

Religion and social organisation in north-western Tunisia

Volume I:

Kinship, spatiality, and segmentation

BOOKS / INDEPENDENT PUBLICATIONS BY WIM VAN BINSBERGEN

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ABOUT THE AUTHOR

WIM VAN BINSBERGEN (*1947) was trained in sociology, anthropology, and general linguistics, at Amsterdam University (Municipal). He held professorships in the social sciences at Leiden, Manchester (Simon Chair), Durban, Berlin, and Amsterdam (Free University). At the latter institution he took his *cum laude* doctorate (1979) and was the incumbent of the chair of ethnic studies (1990-1998), prior to acceding to the chair of Foundations of Intercultural Philosophy, Philosophical Faculty, Erasmus University Rotterdam. Simultaneously, he held senior appointments (1977-2019) at the African Studies Centre, Leiden. Over the decades, he has established himself internationally as a specialist on African ethnicity, African religion, ethnohistory, globalisation, intercultural philosophy, comparative mythology, the Mediterranean Bronze Age, and transcontinental continuities between Africa and Asia in pre- and proto-history. He was President of the Netherlands Association of African Studies, 1990-1993; President of the Netherlands / Flemish Association for Intercultural Philosophy (1998-2022); and one of the Founding Members / Directors of the International Association for Comparative Mythology, 2006-2020. From 2002 he has been the Editor of *Quest: An African Journal of Philosophy / Revue Africaine de Philosophie*. His many books include *Religious Change in Zambia* (1981), *Tears of Rain* (1992), *Intercultural Encounters* (2003), *Ethnicity in Mediterranean Protohistory* (with Fred Woudhuizen, 2011), *Black Athena Comes of Age* (2011), *Before the Presocratics* (2012), *Vicarious Reflections* (2015), *Religion as a Social Construct* (2017), *Researching Power and Identity in African State Formation* (with Martin Doornbos, 2017), *Confronting the Sacred: Durkheim Vindicated* (2018), *Rethinking Africa's Transcontinental Continuities in Pre- and Proto-history* (2019, ed.), *Sunda: Pre- and Proto-historical Continuities between Asia and Africa* (2020), *Sangoma Science: From ethnography to intercultural ontology: A poetics of African spiritualities* (2021); *Joseph Karst: As a pioneer of long-range approaches to Mediterranean Bronze-Age ethnicity: A study in the History of Ideas* (2021); and *Pandora's Box Prised Open: Studies in Comparative Mythology*, 2022). His published work is also available from <http://www.quest-journal.net/shikanda>. Wim van Binsbergen is married with the classical (European and Indian) singer and breathing therapist Patricia Saegerman, and has five adult children. Besides his scholarly work, he is a published poet / novelist, the adopted son of a Zambian king, and a certified and practising African diviner-healer in the Southern African *Sangoma* tradition.




Religion and social organisation in North- western Tunisia

**Volume I: Kinship, spatiality, and
segmentation**

Wim van Binsbergen



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COVER ILLUSTRATIONS *front and back*: Women of the Hillel family harvesting rye in the valley of Sidi Mhammad, 1968 (this book, Fig. 4.2); front: Service géographique de l'Armée, 1889, *Carte de la Tunisie*, Paris: Service géographique de l'Armée; one of the author's graphs analysing distance and interaction in the research area; *back*: the author during the fieldwork on which this book is based, in the village of Sidi Mhammad, 'Ain Draham, Tunisia, May 1968, in conversation with the 'omda (burgomaster / chief / shayb) Mr Hillel bin Hassuna (foreground left) (foto Mr Hasnawi bin Tahar)

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version 1; 8-2022)

to the memory of Douwe Jongmans (1922-2011),
my unforgettable teacher of fieldwork
'een meziya die een laazem is' (derived from a dedication
to the present author, accompanying an offprint of one of
Jongmans's articles on Humiriyya, 1975)

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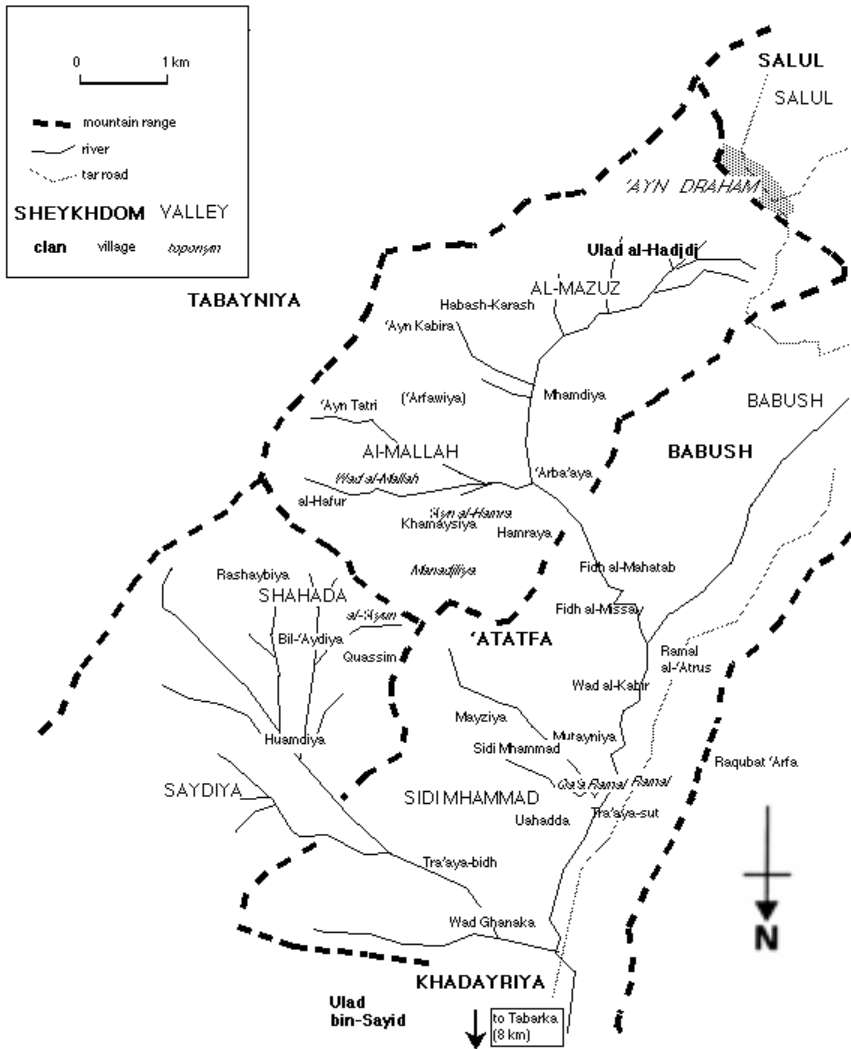
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Cf. the Fig. in Hartong 1965: 8.

Fig. 0.1. The research area.



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Chapter 1: General introduction to the Volumes I and II of this book

1.1. My first anthropological research project

In 1964 I began my anthropological career as a first-year student at the University of Amsterdam, and after the then mandatory seven years of full-time study (with major fields in general linguistics and religious anthropology) I concluded my formal tuition in 1971 with the degree of Drs of Social Science; in 1979 this trajectory was crowned with a *cum-laude* doctorate in the social sciences at the Free University Amsterdam. Closely supervised fieldwork training was a central part of the postgraduate curriculum. In the context of the research training project organised since 1965 by the Anthropological Sociological Centre, University of Amsterdam (*cf.* Jongmans & van Veen 1968), I conducted research on social and cultural aspects of popular religion in the highlands of north-western Tunisia, Ḥumiriyya. My first fieldwork (in Ḥumiriyya, *i.e.* the highlands of North-western Tunisia, 1968) was conceived, not only in terms of Durkheim's (1912) theory of religion and society (which ultimately led to my recent monograph on Durkheim, 2018), but also within an anthropological paradigm in which kinship dominated the conception of social organisation. At the Anthropological-Sociological Centre of the University of Amsterdam, we were trained, in the 1960s, to become primarily kinship specialists, in a monomaniacal way that even professional anthropologists today would have difficulty to understand, let alone emulate. The central problem of my extensive research programme with which I set out for the field in 1968, was the relationship between the several dozens of (nominally Islamic) shrines dotted over the landscape of the highlands of North-western Tunisia, and the present-day social organisation of that region; but the only then conceivable way in which I could approach that problem, was through a very intensive and comprehensive study of kinship and marriage as main windows on local social organisation, in this society

which was by its own conscious ideology, strictly patrilineal.

1.2. Note on Ḥumiriyya as a toponym

French usage calls the highlands of north-western Tunisia 'la Kroumirie', and their inhabitants 'Kroumirs'. Although French was the official language in Tunisia at the time of the research, I prefer to Arabicise the French term into the (hypothetical) 'Ḥumiriyya', with 'Ḥumiri' (sing.) denoting an inhabitant of that region, as well as the adjective in general. In the arabicising form the underlying meaning of the French toponym is preserved: it clearly shows the root Ḥamira, yeast, and goes back to the following myth:

'A long time ago, when the region was still very sparsely populated, a certain poor family was visited by a delegation that came to ask the daughter of that family in marriage. There was nothing in store but a little yeast. However through a miracle that yeast swelled to form a copious meal. Thus the family could fulfil its obligations of hospitality, and greatly impressed the wooers returned to their home, taking their bride with them.'

During my field-work a similar miracle of hospitality was performed for me virtually every day.

The research was conceived in such a way as to necessitate ample coverage of key aspects of the social organisation of the area. The results of the latter dimension of the project are presented in the present argument; thus it forms an integral part of my description of religion in Ḥumiriyya.

Elsewhere I have described my Ḥumiri field-work from a point of view of method, organisation and human interest (van Binsbergen 1969; incorporated as chapter 2 in the present volume). Relevant details of the quantitative analysis are to be found throughout the chapters of both Volumes of the present book.

1.3. The intellectual background and objectives of the present study

In the field, and during the preparatory semester and writing-up period in Amsterdam, the development of my ideas on Ḥumiri society was constantly stimulated by conversations with the two experienced fieldworkers, Douwe Jongmans and Klaas van der Veen. Together with Pieter van Dijk and Marie-Louise Creyghton they supervised the 1968 field-work training project in an excellent manner. It is with great pleasure that I recall the co-operation with the other participants: Piet Ernsting, Peter Geschiere, Coen Holzappel, Gustaaf von Liebenstein and Pieter Tamsma. They were each kind enough to grant me formal permission to use the results of our initial, collective research in the villages of Hamraya and Ḥamaysiyya, as directed by van der Veen; this formed a welcome and necessary supplement to the data I later collected on my own in the other villages of the research area that was allotted to me.

Others, particularly Coen Beeker and Guus Hartong, preceded me as researchers in the same valley (*cf.* Beeker 1967; Hartong 1968). I have benefitted from the suggestions

they made to me before departure, from the great good will they had managed to build up with the local population, and from some of their genealogical and historical data. My old friend the late lamented Jos van der Klei likewise read earlier drafts of the manuscript and was my partner in fruitful conversations on H̄umiri society.

With regard to the overall orientation of my argument (more than with regard to details of its organisation) I am indebted to the advice of Jeremy Boissevain, professor of anthropology at the University of Amsterdam from 1966 till his retirement. In fact his influence upon the present work (especially Volume I) went much further: as one of 'the three B's' (Boissevain, Bailey and Barth)¹, he set out, in the 1960s, on a somewhat Quichotic quest to free anthropology from its, supposedly erroneous, obsession with corporate groups and enduring organisational forms, and, instead, to show the forms of manipulation and indeterminacy that, in his view, largely make up social life (Boissevain XXXX, XXXX, XXXX). The echoes of this endeavour can be heard throughout Volume I, and also for me H̄umiri society turned out to consist, not of corporate groups with well-defined boundaries, but of amorphous and overlapping kindreds upon which only loosely and inconsistently, yet emphatically, the framework of patrilineal descent has been imposed. However, I was sufficiently fascinated by unilineal kinship as a participants's conscious and explicit model of thought, to go into much greater depth in its empirical description and its analysis than non-numerate Boissevain (familiar, through his 1950-60s fieldwork, with the then already globalising environments of the Sicilian maffia, Maltese parishes, and Italian neighbourhoods in Montreal, Canada) has ever attempted – and he never came round to appreciating how the Manchester School had already, in the 1950s, rendered obsolete the classic anthropological model he kept attaching (Werbner 19XXX; van Binsbergen 2007).

Our relationship effectively culminated in the (*cum laude*) acceptance of the Drs thesis (*in status nascendi* this book's Volume I) written under his supervision. It was then that out of his own initiative he proposed (having seen me slave for four years at preparing, conducting, and writing up – with special emphasis on statistical and otherwise quantitative methods – my Tunisian fieldwork) that he should confer a PhD degree upon me on the strength of that work. So I happily left for my first teaching job, at the University of Zambia (UNZA), South Central Africa (which I had secured on

¹ For Boissevain's work on the deconstruction of classic anthropology, cf. 1968 (his Amsterdam inaugural lecture), and *Friends of Friends. Network, Manipulators and Coalitions* (1974) ; he was also to publish, together with the prominent Manchester exponent J. Clyde Mitchell, a collection on Network The institutional niche which Boissevain sought to carve out for himself was the anthropology of the Mediterranean (Boissevain 1979). Analysis (1973), which also contained one of Jongmans's few writings on H̄umiriyya (1973). Bailey (e.g. 1957) was a specialist on the anthropology of caste in India who sought to deconstruct anthropology with his book *Strategems and Spoils* (1969); Frederic Barth, after seminal fieldwork on the Swat Pathans of Pakistan (1956, 1959), edited the influential collection *Ethnic Groups and Boundaries* (1969), and was conceptually especially innovative in his now classic *Models of Social Organization* (1966).

Graeco-Roman Antiquity already boasted a similar expression as 'the three B's': *τρια κακια κακιστα tria kappa kakista* 'the three most evil K's' – the three Mediterranean seafaring nations notorious for their piracy, notably Cretans, Carians, and Cappadocians (Karst 1931: 355 f.; cf. my recent book on Karst, 2021xxx). However, no offence intended – in anthropology our three B's, whatever limitations we may now perceive with the powers of hindsight across half a century, are associated with innovation and liberation from the structural-functionalist straightjacket.

the strength of the network relations of Boissevain's senior colleague, the Africanist anthropologist André Köbben – another one of my principal teachers 1964-1971), in the certainty that (although thoroughly prepared, by familiarising myself with the extensive, Manchester-dominated anthropological literature on the South Central Africa) I would not have to go out of my way to derive a PhD-style research from the Zambian experience. However, when towards the end of my contractual two years at UNZA I contacted Boissevain to inquire how we would go about completion of the doctorate he had proposed, the political situation at the Anthropological Sociological Centre of the University of Amsterdam had dramatically changed, my fieldwork local supervisor Douwe Jongmans, after a protracted conflict with Boissevain (it was especially Jongmans's fieldwork training project in Tunisia that was under attack) and several abortive attempts to obtain a professorial chair for himself had left, first for the African Studies Centre Leiden, then for the Royal Tropical Institute, Amsterdam. Anyway, Boissevain had other promising senior students lined up to be his lieutenants. Also considering the fact that the duration of my initial Tunisia fieldwork had been sub-standard for an anthropological PhD (but my field assistant / interpreter Hasnawi bin Ṭahar and I had worked obsessively for nearly 20 hours a day, so that towards the end of my stay I had a complete qualitative and often quantifiable data set on kinship, descent, and pilgrimage in the valley, but also I was physically near collapse and had to be taken to hospital every day for vitamin-B injections), Boissevain deemed it 'not in my career interest' to honour his own initial offer. Perhaps he was right. My subsequent career has been distinguished; it was indeed (as in the title of the Festschrift my dear PhD student Pius Mosima prepared for me on the occasion of my 70th birthday) *A Transcontinental Career* more than it would ever have been had I remained a Mediterraneanist; I would not have missed for the world the personal encounters and the highly edifying experiences in sub-Saharan Africa; and I do not seem to have been permanently handicapped by the fact that, throughout the five decades of that career, I was dragging my unpublished *magnum opus* on Tunisia behind me like a convict an iron ball in a chain gang. Anyway, in the face of Boissevain's inconstancy there was no way in which I could affirm my unmistakable dedication to, and huge investment in, North Africa. My juvenile life since infancy had been built around the most bitter son-father disappointments, which caused me to be too much shattered by Boissevain's attitude to try and make him change his mind. Thus, I suddenly found myself with empty hands to pursue the next phase of my promising anthropological career. My first wife, the physicist Henny van Rijn, generously consented that I use the financial freedom which the generous completion-of-contract bonus at UNZA was affording us, thus extending my part-time fieldwork among the Nkoya urban migrants in Lusaka by half a year of full-time fieldwork in the Nkoya's rural home in Western Zambia. Left out in the cold by Boissevain (I would never be on speaking terms with him again), I found André Köbben prepared to arrange for me, practically overnight, a one-year writing stipend from the Netherlands Foundation for Tropical Research (WOTRO), and thus I ended up as a reluctant Africanist rather than as the passionate Mediterraneanist that the reader encounters at every page of this book. My nearly full-time Herculean efforts in the context of the Ḥumiri research, 1967-1971, in subsequent years merely yielded a trickle of lesser installments of detail arguments, a few poems, and a novel (*Een Buik Openen*, 1988). I repeatedly returned to my Tunisian texts so as to finalise them for publication, I even received generous financial assistance from my

main employer over the decades, the Leiden African Studies Centre, towards the translation of my original Dutch texts into English (by two very dedicated and competent junior colleagues, Arnold Isaacs and Riekje Pelgrim – as explained in a footnote below); but other paradigmatic and disciplinary passions intervened, and I am only after a long list of other books from my hand that now, after more than half a century, my two-volume monograph on the Ĥumiri fieldwork finally sees the light of day. (

The 1970 thesis, which is effectively published with the present Volume I, largely remained out of focus as a desirable publication project and never was touched by any outside translators, yet I found that Volume II is so irretrievably based on the social-organisational findings of Volume I that it could not stand on its own.

My (essential Durkheimian) strategy has been to try and grab the fundamental features of Ĥumiri society in order to make sense of the patterns of religious behaviour (shrine visits, other oblations, and ecstatic ritual, mainly) that constituted my declared research topic. A new generation of anthropologists such as Boissevain, Barth and Bailey (like, from a more materialist angle, Peter Worsley, 1956) had been deconstructivists *avant la lettre* (cf. Derrida 1996; McKenna 1992; Sallis 1985) – seeking to debunk the stilted, obsessively formalised, kinship-focused concern with social organisation that had been (British) social anthropology's hallmark since the 1940. When Adam Kuper, designated incumbent of the Leiden professorial chair in which I acted 1975-1977, read my thesis which Boissevain had supervised, his characteristically amused, condescending comment was more or less:

'A *bizarre*, unintegrated piece of mixture between classic social anthropology and fashionable non-group deconstruction à la Boissevain'.

However, my objectives in the present Volume One go further than Boissevain's revisionism, and are in part inspired by a different ideal: *to arrive at an exhaustive empirical description of social-cultural forms and behaviour with the aid of quantitative, statistical methods*. Both Jongmans and Boissevain had dabbled in mathematical and statistical analysis, including network analysis, but my main inspirations on this point were the work by M.J. Meggitt on the Mae Enga people of New Guinea (1965), and Elizabeth Colson's piece on 'The intensive study of small-sample communities' (1967). In the background there was the example of my first wife's then current biophysical research, whose quantitative orientation inspired some of the chapters in Volume I.

In retrospect, the major thrust especially of Volume I of the present book is *to deconstruct not the idea of social groups (in the vein of Boissevain), but to deconstruct the idea of kinship as a determining, automatic, independent variable in social life, especially in non-industrial and pre-modern contexts*. Yes, I cannot deny that Ĥumiri peasants are surrounded by neighbours who very largely may be considered their kinsmen; and that these peasants construct their social life world, their production and reproduction, their marriages and their religious actions, as if kinship is its overwhelmingly determining consideration. Yet the painstaking empirica, statistical and

mathematical analysis in the course of this Volume One's argument relegates kinship to a mere *emic*² perspective – underneath of which, at the *etic* level, and largely beyond the grasp of the participants's conscious structuring, more fundamental processes may be demonstrated – the blind (but mathematically reconstructible) flow of people and objects in all directions across the landscape. It is a great pity that Boissevain, while happy with his (characteristically light and incidental) supervision of this argument, did not recognise this potential to underpin, and improve, his own theoretical position; nor (with all his ostentatious dabbling in mathematical network analysis, often reduced to a sorcerer's apprentice at the hands of mathematically more accomplished associates such as his assistant Rudo Niemeyer) could Boissevain summon such appreciation for my exhaustive statistical analyses as (in my mind, then and now) largely compensated for the undeniable limitations of my fieldwork in more qualitative and time-dependent respects.

1.4. Acknowledgements; research strategy

I owe a great debt of gratitude to the hospitable people of Tunisia. The dedication and many-sided accomplishments of my research assistant, Hasnawi bin Ṭahar from the village of Mḥamdiyya were of invaluable significance for the project.

The greatest contribution came from my first wife, the physicist Henny van Rijn. It was she who enabled me to make this book a step, however small (and however regrettable, considering that it apparently implied the dehumanising of one's research hosts into puppets on a string), towards the realisation of a scientific dream that has captivated north Atlantic thought ever since Enlightenment astronomer and mathematician Laplace:

'An intellect which at any given moment knew all the forces that animate Nature and the mutual positions of the beings that comprise it, if this intellect were vast enough to submit its data to analysis, could condense into a single formula the movement of the greatest bodies of the universe and that of the lightest atom: for such an intellect nothing could be uncertain; and the future just like the past would be present before its eyes.' (Laplace, *Philosophical Essays on Probabilities*, originally published as *Essai philosophique sur les probabilités* (1814))

² By analogy with the linguistic distinction between *phonology* (studying speech sounds as consciously distinguished by native speakers of a particular language) and *phonetics* (studying the objective physical characteristics of speech sounds as recordable by any non-human device), the distinction between *emic* and *etic* lies at the heart of modern anthropology and intercultural philosophy. *Emic* is an ethnographic description that seeks to remain true (which in practice can be realised only partially, since ethnographic description is usually in a different language and semantic register from the participants's) to the local participants's own conscious conceptions and interpretations; whereas *etic* is an ethnographic description in terms of the ethnographer's own analytical and theoretical concepts, regardless of whether these have any recognisable meaning for the participants themselves. Cf. Headland *et al.* 1990; van Binsbergen 2003.

The idea of an exhaustive mathematical description soon also took hold of the emerging social sciences (e.g. Sorokin 1928), and was very dear to one of the founding fathers of British Social Anthropology, Radcliffe-Brown, with his book *A Natural Science of Society* (1948). In the anthropology of the 1960s, such ideas had considerable currency, not only in the abstruse, non-empirical model building of kinship theory and game theory, but also in the empirical description of real-life social behaviour through network theory, and as a methodological innovation of village ethnographies, from South Central Africa (Colson 1967) to the New Guinea Highlands. The collaboration with the late lamented Henny van Rijn, a physicist and my first wife (in whose work two-dimensional scatter of physical and physiologica phenomena was playing a central role), prompted me not only to base a great number of empirical generalisations about Ĥumiri society on crosstabulation and simple (usually non-parametric) but effective statistical analysis such as was well within my own anthropological competence, but to go beyond the empirical generalisation to mathematical model building, which brought out a fundamental pattern of *spatial organisation* that I could demonstrate to govern, in the Ĥumiri villages, day-to-day interaction just as well as marital choice, and the frequency and specific targetting of pious visits to the numerous major and minor shrines of the region at large.

Here also lies (in addition to Boissevain's inconstancy and Jongmans's possessiveness)³ another reason for the hilarious delay of over half a century which the present book has suffered: As part of my undergraduate studies I had received elementary training in statistics (although before my first fieldwork I had never had occasion to apply what I had learned, and multivariate statistics was not part of it), but the standards of

³ In all fairness, I should mention a third factor in the delay. My beloved and most inspiring teacher of fieldwork, the late lamented Douwe Jongmans (see van Binsbergen 2011 for an obituary), had been vulnerable and in the defensive for much of his career. He had initially been by-passed by for a professorship on the invalid grounds that his 1955 PhD thesis (on politics in the Polynesia) had been a mere armchair study not based on personal fieldwork; soon however Jongmans was to stand out as the principal teacher of fieldwork, on the ground of his extensive North African experience). Although he did have a number of publications to his name (of which I like best his 1964 synthesis on Libya), writing did not come to him easily. Ĥumiriyya had become his main research site, and considering his own paucity of publications he was inclined to shield that site off to an extent that bordered on the unscientific. As the principal director of the Anthropological Sociological Centre's fieldwork-training facility in Tunisia, he carried a heavy responsibility also in terms of public relations with the Tunisian counterparts and authorities – the country still remembered the humiliations of colonisation, and concepts of honour and shame, so eagerly studied by anthropologists, also informed the life world of the Ĥumiri and hence the fieldwork training, on many different levels. Even so, the fieldwork-training facility (time-consuming and expensive) was constantly under attack; for instance, my own extensive account of the facility and of my own first fieldwork in that framework (van Binsbergen 1987) was originally written, ca. 1969, for no other purpose than to help defend the facility against such criticism. For all these reasons it was understandable that students joining the project had to sign a formal contract to the effect that they would abide by the directives from the project leadership, and would not engage in any personal publications of their results. A decade later, a young colleague in a Netherlands social-science periodical would, not unjustifiably, sneer that this arrangement had turned Ĥumiriyya into Jongmans's private hunting grounds, and had resulted in so much valid social-science data going to waste. Whatever the legal merits of the contractual arrangement, it is true that I have always been under the impression that, in this particular context, I had no right to publish my results, which of course was in contradiction with the scientific ideal of universalism and free communication, and with the expectations of high-standard scientific results which the very same leadership gradually imposed upon us, students, as the fieldwork developed.

mathematical modelling with Bessel functions and differential equations which Henny brought to our daily conversations on my Ḥumiri material, taxed my mathematical abilities to breaking point; and when our relationship shipwrecked in the early 1980s, my most ambitious quantitative work on Ḥumiri had not yet been brought up to publishable standards. Henny was generous enough to sporadically advise me even after we had broken up, but it took many years, in fact decades, before I could resign myself to abandon my vicarious mathematical ambitions for the Ḥumiri material. Meanwhile I had met with great encouragement from a leading member of the Manchester School, J. Clyde Mitchell, whom I had met in Zambia and in Manchester, and who expressed his trust in my mathematical modelling. However, when conducting the final editing of the present Volume I, I increasingly came under the impression that the only way I could meaningfully conclude this book, was by radically deleting, for the time being, all modelling in terms of Bessel functions and differential equations; however, when I found that much of my indispensable quantitative data on actual interaction and on the marriage pattern had been presented in that Bessel-modelling chapter, I reinstalled that chapter and resigned myself to the impression of being, mathematically, the mere sorcerer's apprentice I could not help being anyway. Mathematicians and natural scientists will not fail to spot the many and severe limitations of the present Chapter 10, below – but at least the book's argument is not mutilated by missing out some of its most essential empirical data.

Ultimately, this fascination for quantification and model building turned out to be a dead end for me, both intellectually and emotionally. But whereas our marriage fell apart, and while I drifted away from ethnography, and from North Africa, and towards humankind's long-range intellectual history, comparative mythology, intercultural philosophy, and the global politics of knowledge, the care and enthusiasm with which I have tried to salvage for final publication, more than 35 years later, the products of our collaboration of the late 1960s and early 1970s, may well be seen as a tribute to a personal inspiration that has had a great formative impact upon me throughout my adult life.

Let it be clear that my adoption of statistical and otherwise mathematical methods in analysing my Ḥumiri data was *not in the first place* an attempt to emulate the professional natural-science perspective of my first wife. Strictly from an anthropological perspective, I was facing a very real dilemma concerning the quality of my data. In order to safeguard validity and reliability, the standard duration of fieldwork is in the order of magnitude of one full year, at least. My first spell in Ḥumiriyya was ordained, by the Anthropological Sociological Centre, to last only one third of that period. There were mitigating circumstances stretching the value of that short period: the research took place within a well-established organisational, logistic, and ethnographic framework, which usually, for a solitary fieldworker, takes at least several months to build up; I had acquired a certain knowledge of Arabic, even though the local dialect with its many Berberisms posed specific difficulties; and since my research area overlapped with the area of our student group's collective work during the first few weeks under the inspiring supervision of Van der Veen, I could benefit (after acquiring explicit permission from my team fellows) from the census and genealogical data collected in that connection. But even so my total data set fell hopelessly short of the requirements for respectable anthropological research – in that respect Boissevain's misgivings, on

second thoughts, had been understandable, even charitable. With such a restricted time perspective, I could not hope to retrieve extended cases such as were then, by the example of the Manchester School of anthropology (cf. van Velsen 1967), beginning to constitute the backbone of ethnographic description; nor could I hope to convincingly penetrate deeply into the semantic structure and existential significance of the religious concepts and practices that were being enacted around me, in the valley of Sidi Mḥammad. But what I was certainly able to do, was to construct exhaustive quantitative data sets on easily recordable and observable aspects of Ḥumiri life, and analyse these to the best of my ability: data on residence, marriage, genealogies, actual secular interaction, concrete ritual behaviour. This is the refuge I found, out of the dilemma which the short duration of my fieldwork was posing. I learned, by experience, to rely on a rule of thumb that was forcing itself upon me early in my research career: *in research, do not waste any data, save and analyse even the tiniest scrap of information, and keep considering and reconsidering it from every possible angle, until it finally yields (often with the unmistakable flash of Aha-Erlebnis) its hidden gem of insight; remain true to this attitude especially when confronted with apparent contradictions in the data – these are often the points where the greatest gain of insight is to be expected, if only one can summon the required patience and endurance, and the resourcefulness to try and invent new specific methods to solve the difficulties that manifest themselves.*

It had been an old truism of classic fieldwork anthropology as established in the Interbellum, that the key to a local community is the combination of a census, a village or ward map, and a genealogy, and I – sacrificing almost three years full-time on the exercise – made the most of these humble types of data, tracing by hand (personal computers were still more than a decade away) the consanguineal and affinal ties that linked each villager to every other one, measuring by hand distances between every compound, tracing marital relationships in a bid to identify unexpected *connubia* and avoidances... The present Volume One is largely built upon data of that kind, and its result is a rather novel view of social life: not as the enactment of tyrannically immutable institutions in the fields of residence, kinship and marriage, but as an incessant social process of flow in which people and things travel in increasingly identifiable and modellable patterns all over the valley of Sidi Mḥammad, up the imposing mountain slopes that all but seal it, and again down into the adjacent valleys. What started out as a salvage strategy, was turning into an eye-opener.

1.5. Further details

The rendering of place names poses a particular problem in scholarly writing dealing with the former French Maghrib. Distorted and unsystematic transliterations of the Arab names appear on maps and in the literature. I have already mentioned the difficulties presented by the French colonial designation 'La Kroumirie'. Another problem relating to place names is that, for profound structural reasons which will become clear in the course of my argument, the same name may apply to a locality (valley, neighbourhood), a residential unit, a kin group, a saint and a shrine. The

awkward repetitions in the text resulting from this condition could not be avoided. Apart from proper names and place names, the Arabic words used in this study are all rendered in the singular, with plurals sloppily indicated by -s. The simple transliteration system that has been adopted after Gibb & Kramers (1974) inevitably obscures may orthographic and phonetic distinctions.

By 1970, the *lingua franca* at Netherlands uiversities was still almost exclusively Dutch, so my theses were written in my mother tongue. By that time, English had already eclipsed French and German as the Western language of international academic communication, and when I began to seek publication for my H̄umiri research in book form, it was imperative to have an English translation. When, after acting in the chair of African anthropology at Leiden University, 1975-1977, I was invited to join the African Studies Centre, Leiden, and to give up my preference for a part-time appointment in favour of a full-time one, I accepted on condition that my work on the North African manuscripts would be institutionally facilitated. Within a few years, my assistant Arnold Isaacs did produce a draft translation of *Volume II, Cults of the Land, and Islam*. Nearly twenty years later, when publication had still not materialised (in the meantime I had carried heavy administrative responsibilities as one of the two scientific directors of the Centre, struggling to see it through a time of crisis and reconstruction) another assistant, Riekje Pelgrim, was employed to finalise the manuscript of the same Volume II. Volume I remained out of sight throughout this long period. I am greatly indebted to both Isaacs and Pelgrim, and the African Studies Centre, for spending so much time, money and skill on what more and more began to look like an abortive publication project; ad I am delighted that, with the present book, these efforts are finally coming to fruition.

Of course, half a century is a very long time in the world of modern scientific production, and today the issues of the present argument have largely sunken behind the horizon of current professional interest. Still, virtually all human reproduction takes place in kinship-dominated domestic contexts, kinship therefore still constitutes one of the mainstays of social organisation, and still deserves to be studied in its own right – even if gone out of fashion, both as a scientific subject and as a conscious concern of present-day members of North Atlantic society aged twenty to forty. Constituting an enduring legitimate research concern, an enormous amount of work and reflection has been invested in the present Volume I, in the background it has informed much of my later work on ethnicity, identity, ideology, and thought processes, and it is sufficiently dear to me to justify the present attempt to revive it. Finally, then, my two-volume monograph based on my North African research will at long last see the light.

Fifty-five years have passed since I embarked on this two-volume study, which at the time constituted the bulk of my graduate work in anthropology at Amsterdam Univer- sity, 1967-1971. Most scholarly careers span a substantially shorter period. I hardly recognised this work any more as my own writing; over the decades, my writing style has (hopefully) considerably improved, my dexterity at handling apparent or real contradictions in data and theory has grown, the flow and rhythm of my academic prose have become far broader and sustained; and it does not help that these texts, originally written in my native Dutch at a time when I still considered literary writing my main destiny, have passed through the hands of two dedicated translators (both

competent anthropologists / historians, but both essentially non-native speakers of English and inexperienced as translators) before being finally subjected to my own English editing again. What is more, the dazzling succession of paradigms and theories (not in the last place: in my very own hands), and the complex international developments informing our perspectives on Islam (two international oil crises, the Palestinian insurgency, the Iranian Revolution, the rise of militant Islam, '9/11', the so-called War on Terror), and on local social organisation (the rise of globalisation, the vicissitudes of postcolonial societies in Asia and Africa, the spate of transcontinental migration and the growth of multiculturalism) since the late 1960s means that most of the methods, viewpoints, and the theoretical concerns of my graduate work would now appear to be obsolete. Nonetheless, the detailed ethnographic (including quantitative) study of North African popular Islam has made amazingly little progress in the last half century, and my work therefore may be suspected to still retain some limited relevance and topicality.

Of course, even though I was increasingly preoccupied with other topics (migration, state formation, ethnicity, sub-Saharan Africa, globalisation, intercultural philosophy, African-Asian continuities, comparative mythology, the Bronze-Age Aegean, etc.), yet ever since the late 1960s I have tried to keep in touch with the literature relevant for the present two-volume publication project. Now that I have finally come round to prepare my Tunisian research for publication, I am looking at over 5,000 bibliographic references that, ideally, need to be worked in these two-volume books – a project that is likely to take many months. Considering the rapid pace with which the social sciences and the study of Islam have developed since the late 1960s, it is virtually impossible to bring my argument up to date without essentially destroying it in the process. My advanced age, and increasing health issues, have been warning me that the end of my productive academic life will gradually come into view. Therefore I do not wish to postpone the publication of these books until such time when I shall have exhaustively digested this endless volume of bibliography, and turned it into footnotes and other textual amendments. Instead I propose a two-stage operation:

1. let me first bring out a provisional first edition of the two volumes on the basis of the (heavily reworked) original texts of 1970-1971; and then, when time allows and my other more pressing commitments are largely out of the way, let me
2. bring out a second edition, updating the 1970-1971 arguments with well-referenced amendments in the light of scholarly developments ever since.

Meanwhile, I feel more comfortable if the raw bibliographic material on which I will be working towards (2), is already shared with my readers in the provisional form of a webpage. This is the purpose of the 400-plus extensive table, uploaded to the internet as:

van Binsbergen, Wim M.J., 2022, Materials towards updating my 1967-1971 study of Religion and social organisation in north-western Tunisia, Volume I: Kinship, spatiality, and segmentation, Volume II: Cults of the land, and Islam, Papers on Intercultural Philosophy / Transcontinental Comparative Studies, Hoofddorp: Shikanda, at: <https://www.quest-journal.net/shikanda/Berber/PREVELEMENTEN.pdf>

That webpage is not meant, of course, as anywhere near a finished product that can

stand on its own, but rather as a statement of intent. Needless to stress that its contents is copyright material, (c) 2022 Wim van Binsbergen, and meant for later publication under my own name.

Chapter 2. First fieldwork

On popular Islam in the highlands of North-western Tunisia, 1968

basic English translation by Susan Jansen; my editing

2.1. Preparation

Scattered throughout the landscape on the 200-kilometre-long drive from Tunis to the capital of Tunisia's Ḥumiriya region, I did indeed see the white domed shrines that, as centres of the cult of saints, had played a major part in my research planning back in Holland. If you are all set to study the popular religion of North African peasants such a confirmation is quite welcome when the long-distance taxicab, in which you are sitting crammed tight with five fellow-students, is driven headlong around hairpin bends from the open plains into wooded country bearing an uncomfortable resemblance to the surroundings of Central European holiday resorts visited in childhood. The cold fog turns into rain and we find ourselves amidst the hotels and public buildings of yet another *déjà-vu*: ^cAin Draham.

After having sweated, with varying interest, over foreign language publications on socio-cultural phenomena far and near for three and a half years, without having done any empirical social research as yet, and with mounting doubt that my training within the field of anthropology was a suitable preparation for such research (or for anything at all), I was at long last allowed to take part in the research-training project that the University of Amsterdam had established in North-western Tunisia.

During a preparatory period of half a year the six prospective participants had had ample opportunity, at weekly meetings, to get acquainted with the team of four anthropologists that would be in charge: an experienced North Africanist; a younger lecturer whose brilliant, virtually completed, dissertation about India was merely proof

to us that he could not in any way be knowledgeable about North Africa; and two teaching assistants who had already gone through the North African baptism of fire. In that preparatory group, we brushed up our French. We also discussed some relevant general literature, which mistakenly confirmed our suspicion that there was hardly any anthropological information available about the research area. In those days the common anthropological orientation was still quite mono-disciplinary; for instance, one hardly searched for historical sources concerning the site of one's anthropological fieldwork. We did not receive any training in how to identify and use bibliographies, archives, etc. Besides the physical effects caused by our first tropical injections, doubts about what would be the best equipment, and financial worries (the grant from the University would turn out to barely cover half the costs), the fear preyed on our minds that we were not preparing ourselves in the best possible way for the research project – neither academically, nor for the living conditions and the expectations of the local population regarding our behaviour out there. And so the physical and mental tribulations that – as our South Asianist kept emphasising gleefully – would afflict us with growing intensity once in Tunisia, began before we left Holland.

Much time was spent in discussing these problems. Those in charge (the senior North Africanist Douwe Jongmans and the more junior South Asianist Klaas van der Veen) convinced us (for a few hours) of the fact that they were truly unsolvable. The research-training project had opted for an individualistic set-up in the sense that, after a few weeks of collective work, each participant would be in his own village, dozens of kilometres apart from the others, and that each would be wholly responsible for his own research. Inevitably this implied enormous uncertainty, and the pretence of more effective preparation would not alter that in any way. Unforeseen contingencies would occur up to the very last day in the field. All of this did not, however, relieve us of our obligation of writing a detailed research plan before departure – and just as well because, once in the field, that research plan turned out to be extremely useful.

The human-relations aspect concerned us most of all. What the team of supervisors and previous participants told us became so distorted in our minds that during those last weeks before setting out for Tunisia our future informants, and especially the interpreters who had been recruited for us, came to appear as double-dyed liars, not to be trusted in anything; from the first moment of contact they would be expected to be only interested in our money and our equipment; they would be exceedingly unsavoury in all their manifestations, and capable of lapsing, at any given moment, into the sorts of acts of violence that had characterised the highlands on the border between Tunisia and Algeria before the colonial conquest (1881) – and which had even been the pretext offered for that conquest in the first place. As amongst novices in seclusion, the night before their initiation, the most terrifying rumours circulated.

To top off the preparatory stage, shortly before our departure we were presented with an elaborate schedule of our obligations regarding the reporting and processing of materials after the fieldwork; whereas until then any possible results of our research had been played down as unimportant.

Perhaps I was the only one who spent that last night in Holland delirious and vomiting. Perhaps it had to do with those last injections. At any rate, during my first intercontinental journey (at that time still by car and boat) to my first research location, much of this anxiety had given way to a certain touristic excitement, followed

by weariness and slight disappointment. Our initial group accommodation did not exactly contribute to making the anthropology student's dream come true: a small apartment (ugly and dreary as any comparable concrete building in Holland) was to house the six participants and all of their luggage for the first few weeks. Someone had been hired to do the cooking and the cleaning. The supervisors stayed in a nearby hotel, in what we suspected to be incomparable luxury.



Fig. 2.1. Mrs Mabruka mart' Aissa (1979), my nearest neighbour in Sidi Mḥammad, the mother of my landlord, and despite her poverty one of the leading women in the village

2.2. Collective on-site preparation

Gradually, in well-calculated doses, Ḥumiriyya and its inhabitants are set loose upon us. We meet the first interpreters, who live in villages in the vicinity. Contrary to our expectations, they turn out to be well-dressed, neat, intelligible and friendly. On two fascinating walks around the best known shayḥdoms (the smallest administrative unit in this region) the project leader opens our eyes to the ecology of the mountain region and its socio-cultural consequences. No more enjoying the scenery: even the most magnificent valleys turn into 'social/economic/political units determined by natural constraints', woodlands left intact 'indicate the absence of springs' (the land would have otherwise been cultivated), the signs of erosion (large parts of the mountain slopes denuded of trees, their soil exposed like open wounds by progressive landslides) are not picturesque but tragic.

The distance of non-commitment gives way to the beginnings of participant observation.

Then it is time for our first independent exercise: groups of two students and an interpreter are formed, each to map a section of Hamraya, an extensive village about two and half miles from ʿAin Draham. Hasnāwī *bin* Tahar, the eldest interpreter at thirty-nine, will work with Pieter Tamsma and me in a part of Hamraya where he had lived the previous year with one of our predecessors, Guus Hartong.

The interpreter leads us up the mountain slopes at a rapid pace. At the edge of the forest we manage to take refuge, for half an hour or so, in a discussion about the symbols to be used on our map. The interpreter gets bored. Then the terrible moment comes when we finally have to step into Ḥumiri village society on our own account. Tamsma volunteers to map the highest part of the village all by himself. Hasnawī and I will focus on the lower compounds. Stumbling, I follow the interpreter into a farmyard, where he calls out to the invisible occupants, and I frantically start to pace the area for measurements while taking notes and avoiding the gaze of people appearing in a doorway. My intention is to pretend that these lonely activities are very absorbing and constitute a matter of course to me, but the feeling that what I am doing is completely insane in the eyes of the onlookers, as well as my own, gets stronger and stronger. In the end I find myself standing on a large jutting rock about sixty feet from the farmyard in an expert observer pose, but I seem to be unable to create on paper a coherent pattern out of the tangle of roads, paths, clusters of trees, huts, small plots of land, the brooks down below and the wooded slopes in the distance. I break into a cold sweat. You see now: even at the first, most simple, attempt I give myself away; I am not an anthropologist at all and will never be one

Our South Asianist drops by and takes some of the tension away. Tamsma returns, and we are invited into a house and drink strong sweet tea. Once the people stop being faceless it actually proves possible to carry on a simple conversation through the interpreter. Ignorance of what is considered polite here does not in the least lead to immediate catastrophes: interpreter and host obligingly enlighten us. The name of 'Msjeyer – Monsieur – Goos' (Guus Hartong) is mentioned; as 'brothers' of 'Goos' we are invited to continue his friendly relationship with the residents. The spell is broken. When we return the following day, making a map no longer poses a problem.

Then it is time for collecting census data and genealogies in the same village. Our interpreter is closely acquainted with the inhabitants and has had ample experience with the Dutch students' weird interest for long-deceased or migrated relatives. And sure enough, to our great satisfaction our informants dish up genuine and elaborate genealogies without any problems. The most impressive sections of our textbooks thus come within our reach. Because gathering this kind of information turns out to be so easy, and because we are so flabbergasted at seeing it all work, we forget that these interviews are rather tiresome for the people involved. And we are tongue-tied as soon as the conversation takes a less standardised turn. The informants are, however, very patient. And Hasnawi is very talkative in our stead. One of the interlocutors (already we exclusively refer to them as 'our informants') starts telling us about the history of his lineage and with bated breath we jot down our first real myth of origin.



Back row: from right to left al-Hedi bin ʿAissa (blacksmith and flute-player), ʿAbd Allah bin ʿAissa (blacksmith, UNO Congo war veteran, village non-conformist, and my landlord), Shadli bin ʿAissa, and their FBS from Fidh al-Missay ; front row: centre: Muḥammad bin Seffi (with notebook), and four of the sons of Shayh Hassuna bin Bu Aziz

Fig. 2.2. A group of young men from Sidi Mḥammad who formed my day-to-day companions during the 1968 fieldwork

Every day interviews are conducted or processed. And at nightly meetings the South Asianist (the North Africanist has gone back to Holland for a short while), impressively extemporising, points out the social-structural principles that can already be observed in our modest material and lectures us on various technical aspects of the fieldwork: dealing with the interpreter, interview techniques, taking notes, processing data systematically, recording every observation, incident and conversation into the day journal, etc.

All of this proves a great stimulus to our analytical enjoyment. We work hard and keep to strict timetables. We continually seem to be in a hurry. A few participants come down with a heavy case of research fever and keep on struggling with lengthy genealogies till the early hours of the morning, tallying up marital relationships. Why do their girlfriends have to be so far away for so long

The first letters from Holland are long in coming. And at night it is bitterly cold in the students' apartment; everyone develops a cough. Our hesitant attempts to relax seem to be looked upon with suspicion by the South Asianist and his assistant; our only day off in three weeks turns into an over-collective and over-directed school outing to the ruins of Bulla Regia, an old Roman city. Being in each others' presence continuously, night and day, starts getting on our nerves. There is hardly any co-operation or exchange of information between the twosomes, as though we are already real anthropologists, protecting our own little fieldwork area, our own data, and as though conflicts only remain suppressed by virtue of that 'avoidance', a classic anthropological concept after all.⁴

At the end of the last collective interview day all the participants and interpreters walk across the densely-forested slope to the VW van that will bring us back to 'Ain Draham. Due to what I have mistaken to be the local taboo on all reference to bodily functions, I decided to lag behind unnoticed, and the next thing I know I have lost all sight of the group. I follow the path down, running where possible, but do not catch up with my fellow researchers. When I ask a young boy if he has seen them, he of course does not understand what I am saying and directs me back up the mountain. Soon there are no paths any more; I am walking through an overgrown clearing. I am lost. And at that moment I realise I have not been alone for even one second in the past three weeks. Some of the weight of having been compulsively preoccupied for months now with the research and its preparation is finally lifted from my mind – I am in a beautiful oak forest, a brook with strange red foam flows alongside of me, there are nice little birds. I relish the silence and delight in being a tourist once again for just one moment.

But it is getting dark. I have neither map nor compass and think of the enormous wild boars that allegedly roam these forests. In my mind's eye I see supervisors, students, interpreters and inhabitants of surrounding villages desperately searching through the night; a disaster for the whole project. I call out. After a while I hear the horn of the van being honked in the distance below, and moving towards the sound I pretty quickly meet up with the group again.

⁴ 'Avoidance' designates a mode of highly elusive and restrictive behaviour of individuals belonging to social categories between which strong structural tensions exist, e.g. son-in-law and mother-in-law.

A beaming Hasnāwī claims to have saved me: he was the only one to notice that *'Msjeyer Weem'* was not there when they were just on the point of driving off. He is the interpreter assigned to me. He will live with me for three months in a one-room house of five (!) square meter ground area.

2.3. First days in the village

My lodging had taken quite some doing. Popular religion was seen by the Tunisian authorities as a painful symbol of the backwardness of their country since it varied considerably from the formal, although at that time rather elastic, Islam advocated by urban religious leaders. So my research subject was delicate, and the supervisors had selected an area for me to work in a shayḥdom which had been on very good terms with the project right from the start. Several students had done fieldwork there in the previous years and the population had found that research was nothing to be feared.

The area designated to me consisted of two villages situated one above the other on a mountain slope and separated by a stretch of uncultivated land. In the lower village there was a large shrine dedicated to one of the most important regional saints, Sidi Mḥammad, from whom the settlement took its name. The upper village was called Mayziyya. Another participant in the research project, Coen Beeker, had researched residence patterns in Sidi Mḥammad in 1966, building up excellent relations with the inhabitants, which he still maintained by sending letters and parcels. As he had paid hardly any attention to Mayziyya, it seemed reasonable that I should focus on that village in particular. A small house for me to live in had in fact been found in Mayziyya long before my arrival. However, immediately before I was to take up my residence in the village, it appeared that this dwelling had been deemed not impressive enough by the local branch of the Tunisian unitary political party, which had then proceeded to select a house for me on the outskirts of the village of Sidi Mḥammad. By local standards it was indeed grand, with a decent roof, a good lock on the door, a large yard, a clear view of the major shrine of Sidi Mḥammad (he turned out to have four shrines within a radius of two kilometres) and of the Mediterranean, twelve kilometres away. To accommodate his family, the owner had no option but to build a shelter from branches and leaves elsewhere on his land. This man turned out to be, of all people, ʿAbd Allah bin ʿAisa, the one person Beeker had particularly warned me about. I had got the most controversial figure in the village as a landlord, someone who cared little for the traditional rules, and who was the only avowed antagonist of the local shayḫ (not a religious figure here, but rather a kind of mayor). This was the man who had been forced to vacate his house for me. Even our South Asianist could not do anything about it. However, although I was intimidated by ʿAbd Allah's big body and strong blacksmith's arms, exceptional directness of speech and compelling attitude, he turned out to have qualities of sincerity, rebelliousness, and humour that greatly endeared me to him in the long run.

With other lodgings I undoubtedly would have produced different results. Not so much on account of the disappointment of the inhabitants of Mayziyya (who would also have liked to receive letters and parcels but now had to realise that all the benefits of my stay would once again go to the village of Sidi Mḥammad) or my landlord's peripheral position in the village, but rather because with him, his mother and his

brother (under quasi-kinship obligation to me because I too lived on the late ʿAisa’s land), I now proved to have some extremely intelligent informants, who had intimate knowledge of popular religion and were devoted participants in all the attending ritual activities.

My first weeks in the village were nowhere as bad as I had expected. The accommodation and lack of *modcomfs* were no problem at all: of course there was no gas, electricity or running water, nor a toilet or a shower, but all in all it was less primitive than any hike through the mountains in Europe. That my informants were able to lead a complete life without all the material achievements of my own society, and that I could adapt myself to a fair degree to their situation, I found almost edifying after a while. And besides, Hasnawi saw to it that I had my cup of plain, ordinary, Dutch light tea in the mornings – a despicable beverage in Ḥumiriyya where tea is supposed to be black, strong, syrupy and extremely sweet. He was eager to please and to show his expertise – after all, it was his third lucrative year as interpreter for the research project.

My landlord actually seemed quite sympathetic; although it did get very much on my nerves that he had to observe closely in all detail, the very first day, whatever I was going to unpack in the way of kitchen utensils, office equipment and provisions. His house was decorated with colour photographs cut out from the popular weekly *Paris Match* (which hardly anyone in the village could read), notably a photo report of the coronation of the last Shah of Persia, and a series of cheerful photographs of a girl in varying states of undress showing what one can do with camomile prepared in various ways – washing one’s hair, grooming one’s face, etc. I gladly left them on the walls. They were a fitting preparation for the female breast improvement advertisements (from a similar provenance) I was soon to admire in the major shrine of Sidi Mḥammad, pinned up between the sacred flags and votive candles, right above the tomb of the saint himself.

My first scoutings around the village and surroundings yielded a wealth of fascinating information, because besides being a European and a prospective anthropologist, I was an ignorant city-dweller to boot, without any knowledge whatsoever of farming. After a few weeks those nice green blades of grass that I enthusiastically wrote home about, turned out to have developed into stalks of rye and wheat. Everywhere I only met friendly people. I made a speech in the local store-*cum*-men’s assembly, in which I held forth on the close ties that connected me with ‘*Msjeyer Coon*’. Everything I said was well-received, even when I told them outright that I was interested in the local saints and their veneration. They would help me with everything, they promised, and that is exactly what happened.

As for the reaction of the population, I experienced only one really anxious moment. On the morning of my second day in the village an official of the local unemployment relief work organisation (the villagers’ main source of income) came along to my little house to see Beeker’s mimeographed fieldwork report, which happened to be in Dutch. The pages at which this much-feared official opened up the report contained, to my horror, tables with names of villagers and amounts of money. These tables were an innocent statement of the amount of rent they would be willing to pay if they had to move to a newly constructed village. But the official could not read Dutch (the days of massive migration from North Africa to the Netherlands had not yet started), and I

feared he would misconstrue the report as an indication that I was there to serve some sinister political end. It was just these kinds of complications that the supervisors had explicitly warned us about! I realised only years later that what had prompted my visitor to beat a hasty retreat was probably not his suspicion that I had sinister intentions compromising the security of the state, but the threat of me being in the service of some higher-order officialdom which these tables represented to him.

Later that day I began the interviews. Fairly soon I got into the habit of beginning each conversation with a new informant with questions about census data and genealogies. In this way I could find out about someone's qualities as an informant in an area where he or she could easily supply me with answers without becoming insecure or suspicious. I had worked on these kinds of questions during the preparatory village survey at Hamraya, and could keep the conversation going even though I had hardly any sensible questions to ask yet about religion. In this way the informants got used to the interview situation (in so far as this was still new to them, after Beeker) and to talking through an interpreter while I, as an unexpected bonus, gained insight into the complicated kinship structure of Ḥumiri society. In the course of the research these auxiliary data proved to be more and more relevant to my main subject. Usually these genealogical exercises developed into more religion-orientated conversations after about half an hour.

I resolutely forced myself always to walk around with my notebook, bring it out and take notes, necessary or not, ridiculous or not. Within a few days everybody had become so used to this that no attention was paid any more. This hopefully eliminated the danger of my informants being able to tell from my occasional excited scribbling which spontaneous statements or actions aroused my interest, with undesirable effects on their remarks and behaviour. At the same time it also provided me with a concrete opportunity to identify with my researcher's role, which helped me to overcome a lot of diffidence.

I would continually stumble upon new aspects of the religion. I explored the first few of the dozens of shrines that I was to find in my immediate research area. After one day I was already allowed to witness a ritual slaughter and distribution of meat in honour of Sidī Mḥammad. The high point of those first days was a séance (I was to experience many more) during which the local representative of the Qadiriya brotherhood (widespread throughout the whole Islamic world), went into a trance accompanied by singing and flute and drum music, and manipulated cactus leaves with enormous spines without hurting himself, as if he were rendered invulnerable by the invisible saint that came to possess him in his trance. I was deeply moved by the experience. That night I wrote in my journal:

'If I will be able to penetrate into the conceptual world and the motives behind all this, my stay here will have been worthwhile.'

(It proved to be just that.) My lack of interview technique hampered me more than the much-feared reticence of my informants. Even the women turned out to be surprisingly approachable. It only took a couple of weeks before our interviews with them no longer needed to be chaperoned by elders. After the first ten days I was already under the impression – completely unjustified of course – that I was beginning to comprehend somewhat the cult of saints and shrines in Ḥumiriyya.

In those same deceptively euphoric first weeks, however, my main research instrument, my relationship with the interpreter, was almost irreparably damaged.

2.4. The interpreter

The dangers of getting into an over-friendly and over-relaxed relationship with one's interpreter had been stressed to such an extent during the preparations in Holland and ⁶Ain Draham, that I eagerly – in this respect it was at least clear what I had to do – apply the Western, businesslike, virtually impersonal relationship model:

'He is being paid comparatively well to do this job, and that is that.'

I myopically and stubbornly saw Hasnawi as a needlessly complicated instrument to amend certain bothersome, yet seemingly minor, shortcomings in my communication with the informants: the mere fact that, in spite of having studied Arabic for one and a half years, I neither understood their dialect nor had a clue as to their customs and manners. I refused to admit my total dependence on Hasnawi (though it had been over-emphasised by the supervisors), not only in terms of the language, but in fact at every step I took. And when he alluded to it (emphatically confirmed by the supervisors in his sense of being utterly irreplaceable), I flew off the handle. I accused him, sometimes even in the presence of others, of not translating everything that was being said. That he should be allowed to decide for himself what was relevant enough to translate, never entered my mind – I did not realise that the conversations we took part in, outside of the interviews, were generally of the same silly and diffuse nature as conversations in the pub, the launderette or the doctor's waiting-room in Europe. We did not come to any normal exchange of views about the organisation of the research (*my* research) and of our stay in the village of Sidi Mḥammad. And so, while the informants gradually began to appear to me as the most fascinating and sympathetic people alive, an unbearable tension developed between Hasnawi and myself that expressed itself numerous times a day in peevish or quarrelsome remarks, alternated by irritated silences.

And that despite the fact that the poor fellow was forced to abandon his house and compound, cow and wife in order to work himself to the bone for a pittance, with only one day off a week, one-and-a-half hour's walk away from home under the unsteady guidance of someone young enough to be his son – and work, not only as a translator (which is tiring enough) but also as a cook, cleaner, informant, PR-man and singer-musician (a specialty of Hasnawi which came in handy in the religious sphere). Even more importantly, whatever stressful burden I laid upon our working relationship, his culture demanded the most far-reaching identification between people who work, eat, drink, sleep and spend their spare time together, with a continuous exchange of gifts and services, cordialities and confidences. We had to be 'like brothers', or at least appear that way. Hasnawi surely had reason to complain, and that is exactly what he did in all tones of voice.

My informants of course did not fail to notice the tensions, and several marginal characters from the village (among them my landlord, *i.e.* my most important contact in the village next to my interpreter), aspiring to the lucrative and seemingly cushy

job, came to defame Hasnawi when he was not there. I decided I had to get rid of him as soon as possible. What use was he to me anyway?

Fortunately our South Asianist was able to intervene just in time. His general anthropological insights, his fieldwork experience, and the way in which he applied these assets in his organisational contacts with the local society, more than counter-balanced his lack of specific knowledge about Ḥumiriyya, as I began to realise. In a number of heart-to-heart talks my shortcomings were made quite clear to me, as well as the fact that I would have to get along with Hasnawi anyway, since one was not allowed to change interpreters.

More than thirty years later, Hasnawi's way of behaving and his idiosyncratic French vocabulary have continued to be standing references, cryptic to others, in the family that I since raised. Occasionally I still have nightmares about him, even though I have meanwhile written the novel (*Een Buik Openen / Opening Up a Belly*, 1988) that details the story of our collaboration; here the interpreter appears in a much more positive light than the arrogant, culturally insensitive young researcher.



Fig. 2.3. Mr Al-Hedi bin 'Aissa (1979), blacksmith and the most accomplished flute player of Sidi Mḥammad (indispensable for the performance of ecstatic dances), my close neighbour and brother of my landlord

2.5. Days of distress

After several weeks I had quite gotten over my initial exhilaration. The conflict with my interpreter had taught me (at least that is what I read in it) that fieldwork requires the researcher to be aware at all times of his own actions and of the premises on which they are based, and perpetually to keep track of how his presence influences the relationships of the people around him. These are inhumanly arduous demands, especially at the outset, when new impressions so overwhelm the researcher that he can barely take any distance from himself, can hardly predict how his behaviour will be interpreted, and is as yet unable to assess fully in which respects the society he is researching allows him to be himself, to have his own opinions and preferences, to say 'no' when he does not feel like doing something. Human life and living together require a minimum of distance, knowledge, predictability and routine. With these one has a grip on reality, and the possibility of behaving spontaneously, relying on behavioural automatisms, and being happy. I saw myself voluntarily deprived of these basic conditions and placed in a kind of laboratory simulation of the genesis of neuroses.

This had little to do with culture shock. Apart from the bloody slaughter of sacrificial animals, Ĥumiri society failed to shock me. Coming from a family utterly shattered by internal conflicts and both sexual and physical abuse in a popular neighbourhood of Amsterdam, I was not exactly handicapped by love for the dominant, bourgeois customs of my own European society – into which I had not been effectively initiated until I went to grammarschool. As an adolescent I had had the same problems of disorientation and despair *vis-à-vis* Dutch society that I now had amidst these Ĥumiri peasants. Having just turned twenty-one, I was experiencing an accelerated second puberty, and it was even more painful than the first time around.

If there was a lack of distance between me and my hosts, this was primarily due, not to Ĥumiri notions of privacy differing from European ones, but to my own personality, and aggravated by the professional expectations to which I considered myself to be subjected. I only realised much later that my self-imposed cramped defencelessness in the field was to a large extent due to my taking too literally the advice given by the supervisors. It was neither them nor my informants who made exorbitant demands, it was me. I saw my fieldwork as a Spartan learning strategy for humility, patience, improvisation and living with insecurity, defencelessness, and lack of privacy. I felt I was continually dancing to the tune of my interpreter and informants, and yet still doing everything wrong. And for the first time in my life I experienced the extreme loss of ego that was henceforth to be the characteristic state of my personality in subsequent fieldwork: deprived (even more than when at home) of a sense of self-protecting boundaries around me, I learned extremely fast and without inhibitions, delivered myself wholesale to the host society without holding back any thought of self-interest (sleep, privacy, cherished beliefs), and thus learned the local culture and language at an incredible speed – but at an extreme cost.

Nothing went smoothly. My feverish attempts to discover and adhere to certain rules of interaction did not, at this stage, arise from respect or admiration for the society in which I found myself. I merely wanted to get rid of that paralyzing insecurity and sense of rejection and outsidership. Every word I uttered and every gesture I made, for

weeks, was consciously perceived by me as falling far short of local expectations and norms; the embarrassing stammer of my childhood came back, and I was so conscious of my every movement that my gestures became broken, like a robot's. And for weeks my every word and every gesture would be consciously aimed not so much at getting information (which gradually seemed to become less of an obsession to me) but above all at making myself acceptable in the eyes of my interpreter, informants and the team in charge of the training-project. I derived absolutely no satisfaction from my contacts in the village. I was just playing at dealing with people, but it was a terribly difficult and disagreeable game to me, and I constantly had the desperate feeling of being incapable of ever achieving any real contact with what was, after all, my immediate environment for the duration of the fieldwork. This absence of intimacy and spontaneity was all the more distressing because, except for the few minutes each day when I washed or when nature called, I was always surrounded by people. Even at night there was still the bodily presence of Hasnawi, one metre away, snoring or calling out in a nightmare, instead of my girlfriend.

For several days I experienced almost total distress. I had completely lost my sense of motivation; my research data seemed utterly worthless and meaningless. By now I knew by heart the standard commonplace phrases in which the local people described their religion:

'We ask the saint and the saint asks Rabbi [God]'

'the *baraka* [the divine grace emanated by the various shrines named after one and the same saint] is the same but we visit them all', etc.

But I felt I had no insight into the system. My interview technique and my experience with analysing conceptual systems were as yet far too inadequate to draw out what was not exactly unconscious, but rarely or never needed to be put into words in normal, day-to-day life, even among the most intelligent informants. Instead of being an engaging interlocutor, I was at a loss to bring up new interesting topics at crucial moments in the conversations. And as soon as an interview seemed to be going the right way, I nevertheless irritated people (including my interpreter) through my lack of understanding of the basic social codes of their society, and my diffidence and inability to use their cultural idiom. Citing examples from daily life in Ḥumiriyya, interspersing one's conversation with kinship terms, the name of God and the Prophet, profusely wishing people good health – I did not yet know how to make use of all that. However detrimental it was to the progress of my research, I was really completely tongue-tied at times, literally unable to utter a single word in whatever language, and (like at the time when I was a six-year old boy) reduced to a ridiculous stammer. My ears were ringing with the loud voices in that still almost unintelligible Arabic dialect, and I often could not see a thing in the dark huts, let alone recognise faces or take notes.

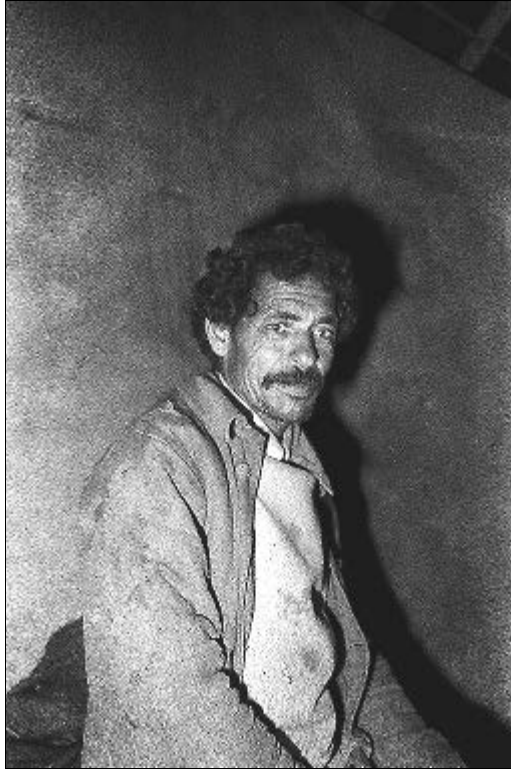


Fig. 2.4. Mr Rabah bin 'Ali (1979), the main warden (*ukil*) of the shrines of Sidi Mḥammad, and a major *fakir* of the religious brotherhood of the Qadiriyya

I just wandered aimlessly around the village with Hasnawi. Occasionally my depression was quite apparent to my informants – who were far more perceptive practical psychologists than I was. The heavy rains and the mail that failed to arrive greatly contributed to my despair. More than once I toyed with the thought of dropping the whole affair and flying back home at the first opportunity; I had never been in an airplane though. Who wanted to be an anthropologist anyway – juvenily, I still believed that my first career priority was to be a literary writer, who (gold-digger fashion) would devote his entire adult life to erecting an eternal written monument of genius to his unhappy childhood. At those instances little but the shame of being a failure in the eyes of my friends and loved ones back home kept me from running away. Next came daydreams about horrible illnesses, real or faked if necessary, that could only be cured in a well-equipped Dutch hospital and that would therefore swiftly and without loss of face release me from my ordeal. Any falling back on the supervisors was out of the question at that point in time, as the South Asianist had just gone back home and the North Africanist had not yet returned from Holland.



left: during out 1968 fieldwork, with an informant's child; right: with his wife Ribha and my eldest son Vincent during fieldwork in 2002

Fig. 2.5. Mr Hasnawi bin Ṭahar

The turning-point of this crisis remains in my memory as the most important moment of my fieldwork. We had slept badly as usual because of the enormous quantities of strong tea we were forced to drink – a possible physiological factor in my distress. After breakfast I listlessly followed Hasnawi's suggestion to make an interview in Mayziyya that morning, where the eldest informants of my research area lived. The forested stretch of land between the two villages afforded ample opportunity for reflection.

At first I was once again seized with the panic of the previous days, but after a few hundred steps I all of a sudden decided, with a clarity of mind that I had not been capable of since my arrival in the village, to keep a stiff upper lip from now on and to make the best of the sizable investment of time, energy, frustration and money that my participation in the research project had already cost me and others. Why this sudden determination? It was not the first time that I made a cost/benefit analysis of the stressful fieldwork situation I found myself in, but so far such an analysis had not been able to lift my depression, on the contrary. Maybe the gentle spring rain reminded me of Holland and made me feel at home. I had trudged up the slope to Mayziyya before, but this time leaving the strip of forest behind and setting foot on the open fields of this other village meant shedding all fear. Behind me spread the valley of Sidi Mḥammad in all its glory. Across the valley, the mountain range that sealed it off towards the West seemed to have receded further away and was no longer

threatening. I felt relaxed when we reached the farmyards of Mayziyya. The interview was pleasant and interesting. I stopped stammering. Food was served and we took the youngest son back home with us to give him some Band Aid for his cousin who had been butted by a ram while we were there.



Fig. 2.6. Photographed while working in her kitchen (1979): Mrs Najma bint Hassuna, mart' al-Hedi bin ʿAli, mother already of three children, one of my principal women informants, and godmother of my eldest daughter Najma bint Ouime

A few days later the principal supervisor of the project appeared in my yard, wearing a parka and a woollen cap as if we were not in Africa. He had braved the sharp-edged, newly cut stones of the metalled roads, recently⁵ built by the unemployment relief

⁵Through this book, whenever the term 'recent' is being used, the reader is to remember that the ethnographic present is 1968.

work organisation, in his small Citroën Dyane car, in order to deliver my mail and take time for the long, intense and immensely stimulating scientific discussions that – repeated once every two weeks from that point on – were to be the backbone of my first fieldwork. The link with my own world had been re-established. I also could not have wished for a better guide to the North African world than Douwe Jongmans. Against the background of his vast cultural knowledge of the region and of the subcontinent as a whole, he grabbed hold of the specific raw data I provided hesitantly, and juggled them *virtuoso* so as to prompt me to formulate provisional theories and generalisations, while, in that yard with a view of the shrine of Sidi Mḥammad and the distant Mediterranean, Hasnawi and my landlord looked on whispering in awe.

2.6. From field data to theory, and back

The main problem of fieldwork, from the point of view of scientific knowledge production, is the enormous distance that exists between the observations and statements the researcher is confronted with, on the one hand, and, on the other hand, the generalisations he has to build on the basis of those raw data – generalisations that moreover, abstracting from the concrete ethnographic data, have to be relevant in the light of some fundamental theory or other. And while these problems of operationalisation, relevance and synthesis are already tough enough in the case of an extended field project of several years' duration, they are virtually insurmountable in the case of a research training project of only a few months, without thorough theoretical or regional preparation and with no time to let everything sink in and take an analytical distance.

By now, though, the Ḥumirī material was no longer totally new and strange to me. I was particularly struck by the many similarities between Ḥumirī popular religion and the Roman Catholicism with which I had been brought up. In many respects the basic concept of North African religion, *baraka*, corresponds with the Catholic concept of 'divine grace'. Many details of the cult of saints (the burning of candles in niches in little white chapels, incense, prayer postures, the eating of consecrated cakes) and some features of the religious brotherhoods seemed very familiar to me. In part this can be attributed to a common cultural origin: both Islam and Christianity originate from the same Mediterranean cultural area; Ḥumirī popular religion is partly rooted in religious modes that are widespread throughout the Mediterranean and much older than these world religions (so that *baraka* corresponds with the Hebrew *barūḥ* בָּרוּךְ), and the inhabitants of North Africa and Southern Europe have belonged to the same or closely related political units for much of the past two thousand years. This parallelism had both advantageous and disadvantageous implications for the research. Because of my background I was perhaps able to penetrate more quickly into some aspects of Ḥumirī religion than would have been possible without my experience with a kindred religion. It is quite likely though that I let myself be unduly influenced by my background, especially in defining the conceptual content and in interpreting the

phenomena I encountered in a wider social context.⁶

I had no doubts that at some time, later, behind my faraway desk, I would be able to write a decent report about my research. But that was by no means my most important incentive for working hard. When I was not engaging with people, I was defenceless against the social and sexual frustrations of the fieldwork situation. Which was one of the reasons why I hardly ever got around to working out data or (in line with local custom which we had been advised to adopt) taking naps in the afternoon: I just could not bear to.



a previous chief's son, wealthy entrepreneur, deputy head of the local unemployment relief service, musician, dancer, faqir *manqué*, and one of my principal informants; in the back the domed shrine of Sidi Mḥammad Jr

Fig. 2.7. Mr Dhiab bin Hassuna (1968)

⁶ The imposition of Christian theological models in the ethnography of African religion and thought is a recognised distortion of African religious studies; cf. Okot p'Bitek 1970. However, only long after I had concluded my first spell of fieldwork in Ḥumiriyya, did it dawn upon me that the region had been emphatically Christian during the larger part of the first millennium CE; It was then that I found that St Augustine, c. 400 CE, bishop of the city of Hippo / Bone / Annaba which on clear days could be sighted from Ḥumiriyya, had already described many of the details of Humiri popular religion which 16 centuries later, I found enacted all around me. Cf. van der Meer 1957, and Vol. II of the present study.

And after a few weeks again, the data collection, and my interaction in Ĥumirī society, became more than a means to control my panic. As I dug deeper into the world of my informants, I finally started to enjoy every new step that brought me closer to an understanding of increasingly complicated situations and ideas. In the field, with the living material almost too close for comfort around me, and in the fruitful contact with experienced researchers, I figured I was beginning to comprehend what after all I had already been studying for several years: human interaction, its complicated manifestations and interrelationships, the tension between expectations and evaluation from various sides, the place of relatively fixed factors such as norms, collective representations and material objects, conscious choices and such restrictions of choice as were, unconsciously, imposed upon the informants by their social environment. I was still a long way off from problems of power, social change, the interplay between heterogeneous semantic, social and economic systems within one field of interaction, corporeality, self-reflection and interculturality- later to become the predominant themes of my scholarly work - but I nevertheless was starting to feel like an anthropologist. It was the decade when transactionalism was introduced into anthropology, and in keeping with the times I was fashionably disillusioned, in the field, with a social science that in the main seemed to aim at abstractions about enormous aggregates of people, kin groups, clans, ethnic groups, classes, genders (and that applied these abstractions with a tendency to reification), but that appeared to have no finely differentiated concepts which were of use in the field for the more or less inchoate, ephemeral micro-phenomena at the individual human interaction level. Yet these micro-phenomena appeared to me to be anthropology's main source of material, and the very essence of the informants' lives.

Nonetheless, I could not avoid working towards macro abstractions and in this the existing Grand Theory proved to be much more of a support (thanks to the research plan I had drawn up back in Holland and the discussions with the supervisor) than I was willing to admit. Studying theories of religion (especially those of Émile Durkheim) had put me on the track of a number of fundamental problems regarding societal integration and the relationship between religious and non-religious organisational structures; and had given me a new perspective on a problem that had occupied me as a literary writer ever since adolescence: the relationship between symbol and that which it refers to. Although Durkheim hitched his splendid generalisations (according to which each society essentially worships itself in its religion) somewhat unfortunately on to the distant and, in those days, ill-understood societies of Australian Aborigines, there was nevertheless a direct connection between those generalisations and the Mediterranean popular religion I was studying: through Robertson Smith's *Religion of the Semites* (1889) that had been Durkheim's main influence when writing his *Les Formes Élémentaires de la Vie Religieuse* (1912).⁷ Despite the limitations imposed by Durkheim's idealism, transposed back into the Mediterranean region the relevance of those basic tenets became more and more clear to me in the field, and this was very encouraging. Saints indeed proved to be direct symbols of the kin groups and neighbourhoods that venerated them; groups identified and differentiated themselves from others by erecting their own shrines; the ascent and decline of shrines

⁷ Robertson Smith 1927; Durkheim 1912.

coincided with the political and demographic rise and fall of local groups; and I was even gradually able to decode legends about saints as more or less historical statements about the settlement history of various kin groups.

The eagerness with which I occasionally imagined seeing confirmations of Durkheim's theories around me as embodied in the ideas and interactions of my informants, was also of course due to the pressure under which I lived. I needed to come back with substantial results, and time was running out. I flattered myself with the thought that the concrete, highly quantifiable material I was collecting would be solid enough to go beyond whatever chimerical notions might have crept into my vision of Ḥumirī religion, and to end up with scientific hypotheses, occasionally with empirical underpinning and all. I especially relied upon my elaborate card index system of data, conjectures, hypotheses and ideas. Toward the end of the fieldwork I used this body of data to formulate a limited number of concrete questions for a questionnaire survey among all adult women of Sīdī Mḥammad and Mayziyya. I proudly worked towards what I then still envisioned as my scientific ideal: a conclusive statistical analysis of miscellaneous forms of local religious behaviour – so that I would ultimately be able to predict with certainty, for every female inhabitant of Sīdī Mḥammad, which four or five of the many dozens of local shrines she visited, and why; or so that I could explain, by reference to such factors as wealth, political power, order of birth and family tradition, why, and which, thirty per cent of the local men were to be recruited as ecstatic dancers, *i.e.* members of the local Islamic brotherhoods.⁸



Fig. 2.8. Mrs Jamila bint Hassuna mart Rabah bin ʿAli, a chief's daughter who (as an aspect of the expansion of the chiefly family into the local shrine complex) married the main shrine warden of Sīdī Mḥammad, with their children (1968)

⁸ For the realisation of this scientific ideal, *cf.* van Binsbergen 1985b. Of course, the whole thrust of my later work (especially van Binsbergen 2003) is that I no longer subscribe to such an ideal. For my other publications based on the Ḥumirī research, *cf.* van Binsbergen, 1980a, 1980b, 1988a, and forthcoming (c); van Binsbergen & Geschiere 1985a.

Now, of course, I can see that with this emphasis on statistics and model building I opted for the mere surface of religious phenomena; but it was a strategically correct choice because I lacked both time and training for more in-depth research into the symbolic and deep-psychological aspects of the cult of saints.

Moreover, I was merely in the process of seeking entrance into the anthropological profession, and was not yet in a position to challenge its scientific misconceptions. While myself undergoing considerable violence (from the part of the village population, the project organisation, and the interpreter) during my professional initiation, I saw no option but to naïvely inflict, from my part, the violence of appropriation and representation upon the host society, – a likely target, since it was the most vulnerable of the three categories of oppressors during my first fieldwork. The professional violence I underwent subconsciously reactivated such violence as I had been exposed to in my childhood. When the initial crisis had subsided and my fieldwork was clearly going to be a success, I yet became tormented by terrible nightmares in which a dark human shape threatened me; the local Qur'anic teacher and diviner (*madhab*) consulted his magical book and told me it was not a saint but a troubling ancestor. A keen awareness of the violence of fieldwork was to remain part of my professional identity, generating (as exposure to violent situations may do) libidinous fascination for and self-effacing dedication in fieldwork, but also profound fear and repugnance, and by and large it was a major source of my lasting professional ambiguity as an anthropologist. When twenty years later (twenty years of recurrent nightmares about Hasnawi) I wrote a novel on Ḥumiriyya, Hasnawi after much soul-searching on my part came out rather gloriously; but when again fifteen years later I arranged once more a meeting with him, I was so tense that I sprained my back on the way to his village, and for days could only walk bent double, with a stick I had picked up by the Tunisian roadside.

2.7. Relations with the villagers

After our initial clash Hasnawi and I occasionally still had our problems. He had definite ideas as to the desired procedure of things, based on his experience with previous project participants, and I could not always bring myself to conform. But I was now more quickly aware of any hitches, and from the growing insight into Ḥumiri society afforded to me (through my interpreter!), I was able to glean strategies to obviate such friction. I got to know something about the complex, semi-conscious methods by which people in Ḥumiriyya (as in every society) can satisfactorily comply with the demands that close relationships impose on them and can nonetheless keep pursuing their individual goals.

But not only was I becoming more aware of the roles played by my interpreter, his expectations regarding me (that I had initially completely disappointed), and the social resources at my disposal to manipulate this field of forces, I also began to realise what tremendous sacrifices he was making for my sake. I really came to value him a lot, in spite of his incessant whining (about what an exemplary life he led, how wicked some other people were – especially other interpreters, how much I owed him), his sometimes really inexplicable moodiness, his irritating, noisy bustling about early in the morning and his modest forms of blackmail. He gradually managed to get me

somewhat accustomed to the local rural rhythm of life in which the time around noon, and the evening, are not meant for work. Once I had emphatically accepted this principle he put up with the fact that the social musical evenings which we found arranged for us almost on daily basis, invariably turned into 'work' for us – and Hasnawi perhaps made his greatest contribution to the research on those evenings, while putting in double overtime as both an interpreter and a singer-musician. He kept giving me ever more detailed advice about how I could protect myself from the continuous assaults on my health by natural and supernatural forces and beings. And also on my part, my continually vocalised concern for his health (part of the local cultural idiom I had finally picked up) was no longer just pretended civility.



In the background, note the domed and horned shrine out Sidi Mhammad Jr, the village's saintly patron

Fig. 2.9. The house (interior dimensions 2x2 meter) where I dwelled with my assistant during the 1968 fieldwork

Hasnawi had considerable influence on the course of the research, even besides his increasingly exemplary role during the interviews and musical evenings. Just like me, he was haunted by the fear of failure. Although he did not have to pass any academic examinations, he was scared to lose his well-paid job as an interpreter and to fail in the eyes of his environment. We constantly had to work, and he thought real work exclusively meant conducting interviews. If I stayed home in the daytime to work out notes he became restless and insufferable, so we quickly set out for the village once again. I often felt forced to comply with his wishes, even when they did not seem conducive to an efficient and comprehensive gathering of material. But I could ill-afford a second crisis with my interpreter even less than a day lost. However, often his suggestions turned out to be valuable. In this, too, my opportunism gradually gave way

to trust. While I learned to rely more on Hasnawi, he started taking a lot more pleasure in his work. After a while we really worked together 'like brothers', as the indigenous ideology stipulated.

The definitive sign of accomplishment in our relationship was when he (greatly weakened in the last weeks of our work just as I was because of bad water, lack of fresh vegetables, and the nocturnal religious and musical séances) did not any longer blame his ailments on the hard work I made him do, but on the Evil Eye that certain of our informants in a neighbouring village had cast upon him, 'out of jealousy about his highly honourable position as an interpreter'!

But all my good will, or the advice of the supervisors, or Hasnawi's devotion, would not have accomplished anything if my informants had not been so incredibly helpful and hospitable. Once I had got rid of my initial tension and fear, I insisted on being one of them for the duration of the project. They rewarded me with a wealth of data of course, and that was exciting and instructive. But even more important during those last few weeks was that I felt at home in the village, having gratifying relationships with dozens of people whose ideas and way of life no longer were quite so alien to me anymore and who in many ways even had become dear to me. I could also carry on simple conversations now without my interpreter's help. The villagers' facial expressions and gestures began to be interpretable by me, and sometimes I was even able to catch their humour. Much to my surprise I now and then adopted their imagery, even their prejudices.

But up till the very last days, my heart sank each time when I walked the few kilometres from the motor road to Sīdī Mḥammad after a short visit to ʿAin Drāham for supplies and for the regular vitamin-B shots that kept me going.

It was a great pleasure to be allowed to take part in the leisure time activities of my informants, especially since the music that was a regular feature at those gatherings greatly appealed to me. For me, as a researcher of the local religion, pleasure was combined with business, as love-songs were lightheartedly alternated with sacred songs, some in honour of the Prophet, others to accompany the ecstatic dances in honour of the local saints. I was thus able to take a relative view of at least one of my Durkheimian premises. For at variance with the theory, my informants by no means treated the supernatural with the utmost respect that Durkheim presents as the basis of societal order. If Sīdī Mḥammad was *jadna* ('our grandfather') he was treated, seldom with awe, but mostly with the jocular, affectionate intimacy which characterises the relationship between grandparents and grandchildren in many societies. If there was any dancing, I had to dance along with the villagers and was given directions as to how it was done. What more effective way is there to get to understand the ecstatic dances' bird symbolism, of which most local people were no longer aware, and which I have now reason to believe were thousands of years old.⁹ At first these dances took place at people's homes, in front of a small group of men, but at the spring festival of Sīdī Mḥammad I had to dance for half an hour, in honour of Him, with the best dancers from the vicinity, while two hundred onlookers watched approvingly. By that time I had completely overcome my initial stage fright and I played my role of

⁹ Van Binsbergen, forthcoming (e).

researcher and of temporary Ḥumirī with gusto.

At a party I had a cordial letter to my girlfriend dictated to me by the village elders, with the prediction that I would be so 'strong' after all the sexual abstinence imposed by the fieldwork that a son would be born to us soon after my arrival, at the intercession of their and my 'grandad' Sidī Mḥammad. Much to my happiness it turned out to be a daughter, but she did get a Tunisian name. She visited the shrine while still in the womb and there undoubtedly was filled with *baraka*. At another party I was joined in mock-marriage with the youngest daughter of Uncle Salah: the climax of merry deliberations about an effective way to present my girlfriend in Holland with the *fait accompli* of a Ḥumirī co-wife, and meanwhile provide my Ḥumirī father-in-law with three Dutch girls who would have to be less 'closed' than his own middle-aged wife.

I eagerly, at first over-eagerly, ate and drank everything that was offered to me. During the fieldwork preparations in Europe much attention had been paid to the social implications of food and food sharing as part of our fieldwork strategies. We were made well aware of the 'Miss Ophelia complex' – named after the White lady from *Uncle Tom's Cabin*¹⁰ who has the best of intentions towards the 'darkies' but shies at any normal physical contact. I was convinced that with that food and drink I also, almost tangibly, ingested the culture of, and relationships with, the Ḥumirī people. I was usually grateful for their hospitality and lightheartedly took the risk of an infection with tuberculosis or any of the other terrible diseases that some of the villagers suffered from; my tropical injections, and Sidī Mḥammad, would protect me. Through my unreserved eating behaviour I indeed prepossessed people in my favour. And not to forget: a good part of the food was consecrated to a local saint, and by eating it I entered into a relationship with this supernatural being that has apparently been propitious for my work and later life. In fact, with my wife I have continued to consecrate meals of *kūsuksī* (i.e. kouskous) and *ḥalāl* (ritually pure) meat to Sidī Mḥammad in my home once every few months for more than thirty years now, with appreciably salutary effects.

Besides all the large and small feasts, we were almost daily invited to have dinner somewhere. The obligation of hospitality called for an elaborate meal, with expensive meat, and consequently I often had the feeling that the kindness shown to me by my hosts had 'eaten up' their whole week's budget. By organising a few feasts myself I was fortunately able to do something in return. It was not only neighbourly love, however, that weighed upon me. In the form of dinner invitations to Hasnawi and me, all sorts of rivalry was being fought out, over our heads, between my informants (belonging to different families, kin groups, factions, neighbourhoods, village, clans, shay·doms); and more than once this got us into problems. Although these invitations resulted in fruitful situations for casual interviews and observations, they were not always opportune, either because we wanted to do something else; or because the intimate, almost sacred bond that sharing a meal creates in Ḥumirīyya, seemed undesirable to us with regard to the particular person who was inviting us; or because we already had had to stuff down a large meal elsewhere only an hour before. Refusing food and drink

¹⁰ Beecher-Stowe 1981.

that is offered to you is, however, socially impossible in this region, and the host mercilessly takes care that his guest consumes huge quantities. So we often overate ourselves, an unusual but great sacrifice for the sake of scientific knowledge. When I returned to Holland I was overweight for the first (but not the last) time in my life.

It was particularly satisfying to me when my landlord on one of the last days of my stay in the village, and in the presence of others at that, refused the tea I offered him. This vicious attack – transgression of the same code that had forced me to consume all those tons of food and hectolitres of boiled tea – I was going to riposte! It turned into a big argument that I participated in with concealed irony and in which my landlord remained just as sympathetic in my eyes as he had gradually become. Hasnawi and I became ‘the winning party’, according to the local rules of the game, by crushing our opponent under a shower of gifts. I presented the landlord with all the fieldwork equipment that he had (unjustifiably) feared he would not get when I left, and that was why he started the whole row in the first place.

2.8. Towards understanding

Three weeks before my departure I had progressed to the point where I could pierce through the local commonplaces about religion and was somewhat able to reach what lay beneath the surface.

The method I had followed in the first weeks – its shortcomings had definitely been a factor in my initial distress – consisted in trying to elicit statements about indigenous concepts from my informants. This I did by asking as vaguely and elliptically formulated questions as possible, jotting down the statements thus provoked as best I could. However, I scarcely absorbed mentally these concepts until near the end – before that time they did not become my intellectual property but remained mere data filed for further use. In this way their content slipped from me right there and then: I did not bother to really try to understand and use the *Ḥumiri* world view during the interviews, but hoped to distil a correct and coherent system from all those (seemingly) conflicting statements by comparing them with each other once I had returned to Holland. It was like mindlessly collecting words and phrases among an exotic group of native speakers in order to learn their language or reconstruct its structure after coming back home, instead of trying to master that language on the spot and from the inside, in continuous contact with the speakers. It was a superficial and unreliable method that caused irritation: much to their despair, informants were urged, for the first time in their life, to give abstract general definitions of their religious concepts, and they were baffled when, during a subsequent interview, it appeared that I still did not understand anything at all. My insistent but incoherent questioning gave them the impression (partly correct, I am ashamed to admit now) that I never listened to them carefully, and thought they were lying or holding back information.

With the help of the project supervisor Douwe Jongmans I finally came to a better approach. When confronted with a context sketching a concrete and potentially real-life, albeit hypothetical, situation, informants usually were capable of giving an indication of the limits of their conceptions – what was still considered *baraka* and

what was not, for instance – even though they were unable to define a concept in a positive and abstract sense. Confronted with hypothetical cases that were recognisable to them (these cases were invented between the project leader and myself), they proved able to pursue an abstract line of thought – even though they could not spontaneously describe to me the connections between various concepts and activities. From among the unspecific local commonplace statements on religious behaviour that I by then knew by heart, I deduced statements about concrete situations. The informants turned out to be able to assess these statements without difficulty, either as being correct and inherent to the system, or as incorrect, nonsensical or offensive, and in this way my insight into the system was gradually being tested and adjusted, which enabled me to penetrate into evermore complicated interrelated structures.

Hasnawi and I pursued our best informants for days with questions about the specific forms of *baraka* that various fictitious sacrifices to different local saints, in all sorts of floridly imagined situations of ritual obligations and misfortune, would yield to us, in various hypothetical roles as pater familias, village elder, female leader, poor widow, a woman recently married into a family, a childless woman of forty, etc. And at long last, with affirmative nods of mutual understanding, the informants began to give us concrete answers instead of the hermetic and tautological commonplaces of the first weeks.

The method worked, especially because Hasnawi and I were by now so well-attuned to each other that we could make people feel at ease during increasingly consistent, pleasant, and well-prepared interviews. Working with an interpreter was hardly a handicap any longer. The connections that I had previously been probing for with the aid of purposely vague questions now became clear to me, and my informants were visibly relieved that I finally appeared to gain a measure of understanding and insight, and that I showed this, not only by producing statements about their religion which they could increasingly recognise as relevant and correct, but also by publicly adopting the observable practices of this religion with rapidly increasing competence.

Although the relatively simple tracing of concrete facts from the present and the past continued all through the last phase of my research, I then focused on the values and concepts behind the facts. The result was fairly satisfactory, considering my lack of experience and the limited amount of time at my disposal.

2.9. Beyond objectifying knowledge

The fervour with which I had initially searched for ways of getting to grips with my interpreter and informants in order to alleviate my distress and defencelessness, equalled the disgust I felt when towards the end of my stay I had mastered some of those ways and was actually at times capable of effectively manipulating people towards the realisation of my research goals. I felt like a hypocrite when my sweet-mouthed talk proved effective to get Hasnawi going again, even though he was justifiably exhausted, or when my invocations of God and the Prophet's blessings proved good for a further extension of an already lengthy interview with informants anxious to return to their more productive activities. But time pressed – if I was to become an anthropologist I could not afford to return from the field empty-handed.

The villagers began to reveal things they apparently would rather have kept secret. Thus they enabled me to complete my most important case histories, such as that of Ḥadūsha, the daughter of Mansūr:

#CASE 2.1. MANSUR'S DAUGHTER AND SIDI MḤAMMAD. Married some 5 kms away in Ḥadayriyya, and hence unavailable for our direct interviewing, Ḥadūsha was the daughter of Mansūr, a penniless ecstatic dancer who lived out his life as a share-cropper with the shayḥ's family into which his sister had married – another example of how the chiefly family through affinal ties sought to gain control over the religious structure of the village. Owing to a ritual transgression, Ḥadūsha was believed to have come into conflict with the local saint Sidi MḤammad (who naturally was still as invisible and dead as ever), which in her case expressed itself in acute paralytic seizures. I already suspected that all this was a reflection of a kinship conflict between her family of origin and the family into which she had married in Ḥadayriyya. She was alleged to have thoughtlessly killed a cock in honour of a local saint in Ḥadayriyya, although she had at first dedicated the same cock to Sidi MḤammad. This Ḥadayri saint was probably Sidi Salima, who in a major myth of origin¹¹ was depicted as initially Sidi MḤammad's master, until he had to admit his servant's superiority as a saint.¹² We had heard the rumour about Ḥadūsha's predicament, but wanted to get this important piece of information straight from the horse's mouth. Several courtesy visits had only yielded evasive answers. Finally, we took the son of Mansur's sister – Jilani bin Hassuna, a boy of my age – into our confidence. When he accompanied us on our umpteenth courtesy call to Mansur he made such a quasi-accidental, but irrevocable, slip of the tongue that Mansur could no longer escape the net of clever questions that Hasnawi now gleefully pulled over him.

Till then I had conscientiously, and at the first indication, respected the limits that the villagers set to the flow of information. But it was evident that in their own dealings with each other they overstepped and manipulated these limits all the time, and I began to learn the rules that went with this game. After all, most of the information I wanted to get was, far from being secret, common knowledge among the villagers themselves. Contrary to accepted wisdom with regard to social research in Islamic societies, my most important informants, as a male researcher, were women, and I could count literally all female inhabitants of both villages (Sidi MḤammad and Mayziyya) among my informants.

Although, after more than thirty years, I can still recall in detail the landscape of Sidi MḤammad, the names of the people, their faces and their social and kinship relations to one another, I have since had much more intense and prolonged experiences of other fieldwork in other cultures with a more conscious, personal and radical commitment on my part. In Sidi MḤammad, I hardly overstepped any other limits than the existentially least important ones: I could manipulate people with culturally

¹¹ This myth is at the heart of the religious life and history in the valley of Sidi MḤammad, as discussed at length in Volume II of this book.

¹² Van Binsbergen 1980a.

specific words and gestures for goals that were alien to them (the pursuit of scientific knowledge, my own career), and thus I gained my own personal access to local public knowledge. Conspiring with Hasnawi and being elated with the scientific results, I did not realise how meagre the yield was from a human point of view. After my initial struggle to find a way into Ĥumiri culture, at such moments I was already dangerously close to the exit again. And in the distance that my effective social manipulation brought about, my hosts' material poverty and the medical needs that had previously escaped my notice (or which I had not deemed of any importance as long as I had still felt I was at their mercy), were suddenly driven home to me. The scales fell from my eyes, as in the Garden of Eden after the Fall of Man, and I saw 'my informants' in their 'true colours': with their frayed blue overalls issued to them by the unemployment relief work organisation, without shoes, with empty storage tables in their huts, coughing, slouching along – people who were willing to shed all dignity as soon as the possibility of working as an immigrant worker in Holland was mentioned. With culturally specific gestures and phrases I finally had a hold over them: I could produce the social mimicry of being one of them; but whereas for them the horizon of their aspirations and achievements coincided with the imposing mountain ranges sealing the valley of Sidi Mħammad, my own ulterior motives propelled me beyond their small and poor world, to global academic production, to ultimately an academic income equal to the sum total of that of all householders in the entire valley, to North Atlantic middle-class patterns of expenditure and security. But what was the more authentic phase in our contact? Initially, when I was still stumbling along in hopelessly ineffective communication, or towards the end, when I could use their own social devices for the benefit of a form of global knowledge production they had never invited in the first place?

Anthropology is more than just a sublimated form of sleuthing or espionage. The increasingly effective collaboration with Hasnawi, the very specific nature of verbal communication in North Africa – where every sentence is, even more than elsewhere, a maze of multiple meanings and references, and above all of contradictions and gradations of the truth – and also my position of dependency as a trainee-researcher in this particular case, drove me across boundaries that I have since approached differently in my later research. When, in subsequent fieldwork elsewhere in Africa, I kept being drawn to those boundaries and often managed to cross them, my primary concern was a much more wide-ranging longing for personal contact (more specifically: longing for acceptance of myself as a person by the initially all-powerful, because locally culturally competent, other), rather than mere scientific curiosity. Not the clever mimicry of an acquired local idiom, but an absolutely vulnerable attitude on my part, abandoning scientific instrumentality, became the condition for such boundary-crossing. The researcher emerged, not as the Faustian manipulator, but as the receptive collaborator of his informants in the production of such intersubjective intercultural knowledge as could be mediated to the global domain of international anthropology.

I was still a long way off from that attitude during my first fieldwork. I was too young, too frightened of the possibility of academic failure, too obsessed with knowledge for knowledge's sake, and had not yet reflected upon the obvious conflict between scientific and human priorities – in my personal life, as well as in my dealings with

people from another culture. And besides, the preparatory phase and the supervision of the research-training project had emphasised the strategic rather than the existential aspects of the anthropological intercultural encounter. Apart from being touched by the harmonious Ḥumiriī vision of nature, life and fertility – and (in the chastest, most brotherly, most respectful possible way) by the disturbingly glorious incarnation of that vision: Najma *bint* (daughter of) Hassuna, married into Sidi Mḥammad from Hamraya, only a few years older than I was but already (like my own mother had been) the radiant mother of four children – apart from these human elements slipping in despite my cramped scientific self, I did not get much beyond manipulation during my first fieldwork. And yet, that last night in Sidi Mḥammad, after the ritual slaughter of my calf as a sacrifice to the local saint, after the last musical evening, when towards midnight I stumbled along the familiar cacti hedges to the car that was waiting for me, suddenly old Aunt Umborka (Mansur's sister and Jilani's mother) darted from of the shadows. She had been waiting for me, far away from the festive commotion, in order to give me secretly a motherly farewell kiss, and this time not the formal kiss on the hand that is the customary way people in Ḥumiriyya greet each other, but a big smack right on my mouth.

Thus my first fieldwork ended in real contact.¹³ When I began to analyse the material I had collected, the instrumental, manipulative side continued to dominate, and by quantifying and abstracting the field data I managed to fulfil in my report the scientific ambitions I had at that time. And yet, in the following decades, the existential side of my first extensive intercultural encounter kept seeking an outlet in my life. Perhaps this tension explains why I have continued to cling to the ethnographic data and to the memories from Ḥumiriyya in a much stronger manner than would be warranted by the length of my stay or the significance of the data that I brought back home. My two-volume English manuscript on Ḥumiriī society and religion is still (but, Sidi Mḥammad willing, not for long) sitting on a shelf, unpublished for lack of opportunity to finish its final editing and bibliographical updating, and for being distracted by an avalanche of later projects that show the maturity, not the infancy, of my scholarship. My eldest

¹³ There was still another form of contact that night, whose significance only decades later consciously registered in my mind:

#CASE 2.2. UNDER THE WATCHFUL EYE OF SIDI MHAMMAD. 'After sacrificing a big calf to the shrine of Sidi Mḥammad, overseeing its slaughter and ceremoniously distributing the meat over all the 80 households of the villages of Sidi Mḥammad and Mayziyya, treating my fellow Dutch student-researchers (who had been brought in from their respective distant villages) to the meal and festivities that followed this rite, and being taking back by car, with all my luggage all my fieldnotes, to the comfortable and privacy-offering town house in the district capital 'Ain Draham which was the project's headquarters, I fell into an exhausted sleep – only to wake up in great alarm, realising that it had been intimated to me that one of my indispensable note books, recording a week or more of my latest, most effective interviews and observations, was lying out in the street in front of the house, invisible from the bedroom. I summarily dressed, sleepwalked through the front door, into the night, and within seconds I saw my precious note book lying – in full view, but undisturbed, near the kerb where we had unloaded the car. No doubt the saint, pleased with his calf, had extended his protection to my indispensable possession...? As he would apparently continue to do in the next half century, in recognition of the *halal* meat and the *kouskous* meals we would continue to nominate for him every half year ever since.' (van Binsbergen 2021xxx: 245 f.; slightly edited)

child had to be named Najma (Nezjma) as a promise that my first existential celebration of otherness across cultural (and simultaneously gender) boundaries might yet grow up and become articulate. And the title of the novel that Ḥumiriyya has finally yielded to me, *Opening up a Belly* (1988),¹⁴ does not only refer to the bloody sacrifices I had to witness and stage, and to the occult information that also in Ḥumiriyya is read from the entrails of sacrificial animals (the anthropologist obtains his insights in a similar way), but also to a birth – as if upon second thoughts the cliché-like comparison of fieldwork with initiation, and of initiation with rebirth,¹⁵ really holds true in this case.

¹⁴ Van Binsbergen 1988a.

¹⁵ Cf. van Gennep 1909.

Chapter 3. Kinship, spatiality and segmentarity

Introducing the present Volume I¹⁶

3.1. The area

The part of Ḥumiriyya where I did my research consists of highlands with steep, forested slopes. The population is concentrated in villages comprising a few score to a few hundreds of inhabitants. The villages are surrounded by fields, pastures and forests. The density of population of the area is about 60 inhabitants per km².¹⁷ The nearest urban centres, both comprising a few thousands of inhabitants, are ʿAin Drāham and Tabarqa; both lie at a distance of about ten kilometers from the centre of

¹⁶]For further information see: Beeker 1967; Bos 1969; Brunt 1969; Demeerseman 1964; van Dijk 1968; Hartong 1968; Jongmans 1968; Martin 1966; van der Meer 1970; Miedema 1967; Souyris-Rolland 1949; Jonker n.d.; de Jong 1968; Schulte Nordholt n.d.

¹⁷ The density of population was assessed by the following method. For the village of Sidī Mḥammad, Mayziyya, Hamraya and Ḥamaysiyya, I have complete census data for 1968; those for Hamraya and Ḥamaysiyya I gratefully derive from the collective research which the participants of the University of Amsterdam field-work training project collected there in the spring of 1968. These villages comprised 623 persons, distributed over 124 households (on the average 5.0 per household). Fidh al-Missay and Raml al-ʿAtrus together had 24 households; a detailed census is not available. Assuming that the households in the latter two villages constitute one population with those of the former four villages, about 120 persons can be said to live in Fidh al-Missay and Raml al-ʿAtrus combined. This means that the six villages in total contain c. 743 persons. The six villages, the area which they enclose between them, and their immediate surroundings in so far as these could not be reckoned to belong to other villages outside our research area, together constitute a contiguous area of 12.2 km² (see Fig.). This leads to a density of population of $743/12.2 = 61$ inhabitants/km², for 1968. This appears to be a fair general estimate for Ḥumiriyya as a whole. However, in the immediate vicinity (up to 3 km) of the urban centre of ʿAin Drāham the figure was considerably higher due to recent migration.

my research area.

The area where I daily carried out intensive research consisted of the villages of Sidi Mḥammad and Mayziyya (see map). In addition I paid considerable attention to the neighbouring villages of Fidh al-Missay, Raml al-ʿAtrus, Hamraya and Ḥamaysiyya. These six villages together I mean when below I shall speak of 'the research area'. Occasionally I visited other villages in the wide surroundings. The collective research mentioned in the Preface was carried out in the villages of Hamraya, Ḥamaysiyya, al-Hafur and ʿArba'aya.

The highlands of north-western Tunisia constitute a peasant society, in the sense of a collection of relatively small local communities characterised by

- the predominance of one particular type of economic activity (agriculture and animal husbandry pursued in a traditional manner), and
- the linking up of those communities with nearby urban centres, in many respects: services, markets, formal political organisation, and the major part of the prevailing culture (cf. Foster 1962: 44 f.).

The urban centres play an important part in the life of the villagers of the research area. A few times each month one visits the market and the shops in ʿAin Drāham and Tabarqa. One goes shopping there, but also meets people from other villages, with whom one exchanges information, *e.g.* about forthcoming marriage ceremonies and saintly festivals. The regional hospital, post office and police force are also situated in ʿAin Drāham. Some Ḥumiriīs are in formal employment there. ʿAin Drāham also houses the regional office of the unemployment relief organisation, which offers an addition to the family budget for many villagers, and besides jobs as brigade chief or foreman for a few who are well educated. ʿAin Drāham is also an administrative centre: the seat of the délégué, head of the délégation ʿAin Drāham, which resorts under the Governorat of Janduba.

In the area the influence of the central government is ever increasing. Since Tunisia attained territorial independence from France in 1956, this influence has led to such major changes as the virtual prohibition of goat husbandry (until recently an important source of income); the reforestation of a large part of the mountain slopes which thus were closed for agriculture and grazing; and changes in housing patterns.

The nexus between the villages and the central administration is formed by the chief (Arab. *šayʿ*, since 1969 called *ʿomda*). Although the petty administrator created by the colonial government was called by the title of honour *shayʿ*, which is also the term used for religious leaders and for saints, I shall designate this secular office by the terms used elsewhere in Africa: chief, chieftaincy and chiefdom.

The chief is appointed by the délégué of ʿAin Drāham and remains answerable to him. Each chief administers a chiefdom: an area over a few dozen km², comprising a few dozen of villages. The research area belongs to the chiefdom of ʿAtatfa. Since independence the position of chief is remunerated by a formal salary; during much of the colonial period chiefs were remunerated by the right to deduct a percentage from the taxes they collected in their chiefdom. Nowadays the office of chief combines nearly all administrative and police powers: the assessment of possessions in the form of land and cattle (for taxation purposes), registry of birth and death, the identification

of youths due for military service, postal affairs, access to medical services, access to unemployment relief, implementation of the state monopoly on the rural retail trade (none the less the present-day chiefs tend to turn a blind eye to such local stores as do exist in their areas), the resolution of small conflicts and, failing this, liaising with the police and the judiciary at the regional level. The chief has a handful of permanent but unpaid assistants who live scattered over the chiefdom.

The local branches of the Tunisian national party constitute another aspect of the political organisation of the research area. The party has a few members in most villages. Forming the grass-roots level of the national party structure, the rural party members may provide checks and balances for the very great power of the chief.

In my analysis I shall concentrate on the social processes within and between the rural villages, and virtually ignore the regional urban-rural relationships however much the latter form part and parcel of the peasant society. And along with the regional towns the Ḥumiriī countryside belongs to even larger units: the state of Tunisia, the subcontinent of the Maghrib, Africa, the Arab world in general. But concentrating on the micro processes of rural society my argument has no bearing on that part in the social orientation of the Ḥumiriī by which he is proud of being Tunisian, supports President Bourguiba, and identifies with Muslims wherever in the world.

3.2. Kinship and spatiality: The problem; points of departure

In the course of my research on religion in Ḥumiriyya, a number of major problems of social organisation presented themselves:

- (a) What is the indigenous system by means of which Ḥumiriī actors structure and classify their social environment?
- (b) How can this indigenous system be described in analytical terms, in other words how can it be approached by an analytical, scientific model?
- (c) Actors differ in their religious action – one is a religious specialist and another is not; one makes pious visits (*i.e.* pilgrimages, *zīyyāra*) to a particular shrine and another does not. *To what extent do these differences in religious action coincide with such social distinctions as are created and maintained by the indigenous system?* For instance: do all religious specialists belong to a particular kin group as defined in the indigenous system? Can one predict the factual patterns of pilgrimage on the basis the structure of local spatial groupings as found in the research area?

Questions of type (c) were central in my research and analysis, but they could only be approached once the former two questions had been answered. To argue such answers as offer an adequate social-organisational background for my analysis of Ḥumiriī religion in the subsequent parts of the present monographs, is the purpose of the present part I on Ḥumiriī social organisation.

The following introductory discussion serves to indicate the lines along which my analysis of Ḥumiriī social organisation will be developed.

If we want to describe people's place in society we may do so by concentrating on each individual's socially relevant attributes.

The most simple of these attributes are those which either the category system of the society under study, or the analytical constructs as used by an ethnographer, can be attributed to an individual in an objective manner, *without* reference to other individuals or groupings. Examples of attributes of this type are: gender, age, wealth, physical and mental skills, skin colour. Let us call attributes of this type: *objective individual-centred attributes*.

In addition an individual will have certain attributes which are exclusively defined by reference to other individuals. for instance, to say that a person A is a friend, colleague, close neighbour, mother's brother, is meaningless, unless we specify the other person B whose friend, colleague, close neighbour, mother's brother A is. Attributes of this type we could call *subjective individual-centred attributes*, for in their attribution our point of reference is a particular subject in relation with others in his or her social environment. In fact this type of attributes is identical with the *relations* which someone has with others in his or her social environment; here we use relation in the general sense and not in the narrower, social-scientific sense of actual interaction and its frequency. At the disposal of each society, and of each ethnographer seeking to describe a society, there is a system of categories to describe these relations, to classify them, and to define the situations when which category of relations is at stake between which individuals. Let me mention a few important principles in such a category system,. Once of them is kinship: it defines relations as formulated in indigenous kinship terminology, or in chains as used by anthropology (FZ, HBW etc.). Another principle is spatiality: the relative distribution of people (*i.e.* their dwellings) in the landscape; this principle leads us to distinguish such relations as 'close neighbours'. Other principles refer, for instance, to the manner in which and the frequency with which members of a society have actual interactions with each other (*i.e.* relations in the narrower social science sense); this defines such relations as friend, enemy, acquaintance, patron and client, etc.

These subjective individual-centred attributes can only be attributed by tracing the relations between one individual and the other individuals in his or her social environment: they are egocentric (*cf.* Mitchell 1963; Boissevain 1968).

A third type of individual attributes is acquired not by reference to objectively ascertainable characteristics and acquisitions, not by reference to other individuals, but by reference to a particular explicitly recognised social grouping. These attributes I shall call *grouping-centred attributes*. We can define a social grouping as a set of individuals which is unambiguously demarcated, either by an indigenous category system, or by an analytical category system which is imposed upon a society by an ethnographer. A social grouping, in other words, is merely a name for a particular set of people. This definition purposely does not stipulate actual interaction and mutual identification among the members of social groupings. Only certain social groupings do display actual interaction and mutual identification among the members, and in that case can be termed social groups.

Thus with regard to a particular individual, a grouping-centred attribute merely implies: 'this individual is a member of that grouping'. A society in which such

grouping-centred attributes are being used, has at its disposal an indigenous category system defining:

- which types of groupings are in existence;
- how these types are mutually exclusive, hierarchically inclusive, or partly overlapping as the case may be; and
- under which conditions which individuals are members of which groupings.

I propose to call such an indigenous system a grouping-centred classification system. Among the important principles shaping grouping-centred classifications in a great variety of societies we can count: kinship and spatiality.

An ethnographer studying a society can seek to design an analytical equivalent for the indigenous grouping-centred classification system: a translation, in scientifically defined terms, of the indigenous system and of the rules of its application. In order, therefore, to account adequately for the patterns of actual interactions as recorded in a certain society, an ethnographer may often analytically define a grouping-centred classification system (*e.g.* in terms of kinship groupings or social strata – both types of groupings defined as abstract models before a scientific audience); in the process such a researcher may deliberately refrain from the attempt to approximate the indigenous classifications as closely as possible.

To the extent to which they have been explicitly, consciously defined (institutionalised) within a certain society, the three types of individual attributes each make their own contributions to the social organisation, because they inform the motivation and interactions of the actors in that society.

Let us return to the problem of the present volume.

An comprehensive discussion of Ḥumiri society in terms of the three types of attributes as distinguished above, in their mutual connections, would amount to a full analysis of the local social organisation. My ambitions in the present argument are rather more limited. In the first place I shall concentrate on an analysis of indigenous grouping-centred classification systems in Ḥumiriyya (*i.e.* type c). I shall try to approach these indigenous systems by an analytical models to be formulated in analytical terms. AS the argument develops we shall have to pay some attention to egocentric relations, *i.e.* to subjective individual-centred attributes (type (b)). The connexion, finally, between these aspects and the objective individual-centred attributes (especially in a context of social inequality) can also be discussed in passing, in the concluding chapter.

This selective emphasis would appear to be the most strategic choice with regard to my later discussion of the place of religion in the social organisation of Ḥumiriyya.

The analytical model that I had in mind as the most plausible analytical approximation of the Ḥumiri indigenous system of grouping-centred classification, was that of the segmentary lineage based on unilineal descent. Since the researches, in the 1930s, by Evans-Pritchard among the Nuer (Evans-Pritchard 1967) and Fortes among the Tallensi (Fortes 19...), this model was developed mainly in the context of British social anthropology. Some aspects of this model will be discussed in chapter 6. The model has been frequently applied to Arab societies, especially in North Africa (*e.g.* Bourdieu 1963: 87 *f.*; Evans-Pritchard 1949: 54 *f.*; Gellner 1969; Favret 1966, 1968; Peters 1960).

Also in Hartong's 1967 research on local history in what was to be my research area the model was considered relevant, albeit that Hartong (1968: 53) spoke of 'a limited, imperfect segmentary lineage organisation'.

Therefore, the problems around which the present argument is organised are the following:

- What system of grouping-centred classification is used by the Ḥumirīs?
- Can this system be adequately approximated with the analytical model of the segmentary lineage?
- Is there maybe an alternative analytical approximation which can claim a closer fit with the empirical data on Ḥumirī society?
- What is the relevance of indigenous grouping-centred classification for day-to-day interaction in Ḥumirīyya?
- With regard to day-to-day interaction in Ḥumirīyya, what is the relation between on the one hand indigenous grouping-centred classification, and on the other egocentric relations (subjective individual-centred attributes)?
- What, in general, is the significance of spatiality and kinship in the social organisation of Ḥumirīyya?

In the course of our argument we shall extend this central problematic by a discussion of other related aspects of Ḥumirī society, in an attempt to prepare ourselves in the best possible way for the analysis of Ḥumirī religion.

3.3. Structure of this Volume I's argument

In chapter 2 I present the indigenous Ḥumirī ideology about local social organisation, and trace its implications. According to this ideology unilineal, agnatic descent is the central structuring principle in this society. Therefore in Chapter 4 I shall analyse how a grouping-centred classification on the basis of unilineal descent takes concrete shape in this society, and whether in that form this classification could possibly play the pivotal role which indigenous ideology accords it. It will be demonstrated that the claims of indigenous ideology concerning the absolute dominance of agnatic descent are grossly exaggerated. In the meantime we shall begin to understand some of the principles which in specific cases govern the actual shape of Ḥumirī classifications in kinship terms. This insight will enable us to reconstruct the residential history of the research area since c. 1800. In chapter 4 we discuss an intermediate form between spatial and kinship-based grouping-centred classification: the Ḥumirī clans. In chapter 5 Ḥumirī society will be exclusively described from the point of view of grouping-centred classification based on spatiality. This approach will turn out to be extremely fertile. So much is spatial grouping-centred classification a central theme in Ḥumirī society that we can introduce a new concept: that of spatial segmentation. This concept is further developed in chapter 6 by reference to some relevant literature. In chapter 7 we explore the relevance, for day-to-day interaction, of grouping-centred classifications on the basis of spatiality and kinship. Spatial classification turns out to have a certain, limited relevance for Ḥumirī social organisation; but the relevance of

kinship-based grouping-centred classification turns out to be almost nil. This result is further explored by reference to a structural-functional comparative analysis (Lewis 1965), and by tracing a parallel between Ḥumiriyya and Cyrenaica (Peters 1967). In addition, chapter 7 will discuss the relation between indigenous ideology (as traced in chapter 2) and social reality. My opening discussion brings out that spatiality and kinship are structuring principles not only for grouping-centred classification, but also for the definition of egocentric relations between two individuals. This aspect will be developed in chapter 8. In egocentric relations spatiality turns out to be a very central datum, while kinship will be shown to have a certain, albeit limited, significance independent from spatiality. This leads to a discussion of the kindred (as an egocentric system of interaction; cf. Mitchell 1963). A comparison between the Ḥumiri kindred and that in the Greek highlands (Campbell 1963) will further deepen our insight. In chapter 9 the argument will be concluded with a discussion on the relation, in general, between spatiality and kinship as principles of grouping-centred classification; on the relation between spatiality and social inequality; and on recent changes in the Ḥumiri social organisation.

3.4. A remark on quantitative analysis

In order to avoid misunderstanding I will already at this stage stress the considerable shortcomings of my quantitative data and of their analysis. Statistical analysis does not annihilate the risk of incorrect conclusions, but merely enables us to quantify that risk. And, as every methodological handbook will point out, even such quantification is only possible if certain conditions have been met: if the data collected are a reliable reflection of reality, if the hypotheses to be tested have been formulated and operationalised before the data were collected, etc. These conditions have scarcely been met in the case of the field-work underlying the present analysis, as will be clear from the extensive discussion of that field-work in appendix ... of the present book. The research was exploratory, the research plan with which I started the field-work was quite sketchy, there was no time to analyse the data in the field in an attempt to formulate new hypotheses and to test the latter with a new series of methodologically impeccable data. These problems are in no way peculiar to the present research but represent the general dilemma of quantitative approaches in anthropological field-work.

But although all this does qualify the quantitative analyses which form the backbone of the present argument, it does not render them worthless. For honesty would compel us to compare these quantitative results not only with all kinds of strictly planned and meticulously executed social-scientific research, but also with the results of other types of non-quantitative, non-statistical anthropological field-work. Both forms of field-work revolve on the impressions which a field-worker undergoes through intensive participatory contact with a specific society. The modest advantage of the quantitative method in field-work is that the research at least assesses to what extent his or her impressions (which have often been triggered by isolated and possibly non-representative fragments of information) do tally with the totality of the data he or she disposes of. yet in both quantitative and qualitative anthropological field-work of the standard, exploratory type it remains uncertain to what extent the data collected do

form a reliable reflection of the social reality under study, and to what extent the data collection has been contaminated by the hypotheses as they gradually arose in the course of the field-work.

In addition my analysis might contribute towards the formulation of hypotheses for later, methodologically more strictly conceived research.

Because of this – with some qualification – quantitative approach the reader will search the chapters of the present book in vain for the nicely illustrative details (cases, quotations from informants, salient bits of information) which are common in the anthropological literature. The general belief is that such illustrations enhance legibility and tighten the argumentation. I have some doubts on both counts. Scholarly argumentation is not automatically rendered more convincing but the inclusion of isolated illustrations whose representativeness one cannot assess. Moreover my personal experience with the reading of ethnographic monographs is that all those cute and picturesque details, indigenous terms and unpronounceable names of actors who yet remain total strangers to me, obscure rather than illuminate the author's argument. Of course this does depend on the topic: complex relationships in the field of values, expectations, emotions can hardly do without the type of illustrations against which I argue here; but such topics are largely outside the present analysis.

3.5. Lineages and clans: Ḥumirī social organisation according to the indigenous ideology

3.5.1. The descent-centred indigenous ideology of social organisation in Ḥumiriyya

In its most rigid form the ideology of Ḥumirī rural society claims that

- this society consists of agnatic segments which are hierarchically inclusive in the sense that each higher-level segment comprises a number of segments at the level immediately below;
- the functioning of Ḥumirī society is entirely governed by this segmentary organisation.

When we try to formulate a common denominator for the way Ḥumirīs would express this ideology, this would result in a statement like the following:

'In an inconceivably remote, mythical past a certain apical ancestor (whose origin is, ideally, both unknown and irrelevant) settled in a certain area: a valley fringed by forests slopes. Until his arrival the area was uninhabited, he was the first occupant. He took possession of the area, grazed his livestock in the forest, and made clearings for horticulture and agriculture. His sons continued to live near him, establishing their own households, having children, and making further clearings. Gradually the area filled with the patrilineal descendants of that one ancestor. They were called by a group name deriving from the ancestor's name. Frequent marital contacts were entertained with the surrounding groups, which therefore came to be considered as the 'brothers-in-law' of the ancestor's descendants. [*Thus inter-group relationships were institutionalised in terms of permanent affinity relations.*] Ever since those mythical times the kin group continued to live at the same place. Wherever is the place of residence of the group

that bears the ancestor's name, there that ancestor himself once dwelled.'

The area associated with such a kin group is a valley, while the kin group itself is called *firqa* or *duār*. The ideology therefore claims a one-to-one relationship between valley on the one hand and *firqa* or *duār* on the other. In actual fact, however, most informants agree that most valleys comprise more than one *firqa* or *duār*. The naming of valleys is governed by peculiarities of the landscape, by religious associations, and possibly other considerations. The word for valley is *hanshir*. Thus there is the *Hanshir al-Millāh* (The Salty Valley, named after a salty spring); *Hanshir Sidi Mḥammad* (named after the major shrine of the local saint Sidi Mḥammad), etc. The name of the *firqa* or *duār* is often formed by adding the suffix *-īyya* to the name of the apical ancestor; in the process, the vowels of the name are systematically transformed. For instance, the ancestral names of 'Arfa and Bu-Maza lead to the group names of 'Arfawīyya and Mayzīyya. The apical ancestor is denoted by the same term as that for grandfather and ancestor in general: *djadd*. (cf. appendix 7 on kinship terminology).

The ideological identification of the present-day residential area of a kin group (as indigenously defined) on the one hand, and the apical ancestor's place of residence, on the other, has important implications:

- In order to present one's actual place of residence as rightful and prestigious in terms of the dominant societal ideology, one is inclined to insist that it was already the apical ancestor's place of residence, regardless of the historical evidence.
- Since the ancestor's descendants are claimed to live in the same place as he himself, the societal ideology implicitly denies the existence of migration.

Thus in my first interviews on the research area's residential history and current kin groups my informants sketched a picture of local society as consisting of a very limited number of agnatic kin groups, which since times immemorial had been tied to the territories which they occupied in the present. It was only when I began to systematically analyse the increasingly manifest contradictions between the informants's statements, eliciting more and more data on this point in an increasingly purposeful manner, that I came to realise to what enormous extent the societal ideology obscured the vertiginous dynamics of residential movement involving, in reality, large number of agnatic groups which turned out to be independent, *i.e.* which could not be traced to a common patrilineal ancestry.¹⁸

3.5.2. Ḥumirī kinship terminology

Ḥumirī society has distinct, simple (*i.e.* non-composite) kinship terms for a limited number of close kinship relations. For other relations the simple terms are concatenated, which results in combinations which are literal equivalents of the analytical terms as used by anthropologists: son's son, father's sister, etc. The set of Ḥumirī

¹⁸ That migration forms a constant aspect of the Ḥumirī society is confirmed by Bos (1969: 11 and *passim*), who did research in *Shahāda* (see Fig. ...[**add number**]). Bos does not dwell on how migration is reflected, or dissimulated, in local ideology and in the pattern of social grouping.

kinship terms can thus be extended ad libitum.

Table 26 presents the simple Ḥumirī terms, preceded by a number.

1 djaddī	6 ^c amtī	<i>lalla</i>
2 djaddī	7 ḥālī	13 rādjalī
3 uboyī (address: <i>baba</i>)	8 ḥaltī	14 martī
4 ummī (address: <i>dada</i>)	9 ḥuyī	15 wildī
5 ^c ammī	10 uḥtī	16 bintī
	11 (senior to Ego) <i>sīdī</i>	17 nsībī
	12 (senior to Ego)	18 nsībī

Table 3.1. Non-composite terms in Ḥumirī kinship terminology.

Fig. 22 indicates which relation, as seen from a central Ego, is indicated by which numbered Ḥumirī term.

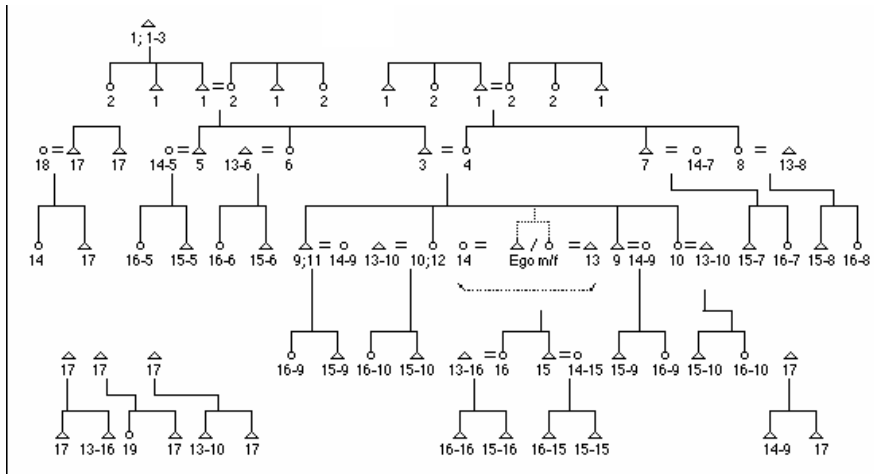


Fig. 3.1. Schematic representation of Ḥumirī kinship terminology (cf., table 26)

A combination of numbers means that the corresponding simple terms from table 26 are concatenated in the order as indicated; in the process, all terms except the final one drop their suffix -i or -a.

The simple terms are both terms of reference and terms of address. All simple terms except 13, 14, 17 and 18 are frequently used in a figurative sense: when the speaker wishes to stress an intimate relationship regardless of genealogical ties. In such a case, the choice of a term is largely governed by age and gender of the referent. Place of residence offers an additional consideration: within one's own village one uses 5 and 6 (FB, FZ) for inhabitants of all superior generations, while with regard to the senior

inhabitants of neighbouring villages the terms 7 and 8 (MB, MZ) are preferred. The composite terms are largely used as terms of reference: when the latter apply, one prefers to address the referent by his or her proper name, in combination with a simple term used in the figurative sense. Exceptions are the terms for FBS and FBD (15-5: wild-^cammi, and 16-5: bint-^cammi), which not only constitute the usual terms of address between actual cousins, but which are also widely used in a figurative sense, between all age mates except brothers.

Chapter 4. The participants's perception of their genealogy over time¹⁹

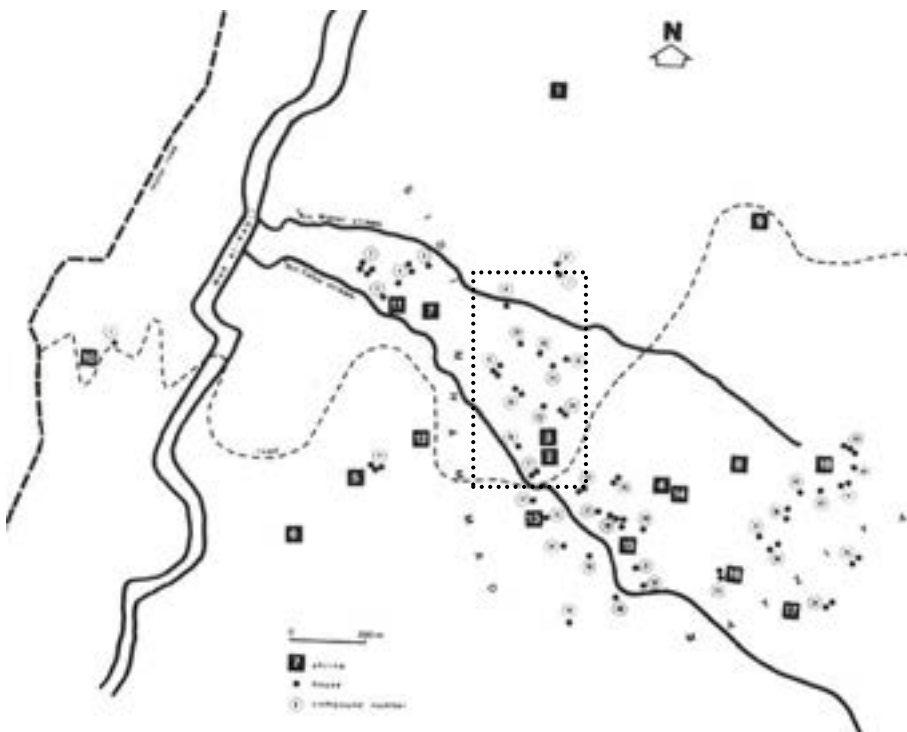
Genealogical knowledge and manipulation in Ḥumiriyya, with special reference to Murphy & Kasdan's theory of agnatic genealogies

4.1. Introduction

The present chapter is an account of specific problems of theory and empirical substantiation in the field of unilineal descent systems, such as was very much *en vogue* in the 1950s-1970s, but has since faded into the background, and is now considered an esoteric, obsolete form of quasi-scientific cabbalism. My aim in retrieving this long paper from its computer grave, is because it shows a detailed and eloquent aspect of

¹⁹ Strictly speaking, this chapter, in its present, more or less achieved, form was never part of my 1970 thesis; it summarises, formalises, and amplifies results contained there in various appendices which were suppressed as such, and incorporated in the main text, in the present redaction. Around 1970s, the Murphy & Kasdan (and Randolph) hypothesis concerning genealogical manipulation inspired me to look for a way out of the myriad inconsistencies and contradictions I found in my genealogical material as collected in Ḥumiriyya. But as this chapter's argument makes very clear, such an escape was far from being offered by Murphy and Kasdan directly. In the 1970s, I meant to publish this chapter as a separate article, but pressing institutional responsibilities and my shifting research focus on sub-Saharan Africa prevented me from pursuing final publication until in the present book. Later the whole focus on unilineal descent and genealogical manipulation came to be considered as an obsolete oddity of late classic anthropology; yet I insist that the principal features of Ḥumiri social organisation can hardly be understood in any other way.

what has since emerged as the *anthropology of time*:²⁰ unilineal descent lies enshrined in the participants' awareness of kinship relations in the past, *but this awareness turns out to be a function of time elapsed, and to display specific time-related patterns, some of which the present argument seeks to highlight* – especially to the extent to which, in the 1950s-1960s, they were the subject of theorising by two American anthropologists, Murphy and Kasdan.



For the rectangle formed by broken lines, see Fig. 4.4, below

Fig 4.1. Dwellings, shrines, and other features in the landscape of the villages of Sidi Mhammad and Mayziyya, 1968

After the initial triumph of the model of the unilineal segmentary lineage in the 1940s (Evans-Pritchard 1967 / 1940; Fortes 1945, 1949, 1953; Barnes 1962), problems in subsequent research soon led to the recognition of a type of societies in which, on the one hand, the dominant local ideology lays much emphasis on unilineal descent – either patrilineal or matrilineal – but in which on the other hand the unilineal model does not give an adequate description of the actual structure of interaction; in fact it is

²⁰ The study of time has emerged, in the last half century, as a major field in its own right, where the anthropology of time and the philosophy of time are important constituent fields. This is not the place to present an overview of these important sub-disciplines.

bilateral kinship which structures these societies (Sahlins 1961; Forde 1963; Karp 1978; more ethnographic publications cited by Befu 1965: 14-34).

Already in 1959, not on the basis of their own ethnographical research but through the analysis of a theoretical model, Murphy & Kasdan arrived at the conclusion that this type of societies includes the Arabian ones: here, too, we find a strong patrilineal ideology, while bilateral kinship is claimed, by these authors, to be the actual functioning principle. The only argument they advance for this view is that because of agnatic endogamy (such as FBD marriage) as occurs in Arabian societies, already after a few generations the patri-line and the matri-line would coincide. Their argument was strongly opposed by Patai (1965; cf. 1955). However, in a later publication Murphy & Kasdan (1967) argue that Patai did not properly understand their argument, and that Patai himself commits the error or confusing local ideology with the practice of interaction.

Murphy & Kasdan's analysis is not directly based on ethnographic research of their own, and hence has typical 'armchair' shortcomings: their model is too simple. In particular, their major claim is undermined by the fact (which was rightly mentioned by Patai), that in the various Arabian societies described so far agnatic endogamy involves only a relatively minor proportion of all marriages: the majority of marriages is always contracted outside the circle of near-agnates.



In the background the shrine of Bu Qasbaya al-Kabir (t the shrubs behind the cattle), the wooded bed of the Wad al-Kabir, the winding unpaved road up the slope towards the motor road

Fig. 4.2. Women of the Hillel family harvesting rye in the valley of Sidi Mḥammad, 1968

But even if based on only a partial argument, Murphy and Kasdan's conclusion is correct. Peters initially interpreted his data on the Cyrenaica Bedouins in terms of the segmentary lineage model (Peters 1951, 1960), but finally concluded that this model is not adequate: 'it does not provide an admissible basis for analysis,' for it is merely 'a fact of their (Bedouin – WvB) social life', 'a people's ideology', behind which hide social structures and interactions which in fact are governed by very different principles (Peters 1967: 279).

A similar situation obtains in the Ḥumīriyya highlands. Here, too, we see an emphatically patrilineal ideology. But (as I could demonstrate by means of extensive quantitative analysis discussed elsewhere in the present book) the single most important recruitment principle for everyday social interaction, for marriage partners, for religious activities, was not agnatic kinship but spatial segmentary organization, in the last analysis based on spatial propinquity. Spatiality as a recruitment principle turned out to be complemented by a number of secondary principles. Kinship was one of these, but then in such a way that the practice of interaction hardly distinguished between agnates, cognates and affines: the bilateral, ego-centred kindred (Mitchell 1963), undifferentiated in terms of agnates, cognates and affines, emerges as an important structural principle in this society. However, the kindred remains a secondary principle, because effective social relationships between kinsmen in most cases can be interpreted in terms of spatiality, *i.e.* as relationships between people who live within a radius of a few hundred meters from Ego's house; because of the residence pattern and the pattern of land acquisition in this society the majority of Ego's kindred happens to live very near to Ego.

One problem which Murphy & Kasdan make explicit, is this: *how can the local patrilineal ideology survive despite the bilateral practice?* (Murphy & Kasdan 1967). Peters (1967) ignores this problem, while Murphy and Kasdan attempt to find its solution primarily in the nature of Arabian genealogies: 'If the memories of the Arabian genealogists were complete and perfect, the kinship system would not work' (Murphy & Kasdan 1967: 11). This idea they work out in the way of two hypotheses. The first hypothesis they derive from a personal communication by R. Randolph, who did research among the Bedouins of the Negev:

'...Among the Bedouin of the Negev, female names are not simply forgotten by the genealogists because of the unimportance of the maternal kin but are deliberately excluded and not mentioned even when perfectly known. (...) The names of women who were taken as wives from other major descent groups are remembered in the genealogies. The function of the genealogical amnesia appears obvious. Since marriage does not serve to maintain unilineality through the practice of endogamy, bonds through females must be deliberately suppressed in order that the matrilineal links do not lead directly back into the endogamous descent group. If this were allowed to happen, the system would become bilateral in form as well as in function'. (Murphy & Kasdan, 1967: 10; also *cf.* Emmanuel Marx 1967.).

The second hypothesis is formulated by Murphy & Kasdan (1967: 11), somewhat in passing, on the basis of Murphy's field-work (1964) among the matrilineal Tuareg:

'The Tuareg were not able to expunge either males or females from their genealogies, for the males were the source of authority and inheritance and the females of descent. What they did do, however, was maintain only shallow genealogies, and it was difficult for the ethnographer to elicit names beyond the second ascending generation (...). This made it equally difficult for the Tuareg to exactly establish their relationships beyond second degree collaterals, and the

multiplex nature of kin ties within the group was accordingly diminished. (...) The absence of long genealogies – or the dropping of one gender from the genealogies – is quite as important a social fact as are the ties that can be established where genealogical depth is present.’



Note the domed and horned shrine of Sidi Mhammad Jr, centre photograph

Fig. 4.3. Centre of the village of Sidi Mhammad Jr on a rainy day in Spring 1979

Without explicitly claiming that unilineal ideology and bilateral practice are also found among the Tuareg, this passage implies that, for Murphy and Kasdan, a reduction of genealogical depth forms a functional alternative to the genealogical manipulation of women and their names. Both devices might then also help to obscure from the actors’ awareness the discrepancy between unilineal ideology and bilateral practice.

In north-western Tunisia, the analysis of the social organization, and of the semantics of the local system in terms of which the actors describe their own society, offers considerable insight into the way in which, at the actors’ level, the spatial, bilateral and patrilineal principles were integrated in such a way as to obscure the contradictions between these principles from the actors’ consciousness, even if these contradictions were only too obvious from the analytical point of view. Local ideology claims that the central role in day-to-day interaction is played by patrilineality; since in fact such a role is played not by patrilineality, nor by bilaterality but by spatiality, my analysis concentrates on the relation between spatiality and patrilineal descent. However, if – like Murphy and Kasdan in their theoretical model – we agree to ignore the spatial factor for a moment and for the sake of the argument, then the situation in my research area corresponds with the contradiction between patrilineal ideology and

bilateral practice as posed by Murphy & Kasdan.

The peasant society of north-western Tunisia belongs to the total set of Arabian societies which form the object of Murphy and Kasdan's theoretical pronouncements (1959, 1967). Also in north-western Tunisia one can demonstrate a discrepancy to exist between unilineal local ideology and bilateral practice, such as has been recognized by these authors. Now, can the presence of this discrepancy be explained by reference to the two hypotheses advanced by our authors? In other words,

- Do we encounter, in north-western Tunisia, the genealogical manipulation with regard to women, as postulated by Murphy and Kasdan?
- What is the genealogical depth in north-western Tunisia?

In order to answer these questions I shall proceed as follows. First I shall work out Murphy & Kasdan's hypothesis concerning genealogical manipulation of women, and operationalize this hypothesis in such a way that it will be amenable to quantitative testing. Then follows a qualitative discussion of the genealogical data from north-western Tunisia. In that context I shall define the sample of genealogies upon which at a later stage I shall test Murphy & Kasdan's hypotheses. Then I assess the genealogical depth of the data. As a next step I apply a quantitative analysis in order to assess which the genealogical manipulation of women actually occurs as postulated by Murphy & Kasdan. This will turn out not to be the case. I shall then discuss why Murphy and Kasdan's theory is inadequate, and advance an alternative hypothesis with regard to the genealogical manipulation of women. Quantitative analysis brings out that my alternative hypothesis does apply to the data from north-western Tunisia.

4.2. A qualitative discussion of genealogical data from north-western Tunisia

4.2.1. Spontaneous and solicited genealogical statements

Some ethnographies give the impression that the people described are constantly contemplating their genealogies, constantly make genealogies the topic of their conversations, and let their interactions to a large extent be determined by such kin relationships as are depicted by genealogies. This image does certainly not apply to the society of north-western Tunisia. Between the actors there is relatively little verbal communication about genealogical matters. To the extent to which there is such communication, it is largely limited to the tracing of genealogical chains between living contemporaries. Statements concerning such chains are spontaneously produced by the actors as explanation for certain forms of interaction (visiting, cooperation in production, assistance in times of illness and bereavement, etc.) between the people involved; in other words, in a context of kinship obligations. In the study of genealogical knowledge we need to distinguish between genealogical statements which have been elicited systematically by an ethnographer in the context of a formal interview, and such statements as are volunteered by actors in real-life conversation. Unsolicited data of the latter type are unavoidably unsystematic, yet give us greater insight in such genealogical knowledge as actually inform social interaction, than the solicited data. When such kinship chains as were traced spontaneously by *ʿumiriī* actors, the following interesting tendencies can be spotted; however, precisely because

we are dealing here with *spontaneous* verbal utterances, the ethnographer cannot systematically control their production and collection, and therefore statistical testing is out of the question.

In the discussion of genealogical knowledge it is useful to distinguish between the length of genealogical chains, and their contents, particularly the horizontal or vertical nature of the links out of which these genealogical chains consist; the horizontal links connect people of the same generation, the vertical links people of successive generations.

4.2.1.1. *Length of chain*

Genealogical chains have a certain length: the number of elements (persons) occurring in them; for instance, if person A is the MBD of person B, then the genealogical chain linking A and B consists of three elements: M, B and D. In this sense the spontaneous produced by Ḥumīrī actors chains are never longer than six elements, and in the great majority of cases they merely comprise one, two or three elements. For the actors, distant kinship obviously is too irrelevant to explicitly and spontaneously mention as a ground for day-to-day interaction. This tallies with my statistical analysis of the significance of kinship in the recruitment of interaction partners (van Binsbergen 1970 and in press (a)).

4.2.1.2. *Emphasis on horizontal links*

We can also make empirical generalizations about the contents of the genealogical chains which Ḥumīrī informants produce spontaneously. Genealogical chains are series of elements, in a fixed order; the nature of the link between two successive elements is given by both the order and the meaning of the constituent elements. This links can be distinguished in vertical links (connecting two generations: child/parent or parent/child) and horizontal links (the connexion between siblings, or the affinal connexion between spouses). In the chain MBD the transition from Ego to M, and from B to D, is a vertical link, while the transition between M and B consists in a horizontal link. In the chain ZHBS the transitions between Ego and Z, Z and H, H and B are all horizontal, while that between B and S is vertical.

Now if between two individuals one could trace more than one genealogical chain, Ḥumīrī actors in their spontaneous pronouncements almost invariably produce the shortest possible chain. In most cases the longer chain is that one which traces the agnatic kin relationship (with emphasis on vertical links), while the shorter chain comprises one or more affinal (and therefore horizontal) links. The possibility of multiple chains is implied in the Arabian context of endogamy; in north-western Tunisia, I found²¹ that kindred endogamy (including agnatic endogamy, e.g. the famous FBD

²¹ #CASE 4.XXX. A PREPOSTEROUS VIEW OF NORTH AFRICAN, ARAB AND ISLAMIC MARRIAGE. Originally, my 1970-1971 studies contained a very extensive analysis of the Ḥumiri marriage system, tucked away in a considerable number of quantitative appendices distributed over both volumes. This led to a number of eye-opening results, notably the fact that (reflecting the comprehensive emphasis on the bilateral and affinal kindred that in general characterises interaction in Ḥumiri villages) the Ḥumiri marriage pattern is characterised by *kindred-endogamy in the broadest possible sense* (comprising patriline, matriline, and affinal relationships established by marriages in previous generations). In this

marriage) accounts for as much as 30% of all marriages (depending of course on the definition and demarcation of the kindred, see elsewhere in the present study). The horizontal tendency in the spontaneous tracing of genealogical chains amounts to a situation where kin relations are primarily traced by reference to contemporaries, while also the persons who function as linking elements in the genealogical chain are preferably selected from among contemporaries. In Ḥumīriyya, spontaneous tracing takes place by reference to persons who are personally known to, or remembered by, the speaker, and with whom the latter has a personal relationship – rather than via higher-generation agnatic ancestors, about whom the contemporary actors have merely a stereotypical or nominal knowledge based on hearsay. In the case of conflict over land one does refer to a very schematic patrilineal genealogy of higher-generation male ancestors. However, enumerations of long chains of names, generation after generation, and featuring not only male lineage members but also female lineage members and women married into the lineage – that type of genealogical knowledge only becomes topical when an alien anthropological researcher comes along. There are no local specialists whose task it is to administer genealogical knowledge in either written or oral form. Neither are there specific occasions when, in mutual agreement between those concerned, the genealogy is explicitly altered in order to bring it more in line with the actual relationships between the various descent groups in society.²² The only systematic knowledge which children are taught systematically consists of the chain of their direct patrilineal ancestors: F, FF, FFF etc. It is only a minority which later deepen their genealogical knowledge by conversations with old men and women, to include collateral ancestors, and their spouses and affines, in higher generations.

For genealogical research all this means that the genealogies which an ethnographer may collect in the area have always an artificial nature. If an informant wishes to state his collateral ancestors (*i.e.* the siblings and cousins of his direct lineal ancestors), or the spouses and affines of lineal and collateral ancestors, then he can never fall back upon fixed series imprinted in his mind through rote learning, but he has to mobilize his individual, specific, concrete information about these people who lived in the past. Such information may sometimes have been acquired through direct interaction with the persons referred to, notably in those cases when the persons listed belong to the informant's own generation or adjacent generations. For higher generations, or with regard to people who have migrated away from the local community and no longer sustain contact with it, the informant has to rely on fragments of information which he may have incidentally acquired from third parties in the course of his life.

light we must take considerable distance from the regional literature's habitual stress on *agnatic endomany*, especially in the form of the famous FBD marriage. From the perspective of my extensive 1970-1971 results, based on the painstaking qualitative and quantitative processing of all possible / traceable kin ties between the marital partners involved, the FBD marriage is merely a specific case of kindred endogamy, and hardly anything more. In the present redaction of my work, for clarity's and continuity's sake, these appendices have been suspended and their data and analyses selectively incorporated in the main text. As a result, my discussion of Ḥumiri marriage in the present two volumes could only be far less exhaustive; the reader eager for further details must be referred to the Dutch originals, van Binsbergen 1970xxx and 1971xxx.

²² Such occasions do occur in other societies, *e.g.*, among the Nigerian Tiv people (Bohannon 1953), *cf.* Fortes 1953.

With this state of affairs there is little wonder that the intensive genealogical research which I carried out in north-western Tunisia, reveals not only major gaps in the genealogical knowledge of the various informants, but also major differences between informants. Even when my informants belonged to the same family, their genealogical views were not consensual with regard to higher generations and to distant kinship, but even with regard to close kinship and to the informant's own generation and immediately adjacent generations! And even among individual informants genealogical information turned out not to be stable. Whether one recognizes someone as a kinsmen, specifically as an agnate, is closely connected with the existence of day-to-day interaction and of a positive trust relationship with that person. Because the pattern of day-to-day interaction changes considerably over the years, what also changes in the process is the extent to which a particular individual is prepared to consider other around him as kinsmen, *regardless of such actual historical genealogical chains as might have been traced by an objective outsider with hypothetical access to full data of local family history*. This means that individual genealogical statements display an element of opportunism: one does not spontaneously mention kinsmen who are enemies, or if one does mention them one disassembles the kin relationship.

It is remarkable that all informants, when asked for the series of their siblings, will first mention the brothers (ordered according to age), and only then the sisters. When comparing data on brothers and sisters it is important to realize that in north-western Tunisia the difference in marital age between husband and wife tends to be 5 to 15 years; therefore, most girls will have left their parental family by the time most of their brothers will be married. Marriage is virilocal in 95% of all cases, so brothers tend to remain in the same village after marriage. When a girl marries this does not always mean that she disappears from the day-to-day sight of her brothers. In this society about 50% of all marriages is village-endogamous; but the 50% who do marry outside the village where they were living just before marriage, only rarely return there: hardly for informal family visits, but mainly in the setting of specific formal occasions – for rare life-crisis ceremonies, for the annual festival of ‘Ayyid al-Kabīr, and twice a year in the context of compulsory pilgrimages to the shrines in their village of origin. Therefore, when sisters are habitually mentioned as a series after a full series of brothers in formal genealogical statements, this partly reflects the gradual disappearance of sisters beyond their brothers' social horizon, but there must be another factor in addition: the effect of a social norm of male precedence, which is enforced in numerous other aspects of Ḥumīrī life.

The pattern underlying the production of spontaneous genealogical knowledge also affected the formal genealogical statements as solicited in my formal interviews.

The purpose of my genealogical research was primarily to gain insight in the social organization of the research area, as a background for the interpretation of residential history, segmentation, and the relation between shrines and social groupings. My data on living persons were derived from a census which I took personally, and from intensive daily participation.

In addition to the non-consensual, opportunist aspect of genealogical knowledge in north-western Tunisia, the collection of reliable genealogical information was influenced by some other factors. My research assistant hailed from the local chiefdom, and although not from the research area itself, knew many of our informants and their

families. He had several years of experience with genealogical research, and was keen on spotting irregularities in the informants' statements. On the other hand, there is no doubt that even so individual communication errors have had a negative effect on the quality of the data. Many interviews would proceed for half an hour or so with only the informant, myself and the assistant present, but then often members of the informant's household, or neighbours, came to interrupt the conversation, steering it away from the systematic and often boring insistence on genealogical information. Most informants would offer their genealogical information without reticence, and especially those who considered themselves knowledgeable on this point took a certain pleasure in these interviews, but most informants would after about fifteen minutes lose interest in this rather boring and impersonal form of data collection, or would be too irritated or embarrassed by the confrontation with their own manifest genealogical ignorance.

In an unknown number of cases, finally, genealogical knowledge which was present was yet denied to me, or was presented by the informant in a purposely distorted way. Local ideology stresses the positive value of living on the land of patrilineal ancestors, and considers all other forms of land acquisition (purchase, donation, theft, matrilineal inheritance) as second rate. However, migration of individual and small residential groups has been a constant and important aspect of this society. Therefore many informants can only mention a few patrilineal ancestors who lived in the same place (village, valley) as they themselves. One dissembles genealogical knowledge concerning ancestors who lived elsewhere, or claims – against one's better knowledge – that they did live in the same place as their present-day descendants. By the same token, the dynamics of honour and shame rendered it difficult to obtain complete and reliable information on all marriages which a person may have contracted in the course of his or her life. Marriages may be dissolved by death or by divorce, specifically by the simple Qur'anic dismissal of the wife which before the alteration of the Tunisian family legislation shortly after Independence (1957) was no rare occurrence; remarriage of both men and women, levirate and polygyny²³ have produced a very complicated pattern of marital relationships, which confuses the ethnographer and which among the actors is often sufficiently embarrassing to distort the factual truth.

4.2.2. Types of ancestors in genealogies

The persons whose names are mentioned in the genealogies, can be divided into two types: mythical ancestors and historical persons.

Historical persons are the members of the informant's own generation, adjacent generations, and a few generations above. The informant has known some of them personally, of others he has learned the names (with additional information on place of residence, other anecdotal detail, often also marital relations) from close kinsmen belonging to higher generations. The genealogical chains which the informant traces

²³Polygyny, although permitted by formal Islam, and sporadically practised in *Humiriyya* (where a loose form of popular Islam prevails, with a remarkably conspicuous admixture of Judaism; see Volume II) was no longer legal after 1957; during the main fieldwork in the late 1960s, polygynous marriages contracted before 1957 still existed but were in the process of dying out.

between these historical persons he considers as historically correct – or at least as close to the historical truth as is considered fit for public consumption. Comparison of the statements from various, closely-related informants with regard to historical persons featuring in genealogies will still show omissions and contradictions, on the basis of which the ethnographer may often reconstruct the historically correct genealogical chain, provided he has sufficient data at his disposal. For these individual variations can be relegated to a limited number of principles genealogical manipulation, in addition to the opportunism already discussed above:

- A genealogy may be pruned by the elimination of those persons who played only an insignificant role in the past.
- Persons belonging to lineage segments which had a residential history different from the informant's segment, may be eliminated from the genealogy; they are omitted because dwelling at a distance from Ego's direct, lineal ancestors has obliterated the sense of kinship.
- Likewise, one may eliminate from the genealogy persons who belong to lineage segments with virtually the same residential history as the informant's, but whose genealogical link with the latter lies in so distant a past that it is no longer remembered.
- One may present the members of the lineage in a different genealogical connexion than corresponds with historical reality (telescoping).
- One may include in the genealogy persons who historically are no true agnates of Ego and of the others included in the same genealogy.

I have elsewhere (van Binsbergen 1970 and in press (a)) discussed and illustrated the social-structural background of these principles.

Mythical ancestors can be easily distinguished from historical persons. Not all genealogies contain mythical ancestors. If they do, mythical ancestors are always found in the apical generations. Mythical ancestors constitute only a small set, whose names are known to everybody. In and around the research area, only about ten different mythical ancestors were recognized. Some mythical ancestors also feature in local myths and legends. Some actors claim close-agnatic relationships to exist between various recognized mythical ancestors: one would be the F or B of the other. The ethnographer is inclined to interpret such a claim in terms of historical relationships between clans in their relations of dependence and struggle for autonomy, contesting such scarce resources as springs and pastures. And even the actors concerned may recognize the allegorical nature of these claimed kin relationships between mythical ancestors. There is little consensus with regard to the way in which mythical ancestors might be related to one another; some actors even deny any such relationship.

When an informant sums up the chain of his direct patrilineal ancestors, the transition between historical persons (the lower generations) and mythical ancestors is often signalled by a certain hesitation. In fact (as was made clear by the informants themselves on many occasions) the transition between the highest historical person and the lower (or only) mythical ancestor in the genealogy is often not considered, by the informants, to be a factual S/F relationship, but as a patrilineal connection across an unspecified number of generations. In the informant's summing-up the mythical

ancestor may sometimes already appear immediately after, that is above, the FF; the informant is then manifestly conscious of the fact that many historical ancestors separate this FF from the mythical ancestor, but is unable or unwilling to specify the names of the intervening ancestors.

Mythical ancestors form the basis for clans. The names of clans are derived from those of mythical ancestors, via a suffix *-iyya* and a vowel change. For instance, the clan name ⁶Arfawīyya is derived from the name of the mythical ancestor ⁶Arfa, Mayziyya from Bu Maza, etc. When members of a particular lineage seek to affiliate to a clan which is already known to comprise a number of other named lineages, they may initially merely adopt the clan name, and only at a later stage add the corresponding name of the mythical ancestor to the individual genealogies. In the research area mythical ancestors are never women.²⁴ Ḥumīrī culture distinguishes yet another type of ancestors, which like all ancestors are designated by the generic term *jadd* (plur. *jadūd*): the saints which are associated with local shrines such as are distributed, in various types, across the spatial segments. Within a certain area (*e.g.* a valley) the names of these saints constitute a fixed set known to every inhabitant, just as is the case with the mythical ancestors. Some local myths claim a relationship to have existed between certain non-saintly mythical ancestors and certain local saints, *e.g.* one is presented as the son or the servant of the other. In very rare cases the set of mythical ancestors overlaps with the set of saints: among the scores of names of Ḥumīrī saints and of Ḥumīrī mythical ancestors circulating in the area, I have known only one saint (notably Sidī Mḥammad, who features prominently in the present study) to feature in genealogies as a mythical ancestor.²⁵ The saints are considered as ‘ancestors’, with this one exception they never feature in genealogies. That they are none the less considered as ancestors stems from a number of considerations:

- The interaction between man and invisible saint follows the role pattern between grandchild and grandparent.
- Much like real ancestors, the saints function as labels for the integration of present-day contemporaries, through reference to the latter’s shared relationships with people in the past.

The above applies to Ḥumīrī genealogical knowledge with regard to agnates. I did not systematically investigate the extent of vertical historical knowledge with regard to cognates and affines. In interviews I seldom pressed in this direction, because I usually had at my disposal such genealogical information concerning the kin groups of informant’s cognates and affines as were derived from these group’s agnatic members. Many cases however show that among men the genealogical knowledge concerning their cognates and affines was usually much less extensive than that concerning their own agnates. For (elderly) women this statement does not seem to hold true: they tend to be as knowledgeable about the agnatic group into which they have married as about their own lineage, even if their marriage was not lineage-endogamous.

²⁴ Cf. Peters 1960.

²⁵ However, cf. Demerseman 1964 and also Souyris-Rolland 1949; these authors, on the basis of their fieldwork in the 1930s-1950s, claim a larger number of saints to occur in genealogies, mainly as mythical ancestors.

4.3. Examples of the actors' genealogical information and interpretation

4.3.1. Introduction

In order to substantiate my argument I will now demonstrate for some ortholineages²⁶ the problems of the lack of consensus with regard to historical knowledge, the actors' systematic genealogical manipulation, and the analytical reconstruction of ortholineages by the ethnographer.

I shall limit my discussion to genealogical knowledge, and not touch on the related problem of the manipulation of information on the places of residence in the past of the people whose names feature in the genealogies. The places of residence mentioned in the present appendix are those which I have myself reconstructed on the basis of contradictory information from the actors.

In the first two examples (the first relating to the ortholineages 6 and 7, the second to ortholineage 1) I shall show part of the genealogical data as offered by the informants in interviews. It was not nearly possible to cite all informants on these points. In order to be able to present the data in a clear and simple fashion I shall take my own reconstruction of the ortholineages in question as my point of reference. I shall not show the process of this reconstruction in itself: that would require the presentation of at least ten times more genealogical data, with extensive commentary accompanying every little step in the reconstruction. Meanwhile the present selection of data will give the reader some idea as to the possibilities for and the procedures to be applied with regard to such a reconstruction. It will also become clear that these reconstructions can never be based on absolute certainty: at least in part they continue to consist of conjectures and half-truths.

With every example I give an excerpt from the genealogy of the ortholineage. There generations are indicated by letters, and persons per generation by figures. The genealogical relations as claimed by individual informants thus take the following form:

b₃ < a₁	(b ₃ is a child of a ₁)
a₁ > b₃	(a ₁ is the parent of b ₃)

²⁶ I have coined the concept of ortholineage (as discussed in van Binsbergen 1970 and in press (a), Part I), to denote an objective, etic account of actual genealogical (including marital and affinal) relationships arranged – in agreement with the dominant societal ideology of Ĥumiriyya and other Arabising / Islamising societies – in a dendrogram (tree Fig.) of patrilineal descent, and unaffected by such conscious and unconscious distortion and manipulation as inevitably affects the individual personal accounts of participants concerning their perceived kin network. I spent many months sorting out and collating ca. two hundred of such contradictory accounts so as to produce one master genealogy comprising all the ortholineages of the research area; that master genealogy has served as background for the numerical analyses in the present argument, and is available to the reader at: http://www.quest-journal.net/shikanda/Berber/genealogy_comprim_trim.pdf. The obvious counterpart of the ortholineage is the pseudolineage: the emic conception, by one individual participant or a small group of closely related participants, of their subjective conception of their kin environment in terms of unilineal descent. On the distinction between emic and etic, fundamental for the anthropological gaze, see Headland et al. 1990; van Binsbergen 2003: 20 f.

b₃ * b₇	(b ₃ married to b ₇)
b₃ (1) * b₇ *(2) b₈	(b ₇ first married to b ₃ than to b ₈)
b₃ + siblings	(informant specifies b ₃ 's brothers and sisters but summing up of their names is irrelevant for the problem at hand)

Table 4.1. Typical formats of genealogical information.

The top of the ortholineage genealogy is formed by an ancestor about whom the ethnographer has sufficient information to be certain of his identity, historical status and genealogical position. Mythical ancestors therefore do not belong to the ortholineage; therefore in the rendering of the individual informant's genealogical statement the names of mythical ancestors, and of the clan names deriving from them, are written in full; the same applies to other insufficiently documented apical ancestors in individual statements.

Wherever the individual statement is in contradiction with the reconstruction of the ortholineage, this is indicated by (!).

The informants' individual genealogical statements, and the comparison between them, will offer examples of the systematic operations. Let me summarise these operations (discussed in van Binsbergen 1970 / in press (a) Part I) briefly:

- (a) general elimination of persons from the genealogy;
- (b) elimination because of out-migration;
- (c) elimination because of the growing apart of the ortholineage, without out-migration;
- (d) telescoping;
- (e) fusion the lineage level or the clan level.

These operations can also be found in the third example, which derives from ortholineage 5. The main point about this example is that it shows the extent to which manipulation of genealogical knowledge concerning people in the past is a reflection of the pattern of dyadic relations between people living now.

4.3.2. Example 1. Ortholineages 6 and 7

Fig. 3 gives an excerpt from the genealogy of the reconstructed ortholineage, while selected informants' statements are presented in Table 4.3.

How do the individual informants' statements of Table 4.3 illustrate the systematic genealogical operations a -e as listed above?

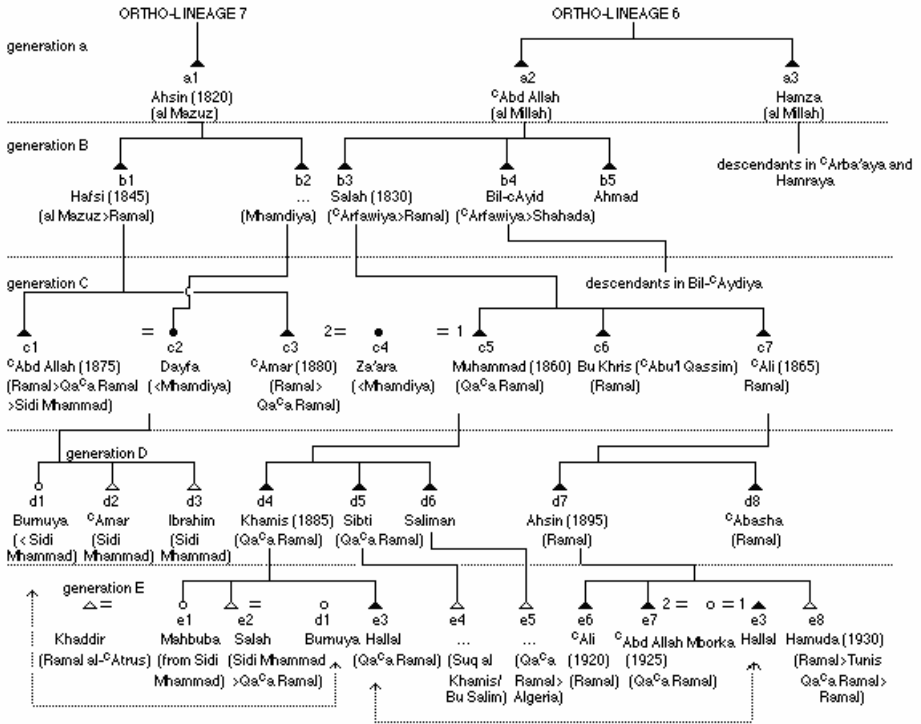


Fig. 4.4. An example of genealogical information

Informant: d₁₊₂

Statement:

d₁₊₂ < c₁ < b₁ < a₁ < Suasi <(!) Salah <(!) Mutani <(!) 'Arif < 'Arfa < 'Arfawiyya

siblings c₁ (including c₃)

c₃₍₂₎ * c₄ * (1) c₅

c₅ <(!) 'Abd Allah <(!) Salah <(!) Mutani <(!) 'Arif < 'Arfa < 'Arfawiyya

Informant: e₂

Statement:

e₇, 8 < d₇ < c₇ <(!) c₅ < b₃ < a₂

Informant: e₇'s widow, who is also the widow of e₃ by an earlier marriage

Statement:

e₇, 8 < d₇ < c₇ < b₃

Informant: e₂

Statement:

e₂ < d₄ < c₅ < b₃ < a₂ <(!) 'Arfa < 'Arif

e₂ * d₁

d1-3 'as cousins' of e1 (!)

siblings c5 is c6

claims the name of Mutayniyya, but fails to specify the chain of descent from Mutani. The same informant was however able to do so in 1967: Hartong (1968: 57) records as his statement:

'e2 < d4 < c5 < a2 < (!) Metenni [Mutani] < (!) Salah < (!) Arif [^cArif] < Arfa [^cArfa]'

Informant: e1's wife

Statement:

e1,2 < d4 < c5 < b3

the siblings of c5 are c6 and c7

c7 > d7, 8

d7 > e6, 7, 8

d1, 2, 3 are not agnates of e1 c.s.

Table 4.2. Pseudolineages: Some informants' genealogical statements concerning the ortholineages 6 and 7.

- The occurrence of operation (a) cannot be demonstrated.
- Operation (b) is evident: no informant could mention the names of the siblings of a1, a2, b1 and b3. No doubt this is connected with b1's out-migration from the valley of al-Mazuz and b3's out-migration from the village of ^cArfawīyya, c. 1890.
- From Hartong (1968), Miedema (1967: Appendix) and Huitzing (1969) we can reconstruct the place of b3 in ortholineage 6. Salah-b3 is the son of ^cAbd Allah bin Mabruk, who lived in the village of ^cArfawīyya towards the end of the nineteenth century. After losing the battle with the inhabitants of the village of Mhamdiyya, the sons of ^cAbd Allah, among others, moved: Bil-^cAyid (b4) went to live in the valley of Shahada; his descendants can still be found in the village of Balaydiyya, named after Bil-^cAyid (Bos 1969). Salah (b3) took up residence in the valley of Sidi Mḥammad; the descendants of Ahmad (b5) now inhabit the village of Habash-Karash (Miedema 1967: appendix). Other members of this large ortholineage in 1968 lived in the village of ^cArba'aya (including the descendants of Hamza a3), Hamraya, ^cAin Tatri, ^cAin Kabira and Habash-Karash. Between them and the descendants of Salah b-3 there is no awareness of specific agnatic kinship any more; even at the less strictly defined clan level they deny that Salah b-3 belongs to the ^cArfawīyya clan with mythical ancestor ^cArfa or ^cArif (Hartong 1968)...
- Hafsi (b1) cannot be incorporated in any ortholineage from the research area as known to me. His mother hailed from the village of Kashayrdiyya, half a kilometer from ^cAin Drāham., whose inhabitants are counted as members of the ^cArfawi clan. Hafsi's family's strong orientation towards Mḥamdiyya is clear from the fact that b2 (also c1's father-in-law) lived in Mḥamdiyya, while also c3's wife hailed from there. Probably Hafsi-b1 belonged to one of the ortholineages in the village of Mḥamdiyya, or perhaps to the clan of the Ulad al-Hadjdj, and not to that of the ^cArfawīyya. Perhaps at the time of the Mhamdi/^cArfawi conflict Hafsi had sided with the ^cArfawīyya, and therefore had to move to an ^cArfawi-dominated valley after the ^cArfawīyya were defeated by the Mḥamdiyya.
- Operation (c) does not appear to be detectable in the statements as presented

here.

- Operation (d) is clear in the statements by e₁ and d₁₊₂. A comparison of the estimated ages of the persons featuring in the statements is an additional argument against the deviant statements of these informants; such estimates I base on an average generation span of 25 to 30 years. With e₁ we see the attempt to make the c₅ branch subservient to the informant's own c₃ branch.
- Operation (e), fusion, is very manifest in these statements, both at the lineage level and at the clan level.

In order to ascertain the occurrence of fusion at the clan level we should first direct our attention to the name of Mutani. Mutani is the mythical ancestor of a clan which must already have existed locally when b₁ and b₃ immigrated into the valley of Sidi Mḥammad, c. 1890. I do not know to which ortholineage Mutani belonged. Many consider Mutani as the descendant of the mythical ancestor ʿArfa (or ʿArif) – but such an opinion might be based on fusion between the Mutayni clan and part of the ʿArfawi clan. The Mutayniyya would then constitute a sub-clan of the ʿArfawiyya. The clan name and toponym Mutayniyya is associated with land which since the 1860s was the place of residence of the ortholineages 3 and 20, now disappeared from the research area. Ortholineage 3 derives from the chiefdom of Tabayniyya, to the south of the research area. Ortholineage 20 came from the north, and of old is counted as a part of the ʿArfawi clan. Hartong (1968) discussed ortholineage 20 under the name of ‘Ombarkia’ [Umbarkīyya]. Members of ortholineage 3 are still designated by the clan name of Mutayniyya, even though now they live at about 1 km distance from the land to which the name of Mutayniyya is attached. Ortholineages 3 and 20 must have adopted the name of Mutayniyya according to one of the procedures as described here: *through affiliation to the local core lineage of the Mutayniyya, or through the simple adoption of the toponym*. The members of ortholineages 3 and 20 did adopt the clan name / toponym of Mutayniyya, but (as far as I know) they did not go to the extent of incorporating the name of Mutani in their own genealogies.

The land called Mutayniyya²⁷ has of old been the place of residence of the ʿArfawiyya. It is adjacent to a mountain formation which is named Raqubat ʿArfa after the mythical ancestor ʿArfa, and also gives on to the southern valley of Babush, from where the ʿArfawiyya spread in northeastern direction in the second half of the nineteenth century (Hartong 1968; Miedema 1967). Because of the ʿArfawi connotations of the name of Mutayniyya the members of ortholineage 3 can now call themselves ʿArfawiyya, too. There are no indications that by 1890, when b₁ and b₃ arrived, members of the core lineage of the Mutayniyya still resided locally. However, good relations existed between the new arrivals and the members of ortholineage 3: the latter had intervened as negotiators in the conflict with Mhamdiyya (Huitzing **in preparation**).

²⁷ Here we need to appreciate the fact that, whatever the dominant patrilineal ideology, in fact constant and unconscious oscillation between descent and locality is the central feature of the Ḥumiri's perception of their social environment. Throughout my work on Ḥumiriyya, and especially in the present Volume I, I have stressed and analysed this feature. I also made it the cornerstone of my theoretical approach to ethnicity, group names, and place-names in the Mediterranean, as part of our monograph on the Late-Bronze Age Mediterranean Sea Peoples (van Binsbergen & Woudhuizen 2011).

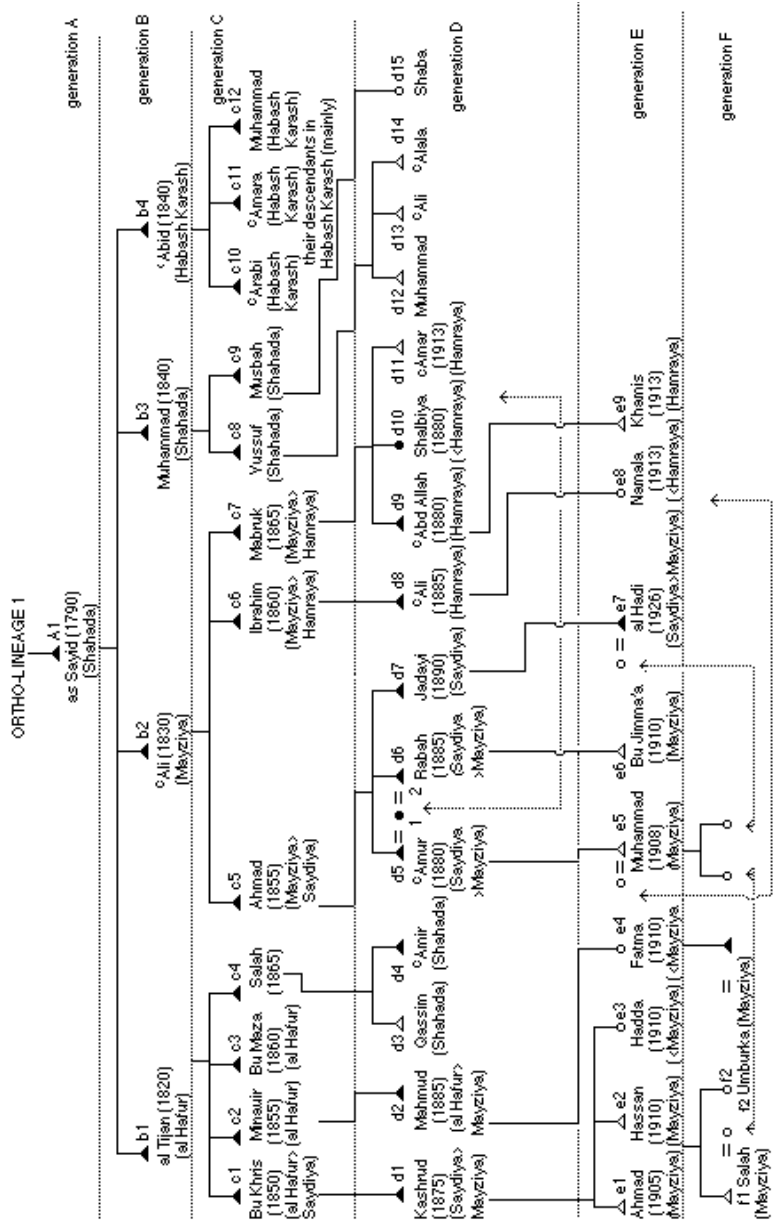


Fig. 4.5. Genealogy of the reconstructed ortholineage 1

However no further affiliation between the new arrivals b₁, b₃ c.s., and the ortholineages 3 and 20 was brought about: the latter shifted their place of residence, and there were no further marital relations. The new arrivals must have acquired the name of Mutayniyya because that name was attached to their land. By contrast with the members of ortholineages 3 and 20, the new arrivals did incorporate the name of Mutani in their genealogies, as is manifested by Table 4.3.

Besides these complicated affiliations at the clan level the statements by d₁₊₂ and e₂ also show mutual affiliation at the lineage level. Probably the name of Suasi still belongs to ortholineage 7, while the name of Salah (in d₁₊₂'s statement) probably derives from ortholineage 6: there it appears as b₃. The name of Salah-b₃ sufficiently stands out in local history to be suitable for such an attempt at affiliation; the two agnatic groupings of which the descendants of b₃ consist are still known as the Ulad Salah. So much does the name of Salah occupy a key position in this affiliation that Salah (b₃) is presented as the *father* of 'Abd Allah a₂: the latter, according to my reconstruction Salah's father and not his son, is likely to derive from ortholineage 6 as well, although members of both ortholineage 6 and 7 do not know anything about him except for his name.

3.3. Example 2: Ortholineage 1

Fig. 4.4 offers an excerpt of the genealogy of the reconstructed ortholineage 1. Table 4.4 shows the statements of selected informants.

In these statements the name of as-Sayyid (a₁) must not be confused with the mythical ancestor of that name, the founder of the clan of the Ulad bin Sayyid north of the research area. However, it is not impossible that a₁ gave his name to the valley of Saydiyya, east of the research area. Since the name of Saydiyya is also of a clan-like nature, in the latter sense a₁ might yet be considered a mythical ancestor. However, in the statements in Table xx.4 the person designated as 'a₁' functions as a historical, 'remembered' ancestor.

A comparison of these individual statements leads to the following conclusions.

- Again, operation a cannot be demonstrated.
- With this ortholineage, operation (b) is very striking. Because of the many migrations several informants lack the awareness of kinship vis-à-vis other branches, even when there are marriage relations. An obvious objection to this line of reasoning would be that interviews may bring out what an informant does know but not when he or she does not. However, the case of e₈, who offers absolutely non-converging chains of descent for herself and her husband (while the latter is her FFBS and MZS at the same time), is very convincing: probably the informant does really not know that she is her husband's agnate. Here we see the significance of an informant who occupies a strategic position not so much through age but because of his or her genealogical position; such an informant is d₄. Although the individual statements do not greatly overlap, and although cognates who are also agnates are often not presented as agnates

by the informants, it yet proves possible to patch the branches of this ortholineage together. It is remarkable that no informant traces descent back to b₄ and his descendants. Yet we must assume that this branch belongs to ortholineage 1; if not, the existence, at al-Hafur / Habash-Karash, of a son of a man called as-Sayyid and born c. 1840 would be an incredible coincidence (Miedema 1967: appendix).

<p>Informant: e6 Statement: e6 < d6 < c5 < (!) Zaghdudi [= Zaghaydi, member of the clan with mythical ancestor Zaghdud] siblings d6 spouses of siblings d6 father, sometimes father's father, of spouses of siblings d6 d10 < c7 < b2 e3 < d1 the persons in the preceding two lines are not presented as agnates of e6(!)</p> <p>Informant: e8 Statement: e8 < d8 < c6 < (!) a1 e8 * e5 e5 < d5 < c5 < b2 < Zaghdudi the persons in the three preceding lines are not presented as each other's agnates (!) among the spouses of e8's children: f1 < e1 < d1 e7 < d7 < c5 the persons in the preceding two lines are not presented as agnates neither of each other nor of e8 (!)</p> <p>Informant: e9 Statement: e9 < d9 < c7 < b2 siblings c7 are c5, c6 c5 > d5-7 among others the sons of d5 and d6 are stated</p> <p>Informant: e4 Statement: e4 < d2 < c2 among the spouses of e4's children: f2 < e1 < d1, recognised as distant agnates of e4 but without specifying the chain</p> <p>Informant: e2 Statement: e2 < d1 < c1 < b1 < (!) Bu-Maza < (!) Zaghdudi among the spouses of e2's siblings: d4 < c4 < b1, recognised as agnates of e2 siblings d1 are stated among the spouses of d1's siblings: d3 < c4 < b1, recognised as e2's agnates</p> <p>Informant: d11 Statement: d11 < c7 < b2 < a1 < Bu-Mandjil < Mḥammad < Muḥammad among the spouses of d11's siblings: d8 < c6 < b2 < a1 < etc, recognised as d11's agnate d10 * d6 > (!) e5, where d6 and e5 are not presented as d11's agnates siblings of c7 are c5, c6 siblings of b2 are b1, b3 b1 > c1, c4, (!) d2 b3 > c8, c9 c8 > d12-14 c9 > d15</p>

Table 4.3. Pseudolineages: genealogical statements by selected informants with relation to ortholineage 1

- Operation (c) cannot be demonstrated: the branches do grow apart, but this is always accompanied by migration.
- Operation (d) can be discerned in the statements by e8 (notably: c6 < a1) and d4 (notably d2 < b1). Another mistake is that d4 claims d6 to be the father of e5, and not the correct person d5; d6 is d5's brother and he contracted a levirate marriage with d5's widow.
- With these statements, operation (e), fusion, cannot be demonstrated at the lineage level but it can at the clan level.
- Probably the series stated by d4 (Bu-Manjil < Mḥammad²⁸ < Muḥammad) is historically the more reliable. Not only because of the strategic position of this informant, but also because he – by contrast with most other informants – does not take recourse to the stop gap of frequently claimed mythical ancestors (Bu-Maza and Zaghdud). When the others claim membership of the Mayzi or Zaghaydi clan this must probably be seen (on the basis of data which I shall not discuss in this context) as an attempt to affiliate to ortholineage cores which could boast a longer history of *permanent* local resident in the informant's village; for although branches of ortholineage 1 have lived at many places in and around the research area, they have always migrated frequently. Moreover there is the recent development in the direction of a moiety-like structure: the ^cArfawi / Zaghaydi opposition such as has dominated the social and ritual organisation of the four contiguous valleys of the ^cAtatfa tribe ever since the advent of the ^cArfawiyya descent group in the 19th c. CE. Informant d4's statement suggests that ortholineage 1 is associated with the Manajliyya clan, whose clan name / toponym is still associated with land near the village of Hamaysiyya (near where a1 and his sons resided); however, to the best of my knowledge this name is no longer used, in those parts, to denote a set of people tracing explicit descent from a mythical ancestor Bu-Manjil.

3.4. Example 4.3. Ortholineage 5

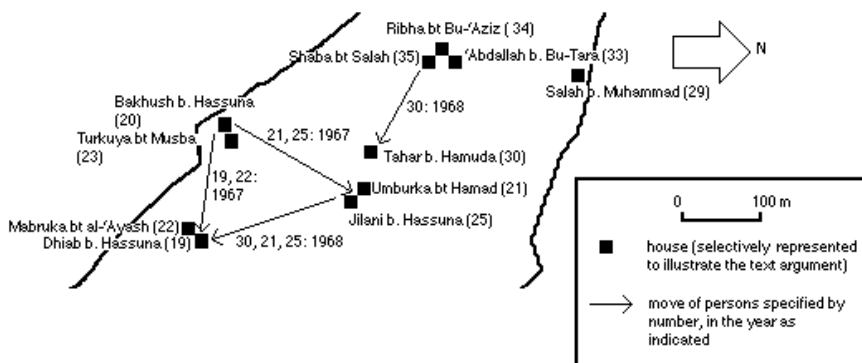
For the discussion of the genealogical manipulations around ortholineage 5 I shall take the statement by informants 20 and 25²⁹ as my point of departure. These are two brothers, about thirty years of age, both married and living in their own house, at a distance of c. 200 m from each other (in 1968). Their father and grandfather had been chiefs in the epoch of French colonial rule (*cf.* note 37, p. 104), and left them a very

²⁸ Tunisia was under Ottoman / Turkish rule during much of the second half of the 2nd mill. CE. Mḥammad is the Turkish form of the name Muḥammad, and although identical in Arabic orthography, there is a marked difference in pronunciation in the local dialect of Ḥumiriyya (in the Turkish variant, the first vowel; sounds like -è-, not like -a-).

²⁹ In the first stage of writing up my field findings, for rapidity's sake I referred to my individual informant by numbers in a list of the village census. The present text is an intermediate product, where this regrettable alienating routine is still not redressed. The undesirable effect is that of a natural-science treatise on objects, rather than an account of human encounter, as all anthropology should be. Ultimately, in the final publication of this text, informants will be referred to by name, whether their own original name, or a pseudonym.

large inheritance.

The statements by 25 and 20 concern their fellow-villagers 30, 33, 34 and 35. The places of residence of these people are indicated in Fig. 4.



the map area occupied by this Fig. is loosely indicated by a rectangle in Fig. 2, above.

Fig. 4.6. Dwellings and residential movement of selected members of ortholineage 5, village of Sidi Mhammad, 1967-1968.

In Fig. 4.4, dwellings which are no relevant for our present discussion have been omitted, in the Fig.; arrows denote residential moves, of the heads of households as indicated, in the year as indicated. Heads of household no. 19, 21 and 29 are members of the kindred of 25 and 20, and we shall discuss them below. The principal kinship relations between the heads of household in Fig. 4.4 are summarised in Fig. 4.5.

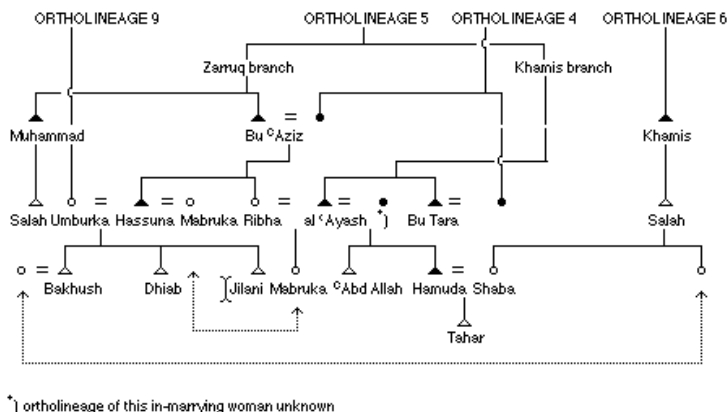


Fig. 4.8. Key kinship relations between selected members of ortholineage 6 and their neighbours

In Fig. 4.5, emphasis is there put on the shortest possible kinship chains, i.e. those

based on the present and the most recent past, without preference for agnatic ties over cognatic and affinal ties. It is marriage relations in the immediate preceding generations which create much of the social cement in Ḥumīrī village society.

How did informants 20 and 25 perceive their relationship with 33 and 30 (BS of 33) in terms of agnatic ties or otherwise?

statement by 20:

'33 belongs to a *firqa* [pseudolineage]³⁰ at Hamraya [about one hour on foot from 20's house], which again goes back to the firqa of Hadjdj Mḥammad at al-Hafur [which is again half an hour on foot beyond Hamraya]. My father [the chief] gave some land to the family of 33, else he could not even have lived here.'

statement by 25:

'30 belongs to our firqa, for his ancestor Salah bin Hamis was a grandson of our ancestor Zarruq. He had a right to the communal land, just as all the other descendants of Zarruq.'

So number 20 denies implicitly all kinship, and relegates 33 to a dependent immigrant whose family has come from as far away as possible, while 25 affirms agnatic kinship between himself and 33 /30, in recognition of the land use rights which the latter derive from this. Now there is abundant data at our disposal on the basis of which the historical truth can be reconstructed reliably (Hartong 1968: 62; the data collected by Ernsting and Geschiere in 1968 at Hamraya and al-Hafur; and my own data deriving from Hamraya and Sīdī Mḥammad.) An excerpt from the genealogy of ortholineage 5 is presented in Fig. 4.6.

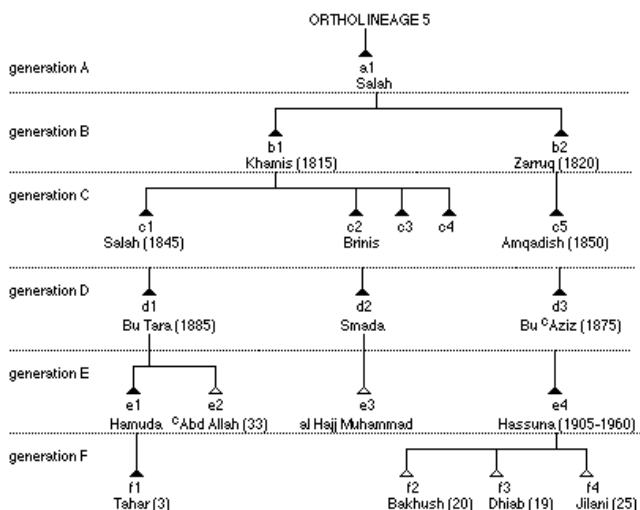


Fig. 4.9. Excerpt from the genealogy of ortholineage 5.

³⁰ This is what the term *firqa* فرقة amounts to in Ḥumiriyya. In standard Arabic the term means 'band, relatively small group of warriors or musicians'; Doniach XXXX: s.v. فرقة .

The relevant local history can be reconstructed as follows. Along with Zarruq (b2), Hamis (b1), born c. 1815 lived about 1 kilometer to the north of the present village of Sīdī Mḥammad. Today most of the descendants of Zarruq live in that village, to which their ancestors moved at the end of the nineteenth century. During the same period the son of b1 moved to Hamraya, whence some of them moved on to al-Hafur. However, Salah c1, one of Hamis's sons, did not join in this migration: he continued to live near the shrine of Sīdī Mḥammad al-Kabir. In c. 1915 Salah's sons Bu-Tara and al-ʿAtrus moved to the village of Sīdī Mḥammad, *i.e.* across a distance of scarcely one kilometer, and certainly not from Hamraya.

With 20 we see how the awareness of kinship is lost with regard to a branch from his own ortholineage; since migration plays a negligible role in this context, this factor (operation c) cannot be invoked to explain the phenomenon. At the same time we see operation (d) at work in 25's statement: he turns brothers (b1 and b2) into father and son.

What brought 20 and 25 to their contradictory, and in both cases historically incorrect, views of their relation with 33, 30 and the widows of the latter's close agnates. Informants 20 and 25 are full brothers with an age difference of 5 years at the most; so there is no conspicuous age difference or difference in generational position which would account for them being exposed to different genealogical information (*cf.* p.). The explanation must lie in individual differences between 20 and 25 in their dyadic relationship with 33, 30 and the widows.

- Diversity of opinion is already possible because 20 and 25 live at considerable distance from each other. My extensive data on and analysis of day to day interaction (van Binsbergen 1970 and in press (a) show that 200 meters is a distance where effective tie of neighbourliness are no longer in operation. In fact, the two brothers have virtually no contact with each other, and are in chronic conflict over their father's inheritance.
- 'Near neighbours', who tend to have intensive dyadic relations (mutashrin relations) with one another, tend to live no further from each other than c. 125 m. No one in the cluster of 30, 33, 34 and 35 is a close neighbour of 20. By contrast, 25 has the youthful 30 as his nearest neighbour, since the boy left his mother's house in spring 1968, after intense conflicts between his adolescent wife and his mother (35). The mother of 20, 25 and 19 is 21: she lives with 25 under one roof but in a separate apartment. 21 intervened in the conflict between 30 and his mother. As neighbours 30 and 25 have a lot of contact. The relationship is even so close that when in the summer of 1968 25 moved to the close proximity of 19, 30 moved along with him.
- Difficulties relating to the large inheritance, and the rapid professional and political career of 19, another brother of 20 and 25 resulted in 1967-1968 in a violent conflict between 20 on the one hand, and 19 and 25 (and additional brothers) on the other. The outbreak of the conflict was preceded by residential moves of 19, 25 and 21 (see Fig. 4). In the conflict 34 (the brother's FZ) and 33 took the side of 19; both 34 and 33 have a very high prestige in the village. An important role was also played by head of household 46 (WF of 20 and F of 35). He is a man of high prestige, living in another neighbourhood of the village of

Sidi Mḥammad. Through intimidation 19 managed to prevent 46 from siding with 20. The conflict was finally adjudicated in a court of law, and 20 came out as the absolute loser.

The difference in point of view between 25 and 20 with regard to their agnatic ties with 33 c.s. turns out to be a direct reflection of individual relationship, at a specific moment of time, within the village's continuously shifting pattern of interactions and relations.

Meanwhile we can analyse operation (d), telescoping, in the genealogical view of 25.

In 25's view, the descendants of Hamis (b1) have lost their historical relative autonomy vis-à-vis Zarruq (b2) and his descendants. Now we can assess which of the factors of genealogical manipulation are at play here.

Table 4.5 brings out the numerical dominance of the Zarruq branch.

ancestor	number of households (1968)		
	with a living male agnatic descendant as a head of household	with a widow of an agnatic male descendant as a head of household	total
Zarruq b. Salah	8	4	12
Ḥamis b. Salah	2	2	4
total	10	6	16

Table 4.4. The strength of two branches of ortholineage 5 in the village of Sidi Mḥammad, 1968

For the Zarruq branch we have included, in Tables 4.5 and 4.6, one head of household we does not live in the village of Sidi Mḥammad but at a distance of c. 700 meter in the nearest periphery of the village of Tra'aya-bidh. There is no doubt that this man is implied in the image the inhabitants of Sidi Mḥammad of the Zarruq branch; the man is wealthy, and participates daily in Sidi Mḥammad as the co-proprietor of the store annex men's assembly there; besides he maintains intensive relations with his brothers who still live in Sidi Mḥammad.

Is there a statistically significant difference in wealth between the members of both branches of the ortholineage 5?

My data on relative wealth have been measured (by applying a Kaufmann test,³¹ on an

³¹ In the study of small-sample communities the researcher may need a quick overview of the relative wealth position of the members, without having the means of going through a detailed assessment of each household's actual assets and liabilities. An accepted method, with which the name of Kaufmann is associated, is to make an individual record card for each individual household, and to let a few centrally placed members of the community state, roughly sort these cards in terms of relative wealth; subsequently, a rank correlation test (Siegel n.d.) is to determine the degree of agreement between the various assessors – only if the agreement is significantly above chance expectations, can the Kaufmann

ordinal scale with three classes (rich / medium ./ poor; cf. van Binsbergen 1970). The data on the relative wealth of both branches are presented in Table 6.

	number of households			
	rich	medium	poor	total
Zarruq branch	2	2	8	12
Hamis branch	0	1	3	4
total	2	3	11	16

Table 4.5. Relative wealth of the members of two branches of ortholineage 5 in the village of Sidi Mḥammad, 1968.

The data available do not show a significant difference in wealth between the two branches (Mann-Whitney U test corrected for ties (cf. Siegel n.d.), $z = -0.44$, $p > 5\%$).

However, the actors do not use statistics, and the conspicuous wealth of two members of the Zarruq branch may make them overlook the fact that the other members are just as poor as, or poorer than, the members of the Ḥamis branch.

The branches also displayed differences in prestige and power. From c. 1916 to 1957 the chiefs of the chiefdom ʿAtatfa were members of the Zarruq branch (the later chiefs belonged to different branches of, still ortholineage 5). The great wealth of two members of the Zarruq branch also renders them powerful at the village level: together they own a store which means that many villagers are tied to them through debts. Moreover one of them, 19. was in 1968 the chief's assistant and a foreman in the unemployment relief organisation. In Ḥumīri society, wealth in itself commands recognition (Jongmans 1968: 31). Apart from the factor mentioned the members of the Zarruq branch have no particular prestige. Those of the Hamis branch, by contrast, are generally esteemed throughout the village of Sidi Mḥammad.

Finally, with regard to duration of permanent residence in the village of Sidi Mḥammad: the village has for a century and a half formed part of, or at least an extension of, the joint residential space of both the Zarruq and the Hamis branch, but there is a slight difference between the branches in that the Zarruq branch took up residence in the village of Sidi Mḥammad proper about 25 years before the Ḥamis branch.

In summary the genealogical manipulation of informant 25, in terms of which within ortholineage 5 the descendants of Hamis lose their segmentary autonomy vis-à-vis the descendants of Hamis's brother Zarruq, turns out to be associated with a numerical dominance, a political dominance, and difference in duration of permanent local residence; the effect of a factor relative wealth cannot be demonstrated; likewise differences in prestige do not seem to play a role.

text be relied upon.

Informant 25 is a son and grandson of chiefs; brother of the rich and powerful 19. because of his good contacts with the rich and powerful in the research area he is assured of a reasonable income; he does not have much prestige within the village. From his social position it is not surprising that he reinterprets the past in the fashion as discussed here. The extent to which someone's individual perspective on his social environment influences his or her genealogical insights, also becomes clear when we compare 25's views of the relation between Ḥamis and Zarruq with those which Hartong (1968: 62, 69) records for other informants.

Hartong's informants on this point were my numbers 29 and 34. These are aged members of ortholineage 5, both of them descendants of Zarruq. Number 34 is the childless widow of a member of the Ḥamis branch, and since she is still living in her husband's house on the family compound of the Hamis branch, I have counted her in Tables 22 and 23 as a member of the Hamis branch. Number 29 does not have close cognatic or affinal ties with the Hamis branch. Before Hartong both informants accorded the Ḥamis branch not less genealogical independence this is historically correct, but *more*: they presented c₁, historically the BS of b₂, as a brother of b₂ and... of b₁ himself!

If we seek to interpret this genealogical manipulation of the part of 29 and 34 in terms of their own perspective on their social environment, we might have to consider these informants as representatives from the period when in Ḥumīriyya the economic and political contradictions were less acute, when prestige as based on the observance of traditional values was still a central social datum, and when within the village of Sīdī Mḥammad the Zarruq branch (and in general ortholineage 5) was far less dominant than it became in latter days. These aged informants' genealogical statements, even if recorded in 1967, would then relive the value system and social reality of several decades earlier.

But let us not rush such elegant explanations! The matter also has a totally different dimension. Between Hartong's field-work and mine only one year elapsed. We had the same research assistant, Hasnawi bin Ṭahar, who was well versed in genealogical investigations. Both aged informants belonged to my best contacts in Sīdī Mḥammad. Well, in an interview with me number 34 offered the genealogy of the Ḥamis branch exactly as it is, on the basis of other information, most plausible, and as it has been incorporated in my ortholineage reconstruction:

c₁ < b₁ < a₁.

Not a trace of the manipulated genealogy as recorded by Hartong! The allegation (also recorded by Hartong) that Salah bin Ḥamis (c₁) would hail from Hamraya, was totally absent. Example 1, above, contains a similar case: e₂ who in 1967 could exactly trace his descent from the ancestor Mutanni, and in 1968 not any more.

I do not think that the explanation for these discrepancies lies in an inadequate research method on my part or on Hartong's part. The insights we have gained in the course of my argument concerning the functioning of genealogical knowledge in Ḥumīrī society, point in a different direction. Genealogical views have a low consensus, and they are strongly influenced by opportunism. Considerable differences in genealogical views occur not only between informants, but even between the statements from the same informant within a limited time span.

For clarity's sake: I am convinced that in the vast majority of cases (also those included in our examples) the Ḥumīrī informants are of good faiths, and only rarely manipulate their genealogies in a conscious way.

It would mean a gross misunderstanding of the functioning of genealogical knowledge in Ḥumīrīyya, if one were to call these contradictions 'lies' or 'misrepresentations'. Genealogical knowledge in Ḥumīrīyya is primarily a metaphorical formulation, projected into the past and in line with the indigenous societal ideology, for actual social relationships in the present. In essence, it is immaterial to the actors whether their genealogical views are factually correct; what matters is whether they fit in with the current social reality around them: with their pattern of social relationships.

4.4. Construction of a sample of genealogical information

Having thus offered some systematic insights in the genealogical data from north-western Tunisia, I shall now demarcate that part of the data which will be analysed in the light of the two hypotheses of Murphy & Kasdan.

In all I have at my disposal about 200 units of genealogical information, which were collected between the end of March and the end of June, 1968. Each unit comprises the information which was collected with one informant at one occasion. In some interviews more than one unit was collected. In the great majority of cases the informant presented information concerning his or her own kinsman and did personally feature in the genealogy; a minority of the units however consisted of an informant's genealogical statements about people to whom he was not agnatically related.

My genealogical data are too extensive and are not of the right quality to use in their entirety for quantitative testing of the hypothesis concerning genealogical knowledge and manipulation. The proper thing to do might seem to draw an a-select sample from these 200 units, and analyze that sample further. However, this will not do either. The units are too different in size and scope. Some were obtained by means of a long, undisturbed formal interview, in which the informant did his best to present to us as much of his genealogical knowledge as possible. Other units contain just one fragment of genealogical information (*e.g.* with regard to just one marriage), acquired in passing during a meal or at a crowded festival, in a context where the genealogical background was already so well-known to me that pressing for more information would have been unnecessary or ridiculous in the eyes of the informants. The Murphy & Kasdan hypotheses can only be tested on genealogies which have been collected very carefully, and for which there is virtual certainty that the informant was in a position to present as much genealogical information as he was prepared to and capable of. A large portion of the genealogical data is thus ruled out. Finally I was left with sixteen more or less extensive genealogies. The informants were invariably men, older than 35 years of age. Each informant invariably presents information concerning his own agnatic group. The informants lived scattered over villages in two adjacent valleys within the research area. The statements of each of these informants always overlapped wholly or partly with other units in my genealogical data set. For all (ortho-)lineages of which these informants present the genealogies, I have extensive and reasonably reliable

reconstructions of the actual genealogical history. Each such reconstruction is different from the statements of the sixteen informants in my sample, and is based on several (sometimes scores) of units of (contradictory) genealogical information in addition to the informant's.

These sixteen genealogies will together form the sample of genealogical information, on which I shall test whether the two hypotheses of Murphy & Kasdan are applicable to genealogies from north-western Tunisia.

4.5. Inventory of the genealogical information in the sample; assessment of genealogical depth

4.5.1. Aggregation of the genealogical information as a stepping-stone towards testing the Murphy & Kasdan hypothesis

Below we shall consider the sample as an aggregate on which we can measure various relevant variables, without going into the detail of the specific statements of the sixteen individual informants. For this purpose I must assume that the sixteen informants do not greatly differ from each other with regard to the extent of their genealogical knowledge and their proneness to genealogical manipulation. Their similarity in age, gender and place of residence renders such an assumption plausible. The conclusions which we shall reach with regard to the sample, will apply to some sort of 'average genealogical informant from north-western Tunisia'.

In the analysis of the sample genealogies I have ignored those persons listed who died young and/or who remained unmarried throughout their lives. This is justified in view of the fact that Murphy and Kasdan's approach revolves around the analysis of marriage systems. In the research area, unmarried adults who never in their lives contracted a marriage, have been as rare in the past as they are today. Therefore their omission does not greatly affect the sample. It is obvious that persons who died young may be overlooked by genealogical informants, who may not even know their names. And since children do not form a relevant category for the present analysis, it is better to ignore them.

In the analysis we only look at the generation of Ego (the informant) and above. Generations below Ego are ignored. Only in the case of a few informants there exist adult kinsmen in the generations below them. Moreover the generations below Ego are not relevant for the Murphy & Kasdan hypotheses.

In the genealogical information of the sample we must always distinguish between historical persons and mythical ancestors. For the problem of the merging of patri-line and matri-lines, as postulated by Murphy & Kasdan, is not relevant at clan level, *i.e.* with regard to mythical ancestors. Clans have a strong spatial anchorage. Among the actors there is only a limited consensus as to which agnatic groupings belong to which clan. It is relatively rare that agnatic groupings affiliate at the lineage level, in other words that historical persons who in reality are not agnates, are presented in a genealogy as agnates by means of direct F/S chains involving not mythical ancestors but historical persons; our sample of sixteen genealogies contains only two cases of this happening. Affiliation at the clan level, however, is a frequent phenomenon: it

means that the various agnatic groupings as distinguished by a particular informant are all attached to one mythical ancestor, in such a way that the informant need not go into the question of the precise genealogical chains between the apical historical ancestors of these groupings, and between them and the mythical ancestor; for the connexion between the highest historical person and the mythical ancestor spans an unspecified number of generations. Often actors are able to trace agnatic kinship chains of the type FFBSD. But mythical ancestors are never included in such chains (as the highest vertical connexion): once ascended, in the genealogy, to the highest mythical ancestor, one never descends again to enumerate (as historical ancestors) other 'sons' of the mythical ancestors. It is only in the rare case of affiliation at the lineage level that an informant can enumerate the (per definition) fictive genealogical chains which, in the higher generation, turn the pseudo-agnates into agnates. Since for the actors an indefinite time span separates these historical persons from mythical ancestors, the problem of the merging of patri-line and matri-line is not relevant at the level of mythical ancestors. For in terms of the universal Arabian ideology all Arabs (via Ibrahim), and even all mankind (via Adam) are agnates, so that in the last analysis the merging of patri-line and matri-line would be as inevitable as socially irrelevant.

Souyris-Rolland (1949) offers information on presumed agnatic relationships between mythical ancestors in and around my research area. He does not disclose the sources of his data, and his sociological insight in the data is decidedly limited. My own informants often managed to place two or three mythical ancestors in some agnatic relationship, but invariably failed to include, in such an attempt, most of the locally acknowledged mythical ancestors. Whereas Souyris-Rolland goes as far as to present some sort of 'national genealogy' encompassing the whole of Ḥumīriyya, I have not been able to discover anything remotely similar. Our analysis is not greatly affected by this state of affairs. For at any rate, the mythical agnatic kinship via Ibrahim and Adam constituted an undeniable fact also in the eyes of my informants.

Implicitly therefore the Murphy & Kasdan approach is exclusively directed at non-mythical genealogical knowledge, *i.e.* at what informants postulate to be agnatic relations at the lineage level. Separating historical persons and mythical ancestors is therefore not only justified but even necessary.

Among the sixteen informants, three did not state any mythical ancestors. Among them was my best informant, my research assistant. The distinction between mythical ancestor and historical person is implicitly made by the actors, but both types are indicated by the same local term 'djadd', which comprises all lineal ancestors both in the patri-line and in the matri-line, to reckon from the parents of Ego's F and M onwards. The genealogies narrow rapidly, so that in most cases all what is left after a few generations is a series of lineal ancestors (*i.e.* historical persons and/or mythical ancestors), without collateral ancestors. In general the top of the genealogy is formed by one or more mythical ancestors. In a few cases however the series comprises one or two mythical ancestors, presented as descendants of persons who are historical persons only in this sense that the latter are not included in the small set of locally recognized mythical ancestors and clan founders.

In such cases the mythical ancestor is borrowed from a clan which was already established locally at the time when the affiliating lineage segment in question arrived; one takes over the mythical ancestor but maintains, above him, one or more of one

original ancestors (historical persons) who are traditionally reckoned to be members of the own lineage, even if one does not know anything about them except their names. This pattern occurs with two informants only.

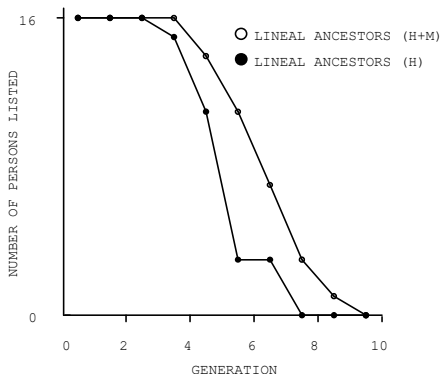
Inspection of the sixteen genealogies in the sample yields, for the various categories of ancestors and women, the information as contained in Table 7.

generation	0	1	2	3	4	5	6	7	8	9	dia-gram
a. number of lineal ancestors (historical only) (listed only)	16	16	16	15	11	3	3	0	0	0	1
b. = a/16	1.00	1.00	1.00	0.94	0.69	0.19	0.19	0.00	0.00	0.00	2
c. number of lineal ancestors (historical and mythical)(listed only)	16	16	16	16	14	11	7	3	1	0	1
d. = c/16	1.00	1.00	1.00	1.00	0.88	0.69	0.44	0.19	0.06	0.00	2
e. number of collateral ancestors (historical only) (listed only) #	60	75	28	2	0	0	0	0	0	0	3
f. number of in- marrying women	59	80	21	0	0	0	0	0	0	0	3
g. number of in- marrying women (historical only) (listed+non-listed) #	118	3
h. number of out- marrying women	59	47	2	2	0	0	0	0	0	0	3
i. number of out- marrying women (historical only) (listed+non-listed) #	90	3

= only available for generation 0

Table 4.6. Summary of aggregate information contained in the sample genealogies

The data are plotted in the following Figures 4.7-8- and 9.



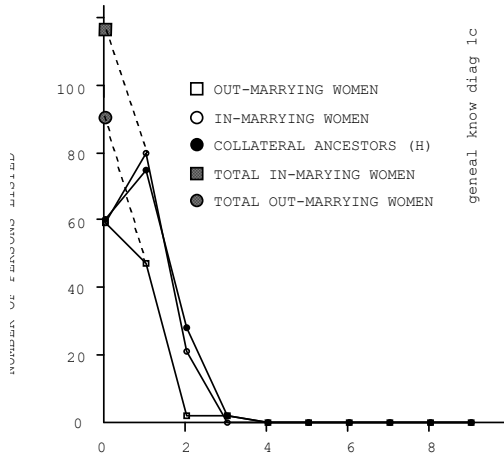
(H = historical persons; H+M = historical persons + mythical ancestors)

Fig. 4.10 Number of listed lineal ancestors per generation in the sample genealogies



H = historical persons; H+M = historical persons + mythical ancestors

Fig. 4.11. Number of listed lineal ancestors per generation in the sample genealogies as a ratio of the maximum number (= 16) of lineal ancestors



(for both in-marrying and out-marrying women, the total number of listed and non-listed are indicated for generation 0)

Fig. 4.12. Listed collateral ancestors (historical persons only), in-marrying women and out-marrying women per generation in the sample genealogies

Whereas Figs. 4.7 and 4.9 are simple transformations of each other and therefore have the same shape, both are very different from Fig. 4.9. In Fig. 3 we deal with real numbers of remembered historical persons; these numbers are related to the size (= 16) of our sample, and in principle have no upper limit as long as we increase the sample. In Figs. 1 and 2 there is, however, such an upper limit: an informant can never have more than 1 lineal ancestor per generation, and hence 16 informants can never have more than 16 such ancestors per generation. This fundamentally different data structure explains the lack of similarity between the curves of 1 and 2 on the one hand,

3 on the other. Yet it will be clear that the downward slope of both curves (1, 2 and 3) in general reflects the decreasing retention with time – they can be considered as special cases of Ebbinghaus’s famous *forgetting curve* (e.g. Krech & Crutchfield 1962: 4).

Inspection of the three curves in Fig. 3 shows the very close similarity between the distribution of collateral ancestors and in-marrying women over the generations. In fact, statistical analysis reveals that the differences between both curves can be attributed to chance.³² On the other hand, it is striking that the scores for out-marrying women per generation are systematically lower than those for in-marrying women; statistical testing reveals that this effect cannot be attributed to chance.³³

These results are squarely opposed to the theory of Murphy and Kasdan, who postulate the suppression of in-marrying women and the stressing of out-marrying women. Moreover, the parallelism between the data for collateral ancestors and in-marrying women suggest that what is at stake is a phenomenon in the collective management of historical knowledge which cannot be solely attributed to such social-structural ‘needs’ as Murphy and Kasdan advance as background for such genealogical manipulation of women as they suppose to take place.

This already points to the line of argument that will be developed further on:

- The retention of knowledge about past members of descent groups is in the first place governed by general psychological mechanisms of human memory;
- In *Ḥumīriyya*, these mechanisms are influenced by the extent to which the remembered persons, as adults living in the past, because of their interaction with other past members of the local group, were in a position to leave adequate traces in the collective memory of the local group. Out-married women, half of which left the local group after adolescence, were obviously at a disadvantage to leave such traces as compared to in-marrying women who spend most of their adult life as members of the localising (but, admittedly, never completely localised) kin group, and as collateral ancestors for which the same would hold true.

So far the interpretation has tacitly assumed that there has been no difference between the generations in terms of average number of siblings of lineal ancestors. This assumption may be somewhat questionable in the light of the fact that the most recent generations (ethnographic present is 1968) have seen explosive population growth. On the other hand it is unlikely that four or more generations above the present informants the lineal ancestors of those informants had no siblings or cousins whatsoever (as the available genealogical data suggest). The curve *ga* for collateral ancestors shows a rapid decline in the generations 1 to 4. This decline must be attributed partly to the factor population growth: it is sure that in the higher

³² Likelihood ratio test (see footnote below) performed on COLHI versus IN for generations 0 through 3, $\chi^2 = 3.87$, $df = 3$, $p = .28$, not significant.

³³ Likelihood ratio test performed on IN versus OUT for generations 0 through 3, $\chi^2 = 20.43$, $df = 3$, $p = .0001$, significant. Testing of COLHI against OUT would of course yield a similar result (cf. previous footnote): $\chi^2 = 22.31$, $df = 3$, $p = .0001$.

generations there were relatively fewer adult, married collateral ancestors. But the decline must also be partially attributed to loss of information. For even though it is certain that an unknown positive number of brothers did occur in the fourth and higher generations, in these generations the number of collateral ancestors equals zero. The precise course of the curve of information loss cannot be ascertained without precise data on population growth. Yet the conclusion may be justified that, at least from the third generation above Ego, the loss of information with regard to lineal ancestors is less than that with regard to their brothers. A direct explanation for this lies in the fact that the series of lineal ancestors is systematically taught to a younger generation, while no systematic transfer of knowledge exist with regard to collateral ancestors: these the latter-day actors only happen to know from accidental anecdotes.

Meanwhile we are witnessing the virtual collapse of the careful *Ḥumīrī* construction of agnatic ideology and the segmentary lineage. For, if with regard to the higher generations the lineal ancestors are know but their siblings and cousins not or hardly, then it is practically impossible to represent, in the higher generations, the opposition and integration of kin groups (agnatic segments) with the aid of a dendrogram-shaped genealogy. In other words, then a segmentary structure on the basis of unilineal descent is unthinkable.

As I have already indicated above, readers with some grounding in the psychology of learning will realise the close analogy between the curves discussed here (in which the knowledge about historical persons is offset against the period of time which has passed since these people were alive) and the experimental curves of learning and forgetting such as have been established since Ebbinghaus' pioneering work. The acquisition, retention and loss of genealogical knowledge is subject to the same principles which also apply to other forms of knowledge. In a practically illiterate society, this knowledge is not the fixed property of a social group, established once for all, but it lies stored in the individual consciousness of the members of that society. This makes it possible that that knowledge is non-consensual, subject to opportunist manipulation, that in its transfer to other individuals some parts of that knowledge (for instance, those relating to lineal ancestors) is privileged over other parts (*e.g.* those relating to the collateral ancestors), and that that knowledge in general declines with time. It is important to make a profound study of the social structural aspects of genealogical knowledge, but in addition we must continue to realise that that knowledge is also, and perhaps primarily, subject to individual-psychological laws.

Such genealogical knowledge, which can be explained psychologically, is useless from a social-structural point of view. What is the use of retaining knowledge concerning ancestors who did not live in the informant's present village or territory and for that reason reference to such ancestors cannot substantiate present-day claims of legitimate local residence but may only jeopardise such claims.

The curves relating to women (both in-married and out-married) show approximately the same tendency as those for male (collateral? lineal?) ancestors. Here we can distinguish between:

- such genealogical knowledge as has been acquired by accident, and as relates to collateral ancestors, and to women,
- and such genealogical knowledge as has been acquired formally, and as relates

to lineal ancestors.

In other words, Therefore, women are not forgotten because they are less important than men or because of some sinister ulterior purpose of the social structure which (by whatever mysterious means) manages to invade and control the mechanisms of the individual actor's minds; they are forgotten in the same way as one forgets collateral ancestors. The difference with lineal ancestors rests on the existence of formal training with regard to genealogical knowledge concerning the latter.

Historical persons only feature up to the sixth generation above Ego, inclusive; the genealogy already tapers to one chain of lineal ancestors at the third generation above Ego. Mythical ancestors appear between the third and the eighth generation position above Ego; because the link between the highest historical person, and mythical ancestor, in the informant's mind pans an indefinite number of generations, we are not allowed to equal the generation position in this sense with a genuine generation. No genealogy in the sample contains vertical chains longer than 8 names of historical persons plus mythical ancestors

Collateral ancestors are obviously only known in Ego's generation, in the first and second generation above Ego, and scarcely in the third generation above Ego. This is fully in line with the pattern which Murphy & Kasdan describe for the Tuareg of the Sahara. Also in ʔumīriyya we find the 'shallow genealogies' which might have the function, as postulated by Murphy & Kasdan, of obscuring the merging of the matri-line and the patri-line from the actors' conscious perception. For if collateral ancestors are suppressed from the genealogy it is impossible to end up with merging lines of descent.

The same applies to mythical ancestors. They, to, appear in the genealogies virtually exclusively as lineal ancestors. Only a few informants mention, in their genealogies, siblings of mythical ancestors. This is not to day that the notion of fraternal relations between mythical ancestors is altogether absent in ʔumīriyya; it probably means, however, that most informant realise that the relationships involving mythical ancestors are in fact only allegorical, and do not properly belong in a summing-up of series of agnates and their spouses which are supposed to be historically correct. To state that two mythical ancestors were brother, also from the actors' point of view, means little else than that the various clans with which their name is associated, had good relationships in the past. Admittedly, sometimes an idiom of fraternal relationships between mythical ancestors may approach the suggestion of real agnatic kinship. Informants may resent such a suggestion in the allegory to such an extent that they explicitly deny any claims of fraternal relationships between mythical ancestors, stressing that nobody today knows these things anymore.

4.5.2. Occamist genealogies?

Another aspect has been brought out by Gellner (1969; *cf.* van Binsbergen 1971b): the principle of the 'Occamist genealogies'; by analogy with the philosophy of William of Occam,³⁴ North Africans (at any rate, Moroccans among whom Gellner did research,

³⁴ William of Occam (1287–1347) was a prominent Christian philosopher and theologian, still known for

with the exception of the kin groups of religio-political specialists) would retain just so many ancestors in their genealogies as would be necessary for the delineation of their own kin grouping against other such groupings at the same segmentary level:

‘the individual will have or know only those ancestors who perform the useful task of defining an effective social group’ (Gellner 1969).³⁵

In my research area, in view of the pattern of continuous migration this Occamist genealogical span would have to be a function of the average period a kin grouping would tend to stay within one territory (a part of a valley or of adjacent valleys); kin groupings have never stayed in their present territory for longer than 200 years, and their local time span would be between two and six generations. In order to assess whether Gellner’s ideas are also applicable to ʿUmīrī society I shall now analyse the relation between the residential history on the one hand, and on the other the listing of historical persons, and of mythical ancestors, in specific generations of the genealogy. Having at my disposal reconstructions of the genealogies and of the residential histories of the ortho-lineages, I am in a position to assess the relation between the mention of ancestor’s names in a specific generation, and the residential history.

Historical persons. With regard to historical persons our findings are as follows. We concentrate on the highest historical person before the lowest mythical ancestor.

Moreover, for the residential history we can distinguish between:

- a grouping’s arrival in the village where the informant himself lives today (and then count the number of generations that his direct ancestors have lived there uninterrupted);³⁶ and
- a grouping’s arrival in the present territory in the wider sense, which may comprise a number of villages within the same valley or spread over adjacent part of several valleys grouped around the same mountain range.

Thus we find:

- The number of direct historical ancestors that the informants of the sample could list before the generation of arrival in the present village, ranged from 0 to +5, with median at +2.
- The number of direct historical ancestors which the informants in the sample could list before the generation of arrival in the present territory ranged from -5 to +3; this is to say, the highest historical ancestor in the sample appears in a

his adage to the effect that ‘*entia non sumt multiplicanda since necessitate*’, in other words, that we should always try to work with as few items / elements / factors/ variables / assumptions as possible. I used the concept in a 1977 article naming Occam in the title, but on Occam himself and his thought I have had little to share. In general, it is my impression that the parsimonious use on which he insists, is a violence of human natural habits of thought and speech.

³⁵ One obvious implication of the Occamist variety of genealogies is that such genealogical knowledge is not knowledge (in the usual common-sense meaning of the word) at all, but ideology: a statement meant to underpin a socially defined and recognised claim.

³⁶Or at least from one generation to the next generation, ignoring a few years of temporary absence of the grouping from the local community.

generation which lies between five generation after immigration into the present territory, to three generation before such immigration. The median is at 0, *i.e.* in the generation of immigration.

In this connexion it is important to note that the number of generations between arrival in the present village and arrival in the present territory ranger from 0 to 6, with the median at two. In other words, immigrant kin groupings tend to have a median of two generations in the local territory before actually settling in the village where their present-day member became my informant.

Mythical ancestors. With regard to mythical ancestors the following we have the following outcome. We concentrate on the lowest mythical ancestor in each genealogy. Mythical ancestors appear from 1 to 6 generations before a grouping's arrival in the present village; the median is at 3. Mythical ancestors appear from -4 to +4 generations before arrival in the present territory; this means that the lowest mythical ancestor appears from the fourth generation after arrival in the present territory, to the fourth generation before arrival there. The median lies at +1. Three genealogies did not contain any mythical ancestors.

Of course the data on mythical persons and on mythical ancestors are complementary in that the highest historical ancestor follows, per definition, after the lowest mythical ancestor.³⁷

The number of generations between Ego and the generation of arrival in the present village ranges from 0 to 3 (when it is 0 this means that Ego himself is an immigrant in his present village), the median is at 1 to 2. These figure in themselves already betray the great spatial mobility in this area.

50% of the informants, therefore, lives in a different village than their FF. Although the sample has not been drawn at random from all heads of household in the research area, this result tallies well with the general pattern.

The number of generations between Ego an arrival in the present territory ranges from 2 to 6; the median is at 4 to 5. 50% of the informants, therefore, lives in a different territory from his FFFFF. In these data there is no significant relation between residential history and the generation depth of genealogies, if by generation depth we mean the number of historical persons listed.³⁸

According to these data it is not so that people whose kin grouping has a long and uninterrupted local history, produce genealogies with a longer generational depth than people whose kin grouping has immigrated more recently. This is a strong

³⁷ We have chosento ignore such historical persons as would be mentioned above mythical ancestors.

³⁸ I assessed this with Spearman's rank correlation test (using the statistic r_s), corrected for ties; cf. Siegel n.d. The highest generation in which a historical person was listed (ignoring loose historical persons separated from the other historical persons in the genealogy by one or more mythical ancestors) yielded a rank number for each informant. The generation in which the informant's kin grouping arrived in his present village yielded another rank number. $r_s = +.13$ for the relation between generational depth (historical persons only) and arrival in the present village, and $r_s = .26$ for the relation between generational depth and arrival in the present territory; with $N = 16$, these values of r_s are not significant at the 5% level.

argument for my contention that, in individual's genealogies, residential history is not related to generation depth.

Although the various informants, as we have seen, differ widely with regard to the generation of arrival in the present territory, they resemble each other in so far as historical depth (of historical persons) is concerned. This is clear from the following Table 4.8.

historical persons are mentioned in, at the highest, the a-th generation above Ego	this value of a is found among the following number N of informants:
a =	N =
2	1
3	4
4	9
5	1
6	1
total	16

m = 3.8; s = .8; median = 0

Table 4.7. Generational depth with regard to historical persons in the sample.

The distribution of Table 8 shows relatively little spread. How can we explain this? Not on the basis of the informants' age and their personal residential history? For in the data there is neither a relation between the informant's age and genealogical depth (historical persons only, but not 'detached' historical persons). (Spearman's rank correlation corrected for ties, $r_s = .35$, $N = 16$, not significant at the 5% level). Why then not adopt a simple explanatory principle, to the effect that people from higher generations are forgotten not for any structural ulterior motive but because remembering their names is cumbersome and meaningless? That is, an explanation in terms of the psychology of learning.

Such remembrance is meaningless, because (as I shall argue throughout the present Volume I) *Humiri* society is integrated not in terms of kinship but of spatiality in other words territoriality or distance; and given the great spatial mobility it is only asking for trouble if one knows too well who were (or were not) one's local ancestors. If one interacts at all with distant agnates (the very people one might identify on the basis of extensive genealogical knowledge), such interaction does not primarily derive from the awareness of a common kinship but from such other principles as spatiality, economic and political interests, and the dynamics of honour and shame.

Does this mean that the names of historical persons who did not yet live in the informant's present village or territory, are replaced by mythical ancestors, or are

being forgotten? This again is not the case, as the following two Tables will demonstrate:

number of generations	number of informants claiming that number of generations in their genealogy
0	0
1	0
2	1
3	4
4	9
5	1
6	1
total	16

$m = 2.2; s = 1.8; \text{median} = 4$

(e.g. if $a = 1$, this means that informant's F is the highest historical person in the genealogy to live in informant's present village)

Table 4.8. The number of generations between arrival in the present village and the highest (not-detached) historical person listed as lineal ancestor in the genealogy

It turns out that lineal ancestors (historical persons) are remembered even if they did not live in the same village as the informant.

number of generations	number of informants claiming that number of generations in their genealogy
-4	1
-3	1
-2	3
-1	2
0	4
1	3
2	1
3	1
total	16

$m = -.4; s = 3.5; \text{median} = 0$

e.g. if $a = -2$, this means that the highest historical person in the genealogy to live in informant's present territory was a SS of the ancestor who first immigrated there; obviously the negative scores are based on additional information not found in the genealogy as processed

Table 4.9. The number of generations between arrival in the present territory and the highest (not-detached) historical person listed as lineal ancestor in the genealogy

Obviously living in a different territory is in itself no reason to forget a certain ancestor. All depends on the number of generations that has passed since. Note that

this is also brought out by the fact that the spread in the preceding Tables is larger than that in the Table of generational depth reckoned from Ego.

That yet the immediately preceding Table has its median at 0 and is rather symmetrical around 0, might yet suggest some relation with the residential history, but considering the large spread this relation cannot be very close.

The explanation for this is that for the actors there is always the opportunity to manipulate the data: one can always claim perennial local residence for the ancestors who in fact lived elsewhere. Knowledge about ancestors' places of residence is even more easily manipulated as knowledge about descent. The claim of 'perennial local residence' can be easily expressed in terms of a kin relationship between the highest historical person in a genealogy, on the one hand, and the mythical ancestor of the locally dominant clan, on the other.

4.5.3. Ancestors as inwardly-gazing, rather than outwardly-contrasting, labels of group identity

Ẓumīrī genealogies are not 'Occamist'. This points in the direction, already indicated, that the vicissitudes of genealogical knowledge cannot be fully understood on the basis of social-structural variables alone. Since the dominant ideology presupposes some general agnatic kinship between all inhabitants of a particular spatial segment, it is for a strict social-structural point of view meaningless to remember the names of ancestors who imply agnatic or lineage heterogeneity. Yet we find such ancestors abundantly in the genealogies of the sample, even in the extreme form of detached ancestors in higher genealogical positions than the mythical ancestors one has borrowed from the dominant local groupings. By the same token it is meaningless, from a social-structural point of view, to exclusively remember lineal ancestors, without their siblings and cousins: the collateral ancestors. Genealogical knowledge which does not tally with the dominant ideology and which is yet perpetuated, from a sociological point of view constitutes a peripheral form of cognitive production for which psychology rather than sociology appears to offer the proper interpretative perspective.

But perhaps the learning and proudly listing of series of lineal ancestors can be shown to be sociologically relevant from a totally different point of view. As in any society, in Ẓumīrīyya we see tendencies towards social integration negotiate with tendencies towards social dissociation. Integration is mainly achieved within the context of spatial segmentation, resulting in a tendency for day-to-day interaction to primarily involve people who live very closely together. The structure of interaction binds the members of one spatial segment. Besides, spatial integration is enhanced by marital ties and by religion – particularly the veneration of local shrines which are distributed – as characteristic attributes – over the spatial segments. Moreover, spatial integration is expressed in terms of ancestors; ancestors, too, are the attributes of spatial segments, and that is why actors' views of who are their ancestors and how are these ancestors related to one another have to be reviewed continually, at the pace of the changes in the spatial structure and in the relationships between the members of the spatial segments. Of old, such integration is relatively weak. Every spatial segment is constantly confronted with newcomers. Alternatively there are always people who

migrate away: out-migration has been and remains a major way of settling conflicts. Moreover, out-migration is a strategy to escape from the lack of resources (land, springs, a salaried job). Even among those who remain behind the pattern of existing relationships changes rapidly. Integration is always precarious. Individual independence is a central value in this society. One subscribes to the ideology of 'we are all one and the same family'; 'we have all one and the same ancestor', for as long as there are no major conflicts, but when these break out one contradicts the ideology by reinterpreting the existing genealogical knowledge. Considerable emotions are invested in one's own lineal ancestors, who made clearings and threshing-floors which can still be identified, who are known to have frequented certain springs and pastures, whose battles are remembered and who have been buried at an identified place. There are clear limits to the extent to which historical knowledge concerning F and FF can be freely manipulated. Living at the same spot as one's father and grandfather, tilling the land which they have marked by planting trees, constitutes an anchorage of consciousness and identity that touches people rather more profoundly than the higher-level ancestors, be they historical persons or mythical ancestors, who are hardly associated any more with identifiable places in the landscape, and which can be manipulated and redistributed as the need arises. From one point of view one drains the available genealogical knowledge and uses the systematised result in order to express and achieve spatial integration – but from another knowledge about the series lineal ancestors is a source of pride of a distinct kin grouping striving for its separate identity. This is why the latter type of genealogical knowledge is cherished, and is formally transferred. *This knowledge about lineal ancestors does not serve to structure the interaction with other similar kin groupings but it is like a flag, a label which mainly supports the group's sense of dignity.* It is not intended to neatly distinguish, as in a segmentary dendrogram, one's own group from others, but merely serves to say: 'this is me', regardless of whether others share or do not share these ancestors. This knowledge therefore is something that is only cherished within one's own narrower kin grouping, and which outside that group is hardly communicated: for where groups interact, one does not need historical knowledge but integrative, systematized formulae, spatial segmentation reformulated in an agnatic idiom. Of course concessions are inevitable. For instance, the ideal way to acquire land somewhere is by patrilineal descent from the major local ancestor; therefore, if one has acquired land in some other way than patrilineal inheritance (purchase, matrilineal inheritance, donation, invasion) one does two things at the same time: for integration's sake one take over the dominant local ancestor in one's genealogy, and for the sake of family identity one maintains, even above that adopted local ancestor, the names of such true ancestors as one has learned from one's father and grandfather. Later generations are no longer aware of this manipulation and have come to consider the adopted ancestor as a true one. The names of higher-generation ancestors are rarely if at all discussed outside family circles, which means that there is no consensus-promoting social control upon these genealogical series.

That integration in a kinship idiom (identification on the basis of common descent) is absent when there is no spatial integration (in the way of dwelling in each other's proximity, *i.e.* the same or adjacent valleys, and especially the same village) is clear

from a number of cases.³⁹ Therefore, if we do not want to give up altogether a sociological approach to the non-Occamist ʕumīrī genealogies, we should look for one not at the integration end of the social dynamics, but at the individualizing end: the symbolic underpinning of one's own identity which does not necessarily require the juxtaposition (in terms of genealogical position of apical ancestors) vis-à-vis other rival kin groupings.

In line with Murphy & Kasdan's first hypotheses (concerning 'shallow genealogies'), in ʕumīrīyya genealogies are so shallow that they *might* play the role, as postulated by Murphy & Kasdan, of eclipsing, from the actors' consciousness, the merging of the patri-line and the matri-line. This does not mean however that the actual functioning of ʕumīrī genealogies can really be understood in terms of Murphy & Kasdan's theory.

- Generational depth did not correlate significantly with the informants' age; however, all genealogical informants were older than 35 years of age.
- Generational depth did not correlate with the length of continued local residence of the informant's agnatic group. Lineal ancestors are not forgotten simply because they happened to live outside their contemporary descendant's village or territory at large.
- In higher generations, collateral ancestors are forgotten far more readily than lineal ancestors. Therefore it is not possible in ʕumīrīyya to base a consistent segmentary structure on kinship: for, in such a structure, sibling relations between ancestors would provide the necessary links between opposing segments at the same segmentary level.
- ʕumīrī genealogies span a maximum of eight generation above Ego, including mythical ancestors if any.

The discussion so far leads us to the following typology with regard to ancestors and genealogical knowledge in ʕumīrīyya. Genealogical knowledge with regard to certain categories of (fictive) kinsmen such as feature in the genealogies can be distinguished in terms of

- the way in which that knowledge has been acquired (accidentally or through formal training)
- the degree of historical factuality which actors themselves attribute to this knowledge (the distinction between historical persons and mythical ancestors)
- the function which that knowledge has for the social orientation of individuals and kin groupings: it may be integrative – at the lineage level (ancestors as historical persons) as well as at the clan level (mythical ancestors) – or it may be individualizing: the series of lineal ancestors whose siblings and cousins are no longer known
- the extent to which that knowledge can be manipulated: at the lineage level this extent is the greater the less close the relationship is, while at the clan level manipulability is virtually unlimited.

³⁹E.g. van Binsbergen 1970: 146-149.

We shall now turn to another aspect of Murphy & Kasdan's theory about the persistence of the agnatic ideology: the genealogical manipulation of women. Our discussion of this specific topic will go through the following steps:

- further elaboration of Murphy & Kasdan's hypotheses
- quantitative analysis by reference to the Tunisian data
- formulation of an alternative hypothesis
- testing the alternative hypothesis by reference to the Tunisian data
- conclusion.

4.6. Operationalization of Murphy & Kasdan's hypothesis with regard to the genealogical manipulation of women

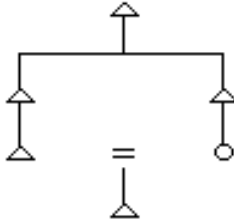
The hypothesis concerning the genealogical manipulation of women receives only a cursory treatment from Murphy & Kasdan (1967: 10). Perhaps Randolph's unpublished dissertation contains a further elaboration, but I have not been able to consult this work. Of course it is important to know on the basis of what kind of ethnographic data Randolph formulated his hypothesis, and how he sought to test it.

Randolph's genealogical data were later used for a numerical analysis of the Bedouin marriage system (Randolph & Coult 1968). But that publication, again, does not contain a further elaboration and testing of the present hypothesis. The authors merely state that informants do not remember the wives of the ancestors, and for that reason the investigators cannot consider potential ambilineal and matrilineal implications in their analysis (Randolph & Coult 1968: 85).

The hypothesis has been formulated in terms of descent groupings. It makes pronouncements with regard to out-marrying and in-marrying women. The spatial factor is ignored. According to Murphy and Kasdan (1967: 10) the hypothesis consists of the following points:

- (a) Women who marry within their own descent grouping are consciously or unconsciously suppressed in the genealogy of that grouping.
- (b) Women who marry into a descent grouping from another descent grouping are emphasized in the former's genealogy.

Mechanism (a) serves directly to eclipse from the actors' consciousness such merging of patri-line and matri-line as, under conditions of kin endogamy, would otherwise be unmistakable already a few generations above Ego. Let us take a closer look at this mechanism. The structural 'need' for this mechanism is the greater, the closer the agnatic relationship between Ego's parents is. It is greatest when Ego's M is his F's FBD: in that case matri-line and patri-line already merge in the person of Ego's FFF. (Fig. 9)



triangle = male person; circle = female person; vertical line = filiation, descent; horizontal line = sibling, = means marriage

Fig. 4.13. Merging of patri-line and matri-line in the case of FBD marriage.

If Ego's M and F are less close agnates, then matri-line and patri-line only merge in higher generations; for instance, if M is F's FFFFBSSSSD, then the lines only merge in Ego's FFFFF. The problem which mechanism (a) is supposed to solve, is then less acute. This leads us to formulate a third point:

- (c) we may expect that mechanism (a) will not or hardly occur in the case of lineage endogamy between distant agnates.

Let us consider mechanism (b). Its function is to reinforce the ideology of the lineage as a distinct, corporative unit, whose demarcation vis-à-vis other such units is manifested particularly through the marital relationships it contracts with the latter. The relevance of exogamy for the demarcation of one's own social grouping is a well-known principle and it is also mentioned by Murphy & Kasdan in this connexion (1967: 13). But this mechanism is not only at work with regard to in-marrying women as stipulated by Murphy & Kasdan (mechanism b). For from the point of view of group demarcation, women who are marrying out of their own descent group into another fulfill structurally the same role as in-marrying women. This allows us to add a fourth point to the hypothesis, complementary to mechanism (b):

- (d) Women who, from their own descent group, marry into another descent group, will be stressed in the genealogy of their own descent group.

This elaboration of the Murphy & Kasdan hypothesis concerning the genealogical manipulation of women now enables us to summarize the contents of the hypothesis in two convenient schemes. This is done in Tables 11 and 12. The arithmetical signs in the Tables have the following meaning:

- = are suppressed in the genealogy
- o = are neither suppressed nor emphasized in the genealogy
- + = are emphasized in the genealogy

The letters in the Tables 11 and 12 refer to the four points in the Murphy & Kasdan hypothesis as formulated above. Because in the case of lineage endogamy the in-marrying women are identical with the out-marrying women, the mechanisms (a) and (c) have been listed in both Tables 1a and 1b. In this form the Murphy & Kasdan hypothesis is amenable to quantitative testing.

origin of in-marrying women	hypothesized effect	mechanism
from informant's lineage (close agnate)	-	(a)
from informant's lineage (distant agnate)	o	(c)
from other lineage than informant's	+	(b)

Table 4.11. Genealogical manipulation of in-marrying women, according to the Murphy & Kasdan hypothesis

destination of out-marrying women	hypothesized effect	mechanism
to informant's lineage (close agnate)	-	(a)
to informant's lineage (distant agnate)	o	(c)
to other lineage than informant's	+	(d)

Table 4.10. Genealogical manipulation of out-marrying women, according to the Murphy & Kasdan hypothesis

4.7. Testing of the Murphy & Kasdan hypotheses concerning genealogical manipulation of women in the Tunisian data

In order to test these hypotheses of Murphy & Kasdan with the use of our sample of 16 genealogies, I counted for each generation the total number of women listed by the informants. These women were divided into the following categories:

- in-marrying, *i.e.* listed as spouse of a male member of the lineage which was depicted in the genealogy;
- out-marrying, *i.e.* listed as the married daughter of a male member of the lineage which was depicted in the genealogy.

This is a matter of the analytical point of view. A women who contracts a lineage-endogamous marriage is out-marrying and in-marrying at the same time, and if she and her husband are both listed in the genealogy, she will be counted twice in the analysis; the two categories overlap. If a woman is only listed as out-marrying (without specific additional information as to her husband being a member of the same lineage) or only as in-marrying (without being identified as a lineage member), then she will be counted only once.

For the in-married women as listed in the genealogies their kin origin was traced; here the relevant distinctions are lineage exogamy versus lineage endogamy; and degrees of agnatic kinship (notably: 'close', 'distant' and 'none').

I shall speak of close agnatic kinship, if between the spouses before marriage an agnatic genealogical chain could be traced of less than 5 elements. For longer chains, in so far as these can still be traced within my reconstructed ortho-lineages, I speak of distant agnatic kinship. Since my reconstructions often trace genealogical connexions which the actors themselves no longer perceive, in a number of cases I shall have to

classify a kin relationship as distant agnatic kinship whereas the actors themselves would only perceive the absence of agnatic ties in those cases. I speak of 'no agnatic kin relationship' if, according to my reconstructions, the spouses truly belong to different ortho-lineages.

In this way the data could be processed with regard to the women in the informant's generation and in the third and second generation above. Only one genealogy out of our sixteen still listed two women for the third generation above the informant's – but in that case only their names were listed, and the informant could not tell us anything about the marriages of these two women. Of course it is absolutely certain that the lineages of these sixteen informants in the higher generations both produced women and received women as in-marrying wives; but of these women contemporary informants have no recollection.

In the processing of these data the informants' own generations posed again a problem: because of the above-mentioned defects of individual genealogical statements, the number of listed women here was lower than in my reconstructions. Because the women concerned are either the informant's Z, FBD, BW or FBSW, it is absolutely sure that the informant does know their names, their kin origin, and the kin origin of their husbands. Therefore we must accept that for the o -th generation the data concerning listed women are mutilated and cannot be used as point of departure for the analysis of genealogical manipulation in the higher generations. Because also the non-listed women in the o -th generation are known to the informants, I have assessed – through a comparison between the informants' genealogies and my own reconstructions – which women in the informant's own generation have not been listed. Here I have limited myself to those women who belong to either (a) the informant's own sibling group, or (b) to those (agnatically rather closely related) sibling groups whose members or whose parents have in fact been listed by the informant. This led me to identify a set of non-listed women in the o -th generation; together with the listed women in that generation this yields the total set of women in the o -th generation.

Another problem was formed by those women who do occur in the genealogies (or, as far as the unlisted women in generation o are concerned, who do occur in my reconstructions), but for whom we have no data concerning their kin origin. These women form a set whose characteristics cannot be interpreted in terms of the relevant variables of the present analysis. This set is of limited size and I decided to ignore it.

Such manipulations as are postulated by the Murphy & Kasdan hypotheses can now be demonstrated by a comparison of the distribution with regard to listed women, against the distribution in the total set of women in the reconstructed ortho-lineages.

Table 4.14 gives an overview of which numbers of women are available in the various categories and the various generations, and for how many women the data are missing. The testing procedure is based on the following reasoning. If genealogical manipulation of women does occur in the way postulated by Murphy & Kasdan, then it may manifest itself in the first place by a systematic difference between the set of listed women and the set of non-listed women in the o -th generation. Murphy & Kasdan postulated that women who were married lineage-endogamously with close agnates would be suppressed from the genealogy. This would mean that the set of listed women, under the two categories of in-married and out-married women, would have

to contain significantly fewer lineage-endogamously married women than was to be expected on the basis of the incidence of such women in the total set of women (combining listed and non-listed), both for the o-th generation. Differences of this nature can be explored, in the first instance, by a comparison of percentages. However, since the numbers are not only subject to hypothetical, systematic manipulations, but also to stochastic variation, a simple comparison of percentages is not the best method: we need a statistical test which deals with the influence of chance fluctuations. One such a statistical test is the likelihood ratio test for the comparison of an empirical distribution with a theoretical distribution.⁴⁰ The total set of data for the o-th generation (which corresponds with the real numbers, and which is not subject to manipulation) can now be used to search for evidence of systematic manipulations in the higher generations. There one usually deals with people who have died and often with people the informant himself has never known; therefore, it stands to reason that the postulated manipulations show themselves the more clearly, the higher the generation under analysis: the transfer of knowledge is filtered through time. The distribution of close-endogamous, distant-endogamous, and exogamous among in-marrying listed women in generation 1 and 2 is therefore compared with the same distribution among in-marrying women in generation 0 (both listed and non-listed together). The procedure is then repeated for out-marrying women.

In the first instance the validity of the Murphy & Kasdan hypothesis with regard to genealogical manipulation of women in the Tunisian data is explored by a comparison of percentages. The data are presented in the Tables below. If the difference is smaller than 10% this is interpreted as no difference at all.

Conclusion: a comparison of percentages does absolutely not point in the direction of the Murphy & Kasdan hypothesis. Such tendencies as we seem to note, both among in-marrying and among out-marrying women, rather point in a very different direction. However, it remains to establish whether these tendencies are statistically significant or must be attributed to chance fluctuations.

The analysis of these distributions by means of the likelihood ratio test can now be illustrated by reference to the o-th generation, in-marrying women, listed against non-listed. The data are presented in the Table below:

generation 0, in-marrying women				
	listed	listed +not- listed	conclusion with regard to listed	expectation Murphy-Kasdan
close-endogamous	14 (24%)	22 (20%)	+	-
distant-endogamous	0 (0%)	6 (5%)	-	0

⁴⁰ Cf. Spitz 1961, who calls this the l'-test; van Binsbergen 1972b; and Wilkinson 1986. The advantages of the likelihood ratio test for cross-tables are several. It is non-parametric, so does not require specific assumptions about the nature of the underlying distribution of the data. As such it is akin to the well-known χ^2 test, but that one requires a minimum cell expectation of 5 - a condition that does not apply for the likelihood ratio, and that is often difficult to meet with the small-sample data of anthropological village and urban-ward studies. The two tests have the same probability distribution, tables for which may be found in any statistic manual. 5 % is an acceptable significance level.

exogamous	22 (76%)	82 (75%)	o	+
total	58 (100%)	110 (100%)		
generation o, out-marrying women				
	listed	listed +not- listed	conclusion with regard to listed	expectation Murphy-Kasdan
close-endogamous	26 (28%)	19 (22%)	+	-
distant-endogamous	2 (3%)	4 (5%)	-	o
exogamous	40 (69%)	65 (74%)	o	+
total	68 (100%)	88 (100%)		

Table 4.11. In-marrying women, generation o

	listed	listed+not- listed	conclusion with regard to listed	expectation Murphy-Kasdan
close-endogamous	14 (24%)	22 (20%)	(+)	-
distant-endogamous	o (o%)	6 (5%)	(-)	o
exogamous	44 (76%)	82 (75%)	(+)	+
total	58 (100%)	110 (100%)		

Table 4.12. Overall assessment of the Ĥumiri data in the light of the Murphy-Kasdan hypothesis

If the manipulation as postulated by Murphy & Kasdan did in fact occur then it would have manifested itself in the distribution of listed women. According to Murphy & Kasdan one would expect close-endogamous women to be underrepresented, exogamous overrepresented, and distant-endogamous unaffected. When comparing the percentages, the hypothesis is not confirmed. The likelihood ratio test now has to demonstrate to what extent these mere impressions are statistically significant. For this test we give the specific results only for one row (*e.g.* close-endogamous), while the result for the two remaining rows will be summarized. See Table h.

	listed	listed+not- listed	expected to be listed
close-endogamous	14	22	(58/110).22 = 11.6
rest	44	88	(58/110).88 = 46.4
total	58	110	

the likelihood ratio test compares the distribution in column 1 with that in column 3; $\chi^2 = 2.08$; $df = 1$; not significant at the 5% level

Table 4.13. Statistical comparison of the set of listed women as against the total set – in-marrying women, generation o, listed woman only

This means that the incidence of close-endogamous women in the set of listed women in generation o does not significantly differ from the incidence of such women in the total set of women (listed and not-listed) in that generation.

In the same way the entire generation may be processed. The results are presented in Table 4.16:

	in-married		out-married	
	test	Murphy-Kasdan	test	Murphy-Kasdan
close-endogamous	o	-	+	-
distant-endogamous	-	o	o	o
exogamous	o	+	o	-

Table 4.14. Statistical comparison of the set of listed women as against the total set – in-marrying women, the entire generation, including non-listed women

If we wished to interpret the differences between the set of listed women and the entire set in terms of the Murphy & Kasdan hypothesis, we find:

- one case of flagrant contradiction (close-endogamous, out-married)
- one case of agreement (distant-endogamous, out-married)
- three cases in which the shift as postulated by the theory was not significant
- one case in which there was a significant shift although it was not postulated by the theory.

4.8. An alternative hypothesis with regard to the genealogical manipulation of women

The differences in distribution between the set of listed women and the total set cannot be explained in terms of the theory of Murphy & Kasdan. I propose the following alternative explanation. *We assume that the set of listed women is in fact an a-select sample from the total set (four statistical results out of six are in agreement with this assumption), whereas the significant shift must be attributed to the specific interview procedure followed.* The latter point is clearest in the case of close-endogamous out-marrying women. When a genealogy is elicited these women feature in the summing-up of their own sibling group. They are less likely to be overlooked by both interviewer and informant. There is likely to be a similar explanation for the underrepresentation of distant-endogamous in-marrying women in the set of listed women.

If we assume that in the o-th generation no manipulation occurs in the sense of Murphy & Kasdan, we can now compare the distributions close-endogamous/ distant-endogamous/ exogamous such as they occur in the higher generations, with the distribution in the o-th generation. The test situation is different again. For the set of listed women in the o-th generation, the total set for the o-th generation constituted the whole of which the set of listed women itself formed a part. This is why the total set could serve as a theoretical distribution, which we compared with the set of listed women by means of the likelihood ratio test. Now we will compare the total sets in the various generations. These sets are subject to demographic chance fluctuations: in one generation a lineage may produce or absorb more women than in the next.

Admittedly, also the o-th generation is subject to such chance fluctuations. Probably we are allowed to ignore the factor of population growth: without any doubt generation o is the most numerous, but (in our rough and ready approximation) it is fair to assume that in that generation the number of married people, and the male/female ratio,. did not undergo massive change as compared to the higher generations. Let us first compare the percentages (Table 4.17):

generation 1			
in-marrying women			
		conclusion (as compared with generation o)	expectation according to Murphy&Kasdan
close endogamous	14 (19%)	o	-
distant-endogamous	7 (10%)	+	o
exogamous	51 (71%)	o	+
out-marrying women			
close endogamous	13 (30%)	+	-
distant endogamous	2 (5%)	o	o
exogamous	28 (65%)	-	+
generation 2			
in-marrying women			
close endogamous	4 (25%)	+	-
distant endogamous	o (0%)	-	o
exogamous	12 (75%)	o	+
out-marrying women			
close endogamous	o (0%)		no conclusion possible
distant endogamous	o (0%)		
exogamous	2 (100%)		

Table 4.15. Testing the alternative hypothesis, higher generations

Conclusion: generation 1 does not in the least display the pattern of manipulation as postulated by Murphy & Kasdan. Neither in generation 2, in-marrying women, does a comparison of percentages reveal the postulated manipulations. For the out-marrying women in that generation there are too few cases to justify any conclusions. For these

higher generations we now still have to test to what extent the tendencies are statistically significant.

Since now we will be comparing two distributions which are each subject to chance fluctuations we use not the likelihood ratio test but the χ^2 test. The results are presented in Table 18:

	generation 1						generation 2					
	in-marrying			out-marrying			in-marrying			out-marrying		
	test	MK	χ^2	test	MK	χ^2	test	MK	χ^2	test	MK	χ^2
close-endogamous	0	-	.001	0	-	1.14	0	-	0.21	-	-	*
distant-endogamous	0	0	1.16	0	0	0.00	0	0	1.67	-	-	*
exogamous	0	+	0.30	0	+	1.05	0	+	0.00	-	-	*

for all statistics in this Table, df = 1.

*insufficient data for meaningful analysis

MK = Murphy-Kasdan

Table 4.16. Testing the alternative hypothesis, as compared with generation 0

We can safely conclude that the genealogical manipulations of women as postulated by Murphy and Kasdan cannot be attested in the present data from north-western Tunisia.

The emphasis on contemporaries in the actors' spontaneous tracing of genealogical chains, and the suppression (or let us simply say, forgetting) of collateral ancestors above the third generation, suggests that an informant's personal acquaintance with a kinsman is a crucial factors in the latter's being included in that informant's package of genealogical knowledge. The only exception to this empirical generalization appears to be the series of lineal ancestors, which however has to be learned through systematic training. This principle leads us towards an alternative form of genealogical manipulation which might, after all, be perceived in the *Ḥumīrī* data.

Ḥumīrī society is constructed out of spatial segments which if of the same segmentary level are opposed to each other while they hierarchically include each other from one level to the next. These segments are dwelling-houses, compounds, neighbourhoods, villages and valleys. In *Ḥumīrīyya*, spatiality is a more fundamental principle governing day-to-day interaction than is kinship. A person's daily interactions are largely confined to within his village.

Kinsmen who live outside one's village and especially outside one's valley one sees at best a few times a year. Now we can assume that the genealogical knowledge concerning those kinsmen who have always lived in the same village as the informant, is larger than that concerning kinsmen who lived outside his village. The constant dispersion of parental families sees to it that not all male agnates live in the same village ; after a few generations we may often find sections of a lineage in other villages

than the original village, and even in other valleys. The marriage pattern offers yet another systematic factor in the dispersion of – particularly female – kinsmen. In the research area 95% of all marriage was virilocal, which means that marriage is largely (for 95%) a matter of women taking up a new residence. About half of all marriages is village-endogamous, the other half village-exogamous; this means that in nearly 50% of all marriages a woman takes up residence in a different village from where her close agnates have lived.

Now we can expect genealogical manipulation, as a function of the differential spatial distance between an informant's place of residence and that of his various kinsmen. Such variations in spatial distance between an informant and his kinsmen as spring from the dispersion of the local agnatic group may equally apply to men and to women. With the exception of a few uxorilocal marriages, such variation in spatial distance between an informant and his kinsmen as stems from marriages almost exclusively affect women. Genealogical manipulation with regard to men as caused by variations in spatial distance between an informant and his kinsmen can in fact be witnessed: male kinsmen who live at a considerable distance appear to be more readily left out from genealogies than male kinsmen who live nearer. However, *I did not investigate this aspect systematically and quantitatively.*

Since Murphy & Kasdan make pronouncements concerning genealogical manipulation of women, I shall here, too, limit myself to women. In principle there are four possibilities, when we compare a women's place of residence with that of an informant (Table 4.19):

the woman lives				
		before her marriage		
		in same village as informant	in different village from informant	total
after her marriage	in same village as informant	++	+	+
	in different village from informant	-	--	-
total		+/-	-/0	

Table 4.17. Four possibilities for a women's place of residence as compared with that of a third person who is the informant

On the basis of the above considerations this schema enables us to make predictions about genealogical manipulation. Women who have lived their entire life in the same village as the informant, will be stressed in the genealogy (+ +), and women who have lived their entire life outside that village, will be suppressed (- -). With regard to women who only lived in the same village as the informant either before or after their marriage, it is difficult to make a straightforward prediction. However, it is likely that women who through their marriage arrived in the same village as the informant, will be stressed (+) as compared to women who lived there only before their marriage (-) (*i.e.* as children, who are relatively unimportant for the structure of interaction); it is equally likely that both categories will be stressed more than women who absolutely never lived in the same village as the informant.

Again we can distinguished between women who married out of the informant's

lineage (*i.e.* female agnates within the genealogy), and women who married into the informant's lineage (*i.e.* the wives of agnates in the genealogy). If the genealogical manipulation as postulated by my alternative hypothesis does actually occur, it will be immediately understandable in terms of the actor's cognitions and motivations. The people with whom Ego interacts are mainly his fellow-villagers, whether or not these are Ego's agnates. To the extent to which these fellow-villagers belong to his genealogy (as Ego's agnates or as spouses of Ego's agnates), he will be unlikely to overlook them when summing up his genealogy. However, he has little interaction with people who do belong to his genealogy but who do not live in his own village; he does not know these people well and is inclined to overlook them. This is all the more likely for members of higher generations, whom he has not personally known.

Murphy & Kasdan's approach does not by far offer a similar, obvious interpretation. It presupposes the suppression of female close agnates, to the extent to which these are married lineage-endogamously, regardless of whether these women were married, and hence lived, in their (and the informant's) own village or in some other place. When applied to Ūumīriyya, their model becomes muddled since (because of the dispersion of parental families) not all close agnates of Ego are his fellow-villagers. And with regard to such female fellow-villagers as belong in Ego's genealogy (as close agnates, distant agnates, or as the non-agnatic wives of agnates), Murphy & Kasdan's approach postulates a difference in genealogical 'memory' in Ego, allegedly suppressing near agnates and stressing non-agnates, even if there are no reasons whatsoever why, within his village, Ego should have more interaction with non-agnatic female affines, or with distant agnates, than with close agnates.

Now in the sixteen sample genealogies the women were counted. The problem of the difference between listed and non-listed women was solved in the same manner as described above. Women for whom it was unknown whether they lived in the same village as the informant or in a different village, before or after their marriage, were omitted from the analysis.

Let us first inspect generation o. The data are presented in Table 20:

(a) listed only; I) in-married women

		before marriage		total
		same village	different village	
after marriage	same village	12 (21%)	23 (40%)	35 (61%)
	different village	4 (7%)	19 (33%)	23 (40%)
total		16 (28%)	42 (73%)	58 (101%)

II) out-married women

		before marriage		total
		same village	different village	
after marriage	same village	11 (19%)	6 (10%)	17 (29%)
	different village	25 (42%)	17 (29%)	42 (71%)
total		36 (61%)	23 (39%)	59 (100%)

(b) listed + not listed; I) in-married women

		before marriage		
		same village	different village	total
after marriage	same village	23 (21%)	36 (32%)	59 (53%)
	different village	8 (7%)	44 (40%)	52 (47%)
total		31 (28%)	80 (72%)	111 (100%)

generation o, listed + not listed

II) out-married women

		before marriage		
		different village	different village	total
after marriage	same village	17 (19%)	7 (8%)	24 (27%)
	different village	34 (38%)	32 (36%)	66 (74%)
	total	51 (57%)	39 (44%)	90 (101%)

Table 4.18. Listed and non-listed in-marrying and out-marrying women in generation o

Conclusion of the basis of the comparison of percentages: if manipulation has taken place, its result will have been the set 'listed'.

In the Tables 21-23 I have indicated how this set compares with the total set 'listed + not listed', with (between parentheses) the prediction on the basis of my alternative hypothesis.

		before marriage		
		same village	different village	total
after marriage	same village	o (+)	+ (+)	+ (+)
	different village	o (-)	- (-)	- (-)
total		o (+/o)	o (-/o)	

generation o, out-married women

		before marriage		
		same village	different village	total
after marriage	same village	o (+)	o (+)	o (+)
	different village	o (-)	- (-)	o (-)
total		o (+/o)	- (-/o)	

NB: differences smaller than 10% are interpreted as o

between parentheses: the prediction on the basis of my alternative hypothesis.

Table 4.19. How the set 'listed' compares with the total set 'listed + not listed', with (between parentheses) the prediction on the basis of my alternative hypothesis.

The alternative hypothesis does seem to find some corroboration. There is not a single case which is in blatant contradiction with the hypothesis; there are a few cases where the predicted tendency does not manifest itself, and finally several cases where the data agree with the prediction.

Yet statistical testing (likelihood ratio test) reveals that the deviations as found can be attributed to change.

- a. 'same village as informant before and after marriage', combined with 'same village as informant after but not before marriage' against 'different village from informant before and after marriage', combined with 'same village as informant before marriage but different after marriage':
 - for in-married women $\chi^2 = 1.22$, $df = 1$, not significant at the 5% level;
 - for out-married women: $\chi^2 = .14$, $df = 1$, not significant at the 5% level.
- b. only 'same village as informant before and after marriage' against 'different village from informant before and after marriage':
 - in-married women: $\chi^2 = .26$, $df = 1$, not significant at the 5% level;
 - out-married women: $\chi^2 = .26$, $df = 1$, not significant at the 5% level.

However, if the manipulations as postulated by the alternative hypotheses occur all, their chances are greatest in the highest generations. We can again compare them with the total set, listed and not-listed, in generation 0. The tables for generation 0 have already been presented.

in-married women				
		before marriage		
		same village	different village	total
after marriage	same village	22 (30%)	22 (30%)	44 (60%)
	different village	5 (7%)	25 (34%)	30 (41%)
	total	27 (37%)	47 (64%)	74 (101%)

out-married women				
		before marriage		
		same village	different village	total
after marriage	same village	14 (30%)	2 (4%)	16 (34%)
	different village	20 (43%)	11 (23%)	31 (66%)
	total	34 (73%)	13 (27%)	47 (100%)

Table 4.20. Comparing the total set listed and not-listed, generation 1

When we compare these percentages with those for generation 0, listed + not listed, we arrive at the conclusions presented in Table O.

in-marrying women				
		before marriage		
		same village	different village	total
after	same village	+ (+)	0 (+)	+ (+)

marriage	different village	o (-)	- (-)	- (-)
	total	+ (+/o)	- (-/o)	
out-marrying women				
		before marriage		
		same village	different village	total
after	same village	+ (+)	- (+)	+ (+)
marriage	different village	+ (-)	- (-)	- (-)
	total	+ (+/o)	- (-/o)	

(between parentheses) the prediction on the basis of my alternative hypothesis.

Table 4.21. Generation 1 as compared (by percentages) with generation 0, in-marrying and out-marrying women

4.8.1. Conclusion concerning my alternative to Murphy and Kasdan's hypothesis

A comparison of percentages suggests that in generation 1 the genealogical manipulation as postulated by the alternative hypothesis does in fact occur. However, there are some cases in which such manipulation cannot be demonstrated, while for other cells (relating to out-marrying women) we witness an effect opposite to that postulated by the alternative hypothesis (albeit that only small differences are involved).

Statistical test: the differences can again be attributed to chance. 'same village as informant before and after marriage', combined with 'same village as informant after but not before marriage' against 'different village from informant before and after marriage', combined with 'same village as informant before marriage but different after marriage':

- In-married women, generation 1 against generation 0: $\chi^2 = .72$, $df = 1$, not significant at the 5% level;
- out-married women, $\chi^2 = .80$, $df = 1$, not significant at the 5% level.

A different method: only 'same village as informant before and after marriage' against 'different village from informant before and after marriage' (according to the alternative hypothesis the difference would be expected to be considerable):

- in-marrying women, $\chi^2 = 1.80$, $df = 1$, $p = 0.18$;
- out-marrying women, $\chi^2 = 3.07$, $df = 1$, $p = 0.08$. Our test statistic assumes values which are somewhat more extreme but still not significant.

The results are presented in Table 4.24:

in-marrying women

		before marriage		
		same village	different village	total
after marriage	same village	6 (32%)	8 (42%)	14 (74%)
	different village	0 (0%)	5 (26%)	5 (26%)
total		6 (32%)	13 (68%)	19 (100%)

out-marrying wome

		before marriage		
		same village	different village	total
after marriage	same village	1 (50%)	0 (0%)	1 (50%)
	different village	1 (50%)	0 (0%)	1 (50%)
total		2 (100%)	0 (0%)	2 (100%)

Table 4.22. Conclusion on the basis of comparison of percentages in generation 2 with generation 0 (as a whole)

in-marrying women only

		before marriage		
		same village	different village	total
after marriage	same village	+ (+)	+ (+)	+ (+)
	different village	- (-)	- (-)	- (-)
total		+ (+/0)	0 (-/0)	

(between parentheses: expectation on the basis van the alternative hypothesis)

Table 4.23. Generation 2 as compared (by percentages) with generation 0, in-marrying and out-marrying women

Conclusion from Table 25:

- *There is complete agreement with the alternative hypothesis.* However, the data concerning out-marrying women are so limited that no conclusion should be based on them.

Statistical test: only for in-marrying women.

- First 'same village as informant before and after marriage', combined with 'same village as informant after but not before marriage' against 'different village from informant before and after marriage', combined with 'same village as informant before marriage but different after marriage': $\chi^2 = 2.90$, $df = 1$, $p = .09$, not significant at the 5% level.
- Then only 'same village as informant before and after marriage' against

'different village from informant before and after marriage': $\chi^2 = 1.60$, $df = 1$, $p = .21$, not significant at the 5% level.

- *Conclusion*: especially in the higher generations the quantitative data do suggest the validity of the alternative hypothesis. We may assume that persons will the more readily feature in the genealogical knowledge of a particular informant, if they are, or ever were, adult fellow-villagers of that informant. However, we must stress that such corroboration of the alternative hypothesis as was found when merely percentages were compared, while statistical testing revealed that these initial impression may well be attributed to chance. Yet the results are sufficiently positive to warrant further analysis with more data.

4.9. Conclusions of the overall argument in this chapter

- (a) *Ḥumīrī* genealogies are shallow in the sense of Murphy & Kasdan but for other reasons than advanced by these authors.
- (b) In *Ḥumīrī* genealogies the genealogical manipulation of women as postulated by Murphy & Kasdan does not occur.
- (c) In *Ḥumīrī* genealogies, however, there does occur a different type of genealogical manipulation of women, notably:
- (d) Genealogical integration and kinship-based segmentation requires collateral ancestors to be explicitly included in the actors' genealogies, as siblings and cousins of direct lineal ancestors. However, such collateral ancestors are present, in the data set, in only the most recent generations: time has blurred them out almost completely for the ascending generations. This means that only the lowest segments can engage in segmentary opposition by reference to collateral ancestors, and to unilineal descent in general. Only a few informants were prepared to state sibling relationships involving mythical ancestors, and when they did their pronouncement were not at all consensual.

Spatial segmentation (as discussed at great length in van Binsbergen 1970 / in press (a), and succinctly but clearly, and in adequately published form, in van Binsbergen 2018) explains conclusion (a) and (c), it renders Murphy and Kasdan's hypothesized genealogical manipulation of women impossible (b). The central factor in the pattern of genealogical knowledge emerging turns out to be not kinship (notably patrilineal descent), but territoriality, in other words the spatial organisation of local society. The contradiction between agnatic ideology versus bilateral practice I have extensively discussed elsewhere in my work on *Ḥumiriyya*.

5. Lineages: Kinship-based group classification in Ḥumirīyya

5.1. Introduction

Formally there is every reason to call Ḥumirī society patrilineal. In the first place there is the indigenous societal ideology as summarised above. Moreover in the hundred of genealogies which I and others collected in the area, the informants largely display a pattern in line with Murdock's definition of patrilineality:

'discarding the mother's kin group and affiliating the child exclusively with the consanguineal kin group of the father.' (Murdock 1965: 44).

The vertical genealogical knowledge concerning cognates and affines is much more limited than that concerning agnates (*cf.* Hartong 1968: 55).

Therefore, when collecting genealogies, I entertained a number of simple expectations:

- that persons who were undeniably agnatically related (*i.e.* persons concerning whom there was a local consensus as to their being brothers, paternal cousins, etc.: in other words: agnatic cores), would produce exactly identical 'consanguineal kin groups of the father';
- that these genealogies would yield, unequivocally, the agnatic links between the agnatic cores, so that on the basis of these links wider agnatic units could be identified; and finally,
- that I would end up with a limited number of very large agnatic clusters, each of them unequivocal of composition, and in the top of each genealogy a specific apical ancestor who would be agnatically independent and irreducible as compared to all other apical ancestors in the data set.

In other words, I expected to find patrilineages, in terms of Murdock's definition:

'A consanguineal kin group produced by either rule of unilineal descent [in this case patrilineal

] is technically known as a lineage when it includes only persons who can actually trace their common relationship through a specific series of remembered genealogical links in the prevailing line of descent.' (Murdock 1965: 46).

Possibly such patrilineages would turn out to be further combined in such larger units as sibs and phratries (Murdock 1965: 47).

I shall begin my discussion with patrilineages, in this chapter, to proceed in chapter 4 to a discussion of higher-level kin groups, which I shall there demonstrate to constitute clans. However, the present chapter and the next are intimately related: *Ḥumiri*s denote both patrilineages and clans with the same terms *firqa* or *duār*; and the regularities which we shall demonstrate to apply to one type of such social groupings in *Ḥumiriyya*, will turn out to apply to the other as well.



In the background centre the ex-colonial farm now owned by the chiefly family

Fig. 5.1. Young women, under the token chaperonnage of a junior male relative, in the process of repairing the threshing floor for the oncoming harvest

5.2. Forms of genealogical manipulation

My expectations were not fulfilled. On the basis of earlier ethnographers of unilineal systems⁴¹ I could have expected a limited degree of inconsistency, but I was completely bewildered by the genealogical chaos which I found to exist, not only at the higher generational levels and distant kinship relationship, but also with regard to relatively

⁴¹ *E.g.* Evans-Pritchard 1967: 198 *f.*; Peters 1960: 32 *f.*; Lewis 1965: 109 n. 3

close kinship relationships in and generations identical with or adjacent to the informants's themselves.

Beeker (1967), who researched actual and preferred housing patterns in the village of Sīdī Mḥammad, published genealogies of the inhabitants, who were to become my day to day informants in the course of my own research. Initially the presence of Beeker's data dissuaded me to carry out further genealogical research in this village. However, I soon found out that the incidental genealogical information volunteered by my informants contradicted Beeker's data on many points. Beeker, however, in his thesis does not mention any inconsistencies in his informant's genealogical information, nor does he present his genealogies as his own attempts at consistent reconstruction. I was led to collect very abundant genealogical data and to reconstruct, on this basis, the actual genealogical relations between the inhabitants of Sīdī Mḥammad. These reconstructions continued to differ very strongly from Beeker's, not only in the higher generations whose members have been long extinct, but also with regard to very close kinship ties between living villagers. For the larger part, these discrepancies must be attributed to genealogical manipulation on the part of the informants; for the rest, the differences may be due to defects in the genealogical method of either researcher, or of both.

Referring to identical individuals, or to undeniable agnatic cores, different informants would state absolutely different sets of associated agnates. Such sets would include persons who, according to other informants, would be absolutely no agnates of the referent individuals or cores. Alternatively, if the sets of agnates associated with a referent individual or core would agree from one informant to the next, these sets often turned out to have a fundamentally different genealogical composition, so that *e.g.* one informant would state that A was B's father, whereas another informant would claim that A was B's FFB – or worse. It was impossible that all genealogies as collected were an exact rendering of the historical reality: at least part of them would have to be manipulated – in the manner amply discussed in the preceding Chapter 4)

Comparisons which I carried out within the body of abundant genealogical data at my disposal, in combination with such data on marital relationships and residential history as served to identify individuals in genealogies), led to the identification of a limited number of basic operations which informants turned out to perform upon their genealogical knowledge.

For a discussion of these operations it is useful to distinguish between two concepts: that of the ortholineage and that of the pseudolineage.

A lineage is based on unilineal descent in either the male or the female line. Our discussion is limited to the male line and in the remainder of my argument therefore lineage ill exclusively mean: patrilineage.

I shall define an *ortholineage* as:

the complete set of legitimate, biological descendants of one apical ancestor, reckoned according to the patrilineal principle of descent, provided that ancestor lived in a past which was still so recent (given the overall average genealogical depth of the society under study) that inclusion of that ancestor in the participants's 'specific series of remembered genealogical links' (Murdock) can be considered a fair possibility.

If one had all historical data at one's disposal, the members of the ortholineage could be included in one historically correct genealogy, showing all members to be mutually connected through chains of agnatic kinship.

Hartong (1968: 54) assessed the generational depth of the genealogical furnished by forty Ḥumiri informants. He found the depth to be from two to eight generations, with a median of three to four. I include Hartong's table (1968: 54):

number of direct patrilineal ancestors mentioned	1	2	3	4	5	6	7	8	9	total
number of informants	0	2	11	11	9	4	2	1	0	40

Table 5.1. Depth of Ḥumiri genealogies

'On the average an informant could name 4.3 ancestors, *i.e.* he could go back just a little beyond his FFFF' (Hartong 1968: 55).

Since fractions of ancestors are meaningless it is better to take to median as a measure of centrality: the median is at 4 generations. I did not assess whether Hartong's data on this point tally completely with mine, but this is very likely: his research area overlapped with mine; and contrary to Beeker, he had the same research assistant Hasnawi bin Tahar. In terms of my definition, ortholineages in Ḥumiriyya will seldom comprise more than six generations.

Practically the ortholineage is Murdock's patrilineage.

I shall define a pseudolineage as:

a specific set of individuals from the past and the present, which according to the views of one specific informant (not necessarily a member of the pseudolineage) are linked through chains of agnatic kinship; the informant can specify these links in detail; in his or her opinion the persons thus linked form a set of agnatic descendants of one identified ancestor.

Ortholineage and pseudolineage can be represented in a formalised manner, by means of a genealogy.

Only then are ortholineage and pseudolineage identical, if the informant manages to state, in their correct genealogical interrelations, all actual descendants of the apical ancestor, without omitting any and without including any non-agnates. Such mechanical completeness and exactitude is rare in real life, and it is particularly in a society like Ḥumiriyya where genealogies play a major role in the day-to-day social process at the village level, as we shall see in the course of my argument. In practice there is always a considerable difference between the ortholineage on the one hand, and the pseudolineages which individual actors produce with reference to the same set of people. Below I shall discuss the reasons for these differences.

It should be clear that the prefixes ortho- and pseudo- are only used from the point of view of the ethnographer. From the informant's point of view the pseudolineage he or she states (in the form of series of individuals and their genealogical relationships) usually seems to sum up that informant's conception of historical truth at that moment of time – even though this conception nearly always turns out to be

systematically distorted.

I defined pseudolineage as the view of one single actor. Intersubjective consensus is emphatically not implied in the concept of the pseudolineage. Such consensus – the convergence of genealogical and historical notions of more than one actor – will have to be demonstrated by empirical research, and it turns out (for instance in the analyses of Chapter 4, above) to be problematic. When stating a pseudolineage, an informant selects (and traces specific genealogical relations between) a few dozen persons – pseudolineages never comprise more than that number of persons – out of the total set of a few thousand people who in the course of the last few centuries dwelled in the twenty-odd square kilometers that constitutes the research area and its immediate environment. The number of ways in which one can make such a selection is truly astronomical, and that figure again has to be multiplied by the number of different genealogical links one can trace between the persons thus included in one's pseudolineage. Without the slightest possibility each informant could construct totally different pseudolineages, even when including the same persons as other informants do. Fortunately the actual variation between Ḥumiri informants's pseudolineages is not that astronomical. We always find some minimal consensus, among a limited number of informant, and concerning a limited number of people past and present, with regard to the latter's belonging to certain, locally more or less consensually discerned pseudolineages. Of course, such consensus at the actors's level does not warrant in the least that all the people included in such a consensual set are actually true agnates, *i.e.* actually belong to a historical ortholineage. Yet these cores are sufficiently important to justify the introduction of a new term: that of consensual pseudolineage core.

The main causes for the discrepancy between pseudolineage and ortholineage in Ḥumiriyya can be demonstrated to be the following:

- (a) People's knowledge of the past is limited. Especially people who lived a short life or who left no offspring are likely to be forgotten.
- (b) One does not know the offspring, in so far as they dwell or dwelled elsewhere, of those persons who emigrated away from what has constituted the ortholineage's territory in the present and near past; or, if one's own ortholineage immigrated into its present territory in the recent past, one does not know the membership of the collateral branches in so far as they have remained in the original territory.
- (c) As one descends along the generations down from the top of the ortholineage, the number of actual members becomes so large that it is no longer practical to know all the branches well in their mutual connections, even if no branches have emigrated from the ortholineage's territory.

These causes lead automatically to a situation where part of an ortholineage is no longer included in an individual informant's pseudolineage. Operations (b) and (c) amount to what is known as fission in the anthropological literature. The occurrence of operation is in itself already to be expected on the basis of the limitations of the human mind and of the exchange of information between human beings and across generations; in this connexion let us not forget the impact of physical barriers such as spatial distance in general, mountain ranges etc. In Ḥumiriyya these general effect are

greatly enhanced by the presence of an indigenous societal ideology one of whose implications, as we have seen, is the claim that migration does not occur. Because of this ideology the members of a certain ortholineage branch can often not afford to include, in their individual public accounts of the composition of their own agnatic group (*i.e.* in their pseudolineage as recorded by an ethnographer), the existence of agnates who dwell or dwelled elsewhere. This ideological bias also renders it difficult to obtain reliable information on the places of residence of people from the past.

Because of the operations (a) through (c) the pseudolineages become only incomplete reflections of the true ortholineages, but it does not render the pseudolineages erroneous, historically wrong. This however is the effect which the next operations have:

- (d) The members of the ortholineage which are included in the pseudolineage are presented in a different genealogical connexion than is historically correct. In a more specific form involving the mixing up of generations, this operation has been described under the technical term of telescoping (Evans-Pritchard 1967: 198 *f.*; Peters 1960: 32 *f.*). Altering the genealogical connexion can be done in many ways. One common structural implication of certain genealogical manipulations are clear from Fig. 5.1.

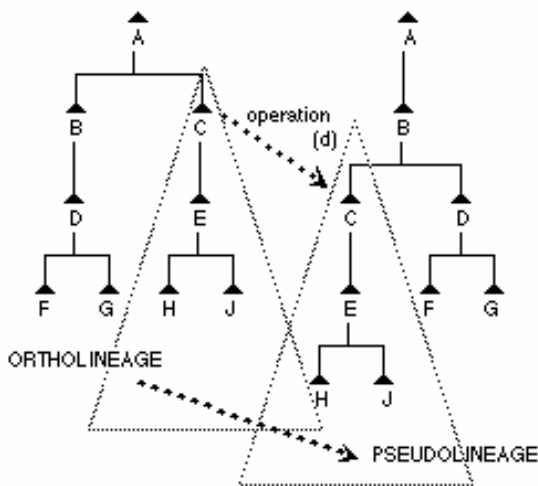


Fig. 5.2. An example of telescoping

Thus a certain subset within the ortholineage loses its historical autonomy vis-à-vis a similar subset at the same generational or segmentary level: in Fig. 1 the descendants of C lose their historical independence vis-à-vis B, because in the pseudolineage B is presented as father of C, whereas in actual fact, *i.e.* in the ortholineage, B and C were brothers. We shall soon explore the structural background of such operations.

- (e) If two ancestors who are historically not each other's agnates, with their

descendants are yet presented as forming one pseudolineage, we have the phenomenon which is known in the anthropological literature as fusion. We could also say that one group affiliates to the other. This operation in many cases also implies fission: fission between the affiliating group and its actual historical agnates in so far as the latter do not go along with the fusion. In such a case fusion and fission are complementary aspects of the same process.

Through these manipulations upon the genealogical data as offered by historical reality the actors revise the past. This reinterpretation is such that the present appears to be more in line with the dominant ideology. When immigration has brought together, within an area of one square kilometer or less, of a number of different ortholineages which cannot be traced to a common agnatic origin, such a situation is irreconcilable with an ideology which implies: 'whoever live together must be agnates'; so one performs operation (e), fusion the different ortholineages into one pseudolineage. Alternatively, a situation where agnates, after emigration of part of the ortholineage, no longer live in each other's close proximity, is in contradiction with the ideology that 'agnates live together'; so one performs operation (b) and structurally forgets the distant branches. The local norm stipulates that one lives on land which one has acquired from one's F, FF, and the latter's agnatic ancestors; living one such ancestral land is far more prestigious (van Dijk 1968: 49 f.) than living on land which one has obtained through gift, purchase, hire, a share tenancy contract, loan or usurpation – most of which alternatives imply immigration. Ideologically migration does not exist, and when it occurs it is a source of shame. Therefore, if at all possible one claims that one's own pseudolineage has 'always' lived where it is found today. Since in actual fact most ortholineages arrived fairly recently in the research area (in the course of the nineteenth and even twentieth century) such a claim means, genealogically:

- affiliation, through operation (e), of one's own ortholineage branch to an ortholineage branch which can boast a longer period of uninterrupted local residence; as well as
- differentiation, through operations (a) and (c), between one's own ortholineage branch and other branches which are equally newcomers

The system of kinship terminology (*cf.* the discussion in Chapter XXX, above) facilitates genealogical manipulation. All ancestors, either paternal or maternal, in all generations above Ego's parents (F, M) are denoted by the same term, djadd. An informant can easily, and without lying, declare that he or she shares 'an ancestor' with a neighbour even if the latter belongs to a different ortholineage – provided that there were affinal relations between these ortholineages in previous generations (which the pattern of local endogamy as set forth in appendix 2 renders very likely). Even if the members of either ortholineage still more or less reckon themselves to belong to different consensual pseudolineage cores, yet the indigenous formulation 'we have the same ancestor' (*'andna jadd wāhad*) contains a very strong suggestion towards one shared pseudolineage: the decisive step towards operation (e) has been made.

Another preparation for operation (e) is the following. By contrast to the ideology there has been a constant migration of smaller and larger social units within and across the borders of the research area. Now it is most likely that the membership of

most of these immigrant groups at the moment of their immigration into the research area was not exclusively recruited on an agnatic basis, as far as the heads of the constituent households are concerned. Alternative modes of recruitment include cognatic and affinal ties with the head of the immigrant group, patronage extended by this head to strangers fleeing from feud, employment as a herdsman, a notion of a shared though distant tribal or clan origin, or membership of the same religious brotherhood.⁴² The agnatic ideology so distorts this historical reality that already after very few generations the descendants of various non-agnates in the immigrant group are considered as agnates within one pseudolineage (*cf.* appendix 2 example 1

So we have three basic situations of immigrating non-agnates; emigrating agnates; and the local growing apart of ortholineage branches without out-migration. How in either case the pseudolineage in the consciousness of the individual informants will differ from the ortholineage depends on a number of factors: the length of uninterrupted local residence of the groups involved; the relative strength of the groups involved in terms of number of members, political and economic power (*cf.* Chapter 4 example 3;

What often happens is that the apical ancestor of a recent immigrant group is going to be considered as a son or son's son of the apical ancestor of the consensual pseudolineage core which locally serves as hosts for the immigrants. Length of local residence is that still the most important factor determining the concrete form the quasi-kinship is going to take: whoever arrived earlier on the local scene will occupy the highest position in the (fictive, pseudolineage) genealogy. The inclusion, in the pseudolineage, of members of the host group however still leaves the possibility that the descendants of the immigrants, despite their affiliation, yet live on as a distinct branch of the now extended consensual pseudolineage core. Now it is possible that after some generations a reinterpretation of the pseudolineage takes place, on the basis of changes which have occurred in the meantime in the configuration of the other determinants as mentioned above. For instance, if in the meantime the numerical strength of the immigrant group has come to approach, or even to surpass, that of the host group, then it is possible that in the pseudolineage the genealogical relationship between the apical ancestors of both groups will be revised in the direction of a greater equality as expressed in generational level: instead of F/S or FF/SS relations typical of the earlier stage of immigrant affiliation the fictive relationship can alter into B/B even S/F.⁴³

A special case in this context is formed by those actual agnates who for a long time (a century or more) have continued to live in the same area, subdivided in various branches which have the immediate offspring of the lineage founder as their apical ancestors. Even if such a case is entirely in accordance with the ideology, yet also here manipulation will occur: operation (c). Genealogies of pseudolineages are, through the various systematic operations as discussed, the projection into the past of group relationships in the present, and this is also true in the present case: the contemporary differences between the various branches in terms of numerical strength, economic

⁴² *Cf.* Favret 1968 and Peters 1967 for North Africa in general; and Souyris-Rolland 1949 for Ĥmiriyya).

⁴³ For examples see notes 16, 20; also Miedema (1967: 27) presents a similar case for the village of Habaš-Karaš, close to my research area.

and political power, and even in terms of length of residence in a specific part of their shared territory. all lead to a reformulation of the genealogy (cf. Chapter 4 example 3).

The manipulation of genealogies, in other words the construction of ever shifting pseudolineages, is not a manifest and conscious process which can be captured at a given moment in time, but an implicit and continuous process. Initially the living members of the more senior generations in the genealogy still know what the actual historical connections were, but this knowledge is already beginning to become irrelevant for them. This will be clear from the following argument. Let us consider a certain ortholineage A. As part of its membership (A') has emigrated, the out-migrants's positions (as marriage partners, fellow-villagers, fellow-users of the land, fellow-pilgrims of the same shrines etc.) in the structure of day-to-day interaction of those ortholineage members who have remained behind (A'') will gradually be taken over by immigrated non-agnates (B) on the local scene. For as will be clear from later chapters, and from appendix 1, the crucial feature of day-to-day interaction partners is that they are neighbours, not that they are agnates.

Now for those remaining behind (A'') it will be largely irrelevant (except for crisis situations) to stick to a formal pseudolineage structure which does include (in line with the ortholineage) agnates who since their emigration hardly play a role on the local scene, and which excludes non-agnatic immigrants (B) who in the day-to-day interaction of A'' have come to occupy role positions which ideologically have been reserved for agnates. The immigrants (B), in their turn, have the same interest in forgetting the historical genealogical relations. The knowledge of the elders, which thus has turned useless if not socially destructive, is not clearly and unequivocally transmitted to the next generation. In a discontinuous process a new genealogy will emerge, in which group relationships in the present are being projected as kinship relations into the past: kinship relations which may be totally fictive but whose significance for day-to-day interaction is based on their very suggestion of historical correctness.

Thus different actors find themselves in different phases of the process of the manipulation of pseudolineages, in an attempt to adapt the latter to the ever changing composition of their direct social environment.

As said before, we may assume that the elderly are rather closer to the genealogical reality of recent, now extinct, generations than their offspring. Hence, for the ethnographer wishing to reconstruct the historical reality of lineages one category of *strategic* informants is of great importance: people who are the youngest of their generation (as measured in terms of a number of generations from a certain ancestor). They may be several, sometimes many, decades younger than the oldest of their generation, and thus find themselves to be the age mates of those who are one or two generations further removed from the same historical ancestor. After my return from the field, when I analysed the data, it turned out that some informants of this strategic type had offered me the genealogical links between branches of ortholineages which were no longer consciously realised by the other actors (even if the latter were in fact older than the strategic informants). For a case in point see Chapter 4, example 3.

No doubt this is one of the causes of the fact that the genealogical data I collected display a very great diversity. Around the same individuals from the past and the

present the informants construct sets of agnates which overlap only very partially (*cf.* Chapter 4, example 2). If this were only a difference in *phase* (in terms of generations, or of number of years elapsed since immigration or out-migration), it would influence all members of a particular generation in a similar manner: we would have to find, among the informants from one generation, a considerable consensus with regard to the identification of pseudolineage cores, and the constituent membership of each core. However, example 3 in Chapter 4 shows that more is involved than just a phase difference.

In that example, the absolute difference of opinion between the informants 20 and 25 turns out to be a direct reflection, not even of group relations, but of individual relations, at a given moment of time, within the constantly shifting pattern of relations and interactions in the village. My data contain more similar cases.

If each individual has a different conception again of the genealogical structure of his or her social environment, it follows that there are but few consensual pseudolineage cores, and that those which do exist contain only few persons each. In general it would seem that, within certain limits beyond which manipulation of the historical facts is hardly possible, actors consider each other, individually or group-wise, as agnates or as non-agnates, depending on their readiness to identify with each other; and this readiness is almost a perfect reflection of the momentary structure of interaction in their social environment. Beyond the limits of manipulation* we find

- the consensual pseudolineage cores: persons whose agnatic ties cannot possibly be denied because the genealogical connections are based on too recent a past; and also, on the other extreme,
- those persons who have very recently immigrated into the area from relatively far away. In the latter case it is impossible to affirm agnatic relations between the newcomers and the consensual pseudolineage cores which boast a much longer local residence. However, as we shall see in the next chapters, the system of group-wise classification allows them and their local hosts other forms of identification: on the basis of clan or tribal unit, and spatial unit.

Apparently with regard to agnatic kinship there is a great difference between the following two concepts:

- (a) The analytical concept of historical agnatic kinship. Using this concept the ethnographer is in a position to reconstruct ortholineages and to determine the complete set of people who are a particular Ego's close agnates, distant agnates and non-agnates, as the case may be.
- (b) The indigenous concept of kinship such as used in *Humiriyya*. This concept is expressed in the constant use of (agnatic) kinship terminology between partners in dyadic relations (especially neighbours and fellow-villagers in general. It underlies such frequently uttered claims of (agnatic) kinship as: 'we have the same ancestor' (*djadd wahad*), or 'we are of the same family' (*firqa wahada*). The set of persons in Ego's social environment to whom Ego applies this concept, overlaps only very partially with the set of Ego's living agnatic kinsmen – as the ethnographer can determine by examining the ortholineages. Many living historical agnates are excluded from the indigenous concept of

kinship: Ego does not interact with them, does not know them, or does not recognise them for the (more or less distant) agnates they are. Alternatively many non-agnates are included *in an agnatic idiom*: cognates, affines, neighbours, friends and acquaintances. That these 'indigenous kinsmen' of Ego have in common is not any historical unilineal descent from a common ancestor, but such a relationship with Ego that the latter is prepared to identify with them in terms of the agnatic idiom which is stipulated by the indigenous societal ideology.

The agnatic concept of kinship is in fact identical with the analytical, scientific concept of social relationship (*cf.* van der Klei, forthcoming).

The above argument does not necessarily already imply that in Ḥumiriyya agnatic kinship does not play any role whatsoever in the formation of social relationships. For it remains possible that in the set of Ego's interaction partners (Ego's 'indigenous kinsmen') yet the share of Ego's historical agnates is larger than could be expected on the basis of the occurrence of historical agnates in Ego's social environment. Moreover, it is possible that Ego interacts more frequently with his historical agnates than with others who are not members of his reconstructed ortholineage. These questions will be addressed in the next chapters, and in appendix no. 1.

Thus for Ḥumiriyya the traditional anthropological discussions of segmentation based on unilineal descent are turned upside down: one does not maintain social relation because of the opportunities for identification as contained in unilineal descent, but unilineal descent, but one identifies in terms of a unilineal kinship idiom because of the existence of actual relationships. Whether one identifies or dissociates in terms of kinship, turns out to be largely inspired by opportunism. And because of this opportunism actors do not arrive a any high level of consensus concerning the kin groups out of which their social environment consists, and the composition of these kin groups.

It is of some importance to assess whether the diversity and lack of consensus with regard to pseudolineages, such as described by me, have been a more or less constant factor in Ḥumiri social organisation across the last few centuries. Alternatively, these features might be the results of recent structural changes, under the influence of external factors (the French conquest, 1881; since, increasing integration of the region in the national state). In some respects we can trace the influence of recent changes on the residential pattern: the emergence of a new, more permanent type of dwelling to replace the tent of goat hair and the kurbi of ephemeral, arboreal material (Beeker 1967: 13, 24; Hartong 1968: 48 *f.*); the increasing concentration of the population in more permanent villages; the massive population increase; the closure, by the state, of a large part of the existing agricultural and pastoral land area, and the prohibition to make new clearings. The armed conflicts between spatial segments, such as were usual before the French conquest ceased completely in the first decade of the twentieth century under the impact of the *pax gallica*; this meant that local inhabitants had lost a major method to keep out immigrants, *i.e.* the introduction of new ortholineage branches into their own spatial segment. Likewise the rise of a structure of inequality in the region (Jongmans 1968) may have decreased the interest in, and hence the precision of, kinship-based social grouping. In this context it is noteworthy that Gellner (1969: 39 *f.*) poses a relationship between the degree of sedentarisation and the

relevance of kinship-based social grouping:

'Very sedentarised tribes, *i.e.*, those in whose lives agriculture (with irrigated and hence immobile fields) plays a far larger part than pastoralism, may dispense with genealogical definitions of the larger, higher level social groupings. In their case, the wider and more general group may be defined geographically. (...) The genealogical conceptualisation of groups only comes in at the lower levels. (...) All this however, does not disturb the tree-like neatness of the segmentary system, though it does conflict with widely accepted theories about Berber mentality.'

No doubt, present-day Ḥumiriyya is 'very sedentarised', rather more so than a century ago. Could this then be one of the reasons why kinship-based social grouping is now at least much less important than social grouping based on locality? However, the Ḥumiri situation does differ from the one described by Gellner: in present-day Ḥumiriyya the largest social groupings as distinguished indigenously (the valleys and chiefdoms) are largely defined along spatial lines, but this locality aspect is not confined to these higher levels but permeates all lower levels of segmentation, right down to the household.

All this should not make us overlook the fact that the inconsistency of kinship-based social grouping in present-day Ḥumiriyya is primarily connected with a principle that is so central in Ḥumiri conceptualisation of human interaction that I cannot believe this principle to be a recent innovation: the indigenous ideology of kinship. According to this concept all those count as Ego's kinsmen, with whom Ego has positive social relations. In any such case where this condition applies, kinship terminology is being used. The people concerned follow in their interaction the norms and expectations which are prescribed, as ideal, between kinsmen; and they create fictive genealogical ties between each other. This fictive kinship has strong agnatic overtones. Most probably this principle was as dominant a century ago as it is today. It is this principle which throws light on genealogical manipulation, and on the inconsistency of kinship-based social grouping. Therefore I would submit that lack of consensus, and opportunism, are likely to have traditional characteristics of the use of kinship between the Ḥumiri social organisation. perhaps the Cyrenaican parallel provides an additional argument for this interpretation. However, it is difficult to assess how a century ago individual actors manipulated their historical genealogical knowledge, although sometimes the pseudolineages of present-day actors offer some hints on this point.

Given this state of affairs no ethnographer can expect to find a consistent system of well-defined indigenous kinship units. The non-consensual, protean kinship structure as used by the Ḥumiris has very little in common with the patrilineages as defined by anthropology – and certainly does not provide a solid basis for segmentation along unilineal descent lines (*cf.* Chapter 6).

After the above argument one will appreciate my amazement that, with the exception of Hartong (1968), many field-workers in Ḥumiriyya notable have considered the Ḥumiri *firqa* as an (ortho-)lineage and have published and have published fully-fledged genealogies of the villages of their research.⁴⁴ Could it be that the residential history of

⁴⁴ I have cursorily defined the Humiri term the term *firqa* above. It is equated to patrilineage by, among others, Schulte Nordholt (n.d.: 14 f.), Hartong (1968: 4 f.), Beeker (1968: 4 f.), van Dijk (1968: 6); extensive

those villages was so much less complicated than in my research area? Would the informants of those ethnographers have so much greater need to preserve and transmit their knowledge of the past in a non-distorted form? I would rather suggest that these analysts did not recognise the lack of consensus in their data, perhaps partly because they each worked with too few informants on genealogical matters. With the exception of Hartong, genealogies in themselves have not been topics of research, but hastily collected basic data for the analysis of other phenomena. The genealogies acquired a reality value which in fact they do not have at all.

If we do want to have genealogies (which may include additional data on residential history and marital relationships) which constitute adequate reconstructions of the historical reality (and for certain analytical problems such genealogies are indispensable), then we have to identify step by step the results of manipulation in the genealogical data. These distortions betray themselves when one has collected genealogical data from a large number of informants: since every informant occupies a slightly different position in the local social organisation, the results of the manipulation of each will not converge to form the same distorted pattern, but on the contrary we will see contradictions between the statements made by various informants. Next we shall have to classify each case of manipulation, once detected, as belonging to any of the types outlined above: the operations (a) through (e). Such classification in itself requires a measure of historical insight, which one derives from previous, more simple reconstructions of ortholineages. Finally the result of manipulation as presented by an individual informant will have to be translated back into what amounts to the most plausible reconstructions of the historical reality – admitting that the later is virtually unknown to us by any other means.

Such reconstructions, meanwhile, have only a limited use. For the reconstructed genealogies, however correct, continue to differ in many respects from the ways every individual informant structures his or her social environment in opportunist, quasi-kinship terms. Thus one cannot possibly take these genealogies as point of departure for an explanation of contemporary interaction in terms of the perceptions and motivations of the actors. Only beyond the limits of manipulation* (*i.e.*: with regard to small consensual pseudolineage cores, sets of close agnates, and very recent immigrants) is it conceivable that the actor's perception of agnatic kinship between two relation partners could be an important determinant for interaction in its own right – in stead of than a result of such interaction as springing from other than kinship factors. But these are the very cases in which an ethnographer would be able to determine the existence and degree of agnatic kinship easily, without all sorts of complicated reconstructions. That even in those apparently obvious cases a researcher still runs the risk of making considerable mistakes is clear from note 5. Therefore even with regard to consensual lineage cores, *i.e.* close kinship between people now living, it is of great importance to collect as much genealogical information as possible. Of course, such information is included in the most recent generations of the ortholineage reconstruction.

Ĥumiri genealogies, but without a discussion of genealogical manipulation and reconstruction, are being offered by, among others, Beeker (1968), van Dijk (1968), Martin (1966), Bos (1969), Miedema (1967) and Jonkhout (n.d.).

With these considerations in mind, some of the following chapters will explore the significance of relatively close kinship in Ḥumiriyya.

The method as described here, for the reconstruction of ortholineages on the basis of the informants's pseudolineages, requires an abundance of data, in which a large number of persons past and present occur more than once, and are to be identified by means of their names and further characteristics (such as: other persons claimed to be their agnates, spouses or close affines; their places of residence; and their approximate data of birth). IN this way one can discover correspondences and contradictions, and localise and classify the apparent manipulations. It is my contention that these requirements have been met for the analyses as presented in this argument. Therefore I ventured to reconstruct the historical pattern of ortholineages in the research area, with their residential history since the first half of the nineteenth century. The sources for these reconstructions are: my own abundant genealogical data; the data from collective research in 1968 as referred to in the preface; and selected passages from Martin 1966, Hartong 1968, Beeker 1967, Miedema 1967, Bos 1969, and Huitzing forthcoming. The full publication of my reconstructions would take up too much space, and would not constitute an essential corroboration for my analysis. In stead I shall present relevant excerpts from my reconstructions throughout the argument of this book. Of course, the full reconstructions and the data on which they are based are available for inspection on request.

For a few salient cases Chapter 4 illustrates the basic data in the form of the genealogical accounts of individual informants, and my own reconstructions. **Fig. 12** moreover gives a very limited excerpt from these reconstructions: it shows, in greatly simplified form, the residential history of the villages of Sidī Mḥammad and Mayziyya since c. 1900.

5.3. **Intermediate** summary

In this chapter I set out to approach the system of group classification in Ḥumiriyya in terms of the classical anthropological model of segmentation based on unilineal descent. This turned out to be an infertile approach. The patrilineage such as habitually defined in anthropology (*cf.* Murdock 1965) does not exist in Ḥumiriyya.

In order to describe to discrepancy between historical kinship, and the conceptions individual actors have concerning the distribution of sets of kinsmen in their social environment, I split the lineage concept of anthropology in two: the ortholineage as historically correct and capable of analytical reconstruction, and the individual, hardly ever historically correct, pseudolineage. By means of a limited number of basic operations actors create their own individual pseudolineages out of their (limited) historical knowledge on ortholineages. The major operations are:

- the pruning of pseudolineages by the elimination of those persons who played an inconspicuous role in the past;
- the elimination of those persons who belong to ortholineage branches with a different residential history than one's own branch (fission through migration);

- the elimination of persons who belong to branches with virtually the same residential history as one's own branch, but whose connection with the latter lies in such a remote past that the relevant historical knowledge has been lost (fission without migration);
- presenting members of the ortholineage in a different genealogical ordering than the historically correct one (telescoping);
- inclusion in the pseudolineage of persons who do not belong to the ortholineage (fusion).

The actors's notions concerning their mutual agnatic relationships differ from historical reality, are non-consensual, and contain elements of opportunism. Such notions reflect existing interactions between individuals, rather than that such interactions are initiated by the awareness of kinship. The individual actors's notion concerning agnatic kinship only converge to a certain consensus with regard to relatively small sets of relatively close agnates (consensual pseudolineage cores), and with regard to very recent immigrants. As a result only relatively close agnatic kinship is conceivable as a possible independent factor in interaction and the formation of social relations in Ḥumiriyya. For with regard to kinship in general there is no fixed frame of reference that lies ready for all actors and in which all actors (in so far as they are likely to come into contact at all) can situate each other more or less unequivocally; therefore, the form and frequency of interaction between these actors cannot be explained by reference to such a kinship frame of reference.

This conclusion is exclusively based on the fact that in Ḥumiriyya genealogical notions are non-consensual, and opportunist. The fact that besides they also tend to be historically incorrect, in itself is not enough to disqualify kinship as an independent factor in interaction. For that genealogies have far more to do with the formulation of structural distance, than with historical correctness, has already been argued a long time ago by Evans-Pritchard (e.g. Evans-Pritchard 1967: 106 *f.*).

The reconstruction of ortholineages and their residential history shows a pattern of very great spatial mobility: a constant spatial expansion of ortholineage branches, followed by out-migration or local extinction. Provided we have enough data such reconstructions can be deemed to be reasonably reliable over a period of a century or slightly more.

5.4. The historical reconstruction of ortholineages in the research area

Especially Guus Hartong's research in the research area and wide surroundings has elucidate the large-scale residential history of the valley of Sidi Mhammad. My own oral-historical explorations has managed to complete his results in some ways. I shall not here try to present an overview of the history of orthologineages; this topic is largely covered in Volume II, where it overlaps with my reconstruction of the history of the shrines in the research area.

5.5. The distribution of ortholineages (1968)

I have at my disposal extensive and reasonably reliable reconstructions of the residential history of the ortholineages in the research area since c. 1800. When these reconstructions are combined with the contemporary census data on the various villages, one can assess the diversity of ortholineages in the research area in 1968. The research area comprises 12.2 km² (cf. note 2). Table 27 specifies the distribution over the various ortholineages of the households in the six villages.

names of clans and of consensual pseudolineage cores, by which the members of these ortholineages are designated by the participants and by some researchers (Beeker 1967, Bos 1969, Hartong 1968, Miedeman 1967)	villages							total
	ortho-line-age	Sidi Mhammad	Mayziyya	Fidh al-Missay	Raml al- ^c Atrus	Ham- raya	Ĥam ay- siyya	
Abaydiyya, Ĥraysiyya, Saydiyya, Mayziyya, Zaghaydiyya	I		15	1		5		21
Zaghaydiyya, Mayziyya	II	9	4					13
^c Arfawiyya, Ayaydiyya, Metayniyya	III	3		1				4
Tra'aya, Zaghaydiyya	IV	4						4
Zaghaydiyya, Suaylhiyya, Ĥemaysiyya, Quassim, Zrarqiyya, Araybiyya	V	19				2	11	32
^c Arfawiyya, Metayniyya	VI	5**)				3		8
^c Arfawiyya, Metayniyya, Shabniyya	VII	2						2
^c Arfawiyya, Shabniyya	VIII	1						1
^c Arfawiyya, Ba'adliyya	IX	2		8	4			14
firqa al-Mehatab	X			7				7
Rekaybiyya, Huamdiyya, alad bin Sayyid	XI		2					2
Saydiyya	XII			1	1			2
Suadliyya	XIII				1			1
Auaniyya, Zaghaydiyya	XIV					29		29
Tebayniyya	XV	0*)						0
Rshaybiyya, Zaghaydiyya	XVI		1					1
Tebayniyya	XVII	1						1
	XVIII	0*)						0

Rba'aya, ^c Arfawiyya	XIX					6		6
	total	46	22	18	6	45	11	148

*) represented by one non-head of household, in a family belonging to ortholineage V

**) including one family at Remal

the authors mentions may use a different system of transliteration

Table 5.2. The distribution of ortholineages in the research area, 1968

In identifying a household as belonging to a certain ortholineage I have taken the head of household as the point of departure. Households with a male head have been reckoned to belong to the latter's ortholineage. Households with a female head have been reckoned to belong to the ortholineage of the deceased husband, the former head of household. The latter approach might lead to distortion in those cases where a women hailing from a different village and from a different ortholineage returned to her own close agnates after her husband's death; however, such cases, not totally unknown in Ḥumiriyya, do not occur among the households in the research area in 1968.

On the basis of the data available the ortholineages as distinguished in table 25 cannot be further reduced to each other.

We may conclude that in such a small area the heads of households already belonged to 17 different ortholineages, while another two ortholineages were represented among the non-heads of household. This diversity is much higher not only than is stated in the Ḥumiri societal ideology, but also than is claimed in the analyses of Ḥumiri society so far.

code number	sections known as	typical members
I	Saydiyya	Muḥammad bin Amur, Ahmad/ Hassan/ Abd Allah bin Kashrud
II	Mayziyya	^c Ali bin Sā ^c ad, Sāfi bin ^c Amr
III		Abd Allah / al-Hadi bin ^c Aissa
IV	Tra'aya	Rābah bin ^c Ali, Salah bin Tarshun
V	Zaghaydiyya, Zar'kiyya	chief Hillāl, chief Hassuna, Diābh bin Hassuna, Amār Bu Tāra, Hasni bin al-Abādi
VI	^c Arfawiyya, Matayniyya	Salah bin Ḥamis, Hamuda bi'l-Ahsin
VII	Hafsiyya	^c Amār and Ibrahim bin ^c Abd Allah
VIII		Bashir bin Ibrahim bin Shabān
IX	^c Arfawiyya	Maṣṣūr bin Ḥamad, Yunis bin Amar, Salah bin Mḥammad
X	firqa al-Mahātab	
XI		Muḥammad bin Ibrahim
XII	Saydiyya	
XIII	Suadliyya	
XIV	Zaghaydiyya	Najma bint Hassuna bin ^c Abda
XV	Tabayniyya	Hafsuya mart Yūssuf bin ^c Amr, Bashir bin ^c Abu'l Qassim

XVI	Rashaybiyya	Muḥammad bin Tayib
XVII	Tabayniyya	Muḥammad bin ^c Amur (Muḥammad bin Tunis)
XVIII		
XIX	Arfawīyya, Arba'aya	

Table 5.3. List of ortholineages in and around the area of research

The research area comprises parts of two valleys. Table 25 makes clear that ortholineage diversity occurs not only at the valley level but also at the village level: every valley and every village comprises more than one ortholineages. Although not explicitly worked out in the table, the same can be said for neighbourhoods and even compounds.

Moreover, the spatial spread of ortholineages is considerable. For most of the ortholineages enumerated in table 25 members one or more members are known to reside in Ḥumiriyya but outside the research area.

Table 25 presents the ortholineages as they are occurring in the research area today (1968). In the period spanned by my historical reconstructions (c. 1800-1968) several other ortholineages are known to have been active in the research area (*cf.* Hartong 1968). Of the ortholineages as discussed in table 18, very few have lived in the research area since before 1800; the great majority settled in the area in the course of the nineteenth and even twentieth century.

6. Clans in Ḥumirīyya

6.1. Clans: Introduction

During my 1968 fieldwork, I hit on a type of quasi-kinship classifications which initially posed as pseudolineages but soon turned out to differ from the latter. With regard to the pseudolineage the actor has a clear, albeit it usually historically incorrect, opinion as to the genealogical links between the members of the set of people which he takes to constitute an agnatic group. This is so by definition, as we have decided in Chapter 4. In the case of classifications of the type I shall discuss in the present chapter, the pseudolineage genealogical chain which in the informant's opinion reflects a historical reality, is extended by adding the name of a certain apical ancestor or of a group name derived from that ancestral name. The transition from the penultimate to the last name in the chain is characteristically marked by a halt, a hesitation, on the part of the informant. I would suggest that the informant does not really consider this final transition to be one between son and father – *i.e.* between two succeeding generations is a continuous line of descent – but as the connection, across an indefinite number of generations, of the (allegedly historical) apical ancestor of the pseudolineage to a 'supreme' ancestor supposed to have lived in some mythical past. The latter does not belong to the pseudolineage, for there is no 'specific series of remembered links' (Murdock).

When an informant states the chain of ancestors, the mythical ancestor may appear already after two 'remembered' ancestors who are truly members of the pseudolineage. A case is point in example 2 in Chapter 4. In such a case the informant turns out to be conscious of the fact that still many intermediate ancestors separate the FF as stated in the second position from the mythical ancestor in the third position, but the informant cannot specify these intermediate ancestors or refuses to do so. Often some other informant, referring to the same chain, is capable of producing a number of 'remembered' intermediate ancestors, suggestive of historical F/S links, preceding the mythical ancestor.

Within the research area there was a definite, if not total, consensus as to which

mythical ancestors had ever occurred there and in adjacent areas. Their number turned out to be modest: Zaghudud, ^ˆArfa (or ^ˆArif), Bu-Maza, Ḥadir, Bu-Mandjil, as-Sayyid, Rashab, Mutani, Bu-Tara, Bu-Dabus and al-Hadjdj. Some would deny the autonomy of some of these supreme ancestors, presenting them as sons of one of the others in the list. Many genealogies would end on any of these names. If genealogy ends in some other name and still refer to persons from the research area, then the conclusion is justified that, in the perception of the informant concerned, the genealogical chain contains only F/S transitions, and not transitions between mythical ancestor and distant descendant – in other words such a genealogy describes the pseudolineage without the addition of mythical ancestors.

Pseudolineage is an analytical term introduced by myself; of course Ḥumiriyya does not know an indigenous term to distinguish between the pure pseudolineage and the pseudolineage with one mythical ancestor attached. Both kinship groupings are denoted by the terms of firqa or duar (for the other meanings of these terms see Table 1 below).

So we can distinguish between a type (a) firqa (= pseudolineage) and a type (b) firqa. For the type (b) firqa we shall now try to find some equivalent analytical term.

6.2. The occurrence of clans in Ḥumiriyya

At first sight the type (b) firqa reminds us of the *sib* in anthropology:

‘When the members of a consanguineal kin group acknowledge a traditional bond of common descent in the paternal or maternal line, but are unable always to trace the actual genealogical connections between individuals, the group is called a *sib*’ (Murdock 19675: 47).

However, with regard to sibs Murdock introduces an element of unanimity:

‘If *all* persons born with the name Smith in our society regarded themselves as related...’ (Murdock 1965: 47).

Now with regard to the Ḥumiri type (b) firqa such consensus is lacking: often the same individual is reckoned, by different informants including himself or herself, to different type (b) firqas; and the same situation occurred with regard to clusters of close agnates. (appendix 4;

Moreover, in Ḥumiriyya persons who ‘acknowledge a traditional bond of common descent in the paternal (...) line’ usually do not belong to just one ‘consanguineal kin group’, but to a number of affiliating kin groups.

The Ḥumiri type (b) firqa has a greater resemblance to Murdock’s *clan*. Murdock defines the clan as follows:

‘For a group to constitute a genuine clan it must conform to three major specifications. (...) It must be based explicitly on a unilinear rule of descent which unites its central core of members. (...) To constitute a clan a group must have residential unity. (...) The group must exhibit actual social integration (...). There must be a positive group sentiment, and in particular the in-marriage spouses must be recognised as an integral part of the membership’ (Murdock 1965: 68).

Let us examine these three requirements for the Ḥumiri type (b) firqa.

'Unilineal rule of descent'. The type (b) firqa is in fact the social unit as stipulated in the Ĥumiri societal ideology, including the rule of unilineal descent which in this society is highly formal and explicit.

'Residential unity'. In my opinion, 'residential unity' occurs when the following two conditions are fulfilled:

- the unequivocal attribution of persons now living to one of the mythical ancestors as enumerated above;
- the complete partitioning of the research area and surroundings over all the mythical ancestors as enumerated above, in such a way that all those who are considered to be, for instance, the descendants of Zaghdud, live in a distinct territory as compared to all those who are considered to be the descendants of any other mythical ancestors.

As argued before, the first condition is not met by far. Neither is the second condition fully met. In the data at my disposal the persons which the informants associate with the same mythical ancestor are dispersed all over the research area. However, if we take the spatial units large enough, then it is true to say that the area within which one expects to find the descendants of a certain mythical ancestor, seldom exceeds a few square kilometers. Most of the mythical ancestors as listed above are supposed to have ancestors within the research area and its immediate surroundings. Moreover it is remarkable that within the research area the relative frequency with which informants associated persons with a particular mythical ancestor, was closely associated with the place of residence of those persons (appendix 5; In this respect the type (b) firqa may not display residential unity to the full extent, but the tendency towards such unity is certainly there. The names of particular mythical ancestors appeared to be attached to specific parts of the research area. I will return to this point in the course of the present chapter.

'Actual social integration, a positive group sentiment'. These are very vague terms , impossible to use for analyse without prior operationalisation. (Incidentally, vagueness is a characteristic of other Murdock definitions as well, cf. Brunt 1969: 9 f.). I believe that we do justice to Murdock's underlying viewpoints if the members of the social unit in questions, with regard to certain forms of interaction and with regard to certain types of relationships prefer to select (perhaps even exclusively select) each other as partners than the members of other such units, provided that this preference is based on the very fact that they consider each another as members of the same social unit (and not on accidental differences in wealth, economic power etc. which may make them attractive as interaction partners regardless of their indigenous classifications in terms of social units). Because of the inconsistent demarcation of the sets of people who are reckoned to descend from the same mythical ancestor, it is impossible to test quantitatively whether this condition is met in the case of the type (b) firqa. However, in general I have found that husbands, and their close kinsmen, who identify as belonging to a particular type (b) firqa, tend to reckon their wife and her close kinsmen to the same type (b) firqa, even when the marriage was contracted over a considerable distance (e.g. 3 km), and even though the wife's kinsmen who live at that distance reckon themselves to belong to a totally different type (b) firqa. The social integration of type (b) firqas is also manifested by the fact that actors perceive a

permanent relationship between a specific type (b) firqa, on the one hand, and on the other: a saint, a shrine associated with that saint, and the pursuit of certain religious specialist statuses.

Murdock's condition concerning the in-marrying spouses is interesting. Virilocality is not only the norm of Ĥumiri society, but is also the practice in 95% of the cases.⁴⁵ Formally, in-marrying spouses retain the membership of their own type (b) firqa. In practice however they are soon identified with the type (b) firqa into which they married. This is demonstrated by the following facts:

- (a) *Women.* There is the norm that women, married in from elsewhere, after the death of their husband continue to live on the latter's land and with his agnates (sometimes formalised in terms of a levirate marriage). Widows who observe this norm are highly respected, and those who return to their own agnates are despised. Many widows observe this norm, and thus continue to live on the territory which is associated with a particular type (b) firqa. Moreover in-marrying women are preferably regarded as belonging to the same type (b) firqa as their husbands from the very start.
- (b) *Men.* My data contain a few cases of uxori-local marriages, contracted in 1910 or later, which led to a situation where already in 1968 the male spouses (or their children) had adopted, in their genealogies, the principle mythical ancestor of their new place of residence, even though these men turn out to identify with a different mythical ancestor in their original place of residence (where their agnates still continued to identify with the latter ancestor).

In summary: Murdock's concept of the clan is, with some reservation, applicable to the Ĥumiri type (b) firqa. Henceforth we shall denote Ĥumiri type (b) firqas as clans. (A similar approach is adopted by Schulte-Nordholt n.d.: 15).

6.3. Clans and other forms of group classification

I return, once again, to the problem that the same individuals and/or close agnates are not being consistently assigned to the same pseudoclans by different informants. This phenomenon can be explained as follows:

When a actor assigns a particular person to a particular clan, this is on the basis of that informant's opinion as to the pseudolineage to which that person belongs. The informants reasoning can be summarised as follows:

'this person A descends from the (pseudo) lineage founder B; B is a descendant from the mythical ancestor C; and that is why A belongs to the clan which bears C's name.'

In a number of interviews I found such a reasoning almost literally. Considering this state of affairs, the lack of consistence in pseudoclan attribution with regard to close

⁴⁵ The relatively low figure of 5% uxori-local marriages applies to marriages contracted in the research area during 1959-1968. This is in accordance with Banck's (1968: 54) findings on the nearby valley of al-Mazuz. Hartong (1968: 67), whose research area comprised both Banck's and mine, offers (without concrete numerical substantiation) the figure of 20% uxori-local marriages, which I consider far too high.

agnates can, theoretically, have two different causes:

- The large differences between actors with regard to the pseudolineages which they distinguish in their social surroundings;
- even when two actors have the same opinion concerning which pseudolineages do exist and which persons are members of these respective pseudolineages, these informants can still differ as to the attribution of each pseudolineage to the various clans and mythical ancestors that circulate in the research area.

Given the nature of my data it is impossible to ascertain which of both factors is at work, and, if both factors operate simultaneously (which is the more probable situation), what is their respective impact. In other words, it is impossible to ascertain whether some *consensus* governs informants's individual opinions as to the specific relationships between certain pseudolineages and certain mythical ancestors. My data on the clan attributions of consensual lineage cores somewhat point in the direction of a limited consensus; but there is also, and unmistakably, the opposite tendency: the ad hoc identification and dissociation on the basis of the existence of current social relationships, as I have been able to demonstrate in the case of pseudolineages.

For such ad hoc identifications the pattern is as follows. When somebody, A, has an interest (because of co-operation, neighbourliness, the existence of marital ties, or the aspiration towards such relationships) to identify with somebody else, B, in his or her social environment, then A's first inclination will be to represent B as a member of A's own pseudolineage. If this fails, because the case falls outside the limits of manipulation, then A can claim the identification to go back to some mythical, untraceable past, by linking up the two respective pseudolineages of A himself and B to the same mythical ancestor, without having to invent the precise genealogical connections.

In addition to genealogical possibilities at identification there are a variety of spatial identification devices, which we shall discuss in the next chapter.]

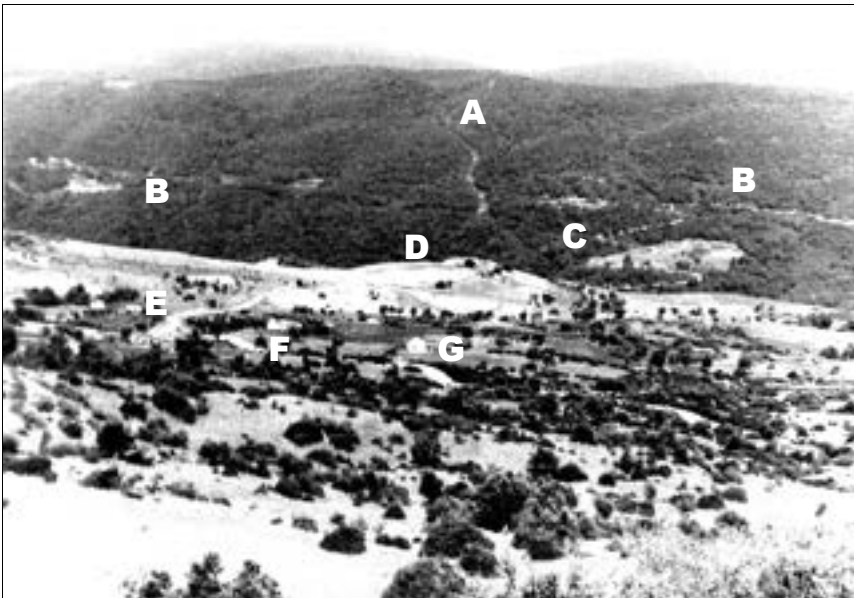
Finally there is the possibility of identification through the sharing of a group name on the basis of political association and dwelling in each other's proximity either at present or in the past. In that case, it is often feasible to dispense completely with the genealogical fiction. Here we are at the level of the tribal fraction, the tribe, and the tribal confederacy. These tribal forms of identification did and do occur in Ḥumiriyya, as is clear from Souyris-Rolland's analysis (1949: passim). Contrary to spatial classifications such tribal classifications often display a measure of spatial discontinuity: tribal groups with the same name and mutual identification can occur in places that are rather distant from one another.

6.3.1. An example of the functioning of a tribal unit: The Drid tribe

#CASE 6.1. THE DRID TRIBE. One example of the functioning of such a tribal unit in the face of spatial discontinuity in Ḥumiriyya becomes clear when we bring together certain data from Hartong (1968), Souyris-Rolland (1949) and Cuisenier (1962). Here I refer to the relationship between the members of certain subgroups (pseudolineages) of the ^ᶜArfawiyya clan in the chiefdom of ^ᶜAtatfa, on the one hand, and on the other the members of two other clans (Ulad Hillal and Huamdiyya) a few kilometers to the south of ^ᶜAin Drāham. On three occasions between 1875 and 1890 various ^ᶜArfawiyya groups from ^ᶜAtatfa joined the Ulad Hillal or the Huamdiyya (Hartong 1968: 29, 74, 77). Hartong does not draw any conclusions from this finding. But in view of the fact that by that time there were a large number of other clans

available in the region, this repeated teaming up of members of precisely these clans cannot be a coincidence: there must have been some strong identification between these clans. The nature of this identification is clear from note 13: ⁶Arfawiyya, Ulad Hilla and Huamdiyya are fragments of the Drid tribe in Central Tunisia, which had reached the ⁶Ain Drāham area between 200 and 150 years ago, long before ⁶Ain Drāham became an urban centre. Until today the link between these three, and other, Drid clans is manifest in religious activities and in the marriage pattern: despite the relatively considerable distances (considering the usual marital distance in Ḥumiriyya) there are a number of marriages between members of these clans.

These tribal classifications in Ḥumiriyya correspond with the units which other authors, notably under the terms of *leff* and *soff*, have described for other parts of North Africa (Gellner 1969: 65 f.; Favret 1966: 105). When in fact a tribal unit does display spatial continuity, its character tends towards that of a spatial unit on the one hand, but on the other hand its members will tend to formulate its integration in genealogical terms.



In the far background (A) the mountain range called Raqbat ⁶Arfa – after the apical ancestor of the ⁶Arfawi clan which dominated the religious history of the area since the 18th c. CE; (B) the motor road connecting Tabarka (North, to the right) to the border town of Babouch and further to ⁶Ain Draham (South, to the left); (C) the winding unpaved road up from the Wad al-Kabir to the motor road, past the extensive clearing of the outsider Mr Hamouda bil ⁶Ahsin (my landlord (⁶Abd Allah bin ⁶Aissa) 's brother-in-law); (D) on Kef al-Hanut ('Bakery Hill') the large ex-colonial farm now owned by the chief's family – realm of the formidable saint Bu Qasbaya; (E) my dwelling during fieldwork, with the other houses of the ⁶Aissa family; (F) the houses of Chief Hassuna's widows Mrs Umborka and Turquyya, and of the faqir Mr Mansur bin Ahmad; (G) the domed shrine of Sidi Mḥammad Jr, behind which the majority of the houses of the village of Sidi Mḥammad are situated. The domed shrine of Sidi Mḥammad Sr is visible from this vantage point but falls outside the photograph, about half the picture's width to the right of the righthand margin

Fig. 6.1. The valley of Sidi Mḥammad as seen from high up in Mayziyya

6.3.2. A political confederation reformulated by the actors in genealogical terms

One example is the tribal grouping named ^ʿAtatfa. This was formed in the second half of the nineteenth century out of a confederation of, among others, the clans of Zaghaydiyya and Ulad al-Hadjdj, in an attempt to counter the expansion of the clans of Tabayniyya and ^ʿArfawiyya (Hartong 1968; Miedema 1967). Incorporation of the ^ʿArfawiyya, among other clans, enhanced the spatial contiguity of the new confederation, which was consolidated when after 1881 the French created a formal political organisation of chiefdoms: ^ʿAtatfa became the name of one such chiefdom. In the meantime, within the context of the functioning of spatial and kinship groupings, such as analysed in the present argument, it is self-evident that at least some actors have come to reformulate this structure in genealogical terms. Thus Souyris-Rolland (1949: 135, cf. note 14) speaks of

‘Berrim [Bu Rihan? – WvB], ancêtre des Atatfa’.

Likewise, Miedema (1967: 6) quotes an informant according to whom

‘Sidi Mohammed [sc. Mḥammad – WvB] was the ancestor of all Atatfas [sic].’

Since the establishment of an externally-imposed formal political organisation in Ḥumiriyya under the control of a central government, such tribal units have lost most of their significance, and present-day Ḥumiriyya only retains a few traces of them. I shall not devote a separate discussion to them. However, more specific research into the present-day and past significance of these tribal units in Ḥumiriyya would be worth-while.

The above serves to place the ad hoc identifications with mythical ancestors in their social context. However, such identifications are not always *ad hoc*: sometimes there is, as said, a certain consensus within the set of people holding such identifications. Therefore we shall now try to trace the mechanisms which create such consensus. In other words: how do clans emerge in Ḥumiriyya?

6.4. The emergence of clans in Ḥumiriyya

It is my contention that at one stage the basis of a Ḥumiri clan was an indigenously perceived, consensual pseudolineage core, much in the way of Murdock’s (1965: 68) general suggestion when he speaks of ‘a unilineal rule of descent which units its central core of members’. Clan nomenclature by means of the proper name of specific mythical ancestors may corroborate this contention. We may then surmise that a particular pseudolineage, as it more and more deviated from the ortholineage through the mechanisms as described in Chapter 4, continued to bear the name of a particular, eminently significant ancestor, even if nobody, not even his historical descendants, were in a position any longer to state the precise continuous chain of descent. Maintaining that name was than a sign of autonomy of this group vis-à-vis other such groups. This autonomy can be explicitly desired by the members of the group in question: for consideration of prestige; or because it afforded them rights to land in the area where that ancestor had lived, in terms of the overall society ideology. Alternatively it is possible that the group name, and the mythical ancestor implied in

it, cling to the group in question because surrounding groups refused to identify with them and lend them their own respective mythical ancestors, again for reasons that may have combined considerations of prestige and land rights.

6.4.1. Mythical ancestors in residential dynamics: An example

One example of such dynamics was investigated by Tamsma and myself in 1968, under the supervision of van der Veen. Of the ʿArfawi clan, two branches of ortholineage 6, still maintaining a mutual sense of agnatic kinship, immigrated into the village of ʿArbaʿaya in c. 1900 respectively c. 1920. Until then, for a couple of decades ʿArbaʿaya had been inhabited by the members of ortholineage 22, which coincides with the consensual pseudolineage core called ʿArbaʿaya, after the historical ancestor ʿArbi (born c. 1800); by many local people the ʿArbaʿaya core is considered to belong to the Dabābsa clan (mythical ancestor: Bu-Dabus). In the research area and surrounding areas the ʿArfawi clan clearly stands out as a distinct grouping, even though there is a lack of consensus with regard to which individuals and pseudolineage core actually belong to the ʿArfawīyya. Members of the ʿArfawi clan command special prestige, and are considered to have a more than average involvement with one particular type of religious activity, the ecstatic cult. The attraction which the ʿArfawi grouping inspires is clear from the fact that many consensual pseudolineage cores in the vicinity of ʿArfawīyya have also adopted the name of ʿArfawīyya. The ʿArbaʿaya grouping in the village of the same name is a case in point; apparently their shift in nomenclature reflects an attempt to integrate with the ʿArfawi immigrants in that village, which had come to dominate the earlier ʿArbaʿaya group both in numbers and in wealth. The integration largely failed, and most members of the ʿArbaʿaya group had to find a refuge in other villages through uxorilocal marriages. From the local ʿArfawīyya there is not the slightest rapprochement vis-à-vis the ʿArbaʿaya; occasionally they identify the ʿArbaʿaya core as Dababsa (which in this area has almost become an insult); and out of the dozens of marriages which they have contracted since their immigration into the village not even one was with the ʿArbaʿaya, despite the fact that the latter are their closest neighbours. The exceptional nature of such a marriage pattern is clear from my discussion, below, of the marriage pattern in the research area. (This passage is based on data jointly collected by Tamsma and myself, data I later collected on my own, and moreover on Hartong 1968: 15 f., 34 f.; Miedema 1967: 23 f.).

The important question here is: on what grounds was the mythical ancestor of the clan selected? If indeed the clan does develop from a consensual pseudolineage core, then both types of units will be provided with an ancestor in basically the same general manner: through the elimination of less significant ancestors, and through emphasising the most significant one. The ancestor lending his name to a pseudolineage usually lived around the time of the immigration of that group into its present territory, or within that territory, into a particular village. Such migrations in the past were always accompanied by interactions with other groups on the local scene. For contrary to the local ideology, Roman and other archaeological remains testify to the fact that the area has seen a history of continuous occupation going back far before the time that the ancestors of the present inhabitants immigrated there. The interaction

between immigrant and host group could take the form of negotiation, but armed conflict was also common.⁴⁶ The immigrant group's leader in such interaction was of sufficient historical significance to lend his name to the group; my data contain indications to this effect, *e.g.* Salah in Chapter 4 example 1. When a consensual pseudolineage core, as named in this manner, then develops to form a clan, that ancestral name remains attached to the clan. Alternatively, however, I have ample indications that in a number of cases the mythical ancestors of clans were already associated with a certain pseudolineage core long before the latter's immigration into its present territory. Such association may already have been mythical at the time of immigration, but in other cases it was not and the ancestors in question, later to be known as mythical, was still a 'remembered' member of the pseudolineage, linked through a specific genealogical chain of specific F/S transitions to the persons who constituted the pseudolineage at the time of immigration.

The following two examples show that in some cases the name of an ancestor, later to become a mythical ancestor, was already associated with a group (normally a pseudolineage core, later to become a clan) before that group's immigration into the research area:

6.4.1.1. A mythical ancestor may already be associated with a grouping before its local immigration: (1) The case of Zaghdud

The mythical ancestor Zaghdud was said to hail from Kayrwan (an important city in eastern Tunisia, primarily a religious centre); his descendants are said to have reached the highlands of north-western Tunisia only very gradually (*cf.* Hartong 1968: 41).

6.4.1.2. A mythical ancestor may already be associated with a grouping before its local immigration: (2) The case of ʿArfa

The case of the mythical ancestor ʿArfa (or ʿArif) of the ʿArfawiyya has been better documented. Cuisenier (1962) made a study of a grouping, the Ulad ʿArfa, in North Tunisia about 100 km east of the research area. The Ulad ʿArfa claim to descend from ʿArfa, and to be a fraction of the Drid tribe in Central Tunisia. The historical link between the Cuisenier's Ulad ʿArfa and the ʿArfawiyya in the research area is clear not only from the (rare) name of ʿArfa, but also from the fact that Souyris-Rolland (1949: 135) claims the Ḥumiri mythical ancestor ʿArfa to be an immigrant from the Drid tribe.

Souyris-Rolland offers an account of the 'histoire traditionnelle de la Kroumirie' without indicating the sources of his data and the method through which he has processed them. His article is a compilation of raw data, probably largely based on local interviews, but lacking sociological insight. With great naivety local myths are presented as reality; thus saints and mythical ancestors are presented as in a discussion of migrations and other exploits which in reality can only be attributed, if at all, to the social groupings which are associated with these saints and mythical ancestors. While I am prepared to adopt Souyris-Rolland's insight that there is a connexion between ʿArfa and Drid, his claim that ʿArfa came to the research area as an individual immigrant is unacceptable: in the migration myth as related by Souyris-Rolland, ʿArfa stands for 'the pseudolineage, or clan which prior to its arrival in the research area was already associated with the (mythical or historical) ancestor ʿArfa.'

The fact that in two relatively remote areas in northern Tunisia people trace their ancestor to a

⁴⁶ *Cf.* Hartong 1968: 26 *f.*; Bos 1969: 11 *f.*; Miedema 1967: 21, 24 *f.*; Souyris-Rolland 1949.

mythical ancestor ^ʿArfa already suggests that the clan of that name originates from outside the research area. Moreover my informants were convinced that also in Algeria there were groupings which trace to the same mythical ancestor. In fact, it seems possible to trace the historical figure of ^ʿArfa in the history of Tunisia! In the middle of the sixteenth century, when the Maghrib was under constant attacks from the Christian states, Kayrwān saw the actions of Sīdī ^ʿArfa, the head of the religious brotherhood of the Shabbiyya. Sīdī ^ʿArfa led the resistance against a weak prince, and the mobilisation against the Christian invaders (Bel 1938: 378 *f.*). Cuisenier tells us that in the past his Ulad ^ʿArfa were maghzan, mercenaries and tax collectors in the service of a central authority. Souyris-Rolland (1949) makes the same claim for the groupings in and around the research area, for the time before they immigrated there, and even explicitly mentions the Shabbiyya as the central authority in whose service these maghzan were; when the Shabbiyya lost their power, the hated mercenaries were allegedly forced to seek refuge in the highlands. I assume that more is involved here than sheer coincidence, and that the mythical ancestor ^ʿArfa from the research area was in actual fact he Sīdī ^ʿArfa from Kayrwān, a few centuries before the clan with mythical ancestor ^ʿArfa arrived in the research area. This is all the more plausible since also the Ḥumiri ^ʿArfawiyya claim that their ancestors once lived in Kayrwān (Miedema 1967: 19).

On the basis of Hartong (1968: 34 *f.*) and of my own data I suspect that the lineage core which lend its name to the ^ʿArfawi clan arrive in the research area and close surroundings (up to a radius of a few km) around the end of the eighteenth century: perhaps a few decades after the other major clan, that of the Zaghadīyya, and perhaps even simultaneously with the latter. The initial dwelling place of the ^ʿArfawiyya in Ḥumiriyya must have been to the southwest of the research area (Hartong 1968: 34 *f.* discusses alternative views). From there they moved c. 1880 to the village of ^ʿArfawiyya, which has now disappeared, southeast of the research area, and from there they spread over the valleys of al-Millah and Sīdī Mḥammad.

The connection between Ḥumiri ^ʿArfawiyya and Drid was also made by one of my informants in the village of ^ʿArba'aya. The knowledge of some ^ʿArfawi informants goes back even further: they claim that their groups history in Tunisia was preceded by a period of residence in Saqīyya al-Hamra in Mauritania (*cf.* Hartong 1968: 36), while the first origin would lie in Arabia itself (Miedema 1967: 19). The religious connotation of the ^ʿArfawiyya (which is already clear from their association with the Shabbiyya and Sīdī ^ʿArfa, and which will be further elaborated upon in Volume II) would be in accordance with such a stay in Saqīyyat al-Hamra: for centuries this region was one of the main centres of diffusion of Islam in North Africa, place of origin of religious orders and of saints (Brunel 1925: 27).

The initial stage of the emergence of a clan is than the occurrence, in a particular area, of a particular consensual pseudolineage core bearing the name of a particular ancestor. The further evolution has formally two different aspects:

- (a) The adoption of the name of one particular pseudolineage core by other groups; such a name ends on -iyya and is derived from a particular ancestor.
- (b) The inclusion of that ancestor at the top of the individual pseudolineages and at the top of the genealogies of the consensual pseudolineage cores, in so far as these individual and pseudolineage cores belong to other, affiliating ortho-lineages.

It will be clear that the adoption of the ancestor in itself implies the adoption of the group name. However, the reverse case does not necessarily apply: it remains possible that the group name is adopted without the concomitant claim of descent from the mythical ancestor in question. I shall presently elaborate on this point.

It is tempting to look at these two aspects (a) and (b) as a difference in phase, where

the adoption of the group name (a) represents the first phase, which may or may not be followed by the genealogical inclusion of the ancestor (b).

Below I will demonstrate the existence of a spatial aspect to the distribution of clans. This has to be related to clan formation. From the spatial point of view we can represent the formation of clans in two different ways, which I shall designate model 1 and model 2 respectively.

6.4.2. Model 1

The most obvious is the representation according to which the adoption of a group name by other groups results from the frequent interaction between these groups. This interaction can have various aspects:

- the expansion (in terms of numerical strength, wealth, or territory) of the one particular pseudolineage core which is to determine the name of the future clan;
- moreover the decline of other consensual pseudolineage cores, either as a result of that rival expansion or such other factors as human and livestock diseases, crop failure, demographic fluctuations, drying up of springs, relations which other groups in the wider surroundings which are not part of this process of clan formation, and social agents outside Ĥumiriyya, such as a central state;
- differences in prestige between consensual pseudolineage cores involved in the same process of clan formation;
- frequent marital relations and other forms of positive integration between the pseudolineage cores involved; and
- shared hostility vis-à-vis other groups not involved in this particular process of clan formation.

If the adoption of the group name is followed by, or is accompanied by, the adoption of the core group's ancestor then it is often necessary that this ancestor is relegated to a mythical past. The linking-up of the affiliating pseudolineage cores has to be situated in a mythical past, and cannot be more recent, because one is only too clearly aware of the fact that in the recent past these cores had a separate existence and separate genealogical identity. Affiliation at the pseudolineage level (instead of the clan level) is only possible if one of the two affiliating cores involved is not strong enough to retain its separate identity: when the numerical difference is too great, or when one core's territory is violently annexed by the other.

I have been able to ascertain, in my data, how in the process of clan formation an ancestor who in fact only lived a few generations back and who could be plausibly placed in a particular position in the ortholineage, was yet relegated to a distant mythical past.

A RECENT HISTORIC ANCESTOR RELEGATED TO THE MYTHICAL PAST: THE EXAMPLE OF THE ^cAŪANIYYA

A striking example is the following, derived from ortholineage 14, whose members inhabit the western part of the village of Hamraya (neighbourhood of ^cAuaniyya). Hartong (1968: 38, 64)

writes on this grouping under the name of Aounia; his reconstruction does not quite coincide with mine.

The genealogy of the oldest generations of this ortholineage is presented in Fig. 4:

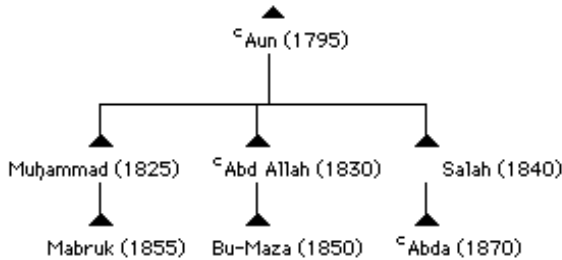


Fig. 6.2. Excerpt from the genealogy of ortholineage 14 (the dates indicate estimated year of birth)

This reconstruction is likely to be reliable since in 1968, at the time of the research, two sons of Mabruk were still alive, and one grandson of Bu-Maza, who were consulted for genealogical information.

In the first half of the nineteenth century this group lived in the vicinity of ortholineage 2 in the valley of Sīdī Mḥammad. Both groups affiliated to form the clan of the Mayziyya, after Bu-Maza. Around the French conquest (1881) the sons and grandsons of Aun left that place, and after wanderings within a radius of a few km they settled in their present place of residence in 1945-1955. Meanwhile the awareness was largely lost of the close agnatic ties between the descendants of Bu-Maza (born c. 1855) on the one hand, and those of Muḥammad and Salah (born 1825 and 1840 respectively). Possible the two branches of the ortholineages arrived at their present place of residence along a different route and through different interaction with third-party groups. The Bu-Maza group now lives in the northern part of Hamraya, higher up the slope, and is generally called Mayziyya; the Muḥammad/Salah group lives in the southern part, lower down the slope, and is generally called Auaniyya. Aun and Bu-Maza, the ancestors from whom these group names have been derived, and historically grandfather and grandson, are now considered as brothers, as distant agnates and even non-agnates: a beautiful example of the genealogical operation (d) as discussed on page 11.

With regard to Bu-Maza (born in 1855, and eponymical ancestor of the name of the Mayzi branch of ortholineage 14) the belief has arisen that he and the mythical ancestor of the Mayziyya clan were one and the same person; even though between these two namesakes a time distance of at least a century must have existed. This belief is found not only among younger informants in Hamraya, but also along some members of ortholineages 1 and 2 who reckon themselves to belong to the Mayzi clan and who now live in the villages of Sīdī Mḥammad and Mayziyya. Because of the confusion between two ancestors with the name of Bu-Maza, the latter informants consider the inhabitants of northwest Hamraya as fellow clansmen, within implying the present Auaniyya (the Mḥammad/Salah branch) in this rapprochement.

The equation of the thoroughly historical Bu-Maza (ortholineage 14) with the distant mythical ancestor becomes apparent from interviews of the following type:

An informant produces for a certain, identified person A (outside ortholineage 14) a chain of descent which, after the typical hesitation, ends in Bu-Maza; this Bu-Maza then turns out to be the mythical ancestor, someone who lived 'a long, long time ago' (*bikiri, bikiri yássir*), and who is claimed to be the ancestor of the present-day members of ortholineage 1 and 2. Then the same informant produces a chain of descent for another person B, who is an identifiable member of ortholineage 14 and notably a descendant of the historical Bu-Maza (born c. 1855). Also the chain for B, be it historically correct or not, is made to end on Bu-Maza as apical ancestor, and the informant declares that this is the same Bu-Maza as the apical ancestor of A. This is convincing proof that in the consciousness of the informant the historical Bu-Maza has been completely absorbed by the mythical ancestor. The extent of manipulation is even more impressive once we realise that in 1968 one grandson of the historical Bu-Maza was still alive!

Incidentally we note that in Ĥumiriyya the use of a limited body of personal names creates one of the conditions for successful genealogical manipulation (*cf.* Peters 1960: 32*f.*).

According to this model the spatial aspect of the distribution of mythical ancestors is nothing but an epiphenomenon of the fact that clan formation occurs between pseudolineage cores who dwell in each other's immediate vicinity.

In order to constitute, in a particular area, a lasting lineage name to which other pseudolineage will affiliate, it is necessary to have a consensual lineage core which has relatively many members and which has played a significant local role for a considerable number of decades, at least.

6.4.3. Model 2

All group names on -iyya derived from (mythical) ancestors are also being used as toponyms, in order to denote parts of villages, entire villages, and clusters of adjacent villages and the territory in between. The use of toponyms I shall discuss in chapter 5.

Starting from the initial stage as described above the process of clan formation may now also be described as follows. Because over a certain period of time (at least several decades) a certain territory has been the place of residence of a particular consensual pseudolineage core, the iyya-suffixed name of that group and the personal name contained in it attach to that territory as a toponym. Later immigrants into the same territory will adopt that toponym as a group name (phase a). In order to justify their presence and to assert their rights to local land in terms of the indigenous societal ideology, the members of the immigrant group do not stop at the adoption of the toponym but incorporate the personal name implied in it at the top of their genealogy. And in order to be able to situate that 'charter' in truly mythical times (such as stipulated by the ideology), the toponymical 'ancestor' becomes a mythical ancestor, *i.e.* one with regard it is no longer meaningful to trace uninterrupted chains of descents consisting of historical F/S links, at the pseudolineage level. The result is a clan, in whose emergence the spatial moment was not accidental, but crucial.

6.4.4. Discussion of the two models

Specific cases of historical clan formation in and around the research area can be satisfactorily explained in terms of either model 1 or model 2. The spread of the ʿArfawi clan to the southeast of the area (in the valley of al-Millah; *cf.* notes 11, 12, 13, 14, and

Hartong 1968: 34 f.) best fits model 1, because the ʿArfawi core arrived in these parts only by c. 1880, i.e. not prior to, but simultaneously with and in some case later than the arrival of other groups in that area which have subsequently affiliated to the ʿArfawīyya. Alternatively, the affiliation processes involving the ʿArfawi clan to the southwest of the research area (Chapter 4 example 1), those involving the Mayzi clan in the centre (cf. note 16), and those involving the Zaghaydi clan to the north and east of the research area (notes 2 and p. 56) rather fit model 2: the names of the mythical ancestors of ʿArfa, Bu-Maza and Zaghudud has been associated with these territories since as far back as the beginning of the nineteenth century, and these names have since been adopted by the pseudolineage cores which immigrated into these territories at a later date.

It is noteworthy that the actors themselves turn out to use the spatial distribution of clans as a guiding principle. This is for instance clear from frequent utterances such as

‘(The) Mayziyya is/are in between (the) Zaghaydiyya and (the) ʿArfawīyya.’

The Arabic formula contains the ambiguity between kin group (‘the Mayziyya are... etc’) and spatial denotation (‘Mayziyya is’). Initially I interpreted this usage as a somewhat cryptic formulation for the unstable politico-social condition in which the Mayzi clan has found itself for the latest few decades.

6.4.5. Ambiguity between kin-based and spatiality-based designation of a social group: The example of the Mayziyya

Because of a number of causes (uneven numerical increase; differential access to positions of economic and political power; normalisation of the clan system by the introduction of identity cards) the twentieth century has seen a moiety-type dichotomy of the clan structure in the research area and immediate surroundings. In this new development only the ʿArfawīyya and the Zaghaydiyya are almost consensually recognised as independent clans, and the other clans tend to be counted as belonging to either of the former – their apical ancestor is either replaced by ʿArfa or Zaghudud, or he is made into a son of these apical ancestors of two principal clans. Thus the other clans, including Mayziyya, have begun to lose their independent existence vis-à-vis the dual structure of ʿArfawīyya and Zaghaydiyya.

Most probably the original pseudolineage core of the Mayzi clan corresponds with ortholineage 2. I believe that two processes can be discerned in the present-day Mayzi clan:

- The pseudolineage cores which affiliated to the Mayzi clan since the beginning of the nineteenth century (foremost ortholineages 14 and 1) now tend to claim Zaghudud instead of Bu-Maza as mythical ancestor; as a result the Mayzi clan now threatens to shrink down only to encompass its original pseudolineage core (ortholineage 2).
- Even the members of ortholineage 2 now begin to consider Bu-Maza as a descendant of Zaghudud; as a result their pseudolineage core (although it can probably boast a longer local residence than that of Zaghudud) is now becoming one of the pseudolineages within the Zaghaydi clan.

It is only when I projected the attributions of individuals to particular clans onto a map (appendix 5), that I realised how simply and correctly such a statement describes reality, if that statement is interpreted in terms of territory. Moreover several informants pointed out to me, spontaneously, where in the landscape the boundary was between the two major local clans of the ʿArfawiyya and the Zaghaydiyya.

In most affiliation processes of consensual pseudolineage cores the two models 1 and 2 are complementary. The occurrence of phase (a) (the adoption of the group name) without phase (b) (the inclusion of the name of the mythical ancestor in the genealogy) can be explained according to either model. Likewise, both models can explain how and why an immigrant group adopts a group name whereas the pseudo-lineage core from which that name originally derived, had already left the area or was already extinct at the time of immigration and name adoption. According to model 1 such a 'posthumous' group name adoption takes place via another group which affiliated earlier than the immigrant group in question, whereas according to model 2 the transfer is made through a toponym. An example of such a process is to be found in Chapter 4 example 1.

6.4.6. Ḥumirī clans as an intermediate form between kinship-based and spatiality-based classification

Apparently clans in Ḥumiriyya form an intermediate form between kinship-based and spatiality-based classification: a clan is associated with a particular area (not absolutely, but statistically); but this association is expressed in terms of mythical ancestors; ideologically a clan consists of agnates, but the recruitment of this set of alleged 'agnates' takes place not on a kinship basis but on a spatial basis.

Accordingly we may formulate the place of clans in the social organisation of Ḥumirī from both a spatial and a kinship point of view.

From the point of view of the principle of spatial classification (see chapters 5 and 6) the clan is a means to interpret the facts of the present in such a way that they match the indigenous societal ideology. Whoever live together in the same valley, maintaining marital contacts and economic relationships, staging festivals together and sharing cemeteries, must have the same ancestor – if not on the lineage level, than at least at the clan level. Even if too large a knowledge on the recent residential history and the genealogies of local groups prevent, for the time being, a successful affiliation at the lineage level, the claiming of a joined clan name/toponym is hardly ever a problem. The actual spatial integration on the basis of existing interactions in the present is thus, in line with the ideology, translated into a fictive kinship integration bases on alleged descent from a shared mythical ancestor.

From the principle of kinship-based classification however, the clan offers the opportunity to surmount the very great diversity which exists at the lineage level. It is much easier for two consensual pseudolineage cores to affiliate at the clan level than at the lineage level.

There is another aspect to this. Clans have a spatial basis, their names are toponyms. In general we can say that the entire Ḥumirī landscape is rather clearly parcelled up in terms of clan names / toponyms / mythical ancestors. A toponym, clan name and

mythical ancestor remain associated with the landscape, long after the consensual lineage core from which this name derives, has become extinct or has emigrated away. This state of affairs results in a pattern of more or less localised clans in the area, a rather static, formal structuring of the landscape not only in terms of toponyms but also of social groups. The model of this formal structure ('which toponyms / clan names / mythical ancestors belong to which part of the local landscape') is an explicit part of the actors's consciousness. Of course we should not assume that this structure remains totally unaffected by the factual residential history of ortholineages: if it were, it would be impossible for new clan names/ toponyms to emerge, whereas most of the present ones did emerge no longer than one or two centuries ago. However, the time span over which this toponymical clan structure survives does appear to be much longer than the average time span (from immigration or emergence as an autonomous group, to emigration or extinction) of ortholineage branches.

In this respect the clans do constitute a permanent means of identification in the continuous circulation of ortholineage branches.

6.4.7. Interim summary

In this chapter I demonstrated the occurrence of clans (Murdock 1965) in Ḥumiriyya. It turned out that the attribution of individuals and sets of close agnates to particular clans was highly inconsistent. On the other hand we found an unmistakable association between someone's place of residence, and the clan to which that person would be reckoned to belong.

The Ḥumiri clan turns out to be an intermediate form between kinship-based and spatial group classification. The form a means of identification wherever the actors's historical knowledge is still to large to proceed to affiliation at the lineage level. Because of their anchorage in the territory, as toponyms, clan constitute a stabilising element in the social organisation: kin groups come and go, but the pattern of clans (within which these kin groups are subsumed) has a rather great persistence. The emergence, and persistence, of clans takes place both through affiliation of interacting kin groups (one of which bears the future clan name), and through the adoption of a clan name which is associated with a certain territory as a toponym; in the latter case it is not necessary that genealogical affiliation occurs between interacting kin groups.

In Ḥumiriyya clans constitute a form of group classification which, like the indigenous kinship-based classifications, is being used in a non-consensual, opportunist fashion. Whatever (limited) consensus and predictability exists in the actors's attributions of specific individuals to specific clans, can largely be described in terms of spatial integration. This shows the clan structure to be largely derived from the spatial functioning of Ḥumiri society. For thus reason the clan structure, as a highly defective indigenous classification system, cannot be of great significance for day-to-day interaction.

This is why in the next chapters we shall be occupied with a form of indigenous group classification which is more fertile for the description and analysis of day-to-day interaction in Ḥumiriyya: group classification based on spatiality.

Finally: in addition to kinship-based, spatial and clan classifications there are rem-

nants of tribal classifications in Ḥumiriyya. At present (1968) they hardly seem to play a role any more. However, further research is desirable on this point.

6.5. The inconsistency of Ḥumiri clans

The inconsistency of clans must be ascertained by a method which clearly distinguishes between clans and pseudolineages: for the latter are in themselves inconsistent in membership, so one cannot be sure whether a certain effect must be attributed to the inconsistency of clans, of pseudolineages, or both.

Data on clan membership are of the following nature: an informant A was asked to name the mythical ancestor the clan (whose name invariably derives from a mythical ancestor) of a certain, identified contemporary or past person B. Sometimes A and B would coincide: when the informant identified his or her own clan or mythical ancestor. These data were collected for a considerable proportion of the present-day heads of household in the research area, as well as for a great many persons in the near and distant past. A few people were scored more than once, usually by more than one informants, occasionally by one and the same informant at different moments of time.

According to the indigenous ideology one belongs to a clan because of agnatic descent from the mythical ancestor who is considered to have founded that clan. Therefore agnates of necessity belong to the same clan.

If the attribution of clan affiliation were consistent, and were to follow analytical principles, one would expect the following situation:

- (1) Randomly chosen informants a_1, a_2, a_3 etc. invariably reckon the same person B to the same clan C. This hypothesis offers the simplest way to test clan consistency. However, the number of multiple scores available for the same informant is too limited to allow for statistical testing.
- (2) Since close agnates are outside the limits of manipulation (p. 17), persons b_1, b_2, b_3 etc. who are undoubtedly close agnates would be reckoned to the same clan C by randomly chosen informants a_1, a_2, a_3 etc.

This second hypothesis lends itself to statistical testing with the data available. Out of the total set of persons for whom we have *one or more* clan scores we have to construct clusters of close agnates. Close agnates are to be defined as in appendix 1: kinship chain length between 0 and 3, lineage alienation 0. The clusters were derived from the genealogies of the reconstructed ortholineages. A number of persons cannot be included in a cluster with other close agnates because the latter are not available in the data set. I did not include in the clusters persons who died more than 100 years ago, according to the estimated dates used throughout my analysis. Each cluster was thus constructed that it exclusively contained people who according to all possible combinations are close agnates. This sometimes necessitated splitting up a cluster in two overlapping ones; for if b_1 and b_2 are each other's close agnates, and so are b_2 and b_3 , b_1 and b_3 will be agnates, but not necessarily close ones, as the following Fig. demonstrates:

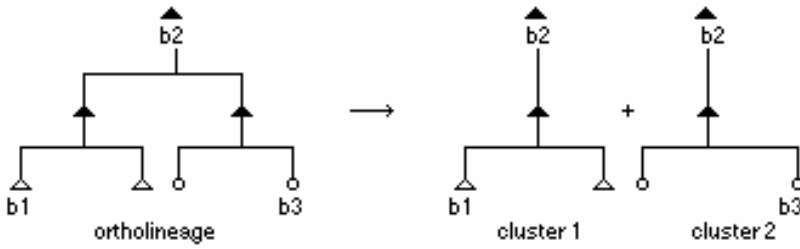


Fig. 6.3. Two clusters of close agnates.

Also those cases where one person B had been scored by several informants were relevant to the problem at hand; the various multiple scores were taken to form one cluster, with or without the additional scores for B's agnates.

This procedure yielded 21 clusters of persons who in every respect were each other's close agnates. Each cluster comprised 1 to 4 persons; in total they comprised 49 persons for whom one or more clan attributions were available.

For each cluster the clan attributions were assessed. As a measure of the degree of clan inconsistency I chose:

$$I_a = 1 - \frac{p_a}{q_a} \dots \dots \dots (6.1)$$

where

- I_a = inconsistency of clan attribution in cluster a
- a = number of cluster
- p_a = frequency of most frequent attribution in cluster a
- q_a = total number of attributions in cluster a.

Note that $0 \leq I_a < 1$

In those cases where a certain mythical ancestor was presented as a son of some other mythical ancestor, the score was divided between both attributions.

Table 24 presents the results of these procedures. The ortholineage numbers correspond with appendix 6. Note that the cluster numbers do absolutely not correspond with the numbers of agnatic groupings in table 16. In the present analysis the approach is very different from that in table 16. In particular, persons who lived at the end of the nineteenth century can be combined in one cluster in table 24, whereas according to table 16 they belong to different agnatic groupings within the same ortholineage; on the other hand some agnatic groupings as found in table 16 are not represented among the clusters of table 24, simply because there were not enough clan scores available for these agnatic groupings.

identif. number of cluster	identific. number of ortho-lineage	number of persons in cluster	number of scores per cluster	distribution of the scores per mythical ancestor						I _a
				Zaghdud	Bu-Maza	Arfa/Arif	Bu-Tara	Hadir	other	
1	1	5	8	3	5					0.38
2	1	3	4	2.5	1.5					0.38
3	1	4	7	1	6					0.14
4	3	3	3			2			1	0.33
5	4	2	2	2						0
6	4	2	2	1.5			0.5			0.25
7	5	3	4	1	3					0.25
8	5	3	3		3					0
9	5	4	5	3	2					0.40
10	5	2	4		4					0
11	6	2	2	2						0
12	6	4	4	4						0
13	6	2	2	1	1					0.50
14	6	1	2	2						0
15	7	4	6			6				0
16	8	2	2			2				0
17	8	2	2	2						0
18	8	2	2			2				0
19	8	2	3			3				0
20	9	1	2			1.5			0.5 ⁴⁷	0.25
21	26 ⁴⁸	2	2					1	1	0.50

Table 6.1. The inconsistency of Ḥumirī clans.

Of the 21 cluster as many of 10 displayed inconsistency ($I_a > 0$). This suffices to demonstrate the overall inconsistency of Ḥumirī clans.

Another illustration of the inconsistency of Ḥumirī clans is to be found in the right-hand column of table 25 (appendix 6). In chapters 3 and 4 we have derived some general patterns of genealogical manipulation; in combination with the reconstructions of the residential history of the area they should enable us to explain, for the majority of clusters, why they do or do not display clan inconsistency. However such a time-consuming and tedious exercise would not truly enhance our insight.

⁴⁷ Informant is not clear as to whether the mythical ancestor is ^cArfa, or somebody else.

⁴⁸ Members of this cluster now (1968) live in Tra'aya-sut; in the research area this ortholineage does not occur.



In the intermediate background to the left the sacred *Raquba* (Hill) of Sidi Mḥammad Sr; from this angle the domed shrine itself cannot be made out

Fig. 6.4. The research assistant Mr. Hasnawi bin Ṭahar, about to traverse a field of rye and the *shabba* (gully) of ʿAin Fallus towards the centre of the village of Sidi Mḥammad, 1968

6.6. Present-day actors' place of residence and the mythical ancestors with which they are associated

I have already argued the inconsistency of Ḥumiri clans from a kinship point of view: close agnates are frequently associated with different clans or mythical ancestors. Now I intend to show the spatial factor in the attribution of mythical ancestors.

The data on this point come in two different types. On the one hand informants have explicitly stated the place where certain mythical ancestors dwelled. On the other hand the informants have stated the mythical ancestors of persons living in the present or the past, whose place of residence was known to me, thus implicitly tying a mythical ancestor to a place. For the present purpose both types of data are interchangeable: both project the names of ancestors onto the local landscape. In total we have, for the research area and surroundings, 101 statements available of both types combined.

I divided the map of the research area and surroundings in 25 squares of 1 x 1 km each. Every place of residence in the data set belonged to one of these squares. Each time a mythical ancestor was stated, explicitly or implicitly, to be associated with a certain

place in one of the squares, that square received one unit score. The combined scores are shown in Fig. 6.19.

The situation of squares in Fig. 6.19 is identical to that in Figs 20 and 21. In the latter two, mountain ranges and rivers are indicated for orientation, so that the squares can be located on the map. In the present analysis I shall limit myself to three mythical ancestors: Zaghdud (Zaghaydi clan), Bu-Maza (Mayzi clan) and ʿArfa or ʿArif (ʿArfawi clan). 88% of the 101 scores in the data set concerned either of these three ancestors; the remainder is combined under the heading ‘other’. In a case where two mythical ancestors were presented as father and son, I divided the score between them. In each square of Fig. 6.19 the centre shows the total number of score for that square, then in clockwise fashion the score for ʿArfa/ʿArif (12 o’clock), Bu-Maza (3 o’clock), Zaghdud (6 o’clock) and other (9 o’clock).

In the next Fig. , 20, shading per square indicates whether that square had scores for any of the three named mythical ancestors ʿArfa/ʿArif, Zaghdud and Bu-Maza. For each ancestor the square in which he occurs turn out to form reasonably contiguous areas.

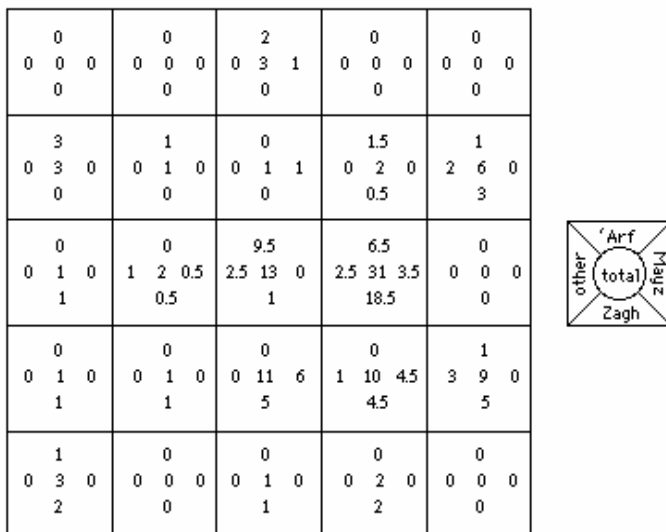


Fig. 6.5. The correspondence between place of residence and mythical ancestors: (a) number of scores per mythical ancestor per standardised area

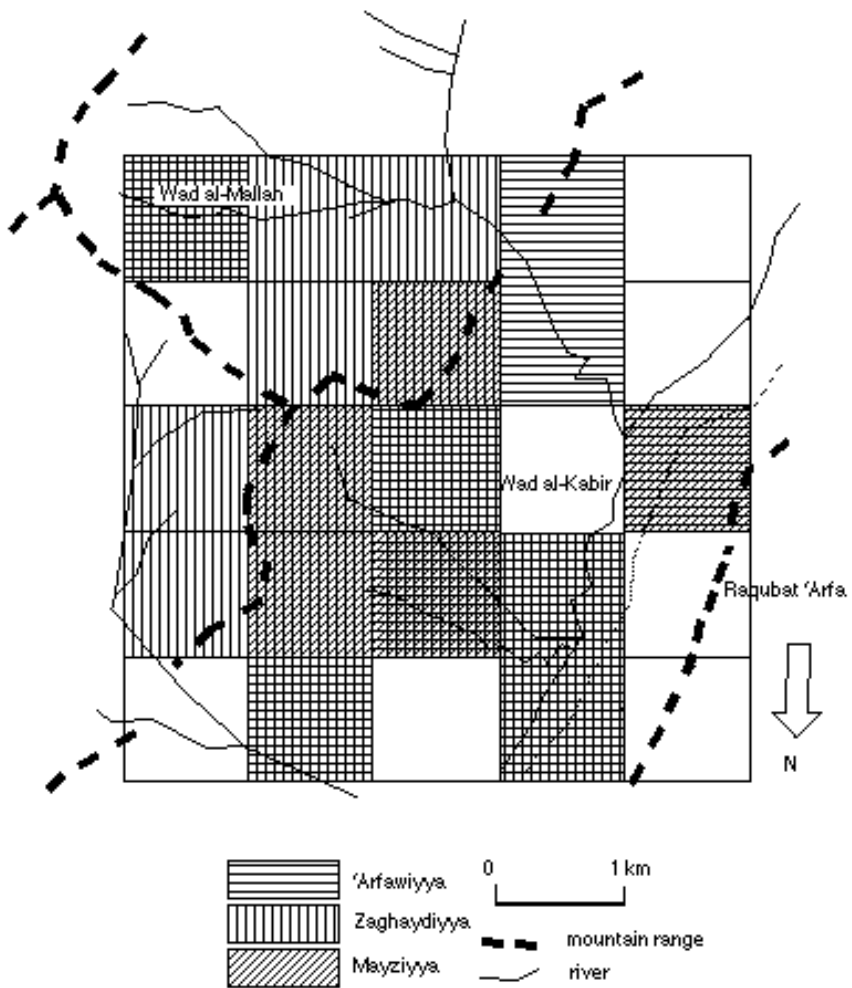
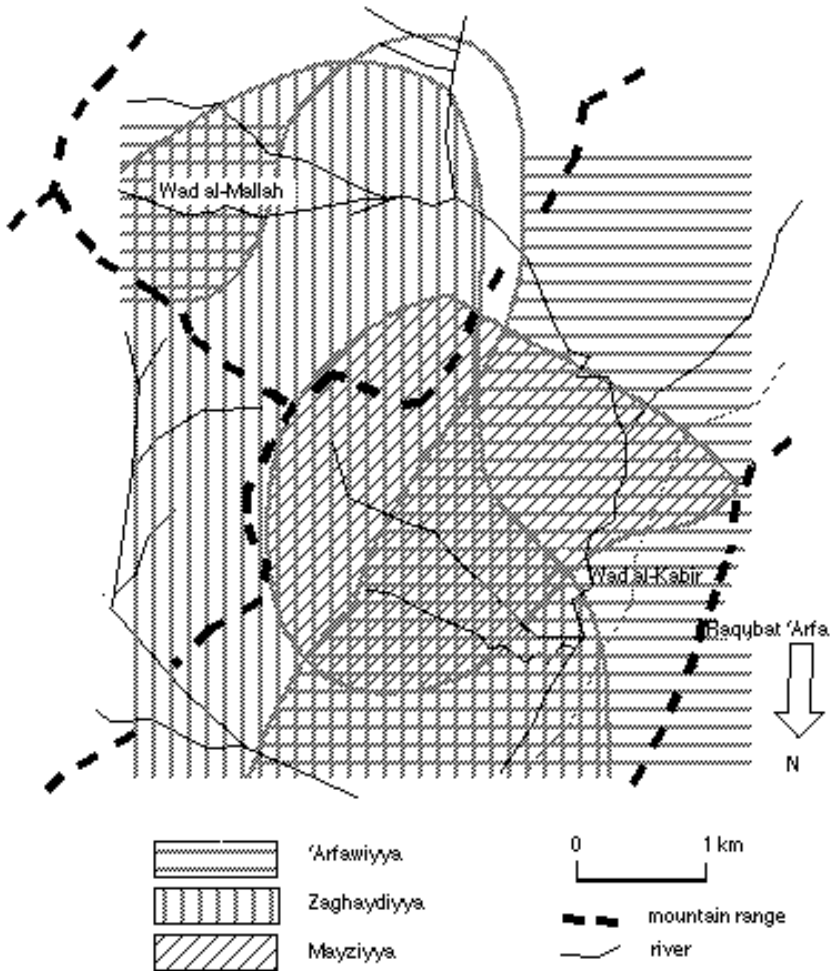


Fig. 6.6. The connexion between place of residence and mythical ancestors: (b): squares with scores for certain mythical ancestors have been shaded accordingly.

This finally leads to Fig. 6.4, where the contiguous areas of Fig. 6.3 have been enclosed by a smooth contour, compensating for gaps in the data.



Note the inverted North arrow; also note how in this map the Mayziyya are literally in between the two major clans indicated

Fig. 6.7. Connection between place of residence and mythical ancestors: (c) contours indicate which part of the landscape is associated with which mythical ancestor.

Each mythical ancestor turns out to be associated with a particular part of the landscape. Admittedly, these parts overlap, and the boundaries are fluid. The isolated 'Arfawi area in the bottom left-hand corner can be linked up with the main 'Arfawi area in the Fig.: the inhabitants of the villages which (outside the Fig.) connect both 'Arfawi areas are themselves also largely considered as 'Arfawiyya. The Fig. clearly shows the spatial position of the Mayzi clan as intermediate between the 'Arfawiyya ad

the Zaghaydiyya.

I consider these results to constitute a major argument for the clan nature of the Ḥumirī kinship groupings as based on mythical ancestors.

Chapter 7. Group classification based on spatiality

7.1. Introduction

In the preceding chapters we have described the Ḥumiri's social environment from the point of view of kinship-based groups and of clans. In the discussion of the pseudolineages and especially of the clans the importance of the spatial principle in Ḥumiriyya already became manifest. In the present chapter we shall deal with this principle. We shall focus our attention on the manner in which in Ḥumiriyya people can be classified on the exclusive basis of their distribution over the landscape, regardless of kinship and even of residential history.

In this chapter I shall develop a model for such spatial classification. In the first instance this will be merely an analytical model: a construction by the ethnographer. Next I shall assess the extent to which this analytical model does correspond with the indigenous group classification. Finally, the relevance of this model for day-to-day interaction will be assessed in chapter 7. There we shall also pay attention to the relation between this spatial model, and the indigenous societal ideology as described in chapter 2.

7.2. An analytical model

From a spatial point of view the social environment of a specific Ḥumiri Ego can be adequate described as follows:

Ego is a member of a number of *spatial units*. Such units exist at a number of *levels* in such a way that each units at a higher level comprises one or more units of the level immediate below. Units at the same level are usually clearly distinguished from each other by a *proper name* and by *boundaries* which tend to be visible in the landscape. Moreover at each level a type of *characteristic attribute* (e.g. a dwelling house, a threshing floor, a shrine) exists which is typical for units at that level; each spatial unit at that level has its own specific specimen of that

characteristic attribute.

The smallest spatial unit to which Ego belongs is the household, whose characteristic attribute is the dwelling house (with its own access from outside). Jonker (n.d.: section 3.2) argues that, from an economic point of view, the storage table (*rahāl*) is the characteristic attribute of the household. The household is named after the head of household, which is usually its oldest male member.

Ideally the household is part of the *family compound*: a small group of households whose heads are close agnates, and occasionally including the spouses and widows of close agnates. The spatial boundaries of the household coincide with the outer walls of the dwelling house. The compound is separated from other compounds by cactus hedges, a path, or a rivulet, as the case may be. The family compound is usually named after the founder, who may be still alive or already deceased. Because households are subject to a continuous process of spatial dispersion (cf. Bos 1969; Beeker 1967) the family compounds usually have a time span of only a few years. On the other hand nearest neighbours are preferably being recruited among close agnates but far from exclusively so, as we shall see in the next few chapters. As a result most households belong to small clusters comprising two to six houses, whose heads of household were not exclusively recruited on the basis of agnatic kinship, and which therefore are not family compounds in the strict agnatic sense as stipulated by the societal ideology. Yet in the spatial perspective such agnatically-heterogeneous clusters are compounds in their own right. The situational nature of agnatic identification means that such a heterogeneous set of nearest neighbours is soon regarded by the actors as a yet descending from a common ancestor. If this affiliation process is not yet completed because the heterogeneity falls outside the limits of manipulation, the compound is denoted by the name of the dominant agnatic cluster and its founder, by the name of a mythical ancestor and the associated toponym, or (rarely) by an agnatically neutral toponym derived from a feature of the landscape. The characteristic attribute of the compound in the shared threshing-floor (qa^ʿa).

The spatial unit which in size follows the compound is the neighbourhood: a set of 5 to twenty neighbouring households, most of which are clustering in distinct compounds. The neighbourhood is separated from other neighbourhoods by a rivulet, a path, and cactus hedges. The nomenclature of neighbourhoods is similar to that of compounds: neighbourhoods are named after their dominant pseudolineage, after a mythical ancestor and the associated toponym, or by a feature in the landscape which does not of kinship connotations: Qa^ʿa Raml ('the Threshing-floor of Raml – i.e. of the Sands'), ʿAin al-Hamra ('The Red Spring'). The characteristic attributes of neighbourhoods are: a spring (adjacent to which is a women's assembly place), and occasionally, as the neighbourhood expands, a men's assembly place. The neighbourhood as an analytical category for the description of Ḥumiri society is also used by Beeker (1960) and Schulte Nordholt (n.d.). Schulte Nordholt (n.d.: 12) also presents the neighbourhood as an indigenous category: the 'jirane' (*jirān*), characterised by its own spring.

A small number of neighbourhoods combine to form a village. The village is separated from other villages by uninhabited terrain: fields, pastures and especially forested slopes. The characteristic attribute of the village is the men's assembly place, which often coincides with the village store. Village nomenclature is similar to that of the

neighbourhood.

A number of villages together constitute valley, which through mountain ranges and rivers is separated from other valleys. Several valleys together form a chiefdom, headed by an administrator, the chief, who is appointed and paid by government. Chiefdoms are integrated in larger geographical and administrative units within the national state of Tunisia, but this wider political organisation is outside our present scope.

Somewhat artificially in this discussion I have refrained from referring to two crucial types of characteristic attributes: shrines and cemeteries. As a result no characteristic attributes could be offered for valley and chiefdom. In the later parts of this book I will discuss the significance of these characteristic attributes for spatial classification at great length. Here I shall limit myself to two remarks: shrines and cemeteries can be attached as characteristic attributes to spatial units at all levels (from household to chiefdom); and shrines and cemeteries play a major role in the nomenclature of higher-level spatial segments from the neighbourhood level upwards.

7.3. Analytical model and indigenous model of group classification on the basis of spatiality

It is a common phenomenon that the analytical categories in terms of which an ethnographer can describe a society adequately, far from always have equivalents in the indigenous categories which the actors use. This leads to the following question: does the above structure of spatial classification also exist at the level of the actors's conscious representations?

We can answer this question by assessing whether the actors have at their disposal terms to describe similar spatial units as outlined above, and whether these indigenous terms are sufficiently well-defined to allow for the distinction between the various hierarchical levels.

At first view the answer to our question appears to be negative. The most frequently used terms for social units in Ḥumiriyya are *dār*, *duār* and *firqa*. The table below summarises the meanings of these Arabic terms (*i.e.* their analytical equivalents), as ascertained by me on the basis of an analysis of an abundance of cases.

term	kinship aspect	spatial aspect
<i>dār</i>	a household (nuclear or extended family); a part of a pseudolineage	a household; a family compound
<i>duār</i>	a part of a pseudolineage; a pseudolineage; a clan; occasionally: a tribal unit	a compound (including a family compound; a neighbourhood; a village; a set of neighbouring villages; a valley; a chiefdom; a tribal unit in parts of more than one chiefdom)
<i>firqa</i>	a part of a pseudolineage; a pseudolineage; a clan; occasionally: a tribal unit	a compound (including a family compound; a neighbourhood; a village; a set of neighbouring villages; a valley; a chiefdom; a tribal unit extending over (parts of) more than one chiefdom)

Table 7.1. The principal indigenous terms to denote social groups in Ḥumiriyya, and their analytical equivalent

For the time being the principal conclusion from table 1 would be that the general terms for social groups in Ḥumiriyya have a very limited precision with regard to the size of the set of people they refer to. For instance the word *duar* can mean everything between a family compound of two households (some ten people), and a chiefdom of many hundreds of households and thousands of people. Other conclusions from this table will be discussed in chapter 7.

Now it is unlikely that the members of a society would be satisfied with such an apparently defective terminological apparatus for such an important purpose as the identification of social units. It is plausible that the addition of proper names (of persons who are members of these social units, or of the social units themselves) works towards greater precision. But although such an addition is usual in Ḥumiriyya, so that the indigenous terms listed in table 1 are in fact seldom used without being followed by a proper name, this does not automatically enhance their precision.

For instance, if one speaks of

‘Ḥamis u’l *duarhu*’ (‘Ḥamis and his *duar*’),

than from the form of this utterance it is impossible to ascertain whether the speaker refers to the little compound where Ḥamis lives, or to the entire valley or even chiefdom within which that compound finds itself. A similar phenomenon occurs in the case of toponyms. In the utterance

‘Ḥamis *sakin fi Sidi Mḥammad*’ (‘Ḥamis lives in Sidi Mḥammad’)

the spatial definition ranges from a very small precision (somewhere in the valley of Sidi Mḥammad which comprises five villages: an area of c. 12 km²), via the one village called Sidi Mḥammad (c. 1 km²), to the fifty times greater precision of the neighbourhood of Sidi Mḥammad (c. 0.25 km²): the neighbourhood which, within the village of the same name, is adjacent to two particular shrines of the saint Sidi Mḥammad from which valley, village and neighbourhood alike derive their names.

However, on closer analysis the actors’s denotations have in practice just the right degree of precision.

So far we have looked at isolated utterances. But since long linguists have realised that the meaning of an utterance can only be ascertained in the light of its proper, situational context. In Lyons’s words:

‘When we use language to communicate with one another, we (...) produce (...) utterances; those utterances are produced in particular contexts and cannot be understood (...) without a knowledge of the relevant contextual features. Furthermore, in the course of a conversation (...) the context is constantly developing, in the sense that it “takes into itself” from what is said and what is happening all that is relevant to the production and understanding of further utterances’ (Lyons 1969: 419).

Well, all utterances by means of which Ḥumiri actors, in concrete situations, denote spatial units, have the following contextual aspect in common: always it is known how large is the structural distance, in terms of the hierarchically inclusive levels of spatial units, between the speaker and the members of the spatial unit to be denoted. This datum defines, from case to case, the level of the spatial unit which the speaker seeks to denote, and hence the numerical and spatial size of the unit as denoted – in other

words the precision of the denotation. For this spatial unit is: the first in hierarchy below the smallest unit which the speaker shares with the people to be denoted (see Fig. 7.1).

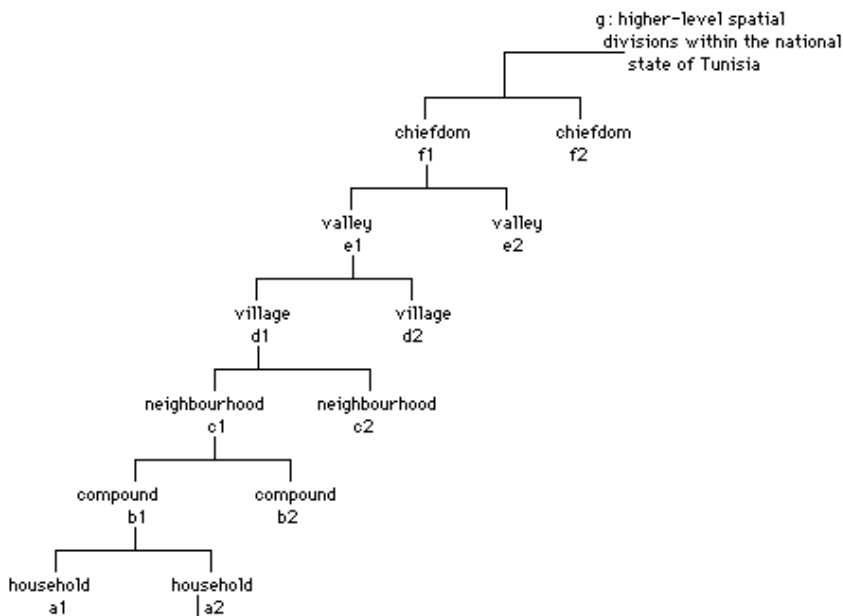


Fig. 7.1. The hierarchy of spatial units in Hmiriyya.

Denotation always takes place at the highest level at which the social position of the speaker vis-à-vis the referent can still be formulated in terms of an opposition of spatial units; for all higher units are shared by the speaker and the referent – there no segmentary opposition exists, but segmentary integration.

For such spatial denotations as ‘Hamis u’l duarhu’ or ‘Hamis sakin fi Sidi Mhammad’, it is never the entire range of possibilities, from extreme vagueness to considerable precision, which is relevant in the same situation. With regard to referents who are close to the speaker within the spatial structure, a certain denotation is sharply focused; with regard to referents at a great structural distance, the denotation is vague. People dwelling in the same chiefdom but in a different valley from the speaker are not denoted precisely, in terms of their compound or neighbourhood, for only by their valley; such is sufficient. Referents dwelling in a different chiefdom, even if their place of residence is in an adjacent valley and possible at a smaller distance than the some people belonging to the same chiefdom as the speaker, are yet denoted by the name of their chiefdom, and not by that of their valley, village or neighbourhood. Incidentally

speakers may deviate from this pattern, but most of the thousands of spatial denotations with which I was confronted in the course of my field-work followed it.

The use of clan names / toponyms does not constitute an exceptions to this pattern. They, too, are being used with greater or lesser precision, depending on the relative positions of speaker and referent within the structure of spatial segmentation. Above we discussed the stabilising effect of clan names / toponyms which are tied to the landscape. Here we must qualify our earlier statements on this point, arguing that this effect can only be relevant among the set of people who, because of a similar position within the structure of spatial segmentation, more or less share the same view of the local land. This set is seldom more comprehensive than the valley; it might be extended to exist on the chiefdom level, but then only with regard to the valleys that constitute the border zones between chiefdoms.

We may conclude that the system of spatial classification as used by the actors, does indeed correspond with the analytical model as sketched in the beginning of this chapter. The nature of the indigenous generic terms for spatial units, which at first seemed to militate against such a conclusion, now turn out to constitute its major corroboration, once we have seen how these terms function in practice.

In the following chapters it will become clear why this indigenous system of spatial denotations offers just the right degree of precision. Let us slightly anticipate these results. Precision of spatial denotation is all the more necessary, the larger the probability of interaction between speaker and referent, or between listener and referent. Well, such probability is largely governed by the structural distance between the persons involved within the spatial structure. Precision of denotation is meaningful, and can be realised within the indigenous system, at the level of the compound, the neighbourhood and the village: it is within the confines of these spatial units that the interaction of actors of Ĥumiri society largely takes place. Precision is meaningless, and is not realised either, with regard to people from a different chiefdom: interaction with such people are relatively so sporadic that they can be said to find themselves at the periphery of the social environment of the speaker and the listener.

In addition to the indigenous spatial denotation there are a number of other reasons for the contention that the analytical model of spatial segmentation as sketched above is also being used by the actors themselves. We shall now turn to these reasons.

The actors have transformed the natural landscape: the original forested slopes have taken their present appearance through human settlement, clearing, artificial systems of irrigation, paths. And in the years preceding the main fieldwork (1968), of course, through reforestation, which however is a state initiative outside the scope of local social organisation, even though through the unemployment relief organisation the labour required is largely supplied by the local villagers. There has to be some reciprocal relationship between the observable results of these transformation (the human ecology), and the indigenous conception of spatial structure. Now when the spatial units can be so clearly perceived in the landscape, and are so unmistakably distinguished from other units at the same structural level through observable features in the landscape (cactus hedges, rivulets, paths, fallow land), then the conclusion is inescapable that the model of this visible structure must be present in the actors's

conceptualisation of their environment.

Moreover the spatial units invariably have an observable characteristic attribute: the dwelling house, the threshing-floor, the spring, the men's assembly place, the shrine and the cemetery. Also this constant in Ĥumiri ecology can only be explained if we assume that the actors do use the system of spatial classification as outlined above, in analytical terms.

The significance of the characteristic attribute for a spatial unit can be described in the following terms.

In the first place the characteristic attribute is a locus of contraction of the social activities of the members of the spatial group. The activities which relate to the characteristic attribute are, in the same order as used above: family life (working, eating, sleeping, child-rearing etc.); agricultural activities; fetching water; visiting the men's assembly; visiting shrines and organising religious festivals; and burying the dead. The activities, as specified for each level, are the major (if not the only) collective activities in which nearly all members of a spatial unit at a certain level participate, and they are in addition the most characteristic and the most strongly sanctioned activities: taking part in them is the norm, and non-participation is surrounded by negative sanctions and is often downright impossible. Because of these activities the spatial units do constitute real social groups. Besides the activities which are directed at the characteristic attributes the spatial grouping (with the exception of the household and the compound) hardly knows any other forms of collective interaction. However, non-collective interaction between only two persons who are both members of the same spatial grouping at a certain segmentary level is an important structural feature of Ĥumiri society (dyadic relationships, see chapter XXX below).

As a static phenomenon, the spatial unit is invariably visible in the landscape. As a statistical datum, the spatial unit manifests itself in the tendency of its individual members to select each other, in pairs, as partners in non-collective dyadic interaction. And finally the spatial unit is a real social group, coming to the fore on the occasion of collective activities focusing on that unit's characteristic attribute. It is primarily in its collective activities that the total set of members of the spatial unit presents itself vis-à-vis the outside world. The collective activities as focused on the shared characteristic attribute are the principal factor in the integration of the spatial unit: both mentally (they reinforce the members's 'we'-feeling vis-à-vis other spatial units at the same level), and in terms of day-to-day interaction: not only the collective activities themselves, but also such other, non-collective dyadic activities as are likely to be initiated by these collective activities.

It is remarkable that the characteristic attributes at each level, and the collective activities directed on them, have always a religious connotation. The house, the threshing-floor, the spring, to a lesser extent the men's assembly place, and of course especially the shrines and the cemeteries which are always situated around shrines, are associated with invisible personal beings. Through his or her activities, man enters into relationships with these beings. They are supposed to influence man's life decisively. Family life, working on the threshing-floor, the maintaining of relationships with always the same spring, the interaction at the men's assembly, the pilgrimages to shrines, and interment: all these collective activities are supposed to have a salutary

influence on the human being who participates in them. They are considered to contribute to a person's health and to convey blessing. The distribution of these collective activities, or the transgression of the rules that apply to them, is supposed to have the opposite effect. Of course these aspects will be dealt with extensively in the parts of this book that deal with Ĥumiri popular religion.

Integration of the spatial group within itself implies opposition vis-à-vis the outside world. When the integration of the spatial group is so closely tied up with the visible characteristic attributes, it follows that the relative position of two spatial segments can be ascertained from the nature of the characteristic attributes which they share and those of which they have each their own individual specimen. Spatial units which share the same men's assembly, are neighbourhoods within the same village, integrated at the village level as is visible by use of the same men's assembly; the opposition between these segments is especially clear from the fact that each frequents a different spring. Spatial units which share the same spring but each have their own threshing-floor, are compounds within the same neighbourhood. In general, the characteristic attributes are the beacons in the spatial structure.

It is in the nature of beacons that one can shift them to a different position. The size (in terms of surface area and membership) and the geographical position of a spatial segment changes constantly, and hence its relationship to other segments at the same level, and its place in the overall hierarchical spatial structure in general.

Because of the dispersion of families of orientation (parental families; cf. Bos 1969; Beeker 1967; Jonker n.d.), changes continually occur at the level of the household and the compound, but in the course of a few decades such changes can have major repercussions on the spatial structure at the level of the neighbourhood and even the village.

Of course this process is being accompanied by a redistribution of characteristic attributes over the spatial units. At the bottom of the hierarchy this is manifested when a young adult male (married or not yet married) moves to a house of his own. One level up, heads of household within what was until then the same compound mark the end of their hitherto close co-operation in agriculture by the construction of a second threshing-floor, which is usually accompanied by a residential move of the users of that new floor. At the level of the neighbourhood fission occurs when part of the neighbourhood refuses another part access to a certain spring – or when part of the neighbourhood on its own account decides to frequent a different spring in future. At the village level fission is accentuated because a certain neighbourhood ceases to frequent the village's central men's assembly, and creates one of its own. At all levels, moreover, there is a progressive differentiation in terms of pilgrimages to shrines: people begin to frequent certain shrines, and drop their visits to other shrines, in reflection of changes in the spatial structure of their surroundings. Finally there is a constant shifts in the selection of cemeteries at all levels from the neighbourhood upwards.

The redistribution of characteristic attributes is the definitive, manifest conclusion of a process of disintegration of a certain spatial group, and the reintegration of other groups. Such a process can take years if not decades, and its decisive factors are demographic pressure, and shifts in economic and political power (cf. Bos 1969; Beeker

1967; Jonker n.d.; Hartong 1968). The redistribution of characteristic attributes creates a new status quo, with visible signs in the landscape and with visible collective activities which for the actors constitute cornerstones in their conceptualisation of current group relationships in their social environment. And of course, the specific distribution at a given moment of time is in itself only the point of departure for again further changes.

A discussion of significant processes of change of spatial units in the course of the last century, with concomitant redistribution of characteristic attributes, will be largely reserved for volume II, which exclusively deals with Ĥumirī religion: among these characteristic attributes, the shrines and cemeteries are best documented. The spatial changes in themselves are also discussed by Beeker (1967), Bos (1969) and especially Hartong (1968). Hartong's (1968: 46 f.) chapter 'Verzelfstanding van groepen' ('How groups grow to autonomy'), on the process through which new social groupings attain relative independence within the segmentary structure, contains a discussion of spatial changes which have in part inspired my approach centring on characteristic attributes.

Besides the visible characteristics as discussed above the actors impose again a different type of attributes on the spatial units: ancestors! It would seem to me that this is the best way to formulate the connexion between spatial structure and kinship-based group classifications: the spatial structure is primary, and the distribution of ancestors over the units within the spatial structure is secondary. Such a view is also in accordance with the ways in which spatial and kinship-based classifications are relevant for day-to-day interaction.

The ancestors which are distributed over the spatial units are being combined in a genealogical structure which reflects the relative positions of the units in the overall spatial structure. These relative positions becomes apparent, foremost, from the distribution of visible, characteristic attributes. According to the ideology all inhabitants of a particular valley descent from one apical, mythical ancestor. The founders of the various villages within the valley are then considered to be the sons or grandsons of the supreme, mythical ancestor. In the next descending generation of the fictive genealogy the founders of the various neighbourhoods appear, and a few generations below them the heads of family compounds and of the constituting households.

In practice this pattern is never elaborated in a totally consistent manner. Individual actors often have a genealogical formulation for the relationship between two specific spatial units, *e.g.*

'neighbourhood (= duār) A and neighbourhood B belong together, for their founders were brothers'.

Sometimes the expression is reduced to:

'neighbourhood (= duār) A and neighbourhood B are brothers'.

7.3.1.the genealogical formulation of relationships between spatial segments

#CASE 7.1. AN EXAMPLE OF THE GENEALOGICAL FORMULATION OF RELATIONSHIPS BETWEEN SPATIAL SEGMENTS. An example is the information given by an old man from ortholineage 14, who claims Zaghdud as his mythical ancestor. The same informant is presented, about the same topic, in Hartong 1968: 60 *f.* According to this informant Zaghdud had the following sons: ʿAun, Bu-Maza, Bu-Mandjil, ʿAbu ʿl-Qassim, Rashab, Bu-Tara and Zarruq. These were the ancestors of the spatial units (villages or neighbourhoods) of ʿAuanīyya, Mayziyya, Manadjliyya, Quassim, Rashaybiyya, Traʿaya and Zaraqqa. On Fig. we can see how these villages and neighbourhoods together form an almost contiguous area: that of the Zaghaydi clan. Incidentally, this area does not comprise one valley (as is claimed in the indigenous ideology), but three valleys around a central massive, the Djabāl ʿAin Falūs. With the exception of Zarruq and ʿAbu ʿl-Qassim (who were actual brothers, born c. 1820 as sons of Salah; *cf.* Fig., 18), the oral historical statement makes brothers out of a number of people from the past who most probably belonged to mutually irreducible ortholineages, with this proviso that some members of the list (Bu-Maza, Bu-Mandjil, Rashab, Bu-Tara) in the 1960s had already attained the status of mythical ancestors, but others had not. In some cases the ancestors listed could not even have been contemporaries (as is clear from the widely differing number of generations between these ancestors and the people who today are considered their descendants). Many other informants produced similar (but very far from identical) 'genealogies' of mythical ancestors and founders of spatial units.

Yet none of the actors manages to present a complete genealogical scheme comprising all spatial units at all levels in his or her social environment.

Apart from being invisible, ancestors have a lot in common with the visible, characteristic attributes of the spatial units: the house, the threshing-floor etc. Both ancestors and characteristic attributes are distributed over spatial segments. The distribution of visible characteristic attributes is not the cause or the explanation of the spatial structure, but its manifestation: an expression of the status quo, within that structure, at a particular moment in time. The same applies to the ancestors.

Yet when it comes to bringing out the Ḥumirī spatial structure, the distribution and redistribution of ancestors is a far blunter tool than the distribution and redistribution of characteristic attributes. To the extent to which ancestors are socially relevant, they only persist in the heads of individual actors. Contrary to the house, the threshing-floor etc. one cannot see ancestors in the landscape, and therefore the actors's thinking about the distribution of ancestors over such viable social groups as the spatial units constitute, can proliferate in all possible directions, without consensus and without, in most cases, any external check or social control. Actor's views, however, concerning the distribution of the visible characteristic attributes in their own social environment must have maximum consensus, for they are constantly checked by the unmistakable evidence of the visible characteristic attributes. In general collective activities tend to correct the divergent ideas that the participating individuals have with regard to the representations and material objects involved in these activities; for such activities require co-ordination of interaction, and hence a certain minimal consensus. Now in Ḥumirīyya there do exist collective activities focusing on the spatial segment's characteristic attributes, as we have seen above, but in general no collective or ritual activities are directed at deceased ancestors.

This statement holds true, provided we define an ancestor as : 'a human being whom

the actors consider to have lived in the past, and whom they link genealogically – either as mythical ancestor, or as ‘remembered’ member of a pseudolineage – to people living in the present’. For we must not overlook the fact that among the characteristic attributes of spatial units there are shrines, which are associated with human beings believed to have lived in the past (saints) and whom the actors generally consider as their ancestors (‘*djadudna*’, our ancestors). Only in very few out of the dozens of shrines involved in my research, did the past figure as associated with a shrine also feature (as mythical ancestor, or as son or client of a mythical ancestor) in the genealogical representations of the people now living in the vicinity of that shrine. One such case is also discussed by Demeerseman (1964: 154). In this handful of cases we can even say that the ancestor can be more or less (notably: in the form of his shrine) perceived in the landscape. Saints, *i.e.* representations concerning personal beings associated with a shrine, are the subject of religious activities; so in these very few cases one could maintain that ritual activities are directed at ancestors. However, nearly all saints as associated with *Ḥumiri* shrines are not ancestors in the strict analytical sense as defined above. And reversely: of the thousands past figures which feature in the genealogies which others and I collected in the research area (which forms only a very small portion of *Ḥumiriyya*), only two or three are associated with shrines.

Thus an important possible source of consensus in the actors’s views of ancestors and their distribution over social groups is lacking in *Ḥumiriyya*. Hence these views can continue to lack consensus and integration, and to lend themselves to the opportunist justification of dyadic relationship.

In the preceding chapters we have already seen to what this leads: not only a fundamental lack of consensus and a high degree of manipulation, but also, in the individual informants’s statements of genealogical relations and residential history, a constant process of redistribution (in the form of alteration of the claimed genealogical relationship between ancestors) of ancestors over social groups, mirroring the relative expansion or decline of these groups.

7.3.2. Intermediate summary

In this chapter so far I developed an analytical model: a hierarchical system of spatial classification, in which each spatial unit has a name and an attribute which is characteristic for the level of that unit. This analytical system turned out to correspond with the system as used by the actors themselves. This conclusion was reached after an assessment of the indigenous forms of spatial denotation, of the landscape as transformed by the actors, and of the redistribution (in response to demographic and socio-political changes in the relative positions of spatial units) of the unit’s characteristic attributes. These characteristic attributes are: house, threshing-floor, spring, men’s assembly, shrine and cemetery. These spots in the landscape, and the activities focusing on these spots, have religious connotations. Also ancestors are the attributes of spatial units, but the distribution and redistribution of ancestors over spatial units is far less consistent than that of characteristic attributes, because of the lack of consensus, the situational kinship claims of individual informants (Chapter 4 and 4), the absence of empirical manifestations of deceased ancestors, and the absence

of collective activities directed at ancestors.

The hierarchical, constantly shifting structure of spatial units is the central datum in Ĥumiri group classification. I propose to subsume these features under the terms of spatial segmentation. The use of such a term gives rise to a number of theoretical questions which I shall attempt to answer in the next chapter.

7.4. The concept of spatial segmentation

7.4.1. Definition of spatial segmentation

By analogy with kinship-based lineage segmentation as described by a great many ethnographers in a great many societies, and as claimed by the Ĥumiri indigenous societal ideology, towards the end of the previous chapter I spoke of 'spatial segmentation'. What is the meaning of that term? And what is the relation between this type of segmentation, and kinship-based segmentation?

In order to be able to speak of a spatial segment, I submit that the following conditions should be met:

- We are referring to a set of individuals which, with regard to all other individuals can be unequivocally distinguished because the members of the set concerned all live on a particular contiguous part of the landscape.
- Such a set is recognised not just by the ethnographer but also by the actors themselves, as is clear *e.g.* from the fact that the set has a proper name.
- The recognition of such a set is fairly consensual.
- Belonging and not belonging to a particular set as defined here plays a statistically significant role as a recruitment principle in day-to-day interaction. (The latter aspect will be dealt with in chapter 7 below.)

Now segmentation occurs when there is not just one type of such sets, but a number of types, which are each others subset, in other words, when segments include each other on successive levels of segmentation, but unequivocally exclude each other on the same level. So far segmentation can be described as a static structure. However, it is also a process: a particular, distinct segment may, and will, in the course of its existence career through the various hierarchical levels: it will first expand, and that decline. But despite this dynamics of the individual segments the general segmentary structure remains in principle unchanged.

I do not assume that spatial segmentation, wherever it occurs, will always constitute the most important form of social cohesion and integration, and as such invariably a central datum in the political structure. Such a claim must be ascertained empirically for each society and each period. In the course of this chapter I shall come back to this point.

Neither does this approach define the various opportunities and strategies (birth, marriage, migration, etc.) which allow individuals to settle in a particular place and thus insert themselves as new members in a spatial segment.



Burdened under enormous bundles of firewood, the women of the ex-colonial farm on Kef al-Hanut return home after a wood-gathering expedition to the forest on the mountain range of Ain Fellus, immediately above Mayziyya

Fig. 7.2. Women take a major share in productive activities in the research area

7.4.2. Other authors on segmentation

I shall confront my approach with that of other authors, especially two ethnographers of North Africa: Gellner (1969) (and Favret (1966, 1968)).⁴⁹

Gellner (1969: 41 *f.*) characterises a segmentary society in the following terms. The central structural feature is that cohesion can be attained by distinguishing, on each segmentary level, between one's own group and an other, opposing group. This is possible through the operation of a 'tree-like' structure: the largest set A is subdivided into mutually exclusive subsets B', B'', the subsets in their turn are subdivided into mutually sub-subsets C', C'', etc., down to the lowest level of households and individuals. Gellner (1969: 48) admits that segmentation could also be attained by spatial opposition. Yet he considers unilineal kinship to be closely associated with segmentation, because

'any more complicated kinship system would generate conflicting ties',

rendering the unequivocal opposition of segments an impossibility. Gellner appears to

⁴⁹ Favret (1966) is a review article of Gellner's doctoral dissertation, of which Gellner (1969) is the final version. It is not always clear where in her article Favret quotes Gellner approvingly, and where she presents her own views. She is a specialist on North African social organisation in her own right.

overlook the fact that this does not really amount to an argument against spatiality as a segmentary principle which is not based on kinship. As a fourth characteristic of segmentation Gellner mentions the fact

‘that groups of all the various sizes resemble or mirror each other’s structure’.

However, he does not attach much importance to this point, for groups on different segmentary levels have very different functions and relate to very different activities.

Besides the enumeration of these positive characteristics, Gellner poses (1969: 48):

‘The crucial defining characteristic of segmentary societies is not merely the presence of segmentation, but also the absence (or very nearly) of anything else.’

According to Favret a segmentary society has to meet the following conditions. In the first place

‘Chaque individu et chaque groupe doit être situé dans l’ensemble tribal de façon non ambiguë. (...) La règle de filiation unilinéaire correspond à la nécessité d’empêcher la confusion des appartenances. Chaque fois qu’une société segmentaire se définit géographiquement, par rapport à un terroir, elle court le risque que les liens de filiation ne viennent croiser les liens territoriaux; la filiation unilinéaire est le moyen le plus simple d’éviter cet inconvénient en assurant la permanence des biens dans la lignée agnatique: les liens de parenté viennent redoubler – et non recouper – les liens territoriaux.’(Favret 1966: 107)

And secondly:

‘...l’ordre doit être maintenu à tous les échelons sans aucun recours à des institutions politiques spécialisées.’ (Favret 1966: 109).

Favret ‘s treatment of the possibility of spatial segmentation is even more furtive than Gellner’s. His line of argument is that segmentation can only be realised through unilineal descent, for whatever spatial segmentation would emerge would always be spoiled by kinship. Favret implicit assumption appears to be that descent is always and everywhere the crucial factor in social organisation...

In a later article Favret (1968: 20 f.) pays extensive attention to this problem. She objects to the reduction of ‘toutes les relations sociales à la spatialité’, an approach which in her mind smacks of a ‘matérialisme grossier’. The classic ethnographers of Middle Algerian Kabylia (Hanoteau and Letourneux) are said to have been guilty of such an approach. Why should this be so?

‘Dans leur esprit, en particulier, les liens territoriaux sont vrais:

- (a) parce que les liens lignagers ou généalogiques sont faux (...);
- (b) parce que les alliances politiques sont instables.’

Favret reviews which social grouping Hanoteau and Letourneux distinguished, and concludes:

‘on comprend à présent comment les auteurs classiques ont pu réduire les liens lignagers et les liens politiques aux liens territoriaux; si on refuse l’idée qu’un lignage définisse une relation sociologique et non génétique, et qu’une organisation politique n’ait pas besoin d’être stable et permanente pour exister, seul le niveau (...) du village manifeste les trois types de relations à la fois; on peut alors aisément les réduire à un seul, le territoire, dont on déduirait tous les autres.’ (Favret 1968: 23)

None the less, the latter resembles my own argument in Chapter 4. Favret does not show why the approach from spatiality is wrong or devious; at best she states the case for an additional approach based on kinship. At any rate she entirely ignores the consensus problem: the fact that a kinship idiom can only be invoked as an autonomous field of explanation of the actors in a social system have by and large converging definitions of the kinship ties that link them.

Admittedly neither Gellner nor Favret present unilineal descent as the central datum in segmentation, but as the most useful means to realise segmentation. They largely agree as to what constitutes segmentation. In particular, both see the existence of an unequivocal classification system as a condition for segmentation.

When we compare Gellner's and Favret's approach with mine, it will turn out that what I have called 'spatial segmentation' is likewise an unequivocal indigenous model of social classification. I would even maintain (contrary to Favret) that spatial segmentation is less ambiguous than kinship-based segmentation. For spatial segmentation is bound by the perceptible situation of people in the landscape, which (in the *Ḥumiri* case, that is) can be directly ascertained from the visible clusters which they form and the visible boundaries between these clusters, and from the visible characteristic attributes of these spatial units. Whereas kinship-based classification (at least in *Ḥumiriyya*) because of lack of consensus and situationality are absolutely not unequivocal.

Therefore, without rejecting in general the concept of segmentation as based on unilineal descent, I submit that in addition the concept of spatial segmentation does form a legitimate concept. This is also the view of Middleton & Tait (1958: 7), although these authors largely confine themselves to kinship-based segmentation.

Middleton & Tait mention yet another characteristic of segmentation: it is not only a hierarchical classification of social units ('the nesting attribute', as they call this aspect), but these units are also involved in 'a continual process of segmentation', in the course of which individual units all the time grown out or decline to attain other segmentary levels, – without this process affecting the persistence of the general segmentary structure as a whole. The same aspect is recognised by Gellner and Favret, but with less emphasis. From this point of view the spatial classification as sketched in the previous chapter, can also be called segmentary: there is a constant changing of the relative positions of the spatial segments vis-à-vis one another, and this goes hand in hand with a constant redistribution of characteristic attributes, against the background of a model which in itself appears to be unchanging.

However my approach is to be distinguished from that of the authors mentioned in so far as the political significance of segmentation is concerned. For Gellner, Favret, Middleton & Tait, Fortes & Evans-Pritchard and many others segmentation as such is primarily a political system: notably virtually the only form of political structure which we find in a society acclaimed to be segmentary.

From that point of view *Ḥumiriyya* in 1968 could impossibly be called segmentary. For since the imposition of French protectorate rule (1881) the region has been ever more effectively incorporated in the political and juridical structure of Tunisia as a whole, which structure has its formal administrators down to the level of chiefdom and valley (the chief and his assistants).

Before 1881 Ḥumiriyya did resort under the Bey of Tunis, but the latter's grip on the region must have been very slight. The region was then the scene of continual conflict between semi-nomadic, spatial groups (and confederacies of such groups), over pastures, livestock, and women (Hartong 1968; Souyris-Rolland 1949). In so far as conflicts were not settled by the emigration of either conflicting party, they were settled by the incidental intervention of prominent men in the near or distant surroundings – often members of religious brotherhoods and/or warden of shrines.

Gellner and the other authors as discussed above would have no trouble to call Ḥumiriyya of that segmentary, with all the political implications the use of that term has for them. What has change in Ḥumiriyya in the meantime? Probably the specific indigenous system of group classification which I have called spatial segmentation has hardly changed; but what did change, and very drastically, is the political significance of that system: the contemporary political and juridical structure hardly leaves any room for the functioning of spatial segmentation as an autonomous political mechanism.

None the less spatial segmentation has a very great significance in Ḥumiriyya today. It is the subject of an elaborate ideology (chapter 2) and even mythology (see the part on toponymical myths etc., below). As we shall see in the next few chapters spatial segmentation is a primary consideration in the selection of interaction partners, the frequency of interaction, and the contracting of marriages. And in my analysis of religion I shall demonstrate how a major sector of religious activities and representation (saints, shrines and their veneration) is entirely governed by spatial segmentation.

7.4.3. Conclusion

For these reasons I feel justified to disconnect segmentation as a classification principle from the political functioning of this principle. The various forms of segmentation in the societies of the world can be assessed empirically, and so can the functions which certain forms of segmentation fulfil in certain societies. It will turn out that in some societies segmentation functions as a political system – and this would then be segmentation in the sense of Gellner c.s. As a classification principle some societies will turn out to rely on unilineal descent, others on spatiality; possibly there are still other principles possible.

It is certainly not my contention that Ḥumiri society can be entirely grasped from an exclusive application of the concept of spatial segmentation alone, and that specifically the concept of unilineal-descent segmentation is totally inapplicable there. But an approach which starts from spatial segmentation does appear to me to be the more fruitful one. Probably defendants of kinship-based segmentation will find in my description of Ḥumiriyya the proofs that 'in the last analysis' Ḥumiriyya (apart from its contemporary political functioning) is really organised along lines of kinship-based segmentation. In the same way their own descriptions give me the feeling that the systems of social organisation they describe are spatially, rather than unilineally, segmented.

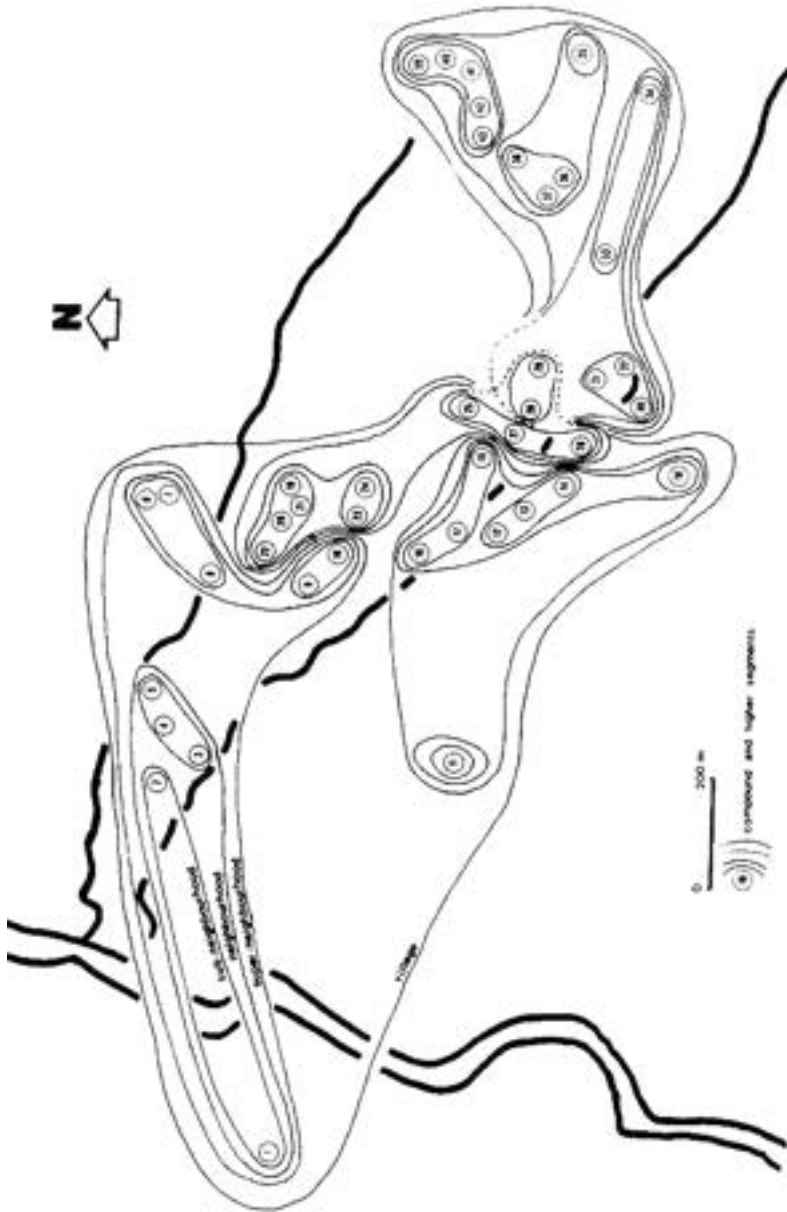


Fig. 7.3. Compounds, various types of neighbourhoods, and villages: The structure of

spatial segmentation in the villages of Sidi Mhammad and Mayziyya, 1968

My analysis of lineages and clans in Ḥumiriyya brings out that these kinship-based classifications primarily play a role in so far as they express in a kinship idiom processes which in reality relegate back to spatial classifications: in Ḥumiriyya lineages and clans are too imperfectly structured, too non-consensual and too much subject to situationality to function as effective principles of group classification in their own right. This also points in the direction of spatiality as the dominant segmentation principle in its own right.

We are now in a position to delineate the spatial segments that were practically in existence in the villages of Sidi Mhammad and Mayziyya in 1968. Such territorial segments are largely eclipsed from the participants's consciousness by their excessive emphasis on the ideology of patrilineal descent and agnatic solidarity. We shall have to wait till the discussion of Ḥumiri popular religion in Volume II until we can adduce substantial evidence of the functioning of such territorial segments in the structuring of actual interaction.

Chapter 8. Kinship-based and spatiality-based group classification as relevant for day-to-day interaction

8.1. Introduction

In the preceding chapters I have described the H̄umiri indigenous classification systems based on kinship and spatiality, with clans as an intermediate form. Spatiality turned out to lead to a clear and elaborate indigenous system of group classification, by contrast with kinship and the clan structure. Now I shall proceed to assess what the significance is of spatiality and kinship for day-to-day interaction.

I shall not explicitly discuss the relevance of clan classification as a determinant for day-to-day interaction. This is not an important omission. For class classifications are non-consensual and situational to such an extent that in most cases they are secondary formulations for existing social relationships; and such relationships have not been initiated because the partners involved belong to the same clan, but by other factors. Moreover this lack of consensus, and situationality, imply a virtually insurmountable problem of operationalisation. If the ethnographer is not reasonably sure how individuals in his or her sample structure their social environment in terms of clans, it remains impossible to relate such classifications systematically, *e.g.* statistically, to other data on interaction, residence, the marriage pattern etc.

Therefore I shall confine myself to spatiality and to kinship. Above it became clear already that these principles of group classification can be relevant for the social organisation in two complementary ways. On the one hand they provide the basis for the delineation of social groups, and in this way they allow us to study the interactions between and within relevant social groups in a particular society. on the other hand

kinship and spatiality produce subjective, egocentric attributes: the relationships between a particular Ego and any other individual in that Ego's social environment. This perspective enables us to study the significance of kinship and spatiality for day-to-day interaction between two individuals.

In a group classification system, kinship creates groups of kinsmen. And because ideologically H̄umir̄i society is patrilineal, kinship there creates primarily agnatic groups.

We are involved in the assessment of the relevance, for day-to-day interaction, of the indigenous system. Therefore we must not base our analysis of kinship on the ortholineage as reconstructed by the ethnographer, but on the individual pseudolineages of the actors. For to the extent to which in their choices in interaction, kinship considerations play a role, this could only be to the extent to which the actor's individual perceptions of kin groups structure his or her social environment. Since we have decided to exclude clans from our analysis, we can ignore such mythical ancestors as the individual pseudolineages may contain. We seek to assess the significance of kinship as a determinant, not as a secondary expression, of day-to-day interaction. Therefore we should limit our analysis to that part of the individual pseudolineages which is outside the limits of manipulation: consensual pseudolineage cores. And this takes us back again to the ortholineages. For consensual pseudolineage cores can we adequately operationalised as ortholineage branches. We can then define the *ortholineage branch* as:

- A limited set of agnates who descend patrilineally from a shared historical ancestor (branch founder), provided the latter lived in a past so recent that
- his descendants are still being recognised as a distinct set;
- they have usually not dispersed too widely over the local area; and that
- this limited set, at or below the generational level of the branch founder, has not yet been contaminated, through genealogical manipulation, with non-members *i.e.* with non-descendants of the branch founder.

This definition does not exclude, of course that the ortholineage branch, along with other branches of the same ortholineage and one or more branches of different ortholineages, is included in more comprehensive pseudolineages – provided the manipulative links are made at generational levels above the branch founder. The limits of manipulations primarily depend on the length of chain: the number of elements in the kinship chain between two individuals; for instance, in the chain FFBSD the length of chain is five. Judging on the grounds of my experience with genealogical knowledge of H̄umir̄i informants I submit that actors who analytically belong to the same ortholineage and, within that ortholineage, are removed from each other by a length of chain of only six or fewer elements, in general do still recognise each other as agnates, regardless of any day-to-day interaction which may exist between the two; at higher lengths of chain such recognition is generally lost. Therefore in the analysis of the relevance of agnatic kinship for day-to-day interaction we may confine ourselves to chains with a length of six elements at the most, *i.e.* to consensual pseudolineage cores whose historical founder was the F, FF or at most FFF of the youngest generation of adult members of the set. In this context, we can speak

of close agnates if the length of chain does not exceed three; and of distant agnates if the length of chain lies between four and six.

Fig. 8.3 summarises the problematic of group classification on the basis of agnatic kinship.

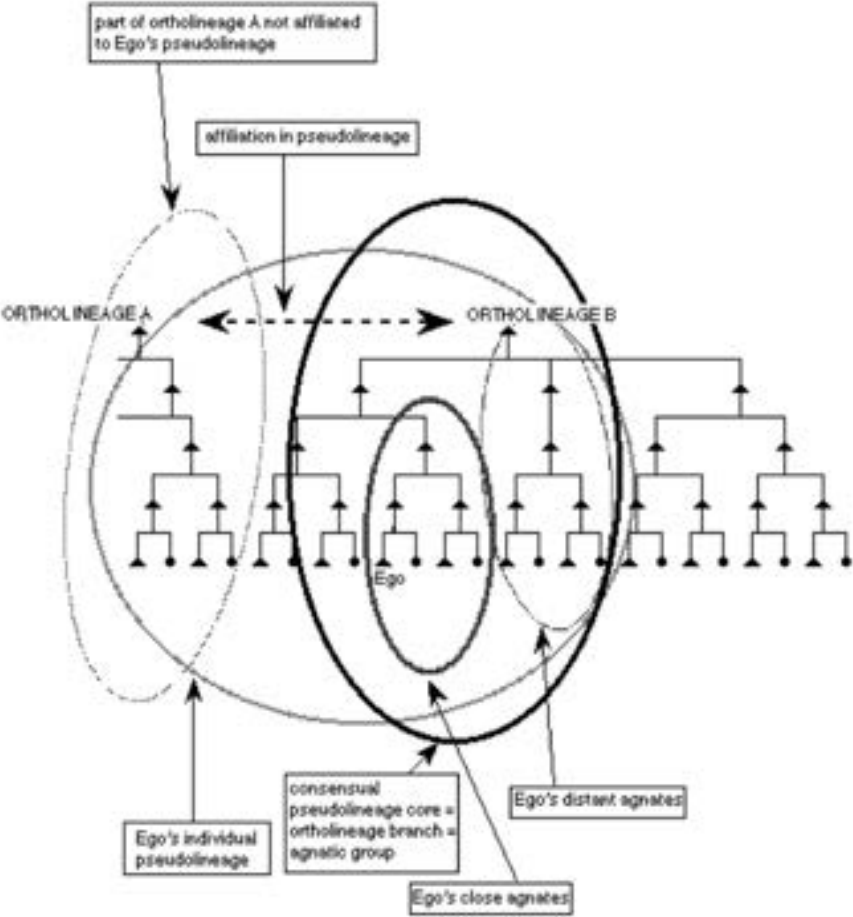


Fig. 8.1. Illustrating the problematic of group classification on the basis of agnatic kinship

The operationalisation of spatiality is less complicated a matter. The spatial groups which are produced by a spatial group classification are simply the spatial segments which we have discussed in the previous chapters: units which are hierarchically ordered, which excluded each other at the same level, which have visible boundaries in the landscape, and over which names, characteristic attributes, and ancestors have

been distributed. From the point of view of egocentric relationships spatiality as a factor can be simply operationalised as: the geographical distance between the dwellings of Ego and Ego's interaction partner. For further details I refer to appendix 1

On the basis of the operationalisations as developed here, in the present chapter I shall discuss the relevance of group classification for day-to-day interaction in Ҳумирӣя; the next chapter then will be devoted to a discussion of kinship and spatiality as principles for egocentric relations in the interaction between two individuals.

8.2. The limited relevance of group classification for day-to-day interaction in Ҳумирӣя

As we have seen in chapter 5, the spatial segments have collective activities which focus on the segments's characteristic attributes. The actors in these collective activities are primarily recruited among the members of the segment concerned. However, close relationships (frequent interaction) between individual members of the segment on the one hand, and non-members on the other, often lead to a situation where one or a few non-members participate in these collective activities. This applies to all segmentary levels, from family life (where for instance occasionally visitors who are non-members of the household share in a meal), to the saintly festival of a chiefdom, where the participation of outsiders, albeit on a moderate scale, is an essential, institutionalised condition. Alternatively, often a few members of the segment concerned may be absent from the collective activities. Sometimes this is caused by the impact of certain prohibitions. A case in point is collective pilgrimages to a particular shrine which is a segment's characteristic attribute; a menstruating woman is not allowed to take part in such a visit. But often absences from a segment's collective activities are unrelated by such generalised prohibitions (or the applicability of these prohibitions is faked in specific cases). In those cases the non-participation is seriously disapproved of by the other members of the segment, and invariably points at underlying conflicts within the segment, which ultimately may lead to its fission into a number of new segments at the same level as the original one.

The relevance of spatial group classification for day-to-day interaction is undeniable. But at the same time this relevance is limited. For most activities in a Ҳумирӣ village are not collective to all members of a particular spatial segment, but they devolve between two partners.⁵⁰ These non-collective activities cannot be described from the point of view of group classification, but have to be analysed as egocentric relationships between two partners; this is the topic of chapter 8.

Let us now turn to the relevance of kinship-based group classification for day-to-day interaction.

⁵⁰ [**note 24**] Several researchers in Ҳумирӣя have done research on the interaction within certain types of spatial units, for instance Beeker (1968): family compound, neighbourhood; Bos 1969: household, family compound, neighbourhood, village, valley; van Dijk 1968: compound, neighbourhood, village; Jongmans 1968: village; Jonker n.d.: household, family compound; Schulte Nordholt 1968: valley, chiefdom. The publications of these researchers extensively deal with the interactions characteristic of the spatial units on each of the various segmentary levels.

The conclusion of Chapter 4 was that indigenous group classification based on kinship can hardly function in Ḥumirīyya. To this we may now add: and indeed it does not function, in the sense that kinship has hardly any significance for day-to-day interaction.

The central question at this juncture is: to what extent do agnatic groups (ortholineage branches, consensual pseudolineage cores) to real social groups?

In the indigenous Ḥumirī societal ideology spatial classification runs parallel to the classification in terms of agnatic kinship: the actors seek to comprehend households, compounds, neighbourhoods and villages within an agnatic genealogy (chapter 5, and the last section of the present chapter). But when we turn to the agnatic groups as discerned by both the actors and the ethnographer his parallelism dissolves beyond recognition. Fig. 13 shows that the residential space of a few individual members of an agnatic grouping is often contiguous. However, the capricious contours of this residential space are such that there will always be members of different agnatic groups who live at a lesser distance than some of the members of one's own agnatic group. Table 16 further develops this point, showing that the members of certain agnatic groups are dispersed not only within the same village but across several villages and even valleys. The same point is made by table 9 and Fig. 9: when we try to capture the average kinship composition of spatial segments by tracing concentric circles around the dwelling house of an average, aggregate Ḥumirī Ego, there is never an inner circle, no matter how much we reduce its radius, within which the heads of household who constitute Ego's neighbours will be exclusively his agnatic kinsmen. This is not even true if we narrow down the radius to the compound level, *i.e.* to about 25 m. Yet in terms of the indigenous societal ideology Ego is supposed to be surrounded by neighbours who are his close agnates, with distant agnates at the somewhat greater distances, and non-agnates virtually beyond his social horizon. Despite the occurrence of agnatic endogamy (which results in a situation where some women in Ego's immediate spatial environment have not married in from a non-agnatic origin but belong to Ego's own agnatic group; *cf.* appendix 2), the same tendency will become even more apparent if we do not limit ourselves to heads of household but to all adults, both male and female. As is clear from the data in table 9, among the heads of household who live at a distance not exceeding 25 meter from Ego's dwelling house (and hence: who share Ego's compound), we find on the average 40% non-agnates (in terms of the above operationalisations in terms of chain length). As distance increases this percentage increases rapidly.

This suffices to demonstrate that, in order to ascertain whether the agnatic groups are real social groups, we are not allowed to follow the indigenous societal ideology and equate agnatic groups with spatial segments.

But perhaps agnatic groups constitute real social groups in their own right, regardless of their spatial projection?

It is undeniable that the members of agnatic groups do identify with each other to a certain extent, and that of course is one of the conditions to constitute a real social group.

Likewise, in many cases there is day-to-day interaction between two individual members of the same agnatic group (appendix 1).

There even exist collective activities in which virtually all members of the agnatic group participate:

- (a) On the one hand these are the collective activities of the spatial segment which comprises the entire residential space of the agnatic group (sometimes at the compound level, more typically at the neighbourhood and village level, occasionally also at the valley level);
- (b) on the other hand life crises (birth, circumcision, marriage, illness and death) of an individual members, Ego, of the agnatic group form the occasions for such collective activities.

But even if virtually all members of a particular agnatic group participate, this does not automatically mean that recruitment to these collective activities is strictly on the basis of agnatic kinship! With regard to the activities under (a), actors are being recruited on the basis of spatial classification. And with regard to (b), recruitment depends on a potential actor's belonging not so much to Ego's agnatic group but to Ego's egocentric kin network: the kindred, which includes not only agnates but at least as many cognates and affines (see below in this chapter). (Moreover, on the occasion of life crises not only the kindred are invited but also neighbours who happen not to belong to the kindred.) In both cases, therefore, many actors to the collective activities do not belong to one and the same agnatic group.

In this context marital relations deserve a discussion on their own. In the first instance a marriage creates a relationship between individuals: the spouses, and the members of their respective egocentric kin networks. Such individual egocentric relations do not lend themselves to an analysis in terms of group classification. However, individuals also belong to agnatic groups, and we might conceive the Hūmirī marriage as a form of interaction between (or in the case of agnatic endogamy: within) agnatic groups. This aspect is analysed in appendix 2. The principle conclusions are the following:

- In the recruitment of marriage partners there is neither a positive selection (preference) nor a negative selection (avoidance) with regard to specific agnatic groups (one's own group excluded from the analysis). By marriage partners I then mean heads of household who supervise the marriage between marriageable men and women belonging to their household or compound; often marriage partners are the fathers of bride and bridegroom, but they may also be their FB, B, or other kinsmen.
- Although marriage within one's own agnatic group do occur, there is no statistically significant preference for such marriages.
- Marriages within one's own agnatic group must be considered as only one of the possible forms which the marriage within the kindred (agnates, cognates and affines) can take, and as such agnatic endogamy is not fundamentally different from other forms of kindred endogamy.

This suffices to show that group classification on the basis of agnatic kinship does not function with regard to marriages either.

In general, therefore, we can conclude that group classification as based on agnatic kinship has hardly any significance for day-to-day interaction within the Hūmirī social

organisation.

8.3. A comparative functional analysis of group classification as based on unilineal descent

The limited relevance of group classification as based on unilineal descent in Ḥumiriyya can also be demonstrated as follows.

Lewis (1965) undertook a comparative functional analysis of unilineal descent. He formulated a number of criteria, which he argued to constitute useful operationalisations of significant aspects of the principle of unilineal descent in various societies. The relative weight of these criteria remained a point for further research. Lewis claims that the principle of unilineal descent is the stronger in a society, the more it corresponds with the following ideal type (Lewis 1965: 108):

- (a) Cognate kinship is not a basis for fictive agnatic connections.
- (b) In about the same manner as men, women remain members of the agnatic group in which they were born throughout their lives.
- (c) There is one 'national' genealogy.
- (d) Unilineal descent creates political cohesion.
- (e) Unilineal descent creates judicial cohesion.
- (f) Unilineal descent creates cohesion with regard to religious activities.
- (g) Besides unilineal descent there are no rival structural principles:
 - (g.1) A group's attachment to a territory is not an important structure datum.
 - (g.2) There is no age set organisation.
 - (g.3) There is no association of group on a contractual (*i.e.* non-kinship) basis.
 - (g.4) There is no centralised government.

Lewis applies this scheme to a number of societies which in anthropology are generally known for the importance that unilineal descent has in their social organisation, including the Nuer (Evans-Pritchard 1967) and the Cyrenaica Bedouins (Peters 1960). All these societies obtain four to eight positive scores each. If we apply the same approach to Ḥumiri society in 1968, positive scores would only appear for the points (b) and (g2). If we go back a hundred years, we might at best be able to add point (g4). Assuming that the attribution of scores for the other societies is correct, we must conclude that Ḥumiriyya is to a significantly lesser degree than the societies in Lewis's sample structured by unilineal descent.

Of course we might scrutinise the correctness of Lewis's scores. Also we might expand his sample, which consisted of only eight societies. Alternatively, if my analysis of Ḥumiri society makes sense, and if the general ideas concerning group classification which I develop in the course of my argument (especially chapter 9) would have some

applicability to other societies besides Ḥumiriyya, this would mean that for the societies in Lewis's sample we would also have to pose the consensus problem, the relationship between kinship-based and spatial classification, and the relevance of such group classifications for day-to-day interaction. In combination these three questions would seem to affect, and problematise, virtually all of Lewis's criteria! It is my hypothesis, which has to remain un-tested for the time being, that while in Ḥumiriyya unilineal descent plays demonstrably only a limited role as a principle of group classification, its significance may not be much larger in those societies which did attain high scores on Lewis's criteria given the way in which that ethnographer operationalised and score these criteria.

8.4. Ḥumiriyya and Cyrenaica

My analysis of Ḥumiri social organisation corresponds well with the one offered by Peters (1960, 1967) for the Bedouin society of Cyrenaica. This is particularly clear from the following quotation:

'What I am arguing is not that the lineage model in its rarefied state is too simple, not that additional facts must be injected into it to complicate it, but that it does not provide an admissible basis for analysis. The Bedouin conception of their social relationships in terms of a genealogical ordering of groups is a fact of their social life (...). My objection to the use which has been made of a people's ideology of their relationships is that it has been elevated from its status as a component of social life to such a position of universal dominance in all sets of social relationships that "every sociological problem", as Fortes writes of the Tallensi, "hinges on the lineage system" ' (Peters 1967: 279).

The parallel between Ḥumiriyya and the Cyrenaica Bedouin has another implication for us. With the exception of the system of group classification (which in both societies, in a similar manner, is claimed in the indigenous ideology, is subject to manipulation, and in fact does not function for day-to-day interaction), the society as described by Peters looks not so much like contemporary Ḥumiriyya with its sedentary pattern of residence, its decline of animal husbandry, and the effective incorporation in a national political and judicial system, but like Ḥumiriyya the way it was up to some seventy (in 1968) years ago: with an emphasis on animal husbandry and with semiannual transhumance which made for the seasonal dispersal and subsequent concentration of what Peters calls 'tertiary tribal sections' (some 200 to 700 people) near springs, fields, pastures and orchards over which these groups claim exclusive rights, – rights which are continuously contested by violent means by other such groups (Peters 1967: 262; *cf.* for Ḥumiriyya Hartong 1968 and Bos 1969). Ḥumiriyya and Cyrenaica are in a different phase of socio-political change. That yet both societies display an indigenous societal ideology hinging on agnatic kinship, and that in both societies the ethnographer has to admit the limited relevance of the lineage model⁵¹, is an argument for the thesis that even in nineteenth-century Ḥumiri society the lineal model cannot have provided an adequate analysis of the social organisation (*cf.* Huitzing, forthcoming).

⁵¹ The article by Peters (1967) came only into my hands after the field-work, and after my analysis of kinship-based social grouping in Ḥumiriyya had been completed.

What alternative does Peters propose for Cyrenaica? In fact, a flexible form of group classification which can be largely described from the point of view of spatiality:

‘Ultimately, feud is a violent form of hostility between corporations which had its source in the competition for proprietary rights in land and water. This competition makes it necessary for groups to combine to prevent the encroachment of others in similar combinations and also to expand their resources whenever the opportunity arises. The significant groups in a discussion of the feud are these power groups, and it is their composition, the shifting alliances within them, the growth and diminution in the power of the tertiary sections constituting the combinations which makes the facts of feud intelligible’ (Peters 1967: 279).

Also these statements appear to correspond well with nineteenth-century Ḥumiri society (*cf.* Hartong 1968 and especially: Huitzing, forthcoming). For present-day Ḥumiriyya (and perhaps even for nineteenth-century Ḥumiriyya) this picture has to be qualified. For crucial in Ḥumiri social organisation is not group classification (not even group classification as based on spatiality) but the day-to-day interaction between two individuals, and the egocentric relationships within which such interaction is implied. This aspect we shall discuss in the next chapter.

In the meantime certain ideological problems concerning the relation between kinship-based and spatial group classification clamour for our attention.

8.5. Kinship, spatiality and indigenous societal ideology

The indigenous societal ideology which we have discussed in chapter 2, (a) presents agnatic kinship as the central principle in the Ḥumiri system of group classification, and (b) suggests that classification based on agnatic kinship are crucial for day-to-day interaction.

In fact neither (a) nor (b) hold true in the fact of the reality of Ḥumiri social life. Group classification as based on unilineal descent is non-consensual and situational; that is why it turns out to be virtually irrelevant for day-to-day interaction. This by contrast with spatial group classification, which results in a clear and elaborate system, produces real social groups and as such has an unmistakable effect on day-to-day interaction.

The existence alongside of two different systems, one of which is ideologically underpinned but does not work, while the other does work and (as we have seen in chapter 5) also represents a tangible reality for the actors themselves (albeit in a manner less explicit than the kinship-based system) constitutes a remarkable paradox. Therefore I shall now assess what the actors’s possibilities are to reconcile both systems. In other words: by means of which operations is it possible to transform one system into the other? Most of these operations have already been discussed at various points in the preceding argument, but it is useful to list them once again briefly and in the proper perspective.

8.5.1. Indigenous terms

The first possibility for actors to integrate kinship-based and spatial classification, is given with the nature of the most frequently used indigenous terms for social units. For such terms always turn out to have both a kinship aspect as a spatial aspect, as is

set out in table 1 above. Therefore the actors are not likely to make a sharp distinction between both principles of classification. The terms reflect the societal ideology according to which the spatial distribution of people can be explained by their agnatic relations.

In order to capture the structure of Ḥumirī society, it is obviously totally inadequate to simply substitute the indigenous terms by apparent analytical equivalents (*e.g.* substituting 'village' for *duar*, or 'lineage' for *firqa*). Yet such an approach is far from unusual in the literature on North African societies, and *a fortiori* in the theses written in the context of the University of Amsterdam's field-work training project in Tunisia. Such an approach entirely ignores the problematic around which the present argument revolves.

Meanwhile it should be noted that besides the terms listed in table... there are others, less frequently used, which do one-sidedly stress either kinship, or spatiality. In the next chapter we shall encounter the term *famiya*, for kindred. In addition van Dijk (1968: 6) mentions the term of *cayla*: part of a (pseudo-) lineage. Another example is the term *hanshir*. Its basic meaning is : territory, heritage. It is being used for stretches of land from a few dozen of hectares upwards, which tend to be associates with a particular village, pseudolineage or clan; in a more defined sense it is the term for valley (*cf.* Bardin 1965: 88 n. 2).

8.5.2. Expressing spatiality in a kinship idiom

The second possibility for the actors to reconcile kinship and spatiality lies in their tendency to express spatial segmentation in a kinship idiom. Ancestors become the attributes of spatial segments, and the genealogical links which one claims to exists between these ancestors reflect the segmentary opposition and integration of the spatial segments involved (chapter 5).

8.5.3. The clan

A third possibility lies in the clan which we have shown to be an intermediate form between spatial and kinship classification (chapter 4).

8.5.4. The indigenous concept of kinship

The fourth possibility for the actors to reconcile kinship and spatiality lies in the non-exclusive nature of the indigenous concept of kinship (Chapter 4).

In fact the indigenous concept of kinship corresponds with the analytical concept of 'positive social relationship'. As we shall see in the next chapter, the nearer two people live to each another, the greater the chance that there is a positive social relationship between them, and the greater the frequency of their interaction in the context of such a positive relationship. Now it follows from the societal ideology that all inhabitants of a clan territory are each other's agnates, and (because the lower-generation ancestors are ideally distributed according to the spatial segments which ideologically are nothing but the spatial projections agnatic segments) people are the closer related as

agnates, the lower the level of the segment which they have in common, in other words the nearer they live to each other. Therefore everybody in Ḥumiriyya can see that the actual patterns of interaction are in line with the degree of ('indigenous' kinship such as is being claimed by the ideology. Spatial segmentation and indigenous societal ideology in agnatic terms converge within the indigenous concept of kinship.

8.5.5. Relegating segmentary dynamics to the principles which unmistakably apply at the lowest segmentary levels

The actors more or less explicitly perceive a pattern of spatial segmentation in their society, as we have demonstrated in chapter 5. Now, what the ideology does in fact is: to render that pattern intelligible by presenting it as the result of the continuous demographic and spatial evolution of spatial units at the lowest level: households and family compounds.

This evolution is a continuous process, which is part of the actors's experience day after day. Viewed thus, the ideological emphasis on agnatic kinship is no longer amazing: for in Ḥumiriyya the male members of the household (and to a rather lesser extent of the family compound) are largely recruited on the basis of agnatic kinship. Only sporadically does a household comprise close cognates or affines, or non-kin farm-hands and herdsmen. Of course we should not overlook the fact that far from all compounds are family compounds: for many compounds it is true to say that the recruitment on the basis of agnatic kinship has been supplanted by a recruitment based on the existence of positive actual relationships (which often involve non-kin) between people who initially lived at a considerable distance and who came to share a compound after a residential move (see chapter 8 p. 70 f;) None the less the agnatic model is rather applicable at the compound level and a fortiori at the household level, and the actors simply extrapolate this model to the higher segmentary levels. Hence the ideology which makes all inhabitants of a valley into each other's agnates: for as the actors see it, and as they have explicitly phrased to me on dozens of occasions, an indefinite number of generations ago their ancestors belonged to the household of the highest mythical ancestor.

It would seem that such extrapolation, within the ideology, forms an example of what Gellner has listed as one of the characteristics of segmentation:

'that groups of all the various sizes resemble or mirror each other's structure' (Gellner 1969: 48; by analogy with Leibnitz's (1898; cf. Hartz [year]) well-known philosophical concept Gellner speaks here of monadism).

That in Ḥumiriyya this monadic feature turns out to relate more to the ideology than to the actual nature and functioning of the segments, is in line with Gellner's statements on the subject.

Now there is no denying that in some cases the actors have striking examples at their disposal, historically fairly reliable at that, in which the spatial segmentation does appear as the result of the fission and gradual growth to autonomy of small groups of agnatic descendants of one ancestor. For instance, for a number of family compounds and even neighbourhoods in the villages of Sidī Mḥammad, Tra'aya-sut, Quassim, Manadjliyya and al-Hafur we can convincingly demonstrate that they did originate

through successive fission within ortholineage 5.⁵² But the point is that in all these villages there are compounds and neighbourhoods which do not belong to ortholineage 5 (*cf.* appendix 6). The share of these other ortholineages in the segments concerned is always at least a few dozen percent, and in most cases they constitute the majority against a minority deriving from ortholineage 5.

The indigenous representation of segmentary dynamics falls short because it cannot accommodate the diversity of ortholineages. Moreover it wrongly presents patrilineal inheritance of land as the only means to get access to a particular spatial segment. Yet in most cases the immigration of an ortholineage branch in a particular segment was not based at all on patrilineal inheritance of land, but on any of the other means to obtain land: matrilineal inheritance, uxorilocal residence, gift, exchange, purchase, temporary contract, obtaining a governmental concession to make a clearing on a forested slope, and up to the early twentieth century: violent conquest (Martin 1966; van Dijk 1968).

Inevitably the extrapolation to valley level of the dispersion of the household and the family compound cannot but produce enormous distortions as compared with the factual residential history, which involves a large number of mutually irreducible ortholineage. In Chapter 4 we have sought to identify the mechanisms behind these distortions, in order to be able to reconstruct the factual residential history. However, as far as the present-day interaction between the actors is concerned, such distortions do not matter at all. For the closer one lives together, the more positive relations, the more interaction – well, so much the better if on the basis of the ideology one can interpret these actual relations as relations between ‘kinsmen’.

However, when these positive relations change into conflictive ones, one will be inclined to drop the claim of being kinsmen as soon as possible, and one will begin to advance different historical claims, now to the effect that the other party lack all rights to belong to the common segment.⁵³ Needless to say that these new claims may be just as distorted as the ones used previously to claim a close bond of kinship.

8.5.6. The circular mechanism of fictive kinship and positive social relationships

The most deceptive shortcoming of *Ĥumirī* ideology (deceptive only when that ideology is erroneously taken to represent the equivalent of scholarly analysis) is its claims that agnatic kinship is crucial to aspect of social life. As argued before, the situation becomes much clearer when one reads ‘positive social relationship’ for the indigenous concept of kinship as expressed in concrete claims of sharing an ancestor etc. This semantic mechanism constitutes the actors’s sixth possibility to reconcile kinship and spatiality. Wherever positive social relations occur, those involved tend to lift their fictive or pseudo-kinship above the metaphorical plane of figurative use of

⁵² See appendix 6 [**anders**] ; table 16; Fig.s 13, 16 and 17. Further data on this point can be found in Hartong (1968: 41*f.*, 44, 60*f.*) and Bos (1969: 10*f.*).

⁵³ See Chapter 4 example 3 [**anders**] : the statements by informant 20 [**geef naam**] ; a similar case in Jongmans 1968: 21*f.*

kinship terminology (*cf.* appendix 7) etc.: in many cases they aspire to justify such pseudo-kinship by means of genealogical claims to which they attribute full reality value as long as the positive relationship lasts.

These fictive genealogical claims shape the close relationship between people, and set the pace for mutual identification, expectations, day-to-day interactions, and disappointments. Their highest expression is the brother/brother relationship. In Ḥumiriyya close relationships are on the one hand subject to constant manipulation by those involved, they are frequently broken and transferred to other, rival interaction partners; but as long as such relationships persist they have, in addition to this manipulative aspect (which reflects the personal interest of each partner), also a very strong emotional aspect, in which the partners identify truly wholeheartedly with each other. It is this aspect which is enhanced by viewing the relationship as an agnatic one: unbreakable, not achieved or contracted but there by right of birth, and offering the highest opportunity for identification which is attainable in Ḥumiri society.

Even if the significance of actual agnatic kinship for day-to-day interaction is limited in Ḥumiriyya, we would not be able to understand the character of close relationships in that society without a thorough appreciation of the ideology with regard to kinship.

But with this analysis we have already left the domain of group classification and entered that of egocentric relationships between two persons. This will be the subject matter of the next chapter.

8.6. Summary

In this chapter we dealt with the relevance, for day-to-day interaction in Ḥumiriyya, of group classification based on either kinship or spatiality.

The significance of spatial group classification is undeniable: spatial groups (segments) exist which have collective interaction centring on the characteristic attributes of these segments. This makes the spatial segments into real social groups.

In Chapter 4 we already concluded that group classification as based on unilineal descent could hardly function in Ḥumiriyya. This insight is confirmed by the data on day-to-day interaction. Despite the limited consensus and the situationality of kin-based classifications in Ḥumiriyya we managed to construct clear-cut agnatic groups (consensual pseudolineage cores or ortholineage branches). The societal ideology claims that these agnatic groups are identical with spatial segments, but such turns out not to be the case. Therefore, while spatial segments may be relevant for day-to-day interaction, such relevance for kinship-based group classification must be demonstrated independently from spatiality. This proves to be impossible. Wherever all members of an agnatic group share collective interaction, invariably non-agnates hares in these activities: either cognates, affines, or such members of the local spatial segment (including, of course, near neighbours) as belong to different ortholineages. Also for an analysis of the marriage pattern group classification based on unilineal descent turns out not to be relevant.

To this picture further relief is added by an application to Ḥumiriyya of the comparative functional analysis which Lewis (1965) made of group classification based

on unilineal descent.

My conclusions converge with those reached by Peters (1967) with regard to the Bedouin society of Cyrenaica.

My analysis of Ḥumirī social organisation pretends to be a scholarly, analytical formulation of the classification which the Ḥumirīs use themselves – classifications which determine their motivations and interactions. Therefore we must not ignore their own conception of their social organisation. However, there is a great discrepancy between our conclusions and the Ḥumirī societal ideology, which present agnatic kinship as the central principle in the indigenous group classification system, and which suggests that such classification is decisive for day-to-day interaction.

It is argued that the analytical model can be translated into the indigenous model by means of a limited number of operations: Ḥumirī denotations of social groups display ambivalence between the spatial and the kinship aspect; ancestors are being used as the attributes of spatial segments; clans constitute an intermediate form between spatial and kinship-based classification; the indigenous concept of kinship is in fact equivalent with 'positive social relationship' – and in the recruitment to such relationships spatiality is a prime determinant.

Seen in this light the contents of the Ḥumirī ideology are not so very different from my scholarly analysis of Ḥumirī social organisation. Yet the ideology falls short on a number of counts: it has no place for the diversity of lineages; it represents patrilineal inheritance as the only means to gain access to spatial segment; and finally it lends to the indigenous concept of kinship ('positive social relationship') yet the underlying suggestion of historical agnatic kinship.

However the fictive genealogical claims are an essential part of the way in which Ḥumirīs regard close and positive social relationships. Therefore appreciation of this ideology is of the greatest importance to understand the nature of these relationships and their crucial role in Ḥumirī social organisation.

Chapter 9. A formal approach to kinship and spatial distance as factors in localised Ego-centred interaction⁵⁴

9.1. Introduction: randomness, kinship and spatial distance

In the previous chapters we have seen how an anthropological analysis of Humiri social organisation along the lines of the participant's *emic*, indigenous, conscious, descent-based societal ideology, leads to insurmountable contradictions, because of the inconsistencies in the participants's application of their views in actual social practice and in explicit commentary. It is time to reverse the perspective and to approach Humiri social organisation in the first place from an *etic* perspective of analyst-imposed formal concepts and relationships. The present chapter therefore presents a totally different approach: a method to measure the impact of kinship and spatial distance upon localized ego-centred interaction networks. Operationalization, data processing, statistics and mathematical formalization are treated in detail. A method is offered to make kinship ties accessible to quantitative analysis, and to isolate the effect of kinship from spatial distance. The method is applied to empirical

⁵⁴ Strictly speaking, this chapter, in its present, more or less achieved, form was never part of my 1970 thesis; it summarises, formalises, and amplifies results contained there in various appendices which were suppressed as such, and incorporated in the main text, in the present redaction. In the 1970s, I meant to publish this chapter as a separate article, and showed it to Clyde Mitchell (the leading Manchester-School authority on network and quantitative approaches) with whom I had become acquainted in Zambia and Manchester; he responded very positively, but pressing institutional responsibilities and my shifting research focus on sub-Saharan Africa kept me from pursuing final publication until in the present book.

data from Ĥumiriyya, and in fact aims at formulating an abstract model of Ĥumiri social organisation as iformingday-to-day social interaction in that region. After analyzing the spatial distribution of kinsmen, it is shown

- that here, for everyday interaction, *spatial distance is a primary determinant*;
- that kinship an inconsistent, secondary determinant;
- that preference for kinsmen in the ideologically prevailing line of descent does not inform actual interaction to any statistically significant degree;
- and that the entire kindred, rather than the unilineal descent group, is the crucial kinship unit.

Since the Modernist shift towards statistics, mathematical models and information theory, it has become fashionable to define the subject matter of the social sciences as 'the non-randomness in human behaviour'. To characterize the social sciences this definition may be not better than any other one; however it is a suitable starting point for the approach set out in this chapter.

When we measure a certain variable for a certain class of phenomena, this variable can only be said to have a random distribution within the population under study in so far as we do not impose any internal differentiation upon this class. For instance, suppose we are interested in the circulation of physical objects between humans. The number of physical objects exchanged between any two persons of a population may tend to a certain central measure, and any individual case may fit somewhere in a random distribution. However, sociological analysis and explanation only comes in if we differentiate within the class of physical objects (*e.g.* money; birthday present; notebook; breeding stallion) and within the class of humans (specifying series of complementary roles in the process of circulation, such as, respectively: shopkeeper/customer; niece/ uncle; good student/ weaker student; neighbour/ neighbour (both farmers)). Once we have introduced these differentiations, the initial random distribution no longer holds sense. However, as long as we do not introduce any differentiation further than these, we might describe with a random distribution the number of birthday presents circulating within any uncle/niece dyad in our population, or the frequency with which any farmer in our population lends his stallion to his neighbour.

Thus, from one point of view, we try to analyse and to explain behaviour by discovering the relevant differentiations that dissolve randomness into a clearly distinct pattern. In so far as behaviour roots in conscious deliberations of human actors, our task is to understand which distinctions our actors themselves make. But as man is not only a rational, but also a ritual and an emotional animal, the actor's conscious distinctions fall short to explain fully most of his behaviour, and the ethnographer, guided by his formal training and experience, has to apply analytical distinctions that do not wholly coincide with those of the actor.

Let us concentrate on one particular form of human behaviour: social interaction. Three types of distinctions are relevant here: those referring to persons, to physical objects and to situations. Again we limit our scope and just look at the distinctions between persons.

If the interaction of a member (A) of a particular society with any other member (B)

does not follow a random pattern, what major differentiations then govern A's choice of an interaction partner (such as B)? This is one of the core problems in interaction analysis. For some aspects of this problem the present chapter suggests a quantitative approach.

All known societies developed, in the kinship system, a tool by means of which a particular individual (Ego) may differentiate between the persons (not all of them necessarily kinsmen) in his immediate social surroundings. Provided that Ego uses this tool, and uses it in a fairly consistent way, it may well have a major impact on his day-to-day interaction – so that kinship analysis will really add to our understanding. Though, even so we still have to explain kinship itself.

The problems of use, of consistency, and of the explanation of kinship itself, were not often raised by our kinship specialists. Dazzled by the early discoveries of such eminent writers as L.H. Morgan, W.H.R. Rivers and A.R. Radcliffe-Brown, and fascinated by the intricacies and mathematical beauty of exotic kinship systems, the anthropologists mostly contented themselves to believe in kinship, not only as the main, universal and automatic differentiating criterion in interaction, but even as the main unifying and moral force within society.

Of course, I do not deny that this tradition produced a considerable number of very fine studies, that, even though formulated in the jargon of kinship theory, have general significance within the social sciences.⁵⁵ On the other hand, this classical anthropological tradition seems to be far over its zenith, and it is easy to cite a number of first-rate publications where this tradition is skillfully attacked.⁵⁶

The contribution this chapter wishes to make is slightly different. Unless we assume that the bulk of anthropological writing has been written by liars and pretenders, we are brought to formulate as a working hypothesis that in many societies kinship is an important, to some extent independent determinant of actual social interaction: making people choose certain interaction partners rather than others. This hypothesis should be subject to quantitative testing, first within one society, then for a large number of societies, while we should try to account for possible differences between these societies.

Even confirmation of our working hypothesis will leave space for a number of major and independent determinants of social interaction, other than kinship. To assess, then, the relative impact of kinship, we must test kinship against these other determinants: power, wealth, age, gender, spatial distance etc. of the persons involved. The problem with all these determinants is that, to a high extent, they overlap with kinship: someone can very well be, at the same time, my kinsman and my superior in power, or my kinsman and my neighbour.

Suppose A prefers interaction with B (both his neighbour and his kinsman) to interaction with C (neither his neighbour nor his kinsman). How are we to explain this non-randomness in A's behaviour?

Classical anthropology would, most likely, attribute the preference primarily to

⁵⁵ *E.g.* Fortes 1945, 1949; Murdock 1949; Lévi-Strauss 1949, Homans & Schneider 1955.

⁵⁶ *E.g.* Worsley 1956; Leach 1968; Mitchell 1969a.

kinship, and might moreover try to show that kinship tends to bind kinsmen by both moral ties and self-interest in such a way as to minimize spatial distance between kinsmen. It is however possible to weigh the determinants involved in a less a priori way.

It is for precisely these two determinants of kinship and spatial distance that the quantitative method set out here tries to solve the problem of weighing two determinants of interaction one against the other.

Instead of spatial distance I prefer the term *social distance*: 'the social aspects of the spatial distribution of people'. Sometimes, distinguished social scientists have claimed *social distance* to be of very great importance for the structure of human interaction.⁵⁷ Moreover *social distance* was often called in as an additional explanation for all kinds of empirical data on interaction, from African villages to London townships. But on the whole, the social sciences paid little systematic attention to *social distance*, leaving its investigation to geography (*e.g.* Zipf 1949; Ollson 1965).

The problem of the relation between *social distance* and kinship rises especially when we analyze domestic and economic interactions occurring between localized households within a limited area. This type of interactions is crucial in traditional rural societies, and remains important in industrial, urban societies. What is the *social distance* of interaction partners? What is the *social distance* of kinsmen? Do kindred neighbours interact because they are neighbours, or because they are kinsmen? Is there an 'automatic' preference to interact with kinsmen, even if this means a relative loss of time and effort, non-kinsmen being available closer-by? In general, how far does kinship really account for the non-randomness we find in interaction?

When tackling these problems with the present method, the unit of analysis will be an individual head of household, Ego, finding himself in the centre of three ego-centred networks:

- As a member of his localized household Ego has a certain, fixed *social distance* to all individuals (*i.e.* their dwellings) in the population under study.
- Ego has real or fictitious kinship ties, of types to be specified, with a number of other individuals in the same population.
- Ego has interaction, of types to be specified, with a number of other individuals in the same population.

The method presented here comprises: a classification of *social distances*; a classification of genealogical ties; an outline of processing methods and statistics by means of which the impact of *social distance* and genealogical relationship can be measured with regard to any specific kind of interaction – starting with an individual Ego who has, or has not, this interaction with a particular other member of the population, and gradually moving on to an abstract 'average Ego' (reflecting a representative sample of Egos from the population) in interaction with abstract 'average other members' of the population, – to end up with mathematical equations.

⁵⁷ *e.g.* Maine 1861: 128 *f.*; Kroeber 1939; Radcliffe Brown 1940: xiv; Festinger, Schachter & Back 1950.

The approach owed something to the developments in the study of so-called ego-centred kindred (Mitchell 1963) and of ego-centred networks in general.⁵⁸ Alternately, this chapter may form a contribution to the already remarkable degree of sophistication and quantification attained in these fields.

After presenting the method itself, I shall apply it to empirical data from rural North Africa. this yields a few conclusions that may have relevance both to the factual description of this relatively little known area, and to the general theory of kinship (kindreds) spatial distance and ego-centred networks.

9.2. Classifying spatial distance

In the easiest case, the location of any household in the area under study can be ambiguously described by two co-ordinates on a horizontal plane – the earth’s surface. This is a common situation in almost all rural areas, and in some urban townships. The occurrence of more-storey houses and apartment buildings complicates the network of spatial distance by introducing a vertical co-ordinate; however we shall not dwell on the practical problems this creates in measuring spatial distance, but concentrate on the horizontal case.

When we make a fairly detailed map (*e.g.* 1: 5,000) of the area we can easily measure the distances between all the houses. These distances we classify. In this chapter, rather arbitrarily, we take 0 – 25 m. as the first class, while all subsequent classes have a width of 50 m. Distance classes will be referred to as DC.

These distances have only an apparent precision. For in so far as spatial distances are socially relevant, we should not measure them as the crow flies, but rather take into account the precise layout of paths, streets, the relative difficulty of the terrain, natural and man-made barriers, the effect of the projection of slopes onto a horizontal plane, etc. However, the distortion produced by these factors is usually so insignificant that it does not justify the enormous amount of work involved in a more precise approach.

The DCs form circle rings, concentric around an inner circle with a radius of 25 m, and with Ego’s house in the centre. As Table 9.1 shows, the area of these rings decreases towards the interior.

DC	radius in m. (upper boundary)	area (.104 * π m ²)
1	25	0.06
2	75	0.50
3	125	1.00
4	175	1.50
5	225	2.00
etc.		

Table 9.1. The area of distance classes (DC)

⁵⁸ *E.g.* Mitchell 1969b; which book also contains an extensive bibliography.

If the houses are roughly evenly distributed over the earth's surface, the observed frequencies of certain phenomena in certain DCs can never be directly compared to the corresponding observed frequencies in other DCs: we have to realize that the more interior DCs (because of their smaller surface) cannot but contain less houses than the more peripheral ones. On the other hand, if we study small rural villages, or relatively isolated townships, the existence of uninhabited terrain at the periphery tends to lower the frequencies in the more remote DCs.

9.3. Interaction

All members of the population other than Ego are considered to be Ego's potential interaction partners (PIP). All persons who are recorded to have been in a particular interaction with Ego at least once during the time in which the empirical data is collected, are considered to be Ego's day-to-day interaction partners (AIP) with regard to this interaction. To facilitate the analysis, interaction of others than householders are ascribed to their respective head of household. To validate the analysis, the type of interaction to be selected should be sufficiently important within the local social structure; moreover, it should not present great difficulties to investigate by observation and interviews. Often various types of interaction may be combined, provided that in the society under study these interactions form expressions of the same overall type of relationship. For instance, in many rural social systems formal visits to one another's house, mutual assistance in agriculture, and domestic co-operation of women, imply one another, and can be combined for the purpose of our analysis. In all cases we have to make sure whether two persons A and B who are simultaneously present at the same place really directly chose one another as interaction partners: their presence might be coincidence, due to their common but independent choice of the same third partner, C; in the latter case we should not count the event as an interaction between A and B.

9.4. Classifying kinship ties

When we try to assess the importance of kinship as a determinant of interaction, three problems arise:

- In all societies, the extent of precise genealogical knowledge is limited
- In many societies, *genealogical knowledge can be manipulated*, purposely or unintentionally, *in order to conceal discrepancies between an indigenous kinship ideology (e.g. 'as descent from our common ancestress is the condition for dwelling here, all inhabitants of our village are matrilineal kinsmen') and the actual situation (e.g. where genealogical research shows that most inhabitants are only patrilineally or affinally, if at all, related to the original matrilineal core of the village).*

'Manipulation of kinship ties', meanwhile, can have two meanings which, though related, need to be distinguished:

- the actor's differential use of particular, essentially unchallenged, kinship ties to

serve certain individual purposes in, *e.g.*, the economic or political field (*cf.* Velsen 1964);

- the actor's invention of factually non-existent ties, and his negation of factually existing ties, in order to harmonize indigenous kinship ideology with the structure of day-to-day interaction. My emphasis is on the latter type of manipulation.]
- In addition to the narrower kinship units (minimal lineage, kindred) where genealogical reckoning is precise, difficult to manipulate (because founded in the first-hand knowledge of too many people) and hence more or less historically correct, many societies have wider kinship units (maximal lineages, clans, phratries) the main function of which appears to be: to allow for a fictitious genealogical formulation for contemporary, structural relationships (*e.g.* in the field of marriage and of politics). Clearly, in case of this genealogical manipulation kinship cannot be said to be an independent determinant of day-to-day interaction: it is rather the effect.
- Closely related to the previous points, there is *the 'consensus problem' of kinship*: the kinship tie between two persons can only be independently relevant for their day-to-day interaction, if both persons agree on the existence of such a tie between them, and, approximately, on its content. Kinship cannot be invoked to explain day-to-day interaction unless it creates in the actors involved a common frame of reference within which they can mutually identify as kinsmen and can adopt particular kinship roles.

Therefore, in this chapter, we should limit our analysis to those degrees of kinship in which there is a precise and consensual knowledge about the genealogical relationship between the people involved. Naturally, the closer the kinship tie between two persons, the less likely that the tie will be manipulated or that there will be no consensus about it. For the purpose of our analysis we concentrate on these relatively short kinship ties.

Methods to establish these genealogical relationships are extensively described elsewhere.⁵⁹ The problem should be tackled seriously, and patiently. The ethnographer must discover, on the spot, what type of genealogical information is likely to be distorted, manipulated or withheld by the informants, and must collect a wealth of genealogical data, from the contradictions in which the actual kinship ties can be reconstructed.

Every genealogical tie is a chain consisting of an ordered selection out of the following basic elements: F, B, S, D, Z, M, W, H. When these elements occur in various numbers, the amount of possible permutations is astronomically large, even if we neglect the longer ties. In order to master these data I tried to devise an acceptable system by which a great number of different chains could be put into one and the same category.

Two important aspects of kinship ties, in many kinship systems, are:

- The length of the chain (= the number of elements in the chain).

⁵⁹ *Cf.* references in Jongmans & Gutkind 1967: 246.

- The 'degree of non-unilinearity'. By this I mean the number of times that, in a given chain, we can, on formal grounds, 'On formal grounds,' *i.e.* without using any other information than the chain itself contains. For it is possible that two persons linked by a chain that shows non-unilinearity, in fact belong to one and the same lineage, as would be demonstrated by another chain that is equally applicable; see Fig. 2. conclude to a transition to a different lineage. The concept of non-unilinearity is only relevant if we apply the present method to societies in whose kinship system a prevailing line of descent is explicitly recognized, so that we can distinguish between, say, patrilineal kinsmen and non-patrilineal (matrilineal and affinal) ones.

Our method enables us to assess whether kinship in the prevailing line has, as such, any special impact on day-to-day interaction. In the North African society that I shall discuss below patrilineal kinship is, at least ideologically, very much emphasized; in order to prepare for this discussion, throughout this chapter we shall pay attention to the degree of non-unilinearity.

In a society that is formally patrilineal, non-unilinearity occurs in a kinship tie at the following elements in the chain (the dash indicates the link with other elements in the chain): -M, M-, Z-, D-, -W, -W, H-, -H; in short: in the case of a woman's offspring, and in case of a marriage. Likewise, in a formally matrilineal society non-unilinearity occurs at the following elements: F-, -F, B-, S-, W-, -W, H-, -H; in short: in the case of a man's offspring, and in the case of a marriage.

If we call the length of the chain: k , and the degree of non-unilinearity: l , then (in just these respects) any kinship tie can be described by an ordered pair: (k, l) . (Where $k \geq 1$; for the degree of non-unilinearity cannot exceed the number of elements in the chain). For instance: under a patrilineal system, BWB = $(3, 1)$ and HZDS = $(4, 3)$; under a matrilineal system FBDS = $(4, 3)$ and MBS = $(3, 1)$. For the sake of brevity, henceforth illustrations will be limited to formally patrilineal societies.

The next step is to take together kinship ties that have the same characteristic, even if these ties differ as to the nature and the order of the elements involved. This procedure is somewhat questionable: we overlook the undeniable differences between, *e.g.*, the following ties: BWB vs. FZD, both having $(2, 2)$. However, generational differences somewhat limit the range of actual chains: it would be very unlikely that, for a certain Ego, both the tie with his FF and that with his SS (both having $(2, 2)$) are simultaneously relevant – either the FF is already dead, or the SS does not yet participate in adult life.

Now we have a number of categories of chains, each category with its own characteristics. The final step is to combine a number of these categories, provided that they are close enough to one another with regard to k and l .

For the purpose of this combination, it seems impossible to give universal rules that apply to all known societies. Application to our North African society however, shows that the following considerations are relevant:

- Where does indigenous practice, or even indigenous theory, put the boundary between people who are 'still kinsmen', and those who are already 'too remote'? This consideration affects both length of chain and degree of non-unilinearity.

In most societies, Ego's FFFFBSSSSS has very little chance of being recognized, in precisely this link, by Ego; and neither has Ego's MZSWFMBSBHBS. Because of genealogical manipulation and the consensus problem, many societies will have no clearly defined boundary here, but the researcher will be able to estimate the k and l values of these boundaries on the basis of his own familiarity with genealogical knowledge and day-to-day interaction between remote kinsmen, in the society under study.

- Is genealogical knowledge about kinsmen in the prevailing line of descent (e.g. patrilineal) as limited as that about other kinsmen? If the former is considerably greater, this implies that, at a certain length of chain, kinship in the prevailing line might still be relevant for day-to-day interaction, while other kinship ties are not any longer. Thus non-unilinearity in a chain, in addition to length of chain, imposes a constraint upon the relevance of kinship.

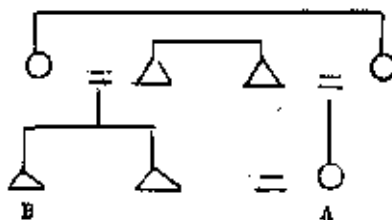
Still assuming that we are dealing with a formally unilineal society, it is useful to reduce all kinship ties to the following categories: close kinsmen in the prevailing line of descent (CP); remote kinsmen in the prevailing line of descent (RP); close kinsmen (both consanguinean and affinal) not in the prevailing line of descent (CNP); remote kinsmen not in the prevailing line of descent (RNP). The categories CP, RP, CNP, RNP jointly make up Ego's kindred (K). The chains that do not fall within these categories refer to persons who, with regard to Ego, should be called non-kinsmen (NK); here we might sometimes be able to trace some link, but one of such length and complexity that it cannot be effective at all for day-to-day interaction.

		length of chain								
		1	2	3	4	5	6	7	8	etc.
lineage alienation	0	close agnates		remote agnates						
	1	close cons./ affines		remote			non-kin			
	2		cogn./ affines							
	3				non-kin					
	4									
	5							non-kin		
	etc.									

Table 9.2. A classification system for kin relations

Table 9.1 shows how, for our North African society, the above considerations lead to a particular choice as to the boundaries of the CP, RP, CNP and RNP categories.

Obviously, for societies where the formal features and the actual practice of the kinship system differ much from our present example, other choices should be made.



- A = B's FBD (3,0; close agnatic)
- = B's Bwi (2,1; close consanguineal / affinal)
- = B's MZD (3,2; remote consanguineal / affinal)

Fig. 9.2. An example of the classification of kinship chains

The concept of kindred in this chapter follows the approach of W.E. Mitchell, as for including affines in the kindred (1963: 351), and for the basic insight

‘that the kindred as an Ego-oriented network of kin, and the extended kin group as a bounded corporate unit have different system-references and cannot be compared as mutually exclusive variations abstracted from the same order of social relations’ (1963: 351).

The concept of non-unilinearity does not permit us to distinguish between consanguinean kinsmen in the non-prevailing line of descent, and affines. As long as we are dealing with a formally unilineal system this is no great disadvantage, I think. In such a system the set of Ego’s unilineal ties in the prevailing line of descent – and then it is, at least formally, only of minor consequence whether Ego shares with these other people ancestors in the non-prevailing line with these other people ancestors in the non-prevailing line (if so, they are his consanguines in the non-prevailing line; if not, they are his affines). In a bilateral system the distinction between consanguines and affines is likely to be important; in that case the present method should be adapted.

Although some problems of detail remain, a scheme like Fig. 1 enables us, for a certain society, to reasonably classify all possible kinship ties into a very limited number of categories, as a basis for further quantitative analysis.

Finally we must pay attention to those cases where between two persons more than one kinship tie can be traced. (Fig. 9.2 gives an example. For these complications (they are common in many societies, *e.g.* N. Africa), the following decision procedure can be used:

- First reject those chains that lead to a remoter kinship category than any one of the other chains (*i.e.* prefer all other chains to one leading to NK; and prefer CP and CNP, jointly, to RP and RNP).
- Secondly, if still more than one chain is left, choose the one in the prevailing line of descent.

A disadvantage of the second step in this decision procedure is that types of kinship ties that are not really mutually exclusive, yet are treated as such; *e.g.* as Fig. 9.2 shows,

somebody can be Ego's CP and CNP at the same time, but he will only be counted as CP. I adopted this procedure in order not to complicate the analysis too much, its deficiency shows clearly in section 8. On this point the method should be further improved.

9.5. Problems for investigation

With the data prepared on the basis of the preceding sections, we are now able to investigate the following series of related problems:

- a. The connection between spatial distance and the recruitment of AIP.
- b. The spatial distribution of kinsmen in the various categories.
- c. The connexion between distance and frequency of interaction.
- d. The connexion between kinship and the recruitment of AIP
- e. The connexion between kinship and frequency of interaction.

All these problems can be investigated with a representative sample of Ego's, drawn from a complete list of all householders in the population (area) under study. For problem (a) we look how each Ego in our sample selects his AIP among the PIP (= all householders) within each DC. For problems (b), (d) and (e) it is necessary to trace all the kinship ties between each Ego in the sample and all other householders in the population – an extremely complicated and time-consuming task which demands a sample that is not too large. For problems (c) and (e) we should have recorded a considerable number of interactions for each Ego in the sample. Of course, we might use different samples for the various problems. Although I did this when applying the present method to a North African society, we shall disregard this complication as irrelevant to the methods of processing the data to be described now.

9.6. Methods of data processing

The sample consists of M Egos, any one of these being represented as E_m . E_m dwells in a certain place somewhere in the area under study. There is a fixed spatial distance between the house of E_m and the house of any other head of household (A_n) in the population, at a certain time. The total number of householders (including M Egos) is N ; so E_m has $N-1$ different A_n . The distance between E_m and A_n falls into one of our DCs. Now with regard to E_m , A_n shows a number of characteristics that together constitute the social relationship between A_n and E_m . Within the narrow limits of our analysis, these characteristics are defined by:

$$E_m R A_n \dots\dots\dots (9.1)$$

Here R is a triadic logical relation, defined by the ordered triple:⁶⁰

⁶⁰ Logical relations, ordered n -tuples, and their obvious applicability to the study of social relationships will not be discussed here, as they get extensive treatment in many modern introductory books on mathematical logic (e.g. Suppes 1958; Lipschutz 1966).

$$(p,q,r) \dots\dots\dots (9.2)$$

where

- p = rank number of An's DC with regard to Em; p runs from 1 to J (J being the total number of DCs in our analysis).
- q = code for An's kinship category with regard to Em; q can take the values: CP, RP, CNP, RNP, K or NK.
- r = number of day-to-day interactions recorded between An and Em; if $r \geq 0$, An is Em's AIP.

As every E_m occupies a different spatial, genealogical and interactional position within the population, the most complicated part of the analysis is to derive, from the rough data, the values of p, q and r for each E_m vis-à-vis each An. When N and M are large, the use of an electronic computer is advisable. However, once these $M.(N-1)$ ordered triples for the empirical values are available, we can relatively easily derive from them a set of variables whose numerical values adequately provide a quantitative description of the impact of kinship and spatial distance upon the ego-centred interaction system under study.

Thus for the relation $R(p,q,r)$ the following countable items can be derived and can, per DC, be counted: PIP, AIP, CPPIP (= CP among PIP; etc., for RP, CNP, RNP, K (= CP+RP+CNP+RNP), NK, CPAIP (CP among AIP; etc. for RP, CNP, RNP, K, NK), FAIP (= number of recorded interaction with AIP), FCP (= number of recorded interactions with CP among AIP; etc. for RP, CNP, RNP, K and NK).

Processing methods then consist of:

- Formal counting procedures, for which a special counting function is introduced below: functions (3) and (4).
- Procedures (5) to (25), to derive from the numerical values obtained under (a) statements about the population as a whole. Here we introduce the statistical constructs of 'aggregate Ego' (E_{com}) and 'average Ego' (E_{av}). Statements in terms of E_{com} are based on the sum total of certain measurements made for each one of the E_m s in the sample; whereas the corresponding value for E_{av} is the one for E_{com} divided by M. Both constructs can be considered as reasonably reflecting general tendencies in the total population. The actual use of these concepts will be made clear in the course of this section.

The general form of the counting function is:

$$\partial X_{m,j} \dots\dots\dots (9.3)$$

where X is the item we are counting (e.g. PIP), and m and j identify E_m and the j-th DC. As we count per DC with regard to a certain E_m , we must exactly specify the conditions under which $\partial X_{m,j}$ obtains a certain value. These conditions and values are outlined in 4(a-f).

With regard to E_m , for any A_n $\partial X_{m,j}$ takes the indicated values under the following

specified conditions

$$\begin{aligned}
 X = \text{PIP}; \partial \text{PIP}_{m,j} = 1 \text{ iff } p = j \\
 \} \dots\dots\dots (9.4a) \\
 = 0 \text{ iff } p \neq j
 \end{aligned}$$

$$\begin{aligned}
 X = \text{AIP}; \partial \text{AIP}_{m,j} = 1 \text{ iff } p = j \text{ and } r \neq 0 \\
 \} \dots\dots\dots (9.4b) \\
 = 0 \text{ iff } p \neq j \text{ or } r = 0
 \end{aligned}$$

$$\begin{aligned}
 X = \text{F}; \partial \text{F}_{m,j} = r \text{ iff } p = j \text{ (N.B. } r \geq 0) \\
 \} \dots\dots\dots (9.4c) \\
 = 0 \text{ iff } p \neq j
 \end{aligned}$$

$$\begin{aligned}
 X = \text{CPPIP}; \partial \text{CPPIP} = 1 \text{ iff } p = j \text{ and } q = \text{CP} \\
 \} \dots\dots\dots (9.4d) \\
 = 0 \text{ iff } p \neq j \text{ or } q \neq \text{CP}
 \end{aligned}$$

Similarly for RP, CNP, RNP, K and NK

$$\begin{aligned}
 X = \text{CPAIP}; \partial \text{CPAIP} = 1 \text{ iff } p = j \text{ and } q = \text{CP} \text{ and } r = 0 \\
 \} \dots\dots\dots (9.4e) \\
 = 0 \text{ iff } p = j \text{ or } \text{CP} \text{ or } r = 0
 \end{aligned}$$

Similarly for RP, CNP, RNP, K and NK

$$\begin{aligned}
 X = \text{FCP}; \partial \text{FCP} = r \text{ iff } p = j \text{ and } q = \text{CP} \text{ (N.B. } r \geq 0) \\
 \} \dots\dots\dots (9.4f) \\
 = 0 \text{ iff } p \neq j \text{ or } q \neq \text{CP}
 \end{aligned}$$

Similarly for RP, CNP, RNP, K and NK.

Let us now look how these procedures lead to quantitative statements (in terms of Ecom or Eav) related to the problems listed in section 5.

9.6.1. The connexion between distance and the recruitment of AIP (Actual Interaction Partners)

From (4a) it follows that for E_m the total number of PIP in the j -th DC from E_m is:

$$\sum_{n=1}^{N-1} \partial \text{PIP} = \text{PIP}_{m,j} \dots\dots\dots (9.5)$$

The corresponding number of AIP is:

$$\sum_{n=1}^{N-1} \partial \text{AIP}_{m,j} = \text{AIP}_{m,j} \dots\dots\dots (9.6)$$

Therefore for E_{com} the number of PIP in the j -th DC is:

$$\sum_{m=1}^M \text{PIP}_{m,j} = \text{PIP}_j \dots\dots\dots (9.7)$$

Whereas the corresponding number of AIP is:

$$\sum_{m=1}^M \text{AIP}_{m,j} = \text{AIP}_j \dots\dots\dots (9.8)$$

In view of the area problem discussed in section 2, we have to divide AIP_j by PIP_j for each DC:

$$\text{AIP}_j / \text{PIP}_j \dots\dots\dots (9.9)$$

It will be noticed that (9) equally applies to E_{av} , as

$$(\text{AIP}_j / M) / (\text{PIP}_j / M) = \text{AIP}_j / \text{PIP}_j \dots\dots\dots (9.10)$$

Form (9) gives the best estimate for the AIP/PIP ratio in the j -th DC_m for the average member of the population. From the different values of (9) for the various DCs the impact of spatial distance upon the choice of AIP can be easily assessed.

Curve based on actual data (*cf.* Fig. 3), as well as certain theoretical consideration (*cf.* Zipf 1949, Ollson 1965, Cherry 1955: 100 *f.*, 209 *f.*) suggest that the AIP/PIP ratio as against distance can be reasonably described by an exponential function of the general type:

$$y^{X_1/X_2} = \alpha e^{-x/\beta} \dots\dots\dots (9.11)$$

where:

- y = the proportion under analysis (here: AIP/PIP)
- X₁, X₂ = the countable items under analysis (here AIP and PIP respectively)
- x = distance (km)
- e = basis of natural logarithms (= 2.72)
- α = a parameter
- β = a parameter (km)

In order to find the numerical value of the parameters α and β (which may be different for each problem, and per problem for each population) we calculate, by means of the well-known method of least squares (*e.g.*, *cf.* Crow, Davis and Maxfield 1960: 152 *f.*), the regression of:

$$y'(X_1/X_2)_j = b \cdot x_j + a \dots\dots\dots (9.12)$$

where:

- y'(X₁/X₂)_j = ln y'(X₁/X₂)_j in the j-th DC (Ecom)
- b = 1/β (km⁻¹)
- a = ln α
- x_j = middle of the j-th DC (km)

When in (11) the calculated parameters are substituted, we shall have abstracted from distance classification, and shall be able to predict directly the size of the proportion y for any distance within the J-th DC (and possibly, by extrapolation, also for greater distances).

Thus we have acquired a fair measure for the impact of spatial distance upon the recruitment of day-to-day interaction partners.

Meanwhile, new assessment of the empirical data and of the theoretical literature on gravity models made clear that:

1. Contrary to current spatial and sociological use, there are no sound theoretical reasons to apply the exponential approach in this case. The exponential approach is suitable for the case of one-dimensional extension (*e.g.* interaction between localized households all sited along the same road), but on the two-dimensional plane we should theoretically get a Bessel function (various references from spatial, physical, physiological and mathematical literature).
2. Curve fitting of the Bessel function onto existing numerical data is a solved problem.
3. The empirical Hūmiri data to be described further in this chapter marvelously fit the Bessel approach, much better actually than the exponential approach; the best fitting Bessel functions have already been computed.

4. Differential equations underlying the Bessel function have been formulated and have been given a sociological interpretation. Binsbergen, W.M.J., & Rijn, H.E. van, forthcoming, A note on the significance of Bessel functions for the analysis of distance and human action. However, these new developments do not affect the present argument too much; just substitute 'Bessel function' for exponential function, throughout.

9.6.2. The spatial distribution of kinsmen

We consider, per DC, the ratio between the total number of householders (= PIP), and the number of kinsmen, in a certain category, among these PIP. I shall discuss CP; the procedure is similar for the other kinship categories.

From (4b) it follows that for E_m the total number of CP in the j -th DC from E_m is:

$$\sum_{n=1}^{N-1} \partial CP_{m,j} = CP_{m,j} \dots \dots \dots (9.13)$$

The corresponding number for E_{com} is:

$$\sum_{m=1}^M CP_{m,j} = CP_j \dots \dots \dots (9.14)$$

For E_{com} (and likewise for E_{av}) the ratio CP/PIP in the $j = th$ DC is:

$$CP_j / PIP_j \dots \dots \dots (9.15)$$

From the different values of (15) for various DCs the spatial distribution of CP can be directly assessed. Smoothing the distance classification into a continuous variable, the ratio (15) can be approximated by a Bessel function, analogous to what is discussed in the previous sections.

9.6.3. The relationship between distance and frequency of interaction

Per DC we consider the ratio between number of interactions recorded and the number of interaction partners. From (4c) it follows that the total number of interactions recorded for E_m in the j -th DC from E_m is:

$$\sum_{n=1}^{N-1} \partial F_{m,j} \dots \dots \dots (9.16)$$

The corresponding value for E_{com} is:

$$\sum_{m=1}^M F_{m,j} = F_j \dots\dots\dots (9.17)$$

The ratio interactions/AIP is, for E_{com} in the j-th DC:

$$F_j/AIP_j \dots\dots\dots (9.18)$$

Smoothing the distance classification into a continuous variable, this ratio (18) can also be approximated by a Bessel function.

9.6.4. The relationship between kinship and recruitment of AIP

This problem is more complicated than the previous ones. I shall discuss the total kindred ($q = K$); the approach is similar for the other kinship categories.

From (4d) it follows that for E_m the total number of K in the j-th DC from E_m is:

$$\sum_{n=1}^{N-1} \partial K_{PIP_{m,j}} = K_{PIP_{m,j}} \dots\dots\dots (9.19)$$

The corresponding value for E_{com} is:

$$\sum_{m=1}^M K_{PIP_{m,j}} = K_{PIP_j} \dots\dots\dots (9.20)$$

Likewise, from (4e) it follows that for E_m the total number of K among AIP in the j-th DC from E_m is:

$$\sum_{n=1}^{N-1} \partial K_{AIP_{m,j}} = K_{AIP_{m,j}} \dots\dots\dots (9.21)$$

The corresponding value for E_{com} is:

$$\sum_{m=1}^M K_{AIP_{m,j}} = K_{AIP_j} \dots\dots\dots (9.22)$$

Now we can cast Table 9.3:

	K	NK	total
PIP	KPIP _j	PIP _j - KPIP _j	PIP _j
AIP	KAIP _j	AIP _j - KAIP _j	AIP _j

Table 9.3. K and NK among PIP and AIP in the j-th DC (E_{com}).

The problem is: does Ecom in the j-th DC choose more K as his AIP, than is to be expected on the basis of the occurrence of K among PIP in this DC? See table 3:

	K	NK	total
expected number of AIP	$(KPIP_j/PIP_j)*AIP_j$	$(PIP_j - KPIP_j)/PIP_j*AIP_j$	AIP _j
observed number of AIP	KAIP _j	AIP _j - KAIP _j	AIP _j

Table 9.4. Expected and observed number of K and NK among AIP in the j-th DC (E_{com}).

When in Tables 9.3 and 9.4 empirical figures are substituted, these are bound to be affected by chance fluctuations. We need, therefore, apply a statistical test in order to decide whether Ecom shows a preference to interact with K (as against NK).

For problems of this type a usual test is the χ^2 test. However, this test has the disadvantage that for each cell a minimum expected value of 5 is required. This requirement is absent in the l' test (Spitz 1961; cf. Woolf 1957) which in all other respects is equivalent to the χ^2 test. The formula for the l' test is:

$$l' = 2 \sum_{i=1}^I g_i \ln (g_i/h_i); \text{ df} = I - 1 \dots\dots\dots (9.23)$$

where:

- I = number of columns in the contingency table (except the 'total' column)
- g_i = number of observed in the i-th column
- h_i = number expected in the i-th column
- ln = natural logarithm
- df = number of degrees of freedom

For the statistical interpretation of the value of l' in terms of the associated chance of such a value we consult a χ^2 table.

Thus we are able to solve the above problem. Per DC the statistical test gives the result 'yes' or 'no'. The analysis has to be repeated for each DC and there is, of course, no point in smoothing the distance classification into a continuous variable. On the contrary, the purpose of calculating the statistic per DC is to keep, per DC, the distance factor constant so that we can isolate the kinship factor as such.

A related problem is whether, within the kindred, there is a marked preference to interact with certain kinship categories. For instance, in many societies kinship ideology claims a preference for interaction partners from the prevailing line of descent (CPAIP+RPAIP). For this case, Table 9.5 gives the relevant distinctions:

	CP+RP	CNP+RNP	entire K
PIP	CPPIP _j +RPPPIP _j	CNPPPIP _j +RNPPPIP _j	KPIP _j
observed number of AIP	CPAIP _j +RPAIP _j	CNPAIP _j +RNPAIP _j	KAIP _j
expected number of AIP	(CPPIP _j +RPPPIP _j)/KPIP _j *KAIP _j	CNPPPIP _j +RNPPPIP _j /KPIP _j *KAIP _j	KAIP _j

Table 9.5. Expected and observed number of AIP among certain kinship categories, in the j-th DC (Ecom).

The statistical testing is as discussed for table 3 (23). Of course, according to the hypothesis to be tested, other kinship categories can be chosen as row entries.

Whereas for the previous problems E_{com} and E_{av} were equivalent, this is by no means the case here. From (23) it is clear that the I' value produced for E_{com} is M times greater than the one for E_{av} . Thus, although the choice between E_{com} and E_{av} is to some extent arbitrary, the former is much more prone to produce significant results. If E_{com} does not produce significant results, then these will be all the more absent for E_{av} .

9.6.5. The connexion between kinship and frequency of interaction

The approach is similar to the one for the previous problem. I shall only discuss the entire kindred (K). From (4f) it follows that, for E_m , the total number of interactions in the j-th DC from E_m is:

$$\sum_{n=1}^{N-1} \partial FK_{j,m} = FK_{j,m} \dots\dots\dots (9.24)$$

The corresponding value for E_{com} is:

M

$$\sum_{m=1} FK_{j,m} = FK_j \dots\dots\dots (9.25)$$

The question is whether E_{com} , in the j -th DC, has significantly more recorded interactions with those among his AIP who belong to his K , than is to be expected on the basis of the occurrence of K among his AIP, in this DC. Table 5 gives the relevant distinctions.

	K	NK	total
AIP	K_{AIP_j}	$AIP_j - K_{AIP_j}$	AIP_j
expected number of interactions	$(K_{AIP_j}/AIP_j)*F_j$	$(AIP_j - K_{AIP_j})/AIP_j)*F_j$	F_j
observed number of interactions	F_{K_j}	$F_j - F_{K_j}$	F_j

Table 9.6. Expected and observed number of interactions for K and NK among AIP in the j -th DC (E_{com}).

The statistical testing is as discussed for Table 9.3 (23)

Having discussed our formal method in this section, we can now demonstrate its uses by applying it to empirical data from Ḥumiriyya.

9.7. The Ḥumiri highlands]

Ḥumiriyya is an area with narrow valleys and steep slopes, covered with forests. The population speaks an Arabic dialect and confesses a popular version of Islam. The people live concentrated in villages, surrounded by fields, pastures and forests. The density of population is 60 inhabitants/km². Animal husbandry and the cultivation of cereals, vegetables and tobacco – and, in addition, unemployment relief work – provide a usually very small income.

In this marginal agricultural economy no household can entirely rely on its own: members of different families assist one another in agricultural and domestic work, lending each other money and foodstuffs, and rendering each other many other services. It is only with a limited number of the surrounding families (within the same and adjoining villages) that a certain family maintains this type of co-operative relationship, for which the Ḥumiris use the word *meziyya* ('pleasure', 'service') (cf. Jongmans 1968, 1971). Where such a relationship exists, it implies a whole range of observable interactions: visiting one another's house; working together in the fields; fetching water, firewood and other forest products together; chatting with one another on the road, etc. For the purpose of our analysis, recorded interactions of all these types can be taken together. The *meziyya* relationship is a typical contractual one, fitting very well Foster's (1961, 1963) ideal type of the dyadic contract; it can be freely initiated and terminated by the partners, without loss of prestige or honour on either side. However, some *meziyya* relations develop into a much more inclusive and ideally permanent relationship: the relationship of *metasrin* ('faithful ones'), where the

partners, irrespective of their actual genealogical relationship (they may be kinsmen or not) are supposed to maintain among themselves the ideal Ḥumirī norms of the relationship between real brothers. This implies: eating together (the recognized ritual basis for this relationship); unconditional solidarity; protection of one another's honour, women, house and domestic animals; and the desire to consolidate the relationship by a marriage between close relatives of either partner. In so far as everyday interaction is concerned, the *metasrin* relationship is only a sub-type of the *meziyya* relationship, and there is no need to distinguish between the two when processing the data along the lines of our method.

It is not only *metasrin* relationships that Ḥumirīs regard as (fictitious) kinship relations. In fact, they claim that all interaction (except conflict) results from (ideally patrilineal) kinship, and on the other hand assume *meziyya* relationships to exist between all kinsmen. The usual explanations given by informants when asked why they had entered into a *meziyya* relationship with a certain person, are: 'he is my brother', 'we have the same forefather', 'we belong to the same house, the same root'. Often the informant fails to produce the precise genealogical link, on further questioning; in other cases the link produced reveals the 'brother' as a remote matrilineal or affinal relative.

Even so, statements like the above always have a very strong suggestion of patrilineal kinship. The occurrence of marriages between close patrilineal kinsmen (so that patrilineal, matrilineal and affinal relatives to some extent coincide) adds to this suggestion – although the frequency of such marriages is lower than Ḥumirīs themselves seem to believe. Ideologically, the patriliney is the prevailing line of descent in Ḥumirī society. A man belongs primarily to his father's family, and lives ideally on land inherited from his father and father's father and so on until the original, mythical ancestor who was, allegedly, the first inhabitant of the valley and from whom virtually all present-day inhabitants of the valley are supposed to be patrilineal descendants.

Thus, the indigenous conception of society centres around the following credos: 'I live here because my (patrilineal) forbears have always lived here', 'my neighbours are my (patrilineal) kinsmen, and the closer-by they live the closer is their patrilineal kinship tie with me', 'I interact with ('help', 'work with', 'go with') people because they are my (patrilineal) kinsmen'. In the indigenous conception, patrilineal kinship, spatial proximity and intensive interaction merge.

Here I shall not go into the details of how Ḥumirīs reconcile their societal ideology with the reality of modern Ḥumirī society. This reality includes: frequent migration (so that every Ḥumirī family is involved in a process of spatial dispersion of its members), constant influx of others than patrilineal kinsmen, and the frequent acquisition of land by other means than patrilineal inheritance. Some other aspects of Ḥumirī social reality, as contrasting with Ḥumirī ideology, will show in the next sections.

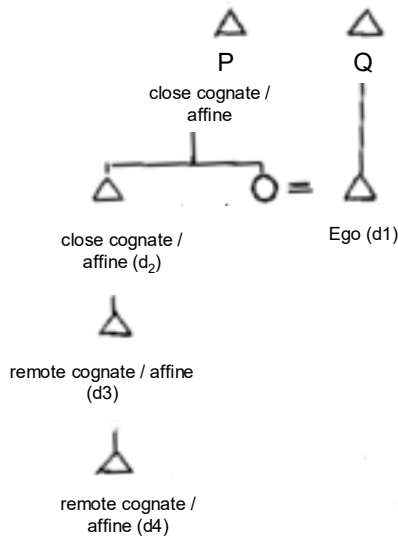


Fig. 9.3. An example of the problematics adhering to the geographic distribution of remote cognates / affines

An important mechanism in the reconciliation between reality and ideology is the manipulation of genealogical knowledge: Ḥumirī statements about existing genealogical ties are extremely opportunist and reflect the momentary structure of day-to-day interaction within the village, rather than historical truth.

9.8. Application of the method to Ḥumirīyya

Intensive genealogical data collection and analysis enabled me to overcome the difficulties created by the informants' genealogical manipulation, and to produce a reasonable correct and complete picture of the genealogical ties in the two adjoining villages where the data on interaction were collected: The villages of Sidi Mḥammad and Mayzīyya, 6 km. NNE. of the small town of ʿAin Drāham, which is the regional centre of Ḥumirīyya.

The boundary between the two villages is rather uncertain and is not important in daily interaction; so the 68 households in both villages together can be treated as one continuous population ($N = 68$). As the distance between any two houses in the area of the two villages was very rarely more than 825 m, I used 17 DCs ($J = 17$). To make the best use of the limited empirical data on interaction, I had to use three different samples of Egos:

- *Sample I.* $M = 68$; the sample comprises all householders, and is used for the

calculation of AIP_j/PIP_j and yAIP/PIP.

- *Sample II.* M' = 15; an a-select sample of 15 Egos, with replacement drawn from the 68 householders; it is used for all calculations, except where samples I or III are used.
- *Sample III.* M'' = 4; a sample of those 4 householders for the largest number of interactions was recorded; this sample is only used for the analysis of frequency of interaction, and in view of its deficiencies results based on this sample are unreliable.

In the symbolism introduced in section 6, the application of our method to Hūmirī society yields the results summarized in tables 6 and 7. The meaning of these results will be discussed in section 9.

sample	1	2		3
		exponential function	associated rS†)	
I	yAIP/PIP	= .5*e ^{-x} /.3	.87	1
II	yCP/PIP	= .3*e ^{-x} /.2	.74	2
	yRP/PIP	= .03*e ^{-x} /4.8	.21	3
	yCNP/PIP	= .1*e ^{-x} /.3	.84	4
	yRNP/PIP	= .2*e ^{-x} /.4	.78	5
	yK/PIP	= .6*e ^{-x} /.3	.95	6
	yNK/PIP	= 1 - .6*e ^{-x} /.3	.95	7
III	yF/AIP	= 2.4*e ^{-x} /.8 (for x ≤ .7)	.52	8

†) r_S_{N=17; 5%} = .41, so all these rank correlations are significant except ,21

Table 9.7. Exponential functions (and associated rS values) derived from empirical data on interaction in Hūmirī.

The formulae in Table 9.7 are but rough approximations of the empirical values. The approximation would have been better if we had more and better data. The data available deny us the use of confidence intervals, mainly because DCs are chosen by the researcher instead of being stochastically distributed. We could consider if any other approximation than these exponential functions would be more satisfactory. Fig. 9.3 shows that for AIP_j/PIP_j the approximation with an exponential function seems

quite justified. But this seems not to be the case with RNP_j/PIP_j , also included in the Fig.; here the empirical curve oscillates heavily and reaches its maximum only at 225 – 275 m. (instead of 0 m.) That $\gamma RNP/PIP$ fits so badly seems to stem mainly from the decision procedure discussed in section 4: in the case of multiple ties many people who were, according to one of these ties, RNP, were only counted as CNP, RP or CD, so that the RNP category is mutilated. The CNP category suffers from the same discrimination (not shown in Fig. 3). The other functions in table 6 fit considerably better than $\gamma RNP/PIP$.

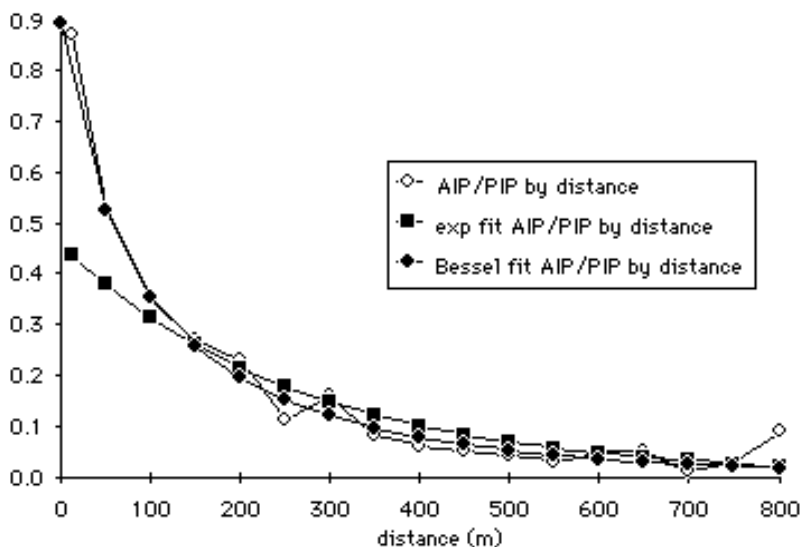


Fig. 9.4. AIP_j/PIP_j as approximated by an exponential function

Whatever be the best approximation of the empirical curves, that the suggested connexions between spatial distance and certain other variables are not spurious can easily be demonstrated by administering a simple rank correlation test upon each series of empirical values $(X_1/X_2)_j$ as against distance (j); cf. Siegel n.d.: 202 f. The results of this procedure are also shown in table 9.6. All r_s values are significant (5% level) except for RP_j/PIP_j .

In Table 9.7, column 3, the preference for interaction partners who are kinsmen in the prevailing line of descent (as against $CNP+RNP$) is investigated without distinguishing between close and remote kinsmen. Pooling of close and remote kinsmen here turned out to be justified on inspection of the empirical data: both for patrilineal kinsmen and for non-patrilineal kinsmen, respectively, the ratio of close kinsmen (as against remote kinsmen) among AIP in nearly all DCs fairly reflected the corresponding ratio among PIP in the same DC. The available data show no preference for CP over RP, neither for CNP over RNP, in the choice of AIP. This facilitates our analysis of preference for the

prevailing line of descent.

9.9. Discussion of the Ḥumiri results

What do the formulae and figures in Table 9.6 and 9.7 actually tell us about the structure of interaction in Ḥumiri? They form the necessary steps in an argument that can be summarized as follows.

If we want to analyse the impact, both independently and in combination, of kinship and spatial distance upon interaction (in a localized ego-centred network), we could first look at the connexion between spatial distance and interaction. Interaction involves the choice of interaction partners, and the frequency of interaction with these partners, once chosen. Table 6, row 1 respectively 8, gives the relevant data. The 'average Ego' preferably chooses his AIP among his very near neighbours, so that the proportion of people with whom he interacts (among the total number of PIP) rapidly decreases as distance increases – even though at greater distances the 'supply' of PIP is substantially larger (*cf.* section 2). Moreover Ego interacts more frequently with his near AIP than with the more distant ones. Thus distance is one major factor governing (*i.e.* imposing non-randomness upon) everyday interaction in Ḥumiri. Elsewhere I tried to explain this fact from the interplay between two principles: the tendency (perhaps universal) towards economizing effort (including distance) in human interaction (*cf.* Zipf 1949, and the limited degree of social and economic differentiation between Ḥumiri householders (so that Ego, in most cases, could as well interact with the nearest). However, several qualifications are needed at this point:

1. As Mandelbrot has shown, we do not need Zipf's magical 'principle of least effort' to explain the kind of exponential curves Zipf, and many sociologists, geographers and economists after him, found empirically; the curve automatically springs from a few basic assumptions of mathematical information theory – not including Zipf's principle.
2. Replacing the exponential approach by the much better fitting and theoretically much better founded Bessel approach makes reference to Zipf irrelevant.
3. When we identify the sociological principles underlying the differential equations of which the Bessel function is the solution, we do not find something equivalent to the Zipf principle, but much simpler mechanisms of flow and change of flow of socially relevant items (goods, migrants, attention, services).
4. The second point, that the system can only work if we can overlook differentiation (social inequality) between the humans involved, remains valid.

Turning now to the impact of kinship upon interaction, we have to realize that kinsmen, took live at a certain distance from Ego, so that at least part of the interaction between Ego and his kinsmen might be explained, not so much by reference to kinship, but on the basis of sheer spatial distance. In how far is kinship really a distinct, determinant in interaction, imposing non-randomness independent from spatial distance?

To answer this question we should first look at the spatial distribution of kinsmen in

the various kinship categories. Table 9.6, rows 2 to 7, gives the relevant data. These are not in accordance with the indigenous ideology. It turns out that the 'average Ego' is nowhere, not even immediately near his own house, exclusively surrounded near at hand by patrilineal kinsmen: there are always non-patrilineal kinsmen, and non-kinsmen. Moreover, though the majority of close patrilineal kinsmen live relatively near to Ego, many do not – whereas remote patrilineal kinsmen (RP) do not even show a statistically significant tendency to live spatially near to Ego. Kinsmen other than in the prevailing line of descent show, just like close patrilineal ones, the tendency to cluster around Ego's house. On the whole Ego finds a greater proportion of kinsmen nearby than further-off. The explanation for this includes: norms and practice of land acquisition favourable for dwelling on paternal land, *i.e.* in close proximity of one's brothers; but leaving plenty of room for dwelling elsewhere); structural conflicts within the family (due to which most Ĥumiri families disperse when the sons come of age: opposition to paternal or fraternal control, discord over inheritance, and conflicts between mothers and in-marrying daughters-in-law make some members remove, often to become neighbours of other fellow-villagers – not necessarily close kinsmen – with whom, for the time being, they enjoy better relations); the tendency for metasrin to become neighbours (thus reshuffling the spatial distribution of kinsmen); the tendency for metasrin to become marriage partners (thus adding to the connexion between distance and affinal, respectively matrilineal, kinship).

It is among the people living relatively near to him that Ego chooses the bulk of his interaction partners. So our dilemma is: if Ego selects his interaction partners on the basis of proximity, he cannot help to include a fair number of kinsmen among them; if he selects on the basis of kinship, he cannot help to include near neighbours among them – so what is the relative importance of kinship and spatial distance?

We can solve this dilemma by keeping the distance factor constant, in the way discussed in section 6. Table 9.7, column 2, reveals then that Ego, when choosing interaction partners, does not show a particular preference for kinsmen (in the four kinship categories that together constitute his kindred): in most DCs the results are non-significant.

How to account for the five non-significant exceptions?

The fact that no significance occurs in the first three DCs (0 – 125 m., where still nearly 40% of Ego's PIP are NK) is in accordance with the qualitative observation that near neighbours tend to be metasrin. Their proximity makes intensive co-operation interaction workable, and even necessary (for no Ĥumiri would like to expose his honour, house, womenfolk and domestic animals to a near neighbour he cannot trust). If near neighbours do not succeed in establishing and maintaining fairly good relations, one of them is likely to move.

The distribution of the significant exceptions over the DCs does not reveal a statistically recognizable pattern (runs test, *cf.* Siegel n.d.: 52 *f.*). In general, we can say that in the more peripheral DCs, where the number of Ego's AIP decreases although the number of PIP increases, Ego uses (under certain conditions, that are not always fulfilled, and that have to be identified by further research) kinship as an additional principle to guide him in choosing AIP out of this large, and in other respects rather unstructured, supply.

We conclude that norms and claims as regarding kinsmen do play some role in the choice of interaction partners, but their effect is limited, and by no means automatic and all-overriding (if it were, we would have significant results in nearly every DC). As a determinant of everyday interaction in Ḥumirī, kinship appears to be secondary to spatial distance.

The results in table 7, column 3, demonstrate that we were right in pooling patrilineal and non-patrilineal kinsmen. In contrast to the indigenous ideology, Ego does not show (with one negligible exception) any statistically significant preference for patrilineal kinsmen, when he chooses AIP from amongst those PIP that belong to his kindred.

The inconsistent preference for kinsmen, where shown in table 7, column 2, cannot be attributed to kinsmen in the prevailing line of descent alone: all kinship categories contribute to it. The entire kindred, and not the unilineal descent group, turns out to be the proper kinship unit to analyze the impact of kinship on everyday interaction in Ḥumirīyya.⁶¹

This is in line with other aspects of the Ḥumirī social organisation, such as the marriage system (where kindred endogamy, rather than patrilineal endogamy, is the crucial feature, in so far as kinship is a determinant of mate selection, the inheritance of land; and the succession to high religious office.

In the literature on Arab, including North African, societies, patrilineal descent groups have always been overemphasized. Murphy & Kasdan (1959, 1967) exposed this bias and tried to demonstrate (unfortunately by a rather sterile, aprioristic argument) that in actual fact bilateral kin groups are more important here. My own, quantitative analysis points into the same direction: however obscured by the (both Ḥumirī and anthropological!) ideology of unilineal descent groups, Ḥumirī kinship structure (if it is at all important for actual everyday interaction) 'hinges' on the total kindred (*cf.* Campbell 1963, 196..)

Although in Ḥumirī society kinsmen may sometimes be preferred over non-kinsmen as interaction partners, the scanty information in table 7, column 4 (based as it is on a deficient sample), suggests (with one negligible exception) that, once chosen as interaction partners, there is no statistically significant preference for Ego to interact more frequently with kinsmen than with non-kinsmen. Kinship does not appear as an independent determinant of frequency of everyday interaction in Ḥumirī.

9.10. Conclusion

As a reaction to the blind belief in kinship among our predecessors in the discipline, many anthropologists today have lost all interest in kinship studies. Others (structur-

⁶¹ This, of course, is not to deny that in time of extreme need, and in life-cycle crises (birth, circumcision, marriage, death) the consanguineal, and especially the patrilineal, kinsmen can play a specific role, according to kinship norms that are enforced by both secular and supernatural sanctions. [**is dat in de hoofdstekst wel voldoende benadrukt ?**] Jongmans (1971) claims moreover that the recruitment of factions in Ḥumirī local politics mainly follows patrilineal lines.

alists and 'ethno-scientists') takes the more ritualistic path and study kinship as a self-contained, highly formalized symbolic system whose relevance for day-to-day interaction remains out of scope. Many others, fortunately, explore with every increasing sophistication the empirical connexions between kinship and other aspects of social organisation; it is here that the formal, quantitative method set out in this chapter might make some contribution, pertinent as it is to some of the crucial problems of kinship analysis: how can we approach, quantitatively, kinship and its relevance to interaction; is kinship an independent, and primary, determinant of interaction; how can we investigate the interplay between kinship and other determinants of interaction (*e.g.* spatial distance)?

The relation between kinship and spatial distance (or 'locality' or 'spatiality') is a central theme in a great number of descriptive studies of localized interaction systems from all over the world; the problem really deserves systematic attention, *e.g.* along the lines suggested in this chapter. However, it should be clear that the present method, even if very much improved, does not explain kinship and spatial distance as such, but merely their effects on limited aspects of interaction.

As the application to Ĥumirī shows, our method yields interesting systematic and, above all, falsifiable insights into the structure of interaction in a certain society. A further use is suggested by the Ĥumirī case, where the Bessel functions in table 6 will form the basis for a mathematical model of the Ĥumirī marriage system. Finally, when applied to different societies, the method may produce fascinating comparative data.

Chapter 10. Modelling Humiri social life: A mathematical approach to the analysis of geographic distance in interaction

in collaboration with Henny E. van Rijn

10.0. The Bessel function as a tool for quantitative distance analysis in the social sciences⁶²

10.0.1. Introduction

For many years the social sciences have been dealing with quantitative data concerning the relation between distance (or propinquity) and various fields of human action: migration, mate selection in marriage, face-to-face interaction in localized groups, transport, and activities involving central place in the commercial, administrative, educational, medical, recreational and religious spheres. While the methodological problems involved in *collecting* such data should not be underestimated, the main problems here appear to lie in the field of analysis and interpretation, and particularly concern:

- The identification and calculation of the mathematical functions which best fit

⁶² This chapter was conceived in the 1970s on the basis of the materials contained in my 1970 and 1971 theses. It was authored by me, largely applying mathematical and theoretical insights deriving from the late lamented biophysicist Henny E. van Rijn, my first wife (1936-2019). Therefore the latter has been adduced as co-author of this chapter. Responsibility for misinterpreting Henny's views remains mine.

the empirical data; and

- The formulation of a general theoretical basis for the application of a particular mathematical function rather than another, to the research problem involved; without such a theory the mathematical result remains *ad hoc* and can hardly be interpreted or appreciated within the mainstream of more discursive social-scientific notions. With such an interpretation, however, the mathematical models become more than just an awkward (that is, for non-numerate social scientists) way of stating the obvious: if the variables and parameters featuring in the formulae can be identified in terms of social-structural phenomena, the mathematical expression acquires heuristic value and will point to structural regularities and relations that otherwise would have escaped sociological scrutiny.

A comprehensive if dated survey of quantitative approaches to gravity models and distance (Olsson 1965) still fairly adequately summarizes the position in this field of enquiry. For curve-fitting, researchers predominantly turn to simple quadratic or exponential functions (Olsson 1965: *passim*), of the general form:

$$y = p x^2 + q x + r \dots\dots\dots (1.1)$$

and

$$y = a e^{-x/b} \dots\dots\dots (1.2)$$

where

- y = a measurement of specific human action
- x = distance (m)
- e = base of natural logarithms, and
- a, b, p, q, r = constants with the appropriate dimensions

Fitted onto empirical curves of distance data, these two types of functions take such parameters that, strictly speaking, their deviation from the empirical data tends to fall within the systematic errors involved to the measurement.

One general weakness of these approaches is, however, that for short-distance contacts they yield expected values which are consistently *lower* than the empirically observed ones (Olsson 1965: 51f).

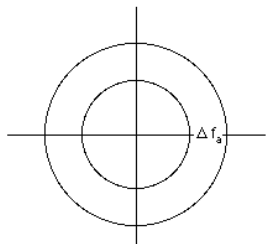


Fig. 10.1. Graphical representation of a two-dimensional interaction field.

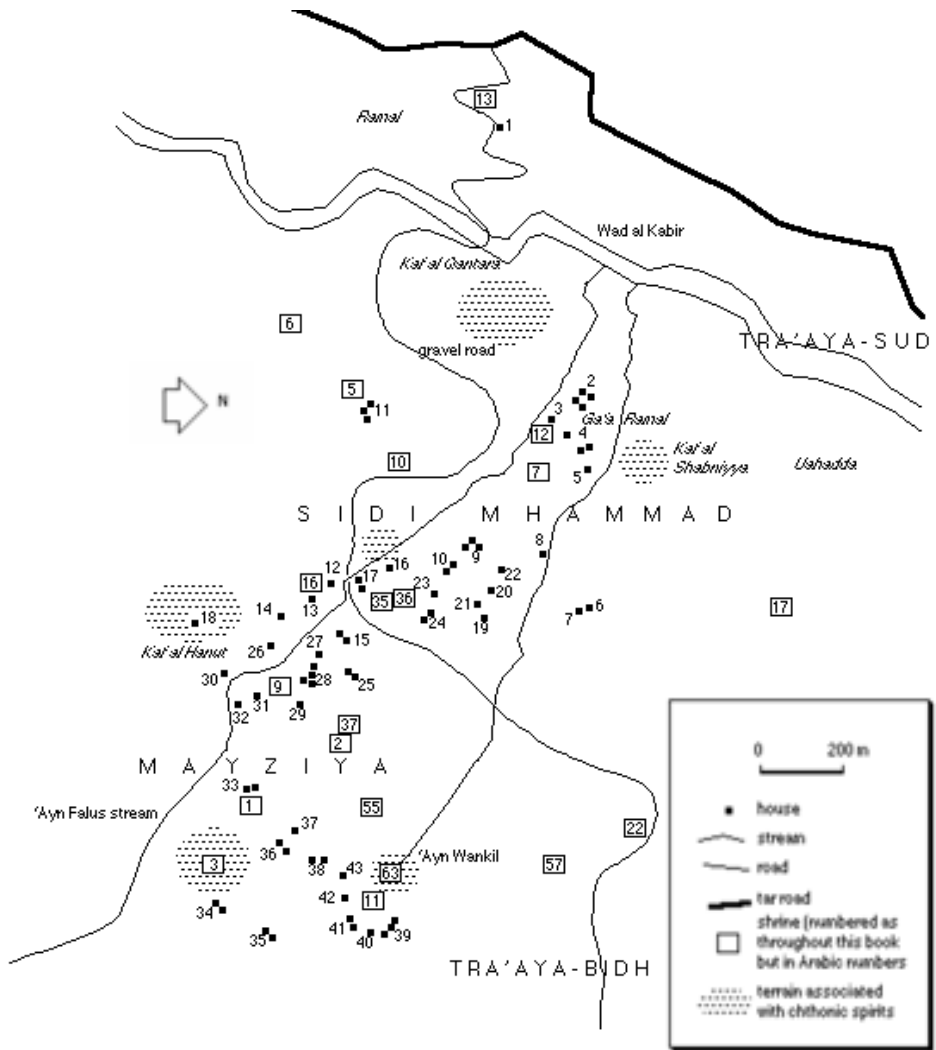


Fig. 10.2. The villages of Sidi Mhammad and Mayziya, 1908

A second, even more crucial weakness is that many authors fail to advance fundamental theoretical reasons for their adopting a particular mathematical function (Olsson 1965: 48f). In particular, when quadratic models (formula 1.1) are adopted for a (fairly common) social action system whose basic form is the emanation, from a central point, of a flow of social elements (goods, services, personnel) in all directions

of a two-dimensional field (Fig. 10.1), it is simply spurious to mathematically represent such a system in terms of quadratic equations; in the physical and life sciences, analogous systems of physical and biological phenomena could never be treated that way, for mathematical reasons outline below.

On the other hand, those authors who propose a theoretical basis for their sociological gravity models, disagree from one another, seldom link up with general, social-scientific theory, and sometimes have been convincingly refuted. A notorious example of the latter is Zipf's (1949) 'principle of least effort', used to 'explain' anything from word frequency to migration and urban ecology, until it was exploded by mathematical information theory (Mandelbrot 1971; Cherry 1957: 102 *f.*, 211 *f.*).

Our aim in this chapter is to propose an alternative mathematical model for sociological gravity problems of this nature: Bessel functions. For this purpose, we present some limited empirical data on distance and face-to-face interaction in a North African village. Upon this data two curves will be fitted (Table xxx.xxx): one of the usual exponential kind (*cf.* formula 1.2), and one Bessel function. The method of calculation will be shown and the goodness of fit of both functions compared. The Bessel function will turn out to fit extremely well, particularly for short-distance contacts, where exponential approaches fail. In the discussion analogies from physics and the life sciences will help to bring out the general, mathematical reasons why Bessel functions are eminently suitable for this type of distance analysis. Here we are proceeding from the level of mathematically-enlightened description to that of sociological interpretation, by assigning a sociological meaning to the parameters and variables of the specific underlying differential equations of which the Bessel function is, after all, merely the mathematical solution.⁶³

10.0.2. Interaction and distance in Ĥumiriyya

One of the authors collected empirical data on interaction between the households in the village of the villages of Sidi Mĥammad and Mayziyya, in the spring of 1968, as part of my more comprehensive study of social organization and religion in the area (van Binsbergen 1970, 1971). The village comprised 70 households ($M = 70$), each located in an immobile, permanent house; the houses were distributed fairly homogeneously over a contiguous area of 1.5 km² (Fig. 10.2).

Types of interaction between two households were selected that were of sufficient significance in the local society: formal visits to the dwelling of another household, mutual assistance in agriculture and in domestic tasks between members of different

⁶³ The following mathematical analysis in the course of this Chapter is predicated on the implicit assumption that, within the village and immediate surroundings, dwelling houses are evenly distributed over the landscape. Fig. 10.2 already on visual inspection brings out that this is an illusion: wherever we choose to place the analytical centre of the village cluster (the vicinity of the shrines 35 and 36 dedicated to the saint Sidi Mĥammad is a plausible position), yet to the south and the north there are far fewer dwellings than to the west and the east. While processing the data, we developed several sophisticated approaches to test the assumed homogeneity of geographic distribution, and when applying statistics to this material found the results indeed negative. However, for the overall picture emerging from this chapter's analysis, this does not make much difference.

households. These types of interaction imply an overall relationship of generalized exchange to exist between the two households involved – a relationship of crucial importance in the local social organisation (Jongmans 1968); the recorded interactions are therefore equivalent and can be added together. Over a fixed period all intra-village interactions that came to my knowledge were recorded. To facilitate the analysis, all interactions of adults were attributed to the heads of their respective households. Naturally, all (heads of) households were *potential interactors* (P) of each other; but only if between two households at least one interaction was recorded, were they counted as *actual interactors* (A). When this raw data was related to the spatial position of households on a detailed map (1:5,000) it became possible to calculate the way in which the selection of people for actual face-to-face interaction relates to distance.

For this purpose a number of simple operations had to be performed upon the raw data. First we classified spatial distance, and numbers the distance classes ($= D_j; 1 \leq j \leq 18$). Taking 0-25 m as the first class, all subsequent classes have a width of 50 m. Thus around any particular household ($= H_m; 1 \leq m \leq M$) the distance classes form circle rings, concentric around an inner circle of radius 25 m. The area of these rings increases towards the periphery.⁶⁴ With the houses distributed evenly over the total area of the village, the more central distance classes will contain fewer houses than the more peripheral ones. Therefore we cannot simply count, per household, A_{jm} ($=$ the number of actual interactors of H_m , dwelling in the j -th distance class from H_m); we have to relate this number of actual interactors to P_{jm} .

An acceptable way to do this is by applying the ratio A_{jm}/P_{jm} . This ratio refers to any individual household H_m . It can be made to reflect the total population, thus suggesting a general social-structural feature, by the statistical construct of the aggregate household (H_{ag}), based on the sum total of the measurements taken for each household in a particular distance class.

Thus for H_{ag} , the proportion of actual interactors out of all potential interactors in D_j is:

$$\sum_m \frac{A_{jm}}{P_{jm}} = (A/P)_j \dots\dots\dots (10.1)$$

The empirical values of $(A/P)_j$ are included in table 2.1, and are represented graphically in Fig. 10.3.

⁶⁴ Notably, if $104 \pi m^2 = F$, then the area of D_1 equals .06 F, that for D_2 .50 F, that for D_3 1.00 F, that for D_4 1.50 F, etc.

distance class	middle of dist. class (km)	y(A/P)			
		empirical data	exponential fitting	Bessel fitting l = 25 n = 26.8	Bessel fitting l = 20 n = 29.8
1	0.0125	0.89	0.437	0.894	0.929
2	0.05	0.53	0.38	0.527	0.521
3	0.1	0.35	0.314	0.352	0.33
4	0.15	0.27	0.26	0.257	0.232
5	0.2	0.23	0.214	0.195	0.168
6	0.25	0.11	0.177	0.15	0.12
7	0.3	0.16	0.146	0.119	0.095
8	0.35	0.08	0.121	0.095	0.0725
9	0.4	0.06	0.1	0.077	0.056
10	0.45	0.05	0.083	0.062	0.043
11	0.5	0.04	0.068	0.05	0.034
12	0.55	0.03	0.056	0.041	0.027
13	0.6	0.05	0.047	0.034	0.021
14	0.65	0.05	0.039	0.028	0.016
15	0.7	0.01	0.032	0.023	0.013
16	0.75	0.03	0.026	0.019	0.0103
17	0.8	0.09	0.022	0.016	0.008
P			0.157	0.009	0.0185

Table 10.1 Empirical data for $y(A/P)$ (i.e. the number of actual interaction partners as fraction of the number of potential interaction partners, per distance class), and fitted exponential and Bessel function, in the village of Bu Lahiyya, 1968

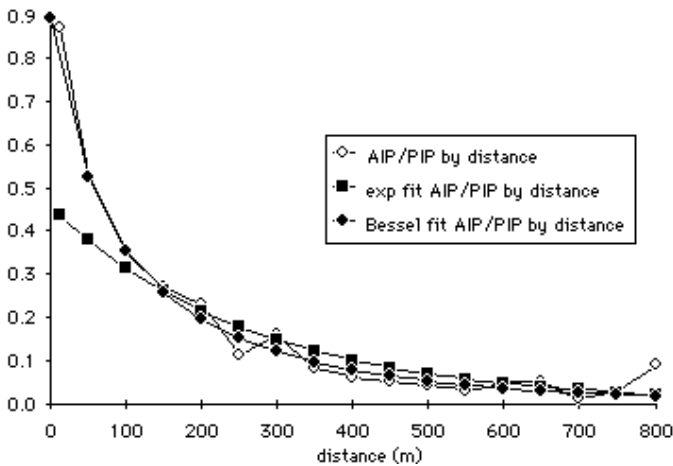


Fig. 10.3. Graphical representation of empirical data, and fitted exponential and Bessel function, for $y(A/P)$ in the village of Bu Lahiyya, 1968

That the suggested relation between (A/P)_j and distance is not spurious can be demonstrated by administering a simple rank correlation test upon the data in table 1 (r_S = -.87 >> r_{S_N} = 17; 5%; Siegel n.d.: 202 f.).

We can now try and fit a function upon the empirical data.

10.0.2.1. Exponential function

By means of the method of least squares squares (Crow et al. 1960: 152f) we calculate the regression parameters a and b out of:

$$Y'(A/P)_j = b x_j + a \dots\dots\dots(10.2)$$

where

- Y'(A/P)_j = ln (A/P)_j
- b = -1/b (km⁻¹)
- a = ln a
- x_j = middle of the j-th distance class (km).
- ln = natural logarithm

This yields as the best fitting exponential function:

$$y_{(A/P)} = a e^{-x/b} = .46 e^{-x/.262 \text{ km}} \dots\dots\dots(10.3)$$

10.0.2.2. Bessel function

By means of a modified method of least squares developed by Henny van Rijn (Jongsma & van Rijn 1972), this yields as the best fitting Bessel function:

$$y_{(A/P)} = n K_0(x/l) = 26.8 K_0 (x/.31 \text{ km}) \dots\dots\dots(10.4)$$

where

- n = K₀ =
- x = distance (m) from aggregate Ego's house
- l = a constant with dimension m⁻¹

10.0.2.3. Goodness of fit

Fig. 10.3 gives a visual impression of the goodness of fit of the two functions fitted. A quantitative measure is

$$p = s / \sum y^2 \dots\dots\dots(10.5)$$

where

- s =
- y = [ws distance]

For the exponential function (2.1.2), p = .16; for the Bessel function (2.2.1), p = .01. The Bessel function has by far the better fit, which is particularly due to its performance for short distances.

10.0.3. Discussion

In the discussion analogies from physics and the life sciences will help to bring out the general, mathematical reasons why Bessel functions are eminently suitable for this type of distance analysis. Finally, since the Bessel function is essentially the solution of a system of differential equations, a tentative attempt will be made to draw up these equations and to interpret them sociologically.

10.1. Further details

10.1.1. Raw data for PIP analysis per kin category

In the following overview table, the acronyms represent the following variables:

AIP = day-to-day interaction partners

PIP = potential interaction partners

CP = close agnates among potential interaction partners

RP = distant agnates among potential interaction partners

CNP = close non-agnates among potential interaction partners

RNP = distant non-agnates among potential interaction partners

K = kinsmen among potential interaction partners = CP + RP + CNP + RNP

NK = non-kinsmen among potential interaction partners

dist. class	mid. of class = x (m)	(a) AIP	PIP	CP	RP	(b) CNP	RNP	K	NK
1	12.5	42	47	25	3	3	10	41	6
2	50	52	98	43	6	6	5	60	38
3	100	57	163	21	13	13	28	75	88
4	150	59	226	25	0	9	54	88	134
5	200	46	200*	26	4	18	18	66	134
6	250	19	173	0	0	9	57	66	107
7	300	32	200*	22	10	16	10	58	142
8	350	18	225*	7	0	19	35	61	164
9	400	15	250*	10	0	3	12	25	225
10	450	17	340*	0	0	11	50	61	279
11	500	8	200*	4	8	4	10	26	174
12	550	15	500*	0	0	10	30	40	460
13	600	9	180*	16	11	0	5	32	148
14	650	21	420*	21	17	0	17	55	365
15	700	5	500*	0	0	0	20	20	480
16	750	6	200*	0	0	0	0	0	200
17	800	10	110*	0	7	0	0	7	103

Table 10.2. Per distance class: (a) day-to-day interaction partners, and (b) various kin categories among the potential interaction partners as a fraction of all potential interaction partners.

10.2. Processing of the data on CP/PIP, RP/PIP, CNP/PIP, RNP/PIP, K/PIP and NK/PIP

D _j	mid	(a)		(b)					
		AIP/PIP	CP/PIP	RP/PIP	CNP/PIP	RNP/PIP	K/PIP	NK/PIP	
1	12.5	.89	.532	.064	.064	.213	.872	.128	
2	50	.53	.439	.061	.061	.051	.61	.388	
3	100	.35	.129	.080	.080	.172	.460	.540	
4	150	.27	.113	0	.041	.243	.396	.604	
5	200	.23	.13	.02	.09	.09	.33	.67	
6	250	.11	0	0	.052	.329	.382	.618	
7	300	.16	.11	.05	.08	.05	.29	.71	
8	350	.08	.031	0	.084	.156	.271	.729	
9	400	.06	.04	0	.012	.048	.1	.9	
10	450	.05	0	0	.0329	.147	.179	.821	
11	500	.04	.02	.04	.02	.05	.13	.87	
12	550	.03	0	0	.02	.06	.08	.92	
13	600	.05	.089	.061	0	.028	.178	.822	
14	650	.05	.05	.040	0	.040	.131	.869	
15	700	.01	0	0	0	.04	.04	.96	
16	750	.03	0	0	0	0	0	1	
17	800	.09	0	.064	0	0	.064	.936	

Table 10.3. Per distance class: (a) day-to-day interaction partners as a fraction of potential interaction partners, and (b) various kin categories among the potential interaction partners as a fraction of all potential interaction partners.

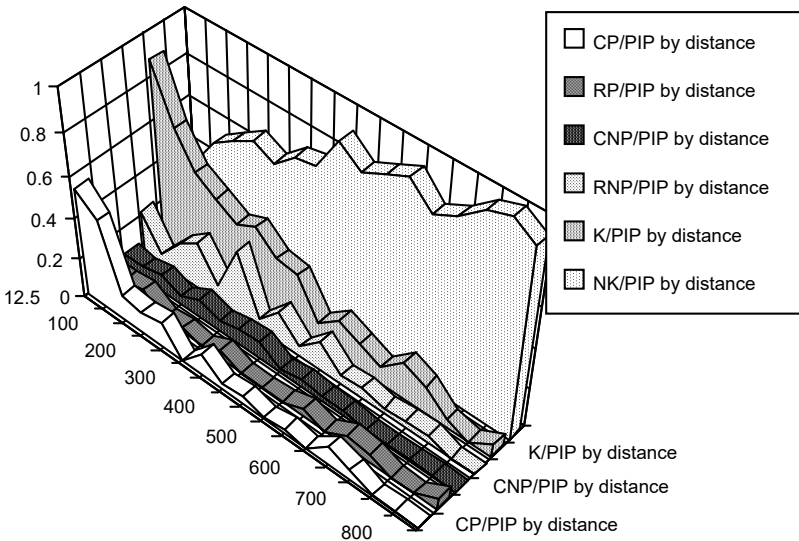


Fig. 10.4. Per distance class: various kin categories among the potential interaction partners as a fraction of all potential interaction partners (empirical data).

10.3.a. Interpolation of estimates for the Bessel function for CP/PIP for coded data

	l'	n'	P
caption needed	16	19.6	.2483
caption needed	20	18.2	.1170
caption needed	22	17.2	.1256
caption needed	25	16.2	.1218

Table 10.4. Various calculated values for l' , n' and P for Bessel curve fitting the distribution over the distance classes of close-agnates, as a fraction of the number of potential interaction partners (coded data).

In other words: you determine the best estimate by making a graph of l' against P . P is a measure for the goodness of fit, and is calculated by means of the method of least squares. The curve has minimum somewhere near $l' = 20$. The curve is shown below (coded lambda)

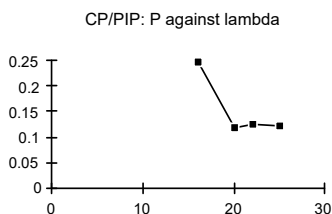


Fig. 10.5. Graphical representation of various calculated values for l' and P towards the Bessel curve fitting for the distribution over the distance classes of close agnates as a fraction of the number of potential interaction partners (coded data).

The corresponding table reads as follows:

data		fitted Bessel functions	
		for $l' = 16$	for $l' = 20$
x'	y' CP/PIP	y'	y'
1	53	54.3	56.5
4	44	30.2	31.9
8	13	18.0	20.8
12	11	12.0	14.1
16	13	8.2	10.3
20	0	5.8	7.6
24	11	4.2	5.8
28	3	3.0	4.4
32	4	2.2	3.4
36	0	1.2	2.7
40	2	.9	2.1
44	0	.7	1.6
48	9	.5	1.3
52	5	.4	1.0

56	0	.3	.8
60	0	.2	.6
64	0	-	.5

Table 10.5. Data, and two fitted Bessel functions for the distribution over the distance classes of close-agnates as a fraction of the number of potential interaction partners (coded data).

As best Bessel function we propose:

$$y'_{CP/PIP} = 18 K_0(x'/20) \dots\dots\dots (10.6)$$

The associated exponential function is:

$$y' = 30 e^{-x'/16}, \dots\dots\dots (10.7)$$

with an associated P value of .224.

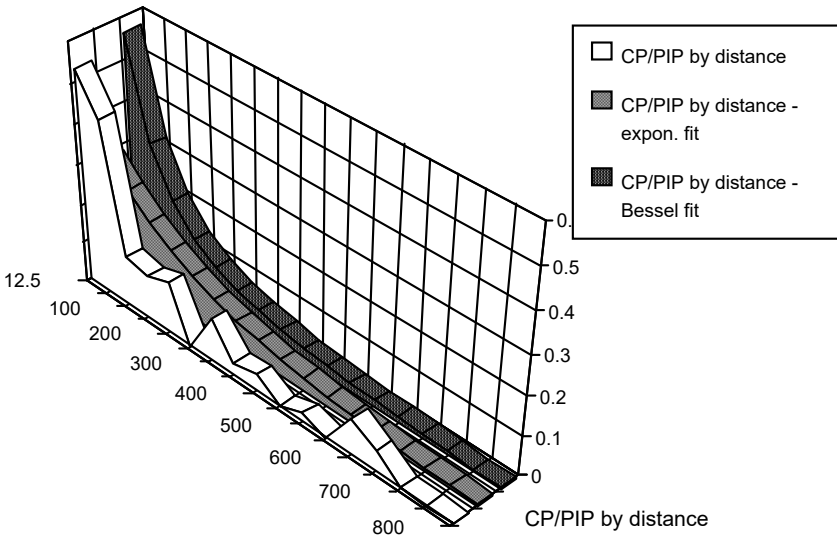


Fig. 10.6. Graphical representation of empirical data, and fitted exponential function and Bessel function, for the distribution, over the distance classes, of close-agnates as a fraction of the number of potential interaction partners.

10.3.b Fitting an exponential and a Bessel function for CP/PIP for non-coded data

The relation between coded and non-coded values is given in table 10.6:

non-coded	coded	function	remarks
x	x'	= x./12.5	dimension of x is in m
y	y'	= y*100	y is a fraction

n	n'	= $n \cdot 100$	in Bessel function; $0 \leq n \leq 1$
l	l'	= $l/12.5$	in Bessel function; order of magnitude of l : 10^2
a	a'	= $100 \cdot a$	in exponential function, a is a constant, $0 \leq a \leq 1$
b	b'	= $b/12.5$	in exponential function, b is a constant; order of magnitude of b: 10^2
P	P		$0 \geq P \geq 1$]

Table 10.6. Relation between code and non-coded values of variable used in distance analyses

10.3.1. The Bessel function

The associated table reads:

data		fitted Bessel functions	
distance		for $l = 200$	for $l = 250$
x	$Y_{CP/PIP}$	y	y
12.5	.53	.543	.565
50	.44	.302	.319
100	.13	.180	.208
150	.11	.120	.141
200	.13	.082	.103
250	0	.058	.076
300	.11	.042	.058
350	.03	.030	.044
400	.04	.022	.034
450	0	.012	.027
500	.02	.009	.021
550	0	.007	.016
600	.09	.005	.013
650	.05	.004	.010
700	0	.003	.008
750	0	.002	.006
800	0	-	.005

Table 10.7. Data, and two fitted Bessel functions, for the distribution, over the distance classes, of close-agnates as a fraction of the number of potential interaction partners (non-coded data).

l	n	P
200	.196	.2483
250	.182	.1170
275	.172	.1256
312.5	.162	.1218

Table 10.8. Various values as calculated for l, n and P, for the Bessel curve fitting of the distribution, over the distance classes, of close-agnates as a fraction of the number of potential interaction partners (non-coded data).

In other words: one determines the best estimate by making a graph of l against P. P is

a measure for the goodness of fit, and is calculated by means of the method of least squares. The curve has minimum somewhere near $l = 250$. The interpolation curve is shown below:

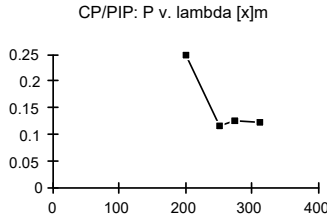


Fig. 10.7. Graphical representation of various values as calculated for l and P towards the Bessel curve fitting of the distribution, over the distance classes, of close agnates as a fraction of the number of potential interaction partners (non-coded data).

As best Bessel function we propose:

$$y_{CP/PIP} = .18 K_0(x/250) \dots\dots\dots (10.8).$$

10.3.b.2. The exponential function

The associated exponential function, is, without coding

$$y = .30 e^{-x/200} \dots\dots\dots (10.9)$$

This function yields the estimated fractions y , provided x is measured in meters.

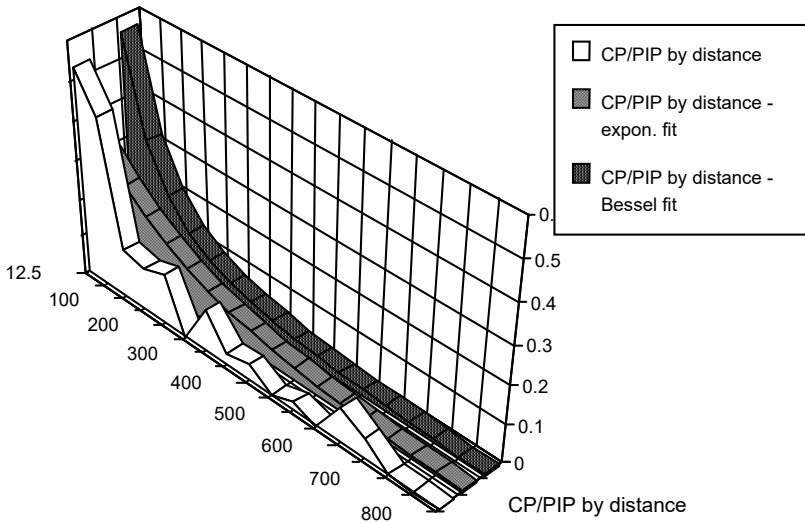


Fig. 10.8. Graphical representation of empirical data (non-coded), and fitted exponential function and Bessel function, for the distribution, over the distance classes, of close agnates as a fraction of the number of potential interaction partners.

10.4. Bessel fitting method: Coded data

The method consists of the following:

- 1) Make an informed guess of l' , for instance $l' = 20$
- 2) Cast a table:

x'^{65}	y'^{66}	x'/l'	$K_0(x'/l')^{67}$	$K_0^2(x'/l')$	$y'K_0(x'/l')$
1	89	.05	3.11423	9.6970	276.156
4	53	.20	1.75270	3.0610	92.893
etc.					

Table 10.9. Calculation method for fitting a Bessel curve (coded data).

- 3) Now calculate $\sum K_0^2(x'/l')$, $\sum y'K_0(x'/l')$ and $\sum (y')^2$. For table 10.9. this yields:

$$\sum K_0^2(x'/l') = 15.3516$$

$$\sum y'K_0(x'/l') = 457.336$$

$$\sum (y')^2 = 13881.$$

- 4) Now the following equations apply:

$$\sum y'K_0(x'/l')$$

$$n'_1 = \frac{\sum y'K_0(x'/l')}{\sum K_0^2(x'/l')} = 29.79 \dots \dots \dots (10.10)$$

$$\sum K_0^2(x'/l')$$

$$s_1 = \sum (y')^2 - n'^2_1 \sum K_0 \dots \dots \dots (10.11)$$

$$P = s_1 / \sum (y')^2 = \dots \dots \dots (10.12)$$

$$\sum (y')^2 - n'^2_1 \sum K_0$$

$$\frac{\sum (y')^2 - n'^2_1 \sum K_0}{\sum (y')^2} = .0185 \dots \dots \dots (10.13)$$

$$\sum (y')^2$$

One has to repeat the calculation of n'_1 for a number of adjacent values of l' , and determine through interpolation at which point the minimum, *i.e.* most favourable, value of P is reached. The values of l' and n' associated with minimum P are the correct ones. In this way we find the parameters for the basis function:

$$y' = n' K_0(x'/l') \dots \dots \dots (10.14)$$

⁶⁵ Here x is the distance to Ego's dwelling as measured to the middle of the circle ring in question, and coded by multiplication with a factor 1/12.5: first ring $x = 1$ (was 12.5 m), second ring $x = 4$ (was 50 m) etc.

⁶⁶ So here: y^*100 . The data are those of AIP/PIP.

⁶⁷ [Look up in a table of Bessel functions, as provided in Abramowitch etc. [**add reference**]

10.5.a. Fitting $y_{AIP/PIP}$ (coded data)

The calculation was executed as indicated above. The table is as follows:

x'	y'	$x'/20$	$K_0(x'/20)$
1	89	.05	3.11423
4	53	.20	1.75270
8	35	.40	1.11453
12	27	.60	.77752
16	23	.80	.56534
20	11	1.00	.42102
24	16	1.20	.31851
28	8	1.40	.24366
32	6	1.60	.18795
40	5	1.80	.14593
44	4	2.00	.11389
48	3	2.20	.08927
52	5	2.40	.07022
56	5	2.60	.05540
60	1	2.80	.04382
64	3	3.00	.03474
68	9	3.20	.02760

Table 10.10. Per distance class, day-to-day interaction partners as a fraction of potential interaction partners: empirical data and Bessel function as fitted with $\lambda = 20$ (coded data)

$$y' = y * 100; x' = x / 12.5;$$

x'	$y'_{AIP/PIP}$
1	89
4	53
8	35
12	27
16	23
20	11
etc.	

Table 10.11. Per distance class: the number of day-to-day interaction partners as a fraction of the number of potential interaction partners (coded data)

The following values were found for l' , n' and P :

l'	n'	P
10	40.8	.090
20	29.79	.0185
25	26.8	.009
30	24.2	.026

Table 10.12. Various values as calculated for l', n' and P towards the Bessel curve fitting of the distribution, over the distance classes, of day-to-day interaction partners as a fraction of the number of potential interaction partners (coded data).

The graphical representation is given in Fig. 10.9:

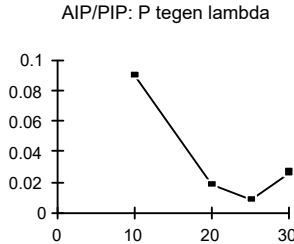


Fig. 10.9. Graphical representation of various values as calculated for l' and P towards the Bessel curve fitting for the distribution, over the distance classes, of day-to-day interaction partners as a fraction of the number of potential interaction partners (coded data).

For coded values, interpolation yields as the best Bessel function:

$$y'_{AIP/PIP} = 26.8 K_0(x'/25) \dots\dots\dots (10.15)$$

The best exponential curve fitting the coded data is:

$$y' = 46 e^{-x'/20.96}; P = .157 \dots\dots\dots (10.16).$$

10.5.b. Fitting yAIP/PIP (un-coded data)

The required data are presented in the following Table:

I. distance class	II. middle of distance class (km)	III. empirical data	IV. standard deviation	V. exponential fitting	VI. Bessel fitting
1	.0125	0.89	0.15	0.437	0.894
2	.05	0.53	0.44	0.380	0.527
3	.1	0.35	0.38	0.314	0.352
4	.15	0.27	0.33	0.260	0.257
5	.2	0.23	0.26	0.214	0.195
6	.25	0.11	0.16	0.177	0.150
7	.3	0.16	0.25	0.146	0.119
8	.35	0.08	0.17	0.121	0.095

9	.4	0.06	0.15	0.100	0.077
10	.45	0.05	0.16	0.083	0.062
11	.5	0.04	0.13	0.068	0.050
12	.55	0.03	0.17	0.056	0.041
13	.6	0.05	0.18	0.047	0.034
14	.65	0.05	0.14	0.039	0.028
15	.7	0.01	0.04	0.032	0.023
16	.75	0.03	0.17	0.026	0.019
17	.8	0.09	0.26	0.022	0.016
18	0	0	0	0	0

$r_{S_{LII}} = - 0.87$, significant at the 5% level; $r_{S_{LIII}} = - 0.87$, significant at the 5% level; in other words, there is a demonstrable positive relationship between geographic distance and the size of the fraction of potential interaction partners that are indeed actual interaction partners

Table 10.13. Empirical data, exponential curve and Bessel curve as fitted, for the fraction of day-to-day interaction partners among potential interaction partners, per distance class.

Fig. 10.10. Empirical data on $y_{AIP/PIP}$, per distance class, with the entire confidence interval = twice the standard deviation above and below the empirical curve

We conclude that the average Ego prefers to choose his interaction partners close to his home, so that the fraction of the people with whom he interacts (as a fraction of the total supply of potential interaction partners) rapidly decreases with increased distance – even though at greater distances the supply of potential interaction partners is exponentially larger (because the area of the distance classes increases with distance).

The calculation of the associated Bessel function is executed as described above. The table has the form:

x	$y_{AIP/PIP}$
12.5	.89
50	.53
100	.35
150	.27
200	.23
250	.11
etc.	

Table 10.14. Per distance class, the number of day-to-day interaction partners as a fraction of the number of potential interaction partners (non-coded data)

The following values were found for l, n and P:

l	n	P
125	.408	.090
250	.2979	.0185
312.5	.268	.009
375	.242	.026

Table 10.15. Various values as calculated for l, n and P towards the Bessel curve fitting for the distribution, over the distance classes, of day-to-day interaction partners as a

fraction of the number of potential interaction partners (non-coded data).

The following Figure is a graphical representation:

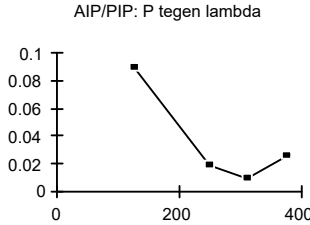


Fig. 10.11. Graphical representation of various calculated values for I and P towards the Bessel curve fitting for the distribution, over the distance classes, of day-to-day interaction partners as a fraction of the number of potential interaction partners (non-coded data).

From this we interpolate as the best Bessel function for coded values:

$$y_{AIP/PIP} = .268 K_0(x/312.5) \dots\dots\dots (10.17)$$

Similar calculations yield the associated exponential curve fitting for non-coded data:

$$y = .46 e^{-x/262}; P = .157 \dots\dots\dots (10.18).$$

The following Fig. shows the associated exponential and Bessel functions as fitted to the un-coded data:

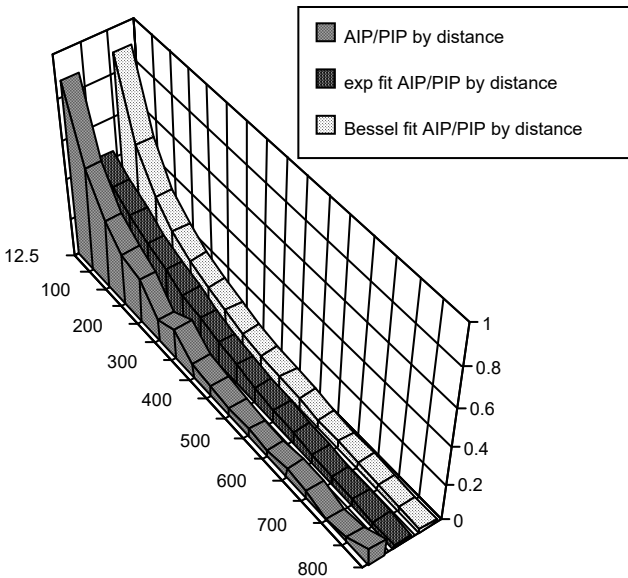


Fig. 10.12. Distribution, per distance class, of day-to-day interaction partners as a fraction of the number of potential interaction partners: raw data (non-coded), exponential function and Bessel function as fitted

As compared with the exponential fitting, the Bessel function has manifestly the better fit vis-à-vis the data!

10.6. The underlying differential equations

Bessel functions, as the results of sophisticated curve fitting, are in themselves the mathematical solutions to differential equations. Our calculations and formulae may offer unexpected insights in the sociology of spatiality, but then only in so far as we manage to draw up the differential equations behind our Bessel functions, and translate these into discursive and recognisable social-science phenomena. Here we take our Fig. 6.1.1 as our point of departure.

10.6.1. Definitions

total supply for ring r

$$A = \dots\dots\dots (18.19)$$

For supply, read: supply of social elements: goods, services, people, etc. The physical equivalent of A is 'current', which is defined as: the passage of charge over time)

$$\frac{A}{2 \pi r p} = \text{supply per person (= 'density of current')} \quad (10.20)$$

where

p = D D r = line density

D = number of residing people/m²

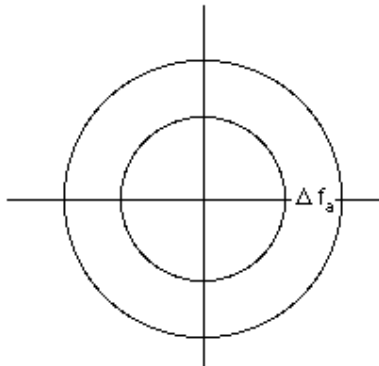


Fig. 10.13. Graphical representation of a two-dimensional field of interaction.

$$f_r = \text{state of excitation per person in ring r} \dots\dots\dots (10.21)$$

d f_r

$$\frac{d}{dr} = \text{field of excitation} \dots\dots\dots (10.22)$$

The excitation 'field' exists between one person in ring r and one person in ring (r+Dr); this is a measure for how does a Ego's interest in a person change with increasing distance.

$$\frac{d}{dr} A = \text{change in total supply between ring r and ring (r+Dr)} \quad (10.23)$$

10.6.2. Generalisations

For any flow taking place in this two-dimensional system, we may posit that

$$\text{change of total supply} = \text{absorption} \dots\dots\dots (10.24)$$

From a physics point of view, this is nothing but the law of the preservation of electrical charge:

$$\frac{d}{dr} A = - 2 \pi p r f_r \dots\dots\dots (10.25, I)$$

density of current is commensurate with field of excitation (II)

$$\frac{A d f_r}{2 \pi p r d r} = - C \dots\dots\dots (10.26,$$

From a physics point of view, this is nothing but Ohm's law, stipulating that the current through a conductor between two points is directly proportional to the voltage across the two points (Anonymous, Ohm's Law').

From II it follows:

$$\frac{d \ 2 f_r \ 1 \ d \ A}{d \ r^2 \ 2 \ \pi \ p \ c \ d \ r \ r} = - \dots\dots\dots (10.27)$$

$$- \frac{1 \ 1 \ d \ A}{2 \ \pi \ p \ c \ r \ d r \ r^2} (\dots - \dots) \dots\dots\dots (10.28)$$

N.B. The first segment of the last term of this equation follows from I:

$$\frac{1 \ d \ A}{r \ d r} = - 2 \ \pi \ p \ f_r \dots\dots\dots (10.29)$$

while from II it follows that:

$$\frac{A - 2 \pi p c d f_r}{r^2 r d r} \dots\dots\dots (10.30)$$

This yields:

$$\frac{d^2 f_r}{d r^2} = \dots\dots\dots (10.31)$$

From this it follows:

$$\frac{d^2 f_r}{d r^2} + \dots\dots\dots (10.32)$$

The solution of the latter equation is the Bessel function:

$$f_r = N f_0 K_0 (r/\sqrt{c}) \dots\dots\dots (10.33)$$

10.7. Mathematical and sociological background of the exponential function

The exponential function applies in cases when we do not have a circular, two-dimensional extension of our system of flow, but instead a band (Fig. 7.1.):

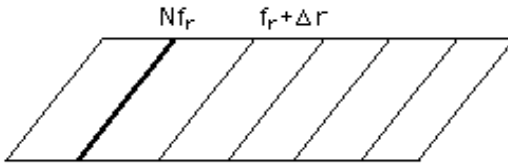


Fig. 10.14. Graphical representation of a one-dimensional field of interaction.

Flow in such a system is to be described as:

$$\frac{d^2 f_r}{d r^2} = \dots\dots\dots (10.34)$$

$$\frac{d A}{d r} = - N f_r \dots\dots\dots (10.35)$$

$$\frac{d f_r A}{d r N} = - C \dots\dots\dots (10.36)$$

$$\frac{d^2 f_r}{d r^2} = C f_r \dots\dots\dots (10.37)$$

which finally yields:

$$f_r = a_1 e^{a r} + a_2 e^{-a r} \dots\dots\dots (10.38)$$

Here the first term approaches zero, which yields:

$$f_r = a_2 e^{-a r} \dots\dots\dots (10.39)$$

10.8.a. Curve fitting for K/PIP: coded data

The table has again the following shape:

x'	y'K/PIP
1	87
4	61
8	46
12	39
16	33
20	38
etc.	

Table 10.15a. Per distance class the number of kinsmen (all categories) as a fraction of total the number of potential interaction partners (coded data)

The exponential function fitting the data of Table 8.a.1 is:

$$y' = 59 e^{-x'/26.4}; p = .07 \dots\dots\dots(10.40)$$

Various Bessel functions fitting the data of Table 8.a.1 are, for instance:

$$y' = 33.7 K_0 (x'/20); p = .14$$

$$y' = 28.3 K_0 (x'/30); p = .07$$

$$y' = 26.0 K_0 (x'/40); p = .04$$

$$y' = 23.3 K_0 (x'/50); p = .02$$

$$y' = 21.7 K_0 (x'/60); p = .02$$

$$y' = 13.7 K_0 (x'/100); p = .39$$

This implies the following values for l', n' and P:

l'	n'	P
20	33.7	.14
30	28.3	.07
40	26.0	.04
50	23.3	.02
60	21.7	.02
100	13.7	.39

Table 10.16. Various calculated values for l', n' and P for the Bessel curve fitting for the distribution, over the distance classes, of kinsmen (all categories) as a fraction of the total number of potential interaction partners (coded data).

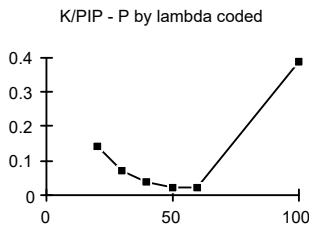


Fig. 10.15. Graphical representation of various calculated values for l' and P for the Bessel curve fitting for the distribution, over the distance classes, of kinsmen (all categories) as a fraction of the total number of potential interaction partners (coded data).

The best fitting Bessel function turns out to be:

$$y'K/PIP = 22.5 K_0 (x'/55); p = .02 \dots\dots\dots(10.41)$$

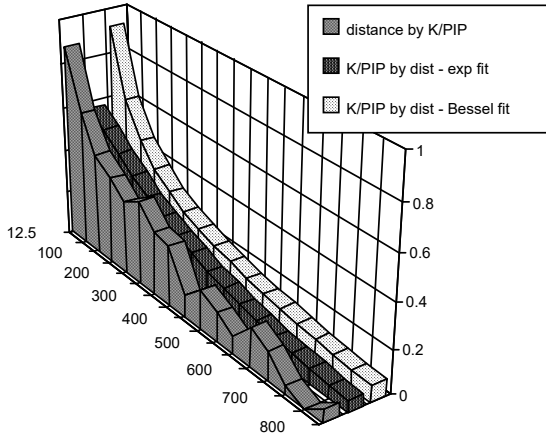


Fig. 10.16. Graphical representation of the distribution, over the distance classes, of kinsmen (all categories) as a fraction of the total number of potential interaction partners: empirical data (coded), fitted exponential curve and Bessel curve

10.8.b. Curve fitting for K/PIP (un-coded data)

The table has again the familiar shape:

x	yK/PIP
12.5	.87
50	.61
100	.46
150	.39
200	.33
250	.38

Table 10.17. Per distance class, the number of kinsmen (all categories) as a fraction of the total number of potential interaction partners (non-coded data)

The exponential function fitting the data of Table 10.17 is:

$$y = .59 e^{-x/330}; p = .07 \dots\dots\dots (10.42)$$

Various Bessel functions which fit the data of Table 10.17 are, for instance:

$$y = .337 K_0(x/250); p = .14$$

$$y = .283 K_0(x/375); p = .07$$

$$y = .260 K_0(x/500); p = .04$$

$$y = .233 K_0(x/625); p = .02$$

$$y = .217 K_0(x/750); p = .02$$

$$y = .137 K_0(x/1250); p = .39$$

This leads to the following values for l, n and P:

l (in m)	n	P
250	.337	.14
375	.283	.07
500	.260	.04
625	.233	.02
750	.217	.02
1250	.137	.39

Table 10.18. Various values as calculated for l, n and P towards the Bessel curve fitting for the distribution, over the distance classes, of kinsmen (all categories) as a fraction of the total number of potential interaction partners (non-coded data).

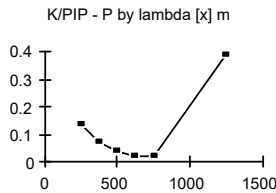


Fig. 10.17. Graphical representation of various values as calculated for l and P towards the Bessel curve fitting for the distribution, over the distance classes, of kinsmen (all categories) as a fraction of the total number of potential interaction partners (non-coded data).

The best fitting Bessel function now turns out to be:

$$y_{K/PIP} = .225 K_0(x/687.5); p = .02 \dots\dots\dots (10.43)$$

The following Fig. gives a graphical representation:

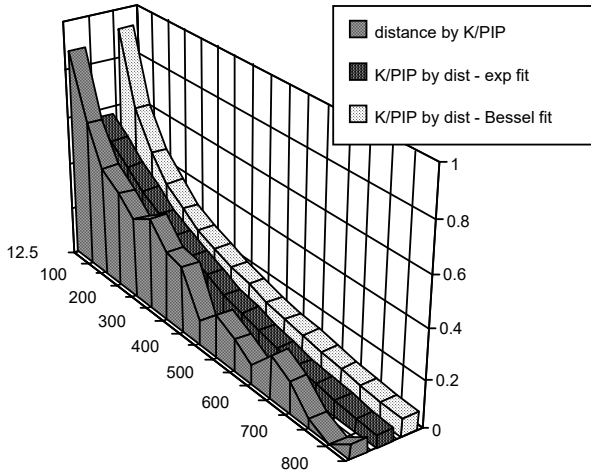


Fig. 10.18. Graphical representation of the distribution, over the distance classes, of kinsmen (all categories) as a fraction of the total number of potential interaction partners: empirical data (non-coded), fitted exponential curve and Bessel curve.

This concludes our modelling of Ĥumiri social organisation. in the context of this book The results, while admittedly far from conclusive, are sufficient as a basis for an exploration of the social-organisational background of Ĥumiri population religion, which will be the subject of Volume II of this book.

⁶⁸ The slight difference between the total observed and total expected is due to rounding off.

⁶⁹ [Volgens een andere kladberekening, vrij consequent toegepast, is de functie:

$$.29 e^{-x/4767}. \text{ [narekenen! dat scheelt een factor } 10! \text{]}$$

Deze categorie doet nogal raar: in de laagste afstandsklassen is zij minder vertegenwoordigd dan in de hogere. Nu klopt dat wel met de inheemse ideologie in Ĥumīrya, maar van de andere kant is het helemaal een kwestie van klassificatie: de mensen dichterbij zouden ook als ver-niet-agnaten geassocieerd kunnen worden gezien de verwevenheid van genealogische en huwelijksrelaties in deze dorpen, maar dat is niet gebeurd – niet door de onderzoeker, en ook niet door de actoren zelf. Niet te veel aandacht hieraan hechten, daarom.

⁷⁰ The slight difference between the total observed and total expected is based on rounding off.

⁷¹ [Volgens een andere kladberekening, vrij consequent toegepast, is de functie:

$$.29 e^{-x/4767}. \text{ [narekenen! dat scheelt een factor } 10! \text{]}$$

Deze categorie doet nogal raar: in de laagste afstandsklassen is zij minder vertegenwoordigd dan in

de hogere. Nu klopt dat wel met de inheemse ideologie in Hūmiryya, maar van de andere kant is het helemaal een kwestie van klassificatie: de mensen dichterbij zouden ook als ver-niet-agnaten geassocieerd kunnen worden gezien de verwevenheid van genealogische en huwelijksrelaties in deze dorpen, maar dat is niet gebeurd – niet door de onderzoeker, en ook niet door de actoren zelf. Niet te veel aandacht hieraan hechten, daarom.

Chapter 11. Kinship and spatiality as principles for Ego-centred dyadic relationships

11.1. Introductory remark

In the introduction to an earlier chapter I have already indicated how spatiality and kinship can form classification principles both for group classification and for dyadic egocentric relationships. There we also discussed the problems of operationalisation involved. Therefore we can now move directly to a discussion of actual dyadic interaction in Ḥumirīyya.

11.2. Actual dyadic interaction in Ḥumirīyya

If between two individuals interaction occurs with a certain measure of frequency and predictability, we say that between these individuals a social relationship exists (in the narrower, sociological sense).

In Ḥumirīyya, social relationships concentrate on the indigenous concept of *mezīyya*: 'a gift or service which is offered freely and with pleasure'. Jongmans (1968) made a profound study of this type of relationships in the village of Mḥamdiyya, just outside the research area. He reaches the conclusion that this type of relationship constitutes a 'fait social total', and that 'there could be no better means of gaining insight in Ḥumiri culture and society'. Several later researchers in the region, supervised by him, confirmed this view.

A *mezīyya* relationship consists of the continuous exchange of goods and services between (the members of) two households: assistance in agriculture and in the day-to-day productive activities which go on at the compound level, the exchange of goods,

the lending of objects and money, mutual visits, and benevolent words. The latter two forms of behaviour enhance the prestige of the person at which they are directed, and thus may be used instrumentally in Ĥumirī relationships.

There are several gradations of *mezīyya* relationships.

Mezīyya relationships in general correspond with the model of the dyadic contract, which Foster (1961, 1963) designed on the basis of his research in the Mexican peasant society. Foster (1961: 1174 f.) describes the dyadic contract in the following terms:

‘These contracts are informal, or implicit, since they lack ritual or legal basis. They are not based on any idea of law, and they are unenforceable through authority; they exist only at the pleasure [the very word used in Ĥumirīyya – WvB] of the contractants. The contracts are dyadic in that they occur only between individuals; three or more people are not brought together. The contracts are non-corporate, since social units such as villages, *barrios* [neighbourhoods –WvB] or extended families are never bound.’

In Ĥumirīyya the most intensive form of *mezīyya* relationships is that of *mutashrin*: ‘those between whom there exists trust’ In such relationships the exchange is unlimited, frequent visits and shared meals are a matter of course, and there exists a very strong identification between the partners. Such identification is formulated in a kinship idiom: the partners consider each other *as brothers*, and the expectations which they cherish vis-à-vis one another derive from the indigenous ideals concerning the relationship between brothers. Shared meals, which in Ĥumirī society have a very special symbolic meaning (*cf.* van Binsbergen 1969: 16 f.) lend to *mutashrin* relationships a simple ritual basis. Moreover *mutashrin* strive to expressing their intensive relationship in yet another ritual form: through a marriage between marriageable members of their close kindred, especially of their own generation (between their siblings) or the next generation (their children). In this respect the *mutashrin* relationship does differ somewhat from Foster’s ideal type, which does not include a ritual basis

In Ĥumirīyya the majority of the interactions between members of different households spring from *mezīyya* relationships and *mutashrin* relationships. These form the constituent elements of the Ĥumirī social process.

However, besides these there are other forms of social interaction: the collective activities of spatial segments; the interactions between specialists and their clients; productive labour in the context of the unemployment relief organisation; patron-client relationships between the wealthy on the one hand, and herdsmen and farm-hands on the other; and finally regular wage labour. Herdsmen, farm-hands and regular wage-earners together comprise only a very small section of the adult male Ĥumirī population (an estimated 5 to 10% in the research area in 1968). Regular wage labour is almost entirely confined to the urban centres and to the formal organisations which have their seats there. Specialist status constitutes a more elaborate social domain, to which I shall come back below.

Mezīyya and *mutashrin* relationships are egocentric relationships between two individuals, in the sphere of day-to-day interaction. The partners in such relationships are recruited according to certain principles. Having assessed, in the previous chapter, the limited significance of group classification for day-to-day interaction, I shall now assess the significance of spatiality and kinship for the recruitment of partners in day-

to-day interaction, in the context of egocentric, dyadic relationships.

11.3. The significance of spatiality for the recruitment of partners in day-to-day interaction

If spatiality is relevant for day-to-day interaction and for the formation of dyadic relationships, this must mean that spatial distance constitutes a central factor in the recruitment of partners for day-to-day interaction. Therefore I have measured the relationship between spatial distance on the one hand, and on the other marital relations, religious activities, and everyday interaction at the village level: visits to a person's house, co-operation in agriculture, in fetching water and in the collection of forest products.

The data on everyday intra-village interaction are presented and analysed in the preceding Chapter 10. This yields the following conclusions:

- There is a strong preference to select one's interaction partners among the members of nearby households. This preference is the more remarkable, since at a greater distance from Ego's dwelling house far more potential interaction partners are available.
- For distances beyond Ego's own neighbourhood, the percentage of households with whom Ego has day-to-day interaction, decreases to under 10% of the total supply of potential interaction partners.
- One does not interact with the same frequency with all day-to-day interaction partners: the frequency of interaction is the higher, the nearer the day-to-day interaction partners live vis-à-vis Ego.

These conclusions have been formulated in terms of 'interaction partners' – remaining as closely as possible to the empirical recording of interaction. Considering the nature of social relationships in Ĥumiriyya, we are justified to equate, in the above conclusions, 'day-to-day interaction partner' with 'partner in a *mezīyya* relationship'.

The data concerning marital relationships have likewise been analysed above. They yielded the following conclusions as to the relationship between spatial distance and the recruitment of marriage partners:

- There is a general tendency for marriage partners to be recruited among the inhabitants of one's own village or the nearest other villages.
- When marriage partners are recruited within one's own village, they tend to start out as nearest neighbours.

These conclusions as based on my quantitative data correspond with the results of other researchers in Ĥumiriyya.

In Volume II of this book I shall discuss at length how also religious activities can be analysed in connexion with spatial distance.

In general it is true to say that in the recruitment of day-to-day interaction partners the fraction of members of one's own spatial segment is always larger than that of any

other spatial segment at the same segmentary level; moreover the frequency of interaction with members of one's own segment is higher than that with members of a different segment at the same level. This applies to all types of interaction: cooperation in agriculture and at the compound; dyadic exchange relations, marital relationships, visits, recreation, the consultation of specialists, and religious activities. For example, when a man sets out to build a house, the members of his household are sure to collaborate with him; the members of his compound and neighbourhood are very likely to collaborate; the members of other neighbourhoods within the same village are rather unlikely to participate; and the participation of others who can only identify with the builder at the level of the valley, the chiefdom or not even there, is most unlikely.

Spatiality turns out to be a major datum in the recruitment of day-to-day interaction partners, and the dyadic *meziyya* and *mutashrin* relationships on which interaction largely revolves in Ḥumiriyya.

In other studies of Ḥumiriyya this spatial principle may have been incidentally recognised, but it was never systematically analysed. In passing, Jongmans (1968: 8) points to

‘the factor of being neighbours, which is one of the determinants for entering into and maintaining *meziyya* relations’.

Van Dijk (1968: 33*f.*) gives an analysis of the word *mutashrin*, for close neighbours and for brothers who live in good mutual understanding (typically on the still undivided patrimony left to them by their father, *i.e.* as close neighbours again); this leads van Dijk implicitly to a discussion of spatiality. The spatial element in the formation of social relations (especially on the neighbourhood level) is also discussed in Beeker (1967) and Bos (1969).

We now turn to the significance of kinship.

11.4. The significance of kinship for the recruitment of day-to-day interaction partners

Now that, at this point in this book's argument, we are no longer occupied with group classification, but with egocentric relations, there is no longer any need to confine our kinship analysis to agnatic kinship which is so one-sidedly stressed in the indigenous ideology (and in the anthropology of North Africa and the Middle East). In the course of my argument I already suggested that in addition to agnatic kinship, cognatic and affinal relationships are significant in Ḥumiri social organisation. Let us elaborate on this point.

One of the conclusions of my analysis of Ḥumiri pseudolineages was that kinship in its own right could only be relevant for interaction in the case of close kinship ties, which are beyond the limits of genealogical manipulation such as actors tend to perform.

The reconstruction of ortholineages, and of past and present marital links between them, enables me to assess, for a sample of heads of household in the villages of Sidi Mḥammad and Mayziyya, precisely how the members of the sample are related

(biologically and genealogically, although this does not not always coincide with the conscious perceptions and verbalisations of the people concerned) to all other heads of households in these villages. In order to process these data I had to devise a system to classify the numerous possible kinship ties into a limited number of categories. The details of this system are to be found above in Chapter 10. I ended up with four categories of kinship: close agnates, distant agnates, close cognates/affines and remote cognates / affines. These four categories together comprise the set of people of whom we may assume that Ego perceives them as kinsmen on the basis of Ego's genealogical knowledge (and not merely because Ego identifies with these people on the basis of the existence of positive social relationships). These four categories together constitute the *kindred* (Mitchell 1963). The remainder of Ego's social environment I consider as non-kin.

Next I assessed how for the members of this sample, kinship in the four categories as specified informed: dwelling in each other's proximity; the selection of partners for actual everyday interaction; the frequency of interaction; and the recruitment of marriage partners. The data and analyses on these points are presented in Chapter 10, above. The main conclusions are discussed in the following sections, itemised under the specific headings.

11.4.1. Residence

There is a statistically significant connexion between kinship and living in each other's proximity. The nearer Ego's own dwelling house, the larger the fraction of kindred among Ego's neighbouring heads of household. This fraction decreases rapidly with increasing distance: it dips under 10% beyond 700 m from Ego's house. *However, at no distance is Ego completely surrounded by heads of household belonging to Ego's kindred, not even at Ego's own compound.* When we subdivide the kindred in the four categories as above, the relationship between kinship and distance turns out to be no longer statistically significant for distant agnates. For close agnates the fraction among Ego's nearest neighbours (at a distance from 0 to 125 m) is exceptionally large, but at larger distances the close agnates no longer stand out from among the other three categories of kindred.

11.4.2. Day-to-day interaction

At certain spatial distances, *i.e.* at certain positions within the structure of spatial segmentation, membership of Ego's kindred turns out to be a determinant in the recruitment of interaction partners in its own right, independent from spatial distance. The distance classes in question are evenly distributed over the entire range. The effect mentioned can be demonstrated for heads of household who belong to Ego's neighbourhood but who are not Ego's nearest neighbours nor live at the extreme periphery at Ego's neighbourhood, and moreover it can be demonstrated for heads of household who, live at the periphery of Ego's village (outside Ego's own neighbourhood) and in the nearest part of an adjacent village. What is it in the structure of Ĥumiri dyadic relations that could explain this erratic pattern? Or is the apparent pattern merely an artefact of quantitative analysis? Further analysis might reveal the

interplay, in this connexion, of such factors as the dispersion of paternal families, patterns of inheritance of land, the influence of wealth on social relationships, and the stereotyped perception of rival spatial segments

With regard to the selection of day-to-day interaction partners the actors do not appear to have a preference for close kindred over remote kindred, or for agnates over cognates/affines.

11.4.3. Frequency of interaction

From the limited data at my disposal it cannot be demonstrated that, besides spatiality, kinship constitutes an independent determinant of the frequency of interaction.

11.4.4. The recruitment of marriage partners: Introduction

A small proportion of the marriage partners is recruited among people who are initially unknown to Ego and who live at a considerable distance. However, in most cases a Ḥumiri marriage is the expression of existing dyadic relationships (especially those of the *mutashrin* type). Therefore the recruitment of marriage partners reflects the recruitment of these relationships. In the recruitment of marriage partners there is no sign of either a positive, or a negative selection of specific agnatic groups other than Ego's own. Many marriages are kindred-endogamous (30%). Of the kindred-endogamous marriage roughly as many are contracted between agnatic kindred as between cognatic / affinal kindred. Most kindred-endogamous marriages can be interpreted as marriages between people who live near each other (*cf.* van Dijk 1968: 37). Since for all kindred-endogamous marriages we can assume that the marriage partners (in fact, their legal guardians) were already each other's partners in dyadic relationships prior to the marriage, it is not meaningful to speak of a preference for kindred endogamy in Ḥumiriyya. It is virtually impossible to prove quantitatively that the fraction of kindred among actual marriage partners is larger than, equal to, or smaller than could be expected on the basis of the supply of kindred among potential marriage partners. But even if the fraction would turn out to be significantly larger, that would only reflect a (slight) preference for kindred in the selection of partners for dyadic relationships in general. The same reasoning applies to the marriage between close agnates, for instance the FBD marriage which has been discussed so extensively in the literature on marriage in North African, Middle Eastern, and in general Arab societies. Because the marriage between close agnates is only one of the possible forms under which kindred endogamy can manifest itself, it is not meaningful to analyse the marriage between close agnates (including the famous FBD marriage) as a separate category, as is frequently done.⁷²

All this brings out the fact that kinship (*i.e.* belonging to Ego's kindred) constitutes, besides spatiality, an independent factor in Ḥumiri social organisation, and notably a principle in the recruitment of egocentric, dyadic relationships. However, the kinship

⁷² *E.g.* Cuisenier 1962; Murphy & Kasdan 1959, 1967; Chelhod 1965; for Ḥumiriyya: Jonkhout n.d.; Banck 1968.

factor only applies in specific contexts and as such is not of the same significance as the spatial factor, which we found to effect all aspects of interaction in so far as touched by our present analysis.

11.5. Aspects of the marriage pattern

11.5.1. Introduction: Exploding anthropological myths about Arab, North African and Middle Eastern marriage

It is not my pretension to offer here an exhaustive analysis of the Ḥumirī marriage pattern. I merely want to show that the principles as discussed in the present book are also applicable to, and illuminate, such a major structural datum as the marriage pattern. The relevance of such a discussion will appear in Volume II, where we shall see that there is an intricate connection, in Ḥumiriyya, between the main form of popular religion (notably: pilgrimage) and the marriage pattern.

Meanwhile, the following discussion may lay claim to considerable comparative and theoretical potential, since it explodes, with complete detailed empirical data painstakingly collected, several of the common anthropological myths attending Arab, North African and Middle Eastern marriage. If kinship and marriage as pet anthropological topics had not been virtually wiped out in the course of the late half century, these findings would have created a considerable sensation. Now I must confined myself that they will lie tucked away in the pages in of this book, to be accidentally tumbled upon by present-day anthropologists seeking some diversion from their relatively infertile fascination with globalisation, the state, identity, and Foucaultian scholastics.

I shall discuss the following aspects of the marriage pattern in the research area:

1. The relation between spatial distance and the recruitment of marriage partners.
2. Agnatic endogamy is widely considered the preferential marriage type in Ḥumiriyya and in most other Arab-orientated society; *now, in so far as exogamy outside the own agnatic group is concerned, is there a preference for certain agnatic groups over others among those present in the research area?*
3. What is the significance, for the recruitment of marriage partners, of intensive dyadic relations between bride-givers and bride-takers?
4. Marriage between kinsmen in the wider sense, *i.e.* kindred endogamy where kindred includes agnatic, cognatic and affinal relatives.

As in many other societies, marital choice in Ḥumiriyya is not primarily a matter of the future spouses, but of the heads of the respective compounds to which the bride and the husband belong: the father, father's brother, brother or other close relatives. Once again: these heads of compounds, and not the spouses themselves, I mean when I speak of marriage partners.

11.5.2. The relationship between spatial distance and the recruitment of marriage partners

The tendency in Ḥumiriyya to recruit partners for interaction and social relations near one's own dwelling house is also found with regard to marriage.

In this connexion Table 11.1 is a convincing illustration. For each village covered by my fieldwork, and for the research area as a whole, this table shows the median distance across which a woman moves on the occasion of her marriage. The population from which these data derive is the complete set of living women who resided in the research area in 1968 and who were either married or had been married (in the latter case the data reflect their last marriage). The very small village of Raml al-^cAtrus has not been taken into account. In the case of uxori-local marriage a woman does not move at marriage, and then a marital distance of 0.0 km has been scored, even if in fact the distance between bride-givers and bride-takers is larger; however, since only 5% of all marriages are uxori-local, table 15 can yet be said to offer a fair picture of the

spatial distance between bride-givers and bride-takers at the moment the marriage was contracted (the actual distances may be slightly higher).

village	median marital distance (km)
Sidi Mḥammad	0.4
Mayziyya	1.8
Fidh al-Missay	0.8 - 0.9
Hamraya	0.8
Ḥamaysiyya	1.4
Raml al- ^c Atrus	no data
research area as a whole	0.7

Table 11.1. Median distance (kms) across which woman move on the occasion of their marriage, for the villages of the research area

We may conclude that in the research area there is a general tendency to recruit marriage partners among the inhabitant's of one's own village or the nearest neighbouring villages.

Elsewhere I shall discuss the considerable differences in marital distance between the villages.

In the case of certain Ḥumiri marriages (especially those contracted over a relatively large distance: 3 kms or more) bride-givers and bride-takers find each other exclusively through the operation of an information network, which has points of concentration at the markets, the regional saintly festivals, and the unemployment relief organisation. In those cases the recruitment of marriage partners is not primarily governed by spatial propinquity.

Even when marriage partners are recruited in the immediate surroundings, this does not yet necessarily mean that all potential marriage partners have the same chance of being selected. A compound head wishing to give his son or daughter in marriage will select according to certain criteria. The indigenous societal ideology would suggest that, among these criteria, the local distribution of agnatic groups might be of some importance. Therefore we may well ask the following question: *Is there a mutual preference for certain agnatic groups to contract marriages with each other?*

11.5.3. Is there a mutual preference for certain agnatic groups to contract marriages with each other

Can we detect such a mutual preference, even though at the same spatial distance there is a sufficient supply of marriage partners belonging to other agnatic groups?

For an answer to this question, we shall take as our point of departure the agnatic groupings such as have been discussed and operationalised in Chapter XXX of this book. Such agnatic groupings constitute the only sets of agnates which can be clearly made out by both the ethnographer and the actors.

On the basis of the reconstruction of ortholineages, and taking into account the distinctions between local kin groupings such as are being consciously made by the inhabitants of Sidi Mḥammad and Mayziyya, I divided the 68 households of both villages in 21 different agnatic groupings; one head of household (belonging to ortholineage 18) on his own constituted agnatic grouping 22. Each ortholineage comprises 1 to 4 agnatic groupings, at least in so far as the members of that ortholineage lived in Sidi Mḥammad or Mayziyya (some local ortholineages only have members in other villages than these two). Recent migration (at the household and compound level) means that not all members of the 22 agnatic groupings lived in Sidi Mḥammad or Mayziyya in 1968; some members have even never lived there. But since we are looking at kinship and not at place of residence, also those members of the twenty-two agnatic groupings who live outside Sidi Mḥammad and Mayziyya must be included in the present analysis. For all these agnatic groupings the marriages were assessed in all generations of which at least one member was still alive. In this way the data comprise the large majority of marriages contracted since c. 1925 with bride-givers, bride-takers, or both belonging to any of our twenty-two agnatic groupings. However, the analysis is not fully complete since it does not include agnatic groupings which in 1968 were no longer represented in Sidi Mḥammad or Mayziyya but which had representatives in these villages in the period between 1925 and 1967.

These operationalisations yield the data as summarised in tables 11.2 and 11.3. [numerical code of ortholineage meestal roman figures]

numerical code of agnatic grouping	numerical code of ortho-lineage	ancestor	represented in 1968 in the following villages	number of households ⁷³	number of marriages processed
1	I	Ahmad bin ^c Ali	Saydiyya, Shahada	8*	20
2	I	Bu-Ḥris b.al-Tidjan	Saydiyya, Shahada	7*	11
3	II	Sa'ad bin ^c Amar		4*	6
4	II	Ahmad bin Muḥammad	al-Mazuz	4	10
5	II	Hasnawi bin Muḥammad	Salul	1	4
6	II	^c Ashdiyya bin Muḥammad	al-Mazuz	4	5
7	III	^c Aisa bin Gharib		3	6
8	IV	Salah bin Silama		4	8
9	XXI	^c Amur bin Ahmad		1	1
10	V	Umqadish bin Zarruq	<small>de open cellen in deze kolom moeten worden opgevuld; ws allemaal in SM or Mayz</small>	8	21
11	V	Bu-Mandjil bin Zarruq	Tra'aya (sut, bidh)	3	11
12	V	^c Amr bin ^c Arbi	Quassim, al- ^c Ayun, Ḥamaysiyya, ^c Ain Drāham	4	27
13	V	Salah bin Ḥamis	Ulad bin Sayyid	4	12
14	VI	Muḥammad bin Salah		4	8
15	VI	Ali bin Salah		1	2
16	VII	^c Abd Allah bin Hafsi		2	8
17	VIII	Ibrahim bin Shaban		1	1
18	IX	Muḥammad bin Bu-Tara	Fidh al-Missay	1	10
19	IX	Salah bin ^c Amur		1	5
20	XI	Salah bin Hamad	Huamdiyya	2*	5
21	XVI	Tayib bin ^c Adhman	Rashaybiyya	1*	3
22	XVIII	Salah bin Uhlayil		0	2
				68	186

Table 11.2. Agnatic groupings with members in the villages of Sīdī Mḥammad and

⁷³ In the village of Sīdī Mḥammad (unmarked figure) and the village of Mayziyya (figure marked with asterisk *); in 1968 no agnatic grouping extended over both villages.

Mayziyya, 1968: branches of ortholineages, historical ancestors, place of residence of members in 1968 (in so far as inside research area), number of households per grouping (in so far as in Sidi Mḥammad and Mayziyya), and number of marriages processed.

Table 11.3 shows which data are available for which number of marriages:

		processed number of marriages			
		bride-takers			
		within our sample	outside our sample	unknown	total
bride-givers	within our sample	53	28	0	81
	outside our sample	41	n.a.	n.a.	n.a.
	unknown	11	n.a.	n.a.	n.a.
	total	105	n.a.	n.a.	n.a.

n.a. = not applicable

Table 11.3. Number of marriages processed for the analysis of marital preferences between agnatic groupings in the villages of Sidi Mḥammad and Mayziyya

Of the 53 marriages within the sample, *to which agnatic groupings did bride-givers and bride-takers belong?* This is clear from the marital network as presented in the following Figure:

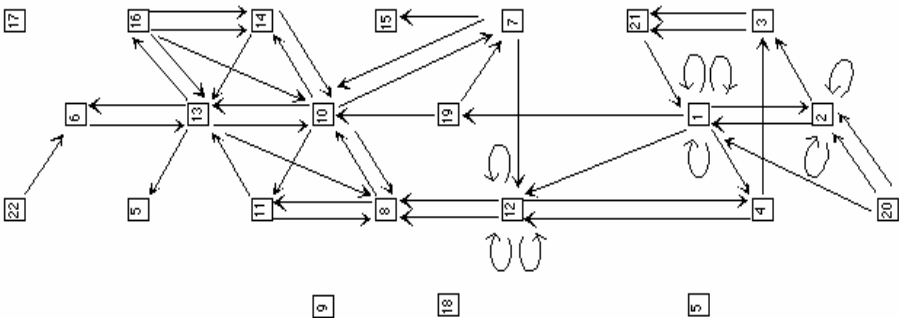


Fig. 11.1. The present-day marriage system of agnatic cores in the village of Sidi Mḥammad and Mayziyya

Analysis of Fig. 11.1 allows us to draw the following conclusions:

- Agnatic groupings 5, 9, 17 and 18 are isolated from each other and from all other agnatic groupings in the sample.
- Only in the cases of agnatic groupings 1, 2 and 12 were there endogamous marriages inside the same agnatic grouping.
- In the period under analysis (c. 1925 – 1968) *no agnatic grouping had more than two* marital relations with any other agnatic grouping in the sample – which is an indication of a deliberate and carefully managed connubium system, where marital risks and benefits are consciously spread over all available agnatic groups
- Branches within the same ortholineage (*i.e.*: the agnatic groupings 1 and 2; 3, 4, 5 and 6; 10, 11, 12 and 13; 14 and 15; 18 and 19) do *not* show a greater tendency to contract marriages between each other (*i.e.* within the ortholineage) than with agnatic groupings from other ortholineages.
- Dual marital relations (*i.e.* two marriages) between any two agnatic groupings occur about as frequently as unique ones (*i.e.* only one marriage) .
- Among the dual marital relations between two agnatic groupings we find both symmetrical ones (one woman is exchanged for another, in the same or adjacent generations) and asymmetrical ones (two women marry from A to B but no women are received in return).
- Within the sample, the number of different agnatic groupings with which a specific agnatic grouping had marital relations, varies from 0 to 7. The more marriages the data set contained for any specific agnatic grouping A (*cf.* table 11.2), the more *different* agnatic groupings A encompassed in its choice of actual marital relationships between c. 1925 and 1968; this relationship is statistically significant (Spearman's rank correlation: $r_s = +0.71$, $> r_{SN} = 22;5\%$) However, for obvious demographic reasons the number of marriages recorded for any particular agnatic grouping A in the data set is a fair reflection of A's size; therefore *we can say that the more members an agnatic grouping A comprises, with the more different agnatic groupings A enters into marital relations.*
- *There results are in absolute contradiction with the hypothesis that there are any marital preferences between agnatic groupings. Chance-governed dispersion, rather than formal preference in terms of the agnatic ideology, appears to structure marital choice in Ħumiriyya.* Even if one marital relation between agnatic groupings A and B is often repeated within a few decades by another marriage so that a dual marital relationship results, both A and B would in the meantime also cultivate marital relations with other agnatic groupings; and Ħumiri marital strategy obviously preclude the concentration of marital relations with any one agnatic grouping; between agnatic grouping A and B zero, one or two marital relations may be contracted, *but never more.*

Thus the marriage pattern as demonstrated by the data set shows two complementary movements:

1. the initiating of marital relations with whatever new agnatic groupings present themselves on the local scene;

2. the maintaining of existing marital relations but on a limited scale never exceeding dual bonds.

While acknowledging the indications for a marital preference *within* one and the same agnatic grouping (in the cases of agnatic groupings 1, 2 and 12), we must reject the hypothesis of a mutual preference for certain agnatic groups to contract marriages *between* each other.

Meanwhile, even if there is no agnatic inter-grouping marital preference, might the isolation of the agnatic groupings 5, 9, 17 and 18 be interpreted to suggest that *in some cases there is a systematic agnatic inter-grouping avoidance*? On closer analysis, the isolation of these groupings turns out to be largely a matter of short supply: with the exception of 18, these are among the agnatic groupings with the smallest number of recorded marriages in the data set, and by implications with this smallest number of members. Their isolation within the marriage pattern seems to have mainly demographic causes and may be explained without reference to the actors' structuring of their social environment in terms of an ideology of agnatic kinship.

Even if Fig. 11.1 does not offer proof of marital selection on the basis of agnatic preference, it does show a certain regularity. We perceive some kind of honeycomb structure: certain clusters of agnatic groupings tend to marry among each other, but as we move along the diagram from left to right we see new agnatic groupings being incorporated in the clusters, so that the pattern keeps shifting gradually. If this honeycomb structure is not explained by agnatic descent in terms of ortholineage branches, then what does explain it? Moreover, what explains the intra-grouping agnatic endogamy of 1, 2 and 12? And finally, what explains the isolation of the rather large agnatic grouping 18? As so many other aspects of Ĥumiri social organisation, the pattern of Fig. 11.1 turns out to be largely governed by the principle of spatiality. The membership of agnatic groupings at the level discerned in the present context of Table 11.2 has not yet been dispersed to widely over the land. Fig. 11.2 offers a summary overview of the residential history of the villages of Sidi Mḥammad and Mayziyya since c. 1900.

In constructing Fig. 11.2 I have ignored such agnatic groupings as have died out or as have entirely emigrated from both villages since 1900. Some data derive from Beeker (1967). The figure only gives the major lines of residential movement; many details had to be omitted. Now when we compare the current (1968) and past distribution of agnatic groupings in both villages with the honeycomb pattern of Fig. 11.1, the striking resemblance becomes manifest: the marital relations which Fig. 11.1 shows to exist between agnatic groupings, turn out to be virtually all contracted between near neighbours! This conclusion based on quantitative analysis is in line with van Dijk's (1968: 37) qualitative remarks on the marital preference between people whose land is adjacent to each other.

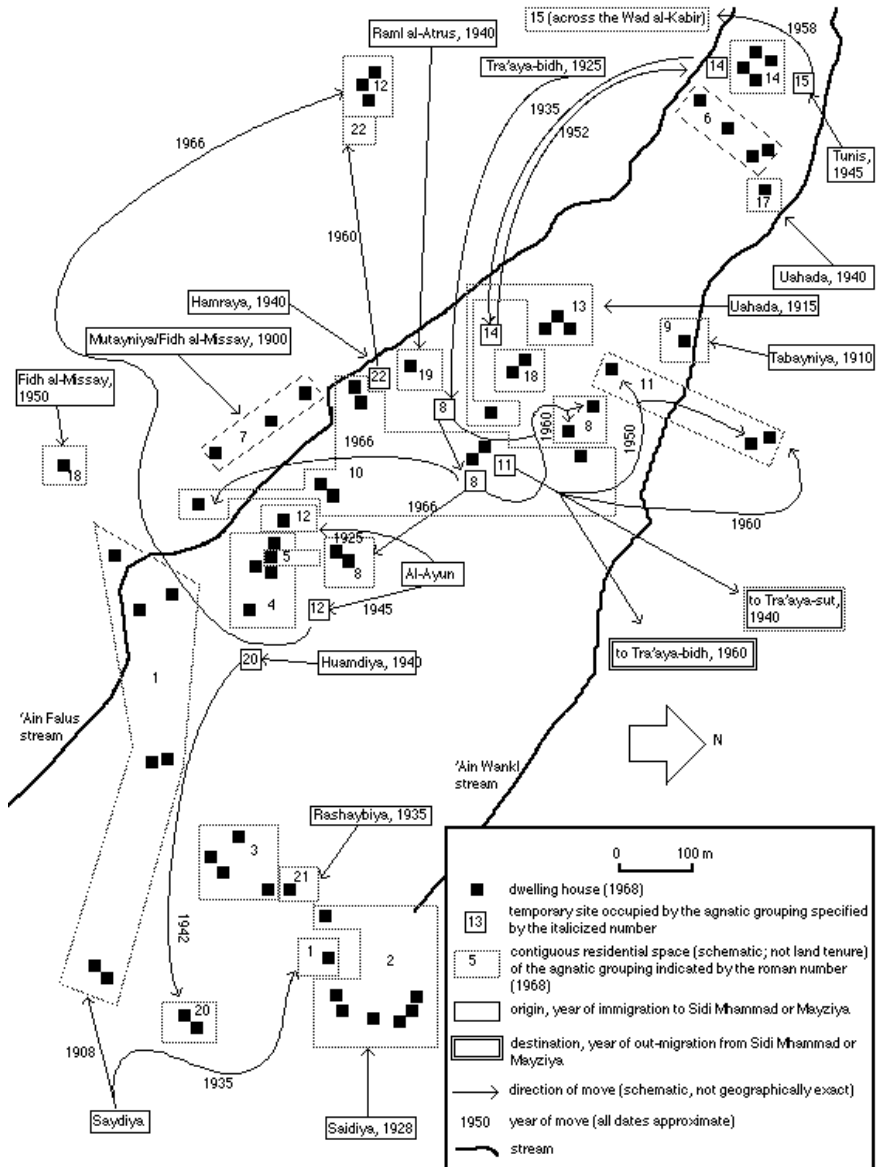


Fig. 11.2. Summary overview of the residential history of the villages of Sidi Mhammad and Mayziya since c. 1900

Even the marriage between the agnatic groupings 7 and 15 is in line with this view:

they have adjacent arable fields in the neighbourhoods of Matayniyya and Raml, and there for a few years they were close neighbours – a detail which does not show in the simplified Fig. 11.2. Similarly, the isolation of agnatic grouping 18 can be attributed to the fact that its constituent heads of household, with the exception of one, have all remained in Fidh al-Missay, at a distance of c. 2 km from Sidī Mḥammad, and have arranged their marital relations from there.

So not only do Ḥumiriis tend to look for marriage partners in their own village – within that village, they have a distinct preference for nearest neighbours.

The above is still a statistical description, and not an explanation. Why does one select close neighbours as marriage partners? Moreover, the data contain a few cases which cannot be categorised as marriage between nearest neighbours. Here the intervening factor yet often turns out to be that of agnatic kinship. Table 11.2 shows that some agnatic groupings are also represented in other villages than Sidī Mḥammad and Mayziyya. Now this state of affairs turns out to lead to marital relations of the type as exemplified in Fig. 11.3.

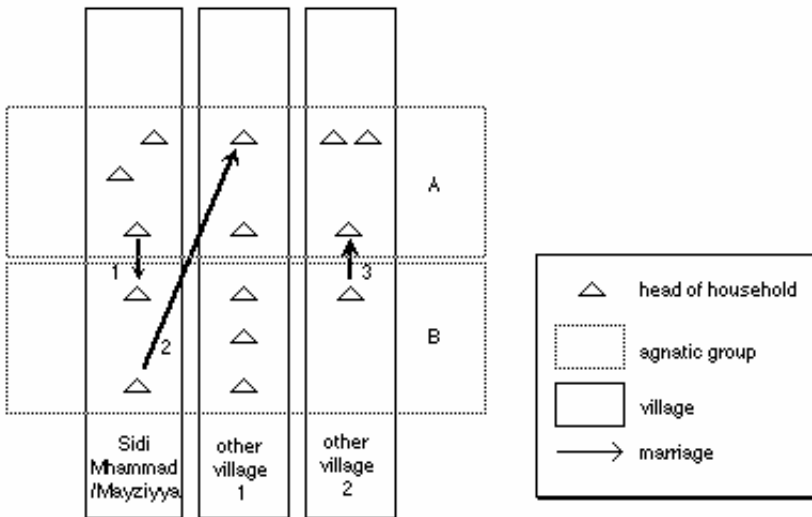


Fig. 11.3. Schematic representation of various types of exogamy in the valley of Sidī Mḥammad

On the basis of neighbourly relations between heads of households a and b in Sidī Mḥammad, in some other village other members of their respective agnatic groupings A and B may contract a second marriage linking Sidī Mḥammad (or Mayziyya) and another village (a' and b', marriage no. 2) or linking two non-neighbours in another village than Sidī Mḥammad or Mayziyya (a'' and b'', marriage no. 3). A similar case occurs when agnatic groupings A and B coincide: when one agnatic grouping extends over more than one village some kinship-endogamous marriages within that grouping

will take place within the same village (and then usually between close neighbours), but other kinship-endogamous marriages will be between heads of households who reside in different villages.

11.5.4. The significance, for the recruitment of marriage partners (i.e. their guardians), of intensive pre-marital social relations

The insights developed throughout the present book, along with the specific data on the Ḥumiri marriage pattern as discussed in this Chapter so far, lead to the following conclusion: *in Ḥumiriyya one often selects one's marriage partner among one's closest neighbours, since it is with them that one maintains the most intensive dyadic relationships: they are mutashrin.*

Not agnatic kinship or geographic distance in themselves are the keys to the marriage pattern in Ḥumiriyya, but: the existence of dyadic relationships between heads of household, and the recruitment and partners to such relationships.

This usually also applies to those marriages within the data set which were recruited no between close neighbours but yet within a distance of a few kilometers. Also in the latter cases an intensive dyadic relationship between the marriage partners precedes the marriage. The marriage is then the consequence, and the highest expression, of the dyadic relationship which has been in existence for years if not for decades. The exceptions are those marriages where bride-taker and bride-giver find each other, usually across a considerable spatial distance, through an information network.

If this analysis holds true, it means that essentially the recruitment of marriage partners mirrors the recruitment of partners in dyadic relationships. The principles governing the latter we have seen as the argument of this book took shape across the chapters: spatiality as a primary factor, with additional recruitment principles in the form of kinship, specialist status, the unemployment relief work organisation, visits to urban centres, saintly festivals.

We can now deepen the conclusions as based on Figs 12 and 13.

In Ḥumiriyya one has to have satisfactory dyadic relationships with close neighbours. If not, one moves to a different site.⁷⁴ Admittedly such relationships are not exclusively found among closest neighbours, but often dwelling in each other's direct surroundings is the most appropriate way to express such relations. In our present data set on marriage we see how in several cases a marriage between close neighbours followed soon after either agnatic group's immigration. Living in close proximity is then not the 'cause' of the marriage, but both the marriage and dwelling in close proximity (as often brought about by moving, and as maintained by not moving again to a different site) are the manifestation of emerging dyadic relationships (as *mutashrin*) between the heads of household concerned.

⁷⁴ Such moves are very frequent and have been analysed below as a specific topic. In the analysis, the constant restructuring of the set of one's closest neighbours appears as an important strategy among Ḥumiri heads of households. In 1968-1970 there was a statistically significant tendency to bring about such restructuring that the aggregate wealth of one's set of nearest neighbours increased.

Those marriages in the sample which are contracted across a spatial distance of some kilometers fit this pattern in the following way. If somewhere the members of two agnatic groupings are close neighbours, this is on the one hand the manifestation of already existing positive social relations between Ego and his neighbours; but on the other hand this neighbourliness creates the opportunities for future intensive interaction between Ego's neighbours and Ego's non-resident close agnates: on the occasion of visits, assistance in agriculture, family festivals and funerals, these overlapping elements in Ego's relational network come into contact with each other and may ultimately decide to enter into more intensive and permanent dyadic relationships.

Now it is clear why actors in Ḥumiriyya often represent the recruitment of marriage partners as a matter of 'marrying "kinsmen"', When doing so, they are not applying the analytical concept of kinship, but the indigenous concept, which is synonymous with 'positive social relationship' and with *mutashrin*.

Against this background I shall now assess whether, after all, kinship does not really form an additional factor in the selection of marriage partners. In other words, in Ego's selection of marriage partners, is it not of significance whether any potential marriage partner belongs to Ego's kindred, or, among the kindred, to Ego's agnates?

11.5.4. Marriage between members of the kindred

We will now first assess the relationship between marital choice and the kindred as a whole; after which we shall turn our attention to the category of the agnates, within the kindred. In the first case we speak of kindred endogamy, in the second of agnatic endogamy.

In order to assess the extent of kindred endogamy, I constructed, for the villages of Sidi Mḥammad and Mayziyya, the total set of living adult inhabitants (which is much larger than the set of heads of household), in so far as they were married or had ever been married. This yielded 68 marriages. In so far as possible I assess, for each couple, their kinship relationship before marriage. Kinship ties were categorised as discussed in Table XXX. Data on their pre-marital kinship relationship were lacking for three couples. The positive results are listed in the following table:

	kindred- endogamous marriages	kindred- exogamous marriages	total number of marriages	% kindred endogamous marriages
Sidi Mḥammad	11	34	45	24%
Mayziyya	7	13	20	35%
total	18	47	65	28%

Table 11.4. Kindred exogamy and kindred endogamy in the villages of Sidi Mḥammad and Mayziyya

The apparent difference in kindred endogamy between Sidi Mḥammad and Mayziyya is not significant. (Likelihood ratio test, $\chi^2 = 0.76$; $df = 1$; $p > 5\%$.)

Table 11.5 specifies the relationship between the spouses in the case of the 18 kindred-endogamous marriages in Sīdī Mḥammad and Mayzīyya, and gives the distance (in kms) across which the marriage was contracted.

kinship chain	village	
	Sīdī Mḥammad	Mayzīyya
<i>agnates</i>		
FBD	2 (2.5; 0.6)	3 (0.0; 4.0; 2.5)
FBSD	1 (0.2)	1 (2.5)
FFBD		1 (2.5)
FFBSD		1 (2.5)*
FFBSSD	1 (0.3)	
<i>cognates/affines</i>		
FBDD	1 (0.1)	
FZD	3 (1.3; 0.5; 0.1)	
FZSD	1 (0.3)**	
MZD	1 (0.3)**	1 (2.5)*
MBD	1 (0.1)	1 (0.1)
ZHD	1 (0.4)	
total	11	7

Key: e.g. line 3, right-hand column, must be read 'in Mayzīyya one man married his father's father's brother's daughter, who before marriage lived at a distance of 2.5 kms'.

In the case of two marriages two different kinship chains of equal weight could be traced between husband and wife. For these cases, the table shows two pairs of data (one pair marked *, the other pair **), but each pair contributes only one marriage to the column totals.

Table 11.5. The nature of kindred endogamy in the villages of Sīdī Mḥammad and Mayzīyya: kinship chains and marital distances (between parentheses, in kms)

From Table 11.5 it would appear that agnatic endogamy occurs more frequently in Mayzīyya than in Sīdī Mḥammad, but the difference turns out not to be statistically significant.⁷⁵ Likewise the marital distance in the case of kindred endogamy (all types combined) would appear to be larger in Mayzīyya than in Sīdī Mḥammad, but this difference is neither significant.⁷⁶ The last result reflects the difference in marital distance between these villages in general (cf. table 15).

A comparison of the Tables 15 and 19 shows that the distance across which kindred-endogamous marriages are contracted does not fundamentally differ from the one across which kindred-exogamous marriages are contracted.

⁷⁵ Likelihood ratio test, $\chi^2 = 0.99$; $df = 1$; $p > 5\%$.

⁷⁶ Mann-Whitney U test corrected for ties, cf. Siegel n.d. : 116 f.: $z = 1.47$, $p > 5\%$.

On the basis of Table 18 , may we conclude to a positive selection, or preference, of kindred in the recruitment of marriage partners?

In the previous Chapter we analysed the spatial distribution of kindred. There it turned out: the nearer to Ego's place of residence, the more kindred. In a society where consistently marriage partners are recruited as near to one's place of residence as possible, it is inevitable that a certain proportion of the marriages will be kindred-endogamous. The main factors keeping down this proportion would be the existence of *incest prohibitions* with regard to certain categories of kindred.

In *Humiriyya* incest prohibitions are of limited scope.⁷⁷ Regardless of the classificatory and colloquial / fictive use of kinship terms, Ego is only prohibited to marry Ego's biological children, grandchildren, parents, grandparents, siblings, children and grandchildren of siblings, parent's siblings. A large part of the kindred therefore remains available for marriage. Kindred endogamy in *Humiriyya* can occur because a large proportion of potential kindred spouses do not fall under incest prohibitions.

It is practically impossible to investigate quantitatively whether the extent of kindred endogamy is in full accordance with the relative distribution of kindred in the immediate spatial surroundings of bride-takers and bride-givers. We would have to draw a representative sample of marriages. For each marriage we would then have to assess the spatial distribution, around bride-givers and bride-takers, of kindred families at the time of the marriage was contracted; moreover we would have to assess whether these families did actually have marriageable sons or daughter at the time; and finally we would have to assess whether in those families of potential marriage partners there was sufficient cash available for bridewealth and for the marriage feast. Only then could it be calculated whether more or fewer kindred are being chosen than is to be expected on the basis of the supply of potential marriage partners. Such an approach is impossible to realise. Therefore we have to limit ourselves here to a more cursory approach.

Part of the kindred-endogamous marriages we may certainly interpret as marriages between people who live in each other's proximity.

At first face we might be tempted to reverse the argument into: 'one marries people from one's immediate spatial surroundings, because one prefers to marry within the kindred, and the kindred is primarily lives in one's close spatial proximity.' However this explanation does not hold: for also kindred-exogamous marriages (which is c. 70% of all marriages) are largely contracted between people who live in each other's

⁷⁷ This finding is far from specific to *Humiriyya*. It is widespread in Arab, North African and Middle Eastern societies. In Antiquity, marriage and procreation between very close kinsmen has been described (as a permissible choice and even as an ideal, if not as common practice) for various Mediterranean and adjacent peoples, such as the Hittites, the Ancient Egyptians, and the followers of Zarathustra in Iran / Persia. Lévi-Strauss (1949) considered the incest prohibition the very backbone of human social life, because of the requirement of exogamy which it imposes on nuclear families. However, although prohibited by most cultures worldwide, and contrary to a widespread belief among geneticists (who would stress the genetic dangers of family inbreeding), there does not seem to be an innate, genetically programmed abhorrence of incest among humans (nor among most mammals anyway), as is also manifest by the fact that in most societies a considerable percentage of women has no choice but to undergo the sexual attentions of close kin.

proximity!

Rather, living (and continuing to live) in each other's proximity is a manifestation of intensive dyadic relationships. Also kindred marriages must be interpreted in this sense.

And because kinship (belonging to someone's kindred) offers an additional recruitment principle for dyadic relationships (in addition to spatial distance), it should not surprise us that kindred-endogamous marriages are sometimes contracted across relatively considerable distances (especially in *Mayzīyya*). Such marriages do not prove that after all there is a preference for kindred marriage. They also show that kinsmen sometimes have a somewhat larger chance to belong to each other's *mutashrin* (partners in intensive dyadic relations) than non-kin. However, once recruited to the set of *mutashrin*, Ego's kindred do not stand a greater chance to become Ego's marriage partners, than Ego's other, non-kindred *mutashrin*.

Therefore my conclusion is that in Ḥumiriyya there is no preference for kindred endogamy.

The same would apply, a fortiori, with regard to specific categories within the kindred, notably the alleged preference for marriage with close agnates.'

In common with other Arab societies in Ḥumiriyya the marriage between close agnates (e.g. between Ego and FBD) is allowed and occurs with a certain frequency. Arab societies have therefore featured in anthropological discussions of marital systems (e.g. Chelhod 1965; Cuisenier 1962; Jonḥout n.d.; Murphy & Kasdan 1967; Banck 1968). The Ḥumiri data in the way I have interpreted them in the present book) could make the following contribution to these discussions:

Marriage between close agnates (which many authors have attributed to the participants's conscious strategy to keep the paternal inheritance safely within the set of close agnates) occurs not so much because it is preferred but because most close agnates do not belong to a prohibited category amidst the total supply of potential marriage partners. Also the data in Table 16 and Fig. 12 suggest that the marriage between (close) agnates is more a question of favourable supply at a given moment of time than of explicit, systematic preference: for we see that marriages within one's own agnatic grouping only begin to occur among the largest agnatic groupings. Among the smaller agnatic groupings the chance is much slighter that at the same time a marriageable daughter and a marriageable son are available. But possibly other factors are at play here, such as economic considerations: a close-agnatic marriage is cheap, and it does prevent the dissipation of the heritage.

- My data show that *agnatic endogamy should not be treated as an isolated phenomenon in its own right*: structurally it is only one of the forms under which kindred endogamy can manifest itself., The frequencies with which non-agnatic kindred endogamy occurs (Table 19 suggests that inside the kindred, and with regard to marriage, the distinction between agnates and non-agnates is not very relevant. The results of the analysis of other forms of interaction (the preceding Chapter 10) point in the same direction.
- Factual kinship can only be a factor for day-to-day interaction to the extent to which this kinship is perceived by the actors. This is only the case for relatively

short kinship chains. The influence of kinship on interaction in general, and also on marriage, is relatively limited. Whatever is at work between the actors in Ḥumiri society, cannot be described, nor be predicted, on the basis of a genealogy. For insight in the marriage pattern it is much more relevant to look at the pattern of residence, day-to-day interactions, and social relationships at a certain moment of time.

#CASE 11.1. AGNATIC FICTION AND MARRIAGE IN THE HANDS OF CUISENIER (1962). One example of the distortion to which the slavish counting of marriage relations (without sufficient insight in the social background) can lead is Cuisenier's (1962) study. As is clear from note 14 his data are very well comparable to those from Ḥumiriyya. Cuisenier limits himself to marital relations between historic, actual agnates – members of an ortholineage which he has constructed without the slightest reference to the problematics of genealogical manipulations which has dominated the first few chapters of the present book. Cuisenier takes it that agnatic kinship is always relevant for marital choice, regardless of the length of pre-marital kinship chains between bride and bridegroom. Most chains in Cuisenier's data contain many more than 6 elements; and the number of 6 elements represents the upper limit of more or less reliable genealogical knowledge, above which I found (in Ḥumiriyya) genealogical manipulation to occur, so that the actor's specific genealogical recognition of agnatic kinship is unlikely to be a decisive factor in the interaction of actors. Cuisenier does not assess the relevance of non-agnatic kinship for marital choice. He does not discuss spatial distance as a possible factor of marital choice. He ignores such intensive dyadic relationships between bride-givers and bride-takers as may precede marriage. Little wonder then that Cuisenier manages to arrive at a model of astonishing simplicity: he argues that his numerical data can be explained if we assume that 1/3 of all brothers marry endogamously' (which in Cuisenier's thinking means agnatic endogamy regardless of the degree of kinship *i.e.* the length of the kinship chain), and 2/3 of all brother marries 'exogamously' (*i.e.* with non-agnates regardless of the nature and shortness of the kinship chain). As an additional recommendation that structuralist-orientated author adds that such a model fits in with 'la pensée indigène' (Cuisenier 1962: 104).

11.6. Further aspects of the marriage pattern: Village exogamy / endogamy, as well as marital distance

In the following analysis kinship and descent will remain out of scope, and the terms endogamy and exogamy will be used exclusively in reference to the *village* where a woman's family of orientation (usually her close agnates) resided at the time of her wedding. A woman is then said to be married exogamously (in short: 'is exogamous') if at the time of her marriage her family of orientation lived in a different village⁷⁸ than her husband; endogamy then refers to the alternative case. Women, not marriages are

⁷⁸ How to operationally identify a Ḥumiri village? This is normally not a problem at all. Villages, of which there is a very limited number in any valley, stand out by a more or less consensually used name (although toponyms are notorious for their situational and perspectival application, especially if the name of a mythical ancestor is part of that name); the concentration of dwellings; usually a substantial, unmistakable stretch of uninhabited land separating it from other nearby villages; and by the concentration of various material characteristic attributes (which may be a shrine, a men's assembly, a cemetery, etc.) which, in the overall structure of spatial segmentation, defines the village as such within a structure of neighbourhood, compounds, valleys etc. Nonetheless the boundary between one village and the next is not always completely unequivocal; in Volume II we shall discuss the case of the border area between the villages of Mayziyya and Sidi Mhammad, where various ambiguities turn out to be piled upon each other, resulting in a marked flaw in segmentary transparency.

the units of analysis. When a woman has had more than one husband successively (which is quite a common situation in Ḥumiriyya, and not a recent innovation either, as is demonstrated by the extensive genealogical material doften going back to the late 18th c. CE), only her last marriage is included in the analysis, so as not to clutter our data with past marriages which may not reflect conditions prevailing today; her place of residence during this last marriage then determines whether that marriage is to be regarded as endogamous or exogamous. In this way we obtain as accurate a picture as possible of the present-day marriage pattern.

The research area contains six villages. For each village two sets of married women are relevant, for analysis of the marriage pattern:

- The set of village wives: all living women who at the time of research (1968) lived in the village concerned as wife or widow of a male inhabitant. A resident woman is endogamous if she already lived in the village at the time of her wedding; and exogamous in the other case.
- The set of village daughters: all living married women whose family of orientation resided in the village concerned at the time of the wedding. A village daughter is endogamous if she is married within her own village, and exogamous in the other case.

For each village the set of village wives partially overlaps the set of village daughters: endogamous village daughters are identical to endogamous village wives, except in those (rare) cases where, during marriage, they have left the village because of migration. (Migrations are frequent but take place predominantly within the village). The analysis concerns itself solely with living women. Deceased women are ignored because the data concerned are less complete and difficult to check. However, it is unlikely that the deceased would differ significantly from living women of the same generations with regard to the variables analysed here.

A census survey produces the set of village wives. The set of village daughters can be determined from the reconstructed genealogies of the various ortholineages as settled in the village. As discussed at length in the earliest chapters of the present book, genealogies of ortholineages are not easily established: relatives who have out-migrated (including village daughters) leave few traces in local genealogical knowledge, and the disentangling of the many cases of genealogical manipulation, and of complex kin relationships involving multiple marriages and re-marriages in previous generations, take patient and extensive exposure to local relationships and gossip through participant observation. That is why only my genealogies of the villages of Sidī Mḥammad and Mayzīyya (where I personally resided, daily engaged in participant observation and informal conversations, and where I returned in 1970, 1979, and 2002) are sufficiently complete to determine the set of village daughters. My data for the village of Hamraya are partially based on collective research of all participants of the University of Amsterdam's research training project of 1968 under the supervision of K.W. van der Veen. They remain incomplete but can fortunately be complemented by data collected by Hartong (1968: 66 *f.*) on the 'Auniyya agnatic core Information about village daughters are too incomplete for the villages of Fidh al-Missay and Khamay-siyya; for these villages only the situation concerning village wives have been analysed. In the entire analysis the village of Raml al-²Atrus had to be ignored; admittedly, it

belongs to the research area, but it is extremely small (only 5 families in 1968) and after having been uninhabited for decades, has only been occupied again from 1958.

Throughout the analysis the emphasis will be placed on village wives because the data pertaining to them are so much more complete and reliable than those on village daughters.

For the analysis of spatial distance in connection with the marriage pattern, I define marital distance as: the spatial distance in kms. between a woman's family of orientation and the place of residence of the husband at the time of the wedding. Detailed data about present and earlier places of residence of families in the research area make it possible to determine the marital distances for practically all relevant marriages. In this connection I have made use of the official survey map (Institut Géographique National [year]), the map provided by Hartong (1968: 5) and my own sketches as made in the field.

The nature of my data on relative wealth is discussed below.

My analysis covers the following aspects of the marriage pattern in the research area.

- The endogamy/ exogamy ratio in all five villages (excluding Raml al-^cAtrus).
- Marital distance in all five villages.
- The association between marital distance and relative wealth in the villages of Sidi Mḥammad, Mayziyya and Hamraya.
- A comparison of the five villages according to size, endogamy/ exogamy ratio among village wives, and marital distance among village wives.

11.6.5. The endogamy/ exogamy ratio

Tables 11.6 and 11.7 below supply the data on the endogamy/ exogamy ratio. For a few resident wives it could not be ascertained if they were endogamous or exogamous. Neglecting these missing cases would have resulted in serious distortion. I solved the problem in the following way. Table 11.7 lists three figures of which two in parentheses; the figure in brackets gives the minimal number (or percentage) of endogamous women, assuming all missing cases to be exogamous. The second figure in brackets gives the maximal number of endogamous women, assuming all missing cases to be endogamous. The figure before the bracket gives the best estimate: the average.

	Sidi Mḥammad	Mayziyya	Hamraya	Khamaysiyya	Fidh al-Missay	total
endo-gamous	24	7 (6-8)	19.5 (21-18)	4 (35)	7.5 (5-10)	57.5 (51-64)
	59%	33% (29-38%)	48% (51-44%)	31% (23-39%)	47% (31-63%)	47% (42-52%)
exo-gamous	17	7 (6-8)	21.5 (20-23)	9 (10-8)	8.5 (6-11)	64.5 (71-58)
	41%	67 (71-62%)	52% (49-56%)	69% (77-61%)	53% (69-37%)	53% (58-48%)
total	41	14	41	13	16	122
	100%	100%	100%	100%	100%	100%

Table 11.6. The endogamy/ exogamy ratio among village wives

The exogamy/ endogamy ratio among village daughters is only known for the villages of Sidi Mḥammad, Mayzīyya and Hamraya:

	Sidi Mḥammad	Mayzīyya	Hamraya	total
endogamous	25	6	20	51
	44%	32%	36%	100%
exogamous	32	13	36	81
	56%	68%	64%	100%
total	57	19	56	132
	100%	100%	100%	100%

Table 11.7. The endogamy/ exogamy ratio among village daughters from Sidi Mḥammad, Mayziyya and Hamraya

On the basis of Tables 11.6 and 11.7 we can conclude:

- Between the villages great differences exist in the endogamy/ exogamy ratio, especially with regard to village wives. Endogamy is highest in Sidi Mḥammad.
- Both Sidi Mḥammad and Hamraya have given more women (in the way of exogamous village daughters) to other villages in the last decades preceding 1968 than they have received from other villages (in the way of exogamous village wives). A possible explanation of this phenomenon lies in *overpopulation*: Hamraya and in Sidi Mḥammad are the largest villages in the research area, and have reached a point where not only village daughters, but also their potential local husbands are pushed out to other villages and to the urban centres of Tunisia.

11.6.6. Marital distance in five villages

Village wives

The median and the range are simple but effective statistics if we wish to compare the distribution of marital distances of the village wives in the various villages. Table 11.8 reflects these measurements:

	Sidi Mḥammad	Mayziyya	Fidh al-Missay	Hamraya	Khamaysiyya	Total
median (km.)	0.4	1.8	0.8-09	0.8	1.4	0.7
range (km)	0.0-18	0.0-7.5	0.0-4.0	0.0-17	0.0-65	0.0-18

Table 11.8. Distribution of marital distance among village wives

The median of the marital distance of village wives is at Sidi Mḥammad much lower than in the other villages. This can be explained in two ways:

- The marital distance of exogamous village wives in Sidi Mḥammad is lower than in other villages.
- The marital distance of the *exogamous* village wives in Sidi Mḥammad is it itself

not exceptional when compared to that of the other villages, but the median marital distance in Sidi Mḥammad is so low because so many village wives are married village-endogamously (*i.e.* over a distance not exceeding a few hundred metres).

The validity of the first explanation can be tested by comparing, for each of the villages separately, the distribution of marital distances for exogamous village wives with that of Sidi Mḥammad. For this purpose we use the U-test as described by Siegel n.d. This does not yield significant results (Table 11.8); we can therefore discard the first explanation, and accept the second:

		U	n ₁	n ₂	conclusion
Sidi Mḥammad against:	Mayziyya	44	13	17	not significant
	Khamaysiyya	73.5	10	17	not significant
	Hamraya	124.5	14	17	not significant
	Fidh al-Missay	41	6	17	not significant

Table 11.9. Test statistics and results: marital distances of exogamous village wives in four villages, as compared with the village of Sidi Mḥammad

11.6.7. Marital distance in three villages

11.6.7.1. Village daughters

From the point of view of relations between villages it is important whether there is a difference in marital distance between village wives and village daughters. In order to assess this difference we may compare, for each of the villages under study, the distribution of marital distances for the total set of village wives with that of the total set of village daughters. This method has the drawback that both sets overlap when it comes to village-endogamous marriages. In fact we are only concerned with a difference in marital distance between exogamous village wives on the one hand and exogamous village daughters on the other.

When we limit the analysis, in this manner, to the villages of Sidi Mḥammad, Mayziyya and Hamraya, we obtain the following results.

- *Sidi Mḥammad*: There are indications that the marital distance of village daughters is larger than that of ‘resident exogamous women’, but this relation falls just short of being significant ($z = 1.59$; $p = 0.06$). The village of Sidi Mḥammad appears to attract women for its male inhabitants (in as far as they do not take a wives from their own village), within a closer range than in which it allows its nubile women to marry (in as far as these marry outside their own village) .
- *Mayziyya and Hamraya*: For both villages the marital distance of exogamous village wives does not differ significantly from that of exogamous village daughters (Mayziyya: $U = 77.5$; $n_1 = 13$; $n_2 = 13$; Hamraya: $U = \dots$; $n_1 = \dots$; $n_2 = \dots$ [.

11.6.8. Marital distance and wealth

A relation between marital distance and wealth may be postulated on the basis of the general structure of Ḥumirī society. The richer and the more powerful householders have many more inter-local contacts than the other village inhabitants; it is to be expected that this fact is also expressed in the marriage pattern. In order to test the association between marital distance and wealth, I proceeded as follows. Reliable data about relative wealth (based on a modified Kaufmann test are available for Sidi Mḥammad, Mayziyya and Hamraya. I analysed these villages separately. As sample I chose the set of village wives. For each woman I ascertained her marital distance and the wealth of the head of her household – possibly herself, if she is widowed. The present-day wealth of a household usually offers a reasonable estimate of the wealth categories of bride-givers and bride-takers at the time the marriage was contracted. For each village separately, marital distance was related to wealth as divided in three categories. This resulted in the following:

- *Sidi Mḥammad*: Here the rich have a significantly greater marital distance than the poor and the middle group ($z = 1.84$). Moreover, the poor have a significantly greater marital distance than the middle group ($z = 2.01$).
- *Hamraya*: In this village there turned out to be no significant difference in marital distance between ‘rich’ and ‘middle’ ($U = 18$; $n_1 = 7$; $n_2 = 7$). There were strong indications that ‘rich and middle’ had larger marital distances than ‘poor’, but this relations just fell short of being significant ($z = 1.57$; $p = 0.058$). Finally it turned out that ‘middle’ has significantly greater marital distances than ‘poor’ ($U = 20.5$; $n_1 = 7$; $n_2 = 12$).
- *Mayziyya*: In Mayziyya ‘rich’ turned out to have greater marital distances than ‘poor and middle’ ($U = 10.5$; $n_1 = 4$; $n_2 = 15$). However, there turned out to be no significant difference in marital distance between ‘poor’ and ‘middle’ ($z = 0.14$).

For the category ‘rich’ the three villages show the same pattern: ‘Rich’ marries over greater distances than ‘poor’. The behaviour of the middle group varies: the middle-wealth category in Sidi Mḥammad has smaller, but in Hamraya larger marital distance than the poorest inhabitants.

11.6.9. Five villages compared according to size, endogamy/exogamy ratio, and marital distance

The five villages whose marriage pattern was analysed here, markedly differ according to marital distance and according to the endogamy/exogamy ratio. How do we explain those differences?

An obvious explanation is in terms of village size. The larger a village, the greater the demographic probability that the inhabitants find a suitable marriage partner nearby, in their own village. As indicated above, Ḥumirī villages are usually separated from one another by stretches of uninhabited terrain, which are at least a few hundred meters wide; considering that the order of magnitude of marital distance is similar, the marital distance of village-exogamous marriages tends to be significantly larger than of

village-endogamous marriages.

The number of resident families is a good measure for village size. A household, I operationalize as: a group of people living together in a house which has its own independent outside entrance).

In the following, five villages are ranked according to size, the endogamy/ exogamy ratio of village wives, and village wives' median marital distance.

	rank		
	size†)	endogamy/ exogamy ratio	median marital distance
Sidi Mḥammad	1	1	1
Mayziyya	3	4	5
Hamraya°)	2	2	2
Khamaysiyya	5	5	4
Fidh al-Missay	4	3	3

†) Volume I : 164 [check] [internal reference]] gives the exact number of households

°) including all families from all ortholineages, not only the 'Auniyya agnatic core

Table 11.10. Five villages compared as to size, endogamy and marital distance

There is a significant relation between village size of village and endogamy/ exogamy ratio ($r_s = 0.90$; $n = 5$). There is no significant relation between village size of village and median marital distance ($r_s = 0.70$; $n = 5$). There is however a significant association between the endogamy/ exogamy ratio and the median marital distance ($r_s = 0.90$; $n = 5$) This confirms the earlier explanation of the a-typical distribution of marital distance in Sidi Mḥammad, attributing the low median marital distance in Sidi Mḥammad to the high incidence of village endogamy there. Probably, if the analysis could be extended to more than only five villages, a significant association would also be found to exist between median marital distance and village size.

But can we relegate all differences in the marriage pattern between these villages to differences in village size? Then Sidi Mḥammad and Hamraya, of virtually the same size (46 and 45 households respectively), would have to have a virtually identical marriage pattern. However, we have seen notable differences between both these villages. For Hamraya, the endogamy/ exogamy,ration, and the median marital distance, correspond with the average for the research area as a whole. Hamraya's daughters (in so far as they marry outside their own village) are married within the same range as within which the local male inhabitants recruit their wives (in so far as these do not come from the village itself); the economic middle group here has greater marital distances than the poorest group. By contrast, for Sidi Mḥammad the endogamy/ exogamy ratio is extremely high, the median marital distance is extremely low, the poor have greater marital distance than the middle group, and Sidi Mḥammad's daughters (if exogamous) are married out across a significantly wider range than that within which the male inhabitants recruit their wives (if exogamous).

The marriage pattern of Hamraya is systematically understandable by reference to the

overall structure of Ḥumiri society. The inverted marriage pattern of Sidi Mḥammad may be partly explained by the fact that this village is a regionally important religious centre (see Volume II); another important factor is that of the high level of land alienation and class formation, due to the fact that for more than half a century chiefs have resided here, while most of the local arable land was alienated early in the colonial period.⁷⁹

As we shall see in Volume II of this book, the larger part of individual pilgrimage in Ḥumiriyya consists of original pilgrimage. During her lifetime, a woman migrates at least once to another spatial segment at a certain level: at her marriage. When she marries in a nearby sub-neighbourhood within the same neighbourhood, she almost always keeps the same shrines as her local shrines. However, the greater the segmentary distance which a woman crosses through her marriage, the more the local pilgrimage pattern of the segment into which she marries will differ from the pattern prevailing in her segment of origin. Through a marriage outside her own village and especially her own valley, a woman certainly acquires one or more 'original shrines' – peculiar only to her among all the other women within the spatial segments where she lives here married life.

Moreover, a woman obtains original shrines through migrations of her family before or after marriage. The larger the segmentary distance over which a migration occurs (before, in connection with, or after marriage), the more likely it is that a woman acquires important *original shrines*, at which throughout the rest of her life (on supernatural sanction from the invisible saint or saints involved) she will be obliged to direct personal forms of devotion, such as semi-annual pilgrimage, and the dedication of festive, meat-containing meals over which the name of the saint is explicitly spoken as a form of sacrifice.

Volume II will explain how the importance of a shrine in the local hierarchy essentially depends on the segmentary level of the segment of which that shrine is a characteristic attribute. If the shrine belongs to the highest category of importance, it will be the characteristic attribute of a valley. It will have its own festival and the material features of the shrine will be elaborate and ornate. I have therefore divided the shrines into three classes of importance. Valley's central saints constitute the highest class. The middle class comprises saints who are visited at the level of village and super-neighbourhood, provided they are regarded by the actors as more or less important shrines, and are not too inconspicuous in outside appearance. The rest belongs to the lowest class of shrines.

⁷⁹ My fieldwork novel *Een Buik Openen / Opening Up a Belly* is set in the research area and mainly shows how gradually the extreme land alienation in Sidi Mḥammad was brought about. In the early 20th c. CE, local villagers living between ʿAin Raml and the Wad al-Kabir killed two Kabyl itinerant merchants who abused one or more local girls; in a bid to further the 'pacification' of the region which had been progressing at a – from the administration's point of view – disappointing pace, the recently established colonial administration responded harshly by evicting the culprits from their land and issuing that terrain to an Italian colonial farmer; moreover the valley became the place of residence of several local representatives (*shayḥs*) who greatly expanded the land area in their possession at the expense of the other local villagers; and finally that chiefly family bought over the colonial farm and used it for their own further enrichment.

Now, when a woman remains residing in her own village, she keeps as her local shrines the shrines associated with the village and the valley; only the unimportant shrines at neighbourhood and sub-neighbourhood level then become her original shrines. Only when a woman, through migration, goes and lives in another valley does she acquire an original shrine of the greatest importance: the central shrine of the valley where she lived before.

It is an explicit norm of Ḥumirī society that a woman must visit, and must continue to visit throughout life, all her original shrines. However, many women do not comply. With which factors then is the observance of original pilgrimage associated? That is a question I shall consider at length in Volume II of this book. but meanwhile it should be clear that the analysis of the Ḥumiri marriage pattern, far from being a mere hobby-horse of a kinship-obsessed obsolete forms of classic anthropology, is of the greatest importance for an understanding of the patterns of segmentation and pilgrimage that make up Ḥumirī popular religion.

11.7. The kindred in Ḥumirīyya and in the Greek highlands

For certain structural aspects (notably the composition of the set of Ego's nearest neighbours) it turned out to be meaningful to differentiate between the various kinship categories within the kindred. In general, though, the difference between close agnates, distant agnates, close cognates/affines and distant cognates/affines turned out to be immaterial.

However, implied in the specific way in which I have defined these categories there is already a certain bias in favour of agnates: for agnatic kinship is still taken into account (as 'distant') at a length of chain where cognatic/affinal kinship has already been relegated to the category of 'no kinship'. This bias is in line with the fact that the Ḥumirīs tend to have a larger knowledge of agnatic kinship chains than of cognatic and affinal ones; however, it is not impossible that this bias has influenced the results of quantitative analysis in a way unforeseen.

I have brought out the significance of the kindred in Ḥumirīyya as a statistical datum. Now in many societies statistical tendencies have, etically, been found by ethnographers, whereas at the conscious, emic level the actual actors in those societies may not have the slightest conscious awareness of these tendencies. So the question is: to what extent do the Ḥumirīs themselves recognise the significance of the kindred, including non-agnates, in their society?

The answer must be given at two different levels,. At the abstract level of general, abstract, normative statements concerning their own society the actors do dissimulate the significance of the kindred, remaining faithful to the indigenous societal ideology which claims agnatic kinship to constitute the dominant structural principle of Ḥumirī society.

However, when concrete situations are involved (of the types 'A visits B', 'C helps D to bring in the harvest', 'E is a near neighbour of F'), then it turns out that one recognises non-agnatic kinship (provided it is of a limited length of chain) to constitute an important determinant of interaction. For the set of somebody's close and distant

agnates, cognates and affines the Ḥumirī have a term *famīlya* (probably a French loan word, perhaps an Italian, even a Roman / Latin one), which means nothing else but kindred. On many occasions, when I questioned individual actors about their maintaining a dyadic relationship with particular non-agnates, they would justify that relationship by pointing out that the partners belonged to each other's *famīlya*; the kinship chains produced on those occasions made it clear that such identification in terms of the *famīlya* is very common among people I have analytically defined as distant cognates / affines. Sometimes such identifications (complete with the accompanying specification of kinship chain) would even occur between people who in terms of my classification of kinship chains would be too distantly related to be still counted to each other's kindred.

For analytical purposes I had to impose fairly strict definitions on the various categories of kinship within the kindred, and on the kindred as distinct from non-kin; but of course, in Ḥumiriyya actors do not sharply demarcate the set of people with whom a certain Ego has relationships *on the grounds* of them being linked to Ego by kinship chains of an agnatic, cognatic, affinal, or more typically a mixed nature and of a fairly short length of chain. Actors do have a notion that kinship may be too distant to deserve to be considered as such any longer, but the criterion may shift from situation to situation. Ego will be more inclined to reckon to his *famīlya* A (a good *meziyya* partner, and a generally respected man) than B (a person with whom Ego is in conflict and who, moreover, is generally despised), even though the actual genealogical relationship between Ego and A is only a very distant affinal one, whereas that between Ego and B may be rather closer. For my quantitative analysis I had to define the categories with far more precision and inflexibility; there I let myself be guided by the impression which, after a few months of participation, I had gained with regard to the actors's average genealogical knowledge and their average relative appreciation of the various types and degrees of kinship – as set out in a previous Chapter.

It is illuminating to compare the Ḥumirī kindred with the kindred in another Mediterranean society: that of the Sarakatsani in the Greek highlands as described by Campbell (1963).

The Sarakatsani have a term for kindred: *to soi* (*to soi*), 'those who belong to one'. The main characteristics of their kindred are, according to Campbell (1963: 60):

'It is bilateral, the limits of its extension are precisely defined, and within these limits a man may not marry.'

Campbell (1963: 87) would consider the bilateral emphasis the main feature of the Sarakatsani kindred. The Sarakatsani would appear to have a very well defined apparatus through which they can classify people as either belonging or not belonging to their kindred. For instance:

'The second cousins of his parents are not, in a formal sense, a man's kinsmen and, therefore, he may marry a kinswoman of this degree', 'a man says of this third cousin: "the kindred had left the house"' (Campbell 1963: 85, 86).

Finally Campbell gives an estimate of the size of someone's kindred:

'the average size of an individual's kindred is relatively small, seldom exceeding 250 men, women and children' (Campbell 1963: 77).

When we compare the Sarakatsani and the Ḥumiri kindreds, the following correspondences and differences meet the eye:

- For both there is an indigenous term.
- Both comprise both agnates and cognates; besides, the Ḥumiri kindred also comprises affines. Ḥumiris apply the general Tunisian-Arabic term of *famila* to agnates, cognates and affines alike. Cf. van der Meer (XXXX), speaking of the Ḥumiri kindred: 'The kindred is only of value if it includes affines also'. The demarcation of the Ḥumiri kindred in the present argument, and the inclusion of affines, is discussed in appendix 1.
- The Sarakatsani kindred is sharply delineated, the Ḥumiri one is not.
- The Sarakatsani kindred is exogamous, whereas in Ḥumiriyya marriages between close- and distant agnates, cognates and affines occur frequently.
- The number of people belonging to somebody's kindred is of the same order of magnitude in Ḥumiriyya and among the Sarakatsani:

11.8. The numerical size of the kindred in Ḥumiriyya

I assessed for every member of the sample of 15 heads of households in the villages of Sidi Mḥammad and Mayziyya how many other heads of household in both villages belonged to his or her kindred. This number lay between 7 and 21; the median was 12-13. The households in the research areas comprise an average 5 persons. The members of the sample therefore have, in these two villages alone, a kindred of between c. 35 and c. 105 persons. Because of marital ties and migrations one's kindred is hardly ever confined to one's own village and one neighbouring village; for the kindred in other villages we must certainly add a few dozen percent to the figures as mentioned above. This means that the maximum size of the Ḥumiri kindred does come out somewhat lower than the maximum of 250 persons which Campbell mentions for the Greek mountains.

Despite the differences the kindred plays about the same role in the social organisation in Ḥumiriyya and among the Sarakatsani: it is an extensive pool of potential interaction partners, most of whom Ego acquired as such at birth, and Ego can mobilise them far day-to-day interaction by reference to special claims of a kinship nature.

In the course of Ego's life the composition of his or her kindred is not entirely constant: certain members are lost through death and long-distance migration, whereas others are gained through birth and new marital relationships. Neither among the Sarakatsani, nor in Ḥumiriyya, is the kindred a corporate group (cf. Murdock 1965: 60). For the kindred does not have a common territory, no shared possessions, and only small subsets within the kindred observe (in the form of visits and assistance at festivals, life crises and misfortune; cf. van der Meer 1970) collective activities on the specific basis of their members being each other's kinsmen. Such interaction as occurs between Ego and a member of his or her kindred is of a highly dyadic nature, at least in Ḥumiriyya. Considering such aspects as the time span of actual relationships, the kind of exchanges implied in the relationship, the strength of the claims and the extent of the expectations which the partners in the relationship have vis-à-vis each

other, the intensity of mutual feeling, and such sanctions as general social control may imposed on the breaking-up of such a relationship, we are justified to conclude that dyadic relationships with kindred differ only gradually, if at all, from dyadic relationships with non-kindred neighbours and friends.⁸⁰

Neither in Ḥumiriyya nor among the Sarakatsani (Campbell 1963: 93 *f.*) does the kindred constitute *the only group* from among which Ego recruits his interaction partners.

I shall refrain here from a more general review of the theoretical discussion of the kindred and of bilateral kinship in general.⁸¹ For our present purpose it suffices to have demonstrated that kinship is an independent (albeit not the crucial) determinant in Ḥumiri social organisation, and moreover, that for certain selected aspects of that social organisation it is unnecessary to differentiate kinship (*i.e.* belonging to somebody's kindred) any further in terms of agnates, cognates and affines.

11.9. A remark on specialist status in Ḥumiri social organisation

In general the *mezīyya* and *mutashrin* relationships as discussed above concern non-specialist activities, characterised by a certain frequency of interaction, and the absence of a cash nexus. By contrast, between specialists and clients there is usually only incidental interaction, which in most cases involves payment in cash.

In Ḥumiriyya, specialist status form an extensive field of research, which could be provisionally divided in: political / administrative specialist status (the chief, his assistants, the local committee members of the Tunisian unitary party, the *délégué*, the police officers in the urban centres, etc.); distributive specialist status (shopkeepers, market vendors); artisan specialist status (blacksmith, plough-maker, wood-carver); medical specialists (diviners, healers, modern physicians and nurses in the urban and rural health centres); religious specialists (Qur'an teacher, Qur'an reader, warden of a shrine, ecstatic adept; all these also deal with illness and death), and finally musicians.

This is not the place to engage in an extensive discussion of the interaction between specialists and their clients.⁸² The main point in the present connexion is that the interaction between specialist and client constitutes a breach of the spatial factor in Ḥumiri social organisation.

Someone from village A may very well visit a divination specialist in village B, at a distance of 5 km, even if with regard to other, non-specialist matters there are hardly or none contacts between these two villages. It is only above a certain maximum that

⁸⁰ Cf. Jongmans 1968 and personal communications.

⁸¹ A review of the underlying problematic and an analysis of the relevance of the Ḥumiri kindred in this connexion can be found with Brunt (1969: *passim*). Also van der Meer's research (1970) revealed the importance of the kindred in Ḥumiri social organisation.

⁸² Ḥumiri specialists in general have been researched by van der Klei (forthcoming); Creighton (1969: 75) discussed Ḥumiri specialist status in the realm of illness and religion, and thus borders on my own analysis of religious specialists as presented below.

the influence of spatial distance or spatiality is yet felt: in Ḥumiriyya it is extremely unlikely that someone visits a divination specialist across a distance of as much as twenty km; but for an inhabitant of Sidi Mḥammad it is not uncommon to combine a visit to the weekly market in ʿAin Draham (7 kms as the crow flies, but nearly doubly that distance along the road) with a consultation of a divination specialist (*tekēza*) there. That specialist status can create a relative breach in the structure of spatial segmentation may be explained from the fact that in somebody's immediate spatial environment the supply of potential partners for non-specialist interaction is, *per definition*, much larger than the supply of specialists.

Certain specialist statuses are only filled at a rate of one or two incumbent per village (e.g. a killer of cows), per valley (a Qur'an teacher, a blacksmith) and even per chiefdom or group of chiefdoms (circumciser; prominent divination specialists and prominent members of the ecstatic cult).

It may well happen that from the interaction between specialist and client, across a large spatial distance, also relations develop in the sphere of non-specialist interaction, which after some time may lead to marriages, etc. My data offer some suggestions concerning such a process. If this impression could be confirmed in a more systematic way, specialists would turn out to occupy an even more important place in the social organisation of Ḥumiriyya.

How are specialists in Ḥumiriyya being recruited to their particular specialist status?

Indigenous ideology has it that the inheritance of skills and specialist status follows exclusively agnatic lines, particularly those between father and son.

In fact people who, simultaneously or in succession, occupy the same specialist status, may turn out to belong to the same agnatic group. However, here the same principle is at work which we have seen with regard to everyday interaction and marital relations: agnatic kinship is only one of the possible forms of kindred membership, and forms no exception as compared to the other kinship categories within the kindred. In Volume II I shall discuss at length the complex succession pattern of incumbents of the specialist offices of chief, of shrine warden, and of *fakīr* (ecstatic dancer). Due to the political ambitions of the chiefs to gain ever more control over the domain of popular religion, these three recruitments patterns have grown to be intertwined in the course of the 20th c. CE.

It is useful to assess whether the incumbents of the various specialist status do or do not belong to each other's kindred. I traced the kinship chains between the wardens of the major shrines in the village of Sidi Mḥammad since c. 1900; between ecstatic adepts in the research area since c. 1925; between chiefs of the ʿAtatfa chiefdom since 1883; between these shrine wardens and adepts; shrine wardens and chiefs; and adepts and chiefs. The detailed data and the conclusions they yield (largely in terms of local politics) I shall discuss in Volume II. But some of the major conclusions fit in the present discussion of Ḥumiri social organisation:

- Incumbents of each specialist status as summed up above very often belong to each other's kindred.
- Across and between these various specialist statuses, the incumbents very often belong to each other's kindred.

- *Adepts are significantly more often related to other adepts along close cognatic ties (especially MB/ZS) than non-adepts are;*⁸³ for the other categories within the kindred there is no statistically significant difference between adepts and non-adepts.

To my knowledge, this remarkable significance of close cognatic kinship is not explicitly recognised by the actors themselves. In other words, we are not dealing here with an explicit institution of Ḥumirī society. Yet it is found back in other aspects of the religion, notably in the recruitment of individuals to enter into specific relations with saints; these relationships manifest themselves in pilgrimages to the shrine, and in offerings and sacrifices. The significance of close-cognatic kinship can then be interpreted in the light of the following complex relationships. Maintaining relations with saints is largely a concern of women, with the exception of specialists (wardens of shrines, adepts). Shrines are associated with certain spatial segments. Because of the consistent virilocal residence in Ḥumirīyya (95% of all marriages are virilocal), a woman at marriage usually settles near other shrines than those she used to frequent before her marriage, but in general she retains (at least for the first decade of her marriage) the relationships with the local saints of her own segment. The latter's shrines she continues to visit with her children on a (semi-)annual basis, usually combining these pilgrimages with visits to her relatives in her segment of origin. Hence Ego's relationships with saints do not limit themselves to the local saints of the segment in which Ego is born; they also have to do with: Ego's mother; the mother's own continued relationship with her spatial segment; and thus with Ego's close cognates, primarily the mother's brother. The specialist status of adept is also a form of a permanent relationship between a living human being and a saint. Although this saint venerated in ecstatic dancing is far from always the local saint of mother's segment, it is probable that the explanation for the remarkable significance of close cognatic kinship in the recruitment of adepts lies in this direction. We could also formulate this as follows: in Ḥumirīyya religion is the field of activities where the strong ideological emphasis on agnatic kinship (in itself scarcely reflected in actual interaction) is relaxed, and cognatic ties are stressed in stead. In fact religion (in the form of pilgrimages to original shrines, *i.e.* the shrines in whose proximity she once lived but which she had to leave behind for reasons of virilocal marriage or migration) provides the main occasion for a woman to visit her own segments (where her own agnates dwell) after her marriage.

These results bring out the significance of the kindred for specialist status in Ḥumirīyya, and make us adopt a relative view of the applicability of the lineage model to Ḥumirī society.

⁸³ We could interpret this as an example of the empirical generation advanced by Fortes (XXXX), to the effect that recruitment of religious status often follows a complementary line of descent, *e.g.* matrilineal in an otherwise patrilineal society. However, given the fact that we have had to considerably underplay the extent to which Humiri society is effectively patrilineal, this interpretation in itself may be an artefact of classic anthropology's obsession with unilineal descent as a key to social organisation.

11.10. Further discussion of dyadic relationships in Ḥumiriyya, and the recruitment of partners for such relationships

In Ḥumiriyya, dyadic relationships (and particularly the intensive *mutashrin* relationships) are ideally relationships between kinsmen. The indigenous concept of kinship, which in practice is synonymous with that of 'positive social relationship', and the use of kinship terminology between partners in dyadic relationships regardless of traceable and recognised biological kin ties, make sure that this ideal is fulfilled even if between the partners lacking such ties.

The relationship, as posed above, between spatial distance and the distribution of kinsmen around Ego's dwelling makes it all the more likely that kindred are each other's partners in dyadic relationships: for they happen to be close at hand.

It is understandable that in Ḥumiri peasant society dyadic relations tend to be most profitable for both partners, if these partners are nearest neighbours. The communication barrier of spatial distance is then minimum, without loss of time they can keep up a constant stream of exchanges, land and gardens of the one partner tend to border on that of the other partner which further facilitates co-operation, as *mutashrin* they can guard over each other's house, cattle, women and honour, and moreover a constant critical check is possible on the partner's relationships with third parties.

Therefore it needs not surprise us that there exist *mezīyya* relationships between virtually all nearest neighbours (up to a distance of 125 m from Ego). Some informants even consider *mutashrin* and nearest neighbours to be synonymous expressions. In the first phases of the dispersion process of parental families, brothers live on one compound as nearest neighbours, and to that situation the term *mutashrin* is eminently applicable: for the co-residence of brothers on the undivided heritage of their father is the model of interpersonal relationships in Ḥumiriyya. For Ḥumiri's the ideal relationship between brothers is that of *mutashrin*, and alternatively *mutashrin* are counted as brothers.

There is no reason to postulate that *mutashrin* relationships between close agnates differ essentially from *mutashrin* relationships between people who are cognatic or affinal kin, or who do not belong to each other's kindred at all. In *mutashrin* relationships actual kinship does not play a role any more. This is in accordance with the finding brought out above: kinship is not a statistically significant factor in the recruitment of interaction partners among nearest neighbours.

In my argument spatiality has appeared as a major structuring principle, along with kinship. Kinship turned out to be subject to constant manipulation. But of course also spatiality can be manipulated, and in fact in a much simpler way than kinship: *through moving!*

If *mutashrin* are nearest neighbours, the breaking up of such a relationship almost invariably is accompanied by the moving of one of the partners. Alternatively: if a nearest neighbour decides to move this is often a sign that he breaks up the existing *mutashrin* relationships which have hitherto surrounded him at his place of residence. However, there may be other or additional reasons involved: putting to use land which one owns elsewhere, or the attempt (to be discussed in part II) to escape the evil

influences of spirits at the previous place of residence. But again, a manifest interpretation in terms of evil spirits is likely to be an idiom (which the actors among themselves scarcely fail to understand and appreciate, either consciously or subconsciously) to express the deterioration of social relations between neighbours.

Because nearest neighbours are deeply involved in each other's lives, and may seriously harm or benefit each other as the case may be, somebody will hardly ever move and become somebody else's nearest neighbour unless such a move has already been embedded in the existence of a dyadic relationship (preferable of the *mutashrin* type) between those involved. Alternatively, if such a relationship exists while the partners are not nearest neighbours, we often see that they spatially adjust this state of affairs by becoming nearest neighbour, through moving.

An analysis of eighteen cases of moving in the village of Sidī Mḥammad from 1966 (cf. Beeker 1967) through 1968 made it clear that the actors, through moving, are constantly revising the composition of the set of their nearest neighbours in such a way that this set ends containing more actual partners in dyadic relations (and especially *mutashrin* partners). The relatively large number of moves (involving more than a quarter of all households) in such a short period of barely two years is also an illustration of the rapid changes which the pattern of dyadic relationships undergoes in present-day Ḥumiriyya. Finally these moves bring out a central aspect of Ḥumiri dyadic relationships which so far has not been emphasised: *the role of relative wealth*. Below I shall dwell on this theme at somewhat greater length. When we interpret the eighteen moves from a point of view of relative wealth the following pattern emerges. Through moving, the poorer part of the village population (the large majority) almost invariably revises the composition of the set of their nearest neighbours in such a way, that this set ends up comprising more households which are more wealthy, as fewer households which are less wealthy, than they themselves are. The more wealthy households follow essentially the same strategy, but because they themselves are already near the top of the scale, their moving usually consists in a retreat towards the periphery of the village, where they have far fewer (potentially profiteering) neighbours; as a rule, these few remaining neighbours then tend to be in a similar economic bracket as the moving households are themselves. In contemporary Ḥumiriyya wealth is scarce and labour power is abundant and under-utilised. For a poor man it is therefore advantageous to have a dyadic relationship with a richer man, for it offers the former one of the very rare opportunities to transform labour power into cash and goods. For the rich man, however, such relationships tend to be too costly: they siphon off his wealth in exchange for a labour supply in excess of what the ongoing productive activities at the fields and at the compound require. An exceptional case is formed by those richer people who are active in local politics: as chief or assistant to a chief. Their position of power is considerable, but it is also vulnerable due to the countervailing power of superior officers and of party members in the villages. In fact, so vulnerable is their power that they cannot afford to refuse dyadic relations to a large number of poorer fellow-villagers, however negligible the latter's access to formal channels of power is. And as a result the village poor flock to the politically ambitious rich and become their nearest neighbours, often concealing actual relations of inequality and patronage under an idiom of *mezīyya* and *mutashrin* relationships.

This analysis is largely in line with the one by Jongmans (1968) for the village of Mḥamdīyya. The proximity of the latter village to the urban centre of ʿAin Drāham, and the absence of formal political office-bearers in Mḥamdīyya, account for the (slight) differences between that village and Sīdī Mḥammad.

There are two sides, then, to the relationship between spatial distance (or spatiality) and the recruitment of partners for dyadic interaction. On the one hand people enter into such relationship because they are each other's neighbours; this particularly applies to close agnates, and (in the first phases of the dispersion process of parental families) to new generations of initially *remote* agnatic groups which because of immigration have come to reside in each other proximity. But on the other hand one also attract people as nearest neighbours, because one already has dyadic relationships with them.

In fact the dispersion process of parental families should not be seen as an autonomous datum, which can be wholly explained by the inherent dynamics of the Ḥumīrī family as if this were a process *sui generis*. Sons and brothers who live together on one compound are each other's *mutashrin*. They stay together on the same compound, as long as the advantages (co-operation, access to land) outweigh the disadvantages: friction as generated through co-operation, especially between women (*cf.* Beeker 1967), and quarrels over the impending division of the inheritance. Whenever the balance tilts in the negative direction, some of the brothers or sons will leave the compound: sometimes in order to settle in isolation, without new neighbours, on a piece of land that hitherto has lain fallow, both usually in order to replace their previous, close agnatic neighbours by a new set of neighbours. The latter are typically not close agnates, yet kindred, and as long as the new situation may endure one will attempt to further develop, with them, such dyadic relations as did already exist, across a distance, in the time when the original family compound was still intact.

While my quantitative analyses show that spatiality is of crucial significance in the recruitment of partners in dyadic relationships, there are also additional possibilities of recruitment. The most obvious among the latter is, of course, kinship; its significance in this connexion we have already discussed. Moreover I mentioned specialist status as a relative breach of the principle of spatiality.

Another such breach is offered by the *unemployment relief organisation*, which daily brings together adult men who have been recruited from villages within a radius of about 5 km. In this way the organisation brings together people most of whom would otherwise seldom if at all interact. In this respect the relief work is an important channel of communication: for intelligence, news, appointments, saintly festivals to be arranged, the availability of marriage partners. And besides I know of a few cases where the co-operation in the context of the relief organisation gave rise to dyadic relationships also outside the work context, between partners living several kilometers apart.

Yet other opportunities to recruit partners for dyadic relationships are offered by the regular visits villagers pay to the urban centres (and particularly to the offices of the unemployment relief organisation, the market, tea houses, shops, the hospital, the court, and other government establishments), and the pilgrimages to regional saintly festivals.

The discussions in this section then lead to the following conclusion. In the course of my argument spatiality proved to be an important structural datum in Ḥumirī society, of far greater significance for instance than kinship. However, we should not fall into the trap of according to spatiality the status of an all-explaining, all-structuring principle – the mistake anthropologists have often committed with regard to kinship and descent. As a principle for the recruitment of day-to-day interaction partners spatiality exists side by side with other such principles; and on the other hand: often dwelling in each other's proximity is (given the marked spatial mobility of the Ḥumirī household) not only the cause of further interaction, but also the result, the condensation, the manifestation, of relations which were already in existence before the people involved because near neighbours. Moreover relative wealth turns out to be an important factor in the emergence and persistence of dyadic relations in Ḥumiriyya.

11.11. Residential dyadic relationships and their strategies: A quantitative analysis of residential moves in the villages of Sidi Mḥammad and Mayziyya (1966-1970)⁸⁴

11.11.1. Introduction

Despite the great ideological emphasis, in Ḥumirī society, on residential stability, in fact there is a considerable amount of residential movement. In 1966 the village of Sidi Mḥammad was the scene of Beeker's research on the prevailing residential pattern, against the background of government plans then existing of moving the entire village to an uninhabited area in closer proximity of the shrine of Sidi Mḥammad al-Kabir. The detailed data gathered in that context (Beeker 1967) enable us to trace such residential moves of households as occurred between 1966 and the time of my initial field-work, 1968. During my research I could also trace such recent moves as had occurred higher on the slope, in the village of Mayziyya. When I visited the area again in 1970, I could assess which of householders who had moved in the period 1966-1968 had remained on their new site, and which had moved on to yet another site. All in all as many as 18 moves could be recorded for the period 1966-68. The considerable background information obtained for each householder concerned before and after the move enables us to subject the residential micro-dynamics of households in the 1960s to detailed quantitative analysis, which is the subject of the present section.

11.11.2. The data set

In principle, for each residential move the following data are available (the moving householder is henceforth identified as Ego):

- The composition of the set of Ego's nearest neighbours (householders only)

⁸⁴ This section, although entirely based on the empirical data collected during the 1968 fieldwork, yet was not part of the 1970 thesis but only written much later, when my skills at multivariate analysis had been further developed.

before the move and after the move. As nearest neighbour we count anyone who lives within a 125 m. radius a somewhat arbitrary criterion but which reflects observed patterns of everyday interaction. As it turned out, Ego;s had between two and eight nearest neighbours before the move, with a median at six ; and between one and thirteen neighbours after the move, with a median at five.

- The relative wealth and the kinship relation to Ego of each member of these sets of nearest neighbours both before and after the move. As throughout our analysis, relative wealth is scored on an ordinal scale from 1 (rich) to 8 (poor).
- The stability of the residential move. We count a residential move as stable if within the period covered (1966-68) it was not followed by another such move. Out of a total of eighteen, three moves were unstable.
- Intra-village nature of the residential move. Out of a total of eighteen, sixteen were intra-village, two were moves away from the village.
- Ego's relative wealth, as measured in the manner which is duly set out below, and displaying an ordinal distribution as shown in Table RR1.
- Ego's age, ranging from 22 to 52 years of age, with a median at 32.
- Distance (in meters) between Ego's dwelling and the main shrine of Sidi Mḥammad al-Wilda, both before and after the move. Distance before the move ranged from 50 to 425 m, with a median on 125 m; and after the move Ego's found themselves at distances from 110 to 480 m, with median at 185 m. I have included this variable because some villagers explained their move as motivated by the desire to reside closer to the shrine of the valley's main saint – a recognised source of blessing (*baraka*).
- Distance (in meters) over which Ego moved house. Of the intra-village moves, the distances lay between 125 and 780 m, with a median of 225 m.

The residential moves in question may be read from the map presented above as Fig. XXXX.

Since a few of these data happen to be missing, we actually employ a data set consisting of fifteen full non-missing cases.

11.11.3. Possible strategies in residential moves

In Ḥumiriyya, being nearest neighbours amounts to a quasi-kinship relationship which implies generalized dyadic exchange between heads of households and their representatives. There are considerable wealth differences between the households in Sidi Mḥammad and Mayziyya, some of them related to the householder's individual life cycle (young men tending to be poorer than mature men with adult sons), others to the general rise of social inequality due to outside influences: the penetration of capitalism through labour migration, and the state with differential access to salaried employment and profitable positions of power. Since in our relevant data set the median distance over which Ego moved lies at 225 m, in most cases it can be said that, through the move, Ego obtained a quite different set of nearest neighbours. Assuming

that both before and after the move Ego was involved in generalized dyadic exchange with most householders living within a 125 m radius, the economic effects of moving house on Ego's economic position may be considerable. If Ego is poor and he moves into the proximity of one or more far richer householders, this is likely to enhance Ego's economic position; alternatively, if Ego is rich and he moves towards a set of neighbours that, as an aggregate, is substantially poorer than the original set, the residential move will be an economic liability to Ego, which however may be compensated by political gains (his new neighbours becoming his factional following) or increased prestige (the new neighbours offering Ego better opportunities to dispose of his wealth in a socially approved manner).

These economic and political aspects of moving house may exist regardless of the kinship relationships that exist between Ego and the other householders in his set of nearest neighbours – for all will be counted, more or less, as fictive kin. However, Ego has considerably more freedom to honour or not honour the claims of a nearest neighbour if the latter is a non-kinsman. Moreover, moving house is often not the result of a specifically economic strategy but an almost institutionalized, spatial expression of the life cycle of agnatic groups: being born in the same household, brothers later in life establish their own households, often move away from each other as conflicts arise, and the same pattern is repeated among their sons and grandsons. Therefore the kinship composition of the set of Ego's nearest neighbours before and after the move is of great importance for identifying the factors behind the residential dynamics. Nor can we lump all members of the kindred together: the analyses throughout this book have demonstrated that for everyday interaction a determining factor to differentiate between kin is not so much the distinction between agnatic and non-agnatic kin, but that between close and more remote kin. People who are relatively remote kin, even if they still count as kin according to local views and according to our operationalisation of these views, have far more merely optional claims on the resources of Ego than Ego's closest kin have, whose claims Ego can only ignore at great social costs. Therefore an assessment of the force of kinship among the set of Ego's near neighbours should also make allowance for the relative weight that various degrees of closeness of kin have in this connection.

If we take the residential move as our unit of analysis, quantitative analysis of the sets of nearest neighbours before and after in terms of kinship and wealth requires us to convincingly convert the data for individual householders in the set, with regard to a particular Ego, into aggregate numerical values. For the economic variable this means that we have to devise a way to convert the ordinal scale of relative wealth (as measured by our modified Kaufmann test) into the interval scale which we would have used if the various components of the peasants' wealth had been measured individually and then added. Assuming that the ordinal scale is essentially a crude representation of an interval scale, we may approximate that interval scale by:

$$W' = e^{(1-W)/(C+1)} \dots\dots\dots (11.1)$$

where

- W' = wealth on interval scale
- e = base of natural logarithms
- W = rank of wealth as measured on ordinal scale
- C = a constant (C > -1)

What would be a reasonable estimate for C? Fig. RR2 gives a number of possible choices.

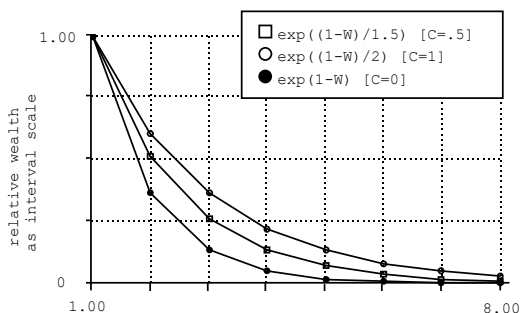


Fig. 11.4 . Transforming ordinal wealth measurement

After some trial and error .5 is found to be an acceptable value for C, leading to an assumption that the richest in the sample are just over 100 times richer than the poorest, and that the poorest are twice as poor as those in the penultimate wealth bracket (Table RR1); such figures roughly tally with the actual wealth distribution as observed in Sidi Mḥammad in 1968.

number of moving householders	W (wealth rank)	W' (transformed scale)
8	8 (poorest)	.009
3	7	.018
1	5	.069
1	3	.264
2	1 (richest)	1.000

Table 11.11. Transforming the relative wealth distribution.

In a similar way we may arrive at a numerical transformation of the relative weight of a kinship relationship between Ego and any of his nearest neighbours. Elsewhere in this study I have argued that in this ideologically patrilineal society where in fact not the agnatic kin group but the collateral kindred is the basic format of kin interaction, essential features of any specific kin relationship between two individuals A and B may be characterized by two parameters:

k = length of genealogical chain between A and B, and

l = degree of lineage alienation (*i.e.* transition from one agnatic descent group to another) occurring in that chain.

For instance, if A is B's FBSS then k = 4 and l = 0; if A is B's FFMZSS then k = 6 and l = 2 (points of lineage alienation indicated by ~: FF~MZ~SS).

If we are to construct a measure of relative weight of a kinship chain, in terms of its relative relevance to the interactions and material exchanges of those bound by it, we

may again take as a general model:

$$G = e^{(1-k-l)/(C+1)} \dots\dots\dots (11.2)$$

where

- G = relative weight of kinship chain
- e = base of natural logarithms
- k = length of genealogical chain
- l = degree of lineage alienation, and
- C = a constant (C > -1)

Regardless of the value chosen for C, G attains its maximum value for k = 1 and l = 0, i.e. when A is B's B, F,S, D or Z. For any value of k and of l higher than 1, the relative weight of the kinship chain, G, decreases in accordance with our qualitative understanding of the kinship system. It would be possible to assign a different effect to l than to k, e.g. thus:

$$G = e^{(1-(k/D)-(l/E))/(C+1)} \dots\dots\dots (11.3)$$

where both D and E are constants, unequal to 0 and unequal to 1, but in practice it seems adequate to adopt the same approach as in the analysis elsewhere and treat their specific effect upon the decreasing weight of the kinship chain as roughly equal. In order to find an acceptable value has been found for C, we had best consider the least complicated case of agnatic kinship (l = 0). A number of possible low values of C have been tested in Fig. 41c:

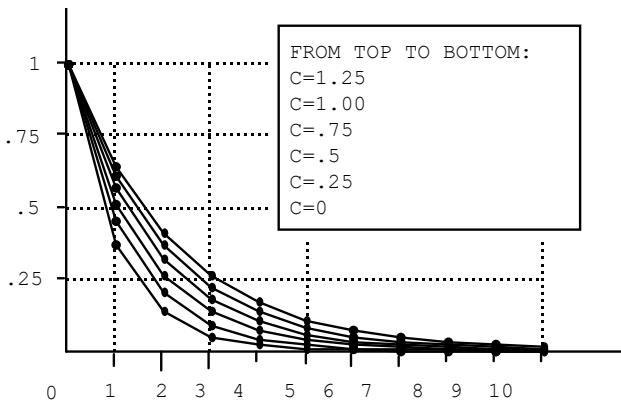


Fig. 11.5. Estimating parameter C for the measurement of relative weight of a kinship chain

The value which most seems to correspond to observed social practice in Ĥumiriyya is C = 1, as is further made clear in Table 11.11:

example of kinship chain	relative weight of kinship chain	C
--------------------------	----------------------------------	---

1	B,F,S,D,Z	1.000
2	BS	.607
3	FBS	.368
4	FFBS	.223
5	FFBSS	.135
6	FFBSSS	.082
7	FFFBSSS	.050
8	FFFBSSSS	.030
9	FFFFBSSSS	.018
10	FFFFBSSSSS	.011

Table 11.12. Effect of chain length in relative weight of kinship chain when parameter C = 1

In other words, when the model and parameters chosen, the kinship weight of a brother would be more than 1.5 that of a brother's son, almost three times that of a first cousin, and almost eight times that of a second cousin. This convincingly reflects the continued importance of close agnatic kin beyond brothers, fathers and sons, yet lets the relative weight of these chains decrease to almost imperceptible as the chain becomes longer than would still be in line with actual genealogical knowledge. Our transformation model thus takes the form:

$$G = e^{(1-k-1)/2} \dots\dots\dots (11.4)$$

Our next step is to calculate, for each set of nearest neighbours before and after every residential move, the wealth contribution which each nearest neighbour makes to the set, and the relative kinship weight of that nearest neighbour's kinship chain with regard to Ego. The reader will trust me that an adequate computer programme was written for the purpose, and correctly executed. We are then in a position to calculate by simple summation, for every move, the aggregate wealth and the aggregate kinship weight of the set of Ego's nearest neighbours both before and after the move.

By simple arithmetic operations upon the variables thus obtained, we may also create such new, secondary variables as

- 'the number of kin among nearest neighbours before the move';
- 'the product of aggregate kin weight and aggregate wealth among nearest neighbours after the move' (as a measure not only of available wealth within the new set of nearest neighbours, but also of Ego's kinship claims on that wealth and of these neighbours' claims on Ego's wealth); etc.,

and such variables may again be divided by the number of householders in the set of nearest neighbours so as to obtain a percentage. There is a considerable danger involved in such operations: they may present the available data in a form which is easier to manipulate or to interpret, but do not add anything to the total amount of quantitative information available in the data set, and therefore we should guard against artefacts of *collinearity* lest the same information should spuriously be counted twice of thrice in the same analysis. A critical assessment of the original variables and

of the arithmetic operations employed, as well as intermediate correlation analyses, assists us to weed out such effects of collinearity and to end up with a transformed data set which, although containing secondarily computed variables, yet is free from collinearity.

On this final data set we now perform a *principal components factor analysis with varimax rotation, using Eigenvalue = 1* as a criterion for retention of factors. The results are set out in Table 11.12:

ROTATED LOADINGS							
	FACTOR1	FACTOR2	FACTOR3	FACTOR4	FACTOR5	FACTOR6	
DIFKIBU	-0.966	-0.005	-0.176	0.025	-0.003	0.077	
DIFBU	-0.919	0.078	-0.162	-0.251	0.000	0.076	
DIFWEL	-0.817	0.194	-0.285	-0.291	0.220	-0.077	
KIBUVO	0.804	0.411	0.172	-0.275	0.040	0.090	
BUVO	0.773	0.497	0.146	-0.112	0.120	0.010	
DIFKIWE	-0.732	-0.092	-0.298	-0.033	0.497	0.085	
DIFKIN	-0.682	-0.493	-0.148	0.115	0.366	0.282	
PKIBUVO	0.525	0.004	0.156	-0.592	-0.225	0.195	
AFSTSMVO	0.115	-0.893	-0.051	-0.112	-0.046	-0.139	
PKINVO	0.275	0.841	0.050	-0.128	-0.294	-0.109	
KINVO	0.494	0.780	0.101	-0.141	-0.244	-0.114	
DIFPKIN	0.245	-0.668	0.006	0.374	0.466	0.205	
DISTMOVE	0.046	-0.523	0.376	0.631	-0.057	-0.322	
PWELVO	0.317	-0.072	0.905	0.110	0.070	0.010	
WELVO	0.432	0.072	0.867	0.040	0.060	0.070	
PKINWELV	0.356	0.241	0.813	0.140	-0.255	0.060	
DIFPWEL	-0.308	-0.138	0.740	-0.051	0.320	-0.406	
KINWELVO	0.381	0.335	0.727	0.117	-0.279	0.124	
DIFPKINW	-0.378	-0.339	0.503	0.197	0.520	-0.134	
DIFPKIBU	-0.050	-0.135	0.020	0.920	0.070	-0.027	
DIFAFSM	0.212	0.063	0.321	0.647	-0.001	-0.127	
STAB	0.177	0.207	-0.021	0.190	-0.836	0.280	
LEEFST	0.076	-0.085	-0.015	0.427	0.760	0.180	
RIJKST	0.131	0.062	-0.044	0.250	0.040	-0.939	
VARIANCE EXPLAINED BY ROTATED COMPONENTS							
	FACTOR1	FACTOR2	FACTOR3	FACTOR4	FACTOR5	FACTOR6	
	6.262	3.936	4.156	2.833	2.693	1.568	
PERCENT OF TOTAL VARIANCE EXPLAINED							
	FACTOR1	FACTOR2	FACTOR3	FACTOR4	FACTOR5	FACTOR6	total
	26.090%	16.399%	17.317%	11.803%	11.220%	6.533%	89.362%

Table 11.13. Factor analysis on primary and secondary variables of data set on residential moves. Together, the factors explain as much as over 89% of the total variance.

Let us, in Table 11.13, briefly identify the nature of the variables listed as acronyms in Table 11.12:

AFSTSMVO	distance to the shrine of Sidi Mḥammad al-Wilda before the move
BUVO	number of nearest neighbours before the move
DIFAFSM ⁸⁵	difference in distance to the shrine of Sidi Mḥammad al-Wilda before and after

⁸⁵ The 'DIF-' variables always consist of a subtraction of situation before from situation after: DIF-S =

	the move
DIFBU	difference in number of nearest neighbours before and after the move
DIFKIBU	difference in number of kin nearest neighbours before and after the move
DIFKIN	difference in aggregate kin weight nearest neighbours before and after the move
DIFKIWE	difference in summated product of kin weight and wealth for all nearest neighbours before and after the move
[DIFNOKI	difference in number of non-kin nearest neighbours before and after the move] ⁸⁶
DIFPKIBU	difference in percentage of kin among nearest neighbours before and after the move
DIFPKIN	difference in relative aggregate kin weight among nearest neighbours before and after the move
[DIFPKIN	difference in relative kin weight of nearest neighbours before and after the move]
DIFPKINW	difference in relative (<i>i.e.</i> divided by number of nearest neighbours involved) product of aggregate wealth and aggregate kin weight before and after move
[DIFPNOKI	difference in percentage of non-kin neighbours before and after the move]
DIFPWEL	difference in relative aggregate wealth among nearest neighbours before and after the move
[DIFPKINW	difference in relative summated product of kin weight and wealth for all nearest neighbours before and after the move]
DIFWEL	difference in aggregate wealth among nearest neighbours before and after the move
DISTMOVE	distance over which Ego moved
KIBUVO	number of kin nearest neighbours before the move
KINVO	aggregate kin weight of nearest neighbours before the move
KINWELVO	product of aggregate wealth and aggregate kin weight of nearest neighbours before the move
LEEFT	Ego's age
PKIBUVO	percentage of kin among nearest neighbours before the move
PKINVO	relative kin weight of nearest neighbours before the move
PKINWELV	relative product of kin weight and wealth among nearest neighbours before the move
PWELVO	relative wealth of nearest neighbours before the move
RIJKST	Ego's wealth
STAB	stability of the move
WELVO	aggregate wealth of nearest neighbours before the move

Table 11.14. Original and secondary variables (listed alphabetically by acronym) as used in factor analysis of residential dynamics

Our factor analysis turns out to be capable of explaining over 89% of the variance in the data set, by reference to mathematically, blindly constructed new factors. For such cases 89% is an amazingly high figure, which inspires considerable confidence in the validity of the results. The interpretation of the new factors found is a creative and subjective process. What socio-cultural interpretation can now be given to the six factors mathematically identified by our principal component analysis?

$S_{\text{after}} - S_{\text{before}}$.

⁸⁶ Not in the factor analysis because of collinearity, but may be included in further analyses once the factors have been constructed.

factor 1 (26.090%) is high if:

- the number of kin neighbours decreases (or the number of non-kin neighbours increases) because of the move (loading on DIFKIBU -0.966)
- the number of neighbours decreases because of the move (loading on DIFBU -0.919)
- the aggregate wealth among neighbours decreases because of the move (loading on DIFWEL -0.817)⁸⁷
- the number of kin neighbours is high or the number of non-kin neighbours is low before the move (loading on KIBUVO 0.804)
- the number of neighbours is high before the move (loading on BUVO 0.773)
- the product of aggregate wealth and kin weight decreases because of the move (loading on DIFKIWE -0.732)⁸⁸
- the aggregate kin weight decreases because of the move (loading on DIFKIN -0.682)
- the percentage of kin neighbours was high (or the percentage of non-kin neighbours was low) before the move (loading on PKIBUVO 0.523)

We might characterize this factor as *the tension between residential ideal (the coincidence of neighbours and kinsmen) and atomization: ATOMIZ(1); this factor explains over 26% of the total variance in the data set.*⁸⁹

factor 2 (16.399%) is high if:

- before the move Ego lived near the central shrine (loading on AFSTSMVO -0.893)
- before the move the relative kin weight was high (loading on PKINVO 0.841)
- before the move the kin weight was high (loading on KINVO 0.780)
- the relative kin weight decreases because of the move (loading on DIFPKIN -0.668)
- the move took place over a short distance (loading on DISTMOVE -0.523)

This pattern is suggestive of a factor which consists in Ego's shying away from the oppressive ties implied in the ideal that kinsmen and nearest neighbours coincide. But what then constitutes the difference with factor1? Perhaps that in factor 2 the intensity of the kinship relationship plays an important part.

However, we are allowed to reverse the signs of the factor loadings:

Factor 2 is high if

- before the move Ego lived far from the central shrine (loading on AFSTSMVO -0.893)
- before the move the relative kin weight was low (loading on PKINVO 0.841)
- before the move the kin weight was low (loading on KINVO 0.780)
- the relative kin weight increases because of the move (loading on DIFPKIN -0.668)

⁸⁷ Probably mainly because of the decline in kin weight.

⁸⁸ Probably mainly because of the decline in kin weight.

⁸⁹ Factor(1) is a combination of process variables and initial variables, describing both the situation before the move, and the situation which has arisen as a result of the move. [**en wat wil dat zeggen?**]

- the move took place over a large distance (loading on DISTMOVE -0.523)

Now it is clear that factor 2 can adequately be interpreted as *redressing the residential situation in a direction of restoring the ideal of coincidence between nearest neighbours and kin* (= REDRESKI (2)); this factor explains over 16% of the total variance in the data set.

factor 3 (17.317%) is high if:

- Relative aggregate wealth is high before the move (loading on PWELVO 0.909)
- Aggregate wealth is high before the move (loading on WELVO 0.867)
- Relative product of aggregate wealth and kin weight is high before the move (loading on PKINWELV 0.813)
- relative aggregate wealth increases because of the move (loading on DIFPWEL 0.740)
- Product of aggregate wealth and kin weight before the move is high (loading on KINWELVO 0.727)
- relative product of wealth and kin weight increases because of the move (loading on DIFPKINW 0.503)

The interpretation may be facilitated when we reverse the signs:

- Relative aggregate wealth is low before the move (loading on PWELVO 0.909)
- Aggregate wealth is low before the move (loading on WELVO 0.867)
- Relative product of aggregate wealth and kin weight is low before the move (loading on PKINWELV 0.813)
- relative aggregate wealth decreases because of the move (loading on DIFPWEL 0.740)
- Product of aggregate wealth and kin weight before the move is low (loading on KINWELVO 0.727)
- relative product of wealth and kin weight decreases because of the move (loading on DIFPKINW 0.503)

So: the aggregate wealth of the nearest neighbours, and Ego's access to this wealth through kinship ties, was low and will still be lower; or: the aggregate wealth of the nearest neighbours, and Ego's access to this wealth through kinship ties, was low and will still be lower

The factor seems to measure *the extent to which Ego's residential situation and the changes therein are determined by the wealth of Ego's neighbours: NEIGRICH (3); this factor explains over 17% of the total variance in the data set.*

factor 4 (11.803%) is high if:

- The percentage of kin neighbours increases (or the percentage of non-kin neighbours decreases) because of the move (loading on DIFPKIBU 0.920)
- the distance to the central shrine increases because of the move (loading on DIFAFSM 0.647)
- the percentage of kin neighbours is low (or the percentage of non-kin neighbours is high before) before the move (loading on PKIBUVO -0.592); note that PKIBUVO revolves on the dichotomy 'kin or non-kin', but the intensity of the kin relationship (the kin weight) is not measured in that variable.
- the distance of which Ego moved is high (loading on DISTMOVE 0.631)

We are allowed to reverse the signs; therefore, this factor is high if:

- The percentage of kin neighbours decreases (or the percentage of non-kin neighbours increases) because of the move (loading on DIFPKIBU 0.920)
- the distance to the central shrine decreases because of the move (loading on DIFAFSM 0.647)
- the percentage of kin neighbours is high (or the percentage of non-kin neighbours is low) before the move (loading on PKIBUVO -0.592)
- the distance of which Ego moved is low (loading on DISTMOVE 0.631)

This factor seems to measure *the replacement of kinsmen for non-kin and for the central saint, while relationships with non-kin neighbours are left intact: emphasis on contract and social distance: CONTDIS (4)*; this factor explains nearly 12% of the total variance in the data set.

factor 5 (11.220%) is high if

- The move is stable (loading on STAB -0.836)
- Ego's age is high (loading on LEEFT 0.762)
- The relative product of aggregate affluence and kin weight increases because of the move (loading on DIFPKINW 0.529)

We are allowed to reverse the signs.

This is the factor GENTENS(5), *generational tension in the residential dynamics; this factor explains over 11% of the total variance in the data set.*

factor 6 (6.533%) is high if

Ego is poor (loading on RIJKST -0.939)

This is the factor EGOPOOR (6); *this factor simply measures wealth; it explains over 6% of the total variance in the data set.*

11.11.4. Discussion

The identification of these factors shows us the main dimensions along which observed residential mobility in Sīdī Mḥammad and Mayzīyya may be explained.

Some additional insight in the processes at work can be gathered when we use the four factors as newly calculated variables, *which(as is standard in factor analysis) are each mathematically constructed in such a way as to be uncorrelated to one another* (however similar their descriptions in discursive terms may seem), and which together show that the variance in the data set on residential mobility can be understood along six fairly identifiable dimensions.

A cluster analysis (Table 11.14) using Euclidean distance and single linkage and a K-means splitting method, on these factor variables lumps most of the moving householders together, with the exception of two of the three richest householders in the sample, who each occupy a cluster of their own. This is in line with participant observation, where one rich mover is politically ambitious and attracts neighbours as clients, whereas the other has entirely withdrawn from social life and has moved away to the very outskirts of the village.

Regression analysis allows us to take the major variables in the data set as dependent variables and using the identified factors as independent variables predicting the

dependent variables. Does this throw light on the variable DIFAFSM, which is interesting from a point of view of the study of Ḥumīrī religion?

SUMMARY STATISTICS FOR 3 CLUSTERS								
VARIABLE	BETWEEN	SS	DF	WITHIN	SS	DF	F-RATIO	PROB
REDRESKI(2)	3.54	2	10.46	12	2.03	0.17		
NEIGRICH(3)	10.90	2	3.10	12	21.11	0.00		
EGOPOOR(6)	8.10	2	5.90	12	8.23	0.01		

CLUSTER NUMBER: 1								
MEMBERS STATISTICS								
CASE	DISTANCE		VARIABLE	MINIMUM	MEAN	MAXIMUM	ST.DEV.	
1	0.30		REDRESKI(2)	-1.66	-0.06	1.75	0.90	
2	0.32		NEIGRICH(3)	-1.09	-0.19	0.84	0.49	
4	0.54		EGOPOOR(6)	-1.84	0.24	0.88	0.67	
5	0.80							
6	1.22							
7	0.22							
8	0.69							
9	0.65							
10	0.16							
11	0.23							
12	1.35							
14	0.27							
15	0.98							

CLUSTER NUMBER: 2								
MEMBERS STATISTICS								
CASE	DISTANCE		VARIABLE	MINIMUM	MEAN	MAXIMUM	ST.DEV.	
13	0.00		REDRESKI(2)	-0.87	-0.87	-0.87	0.00	
			NEIGRICH(3)	3.16	3.16	3.16	0.00	
			EGOPOOR(6)	-0.51	-0.51	-0.51	0.00	

CLUSTER NUMBER: 3								
MEMBERS STATISTICS								
CASE	DISTANCE		VARIABLE	MINIMUM	MEAN	MAXIMUM	ST.DEV.	
3	0.00		REDRESKI(2)	1.66	1.66	1.66	0.00	
			NEIGRICH(3)	-0.65	-0.65	-0.65	0.00	
			EGOPOOR(6)	-2.66	-2.66	-2.66	0.00	

Table 11.15. Cluster analysis of householders involved in removals, villages Sidi Mḥammad and Mayzīyya, 1966-1970

MODEL CONTAINS NO CONSTANT.							
DEP VAR:	DIFAFSM	N:	15	MULTIPLE R:	0.594	SQUARED MULTIPLE R:	0.352
ADJUSTED	SQUARED MULTIPLE R:	0.352	STANDARD ERROR OF ESTIMATE:	106.668			
VARIABLE	COEFFICIENT	STD ERROR	STD COEF	TOLERANCE	T	P(2 TAIL)	
CONTDIS(4)	78.686	28.508	0.594	1.000	2.760	0.015	
ANALYSIS OF VARIANCE							
SOURCE	SUM-OF-SQUARES	DF	MEAN-SQUARE	F-RATIO	P		
REGRESSION	86681.658	1	86681.658	7.618	0.015		
RESIDUAL	159293.342	14	11378.096				

Table 11.16. Regression analysis of variable DIFAFSM: difference in distance to the shrine of Sidi Mḥammad Jr before and moving house

However, regression analysis leads to useless results, since the resulting 'multiple r' is

too low; clearly the identified factors are insufficient basis to find a pattern in the alterations in distance to the village's central shrine – the best interpretation of the statistical evidence is that these alterations are mere side-effects of residential mobility which is determined by other considerations than religious ones.

11.12. Summary and conclusions of this chapter

After discussing, in the previous chapters, kinship and spatiality from a point of view of group classification, in the present chapter we assessed how kinship and spatiality play a role in the day-to-day interaction between two persons: in egocentric relationships.

Most interaction in Ḥumiriyya is not a matter of collective activities but takes place between two individuals. The model of the dyadic contract (Foster 1961) is applicable to Ḥumiriyya. It particularly takes shape through the indigenous concepts of *mezīyya* ('a gift or service which is rendered freely and with pleasure') and *mutashrin* (the partners in a very intensive and inclusive *mezīyya* relationship).

Spatiality turns out to be of great significance for the egocentric dyadic relationships between individuals; this can be demonstrated for the recruitment of partners in everyday interaction; for the frequency of interaction; for marital relations; and for religious activities.

In the analysis of the impact of kinship on egocentric dyadic relationships we should look at non-agnatic ties along with the agnatic ones. I devised a system to classify genealogical ties between individuals in: close agnatic, distant agnatic, close cognatic/affinal, distant cognatic/affinal, and non-kin. For the former four categories combined I use the term 'kindred' (Mitchell 1963). Kinship (*i.e.* belonging to Ego's kindred) turned out to be associated with dwelling in each other's proximity, – in such a way that only for the shortest distances did close agnates turn out to outnumber the other categories of kindred, whereas for other distances the kindred could be treated as a single category. For certain spatial distances between two individuals (*i.e.* for some structural distances within the spatial structure) there turned out to exist a statistically significant preference for kindred (regardless of which category of kindred) as partners in day-to-day interaction; for other spatial distances (and most significantly for that ranging from 0 to 125 meters: the nearest neighbours) such a preference could not be demonstrated. No statistically significant association could be demonstrated to exist between kinship and frequency of interaction. An analysis of the marriage pattern did not show a preference for marriages within the kindred, nor for marriage within certain categories within the kindred, *e.g.* close agnates. However, kindred endogamy is not subject to incest taboo with the exception of very close relatives, and in fact 30% or all marriages are kindred-endogamous.

Kinship turned out to be an independent factor in day-to-day interaction in Ḥumiriyya, but as such it proved only of limited relevance.

For most aspects of Ḥumiri social organisation it turns out not to be meaningful to divide the kindred up into constituent categories. This makes the kindred an interesting structural feature of Ḥumiri social organisation. A comparison with the kindred in

the Greek highlands (Campbell 1963) further deepens our insight on this point.

Specialist status offer forms of interaction which usually cannot be described in terms of *mezīyya* and *mutashrin*. Interaction between specialist and client often takes place across distances that normally do preclude non-specialist interaction. Therefore specialist interaction constitutes a relative breach of spatiality. There are indications that this effect also has an impact on non-specialist interaction. If this were the case specialist status would occupy an even more important place in Ḥumiri social organisation. Specialists belong to each other's kindred, not only with regard to one and the same specialist status but also across specialist statuses.

In an additional discussion I explained why intensive dyadic relationships of the *mutashrin* type are often found among nearest neighbours. Often *mutashrin* who are not near neighbours strive to become just that; and the breaking up of *mutashrin* relationships often goes hand in hand with residential moves across minor distances (a few hundred meters). An analysis of eighteen cases of moving in the village of Sidi Mḥammad (1966-1968) brought out that relative wealth is an important factor in dyadic relationships; its influence on relationships in Sidi Mḥammad turned out to be in line with Jongmans's (1968) analysis of another village at a distance of c. 5 km. Despite the villagers's own insistence on this point, the desire to reduce one's residential distance *vis-à-vis* the main shrine of Sidi Mḥammad did not play a demonstrable role as a factor in these intravillage moves.

Also the dispersion of parental households can be interpreted in terms of the interplay between spatiality and dyadic relationships.

In addition to kinship, spatiality and specialist status there are a few other recruitment principles for social relationships in Ḥumiriyya. Living in each other's proximity is often the final manifestation (through moving) of such dyadic relationships as already existed before those involved became near neighbours. Likewise wealth turned out to be an important factor in dyadic relations in Ḥumiriyya. Against this background we must be careful lest we endow spatiality with too great an explanatory value in Ḥumiri social organisation.

Chapter 12. Conclusion: Patterns of social organisation in the highlands of North-western Tunisia: Social inequality, social change, and the study of group classification in general

12.1. Introduction

We have now reached the end of our analysis of spatiality and kinship in Ĥumirī social organisation. In conclusion I shall discuss a few topics which may round up the argument:

- The relation between spatiality and social inequality
- Relatively recent changes in Ĥumirī spatial segmentation
- The study of group classification in general: spatiality and kinship as classification principles in social organisation.

12.2. The relation between spatiality and social inequality

In the case of group classification (as based on spatiality or kinship), the different attributes a', a'', a''' etc. which may be assigned to a particular individual A, are mutually opposite and exclusive. In other words, such attributes are measured on a nominal scale. For instance, if within the structure of spatial segmentation a particular valley

contains three villages a', a'' and a''', then the classification of individual A as a member of village a'', absolutely excludes the applicability of the values a' and a'''; and likewise the existence, within village a'', of two neighbourhoods b' and b'' means that A belong to either b' or b'', without the possibility of intermediate shadings of membership. The attribution occurs by means of series of yes/no decisions within a fixed categorical system which can be represented as a dendrogram. Incidentally such a form of classification does not just occur with regard to social groupings, but also gender, marital status, and with regard to egocentric relationships in so far as these are defined in terms of kinship or spatiality (e.g. 'A is B's MZ', 'C is D's nearest neighbour'). Whenever we have to attribute to a person A the attribute 'belongs to village a'', the categorical system which we have to use is a spatial one, Fig. 2 (p. 36) represent the prototype of the dendrogram as applicable, and when we fill out in this diagram the specific names of the various segments, we can assign the attribute or conclude that A belongs to a different village, as the case may be.

Group classification as based on spatiality or kinship is a uniform system with a logical structure. The pattern lies ready in the same form for all members of a given society. We may call such a classification system 'categorical'; categorical classifications are then, among others, those in terms of groups, marital status, types of kinship relationships etc.

The possibility of a quasi-mathematical formulation (e.g. in the form of dendrograms) is attractive for many social researchers. It is not by accident that in the last few decades the anthropological research of group classification has received important impulses from symbolic logic and from structuralists fascinated by indigenous taxonomies, such as Lévi-Strauss.

Besides this categorical classification there is evaluation, which is based on a very different principle.

In the case of evaluation, the attributes which are available for a particular individual A are not mutually opposite, but differ only gradually. They can be measured on an ordinal scale, sometimes even an interval scale. The range of possible values which are available cannot be represented by a dendrogram, but by a continuum.

Just as in the case of categorical classification, evaluation is an aspect both of indigenous systems (and in that case the range of values is a creation of the actors), and in analytical models which ethnographers design in order to describe social phenomena.

Wealth constitutes a form of evaluation which in many societies is of eminent importance. Individuals display a specific degree of wealth as an attribute. Another form of evaluation is in terms of prestige or social esteem. This is a very complex variable, which may be connected to wealth (as is the case in Humeriyya: Jongmans 1968), but which besides refers to a variety of other aspects of the indigenous value system of the society. Finally, power forms an important aspect of evaluation.

In all societies, wealth, social esteem and power create forms of social inequality between individuals. In many societies this aspect is worked out in such a way that ethnographers (and often the respective actors themselves as well) consider those societies as composed of entire groupings which differ from one another with respect

to wealth, social esteem and power. Such groupings (or 'strata') can then be termed estates, classes or castes. Those societies we call stratified where such groupings constitute a central aspect in the social structure.

So not only categorical classification (*e.g.* in terms of kinship and spatiality), but also evaluation can lead to group classification by either actors or the ethnographer. This type of group classification we may call evaluative group classification.

It is attractive to regard categorical and evaluative group classification as functional alternatives in the sense defined by Merton (1949: 35):

'It is assumed that there are certain functions, which are indispensable in the sense that, unless they are performed, the society (or group or individual) will not persist. (...) Alternative social structures (and cultural forms) have served, under conditions to be examined, the functions necessary for the persistence of groups. (...) Just as the same item may have multiple functions, so may the same function be diversely fulfilled by alternative items. (...) There is, then, the concept of functional alternatives.' (Merton 1949: 35).

This reference to an important concept from the sociological school of functionalism does not imply, of course, that I look at functionalism as the best or only approach to social phenomena; for a critique of functionalism see *e.g.* Hofstra 1946; Rex 1968: 60 *f.*. Besides, the emphasis, in the present argument, on the consensus problem and on egocentric systems of classification and interaction falls outside the scope of functionalism.

This would mean that in societies where one form of group classification is highly developed, the alternative form would be far less important. For instance: modern industrial societies, as documented by an abundance of social research, display primarily a proliferation of evaluative group classification, and categorical classification is of minor importance there. On the other hand we may expect that evaluative group classification (in terms of estates, classes, castes) is only rudimentary in those societies where categorical group classification (for instance on the basis of spatiality or kinship) is developed to such a degree that we may speak of segmentation.

Such a relationship suggestive of functional alternatives has been recognised by various authors.

In fact the idea of such a complementary relation is as old as the anthropological study of kinship: the model of a society which is both segmented in kinship terms, as egalitarian, is already found (albeit in different terms) with Morgan, the pioneer of kinship studies. Morgan makes use of the concept of gens, which roughly corresponds with clan or lineage. Thus he writes of the North American Iroquois:

'Alle Mitglieder einer Irokesischen Gens waren persönlich frei und verpflichtet, Einer des Andern Freiheit zu schützen; sie waren einander gleich in Befugnisse und persönlichen Rechten. (...), unter sie waren eine durch Blutbande verknüpfte Brüderschaft. Freiheit, Gleichheit und Brüderlichkeit, obwohl nie formuliert, waren die Grundprinzipien der Gens. Diese Tatsachen sind wesentlich, weil die Gens die Einheit eines ganzes gesellschaftlichen Systems war, die Grundlage, auf welcher die Indianengesellschaft organisiert war.' (Morgan 1891: 73).

Morgan perceives the same connections in other societies which, like that of the Iroquois, display an organisation on the basis of gentes.

Many years later we encounter the same view in the introduction by Fortes and Evans-

Pritchard (pioneer in the study of kinship-based segmentation) to their collection *African political systems*. There they identify, among other types, a type of societies

'which lack centralised authority, administrative machinery, and constituted judicial institutions – in short which lack government – and in which there are no sharp divisions of rank, status or wealth. This group comprises the Logoli, the Tallensi, and the Nuer. (...) In societies of [this] Group (...) [it is] the segmentary lineage system, which primarily regulates political relations between spatial segments'.

These are

'economically homogeneous, equalitarian, and segmentary societies' (Fortes & Evans-Pritchard 1969: 5-9).

Lewis, in his comparative functional analysis of unilineal descent, pints in the same direction when he assesses, for the societies in his sample, whether in addition to unilineal descent, the significance of such other structural principles as spatiality, age group organisation, contractual co-operation and a central government.

Gellner (1969: 54) is very clear on this point:

'...it seems to me desirable to regain egalitarianism in the definition of segmentary society (...) for the following reason: in as far as inegalitarian and/or unsymmetrical relationships exist and are sustained in a society, it can hardly be the segmentary principle alone which is responsible for sustaining them, for keeping them in being.'

To such an extent the model of a segmentary lineage organisation is coupled, in anthropology, to a minimum elaboration of systems of social inequality, that Leach (1964: 159, 288) had to present the Kachins of Burma as an exception: they have both a segmentary lineage system, as a stratification in terms of social classes.

Of course only a systematic comparative functional analysis comprising a large number of societies (*cf.* Köbben 1964: 21 *f.*) could settle the question whether categorical and evaluative group classification are really functional alternatives. In the process we could refine the initial hypothesis. For functional alternatives do not come into existence by an act of definition, but have to be assessed empirically by the proper methods. But for the present argument let me limit myself to a discussion of Ḥumiri social structure against the background of this hypothesis..

The remainder of this section will deal with this topic. But let me first point out that the relation between categorical and evaluative classification may take another form than merely horizontal, as functional alternatives. For it turns out that in Ḥumiriyya social evaluation is in certain respects dependent on spatiality.

On every level spatial segmentation creates segments, which internally have collective activities and a mutual identification among the members, and which externally are clearly distinguished from other such segments at the same level. In the first instance the distinction between segments is categorical. But successively this distinction is charged with social esteem, in such a way that one's own segment is considered to be superior to the other neighbouring ('fraternal') segments at the same level, even though at a higher level these fraternal segments are again integrated with one's own segment. This effect is not very conspicuous at the lower segmentary levels: between households within the same compound, between compounds within the same neighbourhood. But between neighbourhoods pertaining to one village, between village pertaining to one valley, between valleys and between chiefdoms there is unmistakably

a strong negative prejudice. The effect disappears again with regard to very distant segments between which hardly any other interaction exists: then the attitude is back to neutral.

Further research would have to establish to what extent this prejudice plays a role in day-to-day interaction between members of different segments at these levels, on such occasions as visits to the men's assembly, saintly festivals, the unemployment relief work, at the market, at life crisis ceremonies marking birth, circumcision, marriage and death. My limited impressions suggest that a measure of avoidance and even animosity between members of higher-level fraternal segments is a constant aspect all these activities. It is possible that this prejudice constitutes an additional factor in the limitation of contacts between such segments – in addition to the more objective spatial factor: the principle that does one not seek at a large distance whatever one can get near one's home.

This coupling between social esteem and the spatial structure also has equivalents in the realm of religious representations. The invisible personal beings which in Ḥumiriyya are considered to play a decisive role in the lives of mortals, to a large extent are associated with visible objects in the landscape: shrines, the dwelling house, the threshing-floor, springs, paths, and uninhabited and swampy places. These beings may have a positive or negative effect on a person's life, depending on their nature and on the relationship which exists between them and that person. Now in Ḥumiri religious representations the positive workings of these beings turn out to be concentrated near one's place of residence: in one's own house, and in the major shrines in the immediate surroundings; whereas the negative effects have to be feared particularly in the periphery of one's own village (springs, patches of waste land), and at greater distances.

Let us now consider Ḥumiriyya from the point of view of the opposition between categorical and evaluative classification.

As became clear in the course of my argument, categorical classification (especially that based on spatiality) plays a major role in Ḥumiri social organisation, and in terms of our hypothesis we would expect a very limited elaboration of evaluative classification.

When we limit ourselves to the indigenous system of representations and values, this expectation is confirmed. Estates, classes and castes are absent in rural Ḥumiriyya: there is no indigenous evaluative group classification. On the contrary, the fundamental equality of all members of society (in so far as of the same gender and of roughly the same age) is a very central value in this society. It is noteworthy that in Ḥumiri rural society hierarchies of formal positions (for individuals) have all been introduced recently from outside. The only cases are: the chief and his assistants (introduced by the French c. 1893); the chief and foreman in the unemployment relief organisation (introduced after Tunisia attained independence, 1956); and the positions of regional superior (*muqaddem*), his substitute (*shawush*), and adept (*faqir*) in the religious orders, foremost the Qadiriyya, which were only established in Ḥumiriyya in c. 1880 (cf. Demeerseman 1964; Miedema 1967).

This value is expressed in many different ways. In everyday social intercourse there is a strong emphasis on symbols of equality such as shaking hands and greeting. One

cannot approach a number of individuals engaged in a collective activity of whatever productive, social or ceremonial nature, without ostensibly greeting each and everyone, and shaking hands with (or having one's hand kissed by, in the case of adult women) each and everyone. Whoever deviates from this norm is reproached for putting himself or himself above the others, or is considered to be mentally deranged. The men's daily get-together at the men's assembly, where one spends time in each other's company, exchanges information, occasionally discusses conflicts, plays cards and drinks tea (the symbolic unifying function of sharing meals also extends to sharing tea), stresses the fundamental equality of all male heads of household in the village. In principle every member of Ḥumiri society is for every other member a potential partner in a *meziyya* relationship and even a *mutashrin* relationship. This is also the reason why the actual pattern of such relationships can change so much within a short time: the actual partners are in principle interchangeable. Fundamentally the partners in such relationships are equals, for the model that underlies the relationship is that between brothers and/or nearest neighbours. In many societies the marriage system offers significant clues as to the system of social inequality (*cf.* van der Veen 1969; Tumin 1967: 53). In Ḥumiriyya the relationship between marriage partners is fundamentally looked upon as one between equals. In most cases the marriage partners had already been *mutashrin* for some time between the marriage was contemplated. My analysis of the marriage pattern shows two basic movements: on the one hand and on a limited scale the replication and strengthening of such marital relationships as already exist; and on the other hand, at a larger scale, the initiation of marital relationships with new marriage partners. The latter again suggests that every head of household is a potential marriage partner for any other head of household.

The value of fundamental equality is closely connected with another Ḥumiri value to which Bos (1969: 63 *f.*) has paid attention: that of independence. Bos argues that this value is intimately connected with the Ḥumiri attitude vis-à-vis the terrain adjacent to the dwelling house (which has to be respected and avoided by non-members of the family), with the relatively scattered pattern of residence which we find in Ḥumiri villages – displaying a tendency to fill the available residential space in such a way as to maximise distances between compounds⁹⁰, and with spatial mobility: conflicts have their spatial expression since moving away is often a method to resolve or temporise conflicts. In my description of Ḥumiri religion I shall show how this value of independence does have an indigenous formulation in the concept of honour, which is not only applicable to the interaction between humans, but also to that between humans and invisible non-human beings.

The value of independence is also expressed in Ḥumiri's statements concerning the norms and motivations governing their society. An often-heard Ḥumiri truism is:

'A person may do this or refrain from doing this; it is up to him.'

⁹⁰ According to Bos (1969: 65) the valley of Shahāda does not even constitute an 'institutionalised local community': 'there is a local community but only in a diffuse, non-organised form.' Bos stresses, rightly, the great significance of dyadic, egocentric relationships. But in doing so he underestimates the significance of social grouping on the basis of spatiality; or it should be that the local society in Shahāda differs strongly from that in the valley of Sidi Mḥammad (at a distance of only one kilometer) with regard to the process of the decline of spatial segmentation.

For a beginning ethnographer trained to look for oppressive norms and social control such statements are confusing.

Obviously there is a close connexion between fundamental equality of all potential interaction partners on the one hand, and on the other spatiality as a central principle in the recruitment of day-to-day interaction partners: if there are hardly any ideological reasons to select one potential interaction partner rather than another, one simply selects the partner who is nearest.

However, interaction is not only structured by ideological factors, and the minimal development of ideological considerations in the selection of interaction partners therefore cannot offer us a full explanation of day-to-day interaction patterns. However equal potential interaction partners may be from an ideological point of view, in fact they are very far from equal. For even in the absence of an explicit indigenous system of evaluative group classification, the members of Ḥumirī society do differ considerably in terms of wealth, social esteem and power.

12.3. The problem of consensus in classification

Throughout this argument we have been occupied with the problem of consensus: categorisation as imposed by the actors can only be of relevance for day-to-day interaction, if this categorisation is consensual – for only then can it offer the actors a shared framework in terms of which to structure their interaction. In this argument the consensus problem has been studied principally from the point of view of spatiality and kinship, both forming principles of categorical social grouping. But of course the consensus problem also exists with regard to evaluative classification. Also the indigenous principles of evaluative classification (in Ḥumirīyya *e.g.* wealth and honour; *cf.* Jongmans 1968) can only be claimed to be eminently relevant for the social structuring of interaction, after we have assessed them on the following points:

- does such a principle have more or less the same meaning for all actors?
- do the majority of actors evaluate one and the same individual in their social environment in a similar fashion in the light of that principle?

Concretely, if the concept of honour (Ḥumir. Arabic: *'horma'*) has a widely divergent meaning for a sizeable proportion of the actors in the local field, and if these actors would greatly differ in the amount or level of honour they would attribute to any individual in their midst, than the concept of honour would perhaps continue to illuminate our understanding of the ideology of social relations, but it would never in itself allow us to understand and even predict the actual pattern of social interaction in concrete social settings.

It is only after we have appreciated this problematic and found a way out of it, that we may proceed to conclusions concerning the actor's perceptions and motivations.

This problematic can be circumvented by radically ignoring the indigenous system of evaluative classifications. We might decide to confine the analysis to the application of our own analytically and operationally defined evaluation principles, regardless of whether the actors structure their social reality in similar terms. In such an approach

the analysis would shed the seemingly unnecessary burden of indigenous terms (which tend to be very diffuse and protean), would gain in clarity (for the evaluation principles used in the analysis are defined with scientific precision), and would lend itself much better for inter-societal comparison. Often the structure of day-to-day interaction can be convincingly described with such an approach, e.g. in terms of power, or of a stratification of professional prestige. But what misses out entirely in such a description is that which is specific and proper to the society under study: we may perceive what is happening, but we can no longer understand what consciously moves the actors, what their culturally patterned motives are – for the tools which they themselves use for their motivations and interaction (their indigenous concepts) have been eliminated from our research. Therefore, depending on the aims of our research and on the persuasion of the ethnographer, one of either approaches, or a combination, can be selected; both approaches have their defects and their strengths, and provided they are applied with sophistication and methodological rigour, none is in principle superior to the other.

Here I want to pass by social esteem as a variable: it is very complex, and in many cases it is dependent on day-to-day interaction much more than that it initiates day-to-day interaction in its own right. Social esteem in Ĥumiriyya, primarily concentrated on the indigenous concept of honour, and its relation with day-to-day interaction was researched by Jongmans (1968).

In present-day Ĥumiri society individuals, and also heads of households, display big differences both in power and in wealth. The existence of an ideology of fundamental equality may therefore be considered to be a cultural lag: the social structure has changed but the old norms and values persist (Ogburn & Nimkoff 1947: 592 f.). This problematic is particularly manifest in the attitude which the average members of Ĥumiri society adopt vis-à-vis those who in terms of wealth and power are at the top. The former insist on continuing to treat the latter as much as possible as equals, they approach them with a remarkable frankness of speech, joke with them and about them in their presence, and in their absence wallow in the murkier side of the biography of these happy few, and mete out extensively the extent to which they fail to live up to the norms of equality and of charity according to wealth, which are deemed to govern human intercourse. All this despite the happy few's access to wealth which otherwise is so very scarce under modern conditions, and their great power over their fellow-villagers.

In earlier decades the overall level of wealth was generally higher, and the differences in wealth between households were smaller; nearly everyone could subsist nicely (Jongmans 1968). The few formal positions of power which exist in Ĥumiriyya today (1968), are colonial or post-colonial creations (cf. note 33). Leadership was informal and achieved – a question of being *primus inter pares*. Such leaders – designated *kabir* (male), *kabira* (female) – still occur; they co-ordinate collective activities within the spatial segments, conduct marriage negotiations and solve minor conflicts. But their significance is very limited as compared to the power of the chief and of the unemployment relief organisation. The chief has moreover usurped the co-ordination of collective interaction of the largest spatial segments: the organisation of saintly festivals.

In Ḥumiriyya and worldwide relationships primarily revolve on the exchange of goods and of services. Despite the indigenous ideology of equality, in the face of the existing great differences in power and wealth we can no longer maintain that all potential interaction partners are equal with regard to such aspects as are relevant for interaction. If such is the case, what then can explain the continuing significance of spatiality?

The main reason is that only a small number of members of members of Ḥumiri society can be said to differ greatly from the majority in terms of wealth and power. Within a village only a few inhabitants are conspicuously rich and/or powerful. Among the great majority there are no very marked differences in wealth and power; these people have as much or as little to offer to each other – and then spatiality can remain the most important recruitment principle for interaction.

Sometimes Ḥumiriṣ still use the concepts for egalitarian dyadic relationships (*mezīyya*, *mutashrin*) for interaction between people with much and those with little power and wealth. Yet relationships of this type have a totally different content: they are patron-client relationships. Such relationships are not egalitarian, but they yet fit in Foster's model of the dyadic contract (Foster 1961: 1174 *f.* and 1963). Because such relationships are advantageous to the client, and because people with little power and wealth find the supply of such relationships very scarce, the client must be prepared to cross greater distances than is customary in the case of normal egalitarian *mezīyya* relationships. Some cases in my data may illustrate this point.

Patron-client relations rarely exist between persons who live in different villages at considerable distance from each other. Therefore I must confine myself here to a few examples, whose representativeness I cannot assess:

12.3.1. Intervillage clienship: three examples

#CASE 12.1. THE REMOTE VILLAGER AS A CLIENT. Muḥammad is a married man in his early thirties, living in the small village of Raml al-^cAtrus. He lives smack on the tar road to Tabarqa, and owns a bicycle. This means that the Tabarqa market is within easy reach for him. It is there that he occasionally buys vegetables and other groceries for the chief. The chief lives in Sidi Mḥammad, at a distance of c. one hour on foot from Muḥammad's house. From Raml al-^cAtrus Muḥammad has to take his groceries to the chief's house on foot, for the steep and rocky path is unsuitable for cycling. Muḥammad also performs other services for the chief which relate to his advantageous position on the main road; thus he assisted the chief in the delivery of building materials, which for some time were stored next to Muḥammad's house.

#CASE 12.2. THE REMOTE YOUNGSTER AS A CLIENT. Rabah is an eighteen-years old unmarried boy, living in his father's house at Fidh al-Missay. Occasionally he works on the chief's land for a few hours; this land is situated at half an hour on foot from Rabah's home.

#CASE 12.3. THE REMOTE VILLAGE FOOL AS A CLIENT. Abasha is a thirty-five years old bachelor living in Tra'aya-sut. No one takes him seriously, he is called by his insulting nickname Kamaun, and is the subject of public humiliation and sexual allusions. Often he runs errands for the owners of a shop in Sidi Mḥammad, at half an hour walking distance from his home. Sometimes he even sleeps in the shop as a night-watchman.

Muḥammad and his wife never come to the village of Sidi Mḥammad except for pilgrimage and

to deliver the chief's groceries. Rabah visits his kindred in Sidi Mḥammad (his elderly FZ and his adult FZs) a few times per week, and sometimes attends the men's assembly in that village. In addition to his client relationship with the shopkeeper in Sidi Mḥammad Abasha has a few other social contacts there (assistance in agriculture, social visits) and he is very frequently found at the men's assembly there – in other words, at the local store.

The relationships between Muḥammad, Rabah and Abasha vis-à-vis the chief and the shopkeepers are patron-client relations for the following reasons: the dyadic partners in these relationships show great differences in wealth and power; the input of either partner in these relationships is of a different nature: the client offers services, the patron offers a meal, castaway clothing, money, protection, access to unemployment relief work. The relationship is governed by reciprocity and it is even possible that those involved consider it to be a *meziyya* relationship, but while the patron is free to choose the nature, the size and the timing of his prestation, the client has no choice but to comply with the wishes of the patron – failing which the patron is free to adopt somebody else as client. It is noteworthy that in all three cases patron and client do not belong to each other's kindred; however, patronage between kindred is no exception in Ḥumiriyya.

In this respect patron-client relationships in Ḥumiriyya mean a breach of spatiality. But often spatiality and patronage go hand in hand, for it is comfortable for the patron to have clients living near his home so that he may appeal to them for menial services at any time. In these cases the clients are the patron's nearest neighbours, or they are enabled (by the patron's making a site available) to become just that through moving. Farm-hands and herdsmen living in the very household of the richest men are the rare extreme form of such relationships combining patronage with proximity.

Within the set of the truly conspicuously rich and powerful we see another breach of spatiality. This set is only small and its members live dispersed over the many Ḥumiri villages. It is my impression that these people interact more closely and frequently with each other than is the case for those who live at similar distances from each other but lack their riches and power.

12.4. Is there avoidance between wealthy fellow-villagers?

This conclusion appears to be in contradiction with Jongmans (1968: 25), who claims that in the village of Mhamdiyya (1965-1966)

'the rich have the tendency to avoid their fellow-rich.'

Jongmans illustrates this conclusion with quantitative data, but does not provide a decisive statistical test of his statement (in other words, the quantitative illustration remains subject to chance fluctuations which have not been assessed statistically). However, Jongmans's extensive data collected over a long succession of years cannot be simply compared with my own comparatively furtive impressions. The category of the rich appears to be much larger in Jongmans's analysis than it is in mine: with Jongmans (1968: 16, figure 3) it comprises almost 50% of the entire village population, with me only %. In both analyses it were the informants who determined the numerical size of the categories of poor, middle and rich, by scoring their fellow villagers

according to a limited number of categories as offered by the interviewer. Apparently the inhabitants of Sidi Mḥammad have a different conception of the rich than those of Mḥamdīyya a few years earlier. Furthermore, in this phase of his research Jongmans ignored such factors as spatiality, and power (the political ambitions of the rich).

When we confine ourselves to the six richest heads of household in Jongmans's data (c. 20% of the total population of heads of household), the result of statistical analysis may not directly support my own views but at least is no longer in contradiction with them. The following Table 12.1 summarises the data (source: Jongmans 1968: 7, Fig. 1):

	among themselves	with less wealthy fellow-villagers	total
number of potential dyadic relationships	15	156	171
number of actual dyadic relationships	2	52	54

Table 12.1. Potential and actual dyadic relationships of the six richest heads of household in the village of Mḥamdīyya, 1965-1966.

Statistical analysis shows that the six richest heads of household in the village of Mḥamdīyya (1965-1966) in their recruitment of interaction partners showed *no significant avoidance* of each other (likelihood ratio test, one-sided, $\chi^2 = 2.17$, $df = 1$).

No dyadic relationships whatsoever were found between the three richest heads of household in Jongmans's data set (10% of the population). On face value, this is in accordance with Jongmans's hypothesis, but a statistical test, again, shows that even this result can be attributed to chance, in other words that the richest's actual recruitment of dyadic interaction partners may be wholly attributed chance without any significant avoidance of each other (I' test, two-sided, $I' = 1.75$, $df = 1$, not significant):

	among themselves	with less wealthy fellow-villagers	total
number of potential dyadic relationships	3	84	87
number of actual dyadic relationships	0	25	25

Table 12.2. Potential and actual dyadic relationships of the three richest heads of household in the village of Mḥamdīyya, 1965-1966.

Here we see an important advantage of the quantitative method in ethnography, as pursued by Jongmans: the data remain accessible for further examination. Incidentally, it is not impossible that further, refined research (with special attention for such factors as spatiality and power) might confirm Jongmans's hypothesis, after all; differences in political ambitions between the richest heads of households would then appear to be an important variable. All I wanted to demonstrate is that Jongmans's hypothesis is not yet confirmed by the data which he offers, and that his data,

therefore, are not in contradiction with my impression of a preference, among the vary rich and powerful, to associate with one another.

12.5. Wealth and the marriage pattern

It can also be demonstrated that the wealthy recruit their marriage partners across greater distances than people in lower economic positions. In this way those with conspicuous wealth and power begin to articulate themselves as a distinct class, preferring to interact among themselves. At present we can witness the development of a distinct life style for these *Ḥumiris*. Some of its ingredients are: the emphasis on formal Islam and the rejection (at least in public, and verbally) of the many religious observances and representations which belong to non-formal, popular Islam in *Ḥumiriyya*; the ability to read and write, and to recite the Qur'an; the wearing of Western clothing; the frequenting of cafés in the urban centres; the smoking of cigarettes; the possession of a certain formal education, sometimes even secondary school; a preference for modern medical services above traditional practices. All this is in line with the development signalled by Jongmans (1968: 32):

'Whereas the situation in the past could be characterised with the term of social differentiation, at present the term of social stratification is the more appropriate.'

Apparently evaluative group classification is developing in present-day *Ḥumiriyya*, and this goes at the expense of categorical group classification in terms of spatiality.

We shall now explore the extent to which the system of spatial segmentation as described in my argument is being eroded by other factors besides power and wealth.

12.6. Recent changes affecting spatial segmentation and characteristic attributes

Of course there have always been cases in which the distribution of characteristic attributes over the segments did not wholly correspond with the model as sketched in previous chapters. There may have been spatial segments which, in terms of size, internal structure and location in the landscape could be said to be villages, but which yet possessed only one spring, or more than one men's assembly. But such cases would invariably be capable of an explanation in terms of the very process of segmentation itself: the redistribution of characteristic attributes may lag behind actual changes in size and function of the segments within the spatial structure, without that structure itself being affected by such a situation; in due time the anomalies will be corrected and the situation return to standard.

In the present section however I shall describe how recent developments produce real and incisive changes in spatial segmentation, and in the attending distribution and significance of characteristic attributes. The emphasis here will lie on those characteristic attributes which have been best documented in my own and earlier research in *Ḥumiriyya*: shrines and cemeteries (*cf.* Demeerseman 1964).

Because of the rapid increase of population, the very drastic reduction of the area

available for the population in the context of reforestation, and in certain places (including the valley of Sīdī Mḥammad) also because of the concentration of land in the hands of only a few persons, for many families today so little land is available that they can hardly engage in agriculture any more.

12.6.1. The land of the village of Sīdī Mḥammad

For the inhabitants of the village of Sīdī Mḥammad c. 125 ha of land was available in 1968. This was in part useless for agricultural purposes: forests, beds of rivulets, steep slopes, very rocky pastures. About 30 ha was in the hands of three households, which had close agnatic ties with one another. Another 40 ha was in the hands of six other households, likewise closely related as agnates. For the remaining 37 households (including some with relative much land) only about 55 ha was available, including useless terrain. The reasons for this concentration of the land in the hands of a few households are the following:

- In the early 1910 dozens of hectares in the valley of Sidi Mhammad was dispossessed by the colonial government in punishment for the murder of itinerant traders, and soon issued to an Italian colonial farmer, who established a large farm on top of Kef al-Hanut.
- From c. 1916 to 1957 Sīdī Mḥammad was the place of residence of two successive chiefs of the ʿAtatfa, Bu-ʿAziz and Hassuna, father and son. The chiefs were not yet paid by the state. They were responsible for tax collection and the settlement of conflicts. They used their office to accumulate wealth, and especially after 1940 the chief bought up the land rights of many inhabitants of Sīdī Mḥammad. The cluster of six land-owning households mentioned above consists of the widows and married sons of chief Hassuna.
- From c. 1915 to c. 1955 Sīdī Mḥammad was one of the few places in Ḥumiriyya where a colonial farm was established. The farm comprised c. 50 ha of land. It was bought by three wealthy members of the ʿAtatfa, including chief Hassuna. In the meantime the land has been divided between the heirs of the three original buyers.

Moreover the animal husbandry which once prevailed in the research area has now been rendered insignificant except for the very rich.

The prevailing land scarcity had caused the stagnation of the outgrowth and decline of spatial segments, especially at the highest levels. It is hardly possible to make new clearings, and as a result spatial mobility has become merely a question of individual households. When a particular family moves this no longer leads to the emergence and subsequent growth to independence of new segments, but only to minute revisions in the composition of existing and persisting segments. This amounts to a general stagnation of the segmentary dynamics.

Among other aspects this has significance effects on the distribution of springs over the spatial segments. Due to prevailing climatic conditions water is abundant throughout the year except in summer; the availability of reliable summer springs of old constituted an ecological limiting condition for demographic development. In the

nineteenth century the drying up of summer springs and the quest for new springs formed an important impulse for spatial mobility, and for the expansion or decline of spatial segments. Because of the contemporary land scarcity one is far less than in the past in a position to respond adequately to changes in the local natural water supply. At present many people live at several hundreds of meters distance from the summer springs. As a result, in some villages the actual distribution of springs over spatial segments deviates from the model as presented in a previous chapter, which has the spring as the characteristic attribute of a neighbourhood.

The parallel drawn above between nineteenth-century Ḥumiriyya and Cyrenaica, also in so far as the armed struggle over springs is concerned, suggests another reason for the contemporary stagnation of the segmentary process in Ḥumiriyya. When French colonial rule was imposed on Tunisia in 1881, Ḥumiriyya became embedded in a national state. Pacification was beginning to be effective as from c. 1900. The use of violence in order to defend or contest spatial claims is no longer legitimate, and property rights on local land are being protected by the judicial apparatus of the Tunisian state. This means that an effective major means of acquiring land and springs, and thus of continually adjusting the distribution of springs as characteristic attributes of ever changing spatial segments, has disappeared from Ḥumiri society.

At the compound level I mentioned the threshing-floor as characteristic attribute. However, when agriculture is becoming a preoccupation of the past, the threshing-floor is ceasing to constitute a sign of collective productive labour and the attending intra-group identification.

The men's assembly (*raqūba*) has been identified as a characteristic attribute at the village level. It has been a trend of the last few decades to situate a small store adjacent to the men's assembly. An effect of this has been that the men's assembly has lost some of its initially public nature and has somehow been drawn into the orbit of the shopkeeper's private concerns. Contrary to the adjacent men's assembly, the shop is the shopkeeper's private property, many villagers owe him considerable credit, and those who are in conflict with the shopkeeper over their credit or for other reasons are not likely to frequent the men's assembly any more.

Shrines exist in various types: from an inconspicuous little pile of rocks, via intermediate forms (some of which resemble the Ḥumiri *kurbi*: a dwelling house made of tree branches), to an imposing whitewashed structure (*qubba*, *djama'a*) complete with a dome, a sepulchral tomb, niches for candles and incense, a trunk for offerings and racks for the display of scores of flags which have been votive gifts to the saint associated with the shrine. The outside appearance of a shrine, and the nature and quantity of the various objects therein, generally corresponds with the function of the shrine as a characteristic attribute of a particular segment: the higher the hierarchical position of the segment in the spatial structure (in other words the higher the segmentary level at which the shrine functions as a characteristic attribute, or the larger the set of people who direct pious activities at the shrine), the more elaborate the shrine will turn out to be. Just as segments go through a developmental cycle in the course of their existence: from emergence and attainment of independence to expansion and decline (through extinction or emigration), also shrines go through their own parallel life cycles. After a shrine has been created somewhere (most often as

the characteristic attribute of a particular household or small group of household settling on that spot), the shrine may become the characteristic attribute of a segment at an ever ascending hierarchical level (commensurate to the expansion of its own segment and the absorption or eclipsing of other rival segments). In the process also the activities directed at the shrine grow in complexity. with as the highest form of development a massive and very elaborate semiannual saintly festival for the local valley and surrounding valleys. A few decades after this point has been reached, the shrine may in its turn be eclipsed by others which are involved in a similar cycle, and this results in a situation where that shrine loses its annual festival, ending up as the focus for a cemetery, declining physically, and ultimately even a passing into oblivion.

In various respects this pattern is subject to modern changes.

Possessing a qubba is highly prestigious for the spatial segment with which such a shrine is associated. Also, a shrine of that nature is much more acceptable for the representatives of formal Islam than the traditional Ḥumiri shrines are. Ever since the appearance of the first qubbas in and around the research area (1880). they have formed the characteristic attributes par excellence of the highest segments (valleys and chiefdoms); such qubbas have invariably been the focus of the major saintly festivals in the region. However, the last few years (since 1967) several kurbi-type shrines have been converted into qubbas, or plans to do so have been made. These modern developments involve shrines whose local significance is much smaller than that of the older qubbas. The shrines involved are, for instance, one which is only the characteristic attribute of a neighbourhood (in al-^ḥAyun, a part of the village of Ḥamaysiyya); or one which is admittedly the characteristic attribute of a valley (Sidi ^ḥAmara in the valley of al-Millah), but which hardly has its own festival any more and which is mainly functions as the focus of a cemetery. When such shrines evolve into qubbas this has nothing to do any more with the overall expansion of the associated segment. The conversion is instigated by a few rich and powerful people living near the shrine in question; they supervise the work, make important financial contributions, and their social position also forces others to contribute their more limited finances and especially their labour power to the project. In these cases the upgrading of the shrine does no longer spring from segmentary dynamics, but from a combination of more individual aspirations: the desire to create status symbols in the surrounding landscape, to honour local saints in recognition for individual success, and finally to render the existing shrines more acceptable in the light of formal Islam which, as said above, has a particular appeal for the local elite. Significantly, the new qubbas are also to accommodate the local qur'anic school.

Moreover the distribution of shrines and cemeteries over the spatial segments has been stagnated by the fact that during the last few decades (since c. 1930), no new shrines and cemeteries have been created in and around the research area. This despite that fact that in that period new compounds, neighbourhoods and even villages did emerge: for instance the neighbourhood of ^ḥAuniyya and the village of Mayziyya. The village of Mayziyya today disposes of shrines and a cemetery which predate the emergence of this village and mostly lie outside the residential space of the village proper: further up or down the mountain slope (so that the major local shrine of Mayziyya is called Sidi Mḥammad, situated halfway between Mayziyya and the village of Sidi Mḥammad); another minor local shrine in Mayziyya was found back

when a present inhabitant made a new clearing – it lay hidden in the impenetrable shrubs which had grown on earlier, abandoned clearings. The lack of a real religious focus in Mayziyya is also clear from the fact that the villagers are almost as much orientated towards the shrine of Sidi Bu-Ḥaruba in the valley of Saydiyya, across the mountain range of the Djabal ʿAin Falus, as they are towards the shrine of Sidi Mḥammad at the distant bottom of their own local valley. An even more extreme case is the neighbourhood of ʿAuanīyya, which contrary to all other Ḥumiri neighbourhoods known to me does not have a single shrine of its own; its inhabitants are orientation towards shrines and a cemetery which lie at a distance of more than one kilometer. By consequence religious activities directed at shrines and saints play a lesser role in this neighbourhood than they do, for instance, in the valley of Sidi Mḥammad. That both in Mayziyya and in ʿAuanīyya no new shrines and cemeteries have been created may be partly explained by the tendency towards a more formal Islam, and partly also by the expanding influence of the central government, which through the chief also extends to the organisation of saintly festivals and of burials.

The very activities which are directed at shrines have undergone changes. In the past, agriculture and animal husbandry were far more central concerned in the local shrine cults: festive and massive pilgrimages, enlivened by musicians playing the local zukkra oboe and the taballa drum, would mark the beginning and the end of the harvest period; grain and agricultural implement would commonly be stored in or near shrines; the religious power of saints associated with shrines was invoked to heal livestock; it was common to make annual sacrifices from among one's herd of cattle or goats, etc. With the decline of agriculture and animal husbandry also shrines have declines as characteristic attributes of segments. There are other causes adding to the same process. Because of the general economic decline of the rural population, because of the loosening of the collective bonds between the members of a segment, because of government suppression, and because of the competition from non-religious, national festivals as organised in the urban centres, the saintly festivals have lost much of their former splendour and significance. These festivals were, and are, held for the shrines which are the characteristic attributes of segments at the highest levels: valley, chiefdom. By contrast with the festivals, the pilgrimages and offering which were directed at the shrines by the smaller local segments at the lower levels, have persisted in more or less the traditional form.

Above we have identified the unemployment relief organisation as an additional recruitment principle, besides spatiality, kinship etc. Now it is remarkable that this organisation often also assumes the role of spatial segments with regard to religious activities. When a relief work squad is at work in the surroundings of a major shrine, the workers bring an animal sacrifice to their saint involved. The initiative come from the squad chief, and the financial contribution of the workers (between whom the meat is divided in painstakingly equal portions) is deducted from their two-weekly pay. So here we have collective activities, concentrated on an object (the shrine) which was traditionally a characteristic attribute of a spatial segment, but organised in such a way that the recruitment of the actors is no longer governed by the principle of spatial segmentation.

Finally the structure of spatial segmentation is disrupted radically, in a few cases, because the Tunisian government has created new villages in a few places in

Ḥumiriyya, forcing the families in the vicinity to move, on a strictly individual basis and without regard for pre-existing spatial or kinship groupings, into the new houses provided there (Beeker 1967; de Jong 1968). In the dyadic interaction between the inhabitants of these new villages the distance factor will continue to be of considerable significance: one is likely to interact primarily with the (new) nearest neighbours, and the new intensive relationships with them are to yield the additional protected of one's own house and of the female members of the household which, in the ordinary village, was being realised by the greater spatial isolation of the houses and compounds. However, in the area where such a new village is established, the existing pattern of spatial segmentation is completely destroyed. Threshing-floors no longer exist, there is one central water outlet, and the planning does not take the situation of shrines and cemeteries into account. It would be an interesting question whether in such settlements new spatial segments come into being, and whether they adopt, perhaps, new characteristic attributes than found in traditional Ḥumiri villages.

Thus the structure of spatial segmentation in Ḥumiriyya is eroded not only by the emergence of social inequality as a form of group classification based on evaluation, but also by a number of other factors likewise related to the expansion of the state, the cash economy, and formal Islam. In certain parts of Ḥumiriyya this development will have progressed further than in other parts; but the model of spatial segmentation as described above is unlikely to persist in a pure form anywhere. Still it is turn out to offer the most convincing analytical perspective upon the veneration of shrines, which forms the focus of Volume II of this book.

12.7. Conclusion: kinship-based and spatial group classification compared – as a stepping stone to Volume II

Kinship-based group classification can assume many forms. A significant part of anthropology used to be devoted to the analysis of these forms (see for instance Murdock (1965) for an important overview of this field of enquiry). One of these forms is the model of the segmentary lineage structure. It was this model which initially appeared to be the most fertile one to describe Ḥumiri social organisation, but which on closer scrutiny turned out to be totally inadequate.

My research made it clear that such a model does not and could not function in Ḥumiriyya, because the actors's notions concerning kinship-based group classification are non-consensual, opportunist, and more over are not permanent but situational. Such a result, with regard to a society which in terms of its indigenous ideology corresponds nicely with the etic anthropological model of the segmentary lineage structure, does offer food for thought. To what extent have the conditions of consensus and of non-opportunist, non-situational stability been met in the cases of the many other societies to which the segmentary lineage model has been applied? As long as this question has not been researched in detail, we should approach description of social organisation in terms of the segmentary lineage model with considerable scepticism. Meanwhile the entire topic of social organisation has moved from the centre to the remote periphery of the anthropological interest.

My analysis converges with what Boissevain argues to be one of the tendencies in the

anthropology of the 1960s:

'The limited vision of social structure as a system of groups begins to crumble. Basic assumptions, such as the all-overriding significance of kinship and kinship groups, are increasingly questioned' (Boissevain 1968: 10).

However, from my analysis another type of group classification does merge as a central datum in Ḥumirī society: *group classification as based on spatiality*. Thus I could pose, for Ḥumiriyya, a model of spatial segmentation against the manifestly inadequate model of kinship segmentation, and argue the merits of the spatial model. In the previous sections of this chapter, however, we had to conclude that also the structure of spatial segmentation has been eroded through recent structural changes and has lost some of its former significance.

The model of spatial segmentation as functioning in Ḥumiriyya can be regarded as the result of culture formation, or institutionalisation, around a central datum in human interaction: *the significance of spatial distance*. Its overall effect may be summarised as follows:

- one does not seek to obtain something from further away (with greater effort, and hence at higher cost), if the same thing is also available nearer-by, at lesser effort and hence at lower cost.

This is a simple principle, which fits very well in the approach of human interaction as a form of economic behaviour (*cf.* Homans 1968). Earlier in this chapter we laid our hands on the specific social-structural aspects of Ḥumirī society which produce the effect that a person for most purposes (goods, services, marriage partners, recreation, collective religious activities) can appeal to his immediate social environment, often even to his nearest neighbours. In particular, this state of affairs is associated with the fact that in Ḥumiriyya most people are more or less equal with regard to the aspects (wealth, power) which are significant for interaction; the prevailing indigenous ideology of equality even claims this to be the case not for most but for *all* Ḥumirīs. In such a society the above principle would yield the spatial distributions which I could identify through quantitative analyses: most day-to-day interaction partners, and most interaction, are concentrated near Ego's home, and their number and relative occurrence (as a fraction of the supply of potential interaction partners) rapidly declines with increasing distance.

This is still a continuous gradient, which can be described in entirely behaviourist terms, without taking into account the specific cultural features and contradictions of Ḥumirī society or any other special human setting from which the quantitative data are derived. I was even able to evolve formulae for the shapes of the attending curves. Now culture formation around this datum consists in the fact that Ḥumirī society has developed an indigenous system of classification which divides someone's social environment in sets or units of people at a number of hierarchical level – sets which at the same level are mutually exclusive and demarcated transitions which are not gradual or continuous, but discontinuous and absolute. For instance: part of Ego's social environment overlaps with that of Ego's neighbour, whereas the remaining part does not. Continuous transitions (in terms of spatial distance) which are entirely not culturally specific, have been replaced by yes/no transitions across the conceptual boundaries of explicitly conceived spatial groupings.

Although in the course of my argument spatial distance and belonging to a certain spatial segment are sometimes used as interchangeable concepts, in fact there is an important difference. For people who live along the boundary between two spatial units on the same level of segmentation may have virtually the same spatial distance vis-à-vis Ego, yet they belong to different units which may have a markedly different position in the formal structure of segmentation. I assessed the relationship between spatial distance and the selection of interaction partners, marriage partners and the frequency of interaction. This showed a continuous distribution. In the recruitment to shared activities centring on the characteristic attribute of a specific segment (such as the threshing floor, or the shrine, or the source) it is an important consideration whether someone does or does not belong to that segment. So as a further extension of our analysis we could now set out to investigate the impact, upon actual dyadic interaction between two persons, of belonging to the same spatial segment while controlling for spatial distance – keeping the latter distant by some statistical technique. Although such an analysis may well enhance our understanding of the significance of spatiality, I have refrained from such a further extension within the context of the present book.

A further elaboration of this indigenous spatial classification would then involve, as we have seen: *the projection, onto the landscape, of the boundaries between the sets of people thus distinguished (which results in visible boundaries: rivulets, cactus hedges, fallow land); the distinction of units at various levels, in such a way that the units comprise each other hierarchically; the assigning of characteristic attributes (the dwelling house, threshing-floor, spring, men's assembly, shrine, cemetery) and of ancestors to the units at each level; the building-in of a mechanism capable of flexibly responding to actual changes in the relative positions of units within damage to the system as a whole (redistribution of characteristic attributes).* And thus, starting with spatial distance, we end up with the model of spatial segmentation which is peculiar for Ĥumiriyya.

It is my impression that such a model may also offer a useful alternative for the description of other societies to which ethnographers have applied the model of the segmentary lineage. For in most cases, the social units into which these ethnographers propose to subdivide the societies under study, have spatial contiguity as well as a more dubious kinship unity, while also the subdivisions of these units tend to follow boundaries which are visible in the landscape. It would seem worth-while to conduct further comparative research into these societies in the light of the present argument.

Spatial segmentation, as defined by me here, is a specific, elaborate form of spatial group classification. Other forms are, of course, possible. Spatial classifications, and in general the social aspects of the distribution of people over their territory (social ecology) have received significantly less attention in anthropology than classifications in terms of kinship. Nonetheless I presume that spatial group classification systems play an important role in virtually all societies.

Above kinship-based classifications, such systems have the advantage that they are based on data which are immediately visible in the surrounding empirical world, both for the actors and for the ethnographer. The spatial distribution of people is a simply, primary datum, whether we deal with a peasant hamlet, a nomads's territory, a middle-sized town, or a work-place within a modern bureaucratic system. Whereas

ancestors, who constitute the basis of kinship-based group classification, already from the first or second ascending generation above their living descendants have ceased to have any empirical basis whatsoever: if they continue to be socially relevant it is through the *ideas* which individual actors have concerning their name, period of time, place of residence, deeds, grave and genealogical position. Nothing is less constant and less idiosyncratic than people's ideas concerning the past. In Ḥumiriyya (and probably in many other societies) this fact is further acerbated by the lack of verbal communication between the actors, as to their belonging to specific kinship groupings and the specific place of ancestors in the definition of such groupings. As a result the notions which individual actors have concerning their kinship-based groupings can always develop in an highly personal, non-consensual, apocryphal direction, diverging in a way virtually unchecked (as long as they have not been publicly expressed) by a consensus-promoting social control – until such time as an anthropological or historical researcher comes along and is bewildered by the lack of consensus and the situational opportunism displayed by the individual statements.

Therefore, far less than spatiality can kinship lead to unequivocal, non-situational group classifications. This advantage of spatiality over kinship renders spatiality not only more attractive for the ethnographer, but also easier to use for the actors within an indigenous classification system.

The research of kinship-based group classifications is an increasingly intricate, specialised and for some increasingly unsatisfactory field of anthropology. In fact, overlooking the past half century, it must be considered a dead end of an enormous amount of talented research – even though societies have almost universally continued to organise and reproduce in kin-inspired settings, and will continue to do so for centuries. One way out of the dilemmas implied in this state of affairs, is to drop the entire preoccupation with group classification, and instead concentrate all attention to 'networks and quasi-groups' (Boissevain 1968). Painstaking research in this direction is certainly needed – although, with the powers of hindsight, we may say (from our 2022 vantage point) that it proved to be another dead end of anthropological endeavour. The extent to which this approach is relevant for the Ḥumiri data is manifest not only from the present argument but also from the work of other researchers in that region (e.g. Jongmans 1968; Brunt 1969; Bos 1969: 61 f.). Meanwhile however the present argument shows the continuing significance and the fascination of the study of group classification in its own right, when it is no longer confined to the study of kinship but sees kinship in interplay with other principles of social organisation, foremost among which – in the Ḥumiri case at least – is that of spatiality.

O that note we may now proceed to Volume II, which offers a detailed ethnographic description, and both theoretical and historical analysis, of Ḥumiri popylar religion.

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In a later edition of this book I hope to systematically refer to the wealth of relevant

publications (my bibliographic database contains some 5,000 items on this point alone) to appear between 1970 and 2022. In the meantime, I have parked these data on:

van Binsbergen, Wim M.J., 2022, Materials towards updating my 1967-1971 study of Religion and social organisation in north-western Tunisia, Volume I: Kinship, spatiality, and segmentation, Volume II: Cults of the land, and Islam, Papers on Intercultural Philosophy / Transcontinental Comparative Studies, Hoofddorp: Shikanda, at: <https://www.quest-journal.net/shikanda/Berber/PREVELEMENTEN.pdf>

14. List of the inhabitants of the villages of Sidi Mḥammad and Mayziyya, 1968

In the course of the analysis of the data, a different system of numbering the persons on this list has been adopted; probably there are discrepancies between the numbers here (even the 'original numbers' and the numbers used in the course of the above argument – this will be corrected in future

† = deceased

no.	original number (corrected)	head of household	wife	co-residing children ⁹¹
IN THE VILLAGE OF SIDI MḤAMMAD				
1	10	°Abd Allah bin °Aissa	Maḥbuba	Muassin, Fuaziya
2	11	Al Hadi bin °Aissa	-	-
3	12	(°Aissa)†	Mabruka	Khamis 03/0 ⁹² , Sibti 03/1 ⁹³ , Jamila, Shadli 03/2
4	13	Al Hadi	Najma	Hussin, Rumdhan, Mburk
5	14	Al °Ayash	Ziyana	Shadli, Hasayida, Baya, Saluha
6	15	Safi bin °Amir	Mahbuba	Muhammad 6/1, ⁹⁴ Habib, Ahmad, Sayid, Zaduq, Eluhi, Ziyana, Salaha, Al °Azar, Salila
	79	Muhammad bin Saffi		
7	16	Hillal	1. °Ayasha	Rahmani, Muṇṇaf, Nashat, Fataya, Layila
8	16	(Hillal, same)	2. Mabruka	Yamina, Farid, Fadhi
9	17	(°Ali)†	°Ayasha	Ahmad, Bu Jimma'a, Hanusha
10	18	Tahar bin °Ali	Fatima	Rudhiya, Zuharra, Layila
11	19	Dhiab bin Hassuna	Falusa	?
12	20	Baḥush bin	Ḥara	Hussin, Sassiya, Sadiya, Hamad

⁹¹Mainly adolescents are listed; for full details see the village genealogy, below

⁹²In military service at the time of the fieldwork

⁹³In military service at the time of the fieldwork

⁹⁴has a separate dwelling in the house of deceased 49

		Hassuna		
13	21	(Hassuna)†	Mburka ⁹⁵	
14	22	(Hassuna, same)†	Mabruka ⁹⁶	
14a	23	(Hassuna, same)†	Turquya ⁹⁷	Iḥmayid (= °Abd al Qadir), 14a/1
15	24	Mansur	Mburka	Hassan
16	25	Jilani	Nashama	Nuri
17	26	Salah bin Tarshun	Ribha	?
18	27	Rabha bin °Ali	Jamila (= Ziyana)	
19	28	Muhammad bin Al °Abadi	—	Mburka °019', and Muhammad's daughter Ziyana (* °Amar), = Mburka's granddaughter
20	29	Salah bin Muhammad	Fatma	Hadda
21	30	Tahar bin Hamuda	Halima	-
22	31	Ibrahim bin °Abd Allah	Ziyana	°Amara (°), Halima (i)
23	32	°Amar bin °Abd Allah	Ziyana	Trahi (i), Fadhila (i), Sassi (°), Bu Jimma°a (°), Baya (i)(* Ga°a Ramal)
24	33	°Abd Allah bin Bu Tara	Fatma	Rahmani (21 years), Hafniya ⁹⁸ (15 years), Dalila (10 years), Munçaf (8 years)
25	34	(Al °Ayash)†	Ribha	
26	35	(Hamuda)†	Zuhara (= Shaba)	Hamda (°), Brada (i), °Aziza (i), al °Azar (°), Fuziya (i)
27	36	Muhammad bin °Amur ⁹⁹	Habiba	°Abd al Huni ¹⁰⁰ , Shadliya, Mariam, Barka, Musfa, Nashat
28	37	°Abd al Hafidh	°Ayasha	-
28b?		Tayib bin Hamuda ¹⁰¹	(daughter of Salah bin Khamis & Burnuya, sister of wife of Bakhush)	
29	38	Muhammad bin Hasnawi	Hadda	
?		(Jilani bin Ibrahim)		
30	39	(al °Abadi)†	Mina	°Abd al Hamid 30.1
31	40	Bashir	-	Rabha (his brother)
32	41	al °Abadi bin Muhammad	-	
33	42	Tayib bin Muhammad	Khadisha	Muhammad (18 years), Nur ad Din (14 years), Muḥtar (8 years), Nashi (5 years), Uadhila (3 years)
34	43	Ahmada bin	Mabruka	Munsha (°), Fatima (i)

⁹⁵ stays with Jilani bin Hassuna, no.

⁹⁶ stays with Dhiab, no. 11

⁹⁷ stays with Baḥush, no. 12

⁹⁸ check name

⁹⁹ usually called Muhamad bin Tunis, after his mother Tunis who was born at Sidi Mḥammad

¹⁰⁰ check name

¹⁰¹ in Tra'aya-sud

		Muhammad		
35	44	Hillal bin Muhammad	(Sharifa)†	Mashid (°), Dunis (°), °Amur (°), Rihana (i)
36	45	Jilani bin Salah (28 yrs)	Ursuya	°Ayasha
37	46	Salah bin Khamis	Burnuya	al Hadi (24 years)
38	47	°Amar bin Hillal (25 yrs)	Baya	
39	48	(°Abd Allah)†	Mburka	Muhammad (20 years), Shadli (17 years), Tahar (13 years), Zuhara (11 years)
40	49	Rahmani bin Yusuf	Ḥamisa	Hussin (11 yrs) ¹⁰² , Hassin (3 yrs), Nabil (2,5 months), Hafsuya (mother of Rahmani); Bashir ¹⁰³ = 43
41	50	Rumghan bin Hamad	Uahida	°Amara, Hamda, °Ali, Fatima, Masubuya
42	51	Mhammad bin Abu'l Qasim	Khara	Munçaf, Nashi, Uadhila, Nur ad Din
43	49 see above	Bashir ¹⁰⁴		
44	??	°Abd Allah bin Mhammad	-	
44a	50	Hamad bin Amir (with 41)	+	
45	??	Salah (with 40) ¹⁰⁵		
46	52	Hamuda bin al Ahsin	Mahbuba	
47	53	Hasni bin al °Abadi	Fatma	
48	54	Habib bin Harassi	Mahbuba	
49		(Bashir bin °Amir)†	±	two °children
50	55	Ghunaya	Ribha	Muhammad, Salah ¹⁰⁶ , Maḥbuba, Baya, Tahar, Munshi
51				
52	56	°Abu'l Qasim ¹⁰⁷	Ribha	
IN THE VILLAGE OF MAYZIYA				
101	57	Bu Jimma°a bin Rabha		
102	58	Ahmad bin Rabha	Masauda	
103	59	Muhammad bin Rabha	Fatma	°Amar
104	60	Muhammad bin °Amur	Namala	°Amara, °Ali, Sibti, °Abd Allah
105	61	Hassan bin Kashrud	Baya	6 °children
106	62	Muhammad bin Hassan	Ribha	
107	63	Rabha bin Hassan	Mburka	

¹⁰² stays in °Ain Draham

¹⁰³ MBS of Raḥmani

¹⁰⁴ MBS of Raḥmani

¹⁰⁵ uncle, with Raḥmani

¹⁰⁶ check: or: Muhammad-Salah

¹⁰⁷ Kaf al Hanut

108	64	°Abd Allah bin Kashrud	Zuhara	
109	65	Muhammad bin Bashir	°Ayasha (Ribha)	
110	66	°Ali bin Sa°ad	Masauda	Rbiha ¹⁰⁸
111	67	°Amar / al Hadi bin °Ali ¹⁰⁹	Hadda	
112	68	Rabha (= Mahmud) bin °Ali	≠ (meaning: unmarried?) 110	
113	69	Muhammad bin Tayib	Hadda ¹¹¹	
114				
115	70	Ahmad bin Kashrud	Ziyana	Turquya, Bashir, al Hadi
116	71	Salah bin Ahmad	Khadusha	al °Azar, Haniya, Maliha, Rahmani, °Azayiz, Haddi
117	72	Bu Jimma°a bin Ahmad	Ribha	°Abd al Karim, Rumdhan, Khamis
118	73	Ahmada bin Muhammad bin °Amur	Haza	
119	74	°Abu'l Qasim bin Muhammad bin °Amur	Baya	
120	75	(al Hadi)†	Zuhara bt Muhammad b °Amur	Ziyan, Mburka, Sibti
121	60	(Ḥamis bin Muhammad)†	Manubiya	Ghanaya, Hadda, Salah
122	76	Muhammad (Sergeant)		
123	77	°Ali bin Muhammad	Dhabiya	
124	78	al Ahḍir bin °Ali	Barka	
	80	Abu'l-Qassim bin Zururī ¹¹²		

¹⁰⁸ this is incorrect, see 0108, 13,6 in typewritten field notes

¹⁰⁹ this is incorrect, see 0108, 13,6 in typewritten field notes

¹¹⁰ this is incorrect, see 0108, 13,6 in typewritten field notes

¹¹¹ this is incorrect, see 0108, 13,6 in typewritten field notes

¹¹² ex-inhabitant still having a house in the village of Sidi Ṃhammad :

15. The reconstructed overall genealogy of the research area, 1750-1968

see: van Binsbergen, Wim M.J., 1969 / 2020, Complete genealogies (reconstructed) for the inhabitants (1968) of the villages of Sidi Mhammad and Mayziya, homdat 'Atatfa, 'Ain Draham, Tunisia, online available at:

http://www.quest-journal.net/shikanda/Berber/genealogy_comprim_trim.pdf

to be viewed with browser set at 200 %

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