 – 2012.” (Öztürk, Jongerden and Hilton 2014b: 377).

<sup>20</sup> Sinem Kavak conducted field research in three different tobacco production areas (Adıyaman in south-eastern Turkey, Soma district in the Aegean region and in Fethiye and its surrounding villages in the province of Muğla) (for details cf. Kavak 2012). Murat Öztürk first published a macro-economic study on “Agriculture, peasantry and poverty in Turkey in the neo-liberal age” (2012). This study was followed by several articles, co-authored with Hilton, Jongerden and others, in which they presented the findings of their research in 25 villages across Turkey as well as among urban migrants. Their research was based on a combination of (mostly) state-produced statistics as well as focus-group discussions with villagers and in-depth interviews with local officers of the Turkish Ministry for Agriculture and members of village and urban migrant households (Öztürk, Jongerden and Hilton 2014a: 343 f). They used the household as analytical framework for their researches (cf. Öztürk, Jongerden and Hilton 2018b: 517).

The work by Kavak as well as by Öztürk and his colleagues illustrates that the effects of the neoliberal restructuring of Turkey's agriculture meant that more and more rural farms were in need of additional income to continue their operations.<sup>21</sup> One of the strategies pursued in this regard was to achieve income diversification through the phenomenon of pluri-activity. Depending on the possibilities (e.g. geographical location), this income diversification included specialization in certain high-quality agricultural products (e.g. organic food), eco-tourism or – as in the past – income generation through short term, circular or permanent migration of individual household members to cities and seasonal agricultural labour to other regions of Turkey such as harvesting hazelnuts in the Black Sea region or working abroad (for details cf. Kavak 2012, Öztürk 2012, Jongerden 2021, Öztürk, Jongerden and Hilton 2014b).

These studies also underline that numerous city dwellers, both the long-established lower middle class former migrants (e.g., teachers, civil servants) and the neo-migrants, had economic difficulties and often lived in precarious income situations (cf. Öztürk et al. 2014 a: 360). For this group of people, returning to their village of origin (permanently or on a seasonal basis) or engaging in additional food production on leased or purchased land – in the case of city dwellers – represented an alternative strategy (cf. e.g., Öztürk, Jongerden, Hilton 2022: 330). By undertaking various income-generating or survival-securing activities (e.g., wage labor in the city coupled with agricultural production, income from pensions, remittances) people and goods move back and forth between urban and rural spaces (cf. Öztürk, Jongerden and Hilton 2014b: 382 and Öztürk, Jongerden and Hilton 2022: 330).

The authors point out that the living space of many people today spans many different geographical areas and people often spend their time between two or more places: “Lived spaces are thus created which span geographically distant locations. These are made into multivalent living structures through human relationship and (other) socio-economic networks. What we observe is that people are increasingly spending their time split between two or three places located in both rural and urban settings, while rural-based households and family complexes are increasingly oriented to living structures that include multiple places, in the village (and hamlets), in the local town and the nearest city, and in the distant metropolis(es) and foreign countries” (Öztürk, Jongerden and Hilton 2022: 331).

It follows that this constant movement between different places and economic activities (e.g., agricultural production, wage labour outside agriculture etc.) undermines the binary between “rural” and “urban” or “worker” and “peasant” that has long prevailed in migration literature (cf. Jongerden 2018:31).

In order to capture the pluri-activity and daily lives that connect various localities/spaces of today's rural population, Jongerden assumes a relational understanding “... in which the city and the village, rural and urban, continually co-produce and redefine each other through a myriad of dynamic practices (living structures)” (Jongerden 2018: 31). Thus, he argues: “The terms “village,” “city,” “urban,” and “rural” do not represent preexisting entities. ... rather... [my addition: they must be] explained as social productions of spatial forms...” (Jongerden 2018: 31f).

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<sup>21</sup> “Currently, approximately 40% of rural employment is engaged in non-agricultural jobs, and total non-agricultural income is higher than agricultural income in many villages (Kentel & Emre 2017). Some of this non-agricultural employment is in rural areas such as grocery, local trade, trucking, and school bus and local trade jobs, while the other parts are in mining, construction and industry in the vicinity and nearby cities.” (Öztürk, Jongerden and Hilton 2022: 330)



To take this into account, Jongerden, Öztürk and their colleagues use the concept of “living structures” and define it as follows: “... living structures, refers to just this sense of space/place as defined in relation to time/movement, with ‘structures’ as the arrangements/patterns of spatiotemporal locations and ‘living’ as the human dynamics of this, the changing (re)construction and more or less flexible employment of these in people’s lives” (Öztürk, Jongerden and Hilton 2014b: 372). Regarding the benefits of using the living structures concept, Jongerden notes: “The concept helps to map actors, practices, constraints, change, and as such helps to capture both the dimensions of agency and structure, while it tries to understand the lives of people without assuming a fixed economic, spatial setting or cultural setting, but a thrown-togetherness and intersectionality.” (Jongerden 2021) Based on the study of living structures, the authors come to the conclusion that most rural residents today lead a “multi-place hybrid life” (Öztürk, Jongerden and Hilton 2014a: 361, Öztürk, Jongerden and Hilton 2014b: 370). This multi-place life results in “... an overarching development of rural-urban connectedness through a dual settlement or multi-place, hybrid life in which people (including nuclear and extended families) integrate innumerable, dynamic residence/employment combinations” (Öztürk, Jongerden and Hilton 2022: 331). And related to the importance of agriculture they note: “Farming as a constituent part of life in the rurality remains fundamental to the new, hybrid living forms emerging, but agriculture is no longer the life-defining matrix of village life that it once was. Indeed, ‘village life’ as such has transmuted – into new modes of living.” (Öztürk, Jongerden and Hilton 2022: 331)

As far as I know, the works of Kavak, Öztürk, Jongerden and Hilton, are the only recent studies that deal comprehensively with the transformation processes in rural Turkey and the connection between agriculture and migration.

## Conclusion

Since Paul passed away the rural areas of Turkey have gained new momentum. Since then, researchers from various scientific disciplines, among them sociologists, anthropologists, economists, architects, environmentalists etc. have studied a large variety of different topics (e.g., changed meaning of village, impact of neoliberal economic policies in the rural areas, etc.) related to Turkey. I can only speculate to what extent Paul would be satisfied with the approaches underlying these studies. In view of his critical attitude towards the “grand theories” (e.g., Marxism and modernization theory, for details cf. Stirling 1994), he would certainly have found critical words here. Nevertheless, these studies seem important to me, as they finally draw attention back to rural areas and their population.

I personally found the studies on the changed meaning of the villages interesting and useful for my own work. They offer us a lot of information on how the migrants perceive their former place of origins. However, what is missing in these studies is the perspective of those who stayed put there. In addition, they remain mute about the current economic activities, the age structure of its population, the socio-economic relations within the village community or the changes in the landscape.

It follows that the concept of “multi-place hybrid living structures” is maybe more useful when studying the current processes in rural Turkey. Here it would be useful to go into even more detail than Öztürk, Jongerden and their colleagues have done in their studies. That is, from my point of view, detailed socio-anthropological studies would be necessary, which go beyond the

pure questioning of village residents and migrants and also use the method of participatory observations. This could give us an even more detailed picture of the actual interactions.

What would also be urgently needed are re-studies. Despite the fact that they provide us with important insights into the socio-cultural and economic transformation processes, they always remained a stepchild of research. Paul's re-studies of Sakaltutan and Elbaşı and those undertaken by some other scholars are notable exceptions here. Yet, to my knowledge no re-study has been published in recent years. While the current studies by Kavak, Öztürk and others present information of more recent transformation processes, they provide little information about the past.

In summary, it can therefore be stated that both detailed monographs and re-studies should be undertaken. In order to do this, several options would be possible from my point of view.

1) We could use Paul's grid for this. In view of the fact that many villages are now depopulated, we might have to reverse the focus and start with the village migrants, as some studies have done (e.g., Fliche 2011, Alyanak 2015) and ask what the village means for them.

2) Another possibility is that we follow the "multi-place hybrid living structure" approach represented by Kavak, Öztürk, Jongergen and other, which assumes that both village and city can only be examined together, and undertake corresponding studies.

3) A third option would be to link both approaches, Paul's grid and the approach used by Kavak, Öztürk, Jongergen.

How Paul would examine the current transformation processes in the rural regions and the relationships between the villagers and the migrants today, whether he would use his grid from 1994, expand it, discard it or maybe yet inspired by the recent research approaches, remains a matter of speculation.

Likewise, it is difficult to estimate what he would think about these new approaches. Yet, I think he would agree that while some gaps have been filled (cf. the more recent publications on the village institutes, e.g., by Ünal 2019), there is still a great lack of recent ethnographic studies of the rural area.

I am sure, however, that he would welcome the refocusing on rural areas and would vehemently advocate carrying out detailed empirical studies on the transformation processes in the villages and among their migrants. Above all, he would advocate examining these processes from the perspective of the villagers and migrants, giving them a voice, a voice that has been neglected for far too long.

## References

Abadan-Unat, Nermin; Ruşen, Keleş, Rinus Penninx, Herman Van Renselaar, Leo Van Velzen, Leylâ, Yenisey. 1976. Migration and Development: A Study of the Effects of International Labour Migration on Boğazlıyan District. Ankara: Ajans Türk Press.

Abadan-Unat, Nermin. 2009. *Turks in Europe, 1957-2007: From Guest Worker to Transnational Citizen*. New York and Oxford: Berghahn Books.

Akkuş, Çetin and Gülizar Akkuş (Eds.). 2018. *Selected Studies on Rural Tourism and Development*. Newcastle upon Tyre: Cambridge Scholars Publishing.

Alyanak, Oğuz. 2015. *The Social Life of Remittance Houses: Scenes from Rural Kayseri*. Working Paper for Turkish Migration Conference, 25-27 June, 2015. [https://www.academia.edu/15077001/The\\_Social\\_Life\\_of\\_Remittance\\_Houses\\_Scenes\\_from\\_Rural\\_Kayseri](https://www.academia.edu/15077001/The_Social_Life_of_Remittance_Houses_Scenes_from_Rural_Kayseri). Accessed 16.6.2023.

Aswad, Barbara C. 1971. *Property Control and Social Strategies in Settlers in a Middle East Plain*. Anthropological Papers, Museum of Anthropology, University of Michigan No. 44.

Bates, Daniel. 1971. *The Yoruk of Southeastern Turkey. A Study of Land Use and Social Organisation*. Ph.D. dissertation, University of Michigan.

Berkes, Niyasi. 1942 *Bazı Ankara Köyleri üzerine bir Araştırma*. Dil ve Tarih – Coğrafya Fakültesi Felsefe Enstitüsü Neşriyatı Sosyoloji Serisi No. 2, Ankara.

Boran, Behice Sadık. 1945. *Toplumsal Yapı Araştırmaları. 2 Köy Çesidinin Mukayeseli Tetkiki*. Ankara Üniversitesi Dil ve Tarih – Coğrafya Fakültesi Felsefe Enstitüsü Sosyoloji Serisi 3, Ankara.

Birkalan-Gedik, Hande. 2011. *Anthropological Writings on Urban Turkey: A Historical Overview*. In: *Urban Anthropology and Studies of Cultural Systems and World Economic Development* 40 (1/2): 1-66.

Birkalan-Gedik, Hande. 2018. *Turkey, Anthropology in*. In: *International Encyclopedia of Anthropology*. 6220 to 6239. <https://doi.org/10.1002/9781118924396.wbiea2020>. Accessed 16.6.2023.

Çelik, Aysun. 2009. *Binnenmigrationsbewegungen in der Türkei. Konstruierte regionale Beziehungen in der Migration. Eine anthropologische Untersuchung der Selbstorganisation von BinnenmigratInnen aus Ortaköy/Erzincan in Istanbul und ihre Beziehungen zum Herkunftsort*. Unpublished MA thesis, University of Vienna.

Delaney, Carol. 1992. *The Seed and the Soil. Gender and Cosmology in Turkish Village Society*. Berkeley, Calif.: University of California Press.

Erdentuğ, Aygen and Paul J. Magnarella. 2001. *Turkish Social Anthropology Since the 1970s*. In: *The Oriental Anthropologist* (1): 60-67.

Erdentuğ, Nermin. 1956. *Hal Köyü'nün Etnolojik Tetkiki*. Ankara Üniversitesi Dil ve Tarih – Coğrafya Fakültesi Yayınları No. 109, Ankara.

Erdentuğ, Nermin. 1959. *Sün Köyü'nün Etnolojik Tetkiki*. Ankara Üniversitesi Dil ve Tarih – Coğrafya Fakültesi Yayınları No. 132, Ankara.

Erman, Tahire. 1993. Becoming “urban” or staying “rural”: The experiences of diverse groups of rural-to-urban migrant women and men living in *gecekondus* and apartments in Ankara, Turkey. Ph.D. dissertation, City University of New York.

Erman, Tahire. 1997. The meaning of City Living for Rural Migrant Women and Their Role in Migration: The Case of Turkey. In: *Women’s Studies International Forum* 20 (2): 263–273.

Erman, Tahire. 1998. Becoming “Urban” or remaining “Rural”: The Views of Turkish Rural-to-Urban Migrants on the “Integration”. In: *International Journal of Middle East Studies* 30 (4): 541–561.

Erman, Tahire. 2001. The Politics of Squatter (*Gecekondu*) Studies in Turkey: The Changing Representations of Rural Migrants in the Academic Discourse. In: *Urban Studies* 38 (7): 983–1002.

Fliche, Benoit. 2004. “La modernité est en bas”. Ruralité et urbanité chez les habitants d’un *gecekondu* d’Ankara. In: Pèrouse, Jean-François (Ed.). *Gecekondu*. In: *The European Journal of Turkish Studies* 1.

Fliche, Benoit. 2005. The *hemşehrilik* and the village: the stakes of an association of former villagers in Ankara. In: Hersant, Jeanne and Anexandre Toumarkine (Eds.). *Hometown Organisations in Turkey*. In: *The European Journal of Turkish Studies* 2.

Fliche, Benoit. 2007. *Odyssées turques. Les migrations d’un village Anatolien*. Paris: CNRS Editions.

Fliche, Benoit. 2011. La cage et le site web. De quelques transformations contemporaines des villages. In: *Anthropology of the Middle East* 6 (2): 14 -20.

Grabolle-Çeliker, Anna Catriona. 2010. Migration and Change among Vanlı from Eastern Turkey. Ph.D. thesis. Eberhard-Karls-Universität Tübingen. [https://www.academia.edu/11848119/PhD\\_thesis\\_Migration\\_and\\_Change\\_among\\_Vanl%C4%B1\\_from\\_Eastern\\_Turkey\\_](https://www.academia.edu/11848119/PhD_thesis_Migration_and_Change_among_Vanl%C4%B1_from_Eastern_Turkey_). Accessed 17.6.2023.

Hart, Kimberly. 2005. *Aci tatli yiyoruz: Bitter or sweet we eat. The economics of love and marriage in Örselli Village*. Ph.D. thesis. Department of Anthropology Indiana University. [https://www.researchgate.net/profile/Kimberly-Hart-3/publication/35457988\\_Aci\\_Tatli\\_Yiyoruz\\_bitter\\_or\\_sweet\\_we\\_eat\\_the\\_economics\\_of\\_love\\_and\\_marriage\\_in\\_Orselli\\_Village/links/600dcd5692851c13fe355e81/Aci-Tatli-Yiyoruz-bitter-or-sweet-we-eat-the-economics-of-love-and-marriage-in-Oerselli-Village.pdf](https://www.researchgate.net/profile/Kimberly-Hart-3/publication/35457988_Aci_Tatli_Yiyoruz_bitter_or_sweet_we_eat_the_economics_of_love_and_marriage_in_Orselli_Village/links/600dcd5692851c13fe355e81/Aci-Tatli-Yiyoruz-bitter-or-sweet-we-eat-the-economics-of-love-and-marriage-in-Oerselli-Village.pdf). Accessed 17.6.2023.

Hart, Kimberly. 2013. *And Then We Work for God: Rural Sunni Islam in Western Turkey*. Stanford: Stanford University Press

Hinderink, Jan and Mübeccel, Kiray. 1970. *Social Stratification as an Obstacle to Development: A Study of Four Turkish Villages*. New York: Praeger Publications.

Jongerden, Joost. 2018. *Living Structures. Methodological Considerations on People and Place*. In: Baser, Bahar, Mari Toivanen, Begum Zorlu and Yasin Duman (Eds.): *Theoretical and Practical Insights from the Field*. Boulder, Lexington Books: 21-33.

Jongerden, Joost. 2021. 75th Anniversary: 13) Reflections: From Rural Sociology to a Sociology of Place? Blog Post, Posted on January 4, 2021. <https://ruralsociologywageningen.nl/2021/01/04/75th-anniversary-13-reflections-from-rural-sociology-to-a-sociology-of-place/>. Accessed 18.6.2023.

Jongerden, Joost. 2022. Civilizing space: addressing disorder in rural and urban landscapes. In: Jongerden, Joost (Ed.). *The Routledge Handbook on Contemporary Turkey*. London, New York: Routledge. 373- 384.

Karpat, Kemal H. 1976. *The Gecekondu: Rural Migration and Urbanization*. Cambridge: Cambridge University Press.

Karkıner, Nadide. 2009. *Feminist Analysis of Rural Woman in a Village of Turkey*. London: LAP Lambert Academic Publishing.

Kavak, Sinem. 2012. *Struggling for Survival in the Village. New Rurality and Patterns of rural Restructuring in Response to Agricultural Liberalization in Turkey*. Saarbrücken: Lambert Academic Publishing.

Kaya, Gazanfer. 2014. *Kentleşmenin Toplumsal Boyutları. Adıyaman Örneği*. Ankara: Ütopya Yayınevi.

Kaya, Gazanfer. 2015. Internal Migration and its Effects in one of the Underdeveloped Regions of Turkey (The Sample of Adıyaman). In: *International Journal of Humanities and Social Science* 5 (3): 71 -80.

Karaömeroğlu, Asım. 2002. The Peasants in Early Turkish Literature. In: *East European Quarterly* 36 (2); 127-53.

King, Russell and Nilay Kılınc. 2014. Routes to Roots: Second-Generation Turks from Germany 'Return' to Turkey. In: *Nordic Journal of Migration Research* 4 (3): 126 -133.

Magnarella, Paul J. 1979. *The Peasant Venture: Tradition, Migration and Change Among Georgian Peasants in Turkey*. Cambridge, Mass.: Schenkman Publishing Company.

Magnarella, Paul J. and Orhan Türkdoğan. 1976. The Development of Turkish Social Anthropology. In: *Current Anthropology* 17 (2): 263-274.

Marcus, George. 1995. Ethnography in/of the World System: The Emergence of Multi-Sited Ethnography. In: *Annual Review of Anthropology* 24: 95-117.

Meeker, Michael E. 1970. *The Black Sea Turks: A Study of Honor, Descent, and Marriage*. Ph.D. dissertation, University of Chicago.

Olson, Emelie A. 1986. Anthropological and Sociological Studies of Turkey: An American Perspective. In: *Turkish Studies Association Bulletin* 10 (2): 69-78. <https://www.jstor.org/stable/43384137>.

Olson, Emelie A. 1995. They work harder than before. In: *New Perspectives on Turkey* 13 (Fall): 121 – 150.

İncirlioğlu, Emine Onaran. 1993. Marriage, Gender Relations and Rural Transformation in Central Anatolia. In: Stirling, Paul (Ed.) *Culture and Economy. Changes in Turkish Villages*. Huntington: The Eothen Press, pp. 115-126.

Onder, Sylvia Wing. 2007. *We Have No Microbes Here: Healing Practices in a Turkish Black Sea Village*. Durham, N.C.: Carolina Academic Press.

O'Neil, Aaron. 2023. Urbanization in Turkey 2021. <https://www.statista.com/statistics/255487/urbanization-in-turkey/>. Accessed 3.6.2023.

Öztürk, Murat. 2012. *Agriculture, peasantry and poverty in Turkey in the neo-liberal age*. Wageningen: Wageningen Academic Publishers.

Öztürk, Murat, Joost Jonderden and Andy Hilton. 2014a. Commodification and the Social Commons: Smallholder Autonomy and Rural–Urban Kinship Communalism in Turkey. In: *Agrarian South: Journal of Political Economy* 3 (3): 337-367.

Öztürk, Murat, Andy Hilton and Joost Jongerden. 2014b. Migration as Movement and Multiplace Life: Some recent developments in rural living structures in Turkey. In: *Population, Space and Place* 20: 370-388.

Öztürk, Murat, Joost Jongerden, Andy Hilton. 2018a. The (re)production of the new peasantry in Turkey. In: *Journal of Rural Studies* 61(July): 244-254.

Öztürk, Murat, Beşir Topaloğlu, Andy Hilton and Joost Jongerden. 2018b. Rural–Urban Mobilities in Turkey: Socio-spatial Perspectives on Migration and Return Movements. In: *Journal of Balkan and Near Eastern Studies* 20 (5): 513 – 530.

Öztürk, Murat, Joost Jongerden, Andy Hilton 2022. Agriculture and Rural Life in Turkey. In: Jongerden, Joost (Ed.): *The Routledge Handbook on contemporary Turkey*. London, New York: Routledge, pp. 322-334.

Paleczek, Gabriele. 1987. *Der Wandel der traditionellen Wirtschaft in einem anatolischen Dorf*. In: *Wiener Beiträge zur Ethnologie und Anthropology* 4. Horn and Vienna: Ferdinand Berger & Sons Publishers.

Planck, Ulrich. 1974. Zur Frage der Verdörflichung orientalischer Städte am Beispiel der Türkei. In: *Orient* 15 (2): 43–46.

Rasuly-Paleczek, Gabriele. 1993. Labour Migration Among Bursa Muhacirs. Some Wider Implications. In: Stirling, Paul (Ed.). *Culture and Economy: Changes in Turkish Villages*. Huntington: The Eothen Press, pp. 95-115.

Rasuly-Paleczek, Gabriele. 2019. Göç ve Çevre: Batı Anadolu'dan bir Örnek Çalışma. In: Pultar, Gönül and Emine Onaran İncirlioğlu (Eds.). *Rüzgâr değil yaprağı kimildatan. Türkiye'de Çevrenin Kültürel Boyutlarına Yeni Bakışlar*. İstanbul: Ürün Yayınları, pp. 81-104.

Ribeiro, José Duarte. 2020. Research: Peasantry and Rural Social Movements in Twenty-First Century Turkey. In: *Europe Now, Journal Council for European Studies*, 9.Nov. 2020.

<https://www.europenowjournal.org/2020/11/09/peasantry-and-rural-social-movements-in-twenty-first-century-turkey/>. Accessed 14.6.2023.

Schiffauer, Werner. 1987. *Die Bauern von Subay. Das Leben in einem türkischen Dorf*. Stuttgart: Klett-Cotta.

Sezer, Özge. 2022. *Forming the Modern Turkish Village: Nation Building and Modernization in Rural Turkey during the Early Republic*. Bielefeld: Transcript Publishing.

Shankland, David. 2003. *The Alevis in Turkey. The emergence of a secular Islamic tradition*. London and New York: Routledge Curzon.

Starr, June. 1978. *Dispute and Settlement in Rural Turkey: An Ethnography of Law*. Leiden: E.J. Brill.

Stirling, Paul. *Turkish Village*. Lexington. Weidenfeld and Nicolson.

Stirling, Paul. 1974. Cause, knowledge and change: Turkish Village revisited. In: Davis, John (Ed.): *Choice and Change*. London: The Athlone Press, pp. 191-229.

Stirling, Paul. 1981. *Social Change and Social Control in Republican Turkey*. Paper for the International Symposium on Atatürk, Türkiye İş Bankası, İstanbul, May 1981. (Unpublished manuscript)

Stirling, Paul. 1988. *Labour Migration and Changes in Anatolia*. Edited version of paper delivered to the conference on Mediterranean Migrations held at the University Al Charif Al Idrissi, at the invitation of the Ministry for Cultural Affairs of the Kingdom of Morocco, 11 – 14 July 1988. [http://www.era.anthropology.ac.uk/Era\\_Resources/Era/Stirling/Papers/Anatolia\\_Migration/anatolia\\_mig\\_1.html](http://www.era.anthropology.ac.uk/Era_Resources/Era/Stirling/Papers/Anatolia_Migration/anatolia_mig_1.html). Accessed 14.6.2023.

Stirling, Paul (Ed.) 1993a. *Culture and Economy. Changes in Turkish Villages*. Huntingdon: The Eothen Press.

Stirling, Paul. 1993b. Introduction: Growth and Changes. Speed, Scale, Complexity. In: Stirling, Paul (Ed.): *Culture and Economy. Changes in Turkish Villages*. Huntingdon: The Eothen Press, pp. 1- 16.

Stirling, Paul. 1994. *Labour Migration in Turkey: Thirty-Five Years of Changes*. In: Altuntek, Serpil N., Suavi Aydın, İsmail H. Demirdöven (Eds.): *Humana: Bozkurt Güvenç'e Armağan*. Ankara; Kültür Bakanlığı, pp. 413-441.

Stirling, Paul. 1996. *Social Processes in the History of the Turkish Republic*. Draft of Paper for the Vienna Meeting on “Social Processes in the History of the Turkish Republic”, May 1996.

Struck, Ernst. 1984. *Landflucht in der Türkei: Die Auswirkungen im Herkunftsgebiet – dargestellt an einem Beispiel aus dem Übergangsraum von Inner- zu Ostanatolien (Provinz Sivas)*. Passau: Passavia.

Struck, Ernst. 1988. Migration Patterns and the Effects of Migration on Household Structure and Production in an East Anatolian Village. In: *Tijdschrift voor economische en sociale geografie* 79 (3): 210 -219.

Talim, Metin. 1985. *Structure and Management Problems of Farms in Turkey*. Rome: FAO.

T.C. Başbakanlık Devlet İstatistik Enstitüsü. 1992. *Türkiye İstatistik Yıllığı 1990*. Ankara, Devlet İstatistik Enstitüsü Matbaası.

Tucker, Hazel. 2003. *Living with Tourism. Negotiating Identities in a Turkish Village*. London, New York: Routledge.

Ünal, İnci. 2019. Remains of a former village institute in Turkey: Memory at the border of Voluntary and Involuntary. In: *Graduate Journal of Social Science* 15 (2): 37–61.

Van der Horst, Hilje. 2010. Dwellings in Transnational Lives: A Biographical Perspective on ‘Turkish-Dutch’ Houses in Turkey. In: *Journal of Ethnic and Migration Studies* 36 (7): 1175-1192.

Wedel, Heidi. 2004. Alltagsleben und politische Partizipation – Gecekondu- Viertel als gesellschaftliche Ort. In: Pèrouse, Jean-François (Ed.) *Gecekondu. European Journal of Turkish Studies* 1.

Wolbert, Barbara. 1995. *Der getötete Paß: Rückkehr in die Türkei: eine ethnologische Migrationstudie*. Berlin, Akademie Verlag.

Yasa, İbrahim. 1957. *Hasanoğlan – socio-economic structure of a Turkish Village*. Ankara: Yeni Matbaa.

Yasa, İbrahim. 1969. 25 Yıl sonra Hasanoğlan Köyü, Karşılaştırmalı bir Toplumbilimsel Araştırma. Ankara Üniversitesi Siyasal Bilgiler Fakültesi Yayınları No. 270, Ankara.

Yazıcı Yakın, Aslı. 2007. Proust Redux “There is a village somewhere out there”: Representations of Anatolia as Terra Incognita. In: *Journal of Historical Sociology*, 20 (1/2): 182-199.

Young, Nathan. 2020. *Modernity’s Other: Nostalgia for Village Life in Turkey*. Ph.D. thesis, Ohio State University.



## **Part Three**

### **Paul Stirling's Contribution to the Study of Social Change in Turkey**

**Bahattin Akşit**

## **Rural/Agricultural Transformations and/or Reproductions in Turkey: Thinking with / against Paul Stirling**

### **Introduction**

In the book edited by Paul Stirling (1993), the chapter I wrote started with a section titled “interconnections between personal biography and rural transformations” which promised a forty-year account of rural transformations in Turkey and transformations of my research framework about these transformations (Akşit 1993:187). My intention in the present paper was to give an account of both transformations, this time, for 70 years and hopefully dialogically and critically with Mübeccel Kıray, Paul Stirling and Çağlar Keyder, sometimes by contesting their accounts, some other times agreeing with their evaluations, approaches and diagnoses of Turkish modernization and rural transformations. The following is what I have been able to accomplish.

Giving an account of one’s approaches in all fields that one did research is almost an impossible task. Giving an account of one’s approach in even *one* field is a very hard task.<sup>1</sup> When I was in the middle of my life, one of my students<sup>2</sup> showed me how he read one of my papers in a Foucauldian way by “including” “excluded” thoughts and discourses into my text. Somehow, I did not or could not practice it very frequently, let alone systematically, in my work although I tried to be as reflexive and self-critical as possible. That is, I hope that I had practiced a critical and reflexive writing of each consecutive text by way of pointing out “common sense” and/or “ideological” intrusions or “mistakes/errors” into my previous writings. Although, I have not been able to do a systematic “reading” of my works (in the way my student advised), in future, I am planning to do an inevitably partial “reading” of my way of doing research (my *modus operandi*)<sup>3</sup> by re-reading my texts and field notes that I kept during field research.<sup>4</sup>

### **Structural Trends in Turkish Agriculture: Land and Agricultural Holding Distributions, 1963-2020**

Before proceeding further, I will present a general historical and structural trend of agriculture in Turkey in statistical terms by using data periodically gathered by Türkiye İstatistik Kurumu

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<sup>1</sup> Butler in her book *Giving an Account of Oneself* (2003), conceptualized it for one’s life and self.

<sup>2</sup> Arkaç Otaran. He wrote a Master’s Thesis on popular culture and constitution of subjectivity (Otoran 1985).

<sup>3</sup> Bourdieu had pointed out that giving an account of “modus operandi” of research is more important than giving produced findings or concepts or “modus operandum”, because to give an account of the way findings or concepts are produced gives away how theory is unified with method in the process of research, contrary to textbooks on Sociological Method and Theory (Bourdieu and Wacquant 1992, p. xx).

<sup>4</sup> I have already promised it in an invited conference in Başkent University Sociology Department with the following title: “Teori ve Metot İlişkileri Çerçevesinde Sosyolojide Alan Araştırmaları(m): 1966-2016” and it is published in an online book (Akşit 2020).

(TÜİK; Turkish Statistical Institute) and the Ministry of Agriculture and Forestry (MOAF). Now, I know and feel that, this is not thinking with Paul Stirling but thinking against his advice and approach. As people who know Paul will remember he was vehemently against gathering data with questionnaires, aggregating data that were collected in different contexts and using them in statistical analyses, as most anthropologists do who value participant observation as a constitutive method of anthropology, thereby differing it from sociology and economics. Paul, I believe, was especially against using statistics that were produced by data collected by state officials from villagers (in less than an hour for a whole village, and aggregated along with data for a sample of 40 thousand or so other villages), on the grounds that such data would be neither reliable nor valid. As an anthropologist who lived in two villages for one and half years to gather data about only those two villages, who followed up those two villages for 40 years, and who was still critical about his own data, Paul Stirling rightfully opposed to using data gathered from a sample of thousands of households and aggregated in statistical tables.

As a sociologist using participant observation and qualitative data gathering techniques, I am very much in agreement with Paul, and to my students I advise relevant literature critical of using official statistics in research (Hindess 1973). Nevertheless, as a sociologist I also use questionnaires in large scale representative sample surveys and official statistics, albeit critically, to analyze macro structural transformations and/or reproductions; a technique Paul would oppose. That is what I will do in the following pages initially and later, hopefully, I will complement and qualify that macro statistical picture with more observational, qualitative and critical evaluations.

First, I would like report about my experiences and findings from a large scale rural-agricultural panel survey research with a sample of 5500 households, between the years 2002 and 2004, conducted for the Agricultural Reform Implementation Project (ARIP). ARIP was a project within the General Directorate of Agricultural Reform of the Ministry of Agriculture and Forestry (MOAF). The panel survey research project was planned and implemented by a private research firm, G&G where I was a sociologist member of the research team working in the design of the questionnaire, its implementation and analyses of panel data. The panel survey research started with a sample of 5500 rural households in 2002, and in the second survey, in 2004, it was possible to reach only 3963 households, unable to contact the remaining 1537. I evaluated the responses to the questions about Direct Income Support (DIS), which was paid to those farmers on the bases of abstained amount of land from growing the targeted crops, including hazelnuts, tea and tobacco. I presented a paper in the 9<sup>th</sup> Congress of Turkish Social Science Association with a title of “Implementation of Direct Income Support and its Future” (Akşit 2005).

Without going into details, it can be stated that the positive evaluation of direct income support dropped from 69% in 2002 to 59% in 2004. The positive evaluation of direct income support came only from tea and hazelnut growers, while the rest of the farmers inclined to have negative evaluations of DIS from 2002 to 2004. All strata of farmers from large land and tractor owning ones to small land owning and renting out labor were negatively disposed to DIS. Middle-sized land owners renting out labor were most negative from 2002 to 2004. One of the expected benefits of direct income support in addition to discouraging growing of targeted crops was registration to Çiftçi Kayıt Sistemi (ÇKS) (Farmer Registration System), a requirement for DIS payment. In this paper, I will not go further into details of direct income support and its fate and other support policies. Rather than that, in the following paragraph I will use data from Farmer Registration System for my historical comparison of land distribution patterns from 1963 to 2020.

First, I would like to compare average land tilled by farmers/households/holdings at the following dates: 1963, 1970, 1980, 1991, 2001 and 2020 (Table 1, last row). The last distribution is data

obtained from Farmer Registration System of General Directorate of Agricultural Reform of Ministry of Agriculture and Forestry as explained in the previous paragraph. The previous distributions of the years 1963, 1970, 1980 and 1991 were from Agricultural Censuses conducted by TÜİK and were already used in a paper I published in a book edited by Oya Baydar for the 75<sup>th</sup> year of establishment of Republic. There, I presented two tables one showing distribution of households/holdings in land-size categories and in a second table corresponding distribution of aggregated amounts of land cultivated by households in each category for each year (Akşit 1999:183). Here, I will combine those two tables by giving only percentages and by adding two new distributions: The first distribution is from the last General Agricultural Census of 2001, and the second is for 2020, obtained from the General Directorate of Agricultural Reform, MOAF, as the distribution of farmers from Farmer Registration System (*Çiftçi Kayıt Sistemi*).

It can be objected that the data from Farmer Registration System and the previous agricultural census results are not comparable. There is some truth in this objection. As far as I can understand, agricultural census data were obtained from large samples and estimating number of farmers ranging from 3,966,822 in 1991 to 3,022,827 in 2001. Whereas the distribution and average for 2020 is from self-reporting of 2,127,957 registered farmers. It is possible that the number of farmers actually dropped from 3 million to 2 million from 2001 to 2020. Even if we think that the number of farmers remained the same but only 2,127,957 of them were registered in 2020, then the percentage of registered farmers will be 70%. However, due to the general trend of decrease in rural population, I believe that registered farmers must be higher than 70% of the total farmers cultivating land.

As it can be seen from Table 1, last row, the average land cultivated was 54 decares in 1963 and it increased to 61 decares in 2001 and 71 decares in 2020. The average land cultivated remained around 60 decares between 1980 and 2001 and increased to 71 decares from 2001 to 2020. During the last 60 years average land holding was just enough for subsistence and petty commodity production of relatively independent peasants/farmers.

	1963		1970		1980		1991		2001		2020	
Size of Holding (decares)	% of Holding	% of Land	% of Holding	% of Land	% of Holding	% of Land	% of Holding	% of Land	% of Holding	% of Land	% of Holding	% of Land
1-50	68.8	24.4	75.1	29.6	62.1	20.0	67.0	22.1	64.8	21.3	62.1	18.3
51-100	18.1	23.9	14.7	23.1	20.2	21.2	18.0	19.9	18.5	20.7	19.5	19.9
101-200	9.4	23.7	7.1	21.8	11.6	23.8	9.7	21.0	10.8	23.8	11.6	23.3
201-500	3.2	17.0	2.6	14.3	5.3	22.9	4.4	19.8	5.9	22.8	5.7	24.7
501+	0.5	11.0	0.5	11.2	0.8	12.1	0.9	17.2	0.7	11.4	1.1	13.8
% Total	100	100	100	100	100	100	100	100	100	100	100	100
Total N Holdings	3 100 850		3 082 986		3 650 910		3 966 822		3 022 827		2 127 957	
Total land (decares)	167 343 350		139 585 520		227 640 289		234 510 993		184 348 223		151 772 248	
Average size of Holdings	54.0		45.3		62.4		59.1		61.0		71.3	

Table 1. Percentage Distributions of Holdings and Cultivated Land According to Holding-size Categories (Akşit 1999:183 with new additions of data)

There had been some large land holdings (*çiftlik*s) in agriculture during and especially before this period in Ottoman and Republican Turkey, but they had not developed into a large landed oligarchic class as in the Latin American model (Keyder 1987: 18). Table 1 indicates that the percentage of farmers in 501 decares and more land category doubled from 0.5% in 1963 to 1.1%

in 2020. However, the amount of land cultivated by them increased only from 11.0% to 13.8%. Similarly, the percentage for farmers in the 201-500 category increased from 3.2% to 5.7%, while the percentage of land cultivated by them increased from 17.0% to 24.7%. One can say that petty commodity production is still the prevailing mode of production in agriculture in Turkey. This evaluation will be correct in cereal production where wage labor employment is nonexistent or low; but in cotton and some other products, wage labor and capitalist relations are more prevalent. Hence, I will say that capitalistic relations in agriculture are increasing, although capitalistic large farms are not a prevalent mode of production.

In the 1-50 decares of land category, the percentage of farmers that was 68.8% in 1963, dropped to 62.1% in 1980 and 2020; it reached to the highest 75% in 1970 and was 67% in 1991 and 64.8% in 2001. The percentage of land in this category dropped from 24.4% to 18.3% in 2020, while it became highest, 29.6%, in 1970 parallel to high percentage of farmers in this category. Close to two thirds of farmers/households in Turkey cultivate 50 decares and less land, cultivating less than one thirds of total land cultivated. In the Aegean and Marmara regions, especially in irrigated areas, substantial percentage of these farmers can be called small commodity producers, especially those using new technologies to produce agricultural goods, vegetables and fruits, for domestic urban and European markets (Keyder and Yenal 2011). In central Anatolia and in non-irrigated areas, on the other hand, they are not even subsistence producers; they are forced to sell their labor out either as fulltime wage workers or, mostly, as seasonal wage workers, while some members of the household migrate to urban areas.

In the 51-100 decares category, the percentage of farmers was 18.1% in 1963 and 19.5% in 2020; there is not much variation except in 1970 when it dropped to 14.7%. In terms of the percentage of land, 23.9% in 1963 and 19.9% in 2020; there is not much variation in between. In 2020, farmers in the 51-100 category were 19.5% while they cultivated 19.9% of the land. So, they are average farmers, cultivating 70 decares of land on the average, 67% below and 14% above average.

The farmers in the 101-200 decares category are about 12% of farmers cultivating 24% of the land in 2020. They were about 9.4% in 1963 cultivating 23.7% of the land. In 1970 it dropped to 7.1% cultivating 21.8% of land. For the following 50 years it was about 11.5% farmers cultivating 23% of land.

While two thirds of the farmers in the 1-50 decares category cultivate one third of the total land, in 100 decares and above category, approximately one sixth of farmers (18.4%) cultivate 61.8% of total land. This is indeed a rather high level of inequality and hence we can say that agriculture in Turkey is populated by small commodity producers who are unequally distributed across land sizes.

## **The Village Law of 1924 and its Transformations**

So far, my analyses based on statistical land distributions proceeded contrary to Paul Stirling's advice. In the following, I will present, first, The Village Law of 1924 (Law No. 442) and debates about the law, and second, some analytical field notes from my research villages, which I hope would have made Paul happy, by conceptualizing them as Bourdieusian fields.

What is a *köy*/village in Turkey? It has been variously defined as a settlement, an administrative unit in state hierarchy, a field of agricultural and husbandry production, a field of property ownership, a field of labor relations, a field of relations of production, a field of relations of social reproduction, a field of kinship and family relations, a field of educational relations, a field of religious/cultural/ideological relations, and so on and so forth. These immense and abstract definitions would be clarified through a historical-sociological / anthropological account of villages from Byzantium and Ottoman Classical period through land codes and laws enacted during the 19<sup>th</sup> century to the beginning of the Republican period (Keyder 1987). But that is beyond the scope of the present writing. So let me start with a brief summary of the village law that was enacted one year after the establishment of the Republic of Turkey – The Village Law of 1924, with law number 442 (<https://www.mevzuat.gov.tr/MevzuatMetin/1.3.442.pdf>). Further comments will follow on the law and its repeated modifications.

To cite Stirling's evaluation of the Village Law of 1924, "with a few important exceptions the law is remarkable for its irrelevance. It serves rather as a document of the attitudes to the villages of a paternalistic ruling class than as legal basis of village organization." (1965: 271-272) Although I tend to agree with both statements, I think and feel that there is a puzzle here. It is true that this law has not been posted in village administrative buildings or mosques, and has not been learned by village children as stipulated in articles 91 and 94. Furthermore, it has been seldom in the discourses of the villagers when they talk about their villages; yet it is remarkable that what is verbalized in the articles of the law are mostly descriptions of the activities of the villagers stated in a normative-legal style.

Comparing the Village Law of 1924 with the laws and bylaws concerning compulsory agrarian production in the period of World War I and War of Independence, I will assert that the Law of 1924 has been a peace-time continuation of those laws and bylaws. These laws and bylaws were unearthed and put together in a book by Ökçün (1983: 147-172) with a title that can be roughly translated as "Documents Related to Obligation to Work and Cultivate: 1914-1922".<sup>5</sup> During the Grand National Assembly debates in Ankara, in March 1922, the implementation of "obligation to work and cultivate" was ended with the following decision and/or law because the activities in the laws and bylaws were already carried out by the households as natural functioning of village community: "It is approved that the method of Obligation to Work and Cultivate will be implemented in a decentralized mode."<sup>6</sup> (Ökçün 1983: 172) Two years later the Village Law of 1924 was enacted to take care of all matters that were covered by previous laws and bylaws related to villages and agricultural production.

The first article of the Village Law of 1924, defined *köy* (village) as a settlement the population of which was less than 2000, while it defined *kasaba* (small town) as a settlement with a population between 2000 and 20,000, and *şehir* (city) as one with a population of more than 20,000. The second article of the law stipulated that for a settlement with a population less than 2000 to be called a village, it should have a mosque, a school, pasture land, summer pasture and coppice forest all of which should be publicly owned. Of course, there should exist residential houses, gardens and fields mostly within the boundaries of village, operated by the residents of the village. Articles 4, 5 and 6 were about village boundaries and how inter-village affairs would be regulated and disputes about common lands would be settled. Articles 9 and 10 defined *muhtar*

<sup>5</sup> In this book Ökçün (1983) details not only the laws (*kanunlar*) and bylaws (*nizamnameler*) about the organization of agricultural production, but also the debates in the İstanbul and Ankara parliaments, the analyses of which require a separate study.

<sup>6</sup> "Mecburi Ziraat Usulünün Tatbik Edilmemesi Hakkında Heyet-i Umumiyye Kararı (28 Mart 1338/1922, No. 243). Mecburi ziraat usulünün adem-i tatbiki tasvip edildi." (Ökçün 1983:172)

(head of the village) and *ihitiyar heyeti* (council of elders) as the extension of the central state, and obligated them to administer the village by giving orders if necessary.

In the second chapter of the law two lists were given, the first of which included thirty-seven detailed activities that should be done by villagers, such as: Fighting against malaria, bringing water to the village and taking care of wells in the village, building walls to separate stables from living quarters, making sure all houses have water closets and that there is a common water closet for the whole village, building a house for the *muhtar* and the *ihitiyar heyeti*, a mosque and a school, a store shop and a smith shop. Additional activities were specified in this list that were necessary for a village to be a community, like helping orphans and households whose members were in the army. In the second list there were thirty-one activities left to the discretion of the villagers, such as: Building stables away from living quarters, building irrigation ditches for the irrigation of the fields, building places for weekly bazaars and for public laundry, and planting trees on roadsides. Like the ones in the first list, these activities, too, most of which were to be done by *imece* (voluntary collective work), were necessary for a village to be considered a community. A thirty-second activity, bringing electricity to the village, was added in 1954.

The third chapter of the Village Law was on Collective Work (*imece*) and Village Fund or Village Tax (*salma*). The article 15 stated that most of the work related to the welfare of the village community was to be done collectively by all villagers. There were four articles about the collection of the village tax or fund and its spending.

The fourth chapter was about the election of the *muhtar* and the *ihitiyar heyeti*, as well as the village association (*köy derneği*) by eligible men and women in the village. The village imam and teacher were considered as natural members of the *ihitiyar heyeti*, and along with the *muhtar*, they constituted three important institutions and agents in the villages. In a television debate with a group of other social scientists, Şerif Mardin (2017) presented his observation that in urban neighborhoods of Turkey the third agent, the teacher, and the secular culture he represented, failed in Turkey resulting in the return of religion to politics. I will venture to say that Mardin's diagnosis had not been valid for all villages. I will volunteer my own biography as an example. My father was the imam of our village, he would have liked to have me to go to the religious school (*imam hatip* school) instead of the secular high school, but teachers in my village, and also my uncles and brothers, were influential enough to convince him and other members of my family for the opposite itinerary that I have been travelling throughout my life.

The fifth chapter discussed in detail, not only the works and activities of the *muhtar* and the *ihitiyar heyeti* concerning the welfare of the village community, but also the relationships of the village with the state and other villages, towns and cities. Stirling (1965) discussed the *muhtar* in both of his research villages, and concluded the section with the following statement: "In general the headman of a Turkish village exercises no more influence, outside of his specific duties as headman, than he exercises before and after being a headman" (Stirling 1965: 257). Yet, there are widespread fights about the election of the *muhtar*. The power of the *muhtar* depends on his/her<sup>7</sup> wealth, education and the family and lineage they belong to.

About the *ihitiyar heyeti*, village council, the 42<sup>nd</sup> article of village law said that it should convene weekly. Stirling's (1965) observation during his fieldwork in the 1950s is as follows: "It would be harsh to state that these councils never meet. The council in Sakaltutan did not meet during my

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<sup>7</sup> Only very recently women are elected as *muhtar*, especially in urban neighborhoods. As it will be discussed later, many villages in 30 metropolitan provinces became urban neighborhoods by a law enacted in 2012.

stay, and the only function attributed to it by villagers was the supervision of the assessment of the contributions to the village chest. People said the Elbaşı council did meet, but it did not do so regularly, and it did not to my knowledge supervise assessment.” (Stirling 1965: 31) June Starr (1978: 99-106) agrees with Stirling’s observations that the village head and the council were not at all functional in dispute settlement.

My own observations change from village to village or even between two time periods within the same village. In one of my villages, the headman was a relative of the largest land owner in 1966, and hence his power was based on that fact. However, during a revisit in 1979 when the largest land owner’s land was divided among his children, I observed that small land owners’ lands were irrigated and they became farmers with tractors, that the headman was one of the smaller land owners and he was very active in achieving benefits for farmers by fighting the children of the large land owner (Akşit 1967: 117-118; Akşit 1985: 74-86). My experiences with the members of the village council are rare: When I go to a village for research, I first look for the village head, and only if he/she is away for a private or village business, I look for a member of the council to solve my problem.

Chapters six and seven of the Village Law were about legal cases that were considered appropriate for the deliberations of the village council. From the 53<sup>rd</sup> article, it can be gathered that the village council could settle disputes between two consenting parties. However, on the basis of evidence from Stirling’s (1965: 272) central Anatolian and Starr’s (1978: 99-106) Southwestern villages, as well as my own experience, I can safely assert that almost no legal cases were settled by the village council.

The most transformed chapter of the Village Law of 1924 is the eighth chapter, which was about village guards (*köy korucusu*). The 68<sup>th</sup> article of the law stipulated that to protect the honor, chastity, life and property of everybody in the village at least one guard would be hired by the village council with the approval of the district officer. The most modified article of the law, the 74<sup>th</sup> article, was extended with the addition of ten new sub-articles so that village guards were transformed into security guards (*güvenlik korucusu*) of a security state starting from the 1980’s and onward.<sup>8</sup> On the basis of the first sub-article which was enacted in 1985 and modified first in 2007 and later 2017 with a presidential decree by the force of law, the number of guards can be increased up to forty thousand (40,000), and further increased by 50%, if deemed necessary by the President of the Republic of Turkey. According to the second sub-article, which was issued in 2016 with a presidential decree: “The area of responsibility of security guard is boundaries of the village to which the guard belongs. With the approval of the provincial or sub-provincial governor, the area of responsibility can be extended or relocated, temporarily and with a specified duration, beyond the boundaries of the village.” (Village Law 1924: 248) This is not a sociology of law paper; hence I will not go into the details of the fifty amendments and changes made between 1925 and 2021 to the Village Law 1924. I will suffice to mention two changes. The first one, transforming the “village guard” into “security guard,” that I have already mentioned above, is such a radical change that Stirling’s (1965: 271) judgment of the village law as irrelevant is not valid any more.

A second major change, that concerns the status of villages, occurred with the enactment of law no. 6360 in 2012, that changed the Metropolitan Municipality Law of 2004 (law no. 5216). By changing the size criterion, law no. 6360 redefined the provinces with 750,000 population and

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<sup>8</sup> In the Bilge Village of Mardin in 2009, 44 people including children were massacred in a blood feud, during a wedding ceremony, by weapons given to village guards by the state. (Akşit, Eşme, Akşit and Telman, 2009)



above as metropolitan municipalities and increased their number to 30. Villages within these municipalities were thus redefined as urban or rural neighborhoods, and became dependent upon metropolitan municipalities for all kinds of services (Sarı 2019). With this modification in the law, the percentage of rural population dropped from 22.7% in 2012 to 8.7% in 2013 and to 6.8% in 2021. The number of villages that are reclassified as neighborhoods of metropolitan municipalities, 30 provinces, is 16,803 and the number of villages in provinces without metropolitan municipality status, 51 provinces, is 18,335. (TÜİK, 2021; Wikipedia, 2022) With these legal changes, the objects of rural sociological analysis, villages, in Turkey became more complicated and more challenging. There is, of course, the option of switching to a Sociology of Food and Agriculture, yet, unfortunately, very few students choose to study rural sociology or sociology of food and agriculture these days. Doing research on virtual life, culture of real virtuality and social media are much more popular among graduate students.

### Field Notes on Villages as Bourdieusian Fields

In the period between 1979-1982 together with Çağlar Keyder and others<sup>9</sup> we carried out a qualitative research project on Paths of Rural Transformation and Seasonal Migration in which we focused on villages, rather than agricultural households or holdings, as units of analysis, and hence we developed a typology of paths of village transformations (Akşit 1985; Keyder 1983).

Some agricultural engineers and economists whose unit of analysis is agricultural households or holdings objected to our choice of village as a unit of analysis. I believe that sociologists who are familiar with Bourdieu's (Bourdieu and Wacquant 1992:94-140) theory of fields –where agents/actors compete with each other in relatively autonomous fields and/or subfields in order to access and gain more resources/profits using their economic, cultural and social capitals appropriate to each field with their motivationally embodied dispositions, skills and knowledge, in short, their habitus– will be more open to accept our approach as legitimate, both epistemologically and ontologically. I will reexamine our research villages and their paths of transformation and see whether they can be considered as Bourdieusian fields.

In our research, we divided the initial status of villages into three categories. The first category was large land holding dominant villages. We studied four villages in this category: one in the Southeastern, two in the Mediterranean, and the fourth in the Aegean regions. The second category was small land holding villages with fertile lands. We studied two villages: one in Southern Central Anatolian and the other in Mediterranean regions. The third category was small land holding villages with poor soil quality. One of the two villages we studied in this category was in the Inner Aegean, and the other in the Northern Central Anatolian regions (Akşit and Keyder 1981:1-8; Akşit and Keyder 1985: 87-104).

I will present the transformation of large land holding villages starting with a village in the Diyarbakır province. Our research team visited this village in 1980. Quite unexpectedly and very interestingly, we found the village in the middle of transformation and bitter processes of class struggles and conflicts of “enclosure” or resistance of peasants to being thrown out of the village

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<sup>9</sup> Çağlar Keyder, Bahattin Akşit, Tosun Arıcanlı, Adnan Akçay, Huri İslamoğlu, Nükhet Sirman, A. Ali Gürkan, David Seddon, Ayşe Saktanber and Neyir Kalaycıoğlu were team members of the research Project “Paths of Rural Transformation and Seasonal Migration” funded by Middle East Awards, MEAWARDS, which was supported by Ford Foundation, Population Council and IDRC.

by the landlord. Actually, at the time of our visit, a large part of the landlord's land was being cultivated with 12 or so tractors and complementary modern agricultural technology in the context of capitalist relations of production. However, there were, also, other parts of the land cultivated in the context of feudal, semi-feudal and commercial relations by the dependent households. Some households were dispossessed of any kind of sharecropping relationship and were being forced to leave the village, while they resisted this attempt by opening multiple court cases against the landlord. Some households had bought some land from the landlord and had become small commodity producers, while others were cultivating land in different combinations of sharecropping and renting land from the landlord. Within the scope of this article, it is not necessary to go into more detail.<sup>10</sup> In Bourdieusian terms, there was one dominant actor with economic capital in this village, reinforced with the political and juridical capital of the state. Dependent peasant households had very weak political and juridical capital on their side to resist expulsion from the village; they were somehow fortunate that there was not a consolidated landed oligarchy in Turkey, and hence they were also able to open court cases and resist being thrown out of the village. This village is indeed a Bourdieusian field with all its struggles using economic and political-juridical capitals and occasionally social capital, considering the academics and graduate students coming and listening to their cause, writing articles and theses.

The second large land holding village was in the Adana province. As a matter of fact, there were two large land holding villages where capitalist relations became dominant earlier, studied, in the 1960's, by Kıray (1966), Kıray and Hinderink (1967 -1968) and Hinderink and Kıray (1970). In the framework of our research project in 1980, we chose a third such village. This village was also a differentiated field where there were six capitalist farmers. While the one with the largest land had modern equipment like tractors, the other five with relatively smaller land competed with each other for wage labor, inputs, and marketing their produce. There were around 10 small capitalist farmers using their own labor and seasonal labor and around 10 small commodity producers using their own labor only and sometimes hiring out labor.<sup>11</sup> Rest of the villagers, about 60 households, were permanent and seasonal wage laborers, who were competing with each other for employment. This village, too, exemplifies field dynamics that operate with hierarchically organized actors.

The third large land holding village was in the Antalya province. The study of two historical moments of transformation of this village has been a part of my career as a rural sociologist. The first moment was in 1966, when I had conducted field research as a third-year sociology student. The second historical moment was in 1979, during our research for the Paths of Rural Transformation project. In the introduction of my report for the first historical moment, I stated from the outset that I was going to study changes in the societal structure of the village due to the expansion of capitalism from Western Europe. My translated long sentence is as follows:

By focusing on the transition from subsistence production to production for market, and by focusing on the interrelationships between them, the following will be examined in a holistic way: the changes in means of production, primitive capital accumulation, land polarization, social differentiation, changes in stratification, adjustment of families to

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<sup>10</sup> For those who are interested in more data and analyses, there is a PhD thesis by one of my graduate students, who carried out further research in this village (Akçay 1985; Akşit and Keyder 1985: 95-96).

<sup>11</sup> The differentiation of capitalists as capitalist, small capitalist and small or petty commodity producers depending upon amount of capital owned and use of wage labor and self-labor is borrowed from Erik Olin Wright (1985).

changing conditions and changes in the system of values parallel to the changes in technology. (Akşit 1967: 72)

In 1966, based on my interviews with the representatives of capitalist landlords and with a sample of household heads from share croppers, I had already learnt the history of the village, that was the settlement of a *Yörüük* tribe in their winter quarters. During the 1930's and the 1940's the head of the tribe had become the landlord as the legal ownership was secured from the state, without the consent of the rest of the tribe who became share-cropping households. During the 1950's and 1960's the landlord started producing cotton in the irrigated part of the land and built a large capitalist farm. In 1966, the capitalist landlord was cultivating cotton using 12 tractors, accompanying modern equipment and skilled wage laborers in the irrigated land, while share croppers were producing cereal in the non-irrigated parts of the land. They claimed that all of the land in the village belonged to them, although usurped by the landlord (Akşit 1967: 71-127). In 1979 when I revisited the village after 13 years, together with another member of our research team, Tosun Arıcanlı, we found the village field to be reconstructed. Cotton was produced now, in the formerly cereal produced land due to new irrigation canals built by the Irrigation Institute of the state, and hence the former share croppers had turned into petty commodity producing farmers, reclaiming both the land and the post of the village head from the control of the large land-holding capitalist farmer whose large farm was, in the elapsed time, divided into medium sized capitalist farms when inherited by his children (Akşit 1985: 74-99).

The fourth village in this category that was in the Manisa Province was a large land-owning village in the 1930's, but starting in the 1940's, and especially during the 1950's and 60's, it turned into a village with differentiated land holdings. This type of villages was one group of villages in the planes of Manisa, studied by Behice Boran during the 1940's. The second group of villages studied by her were mountain villages (Boran 1945). We had chosen this village to study the disintegration of large land holdings and fragmentation in a 50-year perspective. However, due to the short research period in this village, we were only able to observe land fragmentation from generation to generation and migration without going into the details of field dynamics.

Our first village in the second category, was in Polatlı, a sub-district of the Ankara province. The residents of this small land holding village with fertile lands were some 80 households that were settled there during the second half of the 19<sup>th</sup> century and were given equal amounts of land, producing wheat as subsistence for many years and as cash crop after the 1950's. Some 40 households migrated to cities and other regions selling their land to remaining households exemplifying a field dynamic of avoiding fragmentation. The result was the emergence of rich farmers, producing large amounts of wheat as cash crop, what we called the "kulak" farmers, from the Russian example; the path of transformation in this case was towards commodity producing farmers.

The second small land holding village with fertile lands that we studied, our sixth village, was a cotton producing village in Antalya, which I had studied in 1966 on my own and with Tosun Arıcanlı in 1979, as a part of the "Paths of Rural Transformation" project. The transformation of this village started during the 1930's and 40's with the cultivation of sesame as a cash crop, leading to small capital accumulation. As the demand for sesame seed increased, some of the peasants obtained a second and third pair of oxen and/or a pair of horses, and added common lands to their private ownership. As consumption patterns of villagers changed, some of the peasants added grocery store and trading activities to their agricultural activities. As capital accumulation continued during the 1950's and 60's, some of them became small capitalists hiring permanent and seasonal waged workers. Some of them further added new commercial activities to their

capitalistic farming such as establishing transportation and trading firms (Akşit 1967: 77-98). Our observations in 1979 showed that the village was further differentiated by the amount of land owned and cultivated, and in terms of hiring wagedworkers and/or renting labor. Thus, it had become disintegrated and fragmented like the village in Manisa. Small capitalists did not become large land-owning capitalists, and some capitalists who owned more land than others, who had the potential to become large land-owning capitalists, ended up becoming small capitalists by fragmentation of land among progeny (Akşit and Keyder 1985: 100-101). The field dynamics of rivalry, competition, differentiation and hierarchization with different combinations of economic and social capital is waiting for intensive participant observational research.

Our seventh village, one of the two small land holding villages that we studied, with poor soil quality, was in the Uşak province of the Inner Aegean region. In this village, unable to accumulate capital by extensive production of cash crops, the village residents managed to remain in the village by engaging in seasonal labor in 1940's and 1950's, but later concentrating in small scale diversified economic activities such as dairying, livestock fattening, weaving, tobacco farming, viniculture, poplar tree growing, fruit growing, vegetable gardening and poppy growing. These diversified activities were developed as an alternative to seasonal and permanent migration. Thus, field dynamics in this village were based on new, small-scale income generating activities.

The second small-land-holding village with poor soil quality, the eighth village we had chosen, was in the Çankırı the province of the Northern Central Anatolian region. Unlike the Uşak village households, instead of diversification of economic activities, these ones had chosen permanent migration. Only the older people remained in the village, while their children and grandchildren visited during summer vacations. We also observed that some of the old people were returnees in the village after retiring from their jobs in the cities. This village exemplifies field dynamics where migration is chosen as a household strategy of survival and the village is almost deserted.

## **Concluding Remarks**

As discussed above, villages were categorized on the basis of initial land distribution: equalitarian land distribution versus large land-holdings with dependent producers; fertile lands versus poor quality lands which included regional and intraregional diversification. Emergence of divergent paths of transformation depended on field dynamics. One nonverbalized implicit initial condition could have been Stirling's (1965: 134-141) "land in plenty" versus "land shortage" models. The deserted village type that was exemplified by the Çankırı village initially fit the "land shortage" model, but at the end of the transformation it conformed to the "land in plenty" model. With poor soil quality, however, the choice of the migration option eventually left the land uncultivated. The village in Uşak, also started off fitting the "land shortage" model; however, unlike the Çankırı village, households opted for seasonal migration instead of permanent migration, and turned to various non-agricultural economic activities in the village to ensure survival through diversification, in spite of land shortage and poor-quality land. The cotton producing village in Antalya, the one starting with relatively equal land distribution and rich soil quality, can also be considered, initially, a "land in plenty" village which was transformed into a "land shortage" village. In that case, with a fiercer field dynamic of competition and differentiation a stratified social structure emerged, upper levels of which became "kulak" or rich villagers. These rich villagers are in between small commodity producers and small capitalists. They employ seasonal wage workers, whereas small commodity producers use their own labor, hiring only occasionally seasonal wage labor. There are landless or very small landed villagers who live by hiring out their

labor. The wheat producing village in Polatlı, starting with equal amounts of land, was initially a “land shortage” village which later differentiated with field dynamics of the so-called push and pull factors, half of the households migrating out and the other half remaining in the village as commodity producing rich farmers. So far as the large landholding villages are concerned, the main dynamic is class struggle between large land holders and dependent villagers, share croppers. Land quality, land being in shortage or plenty are secondary factors.

Another remark will be about Bourdieu’s research on his village of birth as one of the illustrations of symbolic violence (Bourdieu and Wacquant 1992: 162-168). According to Bourdieu, France’s rural population declined through symbolic violence, while in the case of Soviet Union the declining rural population is the result of collectivization which used political and physical violence. I contrast Turkey with both France and the Soviet Union as a hybrid example combining both symbolic and political violence. This explication attracts the attention of students when I lecture on Bourdieu’s theory of symbolic domination and violence. In this discussion, I use İncirlioğlu’s (1993: 115-125) research in Stirling’s villages as my example of declining rural population through symbolic violence (Akşit 1998). Bourdieu had carried out research in his village during different periods of his life. In one of his visits, he was invited to a bachelor’s ball where he observed his old friends as bachelors and very few women were present. As he deepened his investigations, he discovered a typical example of symbolic violence where these bachelors were unwitting accomplices in forbidding their own reproduction by developing marriage strategies that encouraged their sisters to marry urban men rather than village men thereby being unable to find suitable brides for themselves (Bourdieu and Wacquant 1992: 162-168). In a paper on marriage strategies of villagers in villages of S and E, İncirlioğlu reported that families wanted their daughters to marry urban man with a good occupation, because life in the village for women is very difficult and unpleasant, symbolically violent in Bourdieu’s terms, while urban life is much easier and more comfortable (İncirlioğlu 1993: 118-119).

My last remarks will be on two observations from a friend (Selçuk 2022) on changing gender relations and marriage patterns. The first one is from a village, nearby my village in the Denizli province, with an older name Kösten, changed by bureaucrats to Aydınlar. As the new name of the village indicates the population of the village is rather “enlightened” with educated village girls who have urban jobs while boys, being the valued sons of their families and being accomplices to their incomplete education, remain in the village to take care of their ageing parents and the farms. As a result of these developments a sizeable group of bachelor men are unable to find brides in their own village, like in Bourdieu’s village. Therefore, families search for brides for their sons in less developed villages in the area. Occasionally they are successful in finding some candidates for brides to be brought to the village, but mostly unsuccessful. Selçuk’s (2022) second example, from a village in the Çorum province, is a relatively extreme form of search for a bride. In this case, the family decided that their son should marry one of the girls in a village from eastern Turkey, paying a high bride price through a marriage arranger, a petty entrepreneurial version of a matchmaker. The bride after being brought to the village and to the home of the bridegroom asked to be left alone in the wedding room, with her suit case, understandably being so tired after such a long journey. After a few hours, she emerged from the wedding room dressed as police, with a weapon in one hand and her suitcase in the other, and asked to be escorted to a bus stop to go to the city. Nobody knows whether she will return to her family in her home village or disappear in the “urban jungle” or “urban welfare and freedom,” depending on her habitus, education and life strategies.

## References

- Akçay, Adnan. 1985. Traditional Large Land Ownership and Its Transformation in Two Southeast Anatolian Villages. Unpublished PhD thesis, Department of Sociology, Middle East Technical University.
- Akşit, Bahattin. 1967. Türkiye’de Azgelişmiş Kapitalizm ve Köylere Girişi. Ankara: ODTÜ Öğrenci Birliği Yayını.
- Akşit, Bahattin. 1985. Köy Kasaba ve Kentlerde Toplumsal Değişme. Ankara: Turhan Kitabevi Yayını.
- Akşit, Bahattin. 1993. Studies in Rural Transformation in Turkey 1950-1990. In Stirling, Paul (ed.). Culture and Economy: Changes in Turkish Villages. Huntingdon: Eothen Press, pp. 187-200.
- Akşit, Bahattin. 1998. İçgöçlerin Nesnel ve Öznel Toplumsal Tarihi Üzerine Gözlemler: Köy Tarafından Bir Bakış. In: Ahmet İçduygu (ed.). Türkiye’de İçgöç: Konferans, Bolu-Gerede 6-8 Haziran 1997. İstanbul: Türkiye Ekonomik ve Toplumsal Tarih Vakfı yayınları, pp. 22-37.
- Akşit, Bahattin. 1999. Cumhuriyet Döneminde Türkiye Köylerindeki Dönüşümler. In: Baydar, Oya (ed.). 75 yılda Köylerden Şehirlere. Bilanço ‘98. İstanbul: Tarih Vakfı Yayınları, pp. 173-186.
- Akşit, Bahattin. 2005. Doğrudan Gelir Desteği Uygulamaları ve Geleceğe Yönelik Eğilimler. Paper presented at 9. Ulusal Sosyal Bilimler Kongresi, 7-9 December 2005, Türk Sosyal Bilimler Derneği, Orta Doğu Teknik Üniversitesi, Ankara.
- Akşit, Bahattin. 2020. Teori ve Metot İlişkileri Çerçevesinde Alan Araştırmaları(m): 1966-2016. Sosyoloji Sohbetleri 2014-2016. Ankara: Başkent Üniversitesi, Fen Edebiyat Fakültesi, Sosyoloji Bölümü Yayınları.
- Akşit, Bahattin and Çağlar Keyder. 1981. Kırsal Dönüşüm Tipolojisi. In: Yeni İmece 8-13.
- Akşit, Bahattin and Çağlar Keyder. 1985. Kırsal Dönüşüm Tipolojisi. In: Akşit, Bahattin (ed.). Köy Kasaba ve Kentlerde Toplumsal Değişme. Ankara: Turhan Kitabevi, pp. 87-104.
- Akşit, Bahattin, İsa Eşme, Belma T. Akşit and Nursel Telman. 2009. Mardin’in Bilge Köyü’ndeki Katliam Olayının Sosyolojik, Eğitimsel ve Psikolojik Analizi ve Uygulama Olanaklarının Değerlendirilmesi: Bir Keşif Araştırması. İstanbul: Maltepe Univesity, MÜTAM Research Center. <https://www.maltepe.edu.tr/mutam/tr/tamamlanmis-projeler>. Accessed 4.02.2022.
- Bourdieu, Pierre and Loic J. D. Wacquant. 1992. An Invitation to Reflexive Sociology. Chicago: University of Chicago Press.
- Butler, Judith. 2005. Giving an Account of Oneself. New York: Fordham University Press.
- Hinderink, Jan and Mübeccel Kıray. 1970. Social Stratification as an Obstacle to Development: A Study of Four Turkish Villages. New York: Praeger.

Hindess, Barry. 1973. *The Use of Official Statistics in Sociology: A Critique of Positivism and Ethnomethodology*. New York: Macmillan Press.

İncirlioğlu, Emine Onaran. 1993. Marriage, Gender Relations and Rural Transformation in Central Anatolia. In Stirling, Paul (ed.). *Culture and Economy: Changes in Turkish Villages*. Huntingdon: Eothen Press, pp.115-125.

Keyder, Çağlar. 1983. Paths of rural transformation in Turkey. In: *The Journal of Peasant Studies* 11 (1): 34-49.

Keyder, Çağlar. 1987. *State and Class in Turkey: A Study in Capitalist Development*. London: Verso Books.

Keyder, Çağlar and Yenal, Zafer. 2011. Agrarian Change under Globalization: Markets and Insecurity in Turkish Agriculture. In: *Journal of Agrarian Change* 11 (1): 60–86.

Kıray, Mübeccel. 1966. Interdependencies Between Agro-Economic Development and Social Change: A Comparative Study in Çukurova. Abant (Bolu) Symposium.

Kıray, Mübeccel and J. Hinderink. 1967-1968. Interdependencies Between Agro-Economic Development and Social Change: A Comparative Study in Çukurova. In: *Journal of Development Studies* 4 (4): 497-528.

Mardin, Şerif. 2017. Prof Şerif Mardin Mahalle Baskısı kavramını anlatıyor. <https://www.youtube.com/watch?v=E3yU3TVjMHM>. Accessed 19.10.2022.

MOAF. <https://www.officialstatistics.gov.tr/en/detail/subject/agricultural-holding-registry-system>

Otaran, Arkaç. 1985. *A Critical Review of Literature on Popular Culture: A Case of Cinema in the Constitution of Subjectivity*. Unpublished MA thesis. Ankara: Sociology Department, Middle East Technical University.

Ökçün, Gündüz. 1983. *Tarımda Çalışma ve Ekme Yükümlülüğü (Mükellefiyet-i Ziraiyye) Belgeler: 1914-1922*. Ankara: Ankara Üniversitesi Yayınları.

Sarı, Özgür. 2019. Transformation of Metropolitan Municipalities in Turkey and Law No. 6360. In: *The Journal of Kesit Akademi* 5(21): 1-11.

Selçuk, Yaşar. 2022. *Some Observations on Girls' Education and Marriage in Villages*. Unpublished Observational Notes.

Starr, June. 1978. *Dispute and Settlement in Rural Turkey: An Ethnography of Law*. Leiden: E. J. Brill.

Stirling, Paul. 1965. *Turkish Village*. London: Weidenfeld and Nicolson.

Stirling, Paul (ed.). 1993. *Culture and Economy: Changes in Turkish Villages*. Huntingdon: Eothen Press.

TÜİK. 1965. 1963. Genel Tarım Sayımı Örnekleme Sonuçları (Census of Agriculture). Ankara: TÜİK Yayınları.

TÜİK. 1973. 1973 Genel Tarım Sayımı Sonuçları (Census of Agriculture). Ankara: TÜİK.

TÜİK. 1994. 1991 Genel Tarım Sayımı Örnekleme Sonuçları (Census of Agriculture). Ankara: TÜİK Yayınları.

TÜİK. 2004. 2001 Genel Tarım Sayımı Örnekleme Sonuçları (Census of Agriculture). Ankara: TÜİK Yayınları.

TÜİK. 1995. Türkiye Nüfusu, 1923-1994: Demografi Yapısı ve Gelişimi. Ankara: DİE.

TÜİK. 2001. <https://www.tuik.gov.tr/>

TÜİK. 2021. <https://data.tuik.gov.tr/Bulten/Index?p=Adrese-Dayali-Nufus-Kayit-Sistemi-Sonuclari-2021-45500>. Accessed. 7.02.2022.

Village Law of 1924. T.C. Resmi Gazete. 1924/2022, Köy Kanunu, Kanun no: 442, Mevzuat, pp. 237-260. <https://www.mevzuat.gov.tr/MevzuatMetin/1.3.442.pdf>. Accessed, 1.02.2022.

Wikipedia. 2022. [https://en.wikipedia.org/wiki/Villages\\_of\\_Turkey](https://en.wikipedia.org/wiki/Villages_of_Turkey). Accessed. 7.02.2022.

Wright, Erik Olin. 1985. *Classes*. London: Verso Books.



**Kayhan Delibaş**

## **The Driving Factors of Turkey's Foreign Policy Shifts from West to East: Turkish Politics and Society After Paul Stirling**

### **Introduction**

I met Paul Stirling in 1994, in the beginning of my academic career when I arrived at the University of Kent as a post-graduate student in the Department of Anthropology and Sociology. He was more than a mentor to me – a father-figure, a close friend with an academic depth and warm welcoming attitude, he was one of the last of a *dying-out species of academics* who valued interaction with students and did not see time spent with them as distraction or wasted. Although the 1990s were, clearly, the peak of the neoliberal era at universities and professors were under the pressure to 'publish or perish', Paul was not in that mood. Meeting him was undoubtedly my greatest fortune. His unique experience and decades of academic work continue to lead my way in the intervening twenty years, as it did then.

In early 1994 when my post-graduate studies began at Kent, Paul was still a very active professor emeritus, although he was retired. During the four years we worked together he did not spare either his friendship or academic support. In the early summer of 1998, before a seemingly ordinary weekend, on a Friday afternoon, he hurriedly found me in the Postgraduate Study Room of Eliot College. Apparently, someone was waiting for him outside to go to London. He gave me my essay that he had read and marked with his green pen, saying 'you read the notes I wrote, and we'll talk about it on Monday'. Unfortunately, that Monday never came.

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My contribution to this volume for commemorating Paul Stirling 25 years after he passed away, will examine the multidimensional change Turkey has witnessed under the political Islamist government and its impact on relations with the Western world. This chapter aims to analyse the first ten years of Turkey's Justice and Development Party (*Adalet ve Kalkınma Partisi*, AKP) rule (2002 to 2012) and the seismic shifts Turkey experienced from a socio-cultural and political perspective, including the deterioration of relations with the West and the EU. The socio-cultural and political dynamics of the 'axis shift in foreign policy' will be discussed in the light of empirical data in order to explore general perceptions, feelings, and attitudes towards the AKP government's changing foreign policy priorities in general and Turkey's orientation, as well as specific views about EU membership. Various explanations by, and concerns of, Turks about their role in the world will be examined. It will be argued that none of these major political changes can be justified or explained primarily on the basis of external factors, but rather must be explained by wider socio-political and cultural concerns or shifts that took place since the early 2000s. As an MA student who witnessed Paul's last years, I will try to trace the radical changes Turkey has undergone in the twenty-five years since his death and compare the conditions with those of the country he studied for years. In line with his

approach, I aim to discuss the ‘causes and consequences’ of social, political, and cultural changes after 2002.

In the light of the events in the late twentieth century, Turkey’s standing in world politics was perceived as rather critical for the attainment of peace, security, and the survival of democracy in the Middle East, currently one of the world’s most troubled regions. For many international observers, Turkey had the potential of becoming a role model for the rest of the Muslim world. But after AKP’s succession to power in 2002 that role was now being considered as ‘problematic’ by many within and outside Turkey. Since the 2010s Turkey’s historic strong ties and emotions towards the West have been experiencing major setbacks.

There have been various explanations and theories for Turkey’s re-examination of its attitudes towards the western world. For some, Turkey was turning its back to the western world to lead the Middle East. For others, the ruling AKP was turning to its Islamic roots and using the incidences like *Mavi Marmara*, the Gaza war, and the Gaza blockade—key turning points—as political leverage to re-Islamise the country.

The media coverage in the West as well as in Turkey indicated the changing attitudes, perceptions and anxieties of Western authorities towards Turkey’s newly developed foreign policies. For example, Denis MacShane from Newsweek (July 23, 2010) argued that: ‘In the cold war years Turkey was unquestionably accepted as the West’s most important frontier nation. Now it seems to prefer coddling Iran over backing the U.N. Security Council’s harder line against Tehran. Disputes with Israel, once a key friend of Turkey, have become so bad, there is almost a rupture between the only two democracies in the region’. The deterioration of relations between two friends started with ‘the one minute incidence in Davos’ and it came to rock-bottom in 2010 with the deadly Israeli commando raid on the Turkish flotilla *Mavi Marmara*. Especially the unexpected and unjustifiably tough reaction of Israeli commandos—the killing of nine Turkish activists on board—was a breaking point in this once very close friendship. For some commentators, the *Mavi Marmara* incidence was a key turning moment. Almost in one night, Israel, a close ally, turned into a number one threat. Turkey quickly condemned the attacks as ‘an act of state terrorism’ and demanded an UN investigation to find out the culprits. The US’s slow response to Israel’s harsh treatment was a sign of another problem in Turkish western relations. In this context, one of the key aspects of this new era was the new foreign policies developed by foreign minister Ahmet Davutoğlu, namely ‘zero problems with neighbours.’ For Roger Cohen (2010) of New York Times, ‘He is the man behind Turkey’s “turning East” as Iran’s friend, as Israel’s foe, as a fickle NATO ally wary of a proposed new missile shield, and as the wily architect of Turkey’s new darling status with Middle East’. What was important is that these newly emerging multi-dimensional changes were going to shape and dominate the AKP’s next ten years in office.

These developments, at that time, caused suspicion and deep anxieties in Western quarters and were widely discussed among western commentators as well. For example, Simon Tisdal (2010) argued that: ‘Erdogan’s regional foreign policy initiatives, his flirtation with Iran, his split with Israel, and his courting of supposedly suspect countries such as Syria have led western commentators to speculate about a “strategic realignment” in Turkish policy, away from the west and NATO and towards the Arab and Muslim worlds, in parallel with the AKP’s pursuit of a neo-Islamist agenda at home’.

In a similar vein the New York Times commentator Thomas L. Friedman (2010) went even further to claim that: ‘Turkey’s Islamist government [seems] focused not on joining the

European Union but the Arab League — no, scratch that, on joining the Hamas-Hezbollah-Iran resistance front against Israel’ (See also Tavernise and Slackman, 2010). Undoubtedly some of these comments, observations and discussions that mostly reflected western objectives were exaggerated.

In many ways, these newly emerging shifts in foreign politics in the early 2010s have been dominating not only Turkey’s relations with the west but paved the way to seismic shifts in political, economic, and sociocultural levels at home as well. What were the reasons for Turkey’s shifting orientation? And how was this ‘axis shift’ viewed by society? This chapter is an attempt to answer these questions by looking at several variables.

To address these questions, especially to reveal the perceptions regarding EU membership, I conducted a small-scale research project in 2010 with locals in the Aydın province in southwest Turkey, combining quantitative and qualitative methods. First, a small-scale survey / questionnaire was conducted (N=100) to investigate beliefs, attitudes and perceptions of the public on the subject matter. Then, 20 one-to-one in-depth interviews were conducted to have a better and detailed understanding of the results. The sample was chosen, by using simple random sampling technique, to represent different socioeconomic segments, in order to reveal how these perceptions differed according to socio-economic and demographic variables.

## **History and Social Change**

### **Turkey as a Bridge between the West and the East**

Turkey is a country where social change is extremely rapid, multidimensional, and complex. Social change has been one of the most consistent themes in the social sciences. Pioneers of social theory such as Hegel, Comte, Saint-Simon, Spencer and many of their contemporaries commonly used terms such as development, evolution or progress to describe and explain the changes in societies of their time. The adoption of a more neutral term followed the publication, in 1922, of W.F. Ogburn’s ‘Social Change’ (Bottomore 1979: 290). Social change is relentless, inevitable and universal. The only exception is the unchanging status of change itself. However, not all human societies change in the same direction or at the same rate. Antony Giddens (1995) argued that ‘for virtually the whole of human history, the pace of social change was relatively slow; most people followed more or less the same ways of life as their parents. By contrast, we live in a world subject to dramatic and continuous transformation’ (1995: 225). The factors that cause social changes vary from population movements such as migration or tourism to technological and scientific innovations and developments, from cultural factors to external environmental conditions.

The process of globalization has exacerbated what Harvey (1995: 240) termed ‘the time-space compression’; geographical distances and time differences have diminished or have lost their importance. The time-space compression led to a massive increase in the flow of information and knowledge leading to an increase of inter-societal interactions between countries, societies, and cultures such that the rate of social change has enormously increased. In the last quarter of the twentieth century the acceleration of social change we have witnessed across almost all societies has been at a rate far in excess of anything experienced before. Interpreting this development, the anthropologist Peter Worsley (1984) argued that ‘until our day human society has never existed’ meaning that it is only in quite recent times that we can speak of forms of

social association which span the earth. In this regard, social change at many levels and in many aspects has been one of the most fundamental characteristics of Turkish society since the mid-nineteenth century. There are few countries where the concept of social change fits so precisely as to Turkey—a country where social life has changed so rapidly and extensively. Nevertheless, social change took an entirely new meaning especially under the rule of the AKP government since 2002.

The seeds of Turkish modernization were sown during modernization movements that went back to the 16<sup>th</sup> and 17<sup>th</sup> centuries. This period commenced when the Ottoman armies began losing wars and land to European powers (Lewis 2002: 25), especially after the second unsuccessful siege of Vienna between July 17 and September 12, 1683. This historic defeat was described as ‘a calamitous defeat, so great that there has never been its like since the first appearance of the Ottoman state’ (Lewis 2002: 18). For Lewis ‘the lesson was clear, and the Turks set to work to learn and apply it’ (2002: 19). The process of learning and application was going to be a long and troublesome journey and at the end of the two hundred years of reforms, attempts were not enough to rescue one of the world’s once greatest empires from collapse.

If one needs to find a distinctive historical moment where rapid changes began in the Turkish history, that significant moment would appropriately be the proclamation of *Tanzimat* in 1839, when political reforms were made in the Ottoman state. Western superiority depended on better armament technology, as well as better training and disciplined methods of these armies. In addition to the military sphere, there were advances in Europe in economic, educational, bureaucratic and political areas. Therefore, Muslim rulers in the Ottoman Empire, as in Egypt and Iran, looked to the West to modernize their military, politics, and economy (Esposito 1995: 54). Thus, the *Tanzimat* period was an important milestone in the modernization process of the Empire. While modernizing the Empire, it wide-opened the doors and moved it closer to Western Europe. This rapprochement between Europe and Turkey continued until the present day (Keyder 1987: 29).

A new regime was announced. For the first time, the political, legal, and educational institutions began to change in a way that involved basic social values (Berkes 1964: 137). In many ways the *Tanzimat* period was the ‘Renaissance’ of the Ottoman Empire (Ortaylı 2007: 17). With the beginning of Renaissance, an era was ending in the Ottoman Empire. The new elite began to acquire Western ideas as part of a counter-offensive against the West’s increasing domination of trade in the Empire (Berkes 1964; Ortaylı 2007). The *Tanzimat* period was the age of reforms that intended to make the Ottoman Empire a modern nation like the rest of Europe while at the same time implying it as a ‘bridge’ between the West and the Muslim world.

Although the philosophical foundations of this ‘bridge’ can be traced to the modernization movements of the 19<sup>th</sup> century in the Ottoman Empire, the heart of the matter in this process emerged with the birth of the Turkish republic in the early 20<sup>th</sup> century. Thus, the concept of bridge itself became the process as well as the product of Turkey’s modernization and westernization project.

It was the nationalist, secularist elite led by Atatürk who constructed cultural, social, economic, and ideological components throughout the 1920s and 30s. ‘Geographically, of course, Turkey straddles Europe and Asia. In cultural terms as well, modern Turkey is very much a product of both Eastern and Western influence’ (Fuller and Lesser 1993: 100; Lewis 2002). Turkey did not actively enter the World War II, and made neither friends nor foes; however, the end of the

war signalled a new world order and Turkey was forced by the conjuncture to choose her side (Shaw and Shaw 1978: 399-400). Since 1952, Turkey has played a full part in most Western and European international institutions from NATO to the OECD to the Council of Europe' (Barchard 1985). From the Western point of view, Turkey's geographical position makes her a valued strategic ally. With its geography, culture, common history, and shared religion with the Muslim world as well as the West, Turkey has become a bridge where East meets West. Literally Turkey became a bridge that connects two continents since its foundation in the 1920s.

In his influential study of Turkish history, *The Emergence of Modern Turkey*, Lewis (1962) traces the long history of bridge building in two different periods of Turkish history.<sup>1</sup> Area specialists like Fuller and Lesser (1993) point out that 'the notion of Turkey as a bridge between East and West is a pervasive theme among the political and economic elite in Turkey and sympathetic observers elsewhere. As Turks are quick to point out, Westernization in Turkey is not simply a product of *Atatürkism*. It is much older, and can be observed, for example, in the Byzantine influences on Ottoman society' (Fuller and Lesser 1993: 100). It can be said that this conception of Turkey as a bridge between East and West has been engrained in Turkish social, cultural, political, and economic structures.

Successful modernizing reforms and secularization began with the foundation of the Turkish Republic in 1923, although there were many previous attempts to modernize the military, education, administration and jurisdiction. When the Turkish Republic was established by Mustafa Kemal in 1923 it was a poor peasant society. Traditional agriculture and craft-based production systems were destroyed by the wars and its small industry was severely damaged. The Kemalist revolution of the 1923 shaped the direction and characteristic of the modern Turkish political system (Ahmad 1977: 2). By the middle of the 1930s the new regime consolidated itself. Modernization, secularization and rationalization were established in the urban area, which consisted of about 20 percent of the overall population. The Republican ideology was stronger, especially among the young generations. 'Since then, Turkey has experienced a truly startling rate of change.' (Stirling 1994: 6)

The nationalist movement began its radical secularization program by abolishing the Caliphate, the divine arm of the empire. Religious courts and old religious schools (*medreses*) were replaced by modern ones. Modern and secular institutions replaced all of the traditional and religious institutions: The civil code and the Arabic script, the dress code, the calendar and all measurement systems were replaced with Western and secular equivalents.

For ordinary Turks, the World War II years brought further poverty and deep anxieties. Atatürk's death was a serious setback for the young republic and could potentially harm the revolutions started by him. During the war years all sectors of the economy suffered significantly. Especially the agricultural production plummeted. Wheat production shrunk by 50 per cent (Kongar 2001). This situation led to a price increase in basic commodities, high rate of inflation and a sharp decline in the wages and salaries. The government introduced new and high taxes to finance the increased military expenditure.

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<sup>1</sup> For Lewis, Turks made two important decisions to close the geographic and cultural rift that separated them from the West. The first choice was the migration of Turkish people from central Asia to Europe that took place in the Middle Ages. On this foundation he establishes a long history of Turkish modernization in a linear progress. For Lewis this progressive process was halted with the penetration of Islamic and Arabic influences on the Ottoman Empire. The second phase started when reformist elites in the Ottoman turned their face to West as a model and source of inspiration for reform in the 18<sup>th</sup> century (1962: 353).

However, following the end of the war this situation changed rapidly. The population grew from 13 million in 1923, to 21 million in 1950. The growth rate of the Turkish economy was impressive as well, growing three times over the same period (Stirling 1994: 8). From the early 1950s onwards, Turkey witnessed an extensive and multi-dimensional social transformation.

### **Turkey's Relations with the West after World War II**

The transition from a single-party regime to a multi-party system in 1946 and the Democrat Party's (*Demokrat Parti*, DP) accession to power in 1950 signalled a new phase in Turkey's development. The Democrat Party's victory in the elections of 14 May 1950 'constituted a fundamental break in Turkish history' (Keyder 1987: 124). The Republican People's Party (*Cumhuriyet Halk Partisi*, CHP) represented the status quo until then, with a centrist, statist, and elitist position. Since the DP stood on the periphery, this position was changed for the first time since the 1920s. It was not a surprise that the DP received widespread public support from many different social groups and classes in rural as well as urban areas.

In the 1950s, with the financial backing of the US, the DP government undertook a restructuring program. In line with restructuring programs that were much more market oriented, a relatively liberal economy replaced CHP's closed, statist economic policies. Due to these policy changes 'the first years of Democratic Party (DP) rule witnessed radical economic and political transformations' (Keyder 1987: 127, see also, Hale 1981: 86-113; Şenyapılı 1982: 239). In addition, the Marshall aid, which was put into practice after the war, expanded the use of tractors and mechanization of Turkish agriculture. So much so that between 1950 and 1960, the number of tractors increased to forty thousand. This resulted in the migration of a large population from the countryside to the cities. As a result, the urban population grew from 5.2 million in 1950 to 8.8 million in 1960.

Turkey had neither actively entered World War II, nor took sides. İnönü, then president, took credit for managing rather well the policy of keeping out of the war and adopting 'active neutrality'. The end of the war, however, signalled a new world order, and Turkey found itself at a crossroads between the Soviet Union and the Western alliance. An important factor that contributed to this development was the Soviet demands from Turkey.<sup>2</sup> According to Shaw and Shaw (1978), after the end of the War, Soviet Union demanded a revision of the Montreux Convention to not only assure them access to the Straits during both war and peace, but also allow them to establish military bases along both the Bosphorus and the Dardanelles. There were also talks of demanding Kars and Ardahan from Turkey (1978: 399-400). These demands were significant in Turkey's decision to join the Western world.

For some historians it was in this juncture, on March 12, 1947, that President Truman proposed the Congress a program to provide both Turkey and Greece with military and economic assistance to help protect them from the Soviets. Beginning in 1948, Turkey began to receive military equipment and aid to build up its transportation system, which soon transformed its army into a major military force. The Marshall Plan, announced on June 5, 1947, and Turkey's

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<sup>2</sup> Turkey's choice between west and east after World War II would better be understood when we consider the Crimean war (1853-1856) that was fought mainly on the Crimean Peninsula against Russia by the British, French and Ottomans. This war was one of the main historical events that brought the Western powers and Ottoman Turks closer. With the Treaty of Paris which was signed after the war on 30 March 1856 the Ottomans joined the Concert of Europe. The latest war between the two powers was the Russo-Turkish War of 1877-1878 in which Russia succeeded in claiming provinces in the Caucasus, namely Kars, Ardahan and Batum. The occupied territories were regained in the early 1920s.

subsequent admission into the Organization for European Economic Cooperation (April 16, 1948) further strengthened its economic ties with the United States, leading to a direct economic agreement between the two nations (July 8, 1948) which became a second pillar of their relations (Shaw and Shaw 1978: 400).

In the meantime, Turkey's military contribution to the U.N. in the Korean War with 4000 men, starting in June 1950, and its subsequent entry into the NATO (February 18, 1952), made it an integral part of the joint effort of the Western nations to defend themselves from Soviet expansion and confirmed Turkey as a full member of the Western alliance. Turkey's participation in the Korean war aimed at, on the one hand, breaking the resistance of the Western powers for its NATO membership (Hale, 1994:96), and on the other hand, responding Soviet Union's demand to change the Montreux Convention and redrawing the border in Kars and Ardahan regions. These developments ended the isolation that had begun during World War II. Economic and military cooperation with the West has remained the basis of Turkey's foreign policy and an essential pillar of Western defences ever since (ibid: 400). Some would argue that Turkey's choice of alliance with the west was certainly about security but also for 'belongingness' and 'the quest for Western identity' (Türkmen, 2002). For more than sixty years Turkish foreign policy remained oriented towards the west. Many commentators have observed the crucial role played by Turkey in the region since the 1950s. Since 1952 Turkey has played a full part in most Western and European international institutions (Barchard 1985: 1). Despite this important role, however, its relations with the west have not been undisputed.

### **The Prospect of the EU Membership, Copenhagen Criteria and Failed Expectations**

Turkey's involvement with the European Union, like its association with many other western institutions, dates back to the early sixties (1963). However, it took forty-two years to be named as a formal candidate and the progress in the last decade has been rather slow, frustrating both the Turkish people and the government.

At the Helsinki summit in 1999, Turkey was recognized as a full candidate for membership, to begin substantive talks once political and economic conditions – known as the Copenhagen criteria– were met. In order to fulfil the Copenhagen criteria, revolutionary reforms were approved on August 3, 2002, that delighted human rights activists, one and a half century after the proclamation of *Tanzimat*. With these legislative reforms the Turkish parliament abolished capital punishment, lifted restrictions on the use of the Kurdish language and swept away constraints on free speech. Jonny Dymond (2002a) describes these reforms as 'landmark victory for the pro-European politicians in Turkey'.

Reflecting on the same issue, one human rights activist described the reforms as 'the most positive changes made during the whole history of the Turkish republic' and Prime Minister Bülent Ecevit told the parliament after the vote: 'A great step has been taken in order to improve our democracy' (BBC News, Friday, 5 October 2001). Constitutional changes were so swift that it created mixed reactions in many European capitals. As Günter Verheugen, Member of the European Commission responsible for Enlargement, noted: 'Turkey has been making major and very welcome progress towards meeting the accession criteria. ...With regard to democracy, the rule of law and human rights, Turkey has changed more in the last eighteen months than in the last few decades' (Verheugen 2002).

While these remarks reflect the mixture of surprise and disbelief in the parliament's bold decisions in politically sensitive areas, the reforms of August 3<sup>rd</sup> 2002 had the potential of completing the ongoing process of the integration of Turkey into the European Union.

When Turkey was officially admitted to start membership negotiations on October 3<sup>rd</sup> 2005, it was regarded as a historical moment and public support for the EU membership rose to 74 per cent. However, this public enjoyment and widespread support to EU membership declined drastically to 38 per cent by 2010. The ambivalent attitudes of European politicians at the time, including those of Angela Merkel and Nicolas Sarkozy, following Turkey's rapid implementation of constitutional and political reforms for EU membership helped the AKP government, which had not yet proven itself, to appear innocent in this process. However, the election of Abdullah Gül in the presidential election in 2007 constitutes one of the historical turning points in Turkey. As political Islamists consolidated their political power in the country from this date on, they began to see the EU membership as a hindrance for their political aspirations. Therefore, European myopic politicians on the one hand, and political Islamists on the other, entered into a tacit agreement to put Turkey's EU membership process in a deep freeze, so to speak.

Soon after the full membership talks which started in 2005, there were extremely intense debates over Turkey's membership. The row over Turkey's application had become such an important issue that it came to dominate the election campaigns in many EU countries, as it did in Germany, and referendums over the EU constitution, as it did in France and the Netherlands (Harding 2005). Furthermore, Turkey's full membership talks led to a deep division within the European ruling elite, slowing down the accession process, and giving the much-sought excuse to the AKP government to blame the EU for this as well.

By 2011, the AKP came to a crossroads with both the influential Gülen movement, led by the Turkish cleric Fethullah Gülen who lives in the USA, and the pro-EU liberals. For more than ten years (between 2002 and 2011) the government had ruled the country hand in hand with the Gülen movement. The final break-up came with the failed military coup attempt on July 15<sup>th</sup> 2016. It is widely believed that the master-mind behind it was Gülen.

The anxieties about Turkey's shifting foreign policies were partly a reflection of the changing attitudes towards the European Union at the societal level. By the 2010s, public perceptions of the EU and expectations from Turkey's long-awaited full membership seemed changing. The research I have conducted in Aydın in 2010 sheds light on this situation.

## **Research Findings: Attitudes, Perceptions and Expectations**

The data show that the high support for the EU membership in 2005 decreased drastically by 2010, while a lot of confusion and distrust grew towards the whole project. I began by asking 'In your opinion should Turkey become a member of the EU?' There was no clear majority: Around half of my informants said yes but an almost equal percentage (45%) said no. Comparing to the 74% support of EU membership in 2004, findings showed a significant drop. There was a degree of confusion and estrangement from the idea of EU membership at the public level since 2004. This attitude was also reflected by the current AKP government who had not been fully engaged with the issue. Those who said 'no' to the membership seemed to



perceive the EU as a colonialist and imperialistic bloc, although most of them seemed to have no idea what that meant.

Comparing our data which indicate 50% support, with the data from the Transatlantic Trends 2010 survey, support for the EU was even lower among the general population in Turkey, at around 38% (Kennedy et al. 2010). The difference between these results could be explained by Aydın's distance to the governing AKP. Since 2002 Aydın has been one of the strongholds of the main opposition Republican People's Party.

Another question was: 'In your opinion, if Turkey has to join an international union, does it have to be the EU?' Less than one third (30%) of our informants viewed the EU as the only choice, while by the two third (67%), turning to the EU was not seen as the only option and there were other alternatives that would be much better for Turkey to join rather than the EU. It was obvious that most Turks had lost their belief in the EU project, and this alienation may have encouraged the pro-Islamic government to turn away from the EU project.

In a following question we asked what these alternatives were and who the 'friend' was. While 41% stated that the EU was the best 'friend' for Turkey, almost equal numbers (38%) stated a Turkish Union with the Central Asian Turkish speaking countries would be a better alternative, 7% said an Islamic Union and Arabic Union would be better, and the remaining 15% stated other choices—a union of the Middle East, the Shanghai Cooperation Organization, Eurasia, or no union at all.

The Transatlantic Trends 2010 Report indicated a further decline of support. It found significant changes in public opinion in relation to the EU, NATO and the Middle East. The report said that Turkey was moving in a direction of less predictability on foreign affairs which American and European leaders need to consider in the future. It observed that 'Turkish public opinion seems to reflect the country's new focus on the Middle East'. Its findings are rather serious in terms of EU-Turkish relations in the near future. It continued: 'Compared with the 2009 results, the percentage of Turks who said Turkey should act in closest cooperation with the countries of the Middle East on international matters doubled from 10% to 20%. This was accompanied by a nine-point decline – from 22% in 2009 to 13% in 2010 – in those who said Turkey should cooperate with EU countries. Most strikingly those who said Turkey should act alone rose from 34% in 2009 to 43% this year on international relations' (Kennedy et al. 2010: 4).

What was common in both our findings and those indicated in the Transatlantic Trends 2010 Report was that Turkey seemed to be increasingly looking eastward to develop better relations in its immediate neighbourhood.

When I asked what interviewees thought about 'the intentions of the EU regarding Turkish membership', one third (33%) said that the EU wants Turks to give up their social and cultural values, while 18% believed that the EU wants to divide up Turkey. On the positive side, 31% said the EU wants to help to develop the Turkish economy to the same standard as the rest of the EU countries, and 6% said the EU wants to increase prosperity.

To establish further how this shift in Turkey's orientation has been viewed by the layman I carried out in-depth interviews with 20 locals on a random basis from various quarters of the city of Aydın. I asked related questions in a semi-structured interview format.

The first interviewee was a 27-year-old biologist. He voted for the ruling AKP in the last elections. Unsurprisingly, he finds the government's foreign policies successful. He said 'Now, with this government, we talk to many countries we didn't before. We receive a lot of visiting prime ministers from these countries.'

For him Turkey should join a union, but this union should consist of economic as well as military components. The active foreign policy of the government seemed to attract public support. When asked whether or not 'Turkey is adjusting its foreign policy equally to every direction', he said: 'The priority should be Turkey's own interests. I mean, it is not in equal distance but whatever Turkey's interest dictates, it behaves in that way'. When asked what he thought about the recent discussion about Turkey's orientation. He replied:

Yes, there is a shift in Turkey's orientation, and it is in line with its own interests. When Turkey looks for a union, first it should consider its interests. When it needed a union, for example when we tried to enter the EU, they set a lot of conditions; do this, do that, do not do it like that, etc. If we are going to be part of a union it should be because of economic and military reasons. For me, we should consider membership in these terms. For a long time, Turkey has been trying to open the EU's door but when it did not open, we are now trying the Middle East door. If not, Turkey will try the Arab Union, if not there will be some other options.

He admits that currently Turkey is approaching the Middle East, but this is due to the EU's double standards and delay policies. He is also aware and admits that there will be some consequences of shifting orientations.

The second interviewee was a 31-year-old high school graduate. He worked as a secretary, earned around 1,500 TL per month and like the previous informant, he voted for the AKP in the last elections. Again unsurprisingly, he finds the government's foreign policies successful as well. When I asked how and why he thinks they are successful, he argued:

Because Turkey is a bridge between the West and the East and follows a balanced policy, it does not turn away either from Europe or from the Middle East. Therefore, it stands in the middle. It has very good relations with Iran, Syria, Russia and Greece. In the last five years because of these good relations, even the visas have been abolished.

Principally, he shares the idea that Turkey should join a union but not necessarily the EU. For him '...the idea of a union is good, it will give you strength. It could consist of countries of the Turkish world, countries in the Middle East, or it could be European countries.' Nevertheless, when I asked if he thought Turkey was adjusting its foreign policy equally in all directions, considering the US, the EU and the Middle East, he replied:

It [Turkey] needs to stand the same distance from all these sides. The reason for this is because Turkey is in a strategically important region and this position dictates this balanced approach. It cannot turn its back to Asia, nor to Europe. Turkey has to be in the middle.

Unlike the previous informant he is for a more balanced approach. For him Turkey cannot choose to go in one direction, chose one alliance but should be distancing itself from all sides equally. He also opposed the idea that Turkey's orientation has changed. Instead, he replied:

... Turkey is turning to itself. In other words, Turkey found its real identity and has become active in this region. Because in such a region it cannot be fully with one side. It [Turkey] gives Europe the message that it cannot turn its back to the Middle East, but equally gives the Middle East the same message regarding the West.

Also, he did not agree that the recent changes would have a big impact on Turkey's foreign policies. In his words:

Previously in our country there was an antipathy against the Arabs, and we used to insult them, call them names etc.; there was prejudice. How can we look at these people in this fashion and expect the Arab people to trust us? At first, we need to trust each other because they are our neighbours. Instead, we were directed to Europe, but they did not accept us, and probably they won't ever.

Considering his age, it is understandable that he didn't oppose the EU or the US in favour of the Middle East. This view was also commonly expressed by the current government as well. People from the high echelons of the governing party oppose the idea of moving to the Middle East and abandoning relations with the West. It is a common argument that Turkey has 'found itself', or 'turned to itself'. They argue that the previous orientation was the product of an alien state of mind.

One important finding from the interviews is that those who are politically closer to the government are usually supportive of the government's foreign relations and its policy is defined as a balancing act rather than moving away from the Western alliance.

The following interviewee was a 57-year-old technician who voted for the main opposition party, the CHP in the last elections. For him: 'When domestic politics changes, foreign policy is also affected. Turkey has no clear foreign policy in a consistent direction. It varies with every new government and results with loss of trust in the international arena'.

As he voted for the opposition party, his views on foreign policy contrasted strongly with those of the previous interviewees. When asked about the current direction he replied: 'Right now, it is like a kingdom. They [AKP] do whatever they want to do. It is like a patriarchal system, and they do whatever they want. The government never listens to the opposition parties. Everything is up to them'.

He does not like the work carried out by the state and openly despises what is being done now. For him, the idea of membership is valid only if it is good for the national income.

When I asked whether he thinks the government pays attention to all sides in its foreign relations he said, 'At this moment they pretend as if they were equally distancing themselves from the US and the EU, but they are much closer to the Middle East'. I asked what he thought about this: 'Turkey is a secular republic, and it should stay as it is, but the government is moving towards the Middle East as it fits to their own [religious] ideology. Beside this they are pretending that they are approaching the EU, but they are, in reality, trying to deceive people'.

I asked: 'Do you think these developments will have a consequence on the relations between Turkey and the West?' He replied: 'With America, we want to be seen on good terms, because currently our relations are depending on our interests. With the EU, fewer mind whether we enter it or not. It does not matter'. He repeated: 'Because of our interests we are currently

trying to be seen on good terms with the US. But obviously, our relationship with the EU is a totally different matter'. He meant that the government does not actually want to join the EU. When I asked whether he would choose the EU, US, or Middle East-based union if he was in a position to choose, he replied: 'There can be some political developments in the Middle East, but we should not go with them in this way. Despite the government's distant stand, we can and should join the EU'.

When I asked, 'How do you find the government's current EU policies?' his reply was: 'I would support the government if they made a good decision or action for the EU membership. But currently they are following pro Middle Eastern politics... The current government gives too much priority to issues like headscarf while it should focus on economic progress'. There were other informants who also expressed concerns about the government's limited focus on 'real' problems such as rising unemployment rates, economic hardship, rising prices, economic inequalities, and political and cultural rights, just like it had not pressed for EU membership.

The next interviewee was a 36-year-old woman. With primary school education, she was a hairdresser. She was married, earned middle level income, and voted for the CHP in the last elections. She did not view Turkish foreign policies successful: 'We are giving too much weight to the Middle East because of the current government's ideological position'. She thought that the changes in our foreign policy were not good for the country. 'It is a negative development', she said.

I asked: 'In your opinion, would these developments have a negative impact on the relations between Turkey and the West? In the future, what sort of implications should we expect?' She replied: 'Instead of moving forward we are going backward. This current shift will affect our relations with the west negatively.' If she was in a position to choose, she said, she would choose the EU because 'Western countries look forward rather than backward and so it would be better to join them'.

When asked how successful she considered the government's current EU policies, she responded: 'They are not positive at all. They look like pretended policies rather than real ones. The government does not put its full strength into developing EU-Turkish relations'. She believed that the government's real aim is to stay away from the EU and get closer to the Middle East. And when I asked; 'Should Turkey be part of the East or the West?' she said, 'We should be a Western country, but when we look around us, we are moving gradually to the East and look like an Eastern country. Considering our children's future, the country should not go in that direction. This is important for our future, a better future, and a good education is very important.'

It is evident from her expression that she is actually very much concerned for the future of her children and for the future of the country. No matter how overstated it may appear, certain sections of the Turkish society find the recent developments worrying. These concerns are not just based on Turkey's eastward orientation. It goes without saying that international politics is never only an international matter. It is mostly an extension of internal politics and many people, like the interviewee in consideration, fear that Turkey's close ties with the Middle East will have serious implications on internal politics and policies.

The following interviewee was a 50-year-old retired man, who earned around 1000 TL per month, and voted for the ultra-right Nationalist Action Party (*Milliyetçi Hareket Partisi*, MHP). He did not find the government's foreign policies successful and was supportive of Turkey's

EU membership although he complained about the EU's double standards and discrimination against Turkey. He said: 'EU is good for Turkey, we could benefit from such a union, but it has been delaying its decision, just keeping us at the door; and this government is much closer to the Arabs'.

Our last interviewee was a 42-year-old IT teacher who voted for the pro-Islamic Happiness Party (*Saadet Partisi*, SP) in the last elections. For him, the current Turkish foreign policies were partly successful. In his words: 'There are parts of Turkey's policies that are adequate while in some areas they are not. Turkey should be more active to get what it wants. It should act not in reaction to other countries' actions but for its own policies. Turkey should identify its own interest areas, like the US and Israel do'.

When asked what he thinks about joining a union, he said, 'there is no such necessity. Because, Turkey shouldn't trade its national values and resources for either the East or the West. It shouldn't be in a position to make a choice between them'. He was for a more independent and assertive foreign policy: 'Turkey should give up the EU, because they are applying double standards and for a long time, keeping Turkey in the waiting room'. In his view, 'The Republic of Turkey is not a country you keep in the waiting room'. He went on to say: 'We should leave the EU aside for now. The remaining options are the US and the Middle East. See how the US sells goods to every country; not only to the West but also to Iran, China and Russia. What we should do is what the Ottoman Empire used to do'.

To be precise, he did not directly say that we should abandon the prospect of the EU, but that we should be on an equal footing with them. Even if it means leaving the union all together, the responsible side is the EU. For him, 'this situation is caused by the EU's double standard approaches and discriminating attitudes towards Turkey'. His views mirrored the fact that it is a common grievance among ordinary people that Turkey has waited so long.

How the public understood the reasons for the delay of Turkish membership to the EU was a question I wanted to explore. Regarding the impasses of the Turkish membership, over half (57%) believed that the EU has been applying double standards, discriminating against Turkey, and deliberately delaying the process. In fact, this public opinion has been shared by many national and international politicians and diplomats.<sup>3</sup> In this context, the issue of trust plays a central role in relation to both the European Union's and the Turkish government's positions about willingness for membership. It seems like we have witnessed a rapid erosion of public trust in the EU from 2005 to 2010.

## Trust, Distrust, and the Changing Turkish Attitudes

Trust is an important aspect of our perceptions and key for our understanding of the social world. It is the source of social networks and social actions. Many influential intellectuals pointed out that there has been a significant erosion of public trust, and this has become one of the most defining features of our age (Fukuyama 1996; Sztompka 2000). Collapse of trust in the political process is the subject of ever-growing intellectual concern and reactions (See Furedi 2014; Burgess 2007; Delibaş 2017). Loss of public trust in Turkey is reflected towards

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<sup>3</sup> There is whole plethora of criticism and discussion about EU's double standards towards Turkey's negotiations. See for example Dymond 2002b.

not only the governmental institutions but also other countries and international institutions including the EU.

In a similar fashion, Coleman observes that trust in government or in international institutions does change in time and if there is a climate of distrust against the local political elite, the government, institutions, or locally produced goods, citizens will look elsewhere and develop strong trust in other countries or international bodies (Coleman 1990: 196 cited in Sztompka, 2000: 118). Sztompka also argued that in Poland before the collapse of Communist regime, there was a low level of trust in government agencies while a high level of trust in international bodies, organizations such as the IMF, membership in NATO or the EU (2000: 118). Back in the early 2000s the situation in Turkey was similar. When the country was hit by one of its worst economic crises in 2001 the levels of trust in the government and governmental agencies plummeted sharply.

This trend was the continuation of the so called ‘legitimacy crisis’ that was discussed by various thinkers from the early 1970s onwards. In his famous work *Legitimation crises* (1988), Habermas identified a series of “crisis tendencies within capitalist societies that make it difficult for them to maintain political stability through consent alone” (See Heywood 1997: 196). For him, due to the low levels of economic growth, states will increasingly fail to fulfil the rising citizen demands. In Turkey too, this situation inevitably caused legitimization crises (Delibaş 2016).<sup>4</sup>

Since realizing the importance of trust in world affairs, the World Economic Forum (WEF) has been monitoring public trust levels. In the early 2000s a world-wide study by WEF found an alarming rate of decline in public trust in various institutions. The WEF’s 2006 report stated that ‘trust in a range of institutions has dropped considerably since January 2004 ... to levels not seen since the 9/11 attacks in 2001’. This report indicated even lower levels of trust in politics and politicians in Turkey.<sup>5</sup> Similarly, we witnessed a rapid loss of prestige and trust in the EU in the eyes of the Turkish public. Findings from one of my nationwide studies also indicate widespread public distrust in many institutions: It shows that less than one in five people (17.4%) trust the EU while the majority (66.2%) do otherwise, while 16.5% neither trust nor distrust (Delibaş 2017: 264-265). The lower level of public trust in the EU seems to reflect the public mood about the EU membership as well. In 2005 people who supported membership were fewer than the ones who did in 2003. This was an important shift. One can speculate that as the public support for the EU dwindled, the government found it easier to shift its foreign policies and rewrite Turkey’s priorities from the West to the Middle East.

It was not only the Turkish public who showed disappointment and discomfort with the EU’s ambiguous policies. As Denis MacShane (2010), a Labour MP, argued: ‘...The truth is, everyone bears responsibility. The U.S. has no effective policy for Turkey ... Europeans, including Germany’s Chancellor Angela Merkel and France’s President Nicolas Sarkozy, have delivered a condescending message that only drives Ankara away from Europe’. British Prime Minister David Cameron, in his visit to Turkey, challenged France and Germany over their

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<sup>4</sup> Other scholars like King (1975) and Crozier, Huntington, and Watanuki (1975) dealt with the issue of legitimacy as well.

<sup>5</sup> Only 5 percent of respondents stated that they trust politicians while 8 percent expressed trust in mass media. In the late 1990s and early 2000s, distrust of mainstream institutions had resulted in political upheaval and the collapse of the party system. This situation not only led to the emergence of a new party system, but also opened the doors for the AKP to come to power in 2002 (Delibaş 2016).

opposition to Turkish membership of the EU when he issued a stark warning to the dangers of shutting Ankara 'out of the club' (Watt and Mulholland 2010).

While the seemingly unfair treatment of the Turkish membership has been undermining Turkish – EU relations, it, more importantly, led to a dramatic decline in the public trust in the EU and the West. In this context, two important and interrelated trends emerged which need to be acknowledged: The rise of Islamic political identity and the shift in Turkey's foreign policy priorities.

### **The Rise of Islamic Identity, Neo-Ottomanism and the Shift in Foreign Policy**

Contemporary politics is driven by identity politics (Bauman 2004; Castells 2006; Habermas and Ben-Habib 1981; Melucci 1995; Scott 1995). Turkey has not been an exception from this trend and there has been an emergence of Islamic, Alevi and Kurdish identities in politics. Since the early 1980s, Islamic political identity has been the fastest growing political movement in Turkey, and it came to power with the AKP in 2002 (See Marty and Appleby 1991; Yavuz 2003; Delibaş 2008 and 2015). During its ten years in office the AKP made considerable impact on Turkish politics and political structure, and transferred the conventional class and ideology-oriented politics into identity, value and culture-oriented politics.

Castells argues that religious fundamentalism has always existed throughout history but has become surprisingly powerful and influential as the source of identity in this new century. According to him, information technologies and global capitalism that started in the 1970s have a different meaning for the Islamic world, and that this period means a period of Islamic revival, purification, and empowerment. Islamic fundamentalism as a reconstructed identity and political project, he argues, is central to the most decisive process in shaping the future of the world (Castells 2006: 13-14). This new politics inevitably requires the 'reframing of shared conceptions of ethics, justice, community, and history to develop new methods for advancing tangible social and economic goals' (Yavuz 2003: 7). The radical transformation in the value system that Yavuz underlined and the socio-economic and political arrangements that would support them were realized in the first ten years of the government. In other words, before the political Islamist government took steps to turn the political balance of power in the country in its favour, it primarily succeeded in weakening the forces of the army, media and other institutional structures, and then began to openly reveal its counter-revolutionary attempts against the main characteristics of the regime.

The axis shift in Turkish foreign policy and the ideology of Neo-Ottomanism can be viewed in this vein as well. In this period the government began its so called 'active foreign policy' by breaking with the classical past and developing relations with Iran, Syria, Russia and Hamas leadership. As expected, this change in foreign policy is perceived to be in line with the Islamic political identity of AKP supporters; i.e., my informants saw it as a reflection of the growing Islamic identity in internal politics. For the government officials, this new direction is a sign of growth in Turkey. What they see is that as the Turkish economy grows, so does the Turkish influence in the region. As our ethnographic data indicate, there was a considerable degree of public support at least amongst its voters and the voters of the ultra-nationalist MHP (that later in 2015 entered a coalition with the ruling party) for this so called 'active foreign policy' or more broadly known as *axis shift*. It must be remembered that the government's relations with

the West and key Western institutions like EU, EC, EP and NATO have suffered serious setbacks since the 2010s. Moreover, if the government perceives this to be beneficial for electoral support, it will likely continue in that direction. In fact, research findings confirm this observation. The majority of AKP voters stated that the EU discriminates against Turkey and applies double standards.

It is not surprising that more than half of my informants had different views about the axis shift in Turkey. For the center left CHP and ultra-right MHP voters the new foreign policies were nothing more than the reflection of the growing political Islamic identity which is perceived as a grave cause of concern for the future of a secular republic. As a result, this new orientation is perceived as the evidence of further Islamization and as a move to old, traditionalist, backward values.

### **War in Iraq and the Arab Spring: Turkey's Concerns and Implications**

The invasion of Iraq was the thorniest issue between the US and Turkey, of the last two decades. As Turkish objections to the invasion, because of the Kurdish problem, fell on deaf ears, it also caused widespread suspicion and alienation at the public level. Following the invasion, the division of Iraq into three regions along ethnic and religious lines significantly increased Turkey's security concerns. Furthermore, the Syrian civil war and the support of the Kurdish organizations by the US despite Turkey's objections have created huge problems in the Turkish-US relations that were difficult to overcome. While this situation created anxiety and insecurity in the wider society, it strengthened the hand of the government in foreign policy and prepared a solid ground for the convergence with the Middle Eastern countries that it perceived culturally and ideologically similar.

Yet one of the most important developments that cannot be neglected in understanding the radical change in the foreign policy of the AKP is the Arab Spring that emerged in 2010.<sup>6</sup> The uprisings known as the Arab Spring, which led to the overthrow of the dictatorships of the last thirty and forty years in the Middle East, had a transformative effect on the undemocratic political order in the region, which had been stagnant for centuries. However, the optimism about wider and lasting democratic transformation has dwindled during the following couple of years and the spring has come to look more like winter. Hence some commentators described this era as the 'Arab Winter' (Byun and Hollander 2015: 26). The Arab Spring not only caused the collapse of undemocratic regimes in the Middle East, but also had devastating effects on Turkey's foreign policies and relations with the region for the last ten years.

It must be remembered that in the beginning of the Arab Spring Turkey appeared as the winner, but the situation changed rapidly when Morsi, the leader of Muslim Brotherhood in Egypt, was overthrown by a military coup.<sup>7</sup> Soon after the collapse of the Mubarak regime, the Muslim Brotherhood emerged as the most powerful group which further motivated the AKP to intensify its efforts in Egypt. Once Morsi won the elections in June 2012, Turkey increased its foreign investment with more business partnerships and bilateral projects, (Ayata 2014). It was clearly

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<sup>6</sup> The 'Arab Spring' began in December 2010, with the death of a Tunisian man protesting his treatment by government authorities. Anti-government unrest spread in many countries including Tunisia, Morocco, Syria, Libya, Egypt and Bahrain and around the Middle East.

<sup>7</sup> In late 2011, in a public opinion poll among five Arab countries, 'Turkey voted as the biggest winner of the Arab Spring' (Ayata 2014).



visible that following the military coup the AKP government became such a passionate defender of Morsi and the Muslim Brotherhood. However, this approach severely damaged its bilateral relations with the new leadership who came to power with the military coup. Furthermore, it caused damage to the relations with other Arab populations that increasingly perceived the AKP as a 'Turkish branch of the Muslim Brotherhood'. With the removal of Morsi, Turkey, clearly, lost an important ally in its effort to establish its vision of a new Middle East (Ayata 2014: 106). For some observers the coup and the removal of Morsi had serious impacts on the self-confidence of the AKP government.

The extremely harsh and aggressive policies pursued by Turkey in defending the organization of the Muslim brothers resulted in the severing of relations with many regional countries such as Saudi Arabia, Bahrain, and the UAE. The breakdown of communication was to the extent that pro-government media outlets labelled them as 'enemies of Turkey', and they were often explicitly blamed for supporting the failed military coupe (15 July 2016) staged by the Gülenist army members. Furthermore, during this period, Turkey's persistent biased and erroneous policies regarding the Syrian civil war increased its isolation in the region and worsened the country's relationship with the West like never before.

From the government's point of view, the Muslim Brothers meant a great hope for Turkey to seize the leadership of the region. However, the removal of Morsi did not only end these high hopes, but the government began to follow a paranoid foreign policy, fearing a similar fate. In addition, the Gezi movement, which started just then, increased its fears and oppressive policies. As a result, authoritarianism increased in domestic politics, while an unprecedented isolation (the so called 'precious loneliness') subjugated the foreign policy.

Consequently, these events played a decisive role in the consolidation and continuation of the ideological identity and value-centered attitude in foreign policy (Ayata 2014: 106). It can be argued that the AKP government and high echelons of the power elite have never recovered from the destabilizing effects of the two significant and intertwined developments following the Arab Spring: On the one hand, the ousting of Morsi and the Muslim Brotherhood from power, and on the other, the Gezi Movement after which the government suffered a significant loss of legitimacy.

## **Conclusion**

The political Islamic movement emerged as the fastest growing political force in Turkey in the mid 1990s. It all began with the local elections of 1994, where pro-Islamist candidates, including the current President Recep Tayyip Erdoğan, attained many of Turkey's major cities, including İstanbul and Ankara. This trend continued the following year. While the shock and great controversy caused by the local elections had not subsided yet, the pro-Islamist Party won the general elections in 1995 and came to power. In those years, while the country was experiencing major economic and political crises, it seemed very important, from a sociological perspective, to examine the reasons for the success of an Islamist party in the seventy-year-old secular Republic. My academic interest on the rise of political Islam in Turkey evolved into long-term academic research against this backdrop. In this process, Paul Stirling was a key figure to encourage me to study this movement that I knew very little about. In the first year of my doctoral study, like many other students, it had taken me too, a considerable amount of time to decide what to study. Political Islamist, Alevi and Kurdish movements were on the rise in

the late 1990s. As a good mentor and adviser, Paul, had convinced me that researching the farthest of these movements—the political Islamist movement—would be the right academic choice. So, in this chapter, as my contribution to Paul's commemoration, I tried to examine the major policies and practices of the political Islamist AKP government in its first decade in power and their socio-economic, political, and cultural effects.

During its first ten years of power (2002-2012) the AKP government made considerable impact on Turkish politics and social structure. It transformed the centre of politics from a conventional class-based one to an identity and cultural oriented one. A second but closely related trend was the shift in orientation of Turkey's foreign policies from the West to the East.

As the findings indicate, public support for the EU membership dropped dramatically between 2004 and 2010. In-depth interviews reveal that Turkey's new orientation is largely supported by AKP voters who value this new foreign policy orientation for various reasons, but chiefly because of their identity and ideological preferences. They view this new chapter in Turkish foreign policy as a great way forward as well as a sign of independence and self-confidence. For them this new orientation was something that all Turks should be proud of, as it was viewed as reconquering the old territories which were once ruled by the Ottoman Empire. For those who did not vote for the AKP, however, this shift was a negative, backward slope. They, by and large, feared that all these changes in foreign policies that were expressed at the international level would threaten their lifestyles at home.

It is obvious that the shift in foreign policy is a dynamic and a dialectic process which was fuelled by several internal and external factors. On the one hand there was the rise of Islamic political identity and its impact on Turkish politics (e.g., ruling the country since 2002). On the other hand, there was the frustration with the EU's slow response and uncertainties surrounding the Turkish membership. In addition, the war in Iraq and Syria, as well as the Arab Spring's potential implications regarding the Kurdish region in Turkey seemed to have significant consequences for Turkey.

In many ways Turkey's past eighty years were shaped by Mustafa Kemal's aspirations to catch up with the contemporary civilization and the ideals of modernity. During the AKP period, however, the relations between Turkey and the western world worsened, the bridge between the East and the West which was built by the early republican elite was weakened, and this made an enormous impact on many fronts in Turkish politics and social life.

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### **References**

Ahmad, Feroz. 1977. *The Turkish Experiment in Democracy, 1950–1975*. Boulder, Co.: Westview Press.

- Ahmad, Feroz. 1993. *The Making of Modern Turkey*. London and New York: Routledge.
- Ayata, Bilgin. 2014. Turkish Foreign Policy in a Changing Arab World: Rise and Fall of a Regional Actor. In: *Journal of European Integration* 37(1): 95-112. <https://doi.org/10.1080/07036337.2014.975991>. Accessed 9.10.2021.
- Baker, Gerard. 2005. We're Going Cold on Turkey. *The Times*, 10 June: 21.
- Barchard, David. 1985. *Turkey and the West*. The Royal Institute of International Affairs. London: Routledge & Kegan Paul.
- Bauman, Zygmunt. 2004. *Identity: Conversations with Benedetto Vecchi*. Cambridge: Polity Press.
- Berkes, Niyazi. 1964. *The Development of Secularism in Turkey*. Montreal: Mc Gill University Press.
- Bottomore, Thomas Burton. 1979. *Sociology: A Guide to Problems and Literature*. London: George Allen & Unwin Publishers.
- Burgess, Adam. 2007. Mobile phones and service stations: Rumour, risk and precaution. In: *Diogenes* 54 (1): 125 – 139.
- Byun, C. Christie and Hollander, J. Ethan. 2015. Explaining the Intensity of the Arab Spring. In: *DOMES-Digest of Middle East Studies* 24: 26-46. <https://doi.org/10.1111/dome.12057>. Accessed 17.03.2023.
- Castells, Manuel. 2006. *The Power of Identity* (2<sup>nd</sup> ed). Oxford: Blackwell Publishing.
- Cohen, Roger. 2010. Turkey Steps Out. *New York Times*, October 25. <http://www.nytimes.com/2010/10/26/opinion/26iht-edcohen.html>. Accessed 26.10.2010.
- Coleman, S. James. 1990. *Foundations of Social Theory*. Cambridge: The Belknap Press of Harvard University Press.
- Crozier, Michel, Samuel P. Huntington and Joji Watanuki. 1975. *The Crisis of Democracy: Report on the Governability of Democracies*. New York: New York University Press.
- Darnton, John. 1995. Turkey Serves as Bulwark Against Dictators, Terrorists, Islamic Radicals. *The New York Times*, 2 March.
- Delibaş, Kayhan. 2008. Religious Identity and Participatory Individual: Identity Politics and the Rise of Political Islam in Turkey. In: *TODAİE'S Review of Public Administration* 2 (2, June): 155-189.
- Delibaş, Kayhan. 2009. Conceptualizing Islamic Movements: The Case of Turkey. In: *International Political Science Review* 3(1): 89-103.
- Delibaş, Kayhan. 2015. *The Rise of Political Islam in Turkey: Urban Poverty, Grassroots Activism and Islamic Fundamentalism*. London: I. B. Tauris.

Delibaş, Kayhan. 2016. The Collapse of the Turkish Party System and its Effects on Citizenship and the Legitimacy of Governance. In: Pardo, Italo and Giuliana B. Prato (ed.). *Citizenship and the Legitimacy of Governance: Anthropology in the Mediterranean Region*. London: Routledge, pp. 103-120.

Delibaş, Kayhan. 2017. *Risk Toplumu: Belirsizlikler ve Söylentiler Sosyolojisi (Risk Society: Uncertainty and Rumours Sociology)*. İstanbul: Ayrıntı Yayınları.

Dymond, Jonny. 2002a. Turkey Turns Liberal with Eye on EU. *The Observer*, August 4.

Dymond, Jonny. 2002b. Turkey attacks EU 'double-standards'. *BBC News, World edition*, December 9. <http://news.bbc.co.uk/2/hi/europe/2558707.stm>. Accessed 20.10.2022.

Esposito, L. John. 1995. *The Islamic Threat: Myth or Reality?* Oxford: Oxford University Press.

Friedman, L. Thomas. 2010. Letter from Istanbul. *New York Times*, June 15. [http://www.nytimes.com/2010/06/16/opinion/16friedman.html?\\_r=2&ref=thomaslfriedman](http://www.nytimes.com/2010/06/16/opinion/16friedman.html?_r=2&ref=thomaslfriedman). Accessed 20.10.2022.

Fuller, Graham E. and Lesser, Ian O. 1993. *Turkey's New Geopolitics: From the Balkans to Western China*. Oxford: Westview Press.

Fukuyama, Francis. 1996 [1995]. *Trust: The Social Virtues and the Creation of Prosperity*. London: Penguin.

Furedi, Frank. 2014 [1998]. *Korku Kültürü. Yıldırım, Barış (trans.)*. İstanbul: Ayrıntı Yayınevi.

Giddens, Anthony. 1995. *Sociology*. Cambridge: Polity Press.

Habermas, Jürgen. 1988 (1976). *Legitimation Crisis*. Cambridge: Polity Press.

Habermas, Jürgen and Seyla Ben-Habib. 1981. Modernity versus Postmodernity. In: *New German Critique* 22 (Winter): 3-14.

Habif, Yola. 2002. *The Future of Iraq*. <http://www.turkishpolicy.com/images/stories/2002-04-securitydimensions/TPQ2002-4-habif.pdf>. Accessed 20.10.2022.

Hale, William. 1981. *The Political and Economic Development of Modern Turkey*. London: Croom Helm.

Hale, William. 1994. *Turkish Politics and the Military*. London and New York: Routledge.

Harding, Luke. 2005. Turkey dominates German election. *The Guardian*, September 16, p. 19.

Harvey, David. 1995. *The Condition of Postmodernity*. Cambridge: Blackwell.

Heywood, Andrew. 1997. *Politics*. London: Macmillan Press.

Kennedy, Craig, et al. 2010. Transatlantic Trends Survey. <https://doi.org/10.3886/ICPSR33021.v1>. Accessed: 18.03.2023.

Keyder, Çağlar. 1987. *State and Class in Turkey: A Study in Capitalist Development*. New York: Verso.

King, Anthony. 1975. Overload: Problems of Governing in the 1970s. In: *Political Studies* XXIII (2 and 3): 284-296. <https://onlinelibrary.wiley.com/doi/abs/10.1111/j.14679248.1975.tb00068.x> Accessed: 12.04.2023.

Kongar, Emre. 2001. 21. Yüzyılda Türkiye. İstanbul: Remzi Kitabevi.

Lewis, Bernard. 1962. *The Emergence of Modern Turkey*. Oxford: Oxford University Press.

Lewis, Bernard. 2002. *What Went Wrong? Western Impact and Middle Eastern Response*. London: Phoenix.

MacShane, Denis. 2010. Stop the Blame Game Europe and Turkey need each other. *Newsweek*, 23 July. <https://www.newsweek.com/denis-macshane-turkey-and-europes-conflicts-74659>. Accessed 05.11.2022.

Marty, E. Martin and R. Scott Appleby. 1991. *Fundamentalism Observed*. Chicago: University of Chicago Press.

Melucci, Alberto. 1995. The New Social Movements Revisited. In: Maheu, Louis (ed.). *Social Movements and Social Classes: The Future of Collective Action*. London: Sage, pp.107 – 119.

Ortaylı, İlber. 2007. *Batılılaşma Yolunda*. İstanbul: İnkılap Kitabevi.

Scott, Joan W. 1995. Multiculturalism and the Politics of Identity. In: Rajchman, John (ed.). *The Identity in Question*. New York: Routledge, pp.3-12.

Shaw, Stanford J. and Ezel Kuran Shaw. 1978. *History of the Ottoman Empire and Modern Turkey*, vol. II: 1808 – 1975. Cambridge: Cambridge University Press.

Stirling, Paul. 1993. Introduction: Growth and Changes. In: Stirling, Paul. (ed.). *Culture and Economy: Changes in Turkish Villages*. Huntingdon: The Eothen Press, pp. 1-16.

Stirling, Paul. 1994. Labour Migration in Turkey: Thirty-Five Years of Change. In: Altuntek, Serpil, Suavi Aydın and İsmail H. Demirdöven (eds.). *HUMANA: Bozkurt Güvenç'e Armağan*. Ankara: T.C. Kültür Bakanlığı Yayınları, pp. 413-441.

Sztompka, Piotr. 2000. *Trust: A Sociological Theory*. Cambridge: Cambridge University Press.

Şenyapılı, Tansı. 1982. Economic Change and the Gecekondu Family. In: Kâğıtçıbaşı, Çiğdem (ed.). *Sex Roles, Family and Community in Turkey*. Bloomington: Indiana University Turkish Studies, pp.237-248.

Tavernise, Sabrina and Michael Slackman. 2010. Turkey Goes from Pliable Ally to Thorn for U.S. New York Times, June 8. <https://www.nytimes.com/2010/06/09/world/middleeast/09turkey.html>. Accessed 13.07. 2020.

The Economist. 2001. Turkey's real crisis. 16 May.

Tisdal, Simon. 2010. Turkey's 'zero problems' policy is a flop. The Guardian, 21 June. <https://www.theguardian.com/commentisfree/2010/jun/21/turkey-zero-problems-policy> Accessed 13.04. 2023.

Türkmen, Füsün. 2002. Turkey and the Korean War. In: Turkish Studies 3 (2): 161-180.

Verheugen, Günter. 2002. Towards the Enlarged Union Strategy Paper and Report of the European Commission on the progress towards accession by each of the candidate countries, European Parliament Brussels, 9 October. [https://www.ab.gov.tr/siteimages/birimler/kpb/communication-on-eu-enlargement-policy\\_en\\_2002.pdf](https://www.ab.gov.tr/siteimages/birimler/kpb/communication-on-eu-enlargement-policy_en_2002.pdf). Accessed: 18.03.2023.

Watt, Nicholas and Helene Mulholland. 2010. David Cameron accuses France and Germany of double standards over Turkey. The Guardian, 27 July. [https://www.theguardian.com/politics/2010/jul/27/david-cameron-turkey-double-standards\\_](https://www.theguardian.com/politics/2010/jul/27/david-cameron-turkey-double-standards_) Accessed: 06.22.2022.

World Economic Forum. 2005. Trust in Governments, Corporations and Global Institutions Continues to Decline, (15 December 2005) [http://web.worldbank.org/archive/website00818/WEB/OTHER/TRUST\\_IN.HTM](http://web.worldbank.org/archive/website00818/WEB/OTHER/TRUST_IN.HTM) Accessed 13.04.2023.

Worsley, Peter. 1984. The Three Worlds: Culture and World Development. Chicago: University of Chicago Press.

Yavuz, M. Hakan. 2003. Islamic Political Identity in Turkey. London: Oxford University Press.

## **Part Four**

### **Paul Stirling as Supervisor**

**John Corbin**

## **Ethnography and the History of Violence in the Spanish Civil War**

‘A good ethnography is a joy forever.’ Paul Stirling

Paul Stirling was one of a cohort of social anthropologists—among them Julian Pitt-Rivers, Michael Kenny, John Campbell and Emrys Peters—who after the Second World War as research students at the Institute of Social and Cultural Anthropology at Oxford did field work in countries of the Mediterranean littoral. In doing so, they brought to the study of literate societies with millennia of documented history the research methods and perspectives of a discipline designed by A.R. Radcliffe-Brown and Bronislaw Malinowski to study exclusively oral societies with undocumented pasts. That move, encouraged by E. E. Evans-Pritchard, then head of the Institute, raised a series of methodological issues. Of what relevance was knowledge of Andaman Islanders, Australian aborigines, Trobriand Islanders, and the Nuer to the study of Turks, Spaniards, Greeks and Arabs? What was the relation of the ethnographic present of the social anthropologist to the documented past of the historian? How could information from participant observation be integrated with documented data? To what extent should social anthropologists do historical research? Paul and his cohort were soon embroiled in these issues, and so were their research students. I was one of them.

I first met Paul in 1962 when I enrolled for the Diploma in Social Anthropology at the London School of Economics. The diploma was a graduate conversion course for students who did not have an undergraduate degree in social anthropology. Although I had studied anthropology as an undergraduate at the University of Chicago, only a fourth of my programme had been in anthropology and only a fourth of that had been in social anthropology. Paul was my supervisor for the Diploma, taking me through the classic ethnographies of British social anthropology. By the end of the year, I decided I wanted to continue in social anthropology. As I spoke Spanish and Spain was nearby, I decided to do fieldwork in Spain. Paul, who himself had a project going in Italy, welcomed the decision. As to topic, growing up in Cuba had given me some experience of revolution, so I decided to focus on political conflict and violence.

Paul’s appointment in 1965 to the chair in sociology at the newly established University of Kent at Canterbury meant that if I wanted him to supervise my research I would have to transfer to Kent. Paul then persuaded Marie Corbin, my wife, whom he had taught on her undergraduate programme, that she, too, should register as a research student.

### **Ronda: Fieldwork and Ethnography**

Paul had no special interest in conflict and violence in Spain, but characteristically did not direct his students’ research. He did, however, care strongly about language and would promptly return drafts with comments and suggested rephrasing. He much favoured one



syllable words of Anglo-Saxon origin over learned poly-syllabic Latinisms and Graecisms. As Spanish was my native language, Latinisms to me seemed ordinary and every-day, not learned. We eventually compromised: I tried to keep the syllable count down in the main text; he tolerated Latinisms in my translations of Spanish.

Above all, Paul valued good ethnography grounded in accounts of what people did and said. He suggested I write to the two of his cohort who had worked in Spain—Julian Pitt-Rivers and Michael Kenny—to ask for advice. From both we got encouraging replies, and from Michael some very practical advice on the need to have formal letters of introduction in Spanish with as many stamps and ribbons as possible to ease dealings with local officials. Paul also arranged for us to meet John Campbell at Oxford, who in turn arranged for us to discuss our research plans with three Spanish postgraduate students at the university: the social anthropologist Carmelo Lisón Tolosana, who kindly let us read the galley proofs for his forthcoming book (1966) based on fieldwork in his natal village in Aragon; the agricultural economist Juan Martínez-Alier, who was studying labourer-landowner relations in Andalusia; and the historian Joaquín Romero Maura, whom Paul eventually would appoint as the external examiner for my PhD thesis.

In Andalusia we based ourselves in Ronda, the small city that Pitt-Rivers (1971, first published in 1954) had described as the commercial centre of the mountainous region in which Grazalema, the pueblo he had studied, was located. He also called it an aristocratic city, home to the owners of the biggest estates and best farmland of the area, and in effect the capital of that mountain region. If Grazalema was fairly typical of the places anthropologists choose to study in the Mediterranean area, Ronda was not. With an area 4 times the size of Grazalema, a population 15 times greater, and a more complex social makeup, we thought a study of Ronda would complement Pitt-Rivers's study of Grazalema.

We knew that our own observations would not be able to deal with this size and complexity, so we soon looked for relevant data in official publications and the records of local authorities. Publications we could buy, but access to official records required permission. The letter of introduction that Michael Kenny had recommended was invaluable, accepted as '*documentación de la personalidad*'. The *padrón*, a register of official residents at each address in the municipality, was of particular interest as it could be used for working out household composition. Marriage records in the municipal civil registry might provide details not only on such things as age of marriage but, together with data from the *padrón*, might indicate the extent to which places were linked by marriage and thus kinship. Copying information on over 30,000 Ronda residents and some 10,000 marriages in Ronda and five nearby pueblos was a formidable task but locals helped willingly and provided some interesting commentaries in so doing. The Ronda historical archive, which contained material up to about 1930, was also of interest. We combed it for evidence of social unrest and the official response to unrest back to the beginning of the nineteenth century. Paul was encouraging about this aspect of the research and happily supervised and commented on what we were doing. Indeed, he joined Marie in an SSRC project on using computers to analyse local census data (Stirling and Corbin 1973), one of the earliest attempts by British social anthropologists to process data of this kind by computer.

At the same time, we continued with ordinary fieldwork. We made no secret of the fact that we were social anthropologists living in Ronda to study its society and culture, but few really understood what that entailed. Social anthropology was not at the time a subject in Spanish universities; 'social and cultural researcher' was not a recognized status. We seemed like the

many foreigners—writers, artists, retirees—who over the years had come to live in Ronda without doing any paid work locally. However, we were younger, more fluent in Spanish, more willing to interact with them and more able to do so on and in their own terms as time went on. Casual acquaintances found it easy to deal with us as such foreigners; to people whom we came to know well we were not ‘the anthropologists’ but ‘Chon’ y ‘Mari’, the local version of our first names.

Mostly we opted to let people tell us about themselves, assuming that information volunteered would be more reliable. People disliked being asked direct questions about themselves, often responding with refusals, evasions and, we feared, deceptions, so we avoided asking them. Even so, we were well aware that anything people said to us would be conditioned by who we were: foreigners with no roots in their society. The information we thought most reliable was listening to, and watching, them deal with one another with minimal reference to us.

The diverse network of personal relations we eventually built up was, obviously, only a small portion of the local society, but this limitation was modestly ameliorated because there were two of us, for we could interact with people as a married couple or as individuals, including gendered activities. As our network included aristocrats, doctors, lawyers, architects, civil servants, shopkeepers, butchers, carnies, wood carvers, masons, plumbers, electricians, hairdressers, domestic servants, bailiffs, shepherds, and agricultural day labourers, it was a diverse, if ad hoc, sample of local society.

This research provided material for both Marie’s thesis on family and kinship and mine on conflict present and past. Paul was an excellent editor for both and encouraged us to publish. We decided in the end that our material was best served by three books rather than two, each providing a distinct perspective on the same reality: social inequality. The perspective of the first book (1984) was social structural, examining the interplay of personal and impersonal relations in relation to class and locality; the perspective of the second (1987) was cultural, analysing the system of thought in terms of which people understood their world and their place in it. Both were written in the ethnographic present and jointly authored. The perspective of the third (1993) was historical, deliberately ending the year before fieldwork began, looking at the development of social unrest and political conflict and violence in general and of the anarchist movement in particular. This was my special interest and solely authored by me.

Further material for these books came from numerous returns to Ronda using summer breaks, sabbatical leave, and leaves of absence, so all three books were written in Ronda. In consequence, the ethnographic present of our first two books was exceptionally long: 18 years for *Compromising Relations* (1984) and 21 for *Urbane Thought* (1987).

These were times of major political change. During our first period of fieldwork in 1966-1968, the dictatorship of General Francisco Franco, leader of the Nationalist forces that had won the Civil War of 1936-1939, reached a turning point. The brutal suppression of remnants of the defeated Popular Front forces was over; the ‘hunger years’ of the 1940’s had been weathered, independent labour unions and political parties banned, and democracy severely curtailed. The regime remained absolutist but had moved away from the totalitarianism of its early years. The aging and ailing Franco was still in charge but beginning to think of relinquishing power. He took his first step in December 1966 by holding a referendum on the Organic Law of State, the main proposal of which was the separation of the office of head of government from the office of head of state. It was widely assumed that Franco would remain head of state but give up being head of government, so a vote in favour was understood to be a vote to reduce Franco’s

power. The proposal was approved by 95% of those voting, and this was hailed by the regime as a resounding victory for Franco. In fact, he did not relinquish the office of head of government until 1973. His second step was taken in 1969, the year after we finished our first spell of fieldwork, by designating Juan Carlos, grandson of the last reigning king of Spain, his successor as head of state. Franco died in 1975; Juan Carlos became king and then began a transition to democracy by appointing progressive conservatives to run the government. When the transition was completed, Spain normalized its relation to the established western democracies, being accepted by NATO in 1982 and the European Union in 1986. These changes were in full swing and much discussed by our informants when we returned to Ronda in 1976 and every year following for visits of 2-6 months. What surprised us was that for all the scale and importance of these changes, and all the local interest and excitement that they generated, they seemed to have little impact on either the interplay of personal and impersonal relations in social networks or the terms in which people understood their world and their place in it.

Paul read drafts of all three books. We also benefited from comments by Julian Pitt-Rivers, whom Paul appointed as external examiner for Marie's thesis and I asked to read a draft of *The Anarchist Passion* (Corbin 1993). Both generally approved our project of writing three books with different perspectives on the same reality. David Gilmore, in his review of *Compromising Relations* for *Man* (1985), did not. He thought we were too close to Pitt-Rivers and that our analysis of the effect of personal relations on social class understated class antagonism, and in particular the workers' hatred of the wealthy<sup>1</sup>. He then faulted our analysis for ignoring history, particularly the violence in Ronda in the first months of the Civil War. When we replied that we had reserved history for a separate book, he insisted that history could not be excluded from ethnography (Corbin, Corbin and Gilmore 1986). As it happens, a concern for history was built into my research from the beginning, because one of its starting points was Eric Hobsbawm's (1959) criticism of Pitt-Rivers's (1971, first published in 1954) study of Grazalema for not having explained why that pueblo had been a major centre of pueblo anarchism.

## Grazalema: History of Anarchism

Pitt-Rivers did not ignore history. He had read and cited both of Hobsbawm's major sources on Andalusian anarchism: Gerald Brennan (1962, first published in 1943) and Juan Díaz del Moral (1969, first published in 1929). He consulted the historian of modern Spain Raymond Carr and the Spanish ethnologist and historian Julio Caro Baroja, visiting the latter before beginning fieldwork. That encounter provoked a reaction in Caro Baroja that foreshadowed the problem of social anthropology and history in Spain.

On another occasion, talking with some youth who arrived full of knowledge of the Nuer or the Azande (tribes of the Sudan) who was going to install himself in the mountains of Andalusia, I suggested that he might find it useful to look at the book of

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<sup>1</sup> In *Compromising Relations*, we acknowledged our regard for the work of Pitt-Rivers; whereas Gilmore titled his ethnography *The People of the Plain* (1980) in counterpoint to Pitt-Rivers's *The People of the Sierra* (1971, first published in 1954). Gilmore's thesis was that anthropologists in Spain had worked in small villages like Grazalema and had emphasized community over class, that in larger towns and cities class would predominate and that his own findings in his study of 'Fuenmayor', a pueblo four times the size of Grazalema, demonstrated the point. To be faced with our account of personal relations muting class antagonisms in a city four times the size of Fuenmayor must have been challenging.

Mármol Carvajal on the rebellion of the Moriscos and he looked at me as if I were crazy. I confess that at some point I began to wonder whether I was crazy and asked myself: could the Nuer have more relevance to today's Andalusians than the Moriscos? (Forward to Luque Baena 1974: 12-13)<sup>2</sup>

In the end Pitt-Rivers explored the relevance of neither the Moriscos—perhaps because he thought the 16<sup>th</sup> century too remote—nor the Nuer. He admits that his training in social anthropology was mainly concerned with Africa, especially East Africa, that he started fieldwork with models of lineage systems and age groups, and that he abandoned these when he found no lineal principle or grouping by age in Grazalema.<sup>3</sup> Instead, he opted to study what he found in Grazalema using the standard fieldwork technique of social anthropology.

In principal that technique should be well suited to the study of the kind of people Hobsbawm calls primitive rebels, 'people who neither write nor read books—often because they are illiterate—who are rarely known by name to anybody except their friends and then often only by nickname', which is quite likely true, but he then goes on to say that they are 'normally inarticulate, and rarely understood even when they express themselves', which is not true (Hobsbawm 1959: 2).<sup>4</sup>

Hobsbawm finds Pitt-Rivers's discussion of anarchism wanting.

Its observations on local anarchism are useful, but show too little appreciation of the fact that this little town was not just anarchist but one of the classical centres of anarchism. ... No attempt is made to explain why Grazalema should have been so much more powerful a centre of the movement than other pueblos... (1959: 74)

Hobsbawm is right that Pitt-Rivers makes no attempt to explain Grazalema's pre-eminence as an anarchist centre, but then his field work in the pueblo would not have enabled him to do that. He did not work in any other pueblo and at the time neither had any other social anthropologist, so there were no comparable research results available. In any case, by the time of his fieldwork in the early 1950's, all the anarchists of Grazalema had died, fled, been jailed or were in hiding. He did not study what anarchists had said and done, he studied what people said about anarchists. Had his Grazalema informants told him that they thought Grazalema had been a powerful centre, he would have been able to enquire further, but it seems they did not. Probably with good reason, for they would have known that the local stronghold of militant

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<sup>2</sup> All translations from Spanish are my own.

<sup>3</sup> I, too, began fieldwork with knowledge of the Nuer, but what I had learned from Paul's tutorials was that the Nuer had a segmentary system of alliance and opposition based on proximity of place but thought of in terms of proximity of patrilineal descent. It seemed to me that there was something segmentary in the shifting pattern of alliance and opposition of the towns governed by the anti-French juntas after the Napoleonic invasion of 1808 and also in the pattern of spreading support or opposition to revolutionary *pronunciamientos* in the constitutional monarchies installed after the defeat of the French, though of course without the patrilineal overlay. I'm sure that there are also parallels between the rebellion of the Moriscos and the social revolution of 1936. Despite the huge differences in military ordinance and organization, the tactics used by Catholic King Fernando II to defeat the rebel Moriscos in the Alpujarras in 1500 seem very much like those used by the Nationalist General José Varela to defeat the Popular Front revolutionaries in the Serranía de Ronda in 1936, suggesting some similarity in the structure. Both parallels might be worth exploring.

<sup>4</sup> I am sure that people everywhere are always normally articulate with their fellows and that illiteracy does not limit verbal fluency. As Pitt-Rivers notes about the people he knew in Grazalema 'the benefits of learning have not been showered upon them yet they are often masters of the spoken word' (1971: xxvii). If people who read and write books don't understand what people who don't read and write books say, it is because the former have not spent much time conversing with the latter.

anarchism in their lifetime had been not Grazalema but the neighbouring pueblo of Montejaque. In 1936 the mayor of Montejaque was Pedro López Calle, an anarchist, whereas the mayor of Grazalema belonged to the Republican Left party. In April 1936 Montejaque anarchists arrived in Grazalema to help local radicals burn churches. On 22<sup>nd</sup> July, four days into the Civil War, Pedro López Calle arrived in Grazalema to secure it for the revolution and to execute supporters of the rebels. After the war ended, anarchists from Montejaque continued to operate in the mountains around Grazalema. Most of this would have been known by most of Pitt-Rivers's informants, but the closest he comes to mentioning it is to say that the people of Grazalema regard the people of Montejaque as particularly violent.

Hobsbawm is thus a historian who in 1959 is a bit uncertain about what Pitt-Rivers's anthropological research was and is disappointed that the ethnographic present of Grazalema in the early 1950's does not explain why it had been an important anarchist centre decades earlier. Twenty-one years later, Ginés Serrán Pagán (1980) is an anthropologist who asserts that Pitt-Rivers exaggerates the importance of anarchism in Grazalema because he had not studied its history and because he thought the community to be more egalitarian and solidary than it actually was. Serrán Pagán points out that not all the people of Grazalema were working class, that not all of the working class were anarchists, that many who joined the anarchist CNT (Confederación Nacional de Trabajadores) were not anarchists, and that there were serious divisions among those who were anarchists, particularly about 'direct action' and violence. In support he presents a historical narrative that is especially rich on the violence and conflict of 1936, about which Pitt-Rivers says very little.

Serrán Pagán insists that history is indispensable.

In studying a pueblo of old Andalusia history cannot be ignored. Without it we can not know how the ideological world of the pueblo is produced, nor how the present social structure was formed, nor can we study 'from the present' the changes, the transformations, and the social processes that take place over time. This is, without doubt, the problem with the book of Pitt-Rivers: an attempt to interpret the social life of a pueblo without knowing its history. And this has been the problem for many anthropologists who, influenced by an antihistory current, have founded their interpretations on the mere observation of the present. (1980: 83)

Remarkably, Serrán Pagán's criticism of Pitt-Rivers for ignoring history is itself ahistorical. Conditions for research in both social anthropology and history in the Spain of the early 1950's were very different from those of the late 1970's. The 'Civil War' that ended in 1936 was only the war between armies and governments, it continued in the form of civil repression in the towns and guerrilla fighting in the mountains. The historian Paul Preston (1984) argues that the Franco regime continued the war by obstructing scholarship, closing archives, turning history into propaganda—the 'crusade historiography' written by Spanish policemen, soldiers, priests and government propagandists—and seeking to annihilate history as an academic discipline. So, at first, the academic history of the Civil War and its antecedents was written by foreigners like Brennan and Hobsbawm, who Preston suggests were prone to generalize. Some Spanish historians managed to survive, others received their training abroad, so that in the twilight of the Franco regime a resurgent Spanish scholarship produced detailed local research that challenged not only the conservative historiography of the regime but also the general works of foreign historians.

The trajectory for social anthropology was similar. The early work was done in the 1950's by Julian Pitt-Rivers and Michael Kenny, followed in the 1960's by other foreigners like ourselves and David Gilmore joined by Spanish scholars who had trained abroad (Carmelo Lisón Tolosana in Oxford, Ricardo Sanmartín in Cambridge, and Enrique Luque Baena in Manchester) but were doing their field work in Spain. Serrán Pagán, trained in New York, was one of these Spanish scholars.

The effect of repression by the Franco regime on social anthropologists in the field was indirect: it made informants wary of talking about the conflict and violence of the recent past. We found that to be true of Ronda in the mid-1960's, when the countryside had been pacified and the only visible challenge to the regime was Basque separatism far away to the north; so, it must have been even more restraining for Pitt-Rivers in the early 1950's, when until recently guerrillas were still operating in the countryside and the authorities had prohibited landowners living in town to visit their farms unless accompanied by armed Civil Guards. By 1974, when Serrán Pagán began his work in Grazalema, Francoism was waning, and by 1979, when that work ended, democracy had been established and the restraint on academic research eased. In short, Pitt-Rivers in the early 1950's could not have done the kind of research Serrán Pagán was able to do 20 years later.

It is true that anarchism is the only political ideology Pitt-Rivers discusses, but Serrán Pagán overstates the importance Pitt-Rivers gives to anarchism in the pueblo. Pitt-Rivers does warn against 'overlooking the differences which can be seen to have existed in modern times between the values of the pueblo and those of the anarchists' and insists that 'it would be quite wrong to conclude therefore that the anarchists represented the values of the pueblo as a whole'. He then summarizes the relation between anarchism and the pueblo.

The Anarchist Movement appears ... as a certain number of convinced anarchists, a small percentage of the pueblo, who enjoy the support of the great majority of the pueblo upon certain occasions, but who are simply members of the pueblo for the remainder of the time ... who have no great influence on events.' (1971: 222)

Even so, there is something odd about a study of the ethnographic present paying considerable attention to a phenomenon that no longer exists and was only one of several such past phenomena that could have been considered. Pitt-Rivers says that it was Grazalema's anarchist history that attracted him to it in the first place because he had a theoretical interest in the relation between community and nation, and anarchism represented an extreme position in that relation (1971: xv). So, he asked his informants about Grazalema anarchists, though not without encountering some resistance, for he comments 'the attitude towards them appears to have been ambivalent from the accounts of those who were prepared to discuss them' (1971: 222).

Where Pitt-Rivers conflates pueblo and anarchism, Serrán Pagán sees only inconsistency and contradiction between pueblo and anarchism. The crux of the difference between the two is that for Pitt-Rivers, anarchism was somehow of the pueblo, for Serrán Pagán anarchism (like socialism and fascism) was imposed from outside.

It is difficult to grasp that in these pueblos over and above any social and political doctrine there is a social and cultural familiarity, there are personal relations that on many occasions were incompatible with the principles of an ideological movement. (Serrán Pagán 1980: 109)

This assessment is correct, but it can be arrived at without reference to history. In fact, it is very much what we argued in *Compromising Relations*, written in the ethnographic present, an argument Gilmore thinks we would not have been able to make had we considered history.

The unprecedented conflict of 1936 between on the one hand conservative military rebels backed by national socialists (the fascist Falange) and Catholic traditionalists (Carlists) and on the other hand progressive social revolutionaries backed by internationalist proletarians (anarchists, socialists and communists) who were secularist if they were not anticlerical led to an exceptional epidemic of violence. So exceptional was the violence of that epidemic that even those who lived through it could not understand it, though I am sure they could perfectly understand the endemic conflicts, including those of class, in their society.

The year 1936 represents the most painful page of the history of Grazalema. Both the firing squad executions and the abuses by either side left a footprint that even today has not been erased. The persons who lived it have yet to understand how such a situation could have been produced. And it's that human societies can be moved by politics and economics to situations alien to their own feelings. (Serrán Pagán 1980: 106-7)

Serrán Pagán seems to be saying that politics and economics make people engage in violence so extreme that even those who live it cannot understand it. It is more accurate to say that such extreme violence cannot be explained by economics and politics, which raises the question of what can. Generally speaking, the destructive and disordering physical force that is intrinsic to violence is easily understood when it serves a tactical purpose and the force exerted is proportional to that purpose. When it is not, the violence seems irrational, senseless, or meaningless. The tendency then is to think that the violators are psychopaths or sociopaths. This may be true of some individuals, but it is unlikely that most participants in the repressions of 1936 had temporarily become psychopaths or sociopaths. A more productive approach starts by noting that 'tactical physical force' pertains to a world understood in terms *about which* people think and talk, a world of ideologies, armies and police, government and bureaucracy, lawyers and judicial process, political parties and labour unions. That world produces most of the documents that fill archives, so it is likely to be well represented in the narratives of historians. These narratives focus on physical force as tactic in the conflict between organizations or between the state and its subjects. However, there is another world defined by the terms *in which* people think and talk, a world mostly taken for granted. The civil life of that world is that of the people of the place, the *ciudadanos*, citizens, of the *ciudad*, city. It is a civil life of public standing, of honour and shame, of personal rivalry and obligation. Physical force that threatens, disorders or destroys that world is therefore violence that is harder to think about, harder to explain. However, it is a world that the methods of social anthropology can study, and it is part of the ethnographer's job to talk about it.

The terms in which people think and act, terms which are specific, but not peculiar, to Andalusians, will be used to analyse and compare three insurrections that chart the progress of violence of the Spanish Civil War—the anarchist rising in Casas Viejas in 1933 as its prelude, the proletarian risings and nationalist retaliation in Ronda and Grazalema in 1936 as its climax, and the reactionary attempted coup in Madrid in 1981 as its aftermath. They will then be used to interpret a famous painting from 1937 that both depicts violence and is part of the history of the Civil War and its aftermath, Picasso's *Guernica*,

## The Terms in Which Andalusians Think

The conceptual world defined by the terms in which Andalusians think (Corbin and Corbin 1987) categorizes entities in terms of degree of *vida*, life, on a continuum with lifeless entities, i.e., minerals, at one extreme and body-less eternal living entities, i.e., divinity, at the other. Humans are in the middle of this continuum, distinguished from natural plants and animals by having eternal life, i.e., souls, and from divinity by having finite natural bodies. These entities have corresponding domains. The human domain is *ciudad*, urban space occupied by women and men in *casas*, houses/homes, and *calles*, streets. The relation of the *casa* to the *calle* as institutions is almost classic: the one domestic/economic, the other civic/political; the one the domain of women and children, the other the domain of men. Status in the *calle* depends on the proper domestic and economic functioning of the *casa*, but the *casa* cannot do its work if it is not protected from the civics/politics of the *calle*. So, women and children are not subject to the conflicts of the street, and men in their *casas* are similarly disengaged from *calle* conflict. The domain of plants and animals is *campo*, rural space; the domain of divinity is *cielo*, celestial space occupied by the souls of the ordinary dead, saints and a triple deity. On the border between the urban and the rural are cemeteries with crypts occupied by corpses and on the border between the urban and the celestial are churches occupied by priests, images of saints and images of the once corporeal version of divinity. These distinctions define three orders that are ranked—subhuman, human and superhuman—and analogous, as the human is to the subhuman, so the superhuman is to the human. People living in this conceptual world are engaged in three struggles: strengthening the boundary separating them from the subhuman, weakening the boundary separating them from the superhuman, and remaining equal in the human, because only by remaining equal can they remain truly human.

The dialectic of individual lives in this system balances processes that generate against processes that degenerate an identity of both natural body and supernatural soul. The generative processes are largely the work of the *casa*, including sexual reproduction, body sustenance, and naming and socializing children, all aided by supernatural power wielded by priests in singular weddings, baptisms and first communions, and ongoing soul-maintaining masses and confessions. The identities so constructed become the basis of status in the *calle*, concern for which can make people assert their own integrity, honour and shame, question that of others or even actively try to demean, dishonour, shame others. To such wilful attacks on identity are added those of misfortune, illness, and aging. All can make the person attacked retreat from *calle* to *casa*, where degeneration is resisted by care and nursing which can be reinforced by reproductions of religious images, prayer and promises to saints, and visits from the priest. In the end degeneration causes death, and identity is split into subhuman corpse and superhuman soul. If possible, a priest is called to oversee the split by administering the last rites. Body and soul then receive separate cultural processing, the body being sent from the *casa* through the *calle* to the cemetery, a highly anomalous urban extension into the countryside where no one lives, the soul to heaven assisted by action of a priest, a highly anomalous non-sexual, skirt-wearing ‘father’, in a church, a house of God and thus a highly anomalous extension of the urban into heaven.

The two processes combine in a dialectic of fading identity. Processing the body starts with the women of the *casa* preparing the corpse for burial. After a wake that night, men shoulder the coffin and take it to the entrance of the *casa*. When the coffin is half in the *casa* and half in the *calle*, it is stopped and the priest performs a ritual that separates the body from its *casa*. Women stay behind in the *casa*, men and the priest follow the coffin in a funerary procession through the *calle*. After a short distance the priest leaves; the men continue. Only close relatives enter



the cemetery, where the coffin is placed in a crypt or in a niche owned by the council and rented to the family by the year. These resting places will be marked with the name of the dead person, but they are only temporary. Eventually the bodies will decay, memories will fade, space is needed for the newly dead, and so the bones are moved to an ossuary. The soul is processed by an extended series of church services designed to move it through purgatory, where the soul retains the quality of its individual life history, to an undifferentiated timeless existence in heaven where little attention has to be paid to it as an individual.

The exercise of will in creating and ending life is appropriate when superhuman will is applied to human lives or human will is applied to subhuman lives. Thus, humans breed and kill plants and animals; divinity wills human birth and death. When humans murder or execute other humans, the subordination of killed to killer is too great to be accommodated in the human order, so the killed are in effect relegated to the subhuman. This may be evident in the how and where of the killing and in any deviation from normal funerary practice.

The key concepts in this system of urbane thought (akin to, but distinct from, the *pensée sauvage* of Levi-Strauss 1966: i.e., thought distinctive of city dwellers rather than forest dwellers) have sharp 'either/or' boundaries: *campo* is everything that is not *ciudad*, within the *ciudad*, the *calle* is everything outside the *casa*. Transgressions of incivility are more like violations of taboos than breaking of rules. Thus, the extreme incivility of fighting is least violating when it takes place on the *campo de batalla*, battlefield, more violating when it moves from the *campo* to the *calle*, and most violating when it moves from *calle* to *casa*.

### **Insurrection: Prelude, Casas Viejas 1933**

The anarchist rising in the small Andalusian village of Casas Viejas in 1933 saw a change in tactics. In previous decades the anarchists had engaged in a series of revolutionary experiments, each of which was abandoned when it failed, none of them particularly violent (Corbin 1993: 91-124). A major cause of the earlier failures was anarchism's ingrained localism, which was both its greatest strength in mounting local action and its greatest weakness in coordinating with allies elsewhere and defending against invasion by external enemies. In 1932 anarchists attempted to overcome this weakness by planning a national revolution in January 1933 to coincide with a major strike in Barcelona backed by coordinated risings in major cities and towns spreading to smaller settlements around them (Corbin 1986; see also Mintz 1982). Casas Viejas was at the end of one such chain, starting in the city of Jerez, continuing through the town of Medina Sidonia and ending in the village. In the event, the national revolution failed to take place, and only tiny Casas Viejas in all of Spain rose.

It almost didn't. The anarchists of the village received mixed signals. An early message had arrived from Jerez telling them to rise no matter what, but the beacons in Media Sidonia which were supposed to signal the rising there had not been lit. Older, more moderate anarchist leaders wanted to put off action; younger militants insisted on immediate action. In the end the young militants won. The anarchists went on strike, cut the telephone line connecting them to the outside world, dug a ditch across the main road into town, posted armed men at the entrances to the village to keep outsiders from getting into the *calle* and at vantage points around the *casa cuartel*, both home and headquarters, of the Civil Guard to keep them off the *calle*. The insurgents then took possession of the *calle*, organizing a demonstration which paraded through the streets and led to the sacking of the tax office, a public place that is institutionally *calle*, not

*casa*. In the meantime, two anarchists, an older moderate and a younger radical, called on the mayor. The older asked the mayor to tell the Civil Guards that they would not be harmed if they stayed off the streets. The younger demanded the mayor tell them to surrender. The mayor may have delivered both messages. The sergeant in charge thought compliance dishonouring and demeaning, so he decided to go out to reconnoitre.

Until this point there had been no fighting in the streets, but when the guards appeared, the insurgents fired at them, forcing them to return to the barracks. No one had been hurt, but the sergeant and one of the guards, both armed, went upstairs to a window. The insurgents shot at them again, mortally wounding the two at the window. So, for the first time, fighting invaded the *casa* from the *calle* and two men died. There followed a stand-off until police reinforcements arrived and retook the *calle*, killing one villager and wounding another. Insurgents fled, many into the country others to their homes. The insurrection was over, with the tactical physical force used by either side accounting for 3 dead and 1 wounded. The fighting could have stopped then, with police keeping control of the *calle* until tempers cooled and the standard process of identifying and arresting insurgents could begin. Instead, the police immediately set out to identify and arrest, invading *casas* and beating their inhabitants, including one pregnant woman, to get them to name insurgents. This violation of the protective separation of the *casa* from the politics of the *calle* continued, provoking defensive violence from the inhabitants. One villager was killed defending his home, one policeman was killed and another wounded trying to break into another home. Eventually the police set fire to that house, killing its seven occupants. In further raids the police arrested twelve villagers, then took them to the burned house and summarily executed them.

In total three policemen die, two defending their somewhat anomalous *casa*, the third in attacking a village *casa*. In retaliation 21 villagers are killed, one in the *calle*, eight defending their *casa*, and twelve by summary execution at the site of the burned *casa*. One policeman is wounded attacking a *casa*, one villager in the *calle* and many more beaten in their *casas*.

Five things stand out. First, violence in the *casa* is greater than violence in the *calle*. Second, the retaliatory violence of the police was far greater than the violence of the insurgents. Third, more people were killed by summary execution than were killed in fighting. Fourth, only the first two killings were of and by residents of Casas Viejas, in the remainder killed or killed were outsiders. Fifth, the meaning of violence varied: fighters wounded or killed were themselves trying to wound and kill, any non-fighters wounded or killed in the fighting could be considered 'collateral damage'; but the summary execution in mass of unarmed prisoners after the fighting was over is violence of a different order. This slaughter is like the slaughter of animals; those so killed experience more than the usual threat of death to identity, more than a removal from their *casa* and *calle*, they experience a threat to their identity as humans. This pattern, if not the scale, of violence foreshadows that of the epidemic three years later.

The violence of Casas Viejas need not have happened. Had anarchists not received the message from Jerez that they should rise no matter what, they would not have risen. Having received the message, they would not have risen had the older anarchists prevailed over the younger militants. Having risen, the Civil Guard sergeant might not have felt honour-bound to go into the street had he not been told to surrender. Having retreated from the street, the sergeant and his fellow guard would not have been mortally wounded had they not presented themselves with arms at an upstairs window. One more would die in the ensuing fighting, but had the police limited their use of tactical physical force to putting down the insurrection, 19 more

would not have been killed. Even though the violence need not have happened, it showed what could happen.

The brutality of the repression in Casas Viejas contributed to the fall of the first Liberal-Left government of the Republic. Conservatives then ruled for two years, during which the young General Francisco Franco became Chief of Army Staff, only to be ousted and posted away to the Canaries when the conservative government lost the elections of 1936. For the first time, anarchists decided to take part in elections, joining the Popular Front, which won the elections of 1936.

### **Insurrections: Climax, Ronda and Grazalema 1936**

The anarchist revolution of 1933 failed because the anarchists were unable to co-ordinate a nationwide uprising. The military rebellion of 1936 inadvertently solved this problem by in effect co-ordinating the anarchists' uprising for them, because the widespread rebellion of 18<sup>th</sup> July provoked an equally widespread revolution from the Popular Front, which included the anarchists. Each side won about half the ensuing local confrontations, so the issue had to be decided by war.

In most of the mountainous area of Ronda and Grazalema the revolutionaries prevailed. In Ronda the only army presence was a small recruitment unit of fourteen soldiers commanded by a lieutenant colonel (Prieto Borrego 2010). Units of the quasi-military Civil Guard and Carabineros, armed border and customs officers, were also stationed in the city. On the afternoon of 18<sup>th</sup> July, the communist mayor, a weaver by trade, ordered the arrest of people thought to be supporters of the rebellion. At the same time, Falangists called on the commander of the Civil Guard to offer their support for the rebellion, but in contrast to the Civil Guard in Casas Viejas three years before, the Civil Guard in Ronda remained in its quarters awaiting orders. That the Carbineros were commanded by officers with republican sympathies and the Carbinero force was twice the size of the Civil Guard force may have had something to do with that inactivity. That night, the commander of the army unit led nine of his men to the city hall to confront the mayor and town council and take command in the name of the rebellion. Apparently drunk and incoherent, he drew his pistol and started shooting. The exchange of gunfire that followed wounded two of his men and killed one militant leftist. A crowd that had gathered in the plaza in front of the city hall intervened and disarmed the soldiers. The mayor called the Civil Guard, the commander was arrested and the Guard occupied the building until the dead and wounded were removed. The rebellion was over at the cost of two rebels wounded and one revolutionary dead in fighting that was in a public space, institutionally *calle*.

Rebellion defeated; the revolution took shape. Pedro López Calle came from Montejaque with his *columna*. He joined with the Ronda militia under the command of the two senior officers of the *Carabineros* to march on Grazalema and secure it for the revolution. Revolutionary committees were set up to organize local affairs, of which the three main ones were the Defence Committee, the Supplies Committee, and the War Committee. The Defence Committee had eight members, four of whom were anarchists, two were socialists, one was communist, and one Republican Left. Of the anarchists, two were officers in the Pedro López column, one other

was from Montejaque and the last from Benoajan. The Supply Committee had ten members, five of whom were anarchists and two of these were officers in the Pedro López column.<sup>5</sup>

Other things happened. An attack on religion began that sacked and burnt churches, defiled religious images and symbols, arrested and executed priests. Suspected enemies of the revolution continued to be searched for and arrested. The two army officers who led the attack on city hall were taken out, the commander from the jail, the wounded man from hospital, and shot. The revolutionary terror of Ronda had begun.

How many were killed in that terror is not clear. Estimates range from a high of over 700 to a low of 134. However, many were killed, not all of them were from Ronda. As of 2010 the effort to count named people killed lists 188, of which 153 were from Ronda, 21 from Arriate, and 14 from Gaucin. Among those killed along with the two soldiers were professionals, the property registrar, notaries, municipal officials and 20 priests. Most executions took place on two days, the 28<sup>th</sup> of July, when perhaps 52 were executed, and the 15<sup>th</sup> of August, the last in response to the conquest by the Nationalists of the city of Antequera (Prieto Borrego 2010).

In some ways this republican repression of 1936 repeated the republican repression of Casas Viejas in 1933, putting down an insurrection with excessive violence, though this time the anarchists were repressors rather than repressed. This time, too, the insurrection was better organized, better equipped and nationwide, and the repression was correspondingly greater and longer lasting, and this time it was defeated, for on the 16<sup>th</sup> of September the Nationalists took Ronda and retaliated with executions of their own.

The history of these Nationalist executions in Ronda is not as detailed as the history of the Popular Front executions, but as elsewhere in Spain significantly more were killed. I was told by one worker in Ronda that the number killed by the Nationalists was twenty times the number killed by the Popular Front. It would not be surprising if the multiple was at least as high as the seven (3 into 21) of Casas Viejas.

The course of events in Grazalema was similar. After the arrival of Pedro López and his column, a Defence Committee of three anarchists, one a local advocate of violent 'direct action' and one of the others from Benoajan, and a Supply Committee drawn from various parties were formed. Pedro López wanted to execute some twenty avowed or suspected supporters of the rebellion under arrest, but the Republican Left mayor saved their lives by refusing to give him the keys to the prison. A local anarchist leader took to the balcony of the council building to insist 'in the pueblo we are sufficient to eliminate anyone should that be necessary, and therefore no one from outside should meddle in the affairs of Grazalema' (Serrán Pagán 1980: 105). None the less, the member of the Defence Committee who was from Benoajan organized the execution by firing squad of 11 people on the 29<sup>th</sup> of August, 1936.<sup>6</sup> The churches already had been burnt during the April intervention by the anarchists of Montejaque, but an elderly priest who just happened to be in Grazalema was among those executed on the 29<sup>th</sup> of August.

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<sup>5</sup> On the Defence Committee three of the four who were not anarchists were Freemasons. The communist mayor was also a Freemason, as were the second in command of the Carabineros and two representatives on the War Committee. This configuration suggests two revolutionary power blocks, one of Freemasons who had been living for some time in Ronda, the other of anarchists, most of them recent arrivals.

<sup>6</sup> Those killed were heterogeneous: a 75-year-old pharmacist, a 66-year-old non-commissioned officer of the Civil Guard, a 36-year-old teacher, a 49-year-old merchant, a 24-year-old student, a 70-year-old banker, a 60-year-old farrier, a 29-year-old veterinarian, a 73-year-old priest who just happened to be in the pueblo at the time, and the owner of a boarding-house.

On the other hand, a local parish priest had been appointed to the Supply Committee. More generally, Pepe Rincón, the head of the CNT, and another older and more moderate anarchist leader protected the lives of wealthy and powerful people, for which some of the younger anarchist militants labelled them fascists.

On the 16<sup>th</sup> of September the Nationalists took Grazalema with a force of regular soldiers and Falangists and began a brutal retaliation. The 'direct action' anarchist on the Defence Committee was publicly tried and executed. Estimates of the number killed vary from 200 to 400, including thirty women. According to Serrán Pagán the Nationalists 'committed rapes, robberies, sackings, execution by firing squad of a pregnant woman, murder of a child to steal money it was carrying, and also those charged with this 'cleaning' made some women drink castor oil and submit to having their heads and vaginal parts shaved.' Rincón was saved when one of wealthy he had protected hid him from the Nationalists in his house.

Prieto Borrego says little about how the executions in the Republican repression in Ronda were organized, where they took place, or what happened to the bodies and neither does Serrán Pagán about both waves of execution in Grazalema. However, there is evidence for both places that bodies ended up in the countryside, possibly because they were killed there. In the 1960's crosses and plaques still marked the locations by the roads out of Ronda where bodies were recovered. In 2006 the remains of fifteen women were found in an unmarked pit near the road to Ronda about 10 kilometres from Grazalema. The women have been identified as kin or partners of leading Grazalema republicans. Two are sisters who share a surname with Rincón.

Exactly who was responsible for what violence is unclear, but it is certain that outsiders played a major role in both places. Serrán Pagán (1980) says that Pedro López's anarchists encouraged the church burnings and executions in Grazalema. The same anarchists also operated in Ronda, and there are reports of three lorry loads of heavily armed anarchists driving up to Ronda from the coast to kill prisoners (Brennan 1974: 316). To the extent that executioners were outsiders, they were unlikely to know the people they executed, but that would not be the case for any locals involved in the violence. That raises the issue of the interplay between personal and impersonal relations. Either can be hostile, but in different ways. For example, on one occasion we were told by an artisan that all priests should be rounded up and machine-gunned, on another that his parish priest was a saint whom he held in high regard. These statements are not contradictory. 'All priests' is a category about which the only thing that matters is that they are priests. 'My parish priest' is a person about whom what matters is the memory of past, and expectation of future, interaction. The artisan's relation to the former is impersonal and governed narrowly by what the artisan thinks of priests as a category; the relation to the latter is personal and governed broadly by what the artisan thinks of their ongoing interaction. In this case the personal relation was amicable, so the artisan would not have wanted the priest machine-gunned. Had it been rancorous, the artisan might have had an additional reason to want him machine-gunned.

Serrán Pagán says that in the brutal repression by the Nationalists many people were executed because of 'hatreds, quarrels, revenges, economic debts and broken love affairs' (1980: 106). I heard people in Ronda say much the same. I took it to be an attempt in the 1960's to downplay the role of class antagonism in the killings of 1936, but that doesn't mean that personal antagonisms played no role in those killings. If they did, they are not enough to account for the killings. Such personal antagonisms are common in social life, but in Spain they seldom lead to individual violence, and even less often to killings, much less the systematic slaughters of

the terrors of 1936. Some other factors particular to 1936 must have been in play and chief among them may be intervention of outsiders.

There may well have been two phases to the killings: one in which persons to be killed were identified, the second their execution. Known leaders and militants of the losing side who had not been killed in any initial fighting were likely arrested, but sympathizers were more of a problem, and it is here that people may have denounced as sympathizers those they felt had jilted, cheated, betrayed or maligned them. Even then, the denouncement may have been as much defence as attack, for in the febrile atmosphere of uncovering enemy sympathizers, any who feared that they might themselves be suspect could fend off suspicion by denouncing others. As to the executions, I doubt that many who pulled a trigger were aiming at people with whom they had a personal quarrel.

Despite the sense of chaos and disintegration, there is an underlying systematic. People who had good personal relations, like Rincón, who protected the wealthy and was then protected in return, were more likely to survive the violence than those who had bad personal relations. That systematic has a community dimension; not community as the social solidarity of people in a place, but community as the locus of personal relations, the place where most of the personal relations of most people are located.<sup>7</sup>

There is an issue about the scale of the violence of the Republican repression in Ronda in comparison to other places. Prieto Borrego insists that the pattern of repression did not differ from that of any other place studied, but says that violence was greater than in any other city in the province of Málaga.

Even so, the undisputable magnitude of the repression in Ronda was to be hyperbolized, more than for any city in Málaga, putting in place an incorrect historiographic interpretation, resulting as much from a clear political intention as from the absence of serious studies based on the comparative method. (2010: 23)

She gives no example, but I know of one that claims the violence in Ronda was extremely ferocious, and that is the one chosen by Gilmore to support his critique of *Compromising Relations*.

But even the most superficial historical research would have shown that in the first months of the Civil War, over 500 members of the local bourgeoisie were murdered by local partisans of the Popular Front, who first beat them with heavy flails and then flung them off the cliff that divides the town. (1985: 30-31)

Gilmore cites no source for this account. The only two I have found, and my knowledge of the literature falls far short of encyclopaedic, are brief asides by two English historians of the Spanish Civil War, Gerald Brennan (1962, first published in 1943) and Hugh Thomas (1977, first published in 1961). However, the Brennan aside is not in his history of the war but in his

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<sup>7</sup> Historians wanting to understand the 1936 pattern of *denuncias* might find the work of Africanists like Evans-Pritchard (1937) and Mair (1969) on witchcraft accusations useful background reading. Also useful might be the application of these Africanist models to the history of witchcraft in England by historian Keith Thomas (1971) and the social anthropologist and historian Alan Macfarlane (1970), particularly the latter's account of the historical factors that turned endemic witchcraft into epidemic witch hunts. The frequent references to *depuraciones*, purifications, and *limpiezas*, cleanings, also suggest that the work of Mary Douglas (1966) on purity and pollution might be useful.

later autobiography (Brennan 1974). In 1936 he was living in Churriana, near the city of Málaga, and he tells of three heavily armed lorries of anarchists driving up to Ronda from the coast and demanding custody of prisoners. 'They were then taken out and thrown alive over the high cliff that bounds the public gardens. Five hundred and twelve people died in this way.' (1974: 310) He makes no reference to beatings and cites no source.

The Thomas aside is in his history.

In the country districts, revolution itself consisted primarily of the murder of the upper classes or the bourgeoisie. Thus, the description in Ernest Hemingway's novel *For Whom the Bell Tolls*, of how the inhabitants of a small pueblo first beat all the members of the middle class with heavy flails and then flung them over a cliff is near to the reality of what happened in the superb Andalusian town of Ronda. There five hundred and twelve were murdered in the first month of war. (1977: 274)

Historical narratives, like all stories of past events, are determined only in part by the events themselves. The stories are also determined by the circumstances in which they are told. The teller always constructs the story to suit the circumstances of the telling—the audience, the time, the place, the teller's identity and sense of what is appropriate. The teller selects from the possible elements of the tale those that best suit the circumstances of the telling. Any story of the past thus has a double construction and a double truth. The truth of the tale told is its historical truth; the truth of its telling is its mythical truth (Corbin, 1995).<sup>8</sup>

For the social anthropologist the mythical truth of the stories told by informants is more important than the historical truth, for these stories are part of an everyday process of negotiating, affirming and confirming meaning, and a systematic understanding of that meaning is a prime object of anthropological inquiry. For the historian, of course, the truth of the tale told, its historical truth, is all important, and the truth of its telling, its mythical truth, is only of concern if it distorts the historical truth. The question here is whether the mythical truth of the 'more than five hundred killed by being thrown off the cliff' distorts the historical truth of what happened in Ronda in the early months of the Civil War.

Thomas looks the more likely source for Gilmore's story. Thomas and Gilmore both call those who were killed 'bourgeoisie', Brennan calls them 'prisoners'; Thomas and Gilmore say they were 'beaten with heavy flails', Brennan doesn't mention beating. Even if Thomas is not the source, Gilmore retells the same story. The differences between the two versions are significant. Gilmore does not refer the beating with flails and throwing off the cliff to Hemingway's fiction. Gilmore asserts 'they were beaten ... then flung'. Thomas is more qualified: the beating and flinging 'is near the reality of what happened', but how near is not specified. Gilmore includes the number of those killed in his assertive sentence about how they were killed. In Thomas's version the number is in a separate sentence, followed by a footnote to Pemán's biography of General José Varela, commander of the Nationalist forces that took

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<sup>8</sup> This formulation extends the argument of Levi-Strauss that mythical thought is intellectual bricolage (1966). He developed this argument from observations during fieldwork in Amazonia of how myths varied in one detail or another every time they were told. He regarded mythical thought as a prior 'science of the concrete' that produces novelty by recombining what already exists as compared to modern science that produces novelty by transcending what already exists. Most anthropologists have applied this argument to 'primitive' peoples and stories that would seem exotic and bizarre to any modern European. This reference to the primitive obscures an important point: mythical thought is common and pervasive; it, not modern scientific thought, is the basis of ordinary social intercourse everywhere.

Ronda in September 1936 (Pemán 1954). Pemán is indeed the source for the number killed but says nothing about how they were killed. So, Thomas's version could still be valid if only some of those killed were killed in the Hemingway fashion, whereas Gilmore's version requires that more than five hundred be so killed. In short, Gilmore's version is more constructed by the truth of his telling—that class conflict among the people of Ronda was especially ferocious—than are the versions of Thomas and Brennan.

The truth of Thomas's telling of the killings in Ronda seems out of line with the scholarship on which the rest of the book is based. The main reference is to a novel, a narrative genre in which the historical truth is hardly relevant, and Thomas gives no grounds for his claim that it was near to the reality of what happened in Ronda. The only citation is to Pemán's book, a perfect example of what Preston (1984) calls 'crusade historiography'. Although that does not automatically mean that the figure of 512 killed is exaggerated, that figure is an index of the viciousness of the vanquished in a book designed to celebrate the victory of the virtuous. Thomas then conflates Hemingway's fiction and Pemán's number with a spectacular and nearly unique feature of Ronda: its cliff. This feature, *tajo* in Spanish, has become emblematic—Ronda is often referred to as the *ciudad del tajo*, city of the tajo—and is much featured in tourist literature. That conflation then makes the tajo the instrument of murder. The truth of this telling seems to be making a powerful drama of a short story, but the supporting evidence is too shaky to ensure confidence in the truth of the tale told.<sup>9</sup>

These comments also apply to the abbreviated 'death by tajo' story of Brennan (1974). As he was living on the coast at the time and following events in Málaga, the only novel element of his version, the departure of three lorry loads of anarchists to Ronda, might have been something he saw or, more likely, heard happen. I know of no evidence that he was in Ronda during the killings, and when I visited him in 1966 (we did not speak of the war) he told me that he did not know Ronda well. He may well have heard that many people were being killed in Ronda, but it seems unlikely that talk on the coast would so precisely report 512 killings. However, Thomas's version was published nine years before Brennan's, so Brennan could have gotten the 'death of 512 by tajo' story from Thomas and conflated it with his own 'three lorry loads of anarchists' story.

Locals associate the tajo with death as suicide, not murder. We heard only two stories about the tajo as an instrument of murder in all our time there. The first was in our first few months when we mentioned to neighbours that we had been woken in the night by the moans and cries of a woman in distress. They said yes, she was suffering from a very tragic past. In 1936 her father, an arch-conservative, fearing that 'the reds' would invade his house to take him prisoner and execute him, vowed that he would never let them take him alive. One day his wife, wanting to be free to marry her lover, raised the alarm, shouting 'reds in the house, reds in the house!', and her husband ran out of the house and killed himself by jumping into the tajo. So, in this 'death by tajo' story a man who actually commits suicide is nonetheless murdered by the wife who tricks him into doing it. The setting of the story is a time when militiamen were raiding the homes of implacably hated class enemies and executing them, but in this story 'the reds' neither raided nor killed. As myth this story is about domestics, not politics, and personal relations, not class. The immediate truth of its telling to us was explaining the distress of a woman in the present by reference to an event in her past, but the story also expressed several cultural verities: that female shamelessness subverts male honour, that personal betrayal is

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<sup>9</sup> I have heard that tour guides bringing busloads of foreign tourists to Ronda have used 'death by tajo' stories to dramatic effect. Thomas's description of Ronda as 'superb' suggests he, too, had visited, so he may have heard one of these stories and thought that they were generally accepted as true.



more dangerous than impersonal defeat, and that domestic order is more fundamental than political order. Its message was that life and death were properly domestic, not political, and that to make politics a matter of life and death was foolish and self-defeating.

The other story we heard much later in the 1980's. We told a woman whose father and other relatives and family friends had been killed in Ronda by militiaman in 1936 about Thomas's story and asked if it were true. She replied that it was not and that the only killing of that kind that she knew of was of a man who was beaten to death and his body thrown into the tajo by the Nationalists after they took Ronda because in the months before he had threatened to do that to conservatives. The message here is of the biblical 'you reap what you sow' variety. Another person we asked also said that a guard of prisoners held during the time Ronda was held by the Republic told them they would be thrown into the tajo; and Prieto Borrego reports that the men who disarmed the officer leading the failed military rebellion had wanted to throw the officer into the tajo; but these are all reports of talk, not action.

When asked, people of Ronda flatly deny that 'the reds' killed anybody by beating them up and throwing them into the tajo. Their accounts suggest a pattern much like that described by Serrán Pagán (1980) for Grazalema, which shares with Ronda the intervention of Pedro López Calle, which features no beatings, with or without flails, heavy or otherwise, and which speaks only of executions by firing squad.

As in Casas Viejas three years earlier, the mere fact of execution threatened the human identity of the executed, but in 1936 the executed and their families were usually staunch Catholics who were denied the normal means of processing the souls of the dead because the anarchists had burned or desacralized churches and jailed or killed priests.<sup>10</sup> This intensified the demeaning effect of the treatment of the bodies of the dead. Thus, bodies left in the country were reduced from corpses honoured in a proper funeral to carrion left to rot in the open, from dead people processed as much as possible by human culture to dead processed entirely and directly by subhuman nature.

The mythical truth the stories we were told about these killings is that they attempt to counter the eradication of victims' identities caused by the manner of their deaths. The stories are as much about individuals as they are about class; they are oral parallels to the crosses erected by the relatives of the dead on the places where the bodies were found—crosses that attempt to reconstruct the identity of the dead by converting these places from subhuman nature to human culture. Both actions return to individuals something of the dignity lost by the demeaning way in which they died (Corbin 1995).

The Nationalists added a further step to the process of identity erasure—burying the bodies in unmarked graves in unknown locations. This was not simply atrocity in retribution for previous atrocity, because the Nationalists did it to their civilian enemies in cities where their rebellion was successful, most famously with the arrest in Granada of the poet and playwright Federico García Lorca and his subsequent secret execution and burial in the countryside. The practice

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<sup>10</sup> Denying their enemies funerals was not the main motive of the anarchists' attack on religion. There was a class element to that attack in that the wealthy were better able to command the services of the clergy in their struggle with the problems of human existence, thus enhancing their power at the expense of the powerless. No doubt the anarchists were aware that without clergy and churches the power of the wealthy would be curbed; and it is true that the individual morality that the anarchists wanted to replace religion with was less subject to differences in wealth; but the anarchists' main objection to religion was to subordination of the human to the superhuman, not subordination within the human.

became widespread during the Francoist repression, often on a much larger scale than the 15 women of Grazalema. These killings were not publically recognised until after the return of democracy. A movement began to investigate these deaths, eventually fronted by the formation in 2000 of the Association for the Recovery of Historical Memory, which used the skills of archaeologists to find and excavate the mass graves, of forensic scientists to determine how they were killed and who they were, and of anthropologists among others to interview the living with knowledge of the killed. As a result, the 15 women of Grazalema have been named, stories of them told and retold, their remains deposited in the municipal cemetery of Grazalema and a monument erected to them with the inscription '*Que sus nombres no se borran de la historia!*', 'That their names not be erased from history!'

### **Insurrection: Aftermath, Madrid 1981**

Sparked by Basque insurgency and linked to conspiracy among conservative generals opposed to the new democracy, the occupation of the Congress of Deputies led by Andalusian born Civil Guard Lieutenant Colonel Antonio Tejero was, as it were, the last gasp of Francoism (Corbin 1986). In the evening of 23<sup>rd</sup> February Tejero led some 200 Civil Guards armed with sub-machine guns into the Chamber of Deputies, which was in full session. Guns were fired in the air and the deputies were ordered to lie down on the ground. All but three—acting Minister of Defence General Manuel Gutiérrez Mellado, outgoing Prime Minister Adolfo Suárez, and, head of the Communist Party Santiago Carrillo—complied. The elderly general went up to confront Tejero, Adolfo Suárez went to join him, but eventually the confrontation ended with the deputies returning to their seats. Having secured control of the Chamber and posted guards at all entrances, the insurgents waited for the military to rebel in the rest of the country and for the arrival in the Chamber of 'a competent military authority'. This did not happen. Only General Jaime Miláns del Bosch in charge of the Valencia region managed to declare a state of emergency and put tanks onto the streets. King Juan Carlos opposed the coup. Loyal government forces surrounded the Chamber but did not attack. The next morning some insurgents deserted, the deputies were released, and the remaining insurgents surrendered. The insurrection had lasted 18 hours.

The armed forces involved restricted the use of tactical physical force to what was needed to achieve their immediate objective—for Tejero occupation of the Chamber of Deputies, for the government forces isolating the occupiers, for Miláns del Bosch showing his support for the coup. There was no fighting, no invasion of *casas*. The Chamber of Deputies was invaded, but it was a public space, and the three deputies who refused to lie down when ordered were not shot for their defiance. For all the panic, the submachine guns fired in the air and tanks on the streets, there was no fighting; no one was killed.

I was not in Spain at the time, but returned later that year when people in Ronda were still talking about it, among them a deputy from Málaga who was in the chamber at the time. He had known Tejero during an earlier posting in Málaga; and his appraisal of the meaning of his own conduct and that of Tejero and the leaders of the Communist Party and the conservative Popular (People's) Party was especially pertinent. There was, of course, much discussion of political alliances and opposition, of conspiracies and power relations. But there was even more about the personal conduct and integrity of many of the main players, partly because the first half hour of the occupation was seen on state television, and a private radio station had open microphones in the chamber and continued to broadcast. The three who refused to lie down on

the ground were much admired. The ones who did comply did so because they feared they would be shot, but were almost as worried by Tejero's threat to release them into the street dressed only in their underpants. Tejero managed to keep control of himself and the occupation, even organizing his own surrender. There was less comment on Milans del Bosch but one ardent Ronda conservative compared him favourably to General Pedro Merry, the Captain General of Seville. He said that Merry had not sent his tanks onto the street because he had drunk too much. To which his co-conservative, who knew the General well, replied 'But Perico Merry is always drunk by 7:00!' So this grand affair of state that threatened government and constitution and involved king, politicians and soldiers, also played out as a drama of *calle* honour competition.

If the 1933 Casas Viejas insurrection was a case of violence that need not have been but was possible, and if the 1936 Ronda and Grazalema insurrections realized that possibility on an immensely larger scale, the 1981 Madrid insurrection was a case of violence that could have been but wasn't.

### **Picasso's Guernica, 1937**

Picasso was born in Malaga but was living in France when the Civil war began. A strong supporter of the Republican cause, he was commissioned by the Spanish government in early 1937 to produce a monumental painting for exhibition in the Spanish pavilion of the International Exhibition to be held in Paris that summer. While he was working on it the Basque city of Guernica was bombed and he named the painting *Guernica*. When the war ended the painting was in New York. Picasso insisted that it belonged to the democratically elected government of Spain and should not be returned to the country until democracy was reinstated. It was finally returned to much fanfare in 1981, months after Tejero's 'last gasp' attack on democracy had failed.

The painting provoked much controversy (Corbin 1999). Seen from the perspective of the terms about which people think and talk, 'Picasso's *Guernica*' because of its history symbolized the fascist attack and democratic defeat in Spain and the horrors of modern warfare that soon would be magnified in World War II. However, from that perspective the images in the painting, though clearly images of violence executed with violence, did not seem to be about the bombing or the Civil War. The actual bombing of Guernica was an experiment in aerial bombardment by the Condor Legion, a German air unit using the most advanced military technology of 1937 to fight for the Nationalists, yet the only weapons shown in the painting are a spear and sword, both broken. There are no representations of fighting, of armies, political parties, labour unions, the city of Guernica, or the Basque country. The only thing that might be thought particularly Spanish is the image of a bull because Spain has bullfights, but this bull is not being fought; even if it were, a duel to the death between a man and a bull is hardly an adequate representation of mass aerial bombardment. In contrast, there is no discrepancy between images and event when the painting is seen from the perspective of urbane thought, the terms in which people think and speak. From this perspective it is not about a bombing or any act of warfare. It is about an urban space in which everything that is happening is disastrously wrong. Bulls may appear in city streets but never without being engaged by men. Bulls may be speared, but never horses. Women should not hold dead or unconscious children in their arms or be caught in burning houses. Men should not lie broken and hollow on the street, nor should wild flowers grow out of it. The painting is not about fighting, it is about the

consequences of fighting that violates *ciudad*, *calle* and *casa*. The painting depicts civil war as extreme incivility causing the collapse of Spanish civilization.

In sum:

- An ethnography of meaning can help us understand violations in events like insurrections and in representations like paintings.
- Stories produced as myth may distort historical truth less than some academic stories produced as history.
- The ethnographic present can be studied without reference to the historic past.
- Ethnography can inform history as much as history can inform ethnography.
- The ethnography of wholly oral societies can inform the study of literate societies.
- Social anthropology is the study of the human, not the 'primitive'.

Paul did not read my articles on insurrections and Picasso's painting. If he had, he would no doubt have chided me for Latinisms not justified as Spanish translations, but I think he would have liked my exploration of the explanatory potential of ethnography.

## References Cited

Brennan, Gerald. 1962 (first published in 1943). *The Spanish Labyrinth: An Account of the Social and Political Background of the Spanish Civil War*. Cambridge: Cambridge University Press.

Brennan, Gerald. 1974. *Personal Record 1920-1972*. London: Jonathan Cape.

Corbin, John. 1986. Insurrections in Spain: Casas Viejas 1933 and Madrid 1981. In Riches, David (ed.) *The Anthropology of Violence*. Oxford: Basil Blackwell, pp. 28-49.

Corbin, John. 1993. *The Anarchist Passion: Class Conflict in Southern Spain 1810-1965*. Aldershot: Gower Publishing Company.

Corbin, John. 1995. Truth and Myth in History: An Example from the Spanish Civil War. *Journal of Interdisciplinary History* XXV (4, Spring): 609-625.

Corbin, John. 1999. Images of War: Picasso's Guernica. In: *Visual Anthropology* 13: 1-21

Corbin, John and Marie Corbin. 1984. *Compromising Relations: Kith, Kin and Class in Andalusia*. Aldershot: Gower Publishing Company.

Corbin, John R. and Marie P. Corbin. 1987. *Urbane Thought: Culture and Class in an Andalusian City*. Aldershot: Gower Publishing Company.

Corbin, John, Marie Corbin and David Gilmore. 1986. Correspondence: Compromising Relations. In: *Man, New Series*, 21 (1): 137-138.

Diaz del Moral, Juan. 1969 (first published in 1929). *Historia de las Agitaciones Campesinas Andaluzas*. Madrid: Alianza Editorial.

- Douglas, Mary. 1966. *Purity and Danger: An Analysis of Concepts of Pollution and Taboo*. Abingdon-on-Thames: Routledge and Kegan Paul.
- Evans-Pritchard, Edward. 1937. *Witchcraft, Oracles and Magic Among the Azande*. Oxford: Oxford University Press.
- Gilmore, David. 1980. *The People of the Plain: Class and Community in Lower Andalusia*. New York: Columbia University Press.
- Gilmore, David. 1985. Review of *Compromising Relations*. *Man, New Series* 20 (2): 360-361.
- Hobsbawm, Eric. 1959. *Primitive Rebels: Studies in Archaic Forms of Social Movement in the Nineteenth and Twentieth Centuries*. Manchester: Manchester University Press.
- Levi-Strauss, Claude. 1966. *The Savage Mind*. London: Weidenfeld and Nicholson.
- Lison Tolosana, Carmelo. 1966. *Belmonte de los Caballeros*. London: Oxford University Press.
- Luque Baena, Enrique. 1974. *Estudio Antropológico Social de un Pueblo del Sur*. Madrid: Tecnos.
- Macfarlane, Alan. 1970. *Witchcraft in Tudor and Stuart England: A Regional and Comparative Study*. Abingdon-on-Thames: Routledge and Kegan Paul.
- Mair, Lucy. 1969. *Witchcraft*. London: Weidenfeld and Nicholson.
- Mintz, Jerome. 1982. *The Anarchists of Casas Viejas*. Chicago: University of Chicago Press.
- Pemán, José María. 1954. *Un Soldado en la Historia*. Cádiz: Escelicer.
- Pitt-Rivers, Julian. 1971 (first published in 1954). *The People of the Sierra*. Chicago: University of Chicago Press.
- Preston, Paul. 1984. War of Words, The Spanish Civil War and the Historian. In: Preston, Paul (ed.). *Revolution and War in Spain 1931–1939*: 1-13. *London*: Methuen.
- Prieto Borrego, Lucía. 2010. *La Guerra Civil en Ronda*. [https://www.researchgate.net/publication/348781256\\_La\\_Guerra\\_Civil\\_en\\_Ronda](https://www.researchgate.net/publication/348781256_La_Guerra_Civil_en_Ronda). (Uploaded to website by author 2021.)
- Serrán Pagán, Ginés. 1980. La Fábula de Alcalá y la Realidad Histórica de Grazalema: Replanteamiento del Primer Estudio de Antropología Social en España. In: *Revista Española de Investigaciones Sociológicas* 9: 81-115.
- Stirling, Paul and Marie Corbin. 1973. *Application of Computer Techniques to the Analysis of Community Census Materials with Special Reference to Southern Spain*. SSRC Report HR 720/1.
- Thomas, Hugh. 1977 (first published in 1961). *The Spanish Civil War*. Harmondsworth: Penguin.
- Thomas, Keith. 1971. *Religion and the Decline of Magic: Studies in Popular Beliefs in Sixteenth- and Seventeenth-Century England*. London: Weidenfeld and Nicholson.

## **Nancy Lindisfarne-Tapper**

### **When the Anthropology of Women was Revolutionary – A Memoir**

In 1983 Richard Tapper and I were studying people's religious beliefs and practices in the small Turkish town of Eğirdir. We worked in tandem easily and well, and when women and men segregated themselves, we followed suit. Thus, Richard went regularly to prayers in the mosque, while I often attended prayer meetings women held in their homes.

Most of the women at the prayer meetings were elderly. Many were heavy and arthritic, and it was accepted they could say their prayers while seated. The women were kindly, and the prayer meetings moving and meditative, though at the end there was usually time for the women to catch up with each other and have a good gossip.

At each meeting, I was always aware of which window I should jump for if the police or soldiers came to the door. I cannot remember now who was the first to tell me, probably Hacı Maktume. Certainly, it was something we all knew. And the women told me how, in the 1950s, the women's prayer meetings had been violently broken up by the authorities, and that now, under martial law, no one knew what might happen next.

This is a paper about fieldwork, feminism and the anthropologies of women and religion and how I found myself ready to see the world – the wars, the empires and the anthropology – from those rooms. It is also about Paul Stirling, a kind and generous mentor, who helped me to get to that place.

### **Starting Out**

In 1964, I came to London to study anthropology at the School of Oriental and African Studies (SOAS) as Junior Year Abroad student from the United States. During that year Richard Tapper and I fell in love and married and in the summer of 1965, we travelled together to Iran where Richard was beginning fieldwork with the Shahsevan, Turkic-speaking pastoral nomads living in Iranian Azerbaijan. Richard then stayed on alone, while I returned to the US to finish my undergraduate degree in anthropology at Washington University in St. Louis.

Richard and I knew there was considerable everyday and ceremonial segregation between women and men among the Shahsevan. During his extended fieldwork, Richard had very little contact with the women, nor they with him. We planned that when I rejoined him in the summer of 1966, I would pay attention to the women of the families he knew best. It was a division of labour that fit the fieldwork and made sense to us and to the Shahsevan.

During that year, I looked for, but could not find, anyone to help me learn Azeri Turkish. So instead, I spent the year learning Standard Turkish with Ferit Polat. Ferit was the senior among some six Turkish army officers, all of them doing graduate work in engineering. They were a cohort of charming men who befriended me, invited me to watch them play soccer with the other foreigners and taught me the songs of Zeki Müren long before Martin Stokes wrote his splendid book on the celebrated trans woman and popstar (Stokes 2010).

Ferit told me about growing up very poor in Istanbul and his good luck at getting a scholarship to Robert College. He did his best to help me learn something of the language before I rejoined Richard in the summer of 1966 and began my own first fieldwork with the Shahsevan women.

## The Shahsevan Women

What neither Richard nor I were really prepared for was that the women were burning with curiosity about us and anthropology and they were dying to have their own foreigner to talk to!

The day we arrived in camp in the high pastures around Mount Savalan, I was bundled off by the women and spent the entire day trying to answer their questions about us, about England, and about 'Ijal'. By the end of the afternoon, I was exhausted and desperate to ask Richard about 'Ijal'.

'Ijal? Ijal is what they call me'.

'Wow. So, what they wanted was a good old gossip about you and about men?'

'I guess so', Richard said.

Then he shrugged, hugged me and apologized for landing me in it.

My initiation into fieldwork with the Shahsevan women was intense, but also warm and amusing. That summer I saw that although gender segregation was considerable, the women had plenty of chances to meet other women. For much of the year they lived in fairly large residential tent camps; the women were also free to travel on horseback, sometimes alone, often considerable distances, to attend feast gatherings of 100 women or more.

Through the institution of *kheyr-u-sharr*, meaning sharing literally 'good and evil', women, like men, created around themselves a circle of friends between whom there was mutual support and reciprocal attendance at life-cycle feasts. There was a formality, and real sense of obligation, behind these networks which extended beyond the nomadic community to include local villagers.

I soon understood that the more well-liked and influential a woman or man, the wider the circle of *kheyr-u-sharr* on which their reputation depended. Women leaders were known as *aq birchek*, 'white-haired'. They were able to join the white-bearded men, the *aq saqal*, to discuss the pressing issues of the day. An *aq birchek* was, as people explained, the women's *aq saqal*. Early on, Mashhad Nubar, respected as an *aq birchek*, took me under her wing. She had travelled to Tehran and, as her title indicated, she had also made the pilgrimage to Mashhad.

Her friendship and wisdom made a great difference to how I understood the relations between Shahsevan women and men.

Richard and I found that we worked well together in the field. Ours was a cooperative project and we both felt that the gendered division of labour between us was not about hierarchy or sexism, but the best way to do good ethnography.

When we got back to London, Richard wrote up his fieldwork for a doctorate and later books, while I began graduate work for a two-year master's degree based on a dissertation, somewhere between a master's thesis and a Ph.D. (R. Tapper 1979, 1997).

## Studying Women

In my dissertation, I drew on my fieldnotes on women of the Shahsevan and wrote about what I called 'The Women's Sub-Society'. The phrase sounds antique now, but it was a novel subject at the time.

My interest was in separate, purposeful relations between women. And for those two years, I read widely to see what I could find about women in other Muslim pastoral societies. I scoured the SOAS library, reading about Kazakhs in Central Asia and tribespeople across the Middle East. The best, yet still terribly thin, comparative material I could find was on the pastoral Fulani of West Africa and the Tuareg of the Sahara. In the end, the dissertation title was *The Role of Women in Selected Pastoral Islamic Societies*. Inadvertently, I was in on the ground floor, studying the anthropology of women before it became a field. It continues to be a focus of my work today (N.Tapper 1968, 1978; Lindisfarne 2000a; Lindisfarne and Neale 2016).

In the spring of 1968, Paul Stirling acted as the external examiner for my M.Phil. During the examination, Paul brought forward examples from his Turkish village fieldwork about relations between women and men which he felt deserved reconsideration.

Of course, I knew his 1965 *Turkish Village* book and knew it to be exceptional in its sheer range and great attention to detail. It was also unusual in Paul's determination to look at changing social relations and relations with the state without falling into the modernization theory clap-trap that was current at the time. The detail in *Turkish Village* was rich enough for readers to ask further questions of the material, which was, I understood, the *sine qua non* of a good ethnography.

The book was unusual and much ahead of its time. Most books then typically said nothing about the actual practice of doing ethnography. Paul's account of the fieldwork he and his wife did was personalized, honest and he wrote about their joint study as 'our fieldwork'.

Though the men and women of the villages of Sakaltutan and Elbaşı were segregated from each other during much of their daily lives, both men and women were there, right the way through the book, and they had a three-dimensionality which made them lively and interesting.

During our several hours together, sitting in the sunlight in the house on Woburn Square, then the home of the SOAS Department of Sociology and Anthropology, Paul and I talked in detail about everyday relations.



Looking at his book now, one passage, in the chapter on Inter-village Kinship, captures what I most remember about meeting Paul and my M.Phil. examination.

Women often straddle two villages. Yet women's daily contacts are more restricted than men's. They occasionally go from one village to another in pairs or groups, but normally, for any distance, a man is expected to escort them.

Although people are always to be seen passing back and forth between villages, for most individuals a visit to another village is a comparative rarity. Women visit their natal homes at least once a year; children go to see uncles and aunts; animals, food and utensils are borrowed and returned, or bought and sold; craftsmen are hired, cures and charms and advice sought, marriage discussed and arranged, weddings and funerals attended, the sick visited, loans asked, given and repaid, crops shared, grain milled, border land disputed, refreshment and shelter offered and accepted. Even when kinship is not directly the occasion for contact, it almost always provides a channel. (Stirling 1965: 175)

From our first meeting, Paul became a friend. His interest and approval in what I was writing about women's lives and women's organizations made it easy for me to go on to further study, to fieldwork in Afghanistan, and then later in Turkey and Syria. Paul was unusual, non-judgmental and encouraging. There were plenty of men, and women, who weren't.

Paul was also Richard's external examiner for his PhD thesis. Paul had read the whole of Richard's very long text (which, divided, became two books - R. Tapper 1979, 1997), and turned the examination into a wonderfully productive conversation and the beginning of a long friendship.

## Regional Studies

Mediterranean anthropology, which included Turkey and North Africa, was just getting started in the 1960s. And ethnographies of Middle Eastern communities were scarce on the ground when *Turkish Village* was published, and most of them were not very good. Indeed, an anthropology of what we now call the MENA region of the Middle East and North Africa was late to develop, in part because of the complexity of the wider politics of the region.

After the revolution of Leach's *Political Systems of Highland Burma* (1954) and his *Rethinking Anthropology* of 1966, and *A Runaway World?* of 1967, anthropology in Britain gained a critical edge. It was seen less as 'the child of Western imperialism', in Kathleen Gough's phrase, than as an antidote to empire. Certainly, one of the reasons we chose to do fieldwork in Iran (and eventually in Afghanistan and Turkey), was precisely because these were countries that had not been part of the French or British empires where almost all of our contemporaries and anthropological ancestors had done their research.

But whatever our politics, as anthropologists, we had to accommodate to varieties of nationalism, deal with the Arab-Israeli conflict and understand something of the fierce Cold War politics of the region. And when we did fieldwork with rural Muslims, we were always aware of the deep hostility to religion among urban elites desperate to modernize (Lindisfarne

2000b, 2008; Hafez and Slyomovics, eds., 2013; Deeb and Winegar 2015; Lindisfarne and Neale 2015).

Fieldwork anywhere in the Middle East meant living and working with acutely politicized people. Often both women and men were more aware, more sophisticated and well-informed than the anthropologist about the legacy of British colonialism or the Soviet connections in Nasser's Egypt. They knew a lot about American neo-colonialism in relations with Israel, the Arab Gulf, the Shah in Iran, and NATO in Greece and Turkey. The Shahsevan, sitting with a radio on a mountain top in Iran in the summer of 1966, listened to Britain win the World Cup. They also followed programmes on the BBC World Service and Radio Moscow and had strong views about the American war in Viet Nam.

Yet the anthropological establishment of the time was conservative. There was little place, theoretically, for the overt politics researchers met in the field, and the thought of working towards a new anthropology could horrify more senior colleagues. There were casualties in this process.

In those very early years, women studying women also challenged the anthropological establishment. We were easily disparaged. And because politics was on the boil throughout the Middle East, studying women (or 'gender relations', when later we got there) was also more problematic than elsewhere. Practically everyone was rigid and categorical in their thinking - peasants, working class, professional people and certainly anthropologists.

'Islam' was treated as a monolith and often openly scorned. And the pernicious binaries by which 'men' and 'women' were stereotyped were barely questioned. Indeed, as we see from versions of Islamophobia, and the aim of 'saving Afghan women' as the pretext of the American invasion of Afghanistan, these concerns have continued to play a very considerable part in accounts of the region for the past fifty years (Lindisfarne 2015, Lindisfarne and Neale 2015, 2021).

As it turned out, Paul's book preceded a decade of publications by others on the Middle East, North Africa and the Mediterranean. But most of these in that following decade were over-determined theoretically, focused on a single issue or problem, and quite simply far more limited in scope than his detailed ethnography.

Early on there was remarkably little about women's work, domestic relations or household rituals. Geography meant less than rich detail and sensible analysis. Better to read Phyllis Kaberry on women's economic lives in Cameroon (1952), than be dismayed by the lack of comparative material on the Middle East. Hilma Granqvist's series of books of a Palestinian village, starting with one on birth and childhood published in 1947, were valuable, though folkloric rather than sociological. Joyce Roper was clearly aiming for something rather similar for Turkey, but her book was more travelogue than ethnography (1974). By contrast, B.J. Fernea's *Guests of the Sheikh: An Ethnography of an Iraqi Village* was a joy and inspiration when it came out in 1965. It was good when the Fernea treatment of women and Islam appeared in 1972, while Papanek's (1973) writing on purdah was sharp and very welcome.

And there were other landmark books such as Vanessa Maher's excellent ethnography of women in Morocco of 1974 and Fatima Mernissi's *Beyond the Veil* in 1975, while the project came into full flower with the publication of Beck and Keddie's edited volume, *Women in the*

*Muslim World* (1978). And when Nawal el Saadawi's crossover book *The Hidden Face of Eve* appeared in English in 1980, it marked a new stage in the regional politics of women.

Only later did there follow a spate of books with veils in their title: Beyond was followed by Over, Under, and, frankly, Up the Veil. Whether the titles were chosen by the authors, or imposed by the publisher, there was an opportunism to them that played into an increasing Islamophobia. And even more problematic, 'women' and 'men' were often treated as categorially different in these books. That meant gender relations were seen as fixed, implicitly biological and rarely considered in relation to differences of class or other kinds of inequality (N. Tapper 1979, Lindisfarne-Tapper 1997).

Meanwhile, an almost prurient fascination with the topic of honour and shame dominated the field (see, for example, Peristiany 1964; Lindisfarne 1994a).

And there was little concerning Turkey. Fatma Mansur, who wrote about Turkish women in the Beck and Keddie volume, published her book about *Bodrum* in 1972. Nermin Abadan-Unat's book on *Women in Turkish Society* appeared in 1981 and Çiğdem Kağıtçıbaşı's edited volume *Sex Roles, Family and Community in Turkey* came out in 1982. Yet a full decade later, Chris Hann (1993) would describe the spectre of orientalism that haunted the new writing on Turkey by Carol Delaney (1991), June Starr (1992) and others. But this was changing. In 1994, Jenny White's wonderful *Money Makes Us Relatives* raised the bar significantly, as did Tayfun Atay's 1994 SOAS PhD thesis on Naqshbandi Sufis (published soon afterwards in Turkish and in English in 2012) and the work of Yıldız Ecevit and others who contributed to Şirin Tekeli's edited volume, *Women in Turkish Society* (1995).

There is an argument to be made about this late flowering and Turkish national politics, but that is for another time.

## The London Anthropology Women's Group

There were two main reasons why I felt such a bond with the elderly women in the prayer meetings in Eğirdir. First, and surely most important, they reminded me of the people I'd come to know in Iran and Afghanistan from whom I had learned how much they valued kindness and how easily corrosive politics could hurt people and ruin their lives. And second was women's liberation. For me, caring Muslim women, political opposition and women's lib were part and parcel of each other.

The London Anthropology Women's Group began meeting in the spring of 1972. There were some fifteen of us at the start. We were a very mixed bunch. Several of the women were hardcore, well-read, Marxists. They were older than me, and daunting - no, intimidating. They were scarily confident not least because they were seriously posh. Others weren't that sharp, but mean and ambitious. One was a former girlfriend of Richard, and we were uncomfortable with each other. Yet others were marvels - funny, fun and smart as whips. We were all varieties of hippy. We swigged wine and passed around joints sitting in circles in rambling, ramshackle apartments mostly around Camden Town.

We talked about ourselves as honestly as we could, and our problems with lovers and friends and with doing anthropology. We had all read Simone De Beauvoir, and Erica Jong, Betty

Friedan – and our politics were informed by the horrors of the Viet Nam war, Franz Fanon and our understanding of the anti-colonial movements for independence, and CLR James and the civil rights movement in the United States.

Over the next year we met regularly and became increasingly unhappy both about how women were described in – or more often absent from – anthropological texts. We were unhappy too about how we were treated as professionals.

The meetings were exciting - thrilling actually - and our learning curve was nearly vertical. Soon we were thinking of how to spread the word and began planning a forum as different from the usual academic conference as we could make it. We were determined to remove the sting of competition and censure we knew only too well. In that world every word uttered by paper-givers, or in questions raised from the floor, were designed, not to learn new things, but to showcase the cleverness of the speaker.

By contrast, we aimed to find an informal style for a gathering in which we could introduce others to our new, and explicitly political, ways of thinking about anthropology.

In March 1973, the Women's Anthropology Workshop was the first such conference to be organized in Britain. Through word of mouth, we contacted other women – students and teachers. There were seventy of us at the workshop held in a big room we found at the Polytechnic of Central London. The simple, and most basic, idea we wanted people to take from the day was that understanding social phenomena of every kind meant looking at both male and female dimensions. Though much has changed in the discipline, the lesson is as relevant now as it was fifty years ago.

The day began with Janet Bujra leading a discussion about women and fieldwork. In many ways Janet was the informal leader of the group. Janet had done fieldwork on an island off the Kenya coast. She was in London writing up later research she had done in Nairobi with sex workers, who were not even called sex workers at the time, and no one did that. Janet was from Yorkshire and had a no-nonsense directness about her which I greatly envied. I remember the two of us having an Indian meal on Tottenham Court Road and her eating with her right hand. I'd done that too, with the Shahsevan and in Afghanistan, but I was amazed at her doing it in London. I thought she was absolutely great. I still do!

## **Women and Fieldwork**

Janet Bujra's contribution was balanced, but sharp. She said that we were going to learn to challenge the hostility of our male colleagues, to counter the jokes about 'blue stockings'. We were also going to counter the unexpected but often even fiercer chauvinism of women patronizing other women about working with women as 'terribly unimportant', 'intrinsically boring', a 'waste of time', and as letting 'the side down because now people would insist that that is all women are good for'. And we were going to reject the idea that women only succeeded by 'seducing or being seduced by famous male anthropologists.' In 1973, we were a long way from the MeToo movement which has, among other things, recently exposed the decades of sexual predation by the anthropologists John Comaroff at Harvard and buttying by Arjun Appadurai at Penn. But we were on our way (Bujra 1973: 2-3 (and see Gregory 1984:

322-325 who builds on the work of Bujra and the London Women's Anthropology Group); Lindisfarne and Neale 2022).

Janet Bujra also talked about our need to challenge the sexism in our theories that dominated our understanding of power, and value, and often led women to 'choose particularly difficult topics or locations in order to make it absolutely clear that we were equal to any male anthropologist'.

More recently it has become fashionable to carry out 'women's studies,' and undoubtedly this is a by-product of the women's liberation movement. (We have to add, however, that the movement has sometimes treated anthropological evidence in a cavalier fashion.) In spite of this new interest in women, old attitudes still persist. Those of us who have carried out research relating to women have commented on the contempt with which male anthropologists greet our work, and the condescension with which we are treated as researchers (Bujra 1973: 2-3).

She noted that in many communities where there was strict sexual segregation, women were often absent from public meetings and seemed, for instance, to play little part in dispute settlement. This has meant that women anthropologists interested in such topics often adopted a 'pseudo-male fieldwork role'. She noted with chagrin that as 'the influence of women in public affairs has often been under-estimated in the past, we have to admit that this fieldwork strategy may well distort our understanding of events' (Bujra 1973: 3).

She further commented on our discussion of this dilemma, which included an understanding that women were often less coherent, less straightforward and less effective informants than men. She ended sharply, 'Judging from my own experience, any category of people which is so excluded will be less articulate than those who take a full and equal part in political life'. From this we understood that we needed to develop new fieldwork techniques to learn from such people 'since we cannot fully understand any political system unless we see it from the angle of the ruled as well as those who rule' (Bujra 1973: 4).

It is an insight that led onto the certain understanding 'that communities in which sexual differences are not so clear are not necessarily easier to come to terms with'. Janet went on to speak of taboo subjects like the anthropologists, both male and female, who use their sex explicitly in order to get information. She talked of a male anthropologist who studied prostitutes 'at first hand, so to speak', and how we had all heard stories of women anthropologists who sleep with male informants, and male anthropologists who take local mistresses. Not necessarily a successful fieldwork technique, Janet said scathingly, but she went on to notice that it was a tactic 'condemned far more violently when used by women than by men' (Bujra 1973: 4).

## Reading Against the Grain

Next up was a session reviewing important published ethnographies. I began by rethinking Mary Douglas' ethnography of *The Lele of the Kasai* (1963). Pat Caplan talked about Orenstein's Indian village ethnography, *Gaon* (1965) and lamented the absence of women therein, while Diana Barker took apart John Campbell's account of honour and shame in

*Honour Family and Patronage* (1964), a topic which had such a dominant place in the ethnographies of the Mediterranean and Middle East at the time.

We had worked through the year by deconstructing many of the classic ethnographies, asking questions about the portrayal of women, or its lack. It was a very good way to learn a new feminist practice. The hypothesis behind these ethnographic reviews was that the male bias we found in the publications of many established male anthropologists was not necessarily a direct reflection of their gender alone. Rather it represented a more general preoccupation of the discipline.

One way of testing this was to consider the writing of an equally established female anthropologist whose work was often introduced to undergraduate students. It fell to me to take apart the arguments Mary Douglas was making about how decisions about marriage and economic life were made. I tried to look, simply, but no longer innocently, at how the internal evidence she presented belied her arguments about male power and authority (N. Tapper 1973).

Questioning the evidence from within is a formidable way to approach an ethnographic text, and male power and authority was the central theme of the book. But I was naïve and excited by what I had learned. It hadn't crossed my mind that Mary Douglas might not be so enthusiastic, but she was in the audience, and she was incandescent.

Mary Douglas was a small woman, carefully dressed, from senior colonial civil service families on both her mother's and her father's side. She was a devout Catholic and married to someone important in the city, the head of the Institute of Directors or some such. Her book, *Purity and Danger*, had come out in 1966 and had been a best seller and made her famous. She was by far the best-known woman anthropologist in Britain at the time.

Mary Douglas stood up, her face bright red. She was visibly shaking. Her rage was that of a patriarch, a person used to deference who brooked no challenge. She quivered, then starting shouting that I'd got it all wrong. And she went on ranting for what seemed hours, while I sat there, saying nothing because there was nothing to say.

Of course, I was never forgiven. But for me, and others in the audience, it was revelatory. The session dramatically changed the mood of the workshop for the rest. As we broke for coffee, everyone was talking, and the crescendo of voices came close to raising the roof.

I asked Janet if she would read a draft of this paper for me. By way of commenting on what I'd written, she told me how Lucy Mair had thrown a book at her and reduced her to tears for questioning Malinowski's functionalism. I then confessed to her that Lucy Mair had thrown me out of class for doodling rather than taking notes at a lecture which consisted of her reading from her own book, one that I had just read. Janet and I agreed that each of us in the Women's Anthropology Group probably had a rebel or two in our family background. And she added, 'that older generation of women pioneer anthropologists must have felt so threatened by us' which was probably true, but I suspect we were as nothing compared with the sexism and ridicule those pioneer women suffered from their male colleagues. Alice Kehoe, writing autobiographically about 'sisterhood in a sexist profession', describes how Margaret Mead 'reciprocated the men's coldness' and visited it on Kehoe and other women when Kehoe was a very junior colleague (Kehoe 2022: 22-23).

The practice of questioning the evidence from within has stood me in good stead ever since. Listen to any argument, in anthropology, in the social sciences generally, or in an op-ed piece in *The Guardian* or the *New York Times*. And ask, ‘Does the argument work if you take gender into account?’ And far too often it does not.

Not only do the gender biases jump out, but so too do biases about class and race. Then turn the practice around, and in any account about gender, ask – as a regular habit of mind – ‘What about class? and What about race?’. Or sometimes it is even more telling to ask: ‘What about Islam?’

## The Rest of the Day

After the break, Caroline Ifeka picked up the baton. She too was controversial. She considered how what she called ‘the female factor’ could and should unsettle mainstream anthropological theory. She included the ideas of ‘cognitive structuralists’ like Lévi-Strauss, the neo-Marxist ideas of women’s liberation, and even Edwin Ardener’s 1972 pioneering study of women’s models among the Bakweri of Cameroon. She also asked on what grounds – whether logical, theoretical or political – could we separate the study of women from the study of people. ‘What’, she asked, ‘is the intellectual framework of women’s studies?’ and ‘Do women’s studies exist?’

Caroline Ifeka’s was a forceful challenge to the identity politics that were beginning to take hold at the time. Her concern was that analysis would take on a self-fulfilling dimension. But it was a challenge that lost out to the niche studies that followed.

The meeting moved on when Dorothy Remy discussed the possibility of an economic anthropology of women and the questions that might be asked of hunter-gatherers, traditional agricultural societies and those engaged in cash cropping and of migrants to urban settings. Via an excellent ethnographic account of women’s work in the ‘household factories’ in China, Tricia Langton treated issues raised by Friedrich Engels’ *The Origin of the Family, Private Property and the State* (which had been easily available in English since 1941) but had become required reading alongside Selma James’ ‘Wages for Housework’ campaign (see James 1972). The day ended with Diana Barker and Pat Caplan. They were both already in university posts; they spoke of the sexism in university employment with judicious ferocity.

We wrote up our talks from the workshop, and though I don’t think it occurred to any of us to seek a formal publication, we did produce a mimeographed volume of the day. Looking at it now, it is remarkable that the word ‘gender’ appears only once, in a passing remark. But the revolutionary feel to our Women’s Anthropology Workshop in March of 1973 fed into other radical changes in anthropology that were happening at the time, including Pat Caplan and Janet Bujra’s edited volume, *Women United, Women Divided*, which came out in 1978.

In the summer of 1973, at the Association of Social Anthropologists conference in Oxford, the keynote speakers made our hair stand on end. Michelle Rosaldo spoke of the ideas that would be the foundation of the book she and Louise Lamphere were editing, *Women, Culture and Society* (1974). Sherry Ortner argued that women are to men as nature is to culture - her contribution to the volume, while Terry Turner’s structural analysis of the Kayapo myth of the jaguar was a tour de force. Meanwhile, Maurice Bloch had a Damascus Road conversion to

Marxism, while Joel Kahn, the committed Marxist, and others such as Stephan Feuchtwang and Ralph Grillo passionately joined the fray. Paul Stirling, Richard and I spent some time discussing how what we were hearing would make a difference to the anthropology of the Middle East.

A group of us from the Women's Workshop organized a breakout session. Several senior anthropologists made it clear they thought we were childish, and that such a workshop was completely unnecessary, given that Michelle Rosaldo and Sherry Ortner were both on the platform. We saw it as an activist intervention, and a chance to talk specifically about women in the profession, and we were proud of what we were doing. At one of the formal dinners, Mary Douglas had another go at me. By then I'd been inoculated by the electric atmosphere of the conference and I was mercifully immune to her ire. But it didn't make her look good.

On the last evening of the conference, late into a warm night, many of us sat out in a pub garden talking and laughing. We had a real sense that things were changing. I remember the gathering clearly. I wore a long dress I had made from the shocking orange and pink cotton cloth favoured by the Shahsevan women. I can still see many of the faces of those who sat around the circle, and I am as sure as I can be that Paul was also there.

## **An Afghan Interlude**

In the summer of 1968, when I had completed my M.Phil., Richard and I drove through Turkey on the way to our first visit to Afghanistan. My friend Ferit had visited us in London, and each time we passed through Turkey, we stopped to meet up, once in Gelibolu where he was stationed, and several times in Istanbul. Ferit was good at filling us in on what was happening politically, and that gave us some depth and perspective on the country as we travelled swiftly east.

When we did get to Afghanistan, we spent a month touring the Turkic-speaking areas of the north in the hope of meeting Uzbek pastoral nomads comparable to the Shahsevan with whom we might do research, in my case towards a doctorate. We had done our homework but were surprised that the large numbers of pastoralists we met in northern Afghanistan were almost all Pashtuns, not Uzbeks. And so we turned our attention to Durrani Pashtun pastoralists and villagers for our second stretch of fieldwork, in 1971-72.

Again, a division of labour between Richard and myself made ethnographic sense. And for me, it was also a chance to try out my ideas about gender segregation and women's organizations. I spent most of my time in camp, often with women, while Richard travelled widely in the region. And yet, in spite of many similarities with the Shahsevan, the three conditions I had suggested would be the key to a separate women's organization were not fulfilled among the Durrani. Thus, the women's contacts with men were not as exclusive nor as regulated as those of their Shahsevan counterparts. Veiling conventions among them were such that a Durrani woman normally did not cover her face to any man of any age or ethnic group while she was on her home ground. And a woman could entertain a male guest when no man of the household was present or available to do so.

In effect, Durrani women participated in a wide range of social activities where their primary interactions and identifications were with their immediate household, their wider family and



with other mixed groups of men and women in the wider community. Indeed, except on party occasions, including weddings and religious festivals when huge crowds of women would gather separately from the men, the women seemed to have little sense of operating in an exclusive feminine milieu. This meant, of course, that I had to change my own research focus, and consider the range of other explanations of women-only sociality which were being formulated at the time (as, for example, in Caplan and Bujra, 1978, and see Lindisfarne, 1997, for later developments).

In the end, my work became a study of the politics of marriage. It was another way of thinking about an anthropology of women. Among the Piruzai, the Durrani tribal group with whom we lived, households stood or fell according to how well the members of the household got on with each other, and how well they could pull together to take care of themselves. They were poor people trying to survive in a very unequal world. Getting married among the Durrani was an expensive business and was a way in which social standing was claimed and confirmed. Poor men who could not make good marriages for their sons and daughters became vulnerable in other ways too, while the wealthier households arranged marriages among themselves as a way of managing political conflict and competition and sustaining their ascendancy in the area (N. Tapper 1981, 1991).

The Piruzai we knew were warm, clear-thinking and tough. They lived hard lives in an unforgiving setting and had few illusions about what was possible. People didn't complain but coped with an alert intelligence they described as 'keeping your thoughts in place'. They were good at laughing at themselves and curious and wise about others. Their care and love of their children was a model Richard and I strove to follow when we had children of our own. I have said this many times to people in England, and they have simply not believed me. But it is the case that Richard and I both thought the people we knew in Afghanistan did a better job of raising children than most Brits (R. Tapper with N. Lindisfarne-Tapper 2020).

We left northern Afghanistan at the end of the summer of 1972 fully expecting to return for further fieldwork at the end of the decade. Our original idea was to spend six months in Iranian Azerbaijan studying practiced Islam in the small town of Meshkinshahr which we knew well from our stay with the Shahsevan, and another six months in Afghanistan doing comparative work on practiced Islam in Sar-e-pol, the market town favoured by the Piruzai.

Colleagues kept telling us there was little mileage in the study of practiced Islam, but it had caught our imagination. Richard's regional focus raised many further questions about the political economy of the area. There was so much more to understand about the ethnic mix of the area, about Sunni-Shia relations, about the similarities and differences between domestic rituals, habits of pilgrimage, the Seyyeds and Sufis, and the itinerant religious students who were called *taleban* (R. Tapper 1984; R. Tapper and N. Tapper 1986).

Neither Richard nor I were religious, though we had both been brought up in Christian families, his Church of England, mine the American version, Episcopalian. And though my mother never had much time for religion, in her teens she became the organist in the Episcopalian cathedral in downtown St. Louis. Richard too was a musician and both of us loved Western classical music. This brought with it a respect for people's faith, and that respect in turn made us despise the condescending secularism of the British elite.

In Afghanistan, we found we were particularly intrigued by the competing versions of Islam. So, for example, some prominent Uzbek religious scholars in Sar-e-pol considered that the

Americans were telling the truth about having travelled to the moon. Pashtun religious experts demurred and interpreted the cosmology of the Quran in ways that meant that, in this too, the Americans were lying as was their wont.

But this new research was not to be.

A political uprising convulsed Iran throughout 1978, and in February 1979, the Shah, a secularist, was deposed. Many different leftist parties were important to the overthrow of the Shah, but the religious elements united under Khomeini were far better organized and soon viciously eliminated leftist and other non-religious forces and claimed the revolution for themselves. Islam and Khomeini were undoubtedly popular, but by no means universally accepted. However, Iran was soon declared an Islamic Republic and in December 1979, Ayatollah Khomeini was declared the Supreme Leader of the country.

In Afghanistan, the political landscape over the next decade also changed dramatically and fast. There too the monarchy was overturned and a republic established in 1973, only to end violently in the spring of 1978. Then, to shore up the communist regime that followed, the Soviet Union invaded Afghanistan in December 1979.

The creation of the Islamic Republic in Iran, and the Soviet invasion of Afghanistan were both cataclysmic events, but also completely different. In Iran, many people turned to Islam to rid themselves of a corrupt capitalist dictator propped up by the American state. And in this process, Iran became a pariah state, targeted in a bloody, proxy war with Iraq and subjected to draconian American sanctions. In Afghanistan, people turned to Islam to unite and fight a guerrilla war against a massive military force, and forty years of war and civil war, tragic droughts and famines and the American invasion, occupation and defeat followed.

We were greatly saddened by what was happening, to the people we knew and cared for. Yet in the revolutions of 1978-79, there was next to nothing we could do. We were not welcome in Iran, and Afghanistan was a war zone where our very presence would have endangered the lives of anyone we were with.

As anthropologists, we felt ourselves jinxed, but we knew too how lucky we were to have a chance to do more field work. And more than ever we were keen to study practiced Islam. It had suddenly become rather more topical, with the new theocracy in Iran and the beginning of the *mujahidin* resistance to the Soviet forces in Afghanistan.

We talked at length with Adrian Mayer and Paul Stirling about our options. One question concerned languages – whether to try and work in Pashtu in new fieldwork in Pakistan, or to build on the Azeri Turkish we knew in Turkey. The other important question was, which of the two countries, Pakistan or Turkey, was less prone to political upheaval.

In the end, a study of the range of religious practices in a small town in Turkey seemed the best choice. Suddenly, we were starting afresh, now with two small sons in tow, and we had a lot to learn. To prepare ourselves, we gave ourselves three full summers (1979-81) to get to know more about the country, its anthropology and anthropologists. And straight away, we turned to Paul to help us get started.

## Talking Turkey

Paul was immediately helpful and supportive. In Istanbul in the summer of 1979, he welcomed us *en famille* – Ruard was not quite five, Edward just three. He was enthusiastic about our determination to include the boys in our work and was charming with them. Paul knew everybody, and that summer, through him, we met Şerif Mardin, who was in the middle of his research on followers of the Kurdish theologian Said Nursi. The boys remember swimming in the Bosphorus after lunch, while Richard and I were delighted that Şerif Bey approved our interest in practiced Islam. It was a felicitous start.

It was then that we also visited anthropologist Akile Gürsoy and met her sister Bilge, her parents and her grandfather, Celal Bayar. And Paul introduced us to the anthropologist Neph Saran at Istanbul University, and her student, Taylan Akkayan.

With Paul, we visited the Redhouse bookshop to buy their big Turkish dictionary. We met Deniz Kandiyoti for the first time at Boğaziçi University. Our friend, Ferit Polat had by now left the army and was working at Fiat, whose offices were nearby. Calling to make contact, I couldn't make sense of what I was hearing and thought my Turkish had simply failed me. I asked Deniz to phone again for me, and that was when we learned of Ferit's tragic death in a car accident not long before. Ferit's death cast a long shadow over our travels that summer and it gave our work an emotional depth which stayed with us throughout the fieldwork.

From Istanbul, we travelled to Ankara, where we spoke with David French of the Institute of Archaeology. He said of our proposed field study, neither politics nor religion would be unacceptable. In his notes of the conversation, Richard quotes David as saying, 'the Turks are fed up with being plundered by Americans especially and unless one is a big name or has big contacts like Paul Stirling, one will have to show oneself willing to cooperate in either some kind of collaboration, or practical study. Mardin will know how to go about things, and Akile is known, as is, of course, Güvenç.'

Richard also remembers how Paul also inadvertently helped us with language. We both, in the mid-60s, learned the rudiments of standard Turkish to prepare for fieldwork among the Shahsevan, but we soon had to discard most of what we had learned, since, although Azerbaijani and Istanbul Turkish are just about mutually intelligible, they are very different, particular at vernacular levels. Fifteen years later, we had to relearn Turkish from scratch, and to unlearn almost all the vocabulary, idiom and syntax that we remembered from Azerbaijan.

However, it is also the case that rural Anatolian Turkish shares some features with Azerbaijani that are not found in the western cities. One of these is the standard use of the word *arvad* for wife or woman. Early on, a mutual friend told us with some amusement how Paul on one occasion, in a formal gathering in Ankara, caused considerable shock and consternation when he used the term to refer to his own wife. The word *arvad* is known in Ankara and Istanbul, but in its Arabic sense of women's 'intimate parts', and indeed there is an extensive classical legal literature on the definition of '*awrat* / '*awrah*, whose further meanings, ranging from defectiveness to vulnerability and defencelessness, have provided the main juristic rationale for keeping women covered and secluded.

We had not yet met Bozkurt Güvenç, but knew meeting him was on the cards. Certainly, we were acutely aware of how fortunate we were, and our good luck continued. Paul had put Lale Yalçın in touch with Richard earlier that year, and when they first met, he gave her his copy of

Martin van Bruinessen's dissertation. I remember meeting Lale for the first time in Kensington and how greatly moved I was by her warmth and beauty. Through Lale we got to know Nükhet Sirman, and then in Ankara, Lale introduced us to her parents Aydın and Nilüfer.

We met Akile again in Ankara, and Edward remembers sitting on the balcony of what must have been Celal Bayar's house and watching young men drag-racing in their fast cars up and down Atatürk Bulvarı in Çankaya. We had supper with Paul's students Ayşe and Sencer Ayata and our lads played with their little daughter. Bahattin Akşit had plenty of ideas about possible towns as field sites, and soon everyone was making suggestions about where we should go. Ayşe Ayata suggested we visit her relatives in Kandıra, Akile remembered her family connection in Gediz, while Bozkurt Güvenç, who had become a special favourite with our sons, thought we might do well in one of the small towns around Kütahya (Lindisfarne 1994b).

It was Bozkurt Bey who introduced us to his colleague in the psychology department at Hacettepe University, Doç. Dr. Yıldız Kuzgun. Over a lunch in the cafeteria, Yıldız, without a moment's hesitation, invited us to stay with her family in Eğirdir.

That summer we visited a couple of dozen small towns in western Turkey before we fetched up in Eğirdir. There we found that Bozkurt Bey had got it completely right. Our friendship with Yıldız Hanım deepened quickly. We spent several weeks visiting with her mother and her brother, the filmmaker Yılmaz Kuzgun, in the apple orchards at the end of the lake. Through them, by the end of our stay it felt like we had met a good half of the other people in the town. We decided, with such a welcome, that we had found our town. And, of course, the lakeside setting of Eğirdir is beautiful.

We met Paul again at Redhouse in the summer of 1980 on our way to Eğirdir, where we stayed with the Kuzgunlar again for several weeks. He briefed us on what he was seeing and confirmed that we had been sorely mistaken about the state of national politics when we chose Turkey as likely to be a calmer prospect for fieldwork than Pakistan. He said he was definitely worried about the increase in violence over the previous year. He saw the Turkish state disintegrating, to be brought down by either a military or a right-wing coup, supported by Adalet Partisi and Milli Selamet Partisi. Weapons, he said, 'are freely available. It takes 10,000 liras to hire a killer and a Kalashnikov costs 76,000 lira'. The Soviets, he believed, had reasons for fomenting disturbances on both left and right, and all the political parties, except at the very centre, were anti-American. He talked of the smaller towns like Çorum, Fatsa and Kayseri having gone right, and of the violence suppressed after Süleyman Demirel became Prime Minister again in November 1979. And, Paul added, there was real fear abroad in Adana, and machine-gun fire heard at night.

Others too saw that the country was being torn apart between parties on the left and right. Communists and Muslims turning to new styles of Islamism were on one side, while on the other side, and aggressively so, was the military, hardline Kemalist nationalists, liberal secularists and neo-fascists like those also emerging in Europe at the time. It was clear that Turkey was edging towards civil war. The academic lefties we met that summer, even more than the summer before, scorned our idea of studying practised Islam. And they were more open in their disgust with the mostly poor, working-class people who were turning to evangelistic versions of Islam.

When we got to Eğirdir that second summer we found that the Kuzgun family and their circle of friends and relations were almost all fierce republicans. They were pleased to discuss how

and why the town, and religious practices, had changed in the previous fifty years. But the town was small, with a population of only some 11,000 people, and we already knew other people who were less well off, and also devout Muslims, and yet others, whose sympathies, though they kept very quiet, were with the Communists. There was violence in Isparta, the provincial capital, and in other small towns around the lake, but only one minor incident in Eğirdir that summer.

## In Theory

Theoretically, we arrived in Eğirdir determined to shed a lot of essentialist orientalist baggage, whether sexist or some form of prejudice against Islam. And we understood anthropology to be deeply and fundamentally comparative. We were particularly keen to break away from the conventional binaries which seemed to bedevil anthropology, and particularly the anthropology of the Middle East: between women and men, nature and culture, private and public domains, popular religious forms and those labelled ‘orthodox’ (N.Tapper 1987).

By 1980, the futility of attempting ‘universal’ definitions of terms such as ‘religion’ was widely accepted by anthropologists. Our starting point was similar to that so well described by Martin Southwold in a paper in *Man* in 1978. He argued that, at best, terms such as ‘religion’ may have only a loose analytical value as a “polythetic class”. (1978: 370) This was a quasi-definition based on a bundle of attributes where no one attribute is necessarily found to be common to all the members of the class.

Roughly, then, anything which we would call a religion must have at least some of the following attributes: (1) A central concern with godlike beings and men’s relation with them. (2) A dichotomization of elements of the world into sacred and profane, and a central concern with the sacred. (3) An orientation towards salvation from the ordinary condition of world existence. (4) Ritual practices. (Southwold 1978: 370)

And so on – Southwold listed some 12 attributes. And he continues, ‘The word ‘religion’ designates cultural systems which have at least some of these attributes; this is a polythetic class since some religions lack some of these attributes. (1978: 370-371.)

Our take on gender relations was similarly broad. The whole point of our collaboration was to work against compartmentalization and gender segregation. Rather, our aim was to put apparently separate pieces together. We both believed that it is this kind of determined holism what makes anthropology work. And each day in the field in Eğirdir, we did what we’d done during fieldwork in Iran and Afghanistan, and we exchanged notes, ideas and insights across the divide.

I was excited and gratified when I later saw Marilyn Strathern’s definition of gender in her *Gender of the Gift*. For her, gender was an open-ended category, one based on Wittgenstein’s idea of ‘family resemblances’. Gender is understood as the ‘categorization of persons, artifacts, events [and] sequences... which draw upon sexual imagery [and] make concrete people’s ideas about the nature of social relationships’ (1988: ix). It sounded just right and stretched my thinking that bit further.

My work on gender in Eğirdir started, unsurprisingly, by paying attention to women's gatherings. Barbara Aswad and Peter Benedict had written about women's visiting patterns in Turkey in a dedicated volume of *Anthropological Quarterly* in 1974. Lloyd Fallers had written an unpublished paper on Turkish Islam in 1971, and the Fallers, husband and wife, had also contributed a paper on sex roles in Edremit to Peristiany's edited volume, *Mediterranean Family Structures* (1976), in which there was also a paper by Mübeccel Kıray on changing intra-familial relations.

But the idea of my treating reception day gatherings (*kabul günü*) and the women's prayer meetings and *Mevlut* celebrations of the birth of the Prophet as a paired set first arose in discussions we had with Akile Gürsoy who visited us in Eğirdir during the summer of 1980 (N. Tapper 1983; Tapper, N. and R. Tapper 1987),

Then, not long after we left Eğirdir that summer, on September 12, 1980, there was a military coup.

## Fieldwork in Eğirdir

We kept up with our contacts in Turkey and followed the news as best we could from the UK. We travelled to Eğirdir as tourists for some weeks in summer 1981 and decided to keep to our plan for extended fieldwork. We both applied, separately, for UK Social Science Research Council (SSRC) research grants and were both successful. Mine was comprehensive and included a salary, Richard's met his research expenses only, and we got further support from grants from SOAS and the Nuffield Foundation.

My SSRC grant was to study 'Women and Religion in a Turkish Town', and I was over-the-moon delighted, but others were not. It was ugly being attacked in *The Daily Telegraph* by Adrian Berry, singing loudly the conservative song. Margaret Thatcher's neo-liberal economics were beginning to bite, and cuts in public services and education cuts were being made on every side. My project, among several others, was slated under the headline 'From the faculty of fatuous research' (Berry 1982a, 1982b).

It was a particularly nasty attack and included scathing remarks on research into male contraceptive use just as the HIV/AIDS epidemic was taking off. Equally disparaged was a proposed study of long-distance traders in Libya, just at the time when Ronald Reagan took office and set off on the warpath against Muammar Gaddafi. Another study about car-sharing and peak and off-peak travel was also slated.

Looking back now, the irony is nauseating: as if understanding better the varieties of Islam, epidemic behaviours, a country destined for a proxy cold war, or early ideas for improving transport and reducing carbon emissions, would have made Britain bankrupt.

We all knew that anthropology as a discipline was vulnerable and under attack, as was University funding and the SSRC. Too much social science, too much knowledge and too many critical discussions of the state of the world never please conservatives who do not want us to question or change the status quo. Only a few years later, as a committee member of the Association of Social Anthropologists, I would help organize a two-day conference of

anthropology heads of department as a way of forming a united front and lobbying group to protect the discipline (N. Tapper 1980; N. Tapper, Akeroyd and Grillo, 1980).

But I also knew from Professor Adrian Mayer, who had supervised my Ph.D., that the SSRC was a circus of elites. He described being on one of the more senior committees as like being trapped in an Escher drawing - just when you thought you saw a way through and understood how things worked, there was yet another unexpected level of power play to deal with.

In the end, I took my concern for education, my furious feminism, and my deep anger at the scorn poured on Islam, to Jonathan Benthall, the then Director of the Royal Anthropological Institute. Jonathan was part of the establishment, and he knew how to deal with the *Telegraph* people. We talked and I understood that the situation of the discipline was so parlous that I thought him wise to suggest I ignore the jibe, and just get on with the work.

## Eğirdir Proper

It took a long time to get official permission from the Turkish authorities, but, martial law notwithstanding, in December 1982, we were able to return to Turkey for extended fieldwork. We touched base with Şerif Mardin in İstanbul and Chris and Ildikó Bellér-Hann in Ankara. I remember Ildi's utter dedication to studying Turkish, while Ruard and Edward remember that she and Chris made cassette tape copies of Pink Floyd and Jefferson Airplane for them to play in the car as we set off. We arrived in Eğirdir on the last day of the year. It was brilliantly sunny and bitter cold. At the end of the lake, the wind and waves had pushed up great rafts of ice and turned the trees in the café garden into fantastic ice sculptures.

We were a household of five, Richard and myself, our sons Ruard and Edward, and Susan Richardson, whom we'd hired to help tutor and look after the boys. Within days we had found a flat to rent, and enrolled the boys, who were now eight and seven, in first grade at the Zafer İlkokulu, while Sue took no time at all to find friends in town.

Turkish orthography is phonic, and because the boys already knew about reading, they could soon read Turkish, initially without the slightest idea of what anything meant. But the other children were welcoming and, at first, they lived for the breaks in the playground. Soon they had a gang of friends, and like the other small boys, they had the freedom of the town. Adults kept an eye out for trouble, but mostly they were left to roam as they pleased.

Ruard and Edward never looked back. By the end of our stay, they had a good understanding of Turkish and the shocking vocabulary of street kids. Being there with children gave Richard and myself a status as parents and helped us make friendships beyond any anthropology we were doing. And living in Eğirdir has turned out to be of enormous importance to both Ru and Eddy.

The neoliberal discourse of personal and institutional risk fostered in recent decades has led to the outsourcing of much academic research, and almost certainly would have curbed our practice of fieldwork and analysis and precluded our taking the children to the field (see Andersson 2019; Baczko and Dorronsoro 2020). In light of these limits, it is worth celebrating the breadth and cosmopolitan bonds we, and our children, were able to forge in earlier years. Both Ruard and Edward, one after the other, spent nearly a year with Lale Yalçın-Heckmann

and Friedrich Heckmann in Germany looking after their son, Ilyas as his au pair and *ağabey*. Then, when Ruard was in his early twenties he took himself off to Green Lanes in London and worked at relearning Turkish and played with a SOAS music ensemble in İstanbul.

And about this time, Edward and I returned to Eğirdir with Yıldız Hanım for a brief visit. As we were walking down one of the narrow alleyways near where we had lived, we came upon Hacı Ayşe Ağartan, our erstwhile neighbour, weaving in the downstairs room of her house, just as she had done a decade before. We greeted her with great pleasure, and she urged us to stop and offered us tea and homemade sweet rolls. Then, when we were comfortable, Hacı Ayşe looked at Eddy and smiled a sly smile, ‘He was naughty – *yaramaz*’, she said. ‘But, my goodness, look at him now, he is such a fine young man – *delikanlı*’. Then she pointed to the overhanging room of the old, abandoned house across the street. ‘Do you see that hole in the upstairs floor?’

We nodded and looked up at the gaping hole.

‘Well,’ she said, ‘He made that when he fell through the rotten floor boards and right down here onto the street!’

Edward was completely abashed and went bright red. He had certainly never told Richard nor myself about this adventure. Nor, I should say, had Hacı Ayşe given away his secret either.

When Edward married, he and Natalia travelled to Eğirdir on their honeymoon. Eddy met up with Mevlut and Varol, his best friends from first grade, and they stayed with İbrahim Ağartan, Hacı Ayşe’s grandson, who had been an older brother, an *ağabey*, to the boys when they were little.

## A Visit from Paul

In the politics of the early 1980s, relations between General Evren’s military junta, Kemalist secularism, and the fascist right, created vast areas of confusion about the Republic and Islam and no one in Eğirdir knew what was happening. The tension opened up yards of shared and contentious vocabulary, contested systems of values, conversations riddled with contradiction and a rich range of republican and Islamic rituals to attend. We found there was, literally, nothing people said or did that was not grist for our ethnographers’ mill.

In early March 1983, Paul was on his way to Kayseri, with a project under consideration working with Turkish assistants (one of them was to be Emine Onaran İncirlioğlu, the co-editor of this volume) for a two-year restudy of the villages of Elbaşı and Sakaltutan and their emigrés. Paul was travelling by bus, and when he realized the bus would be stopping in Isparta, he called out of the blue, he asked if he could drop in on us for a visit. And, of course, we said yes.

Our fieldwork was fascinating and productive, and we’d been running around like mad things. After we received Paul’s call, we cancelled our date to have lunch with one of Yıldız Kuzgun’s uncles and his wife, Hacı Nazire, who was just out of hospital. We also postponed a visit to Yalvaç, a town at the northern end of the lake, and then I went round to say we could not visit the Çeliks in the evening, *akşamdan sonra*, because we had to meet our Professor in Isparta. But even so we had little time to get ready. And it was cold, snow was forecast, Richard was



coming down with something, and the petrol cap on the car had frozen. We barely got to the city in time for Paul's bus at 8pm.

When we got back to our flat in Eğirdir, we ate - Paul had been hours on the bus and was starving - and then we talked long into the night. Paul was himself catching up with how things were changing in Anatolia, and it was a wonderful chance for us to try out some of our ideas about what we were seeing. We started by explaining how we were organizing ourselves. It was nice to be able to tell him how we had found ways of collaborating closely while often having to divide our time between women's and men's activities, and he was keen to know how the kids were managing (Tapper, N. and R. Tapper 1989).

Then we told him about the differences and similarities we were seeing everywhere in the town between the ostensibly secular and explicitly religious values and activities. We dared to try out our heretical idea that, somehow, the cult of Atatürk was very much like the cult of the Prophet, by which we meant that there was a consonance between them in the ritual repertoire of the town. Later, in autumn 1984, a cartoon mocking General Evren appeared in *Cumhuriyet*, the republican broadsheet. The caption, 'Elhamdülillah Laikiz' – 'Thank God, We're Secular' – said it all (R. Tapper and N. Tapper 1987, 1991; N. Tapper 1990; R. Tapper 1991; N. Lindisfarne with R. Tapper, 2001).

The next morning, we drove down to the southern end of the lake, where lay the extensive apple orchards that had brought prosperity to the town. At one of the large cold storage facilities we introduced Paul to the owner, Yıldız's maternal uncle Ömer Findos, the 'apple uncle', and his staff. Paul jumped happily into the conversation, asking about the workers and how the international apple business was organized.

Returning to the main town, it was cold and bright and very quiet. Paul asked us about class relations and took pictures of the mixed houses, old wooden structures integrated with new reinforced concrete. Yet, he said, so often the furniture, acquired for a young couple when they first set up a new house or flat, seemed to reflect the date of their marriage, and was rarely renewed – or passed on. And we talked of how much time and expense townspeople invested, not in bettering others, but in 'keeping their own end up', and how the quality, quantity and style of food served to visiting friends and relations reflected real differences in wealth and standing (N. Tapper 1990/1991).

We walked around the municipal buildings and onto the children's school and the ruins of the ancient castle, but it was too cold to walk out along the causeway to the island, once inhabited exclusively by Greeks. That was a shame, because Paul was keen to know about the now moribund fishing economy, and to visit the last of the boat-builders. We told him what we knew but knew we should know more (see Atay 2005).

It was also a shame because Richard and I had been talking a lot about the earlier relations between the Turks and the Greeks, before the exodus of the Greeks in the exchange of populations in 1923. Only ruins of the Greek orthodox church on the island remained, but there were plenty of good memories about the Greeks alive among the people of the main town, and little fondness for the Turkish migrants from Greece who moved into the island houses after the Greeks had gone. It was a subject much on our minds, and we spoke with Paul about how many rituals, and all kinds of domestic details and foods, were a legacy of the relations between the Turks and the Greeks in the past – when sometimes they shared the same customs, sometimes they seem to have fashioned customs deliberately to mark the differences between

them. It is a topic we never got to the bottom of. In the autumn of 1984, just as we were leaving Eğirdir, a bus-load of elderly Greeks from Piraeus appeared, visiting the town for the first time since they left as children in 1923, but, sadly, we were only able to have the briefest of chats with them (R. Tapper 1994).

We spoke too about the earlier sacred geography of the town. We walked by several houses where there were still, now hidden, Muslim shrines. We showed Paul the wonderful Seljuk mosque and medrese, the new mosques being built by migrants who had made money in Germany, the Bektaşî *tekke* and other places important to Sufi practices, as well as other pilgrimage sites and sacred places around the lake. And the *hac* pilgrimage to Mecca was in the spring of 1983, and this too became a topic for us to muse on (N. Tapper 1990).

We also told Paul about the differences we were seeing between the men's and women's *mevluts*, and I made a note of Paul's comment that, 'women make more religious choices than men'. It fitted with what we were seeing. In Turkey, where the state had limited men's public religious activities, the central religious mystery of salvation was celebrated particularly in the women's *mevluts*, through an account of the birth of the Prophet. In effect, the very sexism that hurt women also allowed them to hide things in plain sight.

It was a short visit, but precious.

The next morning Richard had flu and stayed home, while Sue, the boys and I took Paul on a last tour through the narrow, twisty streets high up in the town. We were headed for a house where I was to attend a prayer meeting. And just before I said goodbye, I pointed to the small window part way up the side of the house. 'That is the window we will leave from if the police or soldiers come to the door.'

Paul looked at me. 'Really?'

'Yes, really.'

And Paul nodded. He'd completely understood.

I carried my personal history, of the Shahsevan, Afghanistan, and women's lib, into the rooms where, with those elderly women, I too listened to Süleyman Çelebi's Turkish *Mevlut* being sung. I continue to see anthropology, politics, feminism and Islam from their point of view. To be on their side, against inequality and oppression, is, I believe, a great gift. And Paul, an older, established anthropologist, was one of the first people to give me permission to be there.

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## References

- Abadan-Unat, Nermin. 1981. *Women in Turkish Society*. Leiden: E. J. Brill.
- Andersson, Ruben. 2019. *No Go World: How Fear is Redrawing our Maps and Infecting our Politics*. Oakland: University of California Press.
- Anthropological Quarterly. 1974. Special Issue: Visiting Patterns and Social Dynamics in Eastern Mediterranean Communities, 47.
- Ardener, Edwin. 1972. Belief and the Problem of Women. In: La Fontaine, J.S. (ed.). *The Interpretation of Ritual*. London: Tavistock, pp. 135-158.
- Aswad, Barbara. 1974. Visiting Patterns among Women of the Elite in a Small Turkish City. In: *Anthropological Quarterly* 47: 9-27.
- Atay, Tayfun. 1994. *Naqshbandi Sufis in a Western Setting*. University of London, Unpublished PhD thesis.
- Atay, Tayfun. 2005. *Göl ve İnsan: Beyşehir Gölü Çevresinde, Doğa-Kültür İlişkisi Üzerine Antropolojik Bir İnceleme*. Ankara: Kalan.
- Atay, Tayfun. 2012. *A Muslim Mystic Community in Britain: Meaning in the West and for the West*. Bremen: Europaischer Hochschulverlag.
- Baczko, Adam and Gilles Dorronsoro. 2020. The Ethical, Epistemological, and Conceptual Need to Resume Fieldwork. <https://items.ssrc.org/covid-19-and-the-social-sciences/social-research-and-insecurity/the-ethical-epistemological-and-conceptual-need-to-resume-fieldwork/>. Accessed 03.11.2022.
- Beck, Lois and Nikki Keddie (eds.). 1978. *Women in the Muslim World*. Cambridge, Mass.: Harvard University Press.
- Benedict, Peter. 1974. The Kabul Günü: Structured Visiting in an Anatolian Provincial Town. In: *Anthropological Quarterly* 47: 28-47.
- Berry, Adrian. 1982a. From the Faculty of Fatuous Research. In: *Daily Telegraph* 19.04: 9.
- Berry, Adrian. 1982b. Plea to Save Science Council. In: *Daily Telegraph* 20.05: 19.
- Bujra, Janet. 1973. *Women and Fieldwork*. Women's Anthropology Workshop. London: Mimeo.
- Campbell, John K. 1964. *Honour, Family and Patronage*. Oxford: Oxford University Press.
- Caplan, Pat and Janet Bujra (eds.). 1978. *Women United, Women Divided: Cross-Cultural Perspectives on Female Solidarity*. London: Tavistock.
- Deeb, Lara and Jessica Winegar. 2015. *Anthropology's Politics: Disciplining the Middle East*. Redwood City, California: Stanford University Press.

Delaney, Carol. 1991. *The Seed and the Soil: Gender and Cosmology in Turkish Village Society*. Berkeley: University of California Press.

Douglas, Mary. 1963. *The Lele of the Kasai*. Oxford: Oxford University Press.

Douglas, Mary. 1966. *Purity and Danger*. London: Routledge & Kegan Paul.

Ecevit, Yıldız F. 1995. The Status and Changing Forms of Women's Labour in the Urban Economy. In: Tekeli, Şirin (ed.). *Women in Turkish Society*. London: Zed Books, pp. 81-88.

El Saadawi, Nawal. 1980. *The Hidden Face of Eve*. Boston: Beacon Press.

Engels, Friedrich, 1941. *The Origin of the Family, Private Property and the State*. London: Lawrence & Wishart.

Fallers, Lloyd. 1971. *Turkish Islam*. Unpublished paper. Chicago.

Fallers, Lloyd and Margaret Fallers. 1976. Sex Roles in Edremit. In: Peristiany, John G. (ed.). *Mediterranean Family Structures*. Cambridge: Cambridge University Press, pp. 243-260.

Fernea, Elizabeth Warnock. 1965. *Guests of the Sheik*. New York: Doubleday.

Fernea, Robert and Elizabeth Warnock Fernea. 1972. Variation in Religious Observance among Islamic Women. In: Keddie, Nikki (ed.). *Scholars, Saints and Sufis*. Berkeley: California University Press, pp. 385-401.

Granqvist, Hilma. 1947. *Birth and Childhood among Arabs: Studies in a Muhammadan Village in Palestine*. Helsingfors: Söderström & Co.

Gregory, James R. 1984. The Myth of the Male Ethnographer and the Women's World. In: *American Anthropologist* 86 (2): 316-327.

Hafez, Sherine and Susan Slyomovics (eds.). 2013. *Anthropology of the Middle East and North Africa: An Introduction to the New Millennium*. Bloomington, Indiana: Indiana University Press.

Hann, Chris. 1993. Culture and Anti-Culture: The spectre of Orientalism in New Anthropological Writing on Turkey. In: *Journal of the Anthropological Society of Oxford (JASO)* 24(3): 223-243.

James, Selma and Mariarosa Dalla Costa. 1972. *The Power of Women and the subversion of the Community*. London: Falling Wall Press.

Kaberry, Phyllis M. 1952. *Women of the Grassfields: A Study of the Economic Position of Women in Bamenda, British Cameroon*. Colonial Research Publication No. 14. London: HMSO.

Kağıtçıbaşı, Çiğdem. 1982. *Sex Roles, Family and Community in Turkey*. Bloomington: Indiana University Turkish Studies.

Kehoe, Alice B. 2022. *Girl Archaeologist: Sisterhood in a Sexist Profession*. Lincoln: University of Nebraska Press.

Kıray, Mübeccel. 1976. The New Role of Mothers: Changing Intra-familial Relationships in a Small Town in Turkey. In: Peristiany, John G. (ed.). *Mediterranean Family Structures*. Cambridge: Cambridge University Press, pp. 261-271.

Lamphere, Louise and Michelle Z. Rosaldo (eds.). 1974. *Women, Culture and Society*. Stanford: Stanford University Press.

Leach, Edmund R. 1954. *Political Systems of Highland Burma*. London: Bell.

Leach, Edmund R. 1966. *Rethinking Anthropology*. London: London School of Economics.

Leach, Edmund R. 1967. *A Runaway World? The Reith Lectures*. London: BBC.

Lindisfarne, Nancy. 1994b. Through Children's Eyes: An Alternative Account of Fieldwork in Eğirdir. In: Altuntek, N. Serpil, Suavi Aydın and İsmail H Demirdöven (eds.). *Humana: Bozkurt Güvenç'e Armağan*. Ankara: T.C. Kültür Bakanlığı Yayınları, pp. 351-366. (1994a is listed under Lindisfarne, Nancy. 2017.)

Lindisfarne, Nancy. 1997. Women Organized in Groups: Expanding the Terms of the Debate. In: Chatty, Dawn and Annika Rabo (eds.). *Organizing Women: Formal and Informal Women's Groups in the Middle East*. Oxford: Berg, pp. 211-238.

Lindisfarne, Nancy. 2000a. *Dancing in Damascus*. Albany: State University of New York. (2000b is listed under Lindisfarne, Nancy. 2002.)

Lindisfarne, Nancy. 2002. Antropoloji ve Ayırksıcılık. Kardaş, Suat (trans.). In: Turan, Metin (ed.). *Folklor / Edebiyat, Sosyal Antropoloji Özel Sayısı: Nermin Erdentuğ Anısına*. (Anthropology and Exceptionalism, 2000b) 2 (22): 105-120.

Lindisfarne, Nancy. 2008. Starting from Below: Fieldwork, Gender and Imperialism Now. In Armbruster, Heidi and Anna Laerke (eds.). *Taking Sides: Ethics, Politics and Fieldwork in Anthropology*. Oxford: Berghahn Books, pp. 23-44.

Lindisfarne, Nancy. 2015. Thinking about Feminism and Islamophobia: 1-6. <https://annebonnypirate.org/2015/03/02/thinking-about-feminism-and-islamophobia-1-2/>; 'Traditional' and 'Modern' in Turkey', <https://annebonnypirate.org/2015/03/10/thinking-about-feminism-and-islamophobia-2-traditional-and-modern-in-turkey/> Feminism and Islam 2. Accessed 29.04.2022.

Lindisfarne, Nancy. 2017 (1994a). Variant Masculinities, Variant Virginites: Rethinking 'Honour and Shame.' In Cornwall, Andrea and Nancy Lindisfarne (eds.). *Mislocating Masculinity: Comparative Ethnographies*. London: Routledge, pp. 78-92.

Lindisfarne, Nancy with Richard Tapper. 2001. *Elhamdülillah Laikiz: Cinsiyet, İslam ve Türk Cumhuriyetçiliği*. İstanbul: İletişim Yayınları.

Lindisfarne, Nancy and Jonathan Neale. 2015. Oil Wars and Resistance in Afghanistan, Iraq and Syria. <https://annebonnypirate.org/2021/08/17/afghanistan-the-end-of-the-occupation/>. Accessed 29.04.2022.

Lindisfarne, Nancy and Jonathan Neale. 2016. Sexism, Class and Violence. In: Bora, Aksu (ed.). *Neoliberalizmin Cinsiyeti / Gender of Neoliberalism*. Kadın/Woman 2000 17 (1): 1-17. [https://scholar.google.com/scholar?hl=tr&as\\_sdt=0%2C5&scioq=Kad%C4%B1n%2FWoman+2000.+Bora%2C+Aksu+%28ed.%29.+2016&q=Lindisfarne.+Sexism%2C+Class+and+Violence.+&btnG=](https://scholar.google.com/scholar?hl=tr&as_sdt=0%2C5&scioq=Kad%C4%B1n%2FWoman+2000.+Bora%2C+Aksu+%28ed.%29.+2016&q=Lindisfarne.+Sexism%2C+Class+and+Violence.+&btnG=). Accessed 23.01.2023.

Lindisfarne, Nancy and Jonathan Neale. 2021. Afghanistan: The End of the Occupation. <https://annebonnypirate.org/2021/08/17/afghanistan-the-end-of-the-occupation/>. Accessed 29.04.2022.

Lindisfarne, Nancy and Jonathan Neale. 2022. Harvard, Sexual Politics, Class and Resistance. <https://annebonnypirate.org/2022/02/13/harvard-sexual-politics-class-and-resistance/#more-3990> Harvard. Accessed 29.04.2022.

Lindisfarne-Tapper, Nancy and Bruce Ingham (eds). 1997. *Languages of Dress in the Middle East*. London: Curzon.

Maher, Vanessa. 1974. *Women and Property in Morocco*. Cambridge: University of Cambridge Press.

Mansur, Fatma. 1972. *Bodrum*. Leiden: Brill.

Mernissi, Fatima. 1975. *Beyond the Veil. Male-Female Dynamics in a Modern Muslim Society*. London: John Wiley.

Oakley, Ann. 1972. *Sex, Gender & Society*. Aldershot: Maurice Temple Smith.

Oakley, Ann. 1974. *Housewife*. London: Penguin.

Orenstein, Henry. 1965. *Gaon: Conflict and Cohesion in an Indian Village*. Princeton: Princeton University Press.

Papanek, Hanna. 1973. *Purdah: Separate Worlds and Symbolic Shelter*. In: *Comparative Studies of Society and History* 15: 289-325.

Peristiany, John G. (ed.). 1965. *Honour and Shame*. London: Weidenfeld and Nicolson.

Roper, Joyce. 1974. *The Women of Nar*. London: Faber and Faber.

Southwold, Martin. 1978. *Buddhism and the Definition of Religion*. In: *Man, New Series* 13(3): 362-379.

Starr, June. 1992. *Law as Metaphor: From Islamic Courts to the Palace of Justice*. Albany: State University of New York Press.

Stirling, Paul. 1965. *Turkish Village*. London: Weidenfeld and Nicholson.

Stokes, Martin. 2010. *The Republic of Love: Cultural Intimacy in Turkish Popular Music*. Chicago: University of Chicago Press.

Strathern, Marilyn. 1988. *The Gender of the Gift*. Berkeley: University of California Press.

Tapper, Nancy. 1968. *The Role of Women in Selected Pastoral Islamic Societies*. M. Phil thesis. SOAS University of London.

Tapper, Nancy. 1973. *Ethnographic Review: Douglas: The Lele of the Kasai*. Women's Anthropology Workshop. London: Mimeo. 1-6.

Tapper, Nancy. 1978. *The Women's Subsociety among the Shahsevan Nomads of Iran*. In: Beck, Lois and Nikki Keddie (eds). *Women in the Muslim World*, pp. 374-398.

Tapper, Nancy. 1979. *Mysteries of the Harem? An Anthropological Perspective on Recent Studies of women in the Muslim Middle East*. In: Caplan, Pat (ed.). *Women's Studies International Quarterly. Cross-cultural Perspectives on Women*. 2(4): 481-487.

Tapper, Nancy. 1980. *ASA – A Professional Association?* RAIN 36: 6-7.

Tapper, Nancy. 1981. *Direct Exchange and Brideprice: Alternative Forms in a Complex Marriage System*. In: *Man, New Series* 16 (3): 387-407.

Tapper, Nancy. 1983. *Gender and Religion in a Turkish Town: A Comparison of Two Types of Formal Women's Gatherings*. In: Holden, Pat (ed.). *Women's Religious Experience: Cross-Cultural Perspectives*. London: Croom Helm, pp. 71-88.

Tapper, Nancy. 1985. *Changing Wedding Rituals in a Turkish Town*. In: *Journal of Turkish Studies* 9: 05-313.

Tapper, Nancy. 1987. *Nature and Gender: An Anthropological Perspective*. In: *King's Theological Review* 10 (2): 45-50.

Tapper, Nancy. 1990. *Ziyaret: Gender, Movement and Exchange in a Turkish Community*. In: Eickelman, Dale E. and James Piscatori (eds.). *Muslim Travellers: Pilgrimage, Migration and the Religious Imagination*, pp. 236-255.

Tapper, Nancy. 1990/1991. *“Traditional” and “Modern” Wedding Rituals in a Turkish Town*. In: *International Journal of Turkish Studies* 5 (1-2): 135-154.

Tapper, Nancy. 1991. *Bartered Brides: Politics, Gender and Marriage in an Afghan Tribal Society*. Cambridge: Cambridge University Press.

Tapper, Nancy, Anne Akeroyd and Ralph Grillo. 1980. *Training and Employment of Social Anthropologists*. RAIN 41: 5-7.

Tapper, Nancy and Richard Tapper. 1987. *The Birth of the Prophet: Ritual and Gender in Turkish Islam*. *Man* 22 (1): 69-92.

Tapper, Nancy and Richard Tapper. 1989. A Marriage with Fieldwork. In: Ariëns, Ilva and Ruud Strijp (eds.). *Anthropological Couples*. Focaal (University of Nijmegen, Netherlands) 10: 54-60.

Tapper, Richard. 1979. *Pasture and Politics: Economics, Conflict and Ritual among Shahsevan Nomads of Northwestern Iran*. London: Academic Press.

Tapper, Richard. 1984. Holier than Thou: Islam in Three Tribal Societies. In: Akbar, S. Ahmed and David M. Hart (eds.). *Islam in Tribal Societies: From the Atlas to the Indus*. London: Academic Press, pp. 244-265.

Tapper, Richard. (ed.). 1991. *Islam in Modern Turkey: Religion, Politics and Literature in a Secular State*. London: I.B. Tauris.

Tapper, Richard. 1994. Blood, Wine and Water: Social and Symbolic aspects of Drinking in the Islamic Middle East. In: Tapper, Richard and Sami Zubaida (eds.). *A Taste of Thyme: Culinary Cultures of the Middle East*. London: I.B. Tauris, pp. 215-231.

Tapper, Richard. 1997. *Frontier Nomads of Iran: A Political and Social History of the Shahsevan*. Cambridge: Cambridge University Press.

Tapper, Richard and Nancy Tapper. 1986. "Eat This, it'll do You a Power of Good": Food and Commensality among Durrani Pashtuns. In: *American Ethnologist* 13 (1): 62-79.

Tapper, Richard and Nancy Tapper. 1987. "Thank God We're Secular!" Aspects of Fundamentalism in a Turkish Town. In: Caplan, Lionel (ed.). *Studies in Religious Fundamentalism*. London: Macmillan, pp. 51-78.

Tapper, Richard and Nancy Tapper. 1991. Religious Education and Continuity in a Provincial Town. In: Tapper, Richard (ed.). *Islam in Modern Turkey: Religion, Politics and Literature in a Secular State*. London: I.B. Tauris, pp. 31-55.

Tapper, Richard with Nancy Lindisfarne-Tapper. 2020. *Afghan Village Voices: Stories from a Tribal Community*. London: I.B. Tauris.

Tekeli, Şirin. 1995. *Women in Turkish Society*. London: Zed Books.

White, Jenny B. 1994. *Money Makes Us Relatives: Women's Labor in Urban Turkey*. Austin: University of Texas Press.

Women's Anthropology Workshop. 1973. London: Mimeo, 60 pages.



## **Part Five**

**Paul Stirling  
in  
Sakaltutan and Elbaşı**

## Hülya Demirdirek

### From “Prof. Stirling” to “Paul”

I was standing in front of the apartment door in an old Aşağı Ayrancı building in Ankara, content with myself for having arrived 5-7 minutes early. Prof. Stirling was staying with Yıldız and Mehmet Ecevit at the time. He opened the door himself, took a look at his watch and said in a very friendly tone “you have to learn to be on time”. “Oh sorry, I did not realize I was late” I said surprised, to which he responded “you are early”! Could there be a better way of ingraining a life lesson into a young person’s mind?

That was the start of my relationship with him as an anthropologist and assistant. I had previously been his student, taking two courses – social organization and research methods – from him when I was a third-year sociology student at Middle East Technical University in the mid-eighties. The research methods course, in particular, had made an impression on us both: in my case because I was very eager to do fieldwork in a plastic factory much longer than required by the course work, and in his case because he found my dedication and field report sufficiently interesting as to invite me to be his clerical and fieldwork assistant for his summer fieldwork in Kayseri starting in 1986. My fascination with anthropology, which had started with Nükhet Sirman’s introductory course, became much deeper after working with Paul. Following my graduation in 1986, the pull of anthropology eventually resulted in me leaving the MA programme after the first semester in the sociology department and going to Norway to study anthropology. I used to say “I am a Turkish sociologist, Norwegian anthropologist and Canadian professor of anthropology” in order to give a short-hand educational and geographical background. I met and worked with Paul intensively in my very early formative years, between my exposure to Turkish sociology and British/Norwegian anthropology. By the time he died I was “anthropologist enough,” yet I wish I could have spent more time with him as a more rounded anthropologist and university professor and shown him how I shared with others what I had inherited from him. While his impact on me has been more on the personal level, academically I believe he shaped me most in the methodological realm. I also wish he could have seen the times of chaos engineering where unanticipated calamities and mistakes could be used to make sense. Many of us who write here know about his pioneering efforts in data sharing as well as his frustration with the organization of large volumes of data from his research in Turkey. Perhaps Michael Fischer will offer an account here that would be a remedy to Paul’s sense of endlessly incomplete data and puzzles of social change.

Since the idea of this book first came about, I realized that my retrospective gaze on Paul as a person seems not to have changed much since his death. After deciding to contribute a personal account and subsequently reading the other abstracts, I have had time to reflect thoroughly on what may be the most meaningful framework for rendering my own personal memories of Paul Stirling, the author of *Turkish Village* (1965). By locating myself as a student from a particular era of Middle East Technical University's sociology department and elaborating my encounters with him, I am hoping to perhaps add one modest pebble to the edifice that is the history of social sciences in Turkey. My memories recounted here necessarily reflect the values and assumptions around socio-political order and epistemology in late twentieth-century Turkey. I

do not claim to be a rounded representative of young METU students in the early 1980s. I do, however, at least share some commonalities with others in the repertoire of that period in Turkey before and after the 12 September 1980 military coup in Turkey.<sup>1</sup>

Interestingly, my very first encounter with Paul Stirling was before he became my instructor at METU. In the summer of 1985, while camping at the seaside in Antalya Province, I was staying with my sister and our boyfriends in small tents in Kaş Camping. Very early in the morning we woke up with members of the Turkish gendarmerie asking us to come out. It was a strange but not particularly unfamiliar scene in that era. Pointing their Kalashnikovs at us, they told us that my sister's friend's name had come up on a wanted list and they were going to take him to the station (*karakol*). We understood later that when we had registered at the campsite upon arrival, the rules at the time required the names to be passed on to the gendarmerie. Our friend had been released very recently from jail after being held in custody for a short while due to his left-wing activities at METU. When we walked with the gendarmerie to the campground office where their jeep was parked, before 7 o'clock, Paul Stirling was also there, looking, still wet in his swimming trunks, with a towel in his hand and trying to understand what was happening. I knew who he was because sociology students a year senior to me had already taken a class with him. Without telling him that I knew who he was I just started in English, explaining that we were all METU students and the gendarmerie had told us that there was a warrant for our friend, but in fact he had just been released from jail. Since I was extremely anxious about what might happen, I found it somewhat comforting that at least he had witnessed this scene. We had no idea why there would be a warrant just after he was released without being arrested. After a few hours of waiting with no word from them (there was no mobile phone in those days!), I got a message from my sister who was allowed to phone the campground office to tell me that they were waiting for a fax from Ankara so that the matter could be resolved. Apparently, the list of wanted people had not been updated and his name had not yet been removed. They finally returned in the afternoon and when we next saw Prof. Stirling, we told him what had happened. Some of his Turkish exclamations were very heart-warming. I would later learn that his own research had been affected by the coup in Turkey at the time. Reading other authors in this volume will make it clear that politics and social dynamics in Turkey at the time – and especially the social change that was always on Paul's mind – played a prominent role for students and faculty alike. In our interactions I could see his dissatisfaction about excessively tidy Marxist explanations or leftist student knee-jerk reactions to the mention of certain perspectives and thinkers. While I too carried most of that young leftist baggage, chance combined with my own curiosity seem to have conspired to put me on an explorative footing that enabled me to work with Paul. Still, many of my written and unwritten memories revolve around ideas, prejudices, knowledge and hierarchies of someone who is a product of that era. There are of course other constellations that were a part of the atmosphere back then and are also applicable today: although most of us realized the connection between English proficiency and the writing skills required for a social science education in the context of our study experience, our classmate the late Aslı Düzgünoğlu – who was an almost perfect bilingual – was a case in point. Since she had finished high school in the UK, Paul (surely alongside the other instructors) could see how different her papers and exams were. It was not only her pure English proficiency; she was also well trained to connect with a text – which made both her comprehension and own writing better than the rest of the class. The others who were relatively better equipped for English reading/writing were those who

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<sup>1</sup> The years before and after the 12 September 1980 *coup d'état* have created a generational interval for the high school and university youth of the era. The political violence of the 1970s and the oppression of the military regime have had long-term consequences above and beyond the suspension of democratic rights, banning of political parties or imprisonment at a massive scale.

came from TED College<sup>2</sup> and had skipped over the English prep school year at METU. The rest of us had varying degrees of proficiency depending on the high school attended and prep school class level. Many of us thought that we were critically minded – and we were in some ways –, but I believe we had a poor connection to the text. In one course, we had been asked to create a summary of an article. Many of us considered this assignment to be so pedestrian, not realizing the significance of making a deeper connection with a text. Yet, it is possible that the instructor's intention was not that pedagogically well informed either.

In our later conversations Paul told me that he struggled to be fair in his assessment of the student work because knowledge and English skills went hand in hand. He had no doubt that some students were highly disadvantaged due to their poor English abilities. If the focus here were not on Paul, I would have written a lot more on this issue, but in his case it is about his awareness, honesty and modesty as it relates to both the limitations of his own Turkish and the limitations of many Turkish speakers' English. When it comes to the content of a social science university education, I am similarly unsure about how much has changed since Paul's observations about the interrelated problems of language skills and analytical thinking.

### Juniority meets seniority

Paul had asked me to be his fieldwork assistant in 1986. However, as he subsequently found an anthropology graduate for this role, I stayed back in Ankara and worked in an office as a clerical assistant with the demographic information sheets for Sakaltutan and Elbaşı. When I went to Talas in the summer of 1987, he asked me to address him as Paul instead of Prof. Stirling. I recall trying to say it in an inner voice while he was explaining “eggy potato” in the kitchen and, not having used a microwave before, I needed to attract his attention to get help. “Pr... P... Paul, how do I open it?” It did not come easily to me, and, while I managed it eventually, I continued to address him with the polite form of “you” (*siz*) for many years when I recounted this conversation to others! The hierarchies of the Turkish language, among others, were hard-wired into many of us. Spending time back in Turkey after 26 years, I occupied a position of *hoca* (teacher) mainly due to age, education and job status, although I did also teach one semester at a university there. I cannot measure how much of my own insistence on not being addressed as *hoca* in cases where relationships moved to a more personal level was rooted in my own experience of being on the other end in the junior role. From the outset it might be seen as natural for an English person to ask to be addressed by their name, yet it is quite common for a full professor in the UK to be addressed as “Prof.” in academic settings. Before Paul, two of my METU instructors Nükheth Sirman and Lale Yalçın Heckmann had offered me such an equal linguistic footing. As with Paul, their invitation was similarly motivated by intellectual generosity and a sense of equality and openness.

How these values and dimensions of human interaction influence what kind of academic personas might be unfolding is of course relevant as an extension of these details: I was young and had gone to Norway with a passion to study anthropology. At a dinner in a Portuguese restaurant in Coimbra at the very first European Association of Anthropologists (EASA)

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<sup>2</sup> TED stands for *Türk Eğitim Vakfı* (Turkish Education Association), and its Ankara College I refer to here is the high school where the teaching used to be entirely in English. Although the secondary education landscape is very different now, until the mid-1990s state school graduates (with the exception of those who attended Anadolu High Schools) did not usually have proficiency in English. Only the graduates of TED in Ankara and some private colleges in Istanbul and a few other cities had a strong command of English.

meeting in the summer of 1990, Paul told me (surely not in these exact words since I do not have a record, but the gist is very clearly inscribed in my memory): “Look, those who were once my students such as Maurice Bloch and my peer Ernest Gellner are having a meal together at the forefront of this organization, and I am sitting with you.” He explained that he had no regrets or resentment, but rather he felt more comfortable sitting with me in a way – even though he was also aware of how the connections we make influence what we accomplish. I am not sure how deeply I understood his comment at the time. For me there was only anthropology and my immediate research prospects, including the help Paul could give me by introducing me to Ernest Gellner. I knew some individuals were more famous than others, but that was it. He was showing me how academic success was a layered process that differed from simply being a sociologist or anthropologist. Paul added (and the years subsequently proved him right) “you will be a very good anthropologist but not very good academic, you are too human and not ambitious enough.” Those were the exact words he used.

Our interactions during other EASA conferences, in Baku when he visited me during my fieldwork and in Kent when I was a visiting fellow during my doctoral fellowship in Oslo involved haphazard conversations about social change, politics and academia, among other things. Gender, class and power were commonly discussed concepts here, not least as I kept complaining to him about the “boys’ club” anthropologists in EASA. Sometimes, in confidence, I accused some of the older British anthropologists of behaving like colonial officers or those anthropologists who sat and read a newspaper while others were giving a paper. Paul himself would doze off during some presentations and confess to me – as I would similarly confess to him – his inability to understand the non-native speakers well. Yet in my view Paul was not arrogant. Maybe he was struggling to focus, but he did not ignore the content and at times surprised people with his questions and comments as mentioned in Emine’s contribution here. At the time he was generous and patient with me while I was showing feminist impulses with a mind that was much more open to the fashionable ideas of the day (being rather persuaded about the need for change in certain anthropological hierarchies) and was willing to engage with issues in my field; for example, when we attended a workshop on corruption in post-socialist Europe organized by Steven Sampson, Paul asked me why nobody talked about the fact that the Soviet system also produced many “honest” people.

After Tone Bringa had warned about the future of Bosnia in the very early days of the atrocities there and pressed for EASA to make a statement at its Prague meeting in the summer of 1992, I complained to Paul about Maurice Bloch’s argument that EASA was not a political organization and hence not in a position to make a public declaration. Paul told me that Bloch was a curious thinker with a genuine interest in knowledge and not somebody seeking a sterile, higher status like “X” (naming a well-known anthropologist) and he urged me not to jump to quick accusatory conclusions driven by my moral judgement. He was trying to tell me in a half English half Turkish sentence that while it was still possible to criticise, the automatic reaction I had did not sit well (*yakışmadı yav*) with my generally critical mind.

I told him something similar (*yakışmadı*) when I complained how after living as a vegetarian for more than 10 years – including my Baku fieldwork – I had to eat meat, including lots of pork fat, in the first three months of my stay in rural Moldova. Combined with heavy consumption of homemade strong spirit and wine during numerous life cycle rituals, it made for a rough three months. During and after many wedding parties I threw up and generally suffered a good deal, yet I still had to finish writing my fieldnotes. When I told this to Paul, he said “perhaps this was the subconscious reaction of the Muslim in you”. I was almost furious that he could suggest such an explanation. Mine was quite a Turkish reaction, I thought in

retrospect. Saying “how could you think such a thing” rather than saying “no, that is not the case!”

## **Materiality, rupture and continuity**

In addition to the impact Paul made on me at an early stage of my life, I carry a constant sense of materiality through various objects that I associate with him and that period. Along with my 1986 fieldwork diary and my personal diaries kept until his death in 1998, I have photos, daily life items and objects I inherited from him as well as objects of my own that I acquired in one way or another and relate to him. A black and white photo of him from the 1949 fieldwork has always been on my wall, first in my student hostel rooms in Oslo, then in Baku and subsequently on the corkboard of my university offices in Oslo, Lethbridge and Victoria. When he saw his picture on my wall in Baku he joked, saying “why do you have it there, I am not dead yet.”

One example of material connections is not attached to a particular object, but rather it remains consistently present and revisits me from time to time. Although we had already chatted casually about sinks and plugs in Talas, we had a long funny conversation on the topic in the autumn of 1996 in Kent. I mentioned an observation that I had made about public sinks in the UK since my first visit back in 1988. I understood that he was very attached to his plug so that he could stop the flow of water and collect it to wash himself at his home, although from a Turkish perspective (or at least one that for the most part can be safely generalised to the middle class) even that is quite questionable if the sink that he is using at home is shared with others.

With the exception of very new buildings in the UK, even in public toilets the hot and cold water-taps were separate. Water can only be mixed by putting in a plug and stopping the flow to fill the sink. However, if you are washing your hands in the toilet of a cinema or restaurant, you are hardly going to put in a plug and fill the sink with water to wash your hands. I knew that in Scandinavia notions of modern hygiene developed with the bourgeoisie, so I asked him how things were in the UK. In buildings, at least on those built after the 1950s, one would not expect to still see such separate taps. I suggested it reflected a resistance to change: “Ideas change about hygiene, people do not carry around a plug like you do any more, don’t you see that the building conventions here have not changed.” The entire plug issue was a mutual joke, me making fun of him for his insistence on carrying one, him pointing out my own and my mother’s obsessive cleanliness as I used my mother’s rules like root metaphors for Turkish cleanliness. Thanks to his peer Mary Douglas, Paul looked at sinks in England and cleanliness rules in Turkey from a perspective of purity and danger. He was very quick to analyse how my mother’s insistence on pushing my clean foot away when it touched a pillow for sleeping was about separation. Yet we were not able to agree when it came to sinks and water in the UK, with him convinced I was oversimplifying. My memory of that conversation is full of joy, a sense of light-hearted intellectual ping-pong. I have very little recollection of really light moments like this. If anything, I seem to be the one more inclined to smile when things remind me of him. When I hear about UK politics, for example, I recall my time as a visiting fellow in Kent, which coincided with the run-up to a UK general election<sup>3</sup> and growing hype around Tony Blair. While I got caught up in this excitement, Paul said “he is too liberal for my taste.”

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<sup>3</sup> In 1997, Labour defeated John Major’s Conservatives in a landslide victory and Tony Blair became Prime Minister.

I recalled a fellow student at METU and wished he were alive now to hear this because he used to say that Paul was a Demirel<sup>4</sup> supporter simply because he was able to grasp how the villagers could feel affinity with the right-wing parties. These days, it is not very often that I see Tony Blair on the news, yet when I see my English friends losing their temper at the sight or sound of Boris Johnson on TV, I miss Paul and become really curious to know how he would approach the whole Brexit saga and the current situation.

Since I am not in Ankara but in Vancouver at the time of writing this text, with one exception I do not have access to the objects that were part of the materiality through which Paul has been present in my life since 1986. The exception is a soft, navy-coloured 40 x 70 cm travel bag that I had bought for 27 TL in Kayseri with the 8 pounds I was earning back then as his research assistant and have since been using to carry my laundry to the laundry room in high-rise buildings first in Norway and then in Canada.<sup>5</sup> Those 8 pounds had also generated savings of 200 TL, which became my contribution to my family's first car. This materiality ranges widely from his son Simon's pocket Oxford Dictionary that he had with him and left to me to the fieldnotes writing system used in all my notebooks. I know the dictionary belonged to his son because "Simon Stirling" is written along the edge of the pages. Then there are the library cataloguing boxes that hold small index cards, papers with his writing on them, sealed white Turkish envelopes with the words "Sakaltutan poor photos" in his handwriting... The highlight of the written objects was the Guardian Weekly in its older wafer-thin A3 format! This is perhaps one of the few things to have accompanied me despite ruptures in places as I moved between countries and continents. Paul had a Guardian Weekly subscription that was mailed to him at the Talas apartment where he and Emine were staying and I had joined them later in the summer of 1986. After the fieldwork ended the Guardian Weekly continued to be sent to the Talas address and the kind civil servant (*memur*) whose friendship I inherited from Paul forwarded it to my Ankara address until Paul's subscription ended. Since then, the Guardian Weekly followed me in its newer formats on a subscription basis, although since the launch of digital access it has rather been me that followed the publication.

My *Guardian* reading maintained a connection to Paul both in terms of the content of the news and the accuracy of its reporting compared to many Turkish news sources. I remember reading Chris Hann (1993:224) referring to Paul as an empiricist, in a book review comparing him to other anthropologists working on Turkey. I am not sure how much of this I would attribute to Paul individually, but he would pop up in my mind as part of a broader British trend of being interested in evidence-based reporting on issues or a general quality of fact-checking that may be attributed to *The Guardian* or for that matter the BBC – in short, a good deal of reliability despite some very prominent biases. Perhaps this is more in line with British social anthropology in its traditional 20<sup>th</sup> century practice, i.e., paying more attention to social organization as something that can be talked about in more concrete terms than "culture" – not necessarily due to a denial of what "culture" might mean. In Paul's case I had thought it was more about being intellectually too shy or perhaps tentative due to a fear of getting it wrong, almost feeling self-deprecating due to language proficiency and gender constellations. My intention here is not to discuss "isms", our life experiences enable us to associate certain traits with someone or somebody which are also shared by others. Yet that is what creates the different stories. In my lineage of associations, attention to accuracy and loyalty to direct citation go back to Paul; this encompasses reading the Guardian (Weekly), but also extends to

<sup>4</sup> Demirel (1924 -2015) was the leader of the centre right *Adalet Partisi* (Justice Party). Until his presidency between 1993 to 2000, he had been Prime Minister of Turkey seven times from 1965 to 1993.

<sup>5</sup> Although some of these details end up revealing my personal quirks, I have included them more as historical and/or ethnographic glimpses into the scenery of those times.

dividing the page of the field notebook into two, numbering the items, leaving an empty space for afterthoughts, the fieldwork notebook writing system I had learned from him. When I was teaching research methods classes, the first simple system I used to show my students was the same. Then there was always the anecdote about how Paul had conveyed the warning Evans Pritchard had given him. In other contexts, I know the three things varied, but the combination he told me was: do not sleep with the natives, do not share your religious beliefs with them and do not go to bed before you finish writing your field notes. I told him that I had no doubt about the significance of the third after completing several fieldworks. I doubt that I knew enough anthropology at the time to react to the use of the word “natives” and the whole baggage of the colonial legacy that comes with it. Yet if I had known enough and reacted, my reaction would quite possibly again have been that of a young person not assessing the utterances in their historical context. While teaching social organization, especially with reference to Lucy Mair’s work, Paul had tried to show the significance of historical context. To this day I can remember how Paul’s awareness of the prejudices people carry made him depressed, for example when I hear a junior social scientist talking about a nationalist figure from history without recognising that nationalism was the progressive and resistant movement at the time.

## Caring

Although I met Paul several times during and after my anthropology graduate training, my impressions about him from Turkey during our fieldwork were pretty devoid of sensitivity to intersections of age, gender and cultural difference. I had not previously interacted with a man of his age who was constantly teaching me things (including improving my English<sup>6</sup>) yet did not make his seniority intimidating or undesirable. Having experienced uninvited excessive attention from men of various ages,<sup>7</sup> the type of interaction I had with him was very new to me then. I had never seen a man of that age thinking about household-related needs in such detail, attending to other people’s needs, being friendly but not patronizing or disturbing.

In the spring of 1985 Paul was staying with Sencer and Ayşe Ayata in Emek very close to our apartment in Bahçelievler. He came over one afternoon to go through research details with me. My mother was sitting in the living room with a heavily pregnant friend. At some point my mum called me and told me that her friend had started to have cramps. It felt absurd only because I was too young. Paul asked if her water had broken. I had no idea what that meant. I was almost apologetic for what was happening!! I was fascinated by his calm and matter-of-fact manner. He soothed me, asked about the family connections of the friend as they were leaving for the hospital. This only serves to illustrate my limited vision about men at the time. Some of Paul’s down-to-earth attitudes caused me to believe that as an anthropologist he was able to empathize more closely with other people’s experiences. I do not remember how other sociology students perceived him, but it is possible that he did not check all the boxes of a

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<sup>6</sup> My English surprised me after I revisited old term papers and fieldwork diaries from the late 1990s that I had not read in a long time. I do not remember any discussion about which language I would be better off using in my fieldwork diary. Emine was keeping hers in English. There were surely problems with either approach, so the only remedy available to me was to write in English and keep a record of particular Turkish words. I did not know what emic words meant at the time anyway. Paul was encouraging me to keep a note of things I encountered while I was collecting demographic data, yet we both understood later that I could have pursued much more if I had had anthropological training. Emine’s presence was a source of considerable guidance, but our time together was very limited. I made household visits to migrant families in Kayseri and Ankara on my own.

<sup>7</sup> At the time the Turkish word I used would be *rahatsız edilmek* (disturbed, meaning sexually harassed verbally or bodily).



Marxist social scientist – which for at least half the class was the most desirable currency. The combination of manhood with anthropological knowledge and curiosity presented me with an inspiring picture. While I was aware of his interest in the larger issues that were important for his analysis, his interest in the micro must have given me the impression that methodologically he had a superior understanding of the motivations and reasoning behind various group formations and events. As a result, I found myself thinking “I want to know the mechanism of why villagers vote for Demirel, why they join Hak-İş.”<sup>8</sup> “I do not want to do research simply by giving people questionnaires.” I am still not sure whether equating sociology with questionnaires and anthropology with fieldwork was justifiable based on what I had learned at METU at that time. Even if it did not give me the answers I was seeking, fieldwork felt more satisfying.

Yet fieldwork was not always rosy, and when I encountered its thorny side Paul helped me to put things into perspective. When I was visiting various migrant households in 1986 I had come across some very friendly families; in a few cases, however, young men displayed an attitude that – while too subtle and non-specific to be defined, let alone charged, as sexual harassment even measured by current standards – made me feel very uneasy. I found myself going back to my hotel and showering to get rid of the stresses I had experienced from their gaze. When I told Paul about this mainly in the form of a complaint about disgusting young men, he was very apologetic since I had these unpleasant encounters because of his research. My intention, of course, was not to blame anybody other than those men. I was simply very content to be able to share my feelings openly. On another occasion when I was staying on my own in the Turan Hotel in Kayseri, my then fiancé who was doing his military service in Ankara came to visit me while he was staying in the military guest house (*Orduevi*). When he was in my room somebody was sent to warn us that as we were not married, we could not stay in the room and he needed to leave. Thanks to Paul’s firm support, I was able to tell them that he would leave when it suited us and they had no right to interfere, although I knew it was important to maintain good relations with the local interlocutors.

Having done fieldwork in a plastic factory I had a chance to engage some young female workers in conversations about their intimate encounters. I had not included them in my research paper and subsequently when I told him the stories that I had heard about their sex lives, Professor Stirling told me that it must be his fault not to have taught me that these were also an important part of my research, even though I was studying the women’s relationship to labour. I did not know how to make connections and generate significance from the statements made by those young women. Later, when I heard women in Elbaşı talking about their genitals as *am* just like they refer to their arm or leg, I told Paul that after hearing working-class girls talk and seeing the way women in the village were touching each other’s backs and saying “well, you had a bath so there must have been some action” I could see that the middle classes must be the most conservative when it comes to sex because they do not utter the word *am* unless it is used as a curse word. Without an anthropology education, I still did not know what to do with these insights or how I could contribute them to Paul’s research.

Paul’s most profound statement that always stayed with me was again a sign of his intellectual honesty. He had gone to the hospital to visit a woman who had given birth in Kayseri. She was a relative of one of his long-time Sakaltutan friends. He commented that the “idea of penis envy clearly must be invented by men.” The sense of satisfaction, accomplishment, pride and

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<sup>8</sup> Hak-İş, The Confederation of Turkish Real Trade Unions, was the first trade union federation to be overtly associated with political Islam.

ease on the face of the new mother when she was holding the baby had made him think that what men really had was birth envy. It is possible that I did not expect such an insight from him. Several years later during the EASA conference held in Oslo in 1994 we were talking about Ibsen and Dickens and I made reference to the atmosphere as “dark Dickensian misery times”, prompting him to remark that his grandmother (if I recall correctly) had never seen her husband naked. During the same conversation he also told me a true story conveyed to him by somebody close to him, in which a father gave a bag of coal to his kids and laughed at them when they found the coal instead of ... was it presents or potatoes, I am uncertain now. As his eyes teared up, I think it was only then that I could see some of his sensibilities as a man. It was not just the tears but the expression on his face that made me feel his anguish. Only then did I recognize the presence of his “bleeding heart”, as he himself had described it in his interview with David Shankland in 1996 (Shankland 1999).

Paul may have used the phrase “bleeding heart”, but I saw him as someone who cared: not in a charitable manner, but nevertheless he had a way of caring mixed with curiosity. This was evident in the way he created connections and facilitated study opportunities. For example, he encouraged me to attend a conference in London on the Caucasus organized by Tamara Dragadze, who introduced me to my future host family in Baku, a senior Soviet ethnographer Atiga Ismailova and her daughter Naza Agassi. I later introduced them to Lale Yalçın Heckmann and subsequently Bruce Grant, both of whom stayed with Atiga during their field trips. They became friends with each other and with Atiga. This genealogy would surely have never come about if Paul had not cared.

We know that anthropological knowledge production is shaped in part by the anthropologists whose individual stories shed some light on the conditions of its production. Stories of Paul, a caring, curious and honest human anthropologist, deserve to be told just as much as those of more “famous” academics, perhaps more so for this very reason.

## **References cited**

Hann, Chris. E. 1993. Culture and anti-culture: the spectre of orientalism in new anthropological writing on Turkey. In: *Journal of the Anthropological Society of Oxford* 24 (3): 223-243.

Shankland, David. 1999. An Interview with Professor Paul Stirling. In: *The Turkish Studies Association Bulletin* 23 (1): 1-23.

Stirling, Paul. 1965. *The Turkish Village*. London: Weidenfeld and Nicolson.

## Abdolvahap Taştan

### At The Field with Paul Stirling: Through the eyes of his *mihmandar*<sup>1</sup>

#### Introduction

Paul Stirling is known to be the first person to have done ethnographic field research in Turkey for his seminal work *Turkish Village* (1965), a classic in Turkish Anthropology. He was recognized in the relevant intellectual circles at the time, and while preparing for his research he took Turkish language lessons in Oxford from Professor Fahir İz (Stirling 1965: 3). In his article titled “İngiltere’de Antropolojinin Ellinci Yılı ve Türkiye (The Fiftieth Year of Anthropology in England and Turkey),” Z. F. Findıkoğlu (1965) stated that there were two social anthropologists in Turkey, one local—Mümtaz Turhan, and the other English—Paul Stirling. He particularly underlined that his *Turkish Village* was, indeed, Turkish, and that it would be beneficial to make use of young Paul Stirling in the foundation of anthropology in Turkish academia. Karpaz (1997: 260-262), in his review article about Stirling’s edited volume titled *Culture and Economy; Changes in Turkish Villages, 1993*, critically evaluated the basic dynamics of change and transformation in Turkish villages.

Frequently, the question was raised pondering why Paul Stirling chose this area as his research site where he did fieldwork in full participation for a total of thirteen months during 1949-52. When he decided on a village that was away from urban influences, where Sunni Muslims lived and spoke Turkish, probably he followed his professor Evans-Pritchard’s counsel about the significance of the tribal world of thought and the importance of studying small scale societies (Pals 2019: 433). Here, too, people who shared an agricultural culture were closed to the external world (Stirling, 1986: 76). Another reason he expressed for his choice, not entirely an academic one, was the fact that the area was close to Talas. This reasoning is understandable especially when we consider that the Talas Clinic, founded in 1880, was a medical center where a range of diseases were treated under the adverse circumstances of the time, and that the Talas American College (1889-1967) was conveniently located in the region (see Winkler 2015: 16). It is rumored that Talas was described as the “Versailles of Cappadocia” by western travelers in the 18<sup>th</sup> century, and another traveler, Childs, has called attention to the picturesque beauty of the roads and buildings in Talas as well as the settlements and ways of life exclusive to the diverse cultures in the region (Childs 2017: 175).

I daresay that Stirling was always aware of the sensitivity of this issue and that’s why he assiduously stated that his primary goal was academic and that he was committed to the principle of objectivity, when proposed to the University of Kent, where he was affiliated, his research project for the restudy of the Kayseri villages in 1986. He believed that it was possible to implement the principle of objectivity to a great extent in Social Sciences, and that this framework could be used for developing policies to contribute to the welfare of humanity

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<sup>1</sup> An earlier version of this article, upon which this translation is based, was published in Turkish (Taştan 2022).

(Stirling 1985). Regarding his own research, he was convinced that his work and the following publications could be utilized for Turkey's benefit.

After *Turkish Village* that presented Stirling's initial research, his publications primarily gravitated towards identifying the dynamics of social and cultural change in his research areas and discussing them in a theoretical framework. Accordingly, there were three basic goals in the 1986 research project: To contribute to the theoretical discussions about the transformation of rural societies, to test various theoretical propositions concerning the industrialization process, and to leave a set of historical data to be utilized by the researchers of future generations (İncirlioğlu 2016: 184-185). Stirling emphasized that analyzing social change involved a long duration and that conducting longitudinal research in any given narrow area would not be an easy task. According to him, changes in cognition and values were drastically more fundamental than any other change yet they were way more difficult to analyze. In addition to this knowledge based and epistemic changes, Paul analyzed other significant changes that were experienced in many areas of social life including those among and between families, women's position, and the transforming effects of the continued relations that international migrants had with their native villages. Village people by now had learned new skills within a larger social network, gained new work opportunities and formed new consumption habits. While all children were going to school, young adults had increasingly become literate. Thereby, social change continued as various domains influenced one another (compare Stirling 1965 and 1993).

As for the story of my association with this study; I was tasked by Erciyes University in 1986, to participate for about one year in Paul Stirling's anthropological field research in the Sakaltutan and Elbaşı villages of Kayseri, as an observer and a *mihmandar*. Being a *mihmandar* is defined, in general, as the assignment of guiding/ accompanying a foreigner, and it is compulsory even today for foreign researchers. It turned out later, as far as I could observe him, that Paul knew of the area much better than I did and certainly did not need my guidance, but anyway, I ended up participating in the process. Soon I realized that being a *mihmandar* was a serious task; it could have influenced the course and progress of the research. Especially ethnographic studies bring along, at the outset, a certain hardship. When Paul arrived in Turkey this time (his previous visit was in 1971) the bureaucratic process for obtaining his research permit took an unduly long time and the rumors that followed caused, in the early days of the research, suspicious attitudes in the villages towards Paul and his team, that in turn affected the study in a negative way for a length of time. It goes without saying that in this kind of research it is essential to gain and maintain the trust of the researched community. As İncirlioğlu (2016: 194) related, the *mihmandar* played a considerable role in this respect:

When our second *mihmandar* [who was then a graduate student and research assistant] now a professor of theology, started working with us, all tensions were released and we took a deep breath. This research assistant immediately grasped the value of ethnographic research and participant observation, moreover looked up to Paul Stirling, and identified himself as a student who would make the best of his position by learning from a competent and wise professor. Thus, unlike his predecessor, he not only did not hinder the research but in fact contributed to it as a voluntary assistant. During our stay in Elbaşı, he moved to the village with his wife and four-year-old son. They made a sacrifice from their established life in Kayseri and for one month we all lived in a largish village compound that consisted of two buildings in a spacious garden. Thanks to his personal, cultural and academic background, his perspective on the events we experienced in the village provided valuable support for the research.

## At the field

As a research assistant, those were the first years of my academic life. I was preparing my Master's Thesis on Nestorianism in the field of history of religions. When I was tasked to accompany Professor Stirling for the research he conducted in Kayseri, I had not given much thought on the extent of the duties a *mihmandar* was supposed to perform. I had a great interest in philosophy and sociology. As a matter of fact, soon after the research in 1986, as a consequence of my involvement with Stirling I was appointed to an open position in Sociology of Religion. My mind was set on doing my graduate studies abroad which I could not realize. That's why I considered working with Paul as a great opportunity and I thought I could use it to good account. For someone like me who was raised in a culture where traditional education meant bookishness and theoretical reading, to be "at the field" with Professor Stirling helped me gain significant life experiences. Identity and personality are usually confused and presented interchangeably. In Paul's person, I could observe the harmonious association of applied science and human dimension, and he was never pretentious in presenting this integrity. He had the desire to see that his work served a purpose, and frequently saying "whatever will happen to the state of humanity?" he would complain about the structural conditions that engendered all kinds of inequality. He was also quite punctual; he would determine our meeting time by calculating it to the minute. Once, we arranged a seminar in our faculty for him to give a talk and it turned out to be a very beneficial acquaintance meeting. From what I saw, he was a bit wary of theology as an outsider. After he met my PhD advisor Ünver Günay and examined his works, his bias turned into some sort of astonishment. My advisor had received his PhD from Sorbonne after studying under Jacques Berque's supervision. Paul said he knew J. Berque well. Later, during my own field research with my advisor in Kayseri, I always remembered Paul's principle that was instilled in my memory from those days: "to be skeptical in reaching true information and to be sensitive towards people."

Emine Onaran İncirlioğlu who was at the time a PhD student in the United States, was also there as Stirling's research associate. She would say that she took part in the research with the suggestion of her advisor in the States, in order to do her doctoral fieldwork under Stirling's supervision. Her research was on the role of gender in rural transformation in the same settlements with a particular focus on women. Although it was not easy in the beginning, she succeeded in adapting to village life in a short time. It was her first field research. Nevertheless, before long, the cultural and social capital she had access to, proved to be useful. Both her parents were medical doctors and she started to help downtrodden village families through them. She was particularly interested in Anış, a village woman with whom she continued to have contact long after the field research ended, and her family. Anış and Emine called one another "*bacım* [my sister]" and became even closer after Anış's daughter who had a serious medical condition was successfully treated in Ankara. This assistance brought not only cement but also trust and respectability to the researcher.

I would join them as the *mihmandar* and the three of us would go to Sakaltutan from Talas every day. Our first stop in Sakaltutan would be the house of Uncle Ali Osman who lived at the entrance of the village. Work would start along with all kinds of treatment – our hosts offering us food and drinks and conversations. Everything would proceed so naturally that one would not be able to say if this was work or an ordinary visit and friendly conversation. Paul knew Uncle Ali Osman and every single village man and woman their age since their youth,

and, in spite of the uneasiness during the first few days, villagers considered Paul to be one of them. Soon enough mutual trust was reestablished.

With each passing day I started to grasp the significance of participant observation and to see it as a pleasurable occupation. Being at the field seemed like enabling a new discovery at every turn. I also realized that the researcher had to be prudent all the time. One did not have to sweat in order to have empathy with the researched. Everything was in its natural environment, flowing in its usual homeliness. Stirling was interested in people's personal problems, too, and was making them feel that he would assist them as much as he possibly could. There were times, however, he pushed it a little too hard when it came to collecting information and to make use of a newly risen opportunity. In a funeral house, for example, it would be odd when he produced his pen and notebook while talking to people from other villages who were there to pay their condolences. In such situations, as a result of her cultural sensitivity, Emine would exhibit an intervening attitude. Once in a nearby village, when they were attending a wedding ceremony, Paul acted in a similar manner which, this time, was not regarded as strange. After all he was a researcher and it was understandable that he would mind his chances. As a result of his interrogating manners concerning the research field, he always reflected his efforts to reach invisible clues. According to him, the one word a researcher should use most frequently was "*Acaba?* [I wonder ...?]" Another feature I noted during the research was that Paul would place a voice recorder under his cushion – more often than not, by asking permission – probably because he was not quick enough to follow conversations and to take notes. He would say that these recordings helped a lot afterwards, during the analyses.

In spite of the complications that we experienced in participant observation at times, the research itself was conducted in a quite well-planned manner. The conversations we carried out throughout the day, the notes we had taken and the observations we had made would be recorded one by one in catalogues in the evening. The following day, we would start to work all over again but each day would feel like a new beginning and it would not tire out or bore us. Even in the life of this small village, the interviews conducted with each family would be recorded separately, as a different source of information. How the minute details would diversify the modes of relationship which looked alike at first glance! They also provided us with opportunities to understand and express the invisible networks of interdependency and solidarity in this seemingly ordinary village life, as well as villagers' coping skills and the basic values fostering them.

Paul conducted the research process like a maestro, with proficiency and courtesy. He esteemed the villagers to great effect and treated them as he was one of them to the extent that one would not feel that he was an outsider. Whomever you asked, they would adopt him, saying "*Bizim Pol* [Our Paul]." I suppose this was an unmatched compliment for an anthropologist, an ethnographer. Of course, they all knew that he was an Englishman – probably that was all they knew about him – and they somehow had this sense of privilege that *their* village was selected to be researched. Still, there were times we faced unwanted situations. Some would clash with Paul, exhibiting a suspicious attitude by criticizing him and his research. In those sporadic cases they would usually question why he had singled out their village to do his research, what would come out of this, and whether there was a hidden agenda behind it. Conversely, many thought Paul was "actually a good man" and some even said that, with all the good he had done for the villagers, he had to be a Muslim – even though he himself was not aware of it (İncirlioğlu 2016: 195). I daresay that this is in the nature of ethnography. Come to think of it, why would a man come all the way from England in the harsh conditions of the 1940s and 50s, and not on his own but with his wife, to endure an unfamiliar hardship? At a time when anthropology was

not yet recognized as a legitimate field even in academic circles, this line of thinking was only natural. Even now, aren't there people who would think in similar terms? Fortunately, now we encounter interpretations that appreciate the well-meaning scientific efforts in difficult circumstances, with the hope that they will expand over time.

Especially the Sakaltutan research was not restricted to those who currently lived within the village. Sakaltutan was a hometown of craftsmen, and part of the population lived outside the village as a result of both internal and external migration. Stirling would visit one by one and talk to those Sakaltutan migrants living both in Turkey and abroad, in order not only to ensure the comprehensiveness of the research and make comparisons, but also to get hold of the dynamics of change. Probably, he was also the only anthropologist to have researched the social structure, change and transformation in a Turkish village at this scale (1949-52, 1971, 1986). In the years 1981-82, Stirling had a BBC documentary produced in Sakaltutan, Adana and the Pforzheim town of Germany, called *Sakaltutan: A Time of Change* (İncirlioğlu, 2016: 184). He meticulously underlined how attitudes gained in the process of cultural interaction reflected in behavior, in a way by means of re-socialization especially through internal and external migration. Here the main subjects of change were family structure, kinship relations, and differentiation in inter-family roles. According to him, significant changes had taken place in both the domain of material culture and on the basis of attitudes and values. As discussed in his later work, Paul considered changes and transformations as they related to economic development and modernization, and he assessed their determining effects in other realms of life (Stirling 1993: 5-10). Paul regarded these changes as very important and sometimes he talked about them with expressions of surprise.

The second area of research was the Elbaşı village, administratively connected to the town of Bünyan. In both areas, Paul would select the house of one of the prominent men in the village as his "headquarters." He considered this as an opportunity to have more comfortable conversations with villagers from other households. In Sakaltutan it was Uncle Ali Osman's house and here, the house of Kara Osman, otherwise known as Kara Bey's Mansion – *Kara Bey Konağı*. As a comparatively wealthy and powerful man, Kara Bey was influential in the region. Because Kara Bey died before Stirling started working in Elbaşı, we maintained our connections through his sons and relatives. For Paul's fieldwork that led to the *Turkish Village*, using a "headquarters" had a functional utility in 1949 and 1950. In 1986, however, during the research that lasted for about eight months and that periodically included research assistants, it was more practical to rent an apartment in Talas for the Sakaltutan study, along with a car to commute. Since Elbaşı was considerably distant and daily trips were not suitable, we resided in a large village house that was rented for the duration of the research. As in Sakaltutan, in Elbaşı, too, we visited households and conducted interviews. Paul's visit in 1971 was a short one and he could not fully cover all of the households. So, the 1986 interviews included additional questions to complete the 1971 data.

The role of Elbaşı in the research seemed to be more of a comparative unit. Because it was a *nahiye*, a larger central village to which other villages were administratively attached, Elbaşı had stronger connections with both Bünyan and the many surrounding villages. According to Paul, Elbaşı was more sophisticated than Sakaltutan in the 1950s; the number of literate men was much higher, and among women, though fewer in number, there were those who could read and write. The fact that Elbaşı was relatively wealthier, more educated and more open to the external world, however, did not mean that the attitudes and values of the majority of the population were all that different from those of Sakaltutan. In fact, there were more divisions

among Elbaşı villagers that were based on animosity. From this point of view, the similarities between the two research areas were more prominent than the differences (Stirling 1965: 24).

## **In conclusion**

Whether qualitative or quantitative, each worthwhile research that is done right, is based on a wealth of knowledge and a theoretical background. Because of the nature of participant observation, the world view and concepts of the researched are at the forefront rather than those of the researcher that are more discernible in quantitative research. The researcher who participates and observes, tries to discover, with almost an intuitive dexterity, the invisible networks of meaning in the relations between institutional structures and behaviors. With his knowledge, experience and devotion to research, Paul Stirling, was, in Atay's (2016: 9) terms, both "an armchair anthropologist" and a fieldworker with "muddy shoes."

It is common knowledge that social researchers in general (and anthropologists in particular) and the communities they study come from different worlds, and it is safe to assume that, more often than not, researchers "study down." This may cause, at least during the early phases of fieldwork, an emotional and intellectual upheaval and bewilderment. Stirling used to talk about such an experience, when he and his wife came to Sakaltutan and Elbaşı in 1949. In the words of Claude Levi-Strauss (1994: 56), the ethnographer as a human being is affected by such experiences, often negatively. Personally, I have never witnessed any sign in Paul that would make me feel this during all the time I was working with him. On the contrary, I usually perceived an empathetic emotional bond between him and the community he was studying. But more important than that is his contribution to the accumulation of knowledge and theoretical discussions in his area, with the information he obtained from the field. Moreover, I would say that Stirling represents a historical personality who has witnessed Turkey's modernization adventure.

When all is said and done, Paul Stirling gave 45 years of his life to studying the "Turkish Village" and presenting a research model to those who will do this kind of work on understanding macro structures from micro structures.

In my teaching as an academic in Sociology of Religion when I suggest a village study to my students, from time to time one would say, "Come on teacher, whatever can come out of the village!" That's when I tell them about Paul Stirling.

## **References Cited**

Atay, Tayfun. 2016. Sunuş. Alanda Takım Çalışması: Yorumlama, Müzakere, Uzlaşma. In: Harmanşah, Rabia and Z. Nilüfer Nahya (ed.). *Etnografik Hikâyeler: Türkiye'de Alan araştırması Deneyimleri*. İstanbul: Metis yayıncılık, pp. 9-15.

Childs, William J. 2017. *Yürüyerek Anadolu: Samsun-Halep, 1911-1912*. Tayanç, Füsun and Tunç Tayanç (trans.). İstanbul: Kitap Yayınevi.



Findıkoğlu, Ziyaeddin. 1965 [2011]. İngiltere’de Sosyal Antropolojinin Ellinci yılı ve Türkiye. In: İstanbul Üniversitesi İktisat Fakültesi Mecmuası 25 (1-2).

İncirlioğlu, Emine Onaran. 2016. Alanda Takım Çalışması: Yorumlama, Müzakere, Uzlaşma. In: Harmanşah, Rabia and Z. Nilüfer Nahya (ed.). Etnografik Hikâyeler: Türkiye’de Alan araştırması Deneyimleri. İstanbul: Metis yayıncılık, pp. 182-198.

Karpat, Kemal. H. 1997. Stirling, Paul (ed.) Culture and Economy; Changes in Turkish Villages. The Eothen Press, 1993. In: Bibliotheca Orientalis LIV 1/2 (January-April): 260-262.

Levi-Strauss, Claude. 1994. Hüzünlü Dönenceler. Bozkurt, Ömer (trans.). İstanbul: Yapı Kredi Yayınları.

Pals, Daniel L. 2019. Toplumun ‘Kalbinin İnşası’: Edward Evan Evans-Pritchard. In: Akdağ, Ömer and Mustafa Ulu (trans.). Dokuz Din Kuramı. Kayseri: Kimlik Yayınları, pp. 391-437.

Stirling, Paul. 1965. Turkish Village. London: Weidenfeld and Nicholson.

Stirling, Paul. 1993. Introduction. Growth and changes: speed, scale, complexity. In: Stirling, Paul (ed.) Culture and Economy. Changes in Turkish villages. Huntingdon: Eothen Press, pp. 1-16.

Stirling, Paul. 1986. Cycle domestique et partage du pouvoir dans les villages turcs. In: Kayser, Bernard (ed.). Les Sociétés Rural de la Méditerranée, Edisud, pp. 76-85.

Taştan, Abdulvahap. 2022. Etnografik Bir Hikâye: Paul Stirling ve Türk Köyü. In: bilimname 48 (2): 111-125.

Winkler, Warren H. 2015 [2014]. İki doktor bir yolculuk. 2nd ed. Yalçın Varnalı (trans.). İstanbul: Yapı Kredi Yayınları.

Translated by Emine Onaran İncirlioğlu  
Proofread by Stuart Duckworth

## **Emine Onaran İncirlioğlu**

### **Paul Stirling, the man:**

### **Pink shirt, missing toe and deep wells of depression**

A Google search for Paul Stirling will give you hundreds and thousands of entries about an Irish cricketer who also has a detailed Wikipedia page. He was born on September 3<sup>rd</sup>, 1990 – a Virgo. Our Paul Stirling was an English professor of anthropology, a Libra, who died when the Irish cricketer was eight years old; and as of 2022 he was yet to have a Wikipedia page. *C'est la vie!*

I met Paul Stirling in the fall of 1985. If I were the spiritual kind, I could say that fate had woven its web for us to meet. I had just completed my Master's degree in Anthropology at the University of Florida and taken a couple of doctoral level courses when my supervisor, Professor Paul J. Magnarella, convinced me to continue for a PhD degree. Professor Magnarella thought it would be a good idea for me to study the Laz in the Black Sea region, and gave me two articles written by Russian social scientists, as there were no western sources about them.<sup>1</sup> Another professor at the department, Russ Bernard, however, was making the point that a student from Turkey did not have to do fieldwork in Turkey; I should rather study “the other” in other lands, as did European and American anthropologists. I was undecided. In the summer of 1985, I was taking a beginner's level Spanish course in Summer School, just in case I would do my dissertation fieldwork with Guatemalan refugees or Cuban professionals in Florida.

That summer, my parents were involved in a car accident – “no one would come out alive from that totally wrecked car” eyewitnesses said – and to be sure, my father was seriously injured. One complication followed another. He was hospitalized for a month and had a series of surgeries. His condition was so serious that no one noticed my mother's broken ribs. Although with my tight student budget, I had no intention to travel to Turkey that summer, I decided on impulse to fly to Ankara, abandoning my Spanish class before mid-term, to see my parents. After a long and adventurous flight via Vienna – that I won't go into detail, as I can hear Paul's voice saying the reader does not need any of this – I was at home. When the phone rang one day and Professor Magnarella called, I was keeping my father company while he was doing his physiotherapy exercises in his orthopedic hospital bed that was placed in the middle of the living room. Magnarella was spending a few days in Ankara and he asked me if I would go to the Middle East Technical University with him – my former university where I had received my Bachelor's Degree in Architecture before I shifted to anthropology. I agreed. A friend who worked at METU drove us there. While Magnarella went off to the Sociology Department, I visited old friends in various departments, and at lunch time went to the Humanities Faculty, to meet Magnarella. There was a surprise, however. Paul Stirling, who incidentally was a

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<sup>1</sup> Although Michael Meeker “did his PhD fieldwork in the town of Of, Turkey, in the late 1960s, he never published his dissertation and did not study Turkey for the following two decades until he resumed his research on the Black Sea Coast in the late 1980s, this time with a historical focus.” (Sipahi 2021: 100) And it was not clear whether or not his article on “The Black Sea Turks” (Meeker 1971) was about the Laz.

visiting professor in sociology, had invited Magnarella for lunch, and since I happened to be there, they politely asked me, too, to join them. Stirling took us to the faculty cafeteria where he said food was not boring. During lunch, two well-known professors of Turkish anthropology, sitting across from each other, were talking about serious stuff without including me in the conversation and I was trying to catch what they were talking about while quietly eating my food. I distinctly remember feeling out of place and redundant.

Paul Stirling started to talk about the problems he was facing restudying the Turkish Village. He wanted to collaborate with a female Turkish social scientist during the fieldwork because otherwise, he thought, he would not have access to information from village women. Unfortunately, his contact with an established senior academic did not work, and a brief trial with a junior anthropologist proved to be “disastrous” – in his words – when she tried to “develop and modernize” the villagers. Would Magnarella know of a woman anthropologist who would collaborate with him? At that moment Magnarella pointed at me, and Stirling who was sitting next to me turned over and looked at my face for the first time. Magnarella said I could use the fieldwork data for my dissertation. “But I am not ready for fieldwork,” I said, “I have not completed my coursework yet.” Magnarella said it could be arranged. We went back to the Humanities building, to Stirling’s office, where we exchanged telephone numbers and Paul invited me to his apartment to talk about his project. That was it. I was to work with the first foreign anthropologist to have done fieldwork in Turkey, the author of *Turkish Village*, the famous, first proper ethnography in Anatolia. Like so many good things that have happened to me, meeting Paul was quite accidental, without my doing. Synchronicity.

Since we met in the fall of 1985 we kept in touch until his death in 1998.<sup>2</sup> Now that I think about it our fieldwork in 1986 had two layers: In addition to the intended research that we conducted in the villages, we also ended up with an unintended “participant observation” by sharing daily life in Talas and Elbaşı. During those eight months, Paul and I had the opportunity to learn about one another, experience personal and cultural clash, and accept each other’s peculiarities. Later in June 1989 we briefly visited Sakaltutan and Elbaşı, as well as migrant households in Antalya. In 1991, Paul participated in my doctoral defense at the University of Florida as an additional member of my jury, my “external examiner.” And during the 1993-94 academic year, when I worked as a temporary lecturer at the University of Kent – of course with Paul’s initiative – we worked on our database and wrote a joint article about kinship (1996). It was a most pleasant experience to sit side by side, forming sentences together and having everlasting discussions about which word to pick in a certain context. In the summer of 1998 Paul invited me to the University of Kent to work on the database with him, arranged for my accommodation, and had my name listed on the payroll. I took leave for the summer from Bilkent University in Ankara where I had started working since my return from the States in 1997. As I was preparing to go to England, my husband was diagnosed with an advanced, metastatic cancer and I called Paul to let him know that my plans had to change. That evening he had a heart attack. From his hospital bed, he told his son Nick to call me and say that he would have to postpone the project anyway since the doctors would not let him work for at least six months. During that time, I should take care of my husband, he said, and not worry about the database – he was comforting me. But when Nick called me the following day, he had to give the news of Paul’s death. There are no words to describe how my world came

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<sup>2</sup> We corresponded during the 13 years I have known Paul Stirling. Although I briefly mention his letters here, a content and style analysis of them may yield a separate article.

crashing down on me. My second and last visit to Canterbury was to attend his memorial service where I gave a brief talk “from the heart.”

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Paul Stirling disliked boring clothes just as much as boring food. He said he was envious of women because it was socially acceptable for them to wear colorful outfits while men had to settle with boring clothes in boring colors. He did own, nevertheless, a short-sleeved pink shirt, made of a wash-and-go fabric that didn't need ironing. Zeliha and Zarife, our mother daughter team of housekeepers in the Talas apartment, washed his pink shirt every week because he loved wearing it. Another significant feature of my image of Paul Stirling is his missing toe. In the place of one of his big toes was a conspicuous void. He told me that a hand grenade, falling close to him during the war, had blown off that toe. I never knew if that was the true story or if he was pulling my leg. And the third fragment of the title, “deep wells of depression”? Just so. This writing is mostly about it.<sup>3</sup>

## Fieldnotes

Part of Paul's fieldnotes are accessible online.<sup>4</sup> While working on the present text, I reread them after 35 years. Some of his fieldnotes gave me insights about future research, especially on what I called “ethno-philosophy” thinking that I coined the term, to later discover that it already exists. Once for example, he suggested an “ethnography of truth,” a concept that always preoccupied him, anyway. Similarly, following his fieldnotes, one might focus on an “ethnography of purpose and meaning.”

{A problem} What is interesting is that lots of people are intensely interested in ... details and seem to be worried not about the beliefs of other people but about the precise truth. They want the words to be “true” in a simple literal way. And simply to say different people have different beliefs does not satisfy them. This raises for me very fundamental questions about what one might call the ethnography of truth.

Interesting problem of ‘method.’ The villagers were and are relatively accepting. If they resist, it is for personal reasons about tax and danger of too much information getting round. But once the informants are educated and sophisticated, they have complex models about agents, enemies of Turkey and so on, but absolutely no concept of ‘objective’ social science.

Yet, I'll leave aside the “academic stuff” that I have traced in the fieldnotes, since my intention is to write about Paul, the man. His humorous language carried me away to old memories, and as I remembered the events mentioned – that I had forgotten, erased from my memory –

<sup>3</sup> Two points to clarify for the critical reader. One, “depression,” of course, is a loaded word, having multiple meanings ranging from daily household use for “feelings of sadness and hopelessness” to a technical term for a severe clinical disorder. Not being qualified to diagnose a clinical disorder, I am using the word here in a loose sense. Two, Paul and I both used the term in our conversations and correspondence for describing our emotional states; I am not the only one using it.

<sup>4</sup> A selection of fieldnotes by Paul Stirling and Margaret Stirling taken 1949-1986. <https://era.anthropology.ac.uk/Era/Resources/Era/Stirling/Fieldnotes/index.html>. All quotations from his fieldnotes in this text are taken from this website.

sometimes the encoded passages gave me a smile, sometimes I found myself laughing out loud, and sometimes I couldn't help the tears in my eyes. In any case, they opened up a world to write about Paul, the man.

When an older man known as Deli Mehmet visited us one late evening in our Elbaşı house, uninvited, just to be friendly, bringing a bucket of yogurt, I remember Paul, exhausted after a long day, hardly keeping his head up, while I was talking to him and taking notes. Later in his fieldnotes, Paul wrote: "Mehmet ... talked solidly for 2½ hours about everything – marriage, morals, nations, language, Europe, prison, smuggling, brothers." He had indeed. While I thought Paul had fallen asleep, he was apparently listening. His colleagues and students will remember that he did the same during the seminars at the University of Kent. He would close his eyes, and while everyone thought he was dozing off he would listen to the conversations intently. At the end of the talk, he would open his eyes and right away, ask a poignant question.

Every night when Paul wrote down his fieldnotes – which he almost never neglected following his professor Evans-Pritchard's advice – he made a point of recording his evaluation of the day's fieldwork, and sometimes, his emotional state. "A day of achievement" he would write, "a good interview" ... "another satisfactory interview" ... But usually, he was not gentle with himself during fieldwork and elsewhere. I did not read his diaries – they were private, I don't know where they are now – but he was probably more ruthless towards himself than he was in his fieldnotes. The phrase "chronic remorse" comes to my mind when I see his self-critical fieldnotes and his constant interrogation of himself, frequently questioning his wrongdoings and frequently worrying about the consequences of his / our behavior towards the informants. Some days were, obviously, more productive than others. Yet, he would beat himself up for any shortcoming he might have. When he did not find the time to write up his fieldnotes, for example, he would go into mourning. And wasting time was a major concern for him.

Yet another major failure to write up. No notes since 9<sup>th</sup>

... vast amount to remember. Impossible

As usual, information wasted. I am now off to Elbaşı and shall forget all the ... gems.

... embarrassed - had work to do. Very edgy.

Wished we could have got him on tape but foolishly I was worried about confusing the tapes.

I sat in the corner. I was not concentrating nor taking notes. [I am now a bad field worker. Do as I say, not as I do.] Disastrous.

They wanted photos. We did not respond. ... They pressed us to supper and then to promise for tomorrow. We refused. (Offence given?).

Late brief, T.V. dominated visit. Useless.

Disoriented - wrong trip. ...

Paul particularly noted about economic hardship, poverty and desperation in the villages. Examples abound. I somehow associated the kind of data he gathered, with his depression and general concern.

A poor man - no TV. Said he planted 25 decares of wheat and 10 decares of barley. 25 decares of wheat yield 405 şinik [about 8 kilos] and 10 decares of barley yield 350 şinik. All this is needed for seed and house. Next year - only 12 decares. How will he pay debts? *Hak* [God] and *sıvacılık* [plastering in constructions] by his children. The 3 years of failure have been appalling for him.

[He] seems even worse off. No way to meet debt. Used to live off land and sheep. Forced to sell sheep because straw is very dear and needed cash to live. So, no sheep and debts - no money to rebuild his flock. "These three years *mahvetti beni* [devastated me]."

The scale of the research was itself stressful. It was not only a longitudinal project, tracing the descendants of people from Paul's initial research, stretching from 1949 to the 90s, but also a large-scale one with assistants, Mehmet Arıkan and Hülya Demirdirek in addition to myself, in multiple locations. In addition to the villages of Sakaltutan and Elbaşı, the research included towns, both within and outside Turkey, that were immigrant destinations from these original villages. Moreover, we visited nearby villages, especially those that were the birth villages of women who had moved out after marrying Sakaltutan and Elbaşı men. Feeling "out of control" was inevitable, especially when native Turkish speakers were getting carried away. In one entry in his fieldnotes, Paul wrote, "as usual, with ... Mehmet present, a lot of the conversation was too fast for me to follow," and elsewhere, "Mehmet was collecting secondary interviews and making notes, which makes it hard for me to make notes and collect details. Turkish conversation hard to follow exactly."

As Paul had designed and planned it, the fieldwork at least in 1986 when I was there, was an open invitation for depression. There were many frustrations. In July, we had to find a place in Elbaşı to stay in the village, since, unlike Sakaltutan, it was not close enough to commute from the nearest town. The owners of the only empty house had emigrated to Kayseri but the power was cut off because the electricity bills were not paid since they had left years ago. The owner was not an unreasonable man and he did not want to charge a high rent but he was not willing to pay the accumulated electricity bill for which he would have no use. If we wanted to live in that house, we had to pay the bill along with the added fine to have the power reconnected. Without an alternative option Paul agreed to make this payment, definitely enormous for village standards although not forbiddingly high for him, compared to utilities in the UK. Feeling frustrated and feeling somewhat taken advantage of, he wrote in his fieldnotes that he was "publicly isolated." More frustration followed the first day, when the power was not immediately connected and without electricity, we could neither conduct our interviews nor write up in the dark. It took TEK, the electric company, several days to arrive during which we borrowed electricity from a neighbor via an extension cable.

Paul had a vast capacity for empathy. He was sensitive to the emotions of the informants who had a range of problems. I have personally witnessed so many times how Paul was genuinely concerned about many villagers and migrants, and how he would ache for them, whether he could be of any assistance to them or not. His keen awareness of the hardship and helplessness experienced by an alcoholic village migrant in Adana, for example, would find an emphatic place in his fieldnotes along with the difficulties suffered by his family members:

Terrible fate ... to be landed with such a man. He is charming, open, intelligent, but plainly a failure - lots of beginnings; alcohol...

Since a major focus of the field research was rural transformation, the keywords included land division, agricultural economics, inheritance... obviously there were misleading and at times less than honest answers to his questions when there were unfair dealings in the family. More concern for Paul to be depressed and give away to despair. He was usually uncomfortable on money matters. Every now and then he would provide financial assistance to especially poor villagers, but he did not want to create the impression that he was a rich European ladling out money. All sorts of economic problems faced by villagers, to which he could not turn his back, were stressful situations that contributed to his depression. He would give them cash at times, although he was aware that he could not eliminate their poverty by handing out money. He also felt like he was desperately caught between a rock and a hard place because there was always the possibility that a few villagers were taking advantage of him by exaggerating their situation – incurable health conditions, a bad harvest, unemployment, and so on – in the hope of receiving some cash. Towards the end of August 1986, after I had already left Talas, when he was vacating the apartment that had served as our headquarters, he sold some of the furniture and household goods, and gave away some, to those villagers who needed them. (As a memory of our fieldwork, he saved a couple of plates for me which I still use.) He noted how agonizing the experience was for him:

... I feel I behaved badly. They came to fetch fridge and a desk. No desk left - Emir cross. Anis all right. I took TL 40,000 and gave her the *tüp* [gas bottle] and *ocak* [stove] free. But I was unhappy.

A last remark about Paul's fieldnotes: I feel flattered by some. At this age, whenever I read a fieldnote entry where Paul wrote, for example, "Emine conducted a very friendly but toughly systematic interview, 3 hours - very good;" or when he noted that an informant preferred to talk with me rather than him, I still feel happy that my work had pleased him. On the contrary, when he wrote "a major balls up," after an interview I did with Deli Ağa, I was saddened. I know he was given to hyperbole at times, and it is impossible to understand what he meant in his fieldnotes as I cannot remember the exact events after 35 years. Still, the frustration that I might have caused a serious problem takes the best of me.

## Fieldwork in Fieldwork

Initially, I had thought of myself as Paul Stirling's assistant and him as my boss, and when he talked and wrote about me as his "research collaborator," I considered it as his "gesture," nice but not real. As the research proceeded, however, we started to make joint decisions as equals and I started to feel like Paul's was not a mere gesture and ours was really a collaborative "team" fieldwork. I read this again between the lines in his fieldnotes: "We decided, Emine and I, to make a tape-recording instead of writing it all out." In rural villages where neither of us could share much with the locals, I remember beautiful, unique moments now with Paul when we could share our stories and dreams, bitter and sweet memories, deepest troubles and enthusiasm, intimate secrets.

And we belonged to different worlds. I was a smoker at the time, and Paul was not. He said he wasn't hooked up to smoking because he was too poor, growing up, to buy cigarettes. The

smell of tobacco and the smoke in the apartment must have disturbed him, as it would bother me now since I have quit smoking, but he never complained about it. Our “inner times” were also different: He was an early riser while I would stay up late, writing up my fieldnotes, and get up late. He did say a few times that he’d prefer if we operated in the same time zone so that we could coordinate better, but of course it did not work. Some mornings when I slept later than usual, he would bring a cup of tea and leave it on my nightstand to politely remind me that it was time to get going.

A picture that comes to my mind now: 1986 Talas, Kayseri. A freezing snowy day in mid-February. On our way back home from Sakaltutan, walking from the car Paul parked in front of our apartment building, to the entrance that was at the back, I remember the snow that came up to my waist. I walked straight into my room to change into dry clothes while Paul was lighting the stove. There were two stoves in the apartment. One in the living room, the other in his bedroom. I didn’t know how to light them. It was Paul’s job. I walked into the living room rubbing a warm shawl around my shoulders, still chilled to the marrow, to see the two windows and the balcony door wide open. Proud of his English heritage that advised him to air the house every now and then, Paul was ventilating the room while the firewood was crunching and the poor little stove was struggling to warm up the place. “Paul! What are you doing?!” I asked, shivering. He looked at me, startled but amused, and later that evening told me that I was the only person other than her mother, who talked to him in *that* tone.

Paul and I had frequent arguments. During the fieldwork and afterwards, I always felt comfortable, despite his seniority and our asymmetrical relationship, to express my opinion openly and to oppose when I felt it necessary. This permissiveness was part of the beauty of our relationship. Our main disagreement was over the matter of equality, mostly about but not restricted to gender related subjects. I was young and self-righteous and quick to react fiercely at every opportunity saying that he was being “sexist,” sometimes overtly sometimes in a subtle way. When for example, in a household we visited, he jotted down “all female” – as women who befriended me would invite us for tea, *ayran* or lunch – while not making a symmetrical remark “all male,” assuming it “normal” when everyone in the room were men. Throughout the research period especially in 1986 we had numerous discussions pertaining to his “male bias” in fieldwork. I wrote elsewhere (1994) that I was self-righteous and felt the liberty to criticize him. I think he did accept, at some point during our stay in Talas, that village women were neither as docile nor as difficult to communicate with as he initially thought they were. More and more he began to notice women and to make notes when they approached to talk with him. “Striking that the bunch of women firmly encouraged me in after I had left the men” he wrote on one occasion. He was positive that women had changed a lot since the 1950s and a possible bias that he might have had then had not led to any misconceptions about them. In any case, the mere fact that we could have egalitarian discussions about his filters that might have distorted his observations was an amazing revelation for me about the possibility of team fieldwork to complement one another in spite of differences in age and experience. In any case, I would be quick to criticize him on occasions when, as a youth in her twenties who thought she knew the best of everything, I decided he was culturally insensitive; but I do not remember a single instance when he was offensive or hurtful to me. Nevertheless, we would joke about all kinds of -isms: I would threaten him that I would be “ageist” towards him whenever I caught his sexism. Then during our correspondence, he continued with these jokes: When I was pregnant in 1987, complaining that I was suffering from pregnancy symptoms like nausea and fatigue, he consoled me by threatening that he would be “pregnancist.”



My concerns about the “male bias” in the research had started at the beginning of the fieldwork. Paul had designed the longitudinal 1986 research to follow only the families in patrilineages. This annoyed me but he, probably rightly, argued that there was no way to follow all the descendants of the villagers who lived in 1949-52. By the time I participated in the research, it was too late to change that frame. While the matriline of the original population would not be systematically included in the follow-up, however, the research had no limits. Paul would make use of “chance encounters” and interview total strangers, visitors, and people whom we happened to run into, for example in a wedding or a funeral. Of course, some coincidences would yield invaluable information but more often than not, partly his inconsistency and partly wasting time in our already ambitious project would also annoy me. Sometimes, an overgeneralization he would make in passing, especially when I had a gut feeling that it was dubious but did not have the knowledge nor tools to argue against him – like, for example, when he said that Christianity involved a lot of love discourse while there would be more focus in Islamic discourse on anger, hatred, revenge and war – I would be desperately furious and go into a losing battle with him. Later, when I was teaching as a temporary lecturer at the University of Kent, I was similarly upset to learn that successful students, rather than the work they produced, were ranked “first class.”

We had a fiduciary relationship with Paul and I almost breached his confidence. When I received a small research grant from the University of Kent in 1994 and interviewed Margaret Stirling about her involvement with the *Turkish Village*, I thought my intention was clear for both of them. I was going to focus on, reveal and publicize (from the feminist perspective I had) Margaret’s –rather invisible– contributions to the 1949-51 research. “Women’s perspective” was discussed about in the relevant academic circles when I was in graduate school and I had read about the work of other anthropologist couples. Margaret and I met several times at the University, had lunch together, I asked quite a few questions to her about the research process and she told me many stories about her experiences and impressions in Sakaltutan, as well as the effects of Paul’s research on their family life. When I went “home” to Salt Lake City that year, I worked on these interviews when I found time between my teaching at the University of Utah, and finally completed an article to send to the *Signs Journal of Women in Culture and Society*. As a young aspiring scholar in a competitive academic environment, I was looking forward to getting the article published. As a principle ethical code of conduct, I simultaneously sent it to Paul and Margaret in Rochester, to both check the accuracy of the information I had used, and to receive their consent. I was at a loss when I received their letters. Although my whole aim was to address the gendered nature of research and researchers, and I had no intention to personally attack Paul, I had disappointed him. He somehow took it as my betrayal of his trust; and Margaret wrote to me very clearly that the *Turkish Village* was the work of Paul’s life and she would never let anyone compromise or overshadow it, and that our conversations were in an intimate friendly ambience and not for publication purposes. I immediately withdrew the article from *Signs* and wrote back to the Stirlings, saying that hurting them would be the last thing I wanted to do. We never talked about it again.

Paul and I were usually, but not always alone in the Talas apartment. Mehmet Arıkan, his long-term assistant from METU, had also a room there, where he stayed occasionally. In Elbaşı Hülya joined us, and Paul’s entourage expanded. On August 4<sup>th</sup> 1986, she shows up for the first time in the fieldnotes. We resembled the multi-family households we researched—an extended family living in a compound, with Paul’s research “guard” (*mihmandar*) Vahap, his wife Methiye and their son Hakan, in addition to Hülya and myself, under our household head, Paul. There were two buildings in our compound. Paul stayed in the main house, where also

the kitchen was located, while the rest of us in the guest house where there were two large rooms. Hülya and I shared one of the rooms, and actually slept on the two ends of the same long divan, and Vahap's family shared the other guest room.

With Hülya, who also has an article in this volume, we are still in touch, reminiscing fondly about our days in Sakaltutan and Elbaşı, but Mehmet died at age 32, soon after our fieldwork.<sup>5</sup> He was an ambitious MA student, writing a thesis on labor migration and construction workers from Sakaltutan. Paul liked Mehmet –wrote me once that he was a “pleasant person to work with”– and after the Kayseri fieldwork, he supported Mehmet also at the University of Kent where he was doing post-graduate work. They certainly had a close relationship. When we shared the Talas apartment in 1986, Mehmet told me once, that, in a hotel room one morning, where he and Paul were staying during a follow up study of labor migrants from Sakaltutan, he opened his eyes to see Paul stark naked, doing his morning exercises. I remember how funny we thought this story was, yet it was totally Paul-like. I frequently remember another Paul-like behavior that had to do with Mehmet. Since Mehmet's mother lived in his hometown İzmir and he, being a student at METU, still rented an apartment in Ankara, his transportation and accommodation expenses were much higher than mine. Whenever Paul gave a raise to Mehmet to cover his travel costs between İzmir, Ankara and Kayseri, he increased my wages, too, for the sake of maintaining equality between us. His sense of impartiality and justice has always worked in my favor, without my involvement or request.

## Deep Wells of Depression

When I reminisce about my undergraduate years at the School of Architecture, particularly when I think about the so called “critiques” we received from our professors during the design process and the “jury system” in the evaluations, I have the urge to use words and phrases such as agony, misery, devastation, loss of self-confidence, depression, post-traumatic stress disorder... I have to push myself hard to remember pleasant moments and constructive criticism. There is no place in my narratives for words like joy, purpose, affection or tenderness. If I had the slightest entrepreneurial spirit, I would have designed and marketed a t-shirt, inscribed “I survived School of Architecture” on it.<sup>6</sup> Against that backdrop, imagine how elated I was when Paul told me one day that I was “born anthropologist.”

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<sup>5</sup> Mehmet Arıkan was born in 1960 and died in 1992, during a trip from England to Ankara, a day after he was diagnosed with leukemia. His father had died during surgery when Mehmet was a small child, he was a hypochondriac since then, and he probably died of a heart attack upon learning his diagnosis. When he studied sociology at METU, he was a few years older than his classmates since he had studied an additional three years at the conservatoire before he started secondary school. He was married in England shortly before his death. I am grateful to his old friend Zeynepcan Erk Sönmez from whom I have learned some of these details about Mehmet's life.

<sup>6</sup> After I wrote this confessional paragraph, I came across Aykaç's (2021) thought provoking article about architectural education where, following bell hooks, she suggests that critical thinking in studio education may better be motivated by trying to understand the students' journey and to imagine the kinds of discriminatory processes they go through before their architectural training. She calls attention to the “elitist” tendencies in her experience of architectural education and the likelihood of reproducing the discrimination of students based on their gender, ethnic, political, religious and class identities, or as a result of their background in a “traditional” small city. I would second Aykaç's views, although I feel the urge to note the danger of falling into the trap of reification and essentialist identity politics.

At age 29, I was thinking that my depressive moods would change as I completed my doctoral dissertation, secured a teaching position and established myself. Soon after our fieldwork started in 1986, however, and soon after I started recognizing Paul as a person, and his depressive swings, I saw an older, British, male version of myself in him. I remember feeling hopeless of my future while thinking, “How can one be depressed, if he is an established, Emeritus Professor of Anthropology, a man in a male dominated academia, an heir of the British Empire in a Turkish village?” I didn’t know any better. He made me realize that my depression would have to be a way of life; I could have redefined it as sensitivity and sadness, and a call for “doing something” for betterment, but I had no way of getting rid of it. Paul carried the weight of the world on his shoulders. He expected from himself the impossible, and when he could not deliver what could not be delivered, he felt like a “fraud” – what some would now call the “impostor syndrome.” His feeling of inadequacy in the face of inequality on the planet made him feel hopeless and helpless. When Paul was in a better mood, he would argue realistically that there was, of course, no way for a complete solution for all the world problems but there was always room for “betterment” – hence his attempts in Applied Anthropology – but when he was in the depths of depression nothing would work. In any case he would always look down on “do-gooders” who assumed that they could come up with a quick fix.

One day in late Spring, early Summer of 1986, after a long day in Sakaltutan, we had our dinner and the usual briefing back in our Talas apartment, when I had this craving for a cone of ice cream. The *pastane* (café) in Kayseri would be open until 11 pm and I could not resist the urge to drive there and treat myself. Paul didn’t make much sense about this late-night adventure and tried to put me off but I was determined to go to the *pastane* where I enjoyed my ice cream at one of the sidewalk tables shortly before they closed down for the day. I didn’t think much about it and had not noticed that Paul had made a mental note about it. Not until years later in Canterbury. During the 1993-94 term at the University of Kent, Jack Goody gave a talk at one of the famous weekly seminars organized by the Anthropology Department. It was a pleasant occasion and, as far as I was concerned, there was nothing unusual. Paul seemed quiet after the seminar, though; tired, sad or upset. When he said he didn’t feel right and wanted to drive back home to Rochester, immediately, I insisted that he should stay a little longer, that we could go to the pub and talk about what was bothering him. Maybe for the first time he bluntly shut me up. “Don’t insist,” he said. “Sometimes people want to be alone, just like they sometimes want to have ice cream in the middle of the night.” I was startled that he would put together the two seemingly irrelevant events 8 years apart. He left and we never talked about that evening again but I narrated his story for myself. There must be some truth to it.

Jack Goody (1919-2015) and Paul Stirling were contemporaries. Paul was one year younger than Goody. I don’t know anything about their relationship, but that evening, something was triggered, whatever it was, that hurt Paul deeply. Goody was not yet granted the use of the title “Sir,”<sup>7</sup> but he was definitely more famous and, if publications are a measure of success, more successful than Paul. I do not remember much about Goody’s seminar that evening except that he had a larger theoretical perspective beyond empirical, ethnographic descriptions and an ambitious embrace. Paul, on the other hand, was neither engaged to any school of thought nor adopted any macro theory; he was an obstinate objector to any generalized statement. But that evening, I had the feeling that he compared himself to this bright celebrity and thought that he had not done much to show with his life, that he had wasted it.

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<sup>7</sup> Wikipedia. In the 2005 *Queen’s Birthday Honours*, he was appointed a *Knight Bachelor* “for services to Social Anthropology”, and therefore granted the use of the title *sir*.

Much later I found notes that I had taken on 11 June 1986, quoting him, catastrophizing:

I wish I wasn't what I am. I don't have stamina, I don't read fast enough, I don't know enough, a lot of my colleagues do better. I haven't produced anything.

I don't think I am a good anthropologist; I just withdraw to not listening.

I feel a failure intellectually and morally.

When I recently listened to a popular psychology podcast about the so-called impostor syndrome,<sup>8</sup> I immediately thought of Paul. The description of the syndrome, or the “phenomenon” as some prefer to de-pathologize it, seemed to agree with Paul's relentless, self-critical personality, and the “tormented Puritan” one could see in him from a certain angle, as Chris Hann wrote in his obituary. In any case, he “was notoriously slow to publish and prone to self-doubt, even public self-abasement” (Hann 1998). I suppose I have associated his career path with this inclination. He did tell me how he had persisted for two years to get into Oxford to study philosophy and was accepted only after two unsuccessful attempts; that he considered his employment in London School of Economics and later in Kent as a gradual decline, “stepping down” from Oxford, and that at times he would feel like a “fraudulent.” He was constantly questioning – himself and everything else. Paul made life difficult for himself. I know this because it takes one to know one – we were quite similar at that – and as I am getting older now, I even better understand him. He questioned himself all the time: What may be some possible misunderstandings, where he might get anything wrong. One will see many fieldnote entries like, “It seemed completely pointless to me, but maybe not.” An exhausting existence. Yet, I always saw the self-doubt in him as “a good thing” – an indicator of his honesty, hard work and never-ending search for truth.

Trying to “understand,” itself, may be a non-ending depressive endeavor. How long should I further be interrogating people and myself, before I am satisfied with the explanations? When do I reach the bottom of things? Paul would write, “we could do with much more information about what really is going on.” He thought about every possibility he could imagine, would never rush into a conclusion, would not generalize based on previous knowledge but would usually keep an open mind about alternative explanations.

Later on, I came to think that ethnographic fieldwork is not all that compatible with a depression-prone personality – if depression is a personality trait. It involves a lot of tension, most of which is related to power struggle, in a place where you do not belong. When the “locals” are reluctant to answer your questions and are suspicious of you when you take notes, you end up questioning your presence there even if you do not tend to be depressed. We frequently visited two households in Sakaltutan where we felt at home. Paul's old friend Ali Osman's and my friend Aniş's. They were “safe” spaces where we knew we were accepted without doubt. Yet, people who question their “belonging” even to their birth places, and who suffer from communication even among their own “lot” must have a harder time in places where they are transported temporarily. But then, people are people. If communication and belonging have always been hard, what difference does it make to relate to familiar faces and to total strangers, say in rural Anatolia? As a novice, I was faced with these dilemmas when I

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<sup>8</sup> How to overcome feeling like an impostor, with Lisa Orbé-Austin, PhD, and Kevin Cokley, PhD Speaking of Psychology.

was observing Paul while doing team-fieldwork with him in the two Central Anatolian villages in 1986 and during the follow up visits in the '90s.

Until I read his fieldnotes, however, I was not aware that villagers, including his closest acquaintances, pressured Paul to convert to Islam, and increased his distress.

The *Hoca* began by a talk ... about *Bayram* and the Sacrifice ... Then two *rekat*. Then he mounted the *mimber* and gave a talk in Turkish exhorting love and peace etc. Then ... in Arabic. As he came down, another voice from the front row took over - very musical. Then they broke up and the young rushed for the exit. The old and some middle aged stayed ... [to exchange *bayram* greetings]. I stood still and greeted everyone I could.... Murat Işık came to me and ordered me to repeat some Arabic. I said I simply could not. ... This went on for some time, then he started saying what I had to repeat phrase by phrase, so I did as I was told. When I finished, I realised that many of the others were including these phrases in their greetings, and one or two ... Murat had of course gathered an audience by his noisy insistence - used them again in greeting me. ... [Ali Osman] on the way back to his house said that anyone who said what ... Murat had forced me to say thus became a Muslim; he then once again lectured me on the importance to my eternal welfare of becoming a Muslim. He was quite serious, but I simply kept quiet.

I knew to some extent that, partly with the difficulty of obtaining a research permit from the Turkish government, and the gendarmes coming to the village to ask about him during the process but also partly because of the villagers' own conspiracy theories, they were suspicious of Paul's presence in the village. His notes indicate how troubled he must have felt when he was faced with such remarks. An entry about a Sakaltutan man is just one example:

Full of love and admiration but several times expressed anti-left and nationalist idea that my research was 'against' Turkey. Something of an enquiry? However, he did cooperate. The fact that he voiced this suspicion is interesting. Tip of an iceberg?

Paul had a tendency to see the empty half of the glass, although he did make an effort to be realistic and to present well-rounded arguments during our conversations – our briefing sessions – at home, going over our day in the village. Three days after we settled in Elbaşı, he wrote in his fieldnotes, "Saturday, Sunday, Monday. So far only two interviews. Very slow. Living in a home in the village makes us defenseless against interruption. We do need more time. We have not got it." He was right, of course, in being upset, because we had lost a lot of time waiting for his clearance and research permit in Ankara and had to start the fieldwork much later than he anticipated. We just did not have the time to talk to everyone in the village, so we had to choose one third of the population to interview, at least selecting larger *kabiles* (patrilineal groups). But there was a great advantage to living in the village rather than commuting long distances. We could establish closer relations and better rapport with the villagers. People would stop by to say "welcome" or to be neighborly asking us if we needed anything, and we bought our needs, like milk, yogurt and eggs, from them who in the meantime provided us with unsolicited and unexpected, yet invaluable information... In turn, we would reciprocate their visits and the research would expand to evenings. Paul would not be happy, though, as he wrote in his fieldnotes: "Reciprocity of visits is difficult; and the boundary between social visits and chat and research is also difficult." Still, Paul wanted to differentiate between "social" and "work" times. For one "social evening" when he took Ali Osman from Sakaltutan to Kölete, he wrote, "No real news. A great example of getting too embedded and

comfortable - and tired. I simply did not have my mind on getting ‘data’.” On these occasions he felt guilty. Maybe if we were not pressed for time, he would be more relaxed.

What a labor intensive, tedious task he embarked upon! When I look at the data and the fieldnotes, I cannot help but feel saddened by all the painstaking effort and time spent to produce the detailed database which I am afraid cannot be comprehended by anyone as is. Minute details, for example, calling the same woman as Fatma, Fadime or Fadik, as did her different relatives, would seem like they were different people in the computerized database; so, we had to be very careful to be consistent, which sometimes wore Paul out. It must have taken an awful lot of time to go through those names and the codes assigned to them. With Michael Fischer’s recent initiative, hopefully the project will continue and be completed so that it will be accessible to be fully used by interested academics who may further the longitudinal research.

### **What I have learned from Paul Stirling**

Paul has been influential on not only my career but my whole life. The ease with which he talked about his shortcomings, rather than marketing himself, always meant to me that he was self-confident enough to do so. Something I tried to learn from him and imitate. I didn’t know then, that what Saul Alinsky had written years ago in *Rules for Radicals* would echo my views: “The human spirit glows from that small inner light of doubt whether we are right, while those who believe with complete certainty that they possess the right are dark inside and darken the world outside with cruelty, pain, and injustice” (1972: 4).

It is easy to judge and dismiss meticulous, critical work by saying that one loses sight of the big picture by dissolving in details. If so, I have witnessed Paul guilty of this crime, just as I observed the same in my own behavior. Very similar to Paul, I never felt included in the academia at least partly because I have not been a “sectarian,” have never belonged to any strict schools of thought, keeping all my bridges open and going on questioning anything and everything. That of course did not leave much time to write and publish. Paul set an example for me to encourage this kind of behavior – in addition to my own many shortcomings and life events I encountered – that restricted my academic performance but I can say with a clear conscience that I’ve never stopped thinking critically and I have never been engaged in any doctrine in a blindfolded way. It has been a very lonely and unproductive life which fed my depression, in which I am afraid I also followed Paul’s footsteps, albeit unwillingly.

My sense of responsibility, which I originally inherited from my mother, was also influenced, at least in part, by Paul. He felt responsible for many things, including the employment of his students, as Chris Hann and Roy Ellen mention in this volume. He said to me on several occasions that he felt guilty, that he had “failed” me, because the computerized database was not completed on time and I could not use the whole quantitative tool for my dissertation, and because he was not involved in my dissertation as much as he thought he should have. I had access to all the data, of course, and I did use it, if not the computerized database at the University of Kent. He would have wanted me to continue working with the database and to keep in touch with the villages. By not doing so, I suppose it was my turn to fail him, although I have kept in touch with Aniş and her family.

Looking back, I can link many detailed memories in an array of diverse subjects, with his sense of responsibility – at different levels, abstract and concrete. One of the first things I learned from him – before and independent of the feminist slogan – is, “personal is political.” This, I associate, in addition to his “empiricism,” with his sense of personal responsibility in social situations. His displeasure with the use of passive voice and indirect speech was, again, an invitation for responsible stance. I remember him holding open a newspaper in his hand, staring at it, frowning and complaining that the frequent use of passive voice made the Turkish newspaper discourse unclear for him. By not using active voice, he argued, journalists were not standing by their word and not taking responsibility. Paul’s frequent use of the word “*imiş*” in Turkish (that designates that the information source is either not known or not trusted) has been a good indicator for me of his keen interest in accuracy.

Having a car, and having connections in the city, our fieldwork involved a great deal of social work, especially “ambulance service” when villagers with health problems needed to be taken to doctors—another extension of Paul’s sense of responsibility. We encountered a lot of interesting “medical anthropological” material, like local ideas and beliefs about health and disease, villagers in mental hospitals, people performing magic by getting *muska* (amulets) produced, and such, that we did not focus on simply because our focus was rural transformation. Nevertheless, this material found a place in our fieldnotes, and in a book chapter (2021) I wrote about peasants and doctors, based on my observations in the Kayseri villages (that, initially, would have been included in a book on Social Cognition that Paul Stirling and Gabriele Rasuly-Paleczek were editing but had to be abandoned after Paul’s death). I wasn’t aware then, that Paul had noted similar observations in his fieldnotes:

On this (plus other) evidence villagers have very poor relations with doctors. The palatial premises, the authoritarian manner, the problems of village models of physiological processes. And no attempt to explain medicines and their purposes. Doctors are powerful, ruthless, confident.

There are many issues that I did not think of exploring during our fieldwork and that I later said “I wish I had asked.” I knew, for example, that Kölete, Zek, Zerezek, Kanber and many more, were old Armenian village names in the Kayseri province that were known to and used by the local populations although all were officially changed into Turkish names. After their population was driven out, the names of the villages were also changed to erase whatever had remained from history. But I would not know then, that I would go to Armenia and do research there. I could have learned from Paul a lot about his encounters with the Armenian “question” in the 1940s and 50s. I was quite ignorant then.

I also wish I could have learnt some of his spirit of entrepreneurship, imagination and creativity. Unfortunately, I am hopeless in these matters.

At age 65 now, he still sets me a good example by reminding that chronological age doesn’t matter, that I can still go up and down the stairs three at a time, as he had done in 1986 in our Talas apartment as if he were a 4-year-old at age 67.

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Paul Stirling, a lovable human being, was the most decisively open, honest, unpretentious person I have ever met, maybe at the expense of being defenseless and vulnerable. We had a unique relationship and with him I have also lost a side of me that I can never share with anyone

else – I suppose that is what death is about. I feel very lucky and privileged to have known, and spent that intense and intimate time with, him. I don't think there are many people who have had a similar experience. Paul is still vividly in front of me after all these years, in his pink shirt, still in his deep inevitable sadness and equally child-like playfulness, but I cannot remember some particulars. Were his eyes green? Grayish green? And which big toe was he missing?

## References Cited

Aykaç, Gülşah. 2021. Mimari Tasarım Stüdyosuna Feminist Bakış: Öğrencinin Yolculuğunu Anlamaya Çalışmak (Looking at the Architectural Design Studio with a Feminist Eye: Understanding the Journey of the Student). In: *Feminist Tahayyül* 2(1): 9-37.

Alinsky, Saul D. 1972 (1971). *Rules for Radicals. A Pragmatic Primer for Realistic Radicals*. New York: Vintage Books.

Hann, Chris. 1998. Obituary: Paul Stirling. In: *The Independent*, 25 June.

İncirlioğlu, Emine Onaran. 2021. Doctors and Peasants: Power Hierarchies and Social Knowledge in Health-Care. In: Pultar, Gönül and Louis Mazzari (eds.). *Across a Broad Horizon: A Festschrift in Honor of Belma Ötüş Baskett*. İstanbul, pp. 353-371.

İncirlioğlu, Emine Onaran. 1994. Negotiating Ethnographic Reality: Team Fieldwork in Turkey. In: Chis Hann (ed.). *When History Accelerates: essays on rapid social change, complexity and creativity*. London: Athlone Press, pp. 255-275.

Meeker, Michael. E. 1971. The Black Sea Turks: Some Aspects of their Ethnic and Cultural Background. In: *International Journal of Middle East Studies* 2 (318 -345).

Sipahi, Ali. 2021. In pursuit of intellectual discovery: an interview with Michael E. Meeker. In: *New Perspectives on Turkey* 65: 100-119. <https://doi.org/10.1017/npt.2021.24>. Accessed 2.1.2023.

Stirling, Paul and İncirlioğlu, Emine Onaran. 1996. Choosing Spouses: Villagers, Migrants, Kinship and Time. In: Rasuly-Paleczek, Gabriele (ed.). *Turkish Families in Transition*. Frankfurt am Main: Peter Lang, pp. 61-82.



## No Conclusion

Paul Stirling did not write much. That is not true: He wrote a lot but *published* little. He was an over-thinker. He was caught between perceiving and judging, between process and product – to say the last word and to put a full stop was torturous for him. One of the pieces he wrote for teaching purposes which, to our knowledge, was never published, is telling about Paul Stirling, the man. So, we shall let him have the last word.

## Social Anthropology Research Training Seminar

### Writing a thesis

### A Skeleton for a Talk

### Paul Stirling, February 1980

#### I. What is the point?

##### Motives

1. Instrumental. Step one in the rat race.
2. Social Academic prestige – to impress your colleagues and proclaim your superiority over fools.
3. Personal – to enjoy doing a good piece of creative craftsmanship for other people.

None of us are free of motive 2. and almost none of motive 1. But by concentrating on 3. we are likely to get the best of all worlds.

#### II. What is academic research?

1. Commenting on academic writing – on secondary sources.  
Selecting from a huge pile, and rejecting most of it.  
Stock taking, reorganizing, putting together.  
Criticising, reformulating, refuting.  
New ideas – cumulative edging forward; or even great leaps.
2. New data.
  - (i) Primary written sources (documents and statistics).
  - (ii) Observing, listening, questioning, i.e., creating new primary sources.
  - (iii) Communicating an amalgam of (i) and (ii) to the world – that is, publishing. Without this, the exercise is pointless.

#### III.

1. So while you are reading, de-villing or surveying or writing field notes THINK ABOUT WRITING for publication (but you can still collect stuff because it might be useful; or because no one else will ever have the chance).
2. Write for your readers. Never forget them.  
Do not write for your supervisor nor for your examiner.

AND NEVER WRITE TO YOURSELF. It may be clear to you. But is it clear to your public?

Avoid the sin of COGNITIVE EGOCENTRICITY.

Three questions:

- (i) Is this worth reading? By whom? Why?
- (ii) Have I said it in the most economical time-saving and non-irritating way possible?
- (iii) Is it as attention-holding and enjoyable as I can reasonably be expected to make it?

#### IV. Creative Craftsmen for what kind of product?

I comment on:

##### 1. Content.

- (i) Scholarship – Secondary sources.
- (ii) New primary sources – your data.
- (iii) Using explicitly, and critically, existing concepts and theories; possibly adding to or adjusting them.
- (iv) Relating the relevant theories to the data.

##### 2. Form.

- (i) The overall ordering.
- (ii) The relative weight of parts.
- (iii) English.

#### V. Scholarship.

You must have a full and well-arranged bibliography. Plenty of techniques for getting one together. But booklists are terrifying; and I suspect that I am not the only one who practices a standing confidence trick on this front.

Use your judgement and your wits; be frank; steer between superficial and arrogant rejection of material, and wasting hours and hours ploughing through fields of arid rubbish. FROM THE BEGINNING KEEP RETRIEVABLE, ORGANISED AND ECONOMICAL NOTES, WITH FULL PAGE REFERENCES. YOU WILL SAVE HOURS LATER. (I didn't.)

(N.B. Information simply for its own sake is pointless.)

#### VI. Your own data.

Scribnivas. Soak yourself in your fieldnotes.

Firth. Treat your notes as evidence.

Barnes E-P went through the Nuer taking out as many facts as possible.

Make sure NOT to assume your reader was in the field with you.

Be frank – report your doubts and failures – everyone has them, and the academic is already too full of pretentious dishonesty.

#### VII. Theories and Concepts.

- 1. No-one can think at all without implicit theories. No-one can make all these implicit theories explicit. Try your best; make it brief and as clear as possible.
- 2. You do NOT have to have one major theory, still less to re-write anthropological theory. IF you do have an overall theoretical structure, you are lucky, but be on your guard.

Otherwise, you may have a number of related issues to discuss, and a number of existing concepts and ideas which illuminates your stuff, or which your stuff illuminates or refutes. (Do not bother to refute nonsense.) Be open, and do not worry if your ideas lead to dilemmas or are inconclusive.

3. All the same, the crux lies in relating existing ideas, your ideas, and the new data into some kind of overall intelligible argument, even if it is inconclusive.

#### VIII. Form.

You need an overall plot; some kind of order which holds interest, an ongoing argument – with if necessary explicit, interesting digressions.

Two triads.

1. Background – what your reader must know to get your message.
2. The main message. Your data and your ideas.
3. A summary, a set of (tentative) conclusions; and of pointers to unresolved puzzles.

And

1. Beginning.
2. Middle.
3. End.

These are related. BUT THEY SHOULD NOT EXACTLY COINCIDE.

#### IX. Background / Introduction.

1. Keep your readers interested. So, tell them things in which you have already got them interested.  
They begin eager to know what you are on about. Build on the initial curiosity.
2. They need to know sooner or later: Your main interest / message.  
The state of the existing literature.  
The way you found things out and how reliable your data is; succinctly, but thoroughly. The necessary background information which makes your data comprehensible, and prevents it being misleading.  
Remember they did not do your field work with you.
3. THERE IS NO POINT IN PRECIS of what others have said unless you explicitly build on its excellence, or refute or modify it. That is, your scholarly account of the literature is essential background, but it must fit into the plot. It is not a ritual duty.
4. Do not expect the readers to remember everything; constantly refer to yourself forward to what is coming, backward to what you last said. This is good for you it avoids inconsistency and repetition. It emphasizes the unity of the work for you and for the readers. And it mightily helps readers to follow.
5. Two tricks, from detective stories.
  - (i) Deliberate puzzles, to mystify and arouse interest.
  - (ii) Conveying information by implication without the readers realising it.
 Use them if you like, but skilfully, consciously; and not too often.

#### X. Your main argument and main data.

1. Think out a logical order or argument.
2. If as is possible some data you want to include does not fit easily, just say so and explain why you include it.
3. Good intelligible and relatively unbiased and non-misleading social description is extremely rare. Do not underestimate either the difficulty or the value of doing it well.

4. What you have done and are doing is complex and difficult; no need to achieve certainty or exaggerate your achievements, or to pretend to yourself or to others. Be explicit, honest and modest.
5. Polemics are fun. Watch out for your enemies counter attack; guard your flanks.

#### XI. Conclusions.

Summary – what have you said that was worth saying? How are the bits connected? – or not? positions have you confirmed, established or refuted?  
What further questions now arise for you and others to get on with?  
Let your hair done, but explicitly.

#### XII. Chapters, Sections, Paragraphs.

Chapters obviously need unity. Numbers and length must be your decision BUT use clear sections within chapters, with headings, and make sure that they follow on in as tight and logical a way as possible. Explain sharp breaks in the sequence.  
Keep rigidly to the point in each paragraph. If a digression is necessary or desirable (to fit a point in or to intrigue or amuse your readers) make this a separate paragraph (or section).  
I strongly advocate numbering and sub-numbering for your own guidance. Some of this may even help the reader. A matter of taste and judgement.

#### XIII. English.

1. Maximum clarity.  
Never use technical words, still less common words in a technical or unusual sense, without making sure the readers know, with some accuracy and certainty, what you are at. Offer NOT formal definitions, but a clear account of what you are doing with the word in question.
2. Never waste words. Never use two sentences where one will do. Keep sentences short. Active voice, simple construction.  
Redundancies, tautologies and repetitions simply show you up; and waste your time, the readers' time, patience and good will, and consume a lot of money and even trees.
3. Elegance, wit, the use of striking or evocative words, is NOT a necessary condition for a Ph.D. And literary excellence is sometimes used to conceal confusion (from the author among others) or to mislead. All the same, pleasure in writing and in reading is a very welcome bonus in the academic world. So think about quality when you write, and when you read. Avoid the awful and espouse the good – of course in your own judgement.

To sum up, at all costs avoid obscurity, pomposity, pretentiousness, long windedness.

#### XIV. Techniques.

1. Devise a system for keeping all notes, drafts, bibliography references in order. Worth the initial time getting this right and keeping it so. But avoid time consuming fussy procedures.
2. Construct a thesis outline at a very early stage – before fieldwork perhaps.  
Loose leaf and well-spaced out.  
CONSTANTLY use it – revise it, annotate, remind yourself of it, put ideas in the right place, or add them at the end for later sorting. That is, your thesis should be

your goal from the beginning and the plan should be growing organically as you read and think and collect information.

When writing, annotate other sections with points that come to mind. Never waste an idea.

3. Draft with masses of space for correcting.
4. If possible, teach yourself to touch type. Do not type with two fingers, looking at the keyboard.
5. Spend thirty minutes of writing time a day writing as well as you possibly can, every word thought about, every sentence sharp, efficient, elegant.
6. This way your sensitivity to decent prose will increase, and so will your capacity to write well fast.

XV. My advice.

Do not set out to write a thesis. Write the first draft of a book.

Do everything you do for your study and research with writing in mind.

Write for your readers, not yourself.

Write the most interesting, economical, lucid and enjoyable book you can; eschew the English prose you find in most of the academy.

All the same, this is an academic undertaking.

You need evidence, – and an account of its origins and reliability.

You need logic; concepts as clear and rigorous as possible, arguments that follow from clear premises.

And you need to know, and show that you know what other people have written about what you are writing about.

There is then a hell of a lot to do; and most people end up writing ten hours a day seven days a week to get finished.

But if you make a systematic job of it, and enjoy the creation of a work of your own, even a modest one, you are certain to make it in the end. So do not worry.

XVI. One of my contemporaries typed his thesis straight out – final draft – himself. Mozart was like that.

If you are a Beethoven or less, you may still find much I have said does not fit you, your tasks or your personal goals.

Fine. You are you and it is your thesis.

## Contributors

**Bahattin Akşit** obtained his bachelor's degree from Middle East Technical University (METU, Ankara) in sociology (1968) and his MA (1971) and PhD (1975) from the University of Chicago. He worked at METU for 37 years as assistant, associate and full professor. He also worked at Maltepe University, İstanbul for 15 years as a professor in the Department of Sociology. He was a visiting scholar at the School of Oriental and African Studies, University of London, for six months and also worked at Kyrgyzstan-Turkey Manas University for two years. Prof. Akşit has conducted research and published in the following areas: social change in villages, towns and cities; rural structural transformations; sociology of religion and secularism; sociology of the Middle East and Central Asia; army and conflict resolution; psycho-social aspects of disaster management; NGOs and civil society; educational sociology; street children and sociology of science.

**Sencer Ayata** obtained his undergraduate degree in Social Sciences from the Middle East Technical University (METU) in 1973. In 1982, he completed his doctoral studies in Sociology and Social Anthropology at the University of Kent, Canterbury. From 1982 to 2010, he served as a lecturer in the Department of Sociology at METU, while he also held positions as department head (1997-2003) and Director of the Institute of Social Sciences (2003-2010). Throughout his career, he has taught as a visiting professor at various renowned institutions including the University of Manchester and the Wissenschaftszentrum Berlin für Sozialforschung. Subsequently, he transitioned into public life, serving as a member of parliament and in various other roles. Prof. Ayata has published extensively in both Turkish and English, including *Kapitalizm ve Küçük Üretim* (Capitalism and Small-Scale Production, 1987), *Sermaye Birikimi ve Toplumsal Değişim* (Capital Accumulation and Social Change, 1991), *Konut, Komşuluk ve Kent Kültürü* (Housing, Neighborhood and Urban Culture, 1996, with Ayşe Güneş-Ayata), and recently an edited volume, *Türkiyede Yoksulluk ve Eşitsizlik: Nedenler Süreçler* (Poverty and Inequality in Turkey: Causes, Processes, Solutions, 2020).

**John Corbin** was born in Cuba in 1940. He attended grade school in Brazil, the United States, Colombia and Cuba; high school in Cuba, and college at the University of Chicago. In 1962, he began postgraduate study in social anthropology at the London School of Economics, transferring to the University of Kent at Canterbury in 1965. He was appointed to a Temporary Lectureship at Kent in 1969, moved to a permanent Lectureship at the University of East Anglia in 1970, and returned to Kent in 1989. He also had spells as visiting professor at universities in other countries: Cornell, Columbia, Florida International in the USA; Hobart in Australia; and Complutense de Madrid in Spain. He retired in 2006.

**Kayhan Delibaş** is a Professor of Sociology and has been working at the Adnan Menderes University, Aydın, Turkey. He was a visiting professor at the University of Durham (England), University of Pisa, University of Messina, (Italy), University of Oredea (Romania). He is a member of several international organizations and serves on the board of the RN22 - Sociology of Risk and Uncertainty [ESA]. His research in sociology focuses, in particular, on political Islamic movements, grassroots activism, identity, and secularization in Turkey. Secondary research interests revolve around risk society, uncertainty, trust, and rumors. He is the author of two books, numerous chapters in several publications, including ‘Conceptualizing Islamic Movements: the case of Turkey’, *International Political Science Review*, (Vol. 30, No. 1, January 2009), *The Rise of Political Islam in Turkey: Urban Poverty, Grassroots Activism and Islamic Fundamentalism*, (I.B. Tauris, London, 2015), and *Risk Toplumu: Belirsizlikler ve Söylentiler Sosyolojisi* (‘Risk Society: Uncertainty and Sociology of Rumours’), Ayrıntı Yayınları, İstanbul (2017).

**Hülya Demirdirek** was a fieldwork assistant to Paul Stirling while a student at Middle East Technical University in the late 1980s. She trained as an anthropologist in Norway, conducting her MPhil fieldwork in Azerbaijan and her doctoral and subsequent research among the Gagauz in post-socialist Moldova. Her publications focus primarily on nation building, gender and immigration. She taught anthropology at Canadian universities for 11 years. Since 2012 she has worked on research projects with think tanks and civil society organizations, and as an independent consultant in Ankara. Currently she divides her time between Ankara and Vancouver.

**Roy Ellen** is Emeritus Professor of Anthropology and Human Ecology at the University of Kent, a fellow of the British Academy and past president of the Royal Anthropological Institute. He has undertaken research in Southeast Asia, especially in the Moluccan islands, and in the United Kingdom. His current interests include the relationship between cultural cognition and the history of science, and his most recent books are *The Nuaulu world of plants* (2020) and *Nature wars* (2021). He was a junior colleague of Paul Stirling from 1973 until 1998.

**Chris Hann** is Emeritus Director of the Max Planck Institute for Social Anthropology (Halle/Saale). Before moving to Germany in 1999, he taught social anthropology at the Universities of Cambridge and Kent (Canterbury). Primarily an economic anthropologist by training, Hann carried out fieldwork in socialist rural communities in Hungary and Poland in the 1970s. In order to analyse comparable processes of development in a capitalist context, he began his investigations of tea-growing smallholders in Turkey in 1982. Later, in the early 1990s, together with Ildikó Bellér-Hann, this research expanded to become a more comprehensive study of changes in political economy and socio-cultural factors on the east Black Sea coast. Paul Stirling was an important source of advice and support for the entire duration of these projects. Publications include: *Tea and the Domestication of the Turkish State* (Eothen 1990); *When History Accelerates* (edited, Athlone, 1994); *Turkish Region. State, Market, and Social Identities on the East Black Sea Coast* (with Ildikó Bellér-Hann, James Currey, 2000).

**Emine Onaran İncirlioğlu**, after receiving her first degree in architecture at Middle East Technical University, Ankara, completed her graduate work in anthropology at the University of Florida in Gainesville. She collaborated with Paul Stirling for her PhD dissertation fieldwork in the Sakaltutan and Elbaşı villages of Kayseri, and taught at various universities in the US, UK, Germany and Turkey, in both social science and design departments. One of the founding members of the Cultural Studies Association of Turkey, İncirlioğlu's research, teaching and interest areas include cultural studies, ethnographic fieldwork, ethnographic novel, gender studies, Gypsy/Romani studies and themes under the rubric of space-culture-identity. After her retirement, she stayed in Armenia in 2015 with a scholarship from the Hrant Dink Foundation and wrote her semi-ethnographic memories to be published soon.

**Krishan Kumar** is University Professor and William R. Kenan, Jr., Professor of Sociology at the University of Virginia, USA. He was previously Professor of Social and Political Thought at the University of Kent, Canterbury, England. Among his publications are *Prophecy and Progress: The Sociology of Industrial and Post-Industrial Societies* (1978), *Utopia and Anti-Utopia in Modern Times* (1987), *From Post-Industrial to Post-Modern Society* (second edition 2005), *1989: Revolutionary Ideas and Ideals* (2001), *The Making of English National Identity* (2003), *The Idea of Englishness* (2014), *Visions of Empire: How Five Imperial Regimes Shaped the World* (2017), and *Empires: A Historical and Political Sociology* (2021).

**Nancy Lindisfarne** is an anthropologist, painter and printmaker, and has won prizes for her short fiction. She did graduate work at the School of Oriental and African Studies, University of London where she taught for many years. She has done fieldwork in the Middle East – in Iran, Afghanistan, Turkey and Syria - and in South Wales and New England. She has published widely on the politics of marriage, practiced Islam and climate jobs. Her latest book, written with Jonathan Neale, is *Why Men? A Human History of Violence and Inequality*. It is due out with Hurst in September 2023.

**Gabriele Rasuly-Palczek** holds an MA in Sociology and Political Sciences (1979) and a Ph.D. in Anthropology, Turkology and Islamic Studies (1984) awarded by the University of Vienna, Austria. Until her retirement in 2021, she worked at the Anthropology Department of the University of Vienna. Additionally, she has lectured at various academic institutions. In 1999-2000, she served as a post-doctoral fellow at Yale University, and in the fall term of 2012, she held the position of visiting professor at the University of Southern California, Los Angeles. She has conducted extensive fieldwork in rural Turkey, as well as among Afghan refugees in Pakistan, Turkey and Austria. She has published numerous articles and books focusing on rural Turkey, local politics, identity issues in Afghanistan, and the current situation of Afghan refugees in Austria.



**David Shankland** is Director of the Royal Anthropological Institute, and Honorary Professor of Anthropology at University College London. He conducted fieldwork in Turkey in the late 1980s in an Alevi village, and subsequently conducted a longitudinal study of migration from the village to Germany. Amongst other publications he is the author of *The Alevis in Turkey: the emergence of a secular Islamic tradition*, and *Islam and Society in Turkey* (2003).

**Abdulvahap Taştan** started to work as research assistant in the Faculty of Theology at Erciyes University, Kayseri in 1985, after completing his education in the same faculty and working as a teacher. In 1986, he took part as an observer (*mihmandar*) in Professor Paul Stirling's ethnographic study in Kayseri, Türkiye. He worked as a visiting professor at Baku State University between 1996 and 1999; lectured in sociology of religion as a professor and directed postgraduate studies before he retired in 2019.

**Lale Yalçın-Heckmann** studied sociology in İstanbul and anthropology at the London School of Economics. At Max Planck Institute for Social Anthropology in Halle/Saale she wrote on agrarian property in Azerbaijan, led a research group and worked as a senior researcher. Since 2010 she teaches at the University of Pardubice, the Czech Republic. Her publications include: *Tribe and Kinship among the Kurds* (1991); *Die Kurden: Geschichte, Politik, Kultur* (together with Martin Strohmeier) (2000); *Caucasus Paradigms: Anthropologies, histories and the making of a world area* (co-editor Bruce Grant) (2007); *The Return of Private Property: Rural life after Agrarian Reform in the Republic of Azerbaijan* (2010); and *Moral Economy at Work: Ethnographic investigations in Eurasia* (single editor) (2022).