The Human Sociobiological Analysis of Incest Avoidance:

The State of Play and Directions for Future Research.

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To Professor Anthony Forge with fond memories and gratitude,

and to my M, F, B, Z, MM, FM (*d.*), MF, and FF (*d.*) whose coefficient of relationship with ego exceeds or equals .25 and whose altruism made this thesis possible.

> Natural science will one day incorporate the science of man, just as the science of man will incorporate natural science; there will be a *single* science. (No, not E.O. Wilson. Karl Marx 1844.)

I certify that this thesis is my own work and that all sources used have been appropriately acknowledged.

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A.J. Uhlmann

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Rules governing the submission of theses require me to certify that this thesis is my own work. However, this volumes represents the effort of a great many people who have contributed both directly and indirectly to the process that resulted in this thesis. It would be impossible to acknowledge them all here. Therefore, I decided to adopt a minimalistic approach and confine myself to acknowledging some of the contributions of those who were directly involved in the work on this thesis.

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Prof. Anthony Forge quite aptly described once the important task of anthropologists. Historians, he observed, study dusty papers in archives; economists study the way models behave; psychologists study the way white rats and American college students respond to torture in the lab; and sociologists study the way people answer questionaires. Only anthropologists study the way people behave.

Prof. Forge is one of those who should be thanked (or blamed) for my admission to the Department of Prehistory & Anthropology. He passed away only a few months ago. This thesis is dedicated to him, with gratitude and fond memories, and to my family whose encouragement and assistance have been crucial.

Chapter 1

Introduction

The application of sociobiology to humans, a field to which I shall refer here as human sociobiology, gained recognition in the mid 1970s in Edward O. Wilson's 1975 *Sociobiology: The New Synthesis* (ch 26). Ever since this book was published, a great debate, often acrimonious (e.g. Sahlins 1976), has flared over the academic and often even moral legitimacy of some of the propositions of human sociobiology (e.g. Kitcher 1985).

The study of incest avoidance is considered by some of its sympathetic observers as a major success of human sociobiology, a success which demonstrates the power of human sociobiological method and propositions (Ruse 1982, ch. 11 ; Wilson 1983; Dawkins 1989, 293-294). Less sympathetic observers of human sociobiology are critical of both the evidence and the propositions put forth by human sociobiologists (e.g. Leavitt 1990).

In this thesis I will try to avoid the general debate over human sociobiology and its acrimony. I will attempt a dispassionate assessment of the suggestions and propositions advanced by human sociobiologists concerning incest avoidance, and draw implications for future directions of anthropological research into human incestuous behaviour.

The scholars whose work I discuss below (primarily Ray H. Bixler [1981], Joseph Shepher [1983], Pierre L. van den Berghe [1980; 1983] and Clive V.J. Welham [1990]) seem to follow Charles J. Lumsden & Wilson's understanding of human sociobiology to the effect that "The central tenet of human sociobiology is that social behaviors are shaped by natural selection" (Lumsden & Wilson 1981, 99; cf e.g. Shepher 1983, 85-86; this, however, may not accord with other practitioners' understanding of the field, e.g. Caro & Borgerhoff Mulder 1987). Therefore, this thesis focuses, in effect, on the application of considerations of natural selection to human incest avoidance. I would like to stress that my aim does not include an anthropological critique of neo-Darwinism (for which see Ingold 1990) or of human sociobiology in general (for a recent debate on human sociobiology see Kitcher's 1987, which includes responses of various scholars in the field).

When referring to incest I mean full sexual intercourse between primary kin of opposite genders. The stress both on full heterosexual sexual intercourse, and on the participants being human primary kin, follows from the nature of the human sociobiological treatment of incest avoidance. This treatment considers incest avoidance to be the human case of close inbreeding avoidance. The term "incestuous behaviour" will be taken to mean behaviour in relation to incest and will include both the perpetration and the avoidance of incest, because both are essential to human sociobiological analyses. The term "incestuous strategies" will refer to patterns of incestuous behaviour when their role in fitness-maximisation is considered.

After serious consideration, I decided to exclude two issues from the thesis. One is royal incest, studied from a sociobiological perspective by van den Berghe and Mesher (1980). Their work has been quite effectively criticised by Philip Kitcher (1985, 275-279) on theoretical grounds, and by Arens, who pointed out that in most instances analysed as royal incest, there is no indication as to whether the marriages were reproductive or merely symbolic and devoid of sexual content, and in some instances they were most probably of the latter type (Arens 1986, 107 ff.). In addition, since the number of verifiable instances is small, a successful demonstration of a fit between consideration of maximisation of individual genetic fitness on the one hand and social custom on the other will not be of great value as any fit could be attributed to coincidence. This is probably the reason the topic of royal incest has not been the focus of later human sociobiological studies (for further discussion of royal incest from a sociobiological perspective see Bixler 1982b; 1982c). The point I do wish to pursue is whether the behaviour of most humans in most instances is shown to be influenced by natual selection.

The other excluded issue is the attempted incorporation of the analysis of social taboos and rules concerning incest into evolutionary theory. I decided to exclude this topic for a few reasons. One is that most human sociobiologists who have focused on human incest avoidance have considered these taboos to have some effect in inhibiting incest, but did not pursue the matter any further (e.g. van den Berghe 1983, 92-93, 98 ff. 151; Shepher 1983, 107 ff., 113-114; Lumsden & Wilson 1980a; 1980b; 1981, 71, ch.6, 346-350). Lumsden and Wilson used incest taboos as an instance of a hypothesised process of cultural evolution (Lumsden & Wilson 1981; 1982; c.f. Durham 1990, ch. 6 for a different cultural evolutionary model of incest taboos). In this instance the aim was not to further the explanation of human incest avoidance, but rather to illustrate particular models by the instance of human incest avoidance. I therefore felt that these works fall beyond the scope of this thesis. In addition, the model they had developed was severely criticised (successfully, in my view) by Kitcher (1985, ch 10) and others (particularly Caplan 1982; Charlesworth 1982, Hallpike 1982; Hartl 1982; Johnston 1982; Maynard Smith 1982; Schubert 1982; Slobodkin 1982).

Nancy Wilmsen Thornhill is currently pursuing an argument which is novel in the evolutionary study of cultural rules relating to incest. In her work published thus far (Thornhill 1990; 1991) she argued that rules banning incest were not devised for the purpose of reducing copulation between primary kin because other mechanisms cause the sexual avoidance of primary kin (e.g. the Westermarck effect to be discussed below). She suggested that rules banning incest serve the interests of elite men in stratified societies, and of men who share power in more egalitarian societies. This is so because incest prohibitions are extended to include close relatives other than primary kin. This prevents affinal marriages in general, and cousin marriages in particular, from taking place, a situation which in turn prevents the concentration of too much power among those of subordinate status in stratified societies, and among the equal male members of egalitarian societies (egalitarian for males, that is). Thus, according to Thornhill, the interests of the two groups of men nominated above are promoted (Thornhill 1990; 1991, 250).

Thornhill's work is not yet fully published. In addition, her analysis is very much along the lines of conflict theory in sociology. Considerations of adaptation and genetic fitness are not relevant to her analysis[1]. Because her work is not yet fully published, and because her theoretical approach is considerably different from that discussed below, I decided not to incorporate an analysis of Thornhill's work in this thesis.

Having outlined what this thesis is not about, I will briefly describe the scheme of things to follow.

Chapter 2 describes the extent to which incest avoidance is universal in human societies. This universality has been the basis of the many academic approaches to human incestuous behaviour, including those whose perspective differed from the human sociobiological (e.g. Mead 1968; Levi Strauss 1969, chs 1, 2). One well documented exception is described, but by and large, the cross-cultural universality of the tendency to avoid incest seems striking.

Having considered a proposition which is basic to many analytical approaches towards incest avoidance, the discussion moves to cover two topics, which are fundamental particularly to the human sociobiological approach. Chapter 3 looks at these two fundamental topics, namely inbreeding depression and its effects, and close inbreeding avoidance in other species. The first is crucial because it constitutes the selective pressure against inbreeding in human sociobiological theory. The second both underpins the importance of inbreeding depression and suggests that early hominids avoided incest as well. The discussion ends in the

[1]Even though her work is not yet fully published, I would like to point out in brief what I think are two fundamental probelms in Thornhill's approach. One is her assertion that incest taboos and prohibitions play no role in preventing incest. This is not backed by any analysis and remains an assertion, which in my view is unfounded. The other problem is the fact that her functionalist interpretation does not accommodate patrilineal parallel cousin marriages which are widely practised in the Middle East (see Section 4.3.3 below).

conclusion that even though it is plausible that inbreeding depression formed a selective pressure against excessive inbreeding, there remains some scope for further investigation of the possible evolutionary effects of inbreeding depression.

Chapters 2 and 3 set the stage for the discussion of human the sociobiological treatment of incestuous behaviour in chapters 4 and 5. Chapter 4 focuses on the ontogenetic process first suggested by Edward Westermarck, according to whom proximity between individuals when either one or both are very young will result in sexual indifference. Following Westermarck, human sociobiologists argue that this process has been selected for by natural selection. I review the evidence and assess what is already known about the process, and what further research is needed. I conclude that contrary to what is believed by some (e.g. Dawkins 1989 293-294; Wilson 1983), the knowledge we do have about the putative process is remarkably little, and a great deal of research lies ahead.

Chapter 5 moves from the ontogenetic and proximate levels of the discussion. Human sociobiological explanations of human incestuous strategies in terms of maximisation of genetic fitness are considered. I review the argument, analyse its theoretical aspects, and test it against empirical data. I conclude that as opposed to the subject covered in the previous chapter, this line of investigation seems to hold little promise.

Chapter 6 draws together the lines of argument developed in the thesis and points out directions for further anthropological research that could follow from the human sociobiological analysis of incest avoidance.

Chapter 2

How Universal Is the Avoidance of Incest?

That incest avoidance is universal is a basic assumption of most analytical approaches to human incestuous behaviour including those opposed to the human sociobiological (e.g. Mead 1968; Levi Strauss 1969, chs 1, 2; but see Wagner 1972 and Schneider 1976 according to whose approach this universality is insignificant and irrelevant). This chapter assesses what is meant when incest avoidance is said to be universal, and the extent to which incest avoidance is indeed universal.

As used here, the concept of universality does not require that incest should be shunned altogether (cf Brown 1991, ch. 5). What it does mean is that in most societies, most people do not engage in sexual intercourse with their primary kin. There is virtually no statistical data on the extent to which individuals practice incest. This is due to the sensitivity of the topic, and to the fact that in most societies it is banned. (There are rough estimates in various places about the rates of child sexual abuse [e.g. Laurance 1988 and Baker & Duncan 1985 in Britain]. But in addition to methodological difficulties stemming from the sensitivity of the matter, the concept of child sexual abuse includes acts other than intercourse, refers to perpetrators other than primary kin, and excludes consensual sexual relations between adults. Therefore, estimates of child sexual abuse are of little use here.[2]) Still, even though statistical data are lacking, it is a commonly held view that incest is not practised by the majority of people.

This seems to be true in virtually all cultures that have been studied. There are no contemporary societies in which incest is reportedly commonly practised.

Thornhill examined the 186 societies found in the Standard Cross Cultural Sample (SCCS) of the Human Relations Area Files (HRAF). Using the original ethnographies, and data from the HRAF, she focused on the 129 societies for which data on mating and marriage customs exist. Even though 56% were found not to have explicit rules banning incest (Thornhill 1991, 249, 258-260) it seems that in all societies sampled in the HRAF sexual relations between primary kin are generally avoided even when explicit laws banning incest do not exist (Frayser 1985, 102 ff.). Even though it can be quite safely stated that in virtually all

[2]This distiction between child sexual abuse and incest avidance was criticised by J.S. La Fontaine who argued that it ignores the similarities of relationships in the family between cases of incest and cases of child sexual abuse (La Fontaine 1988).

societies sexual relations between primary kin are not the norm, there have been some societies in the past where incest was common.

In Ancient Greece half siblings (r=.25) were allowed to marry. Marriage between offspring of one father (but of different mothers) was permitted by the Athenians. The Spartans accepted the marriage of half siblings who shared a mother in common. There is evidence that such marriages were practised by the Greeks at various social strata, though I could not find in the literature any quantitative evidence that can throw light on the extent to which they were practised (Harrison 1968, 22-23 [incl. f.n. 3 on p. 22 and f.n. 1 on p. 23]; Hopkins 1980, 311; Lacey 1968, 106, f.n. 31-33 on p. 284; Licht 1932, 516; Westermarck 1921, 97).

There are various times and places, like Iran of the late centuries B.C. and the early centuries A.D., at which full sibling marriage was said to be common practice amongst the general public. However, the credibility of those reports is debatable (Westermarck 1921, 82-97, 199-201; Hopkins 1980, 354 f.n. 89; Middleton 1962, 609 f.n. 45). The only society for which there is conclusive evidence of socially sanctioned, widely practised marriages between full primary kin (i.e. individuals with a coefficient of relationship r=.5 [see below]) amongst the general population (not royal incest, that is) is Roman Egypt where full siblings commonly married.

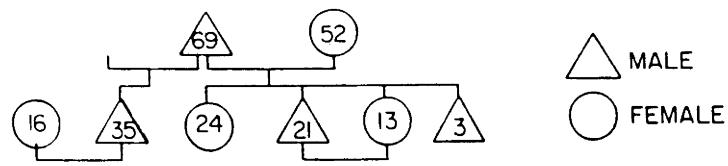
Evidence on the practice includes reports of foreign writers who had recorded this custom (Hopkins 1980, 312; Middleton 1962, 607-608); popular stories, poems and other aspects of folklore that treat full sibling erotic love as commonplace and full sibling marriage as a fortunate event (Hopkins 1980, 344-348; the horoscope quoted *ibid* 303); and, most importantly, the papyri used by the public administration that have remained intact thanks to Egypt's dry climate (Hopkins 1980, 303, 315).

Most of the surviving papyri that contain evidence of full sibling marriages are those that were used for official purposes: they include letters, birth certificates, marriage and divorce settlements. There is, for example, a letter sent to the tax authority by a woman on behalf of her son. She applied for special tax conditions for him and included his detailed genealogy. The genealogy shows three consecutive generations of full sibling marriages on the mother's side (her parents, grandparents and great-grandparents) covering roughly the period from 50 to 120 A.D. (Middleton 1962, 607; cf Hopkins 1980, 321-322).

Private correspondence also provides evidence of the mundane nature of the practice of full sibling marriages. Wedding invitations and private letters nonchalantly refer to full sibling marriages. They convey the impression that such marriages were taken as a matter of course (Hopkins 1980, 321, 324-325).

The most crucial information on the extent of the practice of full sibling marriage comes from household census returns which date back to the first three centuries A.D. The authorities in Egypt conducted a census on a regular basis for tax purposes. The returns included the names of the individuals in households and the relationships between them (Hopkins 1980, 312 ff.), as shown in the following example, figure 2-1 (figure 1 in Hopkins 1980, 314).

Figure 2-1: A Translated Census Return



[File] 97: To Apion, imperial scribe of the Prosopite district, from Pantbeus son of Phibis. In accordance with the orders of the most distinguished Prefect, Calvisius Statianus, for the house to house census executed under his auspices, I declare my property situated in the village of Thelbonthon Siphtha, namely a house with the land uncultivated owned by Pantbeus son of Phibis the younger son of Pnepheros.

Occupants:

A Pantbeus, son of Phibis the younger son of Pnepheros; mother Thasachmounis daughter of Pnepheronphis, with a scar on his left cheek, aged 69 years

B Taapollos, daughter of Orsenoupis; mother Taaronnesis, daughter of Anl. . . aged 52 years

C Isidoros, son of Pantbeus, born of the two above parents, with no distinguishing mark, aged 3 years

D Pkouthis, his brother, son of Pantbeus; mother Thaesis, daughter of Perpheis, aged 35 years

E Thermouthis, his wife, daughter of Nemesas son of Tithoes; mother Tatithoes, aged 16 years

F Phibis, his brother; mother Taapollos, daughter of Orsenoupis, with no distinguishing mark, aged 21 years [brother-sister marriage]

G Thermouthis, his sister and wife, born of the same parents, aged 13 years

H Taaronnesis, his sister, born of the same parents, aged 24 years

In the 14th year of the Emperor Caesar Marcus Aurelius Antoninus Augustus, Conqueror of Armenia, Media, Parthia, Germany, The Greatest, on the 25th Epeiph [= 19 July, AD 174] I Pantbeus, the son of Phibis, we the aforementioned (have) deposited this declaration as here set out; I, Petos son of Petsiris have written on his behalf, since he does not know how to write (P. Brux. 5)

The conclusion that full sibling marriages were acceptable and common practice can be drawn with a fair degree of certainty. But since the sample of preserved returns is rather small, 172 returns listing 880 persons, and a little biased in some ways (towards the literate for instance), further conclusions should be drawn with caution (Hopkins 1980, 312-320).

The extent to which full sibling marriage was practised is remarkable. It is possible to establish the full parentage of individuals from returns of the second and third centuries A.D., when mothers' names were often entered on the returns. Of 113 marriages that were current when the returns were lodged, up to 23 (17 certainly and 6 possibly), i.e. 15-21%, were between siblings. Of these 23 cases, 11 or 12 were between full siblings (9 or 10 of the 17 certain cases), 8 were between half siblings (6 of the same father, 2 of the same mother), and in the 3 remaining instances it was unclear whether the married pair were full or

half siblings. There were a further 13 marriages between siblings traceable among the ancestors of the individuals who were registered in the census (it is not clear from Hopkins' writing whether he means full siblings or half siblings) (Hopkins 1980, 304, 320-321). When taking into account demographic factors such as high infant mortality rates and varying sex ratios which would have left many without eligible siblings, the figure becomes even more remarkable. Hopkins estimated that full sibling marriage took place in well over one third of the instances in which it was possible (Hopkins 1980, 304).

One could take issue with Hopkins' conclusions concerning the extent of the marriage. The definite number of full sibling marriages is 9 of a sample of 113. This is just under 8%. Even so, the number is remarkable.

The nature of the relationship between full sibling spouses is important for the purpose of establishing whether sexual relations between siblings were indeed entered into by such a large number of people, or whether the marriages were of symbolic rather than sexual and reproductive significance. The data suggest that spouses normally formed erotic relations and that the conjugal unit was a unit of reproduction as well. Erotic love was indeed an important element of the relationship between the sibling-spouses and of the cultural construction of marriage in general (Hopkins 1980, 304, 324-325). Individuals were not forced into relationships to which they were averse. This is evident in some cases from private correspondence between spouses who were full siblings (Hopkins 1980, 324-325; Hopkins quotes such a correspondence).

Marriage settlements are also enlightening. One of the possible causes of divorce cited in the text of marriage settlements was the waning of romantic feelings between the husband and wife. There are records of divorce on these grounds between married full siblings. The fact that women were economically independent of their husbands, owned property in their own right, and could initiate divorce procedures reduced the possibility of their being forced to remain within a conjugal bond against their will (Hopkins 1980, 322-323, 334-342, 353).

The direct evidence of full sibling marriage covers the period of the first three centuries A.D., especially the second and third. There is no conclusive evidence as to when the practice started and when it grew popular. There is little evidence of full sibling marriage among the commoners in the Pharaonic times, up until 323 B.C. (Hopkins 1980, 311; Middleton 1962, 605-606). Some circumstantial evidence points to the possibility that full sibling marriage was common practice in Egypt during the Ptolemaic period which lasted from 323 BC up to the Roman period (Hopkins 1980, 311-312).

The circumstances which brought about the end of the practice are much better documented. In the 3rd century A.D. the Romans extended Roman citizenship to most free residents of the Empire for taxation

purposes. This involved the imposition of Roman laws on the new citizens in Egypt as well as in other parts of the Empire. These laws viewed full sibling marriage as incest, and banned them. The state started enforcing these laws in Egypt in 212/213 A.D., but the Romans apparently still had problems in preventing full sibling marriages as late as 295 A.D. (Hopkins 1980, 353-354; Middleton 1962, 607).

The Roman Egyptian example demonstrates that societies in which siblings frequently have sex and marry can and at least in one instance did exist and flourish. It also demonstrates that cordial familial relationships on the one hand and romantic, erotic affections on the other need not be mutually exclusive. However, even in Roman Egypt there are no records of parent-offspring marriages.

Roman Egypt is the only society for which we have documented evidence of commonly practised erotic relationships between primary kin. This uniqueness, and the fact that even in Roman Egypt there is no evidence of widely practised parent-offspring erotic relationships underpins the absence of evidence of widely practised incest in all other studied societies. It therefore seems a safe conclusion that in virtually all societies marriages and sexual relations between primary kin are not normally practised. The questions of whether Roman Egypt was so unique in allowing siblings to marry, and if so, why did similar practices not prevail in other societies, are yet to be answered. Obviously, social or geographical isolation cannot explain such practices in a large society like the Roman Egyptian.

Reliable statistics about the practice of incest in various societies needs to be gathered. In particular, research is needed into the extent to which individuals do actually engage in incestuous relations. This might help throw light on the mechanisms behind individual incest avoidance, for instance in providing data on the extent to which social bans on incest play a role in incest avoidance or not. In addition, predictions concerning patterns of incestuous relations, for instance predictions concerning the various rates of M-S, F-D and B-Z incest (see below) could be tested, and the picture of human incestuous behaviour made much clearer.

The universality of incest avoidance is at the heart of most analytical approaches to human incestuous behaviour. Human sociobiologists add a couple of understandings which are basic to their analysis of human incest avoidance. I shall turn to these understandings in the following chapter.

Chapter 3

Fundamental Propositions

Two propositions are common to various suggestions that natural selection has affected human incestuous behaviour. One is that in humans inbreeding depression reduces reproductive success in matings where $r \geq .25$, and particularly when $r \geq .5$, thus forming a selective pressure against close inbreeding[3]. The other is that there is a wide-spread cross-specific tendency to avoid close inbreeding (r=.5) particularly among primates. (e.g. Bixler 1981, 640-642).

In this chapter I will discuss these two issues and outline their implications for the evolutionary study of incest avoidance.

3.1. Inbreeding depression

In this section I will review the suggestions that inbreeding depression occurs in humans, and that it reduces reproductive success thus forming selective pressure against close inbreeding.

There is evidence that humans whose coefficient of inbreeding[4] $F \geq .125$, and particularly $F \geq .25$, suffer reduced viability. Such individuals are born of parents whose coefficient of kinship[5] $k \geq .125$, $k \geq .25$ in the latter instance, i.e. whose coefficient of relationship[6] $r \geq .25$, e.g. uncle/aunt-niece/nephew, double first cousins and half-sibling pairs, as well as more closely related individuals.

Inbreeding depression is the term used to describe the reduced viability and fertility of individuals whose

[3]There are other costs that may be associated with close inbreeding in special circumstances, like a more intense competition between siblings owing to increased similarity (Bateson 1983a, 259) or reduced cooperation and increased competition between different groups in the population owing to the decline in relatedness between members of these groups and hence a decline in fitness gains one can achieve through assisting members of other groups in the population.

[4]The coefficient of inbreeding is defined "... as the probability that an individual will inherit at a given locus two genes which are identical by descent" (Harrison 1988, 190; and see Cavalli-Sforza & Bodmer 1971, 342 for a similar definition) and in practice is only calculated for a few generations of ancestors.

[5]"The coefficient of kinship of two individuals is the probability that a gene taken at random from one of the individuals is 'identical' ... *by descent* to a gene taken at random from the same locus in the other individual"(Harrison 1988, 200).

[6]Defined here as "... the fraction of genes in two individuals that are identical by descent, averaged over all of the loci" (Wilson 1980, 35. The reason definitions in this chapter were gathered from different sources is that I decided to sacrifice loyalty to one single source on the alter of selecting the most appropriate definition available.).

F coefficients are high[7]. Inbreeding has the effect of increasing the proportion of homozygous loci in individuals. The closer the inbreeding, the more loci are homozygous. This means an increase in the probability of rare single autosomal recessives or clusters of rare polygenic recessives being expressed, recessive genes being deleterious more often than their dominant alleles (Bittles & Makov 1988, 153-154; cf Bittles 1980, 768, 776).

Studies have been conducted in an attempt to assess the effects of inbreeding at the level of $r \geq .25$ (Schull & Neel 1965; Adams & Neel 1967; Seemanová 1971; Baird & McGillivray 1982). Based on the assumption that the ratio of child mortality and disability to individual inbreeding coefficient is constant even in close inbreeding, a method used by William J. Schull and James V. Neel in their studies in Japan (1965), Shepher extrapolated from the data to generate the following table 3-1 (table 7.2 in Shepher 1983, 93; for further information on the assumption that reproductive mortality is linearly dependent on the inbreeding coefficient of an individual see Cavalli-Sforza & Bodmer 1971, 362-364):

Table 3-1: Child Mortality and Disability as Related to Consanguinity of Parents

Relatedness of parents	Disability (%)	Disability and child mortality (%)	Viability (%)
1/32	6.47	10.87	89.13
1/16	7.35	12.35	87.65
1/8	10.30	17.30	82.70
1/2	24.80	41.80	58.20

The results portray the effects of high inbreeding as harmful. Over 40% individuals whose $F=.25$ suffered disability or child mortality, compared with about 11% in offspring whose $F=1/64$ (an increase of almost four times in child mortality and disability), or with about 17% in individuals whose $F=.125$ (well over a twofold increase in child mortality and disability).

However, Shepher's results are somewhat overstated as the studies cited by Shepher (e.g. Seemanová 1971) as well as other studies of inbreeding depression in offspring of incest (e.g. Baird & McGillivray 1982) were biased towards overemphasising the level of risk to the offspring. This bias was caused by methodological problems, including lack of an adequate control group (Bittles 1979; Bittles & Makov 1988, 156 ff.; Bittles 1983, 103).

The extent to which inbreeding would be harmful varies widely across populations, and is affected by

[7]Inbreeding is defined as "the crossing of closely related plants or animals," and inbreeding depression as "decreased vigor in terms of growth, survival, or fertility following one or more generations of inbreeding" (King 1972, 148).

various factors. The genetic load[8] of a population would have a direct effect on the cost of inbreeding (Bittles 1980, 774).

Another factor which is said to lower the increase in mortality and morbidity associated with inbreeding is the mean coefficient of inbreeding in a population. In small populations with a high mean coefficient of inbreeding the increase in an individual's coefficient of inbreeding as a result of both his/her parents being primary kin would be lower than in a population with a low mean coefficient of inbreeding (Livingstone 1983, 111). This is so because the smaller the increase in the coefficient of inbreeding, the fewer additional loci become homozygous, thus lowering the probability of further recessives being expressed. The fact that the increase in one's coefficient of inbreeding relative to the mean coefficient of inbreeding will be negatively correlated with the mean coefficient of inbreeding is mathematically necessary from the formula used to calculate F: $F=(1/2)^{n+n'+1}(1+F_z)$ for each ancestor, where n, n' are the number of steps in the lines of descent from the common ancestor to the parents of the individual concerned and F_z is the coefficient of inbreeding of the common ancestor (Harrison 1988, 190). The latter may be generalised as the mean coefficient of inbreeding (On mean coefficients of inbreeding see Harrison 1988, 191; Cavalli-Sforza & Bodmer 1971, 352-353).

To examine the effect of the mean coefficient of inbreeding I propose to examine the most commonly cited example of an extremely high mean coefficient of inbreeding -- that of the Samaritans (e.g. Harrison 1988, 191). For them the mean F was estimated to be 0.0434 (Bonné 1963). The coefficient of inbreeding of an offspring of full siblings would be $2 \cdot (1/2)^{(1+1+1)} \cdot (1+0.0434)=0.26085$. If the mean coefficient of inbreeding is subtracted, the increase in loci which are homozygous by descent would be 0.26085-0.0434=0.21745. This is indeed lower than 0.25, the increase that is incurred in situations where the mean coefficient of inbreeding is 0. However, two points need to be made. One is that the Samaritans present an extreme example of an inbred population. A mean coefficient of inbreeding which is higher than 0.01 is rare (Cavalli-Sforza & Bodmer 1971, 352). In addition, the increase in homozygosity is still very dramatic even in this extreme situation (roughly 0.22).

It has been suggested that continuous inbreeding over generations would increase the exposure of recessive genes to natural selection thus dramatically reducing the genetic load (Livingstone 1969; cf Arens 1986, 21-23; Leavitt 1990, 974-975; Bittles 1983, 103-104; Bittles 1980, 775). This has been demonstrated in laboratory studies on rat strains (Dawkins 1983, 106), but failed to be confirmed in studies like those conducted in India by A.H. Bittles and colleages, studies of large human populations with a high mean

[8]Genetic load is defined as "the average number of lethal equivalents per individual in a population" (King 1972, 121; for a somewhat different definition see Cavalli-Sforza & Bodmer 1971, 354). Relevant to the topic at hand is the average number of lethal and sub-lethal recessives per individual in a population.

coefficient of inbreeding (Bittles et al. 1987, 742; Bittles 1980, 776; Reddy 1985). Bittles explains this as the outcome of processes which maintain deleterious recessives in the gene pool, namely recurrent mutation and heterozygote advantage (Bittles 1980, 775-776; 1991; see Hann 1991; Thornhill 1991, 285).

The net effect of inbreeding depression may be further reduced by reproductive compensation, which would be aided by decreased primary sterility and early foetal loss, both due to the greater genetic similarity of the parents and hence diminished maternal-foetal incompatibility; and by fecundity increases, observable as a reduction in birth interval (Bittles 1980, 774, 776).

In short, inbreeding at the level of $r \geqslant .25$, and particularly $r \geqslant .5$ (i.e. mating breeding with primary kin), is of increased risk to the offspring. However, the exact severity of the risk most probably varies in different societies (Bittles & Makov 1988, 164).

If there is a high level of risk associated with inbreeding, why did the Roman Egyptians carry on their practice of full sibling marriage? Hopkins suggested that given the high infant mortality rate that prevailed in Roman Egypt, the negative effects of inbreeding would pass unnoticed (Hopkins 1980, 325-327). Other factors like a possible low genetic load and reproductive compensation might have mitigated the effects of inbreeding depression. In addition, the marginal increase in r of incestuous couples as compared with r of the general population might have been lowered owing to a high mean coefficient of relationship in the highly inbred Roman Egyptian society.

It is not clear whether inbreeding at the level of $r=.25$ does result in reduced reproductive success. Katherine L Hann (1991) and A.H. Bittles (in Thornhill 1991) argued that studies in the Indian subcontinent (Bittles et al. 1990; Devi et al. 1981; Hann 1985; Rami Reddy & Papa Rao 1978; Rao & Inbaraj 1979; Reid 1976; Shami et al. 1990), in Nubian Egypt (Hussein 1971) and in French Canada (Philippe 1974) show it does not. They suggested that this is the result of greater gamete and maternal-foetal compatibility (Hann 1991, 270), reproductive compensation ascribable either to a conscious decision by parents to achieve their desired number of offspring or to the cessation of lactational amenorrhoea following the death of breast-fed infants (Bittles 1991, 265; Hann 1991, 270), the younger age at marriage and first livebirth in consanguineous marriages as a result of simpler necessary nuptial arrangements and the removal of bridewealth and dowry (Bittles 1991, 265). Thornhill doubted for "theoretical reasons" which she did not share with her readers that reproductive compensation could balance the effects of inbreeding depression, and promised to provide in a publication still in press a review of studies on the matter that shows that inbreeding between individuals whose $r=.25$ does result in reduced reproductive success (1991, 285). The effects of inbreeding at the level of $r=.5$ on fertility rates are yet to be studied statistically. If one assumes that in an early phylogenetic stage many hominids kept reproducing for the

duration of their lives (rather than reach a certain number of offspring and then stop), it is plausible that inbreeding depression did result in reduced reproductive success.

On the other hand, scenarios could be developed according to which inbreeding depression may contribute to reproductive success in some circumstances. For instance, it could be that in a particular society one of inbreeding depression's major effects would be to cause still births. In such a situation inbreeding depression may function to increase the time gap between the birth of successive viable offspring. Presumably this will allow the parents to invest more resources like food and shelter in each offspring when s/he is an infant. One can envisage circumstances under which this further investment would increase the probability of offspring survival and reproduction to such an extent, that the loss of offspring due to the increase in still births would be more than compensated for.

The conclusion must be, then, that even though there is evidence of inbreeding depression, its magnitude and putative effect on reproductive success probably vary greatly , and cannot be taken for granted. It seems premature to conclude with confidence that inbreeding depression exerted selective pressure against close inbreeding throughout hominid evolution, though it is plausible.

3.2. Close inbreeding avoidance in non-humans

Many anthropologists used to believe that incest prohibitions and its ensuing avoidance constitute the great distinctive feature that marks humanity off from the rest of the animal kingdom (e.g. Levi Strauss 1969, chs 1-2). The underlying assumption of this contention, namely that animals regularly practise "incest" (i.e. close inbreeding) is now recognised by anthropologists as wrong (e.g. Arens 1986, XI, 85 ff., 150 ff.; Leavitt 1990, 975-980; Westermarck 1921, 223-224). (Arens went as far as suggesting that it is the practice of incest rather than its avoidance that marks humanity off from the rest of the animal kingdom [1986, xi, 85 ff. 150 ff.].)

Some species, like termites and some species of wolves (Mech 1970; Dawkins 1989, 317-319) are known to inbreed closely. However both inbreeding depression and the tendency to avoid close inbreeding have been documented for many species (Westermarck 1921, 195-197; 218-224; Sausman 1984; Ralls et al 1979; Ralls et al 1980; Ralls et al 1988).

The situation among primates is of particular interest as they are phylogenetically most closely related to humans. Inbreeding depression has been empirically demonstrated in primates. In 15 of 16 captive colonies of various species of primates, inbred young suffered increased mortality rates in comparison with non-inbred young (Ralls & Ballou 1982). C. Packer noted a case of a male olive baboon who had through a set of unusual circumstances come to immigrate into a troop with females who were closely related to

him. 50% of the offspring assumed to be his (4/8) died before the age of one month, compared with a rate of about 16% which was common in that and the two other troops (1979, 8-9). This, however, is a single instance and can only be treated as anecdotal.

Close inbreeding in at least some non-human primates has been observed more rarely than would be expected under conditions of random mating in the population (Carpenter 1940; Sade 1968; Itani 1972; Enomoto 1974; Richard 1974; Gray 1985, 197-201; Jolly 1985, 240-242).

The most widely documented mechanisms that were found to reduce close inbreeding in primates (as well as in other non-human animals) were sex biased dispersal, whereby individuals of one gender emigrate from their natal troops, and avoidance of copulation in maternal sibling and in M-S pairs by one or both partners (Read & Harvey 1988, 119-122; cf Bateson 1983a, 264 ff.; Kortmulder 1974, 60-65; Westermarck 1921, 218; Keane 1990; Walters et al 1988; Holekamp 1984; for a sociobiologically informed review of non-human primate mating patterns see Quiatt 1988; Gray 1985, 195-201). There is, nonetheless, a controversy over the ultimate causes of the evolution of gender biased dispersal. Jim Moore and Rauf Ali pointed out that in addition to explaining gender biased dispersal as the result of selective pressures against inbreeding, such a dispersal can be accounted for as the result of intrasexual competition and territory choice. Their analysis was not confined to primates or mammals (Moore & Ali 1984; Moore 1984). The relevance of intrasexual competition and territory choice as such does not, of course, mean that the cost of inbreeding is irrelevant, and it could be that the patterns of dispersal of each individual species are the outcome of its particular evolutionary circumstances. Equally controversial are the proximate and ontogenetic mechanisms behind gender biased dispersal and sexual avoidance in maternal sibling and M-S pairs. These may differ in different species and in different circumstances (Gray 1985, 198-201).

The two species of primates which are most often analysed to help throw light on human natural history are the chimpanzees and savanna baboons. This is so because the former are genetically and therefore phylogenetically most closely related to humans, and the latter are presumed to share common elements in their ecology with early hominids (Reynolds 1980, ch 5). Observations of both species demonstrate that close inbreeding is rare.

Anne E. Pusey studied the behaviour of a chimpanzee community in Gombe National Park, Tanzania. Close inbreeding of types that could be recognised, namely maternal sibling and M-S mating were seldom observed. The mutual interest between females and the males in the company of which they were observed, usually their siblings, dropped dramatically when females commenced full oestrous cycle (Pusey 1980, 544-545, 546-547). By comparison, the two known brother pairs were still very close after both males had reached puberty which strongly suggests that the drop in B-Z relations is directly related to the partners' gender difference (Pusey 1980, 545).

Jane Goodall (1986) pointed out that in well over a decade of observation no pre-copulatory courtship was observed between mothers and sons or between maternal siblings. Such courtship behaviour normally precedes copulation among chimpanzees. She provided figures that demonstrate the infrequency of maternal sibling and M-S copulations among the chimpanzees in Gombe (cf Pusey 1980, 546-547). Table 3-2 (table 16.2 in Godall 1986, 466) presents the interaction between sexually mature sons and their oestrous mothers when both mothers and sons were seen together. It shows that incestuous matings between mothers and sons were extremely rare (Goodall 1986, 466-467).

Table 3-2: Frequency of Copulation between Mothers and Their Adult Sons as Observed in Gombe Park Chimpanzees

Mother	Son	Age of son (years)	No. of cycles seen together	No. of copulations	
				Seen	Attempted
Flo	Faben	16	2	0	0
		20	1	0	0
		22	2	0	0
	Figan	14	1	0	0
		16	2	0	0
Olly	Evered	15	4	0	0
Sprout	Satan	18	2	1[a]	0
Melissa	Goblin	19	2	1[a]	3
		20	2	1	1
Nope	Mustard	16	2	0	0

a. Mother protested violently, screamed, pulled away prior to ejaculation.

Table 3-3 (table 16.3 in Goodall 1986, 469) demonstrates that copulation between maternal siblings was rarer than would be expected under conditions of random matings (Goodall 1986, 467-470).

The situation in F-D pairs was not studied as paternity is usually unknown and cannot be established by observation alone (Goodall 1986, 470).

Females normally changed their ranging pattern within their natal group and were likely to emigrate from their natal community either permanently or temporarily, usually while in oestrus, thus reducing the probability of inbreeding (Pusey 1980, 545-546, 547-549; Goodall 1986, 86-92, 470-471). Pusey concluded that this migration was the result of the females' following unfamiliar males from other groups, males to which they were attracted. She justified her conclusion on the grounds that emigration appeared to be related to the females' being in oestrus; that females often returned to their natal group when anoestrous or pregnant; that females showed a preference for mating with less familiar males; and that females seemed to have chosen to emigrate without being forced to by other members of their troop (Pusey 1980, 548-549, 550).

Association between sons and mothers seemed to have dropped dramatically around the stage when the

Table 3-3: Relative Frequency of Observed Copulations Between Maternal Siblings versus Mean Number with Non-Related Reproductively Mature Males durnig Selected Periods

Male		Female		Observed female copulations	Mean no. per reproductively mature male	Copulations with sibling		
Name	Age (years)	Name	Age class			Number observed	Number resisted	Number resisted successfully
Figan	14	Fifi	Late adolescent	250[a]	14.0	1	0	0
	22		Between 1st and 2nd offspring	38	6.2	1	0	0
	27		Between 2nd and 3rd offspring	154	22.0	12	7	5
Faben	20	Fifi	Late adolescent	250[a]	14.0	2	0	0
	28		Between 1st and 2nd offspring	38	6.2	2	0	0
Pepe	16	Miff	Late adolescent	100[b]	6.0	1	0	0
Goblin	16	Gremlin	Late adolescent	128	18.3	26	4	3

a. Four months during 1967 and 1968 (random).
b. Three months during 1967 and 1968 (random).

sons reached sexual maturity, partly because they showed more interest in adult males and oestrous females at the expense of their relationships with their mothers. However, the few adult males for whom data were available associated more with their mothers when anoestrous than with other anoestrous females. In the four instances when sexually mature males were observed in the company of their mothers while oestrous, the sons were not observed to court, and rarely attempted to copulate with their sexually active mothers (Pusey 1980, 549; Goodall 1986, 466-467).

Pusey concluded that sexual activity is inhibited between maternal primary kin. She noted that the switch at sexual maturation to an attraction to less familiar males was observed in female baboons as well (Pusey 1980, 549-550; Jane Goodall's report agrees with Pusey's [1986, 86-92, 466-471] and see Nishida & Kawanaka 1972).

Packer reported on his nine year long observation of three adjacent troops of olive baboons (*Papio anubis*), which are savanna baboons, in Gombe National Park. All 48 males emigrated from their natal troop, usually after reaching sexual maturity. Except for two females, a mother and a daughter, all females remained in their natal troop (Packer 1979, 3). During inter-troop encounters, immigrant males acted in ways that reduced the contact between females of their new troop and outsiders. They kept females away from the new males which they fended off. This lowered the probability of newcomers immigrating or mating with the females in the troop (Packer 1979, 15-18). The immigration of males who took control over female sexuality reduced the probability of inbreeding. (There were seven cases in which males

emigrated from their new groups. In these instances later emigration had no effect on inbreeding prevention [Packer 1979, 7, 10-13].) Natal males showed interest in female members of other troops but little interest in females of their natal troop. In both immigrant and natal males, dominance was related to age. However, in natal males, as opposed to immigrant males, reproductive activity decreased with age prior to their emigration. Instead of engaging in aggressive competition for access to oestrous females, those natal males mated surreptitiously (Packer 1979, 18-24). Packer offered an evolutionary interpretation to the last observation, namely that males were avoiding the cost of competing for females which are not worth the risk because they offer limited reproductive potential owing to inbreeding depression (Packer 1979, 23). Female choice was also instrumental in reducing inbreeding. Females showed a preference (e.g. by presenting) for transferred males over natal males, and for males who were too young to have sired them over males that may have (Packer 1979, 24-27).

3.3. Conclusion

From the preceding discussion several conclusions may be drawn. First, since close inbreeding is avoided in species closely related to humans, it is very likely that hominids have avoided close inbreeding at early phylogenetic stages as well. Second, it is plausible that similar selective pressures, namely inbreeding depression, are behind incest avoidance in non-human primates and in humans. Third, it is possible that the proximate or ontogenetic factors behind incest avoidance in primates, including humans, are in part homologous.

As for inbreeding depression -- it is plausible that inbreeding depression has exerted selective pressure against close inbreeding, though more research is needed to test the possibility that inbreeding depression did not reduce reproductive success among those who closely inbred. Further research about the effect of inbreeding depression and genetic load in different populations will be very instrumental in quantifying the selective pressure against inbreeding, and helping focus the analyses of the phylogeny of human incest avoidance and of the avoidance of close inbreeding in other species.

Chapter 4

The Westermarck Effect

This chapter focuses on the ontogenetic process first proposed by Edward Westermarck. According to this proposal, proximity between two individuals of the opposite gender whilst either one or both are very young, will result in mutual sexual indifference after sexual maturation. Human sociobiologists claim this process has been selected for by natural selection in an evolutionary stage which van den Berghe dubbed pre-cultural (van den Berghe 1983, 93, 95-98; Parker 1976). I review the evidence and assess what is currently known about the process, and what further research is needed. I conclude that the knowledge we do have about the putative process is remarkably little, and a great deal of research lies ahead.

4.1. Fox's formulation of the effects of childhood proximity

At the turn of the century, a controversy flared over the explanation of individual incest avoidance. Edward Westermarck argued that an innate mechanism was behind incest avoidance: individuals who are in close contact when one or both are in early childhood do not develop a mutual sexual interest at sexual maturity. This, he argued, was the reason individuals normally avoid incest (Westermarck 1921, ch. XX). Freud and his followers insisted that individuals who live in close proximity do develop a sexual attraction that needs to be checked: it is social taboos and prohibitions that require individuals to repress their incestuous desires, this process resulting in a situation of incest avoidance.

There seems to be a slight confusion about Freud's position. If taboo is understood as social taboo, than his suggestion is in conflict with Westermarck's. If taboo is understood as a psychological taboo which may end up in repression of desires, Freud's and Westermarck's positions are in agreement with one another as in both cases the situation would be sexual indifference following close childhood association. Thus, Westermarck interpreted Freud and his colleagues' claims the former way (Westermarck 1921, 179 ff.), while Wolf interpreted these claims the latter way (Wolf 1966, 884-885). Presently I will take Freud's position to be the one which is contradictory to Westermarck's (On the debate around Freud's position see Spain 1987 [including comments by others]; Spain 1988a; Walter 1990, Rodrigues de Areia 1988, Spain 1988b).

While analysing the two theories in the context of sibling incest avoidance, Robin Fox combined these seemingly contradictory depictions into one unified model. He suggested that the two scholars were actually describing the two poles of one continuum. According to him, Westermarck described the outcome of unrestricted physical proximity at childhood. Freud discussed the opposite situation in which childhood proximity was heavily restricted by taboos, prohibitions on sex play and similar experiences. This restriction is the reason children did not develop a mutual sexual indifference in the situation described by Freud. What both Freud and Westermarck were in effect describing were the outcomes of two different childhood experiences. Fox argued that these two descriptions of the outcome of childhood proximity are not conflicting but complementary (Fox 1962; 1980, ch. 2).

Fox's model was designed to accommodate both theories and to account for cultural attitudes as well. The following version, table 4-1, is taken from his *Red Lamp of Incest* (Fox 1980, 27)

Table 4-1: Fox's Model -- 1980

Conditions during sexual immaturity	Resultant motivation	Associated sanctions
Intense physical interaction	Positive aversion	Lax
Physical separation plus propinquity	Strong desire	Fierce

(Following Fox I shall refer in what follows to the correlation between "intense physical interaction" and "positive aversion" as the Westermarck effect and to the correlation between "physical separation plus propinquity" with "strong desire" as the Freud effect). Fox's 1962 formulation of the paradigm had gone much further in attempting to account for actual behaviour and emotions. The table, table 4-2, goes under the same headings as the 1980 table (Fox 1962, 134).

What Fox called positive aversion I shall refer to, in line with Westermarck's terminology, as sexual indifference. By indifference I mean the situation in which individuals are not sexually attracted to one another, and if the possibility of sexual relations between the two were suggested to them, they would be averse to it. This is in total agreement with Westermarck's understanding (Westermarck 1921, 192, 198), and, I believe, is exactly what Fox meant by the term sexual aversion.

Table 4-2: Fox's Model -- 1962

Conditions during sexual immaturity	Resultant motivation	Associated sanctions
Intensive tactile interaction	'Positive aversion'	Lax (few occurrences)
Complete separation	'Strong desire' (non-guilty temptations)	Severe (external) (breaches where possible)
Separation and guilt inculcation	'Strong desire' (guilty temptation)	Self-punishment (internal)

Fox's 1962 formulation ushered in the era of modern biosocial analysis of human incest avoidance. Since his seminal article was published, most of the work on the Westermarck effect has focused on sibling incest avoidance and co-socialisation. The only exception is Parker and Parker's 1986 article. In this chapter I will review the evidence put forward to demonstrate the Westermarck effect. Given the studies' concentration on sibling incest avoidance, it is inevitable that the discussion below will concentrate on sibling incest avoidance too.

But first, do Fox's formulations not clash with the evidence from Roman Egypt? Obviously, the prediction that sibling incest will be shunned one way or another is not supported in this case. However, there is no evidence in Hopkins' data to disprove the putative relationship between unrestricted proximity and sexual indifference.

Hopkins provided no information on the practices of child rearing and the relationship between siblings of opposite genders in their early years of childhood.[9] One piece of supporting evidence that suggests that proximity might have been restricted between siblings who married has to do with their respective ages. According to the papyri the average age gap in ordinary first marriages between non-siblings was three years (Hopkins 1980, 333-334. Though not explicitly discussed in his article, I believe the average age gap was calculated on the basis of gaps regardless of direction, i.e. husband-wife wife-husband). However, it is likely that a rather large age gap between marriageable siblings existed at the time due to lactational

[9]Attempts to contact Hopkins and obtain the information have proven unsuccessful.

amenorrhoea, high infant mortality, and the chance of consecutive siblings being of the same gender. A considerable age gap might have meant that contact between the siblings was limited in the first few years of the younger sibling's life, a situation which may prevent unrestricted physical proximity between the two. The Roman Egyptian situation would not undermine the validity of the Westermarck effect, unless it were shown that the children were living in a situation of unrestricted physical proximity from infancy or early childhood.

4.2. Secondary ethnographic evidence

The first type of evidence advanced in support of Westermarck's hypothesis was secondary ethnographic evidence. By secondary ethnographic evidence I mean evidence extracted from ethnographies that were not intended to demonstrate the Westermarck effect and focused on other aspects of social life (For some anecdotal evidence from last century see Westermarck 1921, 194-195).

Fox collected data from various ethnographies in support of the relationships he postulated between conditions during sexual immaturity and the resultant motivation, and between the resultant motivation and the associated sanctions (Fox 1962, 136 ff.; Fox 1980, 29-49). The ethnographic data represents research in many different places. Fox mentioned the kibbutzim in Israel where co-socialised unrelated individuals seemed to be sexually uninterested in one another (Fox 1980, 29-32, 47-51; 1962, 136-137; Fox originally cited this instance before Shepher's studies which will be discussed in section 4.3.2 below). Also discussed were the American Chiricahua. They severely restricted physical proximity between boys and girls. In line with Fox's theory, incest evoked grisly horror among the Chiricahua. It was regarded as a crime of the worst kind (Fox 1980, 32-34; 1962, 138-139). The Tallensi of West Africa were cited too. Tallensi socialisation patterns resulted in unrestricted tactile proximity, including sex play, between Bs and Zs. They conceived of incest as rare and of its possible motivations as arcane. Perpetrators of incest were merely ridiculed, and suffered no other forms of punishment (Fox 1980, 34-36; 1962, 139-140). Also discussed were the Trobriand Islanders. Their socialisation patterns were characterised by the siblings' being in close daily contact, but living under strict rules of psychological separation (e.g. they would never exchange light remarks or look at one another). As expected B-Z relationships were reportedly charged with unfulfilled sexual desire and anxiety (Fox 1980, 36-39; 1962, 140-142). Fox further points out the Pondo of Southeast Africa. They not restrict physical proximity among siblings. As Fox's models would predict, they treated the occurrence of incest lightly (Fox 1980, 39-40; 1962, 142). The Mountain Arapesh of New Guinea were another case in point. They did not compromise childhood tactile proximity. They reportedly suffered no incest anxiety and considered incest to be merely stupid, not an abomination (Fox 1980, 40-43; 1962, 143-144). Also among the ethnographic examples which Fox cited were the Tikopia,

who allowed their offspring a great degree of freedom of interaction. They thought of incest as almost impossible, ascribing its rare perpetration to momentary sex passion that may besot a man (not a woman) (Fox 1980, 43-44; 1962, 144-146). Fox also mentioned the Taiwanese couples who underwent the minor marriage whereby they married after they had been reared together in childhood. Their conjugal sex life seemed troubled by lack of sexual attraction between the spouses (Fox 1980 45-47; this case will be discussed in section 4.3.1 below).

To these instances van den Berghe added four African cases which he summarised in the table 4-3 (table 15.1 in van den Berghe 1987, 357) quoted below

Westermarck hypothesised that sexual aversion between two individuals would result if at least one of them was in his/her early childhood when their close relationship commenced. This is Westermarck's explanation of the aversion between parents and their offspring. Robbins Burlind described a situation that agrees with Westermarck's hypothesis for F-D dyads.

In a retrospect of over thirty years, Burling reported that amongst the Garo, a people in India whom he had studied in the 1950s, men would occasionally come to be required to marry both a mother and her young daughter, who may still be an infant, as wives. According to Burling's recollections, when the younger wife was very young the relationships of a husband and his young wife typically took the form of a F-D relationship in which the father participates in the rearing of his young wife. This participation in rearing was characterised by a regular very close physical contact between husband and young wife, a contact which was typical of normal F-D relationships (Burling 1985, 130-131).

In line with what Westermarck might have predicted, Burling remembers that such marriages took longer to consummate (the term used by Burling) than other marriages (Burling 1985, 131).

The cases reviewed thus far are suggestive, but hardly conclusive in their own right. The data are impressionistic and were collected by ethnographers who were not particularly interested in examining the Westermarck effect, and who were probably not aware of the issue while in the field. However, the case for the Westermarck effect is greatly strengthened by some explicit studies of it, studies which provide quantitative data as well.

Table 4-3: Summary of Evidence on Sex and Incest Taboos in Four African Societies

Society and Location	*Attitudes to Sex and Child-Rearing Practices*	*Attitudes to Incest*
Tallensi (Northern Ghana)	Sex a "normal and natural appetite." Sex play tolerated in children. Few sexual prohibitions. Brother and sister are initimate childhood associates.	Incest is abnormal behavior, but neither a crime nor a sin. No penal sanctions against it. No special word for it.
Ashanti (Central Ghana)	Many strong sexual taboos with harsh punishments (e.g. death penalty for intercourse during menstruation). Harsh, repressive treatment of children past infancy. Brothers and sisters separated after weaning.	A heinous crime traditionally punishable by death or expulsion from clan (after British rule, by heavy fine and ostracism).
Ibo (South-Eastern Nigeria)	Many sexual taboos (e.g., on menstruation, post-partum). Emphasis on premarital virginity. Clitoridectomy. Brothers and sisters segregated after 5. Sex play repressed. Masturbation punished.	Incest an abomination. Contaminates the earth. Death penalty.
Zande (Southern Sudan and North-Eastern Zaire)	Sexually permissive. Incest between agnatic half-siblings permitted in nobility. Much adultery. Premarital sex. Open homosexuality. Bawdy sexual bantering. Brothers and sisters intimate until 6. No repression of sex play.	Permitted between half siblings and fathers and daughters in aristocracy. Among commoners, elicits "feelings of shame," but no strong sanctions against it.

4.3. Primary evidence

The studies which informed Fox's van den Berghe's and Burling's analyses and which were considered in the previous section did not focus on the Westermarck effect. The studies discussed in this section are all explicit attempts to explore and demonstrate the Westermarck effect.

4.3.1. Minor marriages in Taiwan

The first of the studies that explicitly sought to demonstrate the Westermarck effect is the work of Arthur P. Wolf and his assistants on the Chinese minor (as opposed to major) marriage. They described a situation in which childhood co-socialisation of future spouses leads to mutual sexual indifference once the two are married (Wolf 1966; 1968; 1970; Wolf & Huang 1980). The work included two distinct stages. First Wolf conducted field-work in two village communities in Taiwan in the 1960s. The insights he gained in that work informed the second stage of his analysis which combined a study of official marriage and birth records with information gained in the field. The second stage of the study provided statistical data that corroborated the conclusions Wolf reached in his the first stage.

The minor marriage was patrilocal. It commanded little prestige and had for centuries been considered by China's elite to be vulgar and inferior (hence the Chinese name *hsiao-hun* -- minor marriage) (Wolf 1968, 866-887). This form of marriage was widespread well into the first three decades of the century in Taiwan and in vast areas of Mainland China (Wolf 1966, 883; 1968, 864 ff.).

The minor marriage coexisted with the prestigious form of patrilocal marriage -- the major or grand marriage, and with three other uxorilocal marriage types (Wolf 1966, 883). The uxorilocal marriages ranked lowest of all conjugal arrangements. A man who married uxorilocally was stigmatised (Wolf 1966, 887).

The minor marriage was an arrangement in which a young girl, often still an infant, was introduced into the household of a young boy with the intention that the two should marry when they reached their mid to late teens. Until then they were brought up the same way they would have been had they been siblings (Wolf 1966, 883-884 Wolf & Huang 1980, ch 6).

This form of upbringing entailed remarkable physical proximity. The couple would spend most of their time together, sleep together up to the age of seven or eight, as well as eat, play, bathe, study and work in each other's company (Wolf 1966, 884). Wolf argued that in accordance with Westermarck's hypothesis, minor marriages are subject to difficulties resulting from the spouses' mutual sexual indifference (Wolf 1966, 889-890).

He found that the sibling-like relationship would dramatically change once the two spouses-to-be reached their early teens, when shyness between the two appeared and they tried to avoid one another as much as possible. This was totally spontaneous and not encouraged by society at large (Wolf 1966, 884). Wolf further argued that minor marriages relied on parents' power for their eventual conclusion. Thus, when economic circumstances enabled youngsters to be economically independent, there was a dramatic drop in the number of minor marriages being concluded, even though just as many minor marriages were being arranged. (Before 1910, 22 minor marriages had been arranged in the village community that Wolf studied. In four cases one of the future spouses died before the marriage could be finalised, in one the parents decided against the arrangements, and in one case the parents decided not to insist on the marriage. In the other sixteen the boys and girls married their designated spouses. In 1910-1930, with economic transformation which created a job market, there was a drop in the number of minor marriages that were indeed finalised. Out of the 19 arrangements two dissolved with the premature death of one of the parties; two were carried out, and 15 met with the refusal of the future spouses and were subsequently dropped.) (Wolf 1966, 885-887; 1970, 504-505; Wolf & Huang 1980, ch 14). Many old villagers who had married their *sim-pua* (the Chinese term for the girls involved in the minor marriage) before the change in the economic structure confessed to having been opposed to the arrangement but unable to do anything about it (Wolf 1966, 887).

Wolf collated information obtained from individuals who were well positioned in the gossip network about the extra-marital sexual liaisons of individuals in the village (Wolf 1966, 888). Minor marriage husbands were found to have engaged in more extra-marital sex than their counterparts who married uxorilocally or in the grand marriage (Wolf 1966, 889). In the village of Hsiachichou of 119 men, 25 were reputed to be frequenters of prostitutes. These include 10 of the 70 men who married in the grand way, 4 of 26 uxorilocally married men, and 11 out of the 23 who were married to a *sim-pua* (Wolf 1966, 889). In the town of Sanhsia, the five men who were known to live with mistresses and to visit their wives and children only during New Year festivals were all married to a *sim-pua* (Wolf 1970, 508).

For a woman to engage in extra-marital sex was a grave moral offence, hence Wolf concluded that his figures show that the incidence of female extra-marital practice is much lower than male (Wolf 1966, 889). However, it is possible that the scarcity of information on such affairs is due, at least in part, to the women's being more careful in concealing these affairs and not only to the women's tendency to shun them altogether as Wolf assumed. At any rate, 5 out of 121 married women were known to have engaged in extra-marital relationships. One was a former prostitute whose husband married uxorilocally, and the other 4 were all *sim-pua* (Wolf 1966, 889).

Wolf acknowledged the dubious reliability of gossip, and assured his readers that the information was carefully collated and verified (Wolf 1966, 888; Wolf & Huang 1980, 147). He also provided much harder data, namely statistics based on marriage registration.

In the village of Hsiachichou 13 men lived with mistresses after fulfilling what was considered their prime filial duty, that of producing offspring. They included 3 of 70 who married in the grand way, 2 of 26 who married uxorilocally, and 8 of 23 who married a *sim-pua*. Furthermore, among the 5 who did not marry in the minor way extra-marital relations lasted for less than four years, whereas the 8 who have married the minor way included at least 4 whose extra-marital relations lasted for ten years or more (Wolf 1966, 890).

Further support for the suggestion that sexual indifference is behind the marital problems associated with minor marriages comes from the way the villagers in Hsiachichou answered the question why they preferred not to marry in a minor marriage.

> "When asked why they dislike making marriages of the alternative type [minor marriages], people in Hsiachichou did not refer to the matter of the dowry or to the advantages of strong affinal ties. They said nothing about prestige or the comparatively uneventful nature of the wedding. Nor did anyone mention childhood antagonisms or any other reasons for a personal animosity. Instead, they acted embarrassed. When pressed, a few people said that they didn't like the idea of making such a marriage because it was *kian-siau*, 'shameful' or 'embarrassing.' Others admitted that they thought that such a marriage was *bou i-su*, 'uninteresting' or 'meaningless,' a phrase commonly used to describe a dull performance or a pointless conversation. By and large, however, questions about this subject elicited no answers at all, only long and uncomfortable silences. Most of the young women questioned giggled and blushed. What is significant about this is that the only other times I obtained such responses were when I attempted to interview people about their personal sexual habits" (Wolf 1966, 891; see Wolf & Huang 1980, 89-91).

Wolf took the embarrassed reactions of the villagers to demonstrate the sexual basis of the difficulties people have with the minor marriage, namely the working of the Westermarck effect. His impression received support from folk wisdom in the community he was studying.

Stories of difficulties in minor marriages abound in Taiwanese society, and tales of newly wed couples refusing to consummate the marriage and being forced to by their parents are very common. Similar stories concerning other forms of marriage do not occur (Wolf 1970, 508; Wolf & Huang 1980, 89).

Wolf pointed out aspects of folklore that underpin the misfortune that is considered to have befallen a person who was married in the minor marriage. Minor marriages and the problems in relations between those who enter these conjugal arrangements loom large in folksongs, proverbs and popular expressions (Wolf 1968, 866).

Wolf extended his analysis to include statistics from records of marriage registration. He presented data about divorce, adultery and birth rates which were consistent with a hypothesis of sexual aversion between the spouses. These were based on data from the household registration records that had been initiated by

the Japanese at the turn of the century and maintained ever since (Wolf 1970, 506). The data analysed were relevant to two districts in Sanhia Chen where Wolf was conducting his field-work. They covered the period 1900-1925, before birth rates, socio-economic conditions and incidence of minor marriages changed dramatically, making statistical comparisons of the various types of marriage impossible (Wolf 1970, 506-507). The sample was further limited to instances when the woman was married before the end of her 25th year to keep the ages of all types of marriages roughly comparable (Wolf 1970, 507). These data were corrected through the information collected from individuals who were well versed in gossip and the "ins and outs" of the town's social life (Wolf 1970, 510). Thus, children who were born as a result of extra-marital relations but were nonetheless registered as the offspring of the husband and wife could be allocated to their right category (Wolf 1970, 509 ff.; Wolf & Huang 1980, chs 11, 12, 13)

Wolf expected to find a higher divorce rate among minor marriages than among grand marriages and a lower birth rate (Wolf 1970, 506).

The results were as follows:

Table 4-4 (table 3 in Wolf 1970, 511) shows a much higher rate of divorce among people married in the minor way (Wolf 1970, 510-511).

Table 4-4: Number and Percent of Marriages Ending in Divorce or Separation

	Minor marriage	*Major marriage*
Total number of marriages	132	171
Number ending in divorce	25	1
Number of permanent separations	7	1
Percent ending in divorce or separation	24.2	1.2

Table 4-5 (table 4 in Wolf 1970, 512) is based on gossip and common knowledge (Wolf 1970, 511).

Since the 26 cases that ended in divorce include only 6 of 60 that were involved in adultery the two are poorly correlated: in other words, divorce rates cannot be explained by adultery rates or vice versa (Wolf 1970, 511).

Table 4-6 (table 5 in Wolf 1970, 512) combines the data from the two tables to show the great difference between the two forms of marriage.

Table 4-5: Number and Percent of Married Women Involved in Adultery

	Minor marriage	*Major marriage*
Total number of women	127	159
Number involved in adultery	42	18
Percent involved in adultery	33.1	11.3

* Five minor marriages and twelve major marriages were dropped for lack of information.

Table 4-6: Number and Percent of Marriages Ending in Divorce and/or Involving Adultery by Wife

	Minor marriage	*Major marriage*
Total number of marriages	132	171
Number involving divorce and/or adultery	61	18
Percent involving divorce and/or adultery	46.2	10.5

As for birth rates, table 4-7 (table 6 in Wolf 1970, 512) shows the figures according to the registration records.

Table 4-7: Average Number of Children as Taken from Household Registration Records

Years of marriage (in five-year intervals)	*Minor marriage*	*Major marriage*
1st	1.27	1.81
2nd	1.19	1.62
3rd	1.12	1.54
4th	1.06	1.23
5th	0.54	0.75

And table 4-8 (table 7 in Wolf 1970, 513) shows the data after correction (by reliance on collated gossip) for false reporting.

Again, minor marriages are much less fertile than grand marriages.

Table 4-8: Average Number of Children as Corrected by Informants

Years of marriage (in five-year intervals)	*Minor marriage*	*Major marriage*
1st	1.06	1.74
2nd	1.01	1.55
3rd	0.97	1.51
4th	0.94	1.21
5th	0.49	0.75

Wolf discussed possible alternative interpretations of his data, namely that adoption trauma or the up-bringing conditions of *sim-pua* lead to low fertility (Wolf 1970, 513). Wolf put these interpretations to the test by comparing the divorce and birth rates of minor marriages with those of women who were brought up as *sim-pua* but ended up marrying in a grand marriage a man other than the intended minor-marriage spouse (Wolf 1970, 513-515). His results are presented in tables 4-9, 4-10 and 4-11 (tables 8, 9 and 10 in Wolf 1970, 514).

Table 4-9: Number and Percent of Marriages by Adopted Daughters Ending in Divorce and/or Involving Adultery by Wife

	Minor marriage	*Major marriage*
Total number of marriages	132	42
Number ending in divorce	25	1
Number ending in adultery	42	4
Percent involving divorce and/or adultery	46.2	9.5

Table 4-10: Average Number of Children by Adopted daughters as Taken from Household Registration Records

Years of marriage (in five-year intervals)	*Minor marriage*	*Major marriage*
1st	1.27	1.78
2nd	1.19	1.77
3rd	1.12	1.76
4th	1.06	1.31
5th	0.54	0.90

Table 4-11: Average Number of Children by Adopted Daughters as Corrected by Informants

Years of marriage (in five-year intervals)	*Minor marriage*	*Major marriage*
1st	1.06	1.73
2nd	1.01	1.73
3rd	0.97	1.76
4th	0.94	1.31
5th	0.49	0.90

The differences between the minor and major marriages remain (Wolf 1970, 514).

Given all the difficulties associated with minor marriage, the question still remains why it was practised.

One possibility is that a poor family may seek to avoid the costs associated with a grand wedding by introducing a *sim-pua* into the family (Wolf 1968, 867). Wolf rejected the suggestion that this is a central element of a family's decision to undertake a minor marriage arrangement, because the economic advantages are very small; because the disgrace associated with the arrangement is strong; because the social disadvantages like the loss of affinal alliances substantial; and because the arrangement is made more than ten years before the marriage is to be finalised so that it cannot be a last resort (Wolf 1968, 867-869).

He argued that the persistence of the practice of minor marriage had to do with the familial political structure. In the grand marriage the new woman who is introduced into the family enters a power struggle with the mother-in-law. They compete over the young man's affection (Wolf 1968, 869-870). The daughter-in-law being brought up as a daughter would be expected to show more loyalty to the mother of the family and tension would be reduced (Wolf 1966, 888; Wolf 1968, 870; Wolf & Huang 1980, 83-86). In this context the sexual aversion he described was deemed by Wolf to be an added element that prevented the spouses from standing united against the mother, thus increasing her power (Wolf 1968, 870-872; Wolf & Huang 1980, 86-88, 93). Wolf did not argue that *sim-puas* necessarily develop deep emotional ties to their mothers-in-law. Rather, he argued that the parent-offspring relations and the roles they entail were extended to the *sim-pua* - mother-in-law relations. Young girls were often abused and mal-treated by their in-laws, but Wolf reported that they expressed bitterness towards their natal family, not towards their adoptive in-laws (Wolf 1968, 873; Wolf & Huang 1980, 88-89, 91-92).

> "Whatever her jural status in the family, a sim-pua is still her mother-in-law's daughter. For her to criticize her parents-in-law would constitute a breach of filial piety. More often treated like a servant than like a daughter, a sim-pua has no choice but to accept her status as a daughter and behave like a good daughter-in-law" (Wolf 1968, 871-872).

Hence "The real purpose of the minor form of marriage is not to save money but to preserve the family" (Wolf 1968, 872).

Wolf argued that

> "The only conclusion justified by the data presented... is that there is some aspect of childhood association sufficient to preclude or inhibit sexual desire. This suggests that the [incest] taboo is not a response to the needs of the social order, instituted to suppress private motives, but that it is instead an expression of these motives, a formal statement of the feelings of the community, socially unnecessary but psychologically inevitable" (Wolf 1970, 515).

Wolf rejected the possible claim that his findings are part of a broader picture of the younger generation's objection to excessive parental control, on the grounds that most young men who had refused to marry a *sim-pua* (9 out of 15) accepted an arranged grand marriage (Wolf 1966, 886-887). But this can be accounted for by other considerations, namely that the minor marriage is much less prestigious than other forms of marriage (Wolf 1966, 887); that young people did not want to miss their one chance to play a central role in an important ceremonial event (Wolf 1966, 887); that young men were reluctant to give up the material advantages of a grand marriage like dowry, which would not be received by marrying a *sim-pua* (Wolf 1966, 887-888); and that young men did not want to forgo the affinal alliances which are very loose if existent at all in the minor marriage (Wolf 1968, 866-867).

Whereas Wolf conceded that he could not disprove the role played by these alternative considerations, he insisted that they could not account satisfactorally for the reduced fertility rates and high rates of extramarital affairs associated exclusively with minor marriages (Wolf 1966, 888-889).

However, Wolf's analysis of the power structures in families points to another possible explanation of the sexual problems faced by spouses in the minor marriage system. The mal-treatment described by Wolf attests to hostility between mothers-in-law and *sim-pua*, be it explicit or implicit. The strong relations between mother and son may create a situation in which the son identifies, at least in part, with his mother and her negative attitude towards the *sim-pua*. The *sim-pua*, having been poorly treated for years by her mother-in-law, may identify her husband with his mother. This may well create a situation of marital stress that troubles minor marriages. In other words, the problems in minor marriages may be the outcome of the political structure of the family as such, and the fact of co-socialisation may be irrelevant.

In addition, the fact that no explicit rules ban sexual relations -- or as is the case here, the fact that sexual relations are effectively enjoined upon the couple -- does not necessarily mean that the sexual indifference was independent of cultural construction. Wolf made it clear that minor marriages bestow much less prestige to the couple. Further gossip and common knowledge hold that people who are married in the minor way have marital difficulties. Gossip and common knowledge can go beyond merely reporting

events and actually shape them. That is, gossip and common knowledge can act as a self fulfilling prophecy, and the individuals who know that they should have problems end up having problems. This begs the question of why the gossip is there in the first place, a question that cannot be answered as the circumstances of the origin of minor marriage are unknown. At any rate, even if one accepts that for whatever reason sexual indifference is caused by unrestricted proximity in childhood, the additional effect of cultural construction through gossip and common knowledge should not be ignored.

In his discussion of possible explanations for the phenomenon he described, Wolf pointed out the possibility that the two future spouses conceive of each other as siblings and are therefore loth to marry one another (Wolf 1966, 893).

> "This possibility was first suggested to me by the comments of a girl who had herself refused to marry a boy whose family she had joined as a child. When asked why she had refused, she answered, 'I just couldn't do it. It was too embarrassing. Imagine marrying your brother!' *The negative reaction to the alternative form of marriage* [the minor marriage] *may be the result of an inadvertent extension of the incest taboo rather than an example of the conditions that give rise to the taboo* [italics added]" (Wolf 1966, 893).

Wolf discarded this explanation on the grounds that the only two minor marriages that were finalised of the 19 that were arranged in 1910-1930, were between individuals who happened to have been separated in the process of their socialisation for a good number of years (Wolf 1966, 893-894). This he takes to mean that it is not the idea of marrying a person related as a quasi-sibling which is problematic. Rather it is co-socialisation as such which is relevant.

Whereas this may support the suggestion that childhood proximity effects sexual indifference, it does not rule out the possibility that treating one another as siblings causes the future spouses to develop sibling-like roles and expectations which include sexual avoidance, without this development of sibling-like roles having anything to do with proximity at childhood. The two couples who had been separated may be said not to have developed truly sibling-like relationships because their separation precluded the possibility of their developing sibling roles and expectations.

To sum up, Wolf showed quite convincingly that spouses who are co-socialised and subsequently marry in the minor marriages are significantly more likely to be sexually indifferent towards each other. Wolf's suggestion that this derives directly from co-socialisation as such has not been conclusively proven as alternative factors, namely, cultural constructions relevant to minor marriages, an inadvertent extension of sibling relations and familial power politics, may equally account for sexual indifference in minor marriages.

4.3.2. Co-socialised kibbutzniks in Israel

The second major ethnographic support for the Westermarck effect comes from the Kibbutzim in Israel.

The kibbutzim are collective settlements in Israel with a varying degree of ideological commitment to socialism. The first kibbutz was founded in 1910 (Shepher 1971, 294; 1983, 52).

In the more hard-line kibbutzim socialisation of children is carried out collectively with little emphasis placed on the nuclear family. In such settlements infants are introduced into the nursery at the very young age of several weeks. They spend most of their childhood and adolescence in the nursery with their peers. Only a little time is dedicated to contact with their nuclear families. The children go through their entire daily routine together. In the less hard-line kibbutzim sleeping arrangements are based on the nuclear family, but here too children would spend most of their waking time with their peers as of several weeks after they had been born (Shepher 1971, 294-295; 1983, 52). The children are divided into peer groups of roughly the same age. Normally, such groups are comprised of six to eight children of both genders. Through the years peer groups may be combined for educational purposes, but the original peer-group would remain intact throughout childhood and adolescence. Group solidarity is emphasised in kibbutz ideology and in the children's up-bringing (Shepher 1971, 294).

In the kibbutz movement as a whole, physical proximity in peer groups is normally unrestricted. Children are constantly exposed to their peers, except for daily periods of 1-4 hours which are spent in the company of the nuclear family. Sex play is not interfered with in any way, and occurs quite regularly. It begins at infancy, grows very intense at childhood, and drops in intensity during the first years of school. At the age of 9-10 sexual shame appears and the relations between the genders becomes tense. From the age of 13-14, tension is replaced by a warm, friendly and de-eroticised intimacy (Shepher 1971, 294; 1983, 60).

The fact that children who were brought up together in the same peer group do not marry nor do they normally form erotic relationships has attracted the attention of scholars for quite some time (see the review in Shepher 1983, 53-56 of the history of the social study of the kibbutzim in general, and particularly the study of the psychosexual dynamics of peer groups -- Spiro 1958, Rabin 1965, Bettelheim 1969, Talmon 1964).

Shepher's work was the first to provide quantitative data on sexual relations within peer-groups. All previous writers on the matter, with the exception of Talmon 1964, who analysed data from three kibbutzim, relied on impressionistic observations . Talmon's major focus, however, was marital arrangements rather than erotic relations (Shepher 1971, 295; Talmon 1964).

Shepher's study was based on field-work in a kibbutz he named Ya'ara in which children slept in the communal nursery, and on records from the kibbutz movement at large. In Ya'ara patterns of pre-marital sexual behaviour and marriages were studied. The data collected from the kibbutz movement records yielded information on marriage practices across the entire movement, with the exception of the religious kibbutzim and two unaffiliated kibbutzim that were not included in the analysis (Shepher 1971, 295).

Shepher explained the rationale of his analysis as follows:

> "1. If premarital sexual relations are similar to mate selection patterns in marriage in one kibbutz, and 2. If mate selection patterns in marriage are similar to those in all the kibbutzim, then 3. I can infer that premarital sexual relations in all the kibbutzim are similar to those investigated in one kibbutz, and 4. This would be the best possible inference under the existing conditions" (Shepher 1971, 295).

The assumption that only one form of premarital sexual practices can correlate with one form of marriage patterns is speculative and unsubstantiated. Having said that, the speculation is not unreasonable and allows an examination of data from the entire kibbutz movement is to be conducted. I therefore believe that this method should be accepted, at least tentatively. Of course, further studies that demonstrate that propositions 1 and 2 hold for other kibbutzim would increase the confidence in Shepher's conclusions.

Shepher interviewed and observed the behaviour of 65 adolescents and adults of the second generation in Ya'ara. All the educators who had ever been involved with the 65 were interviewed as well (Shepher 1971, 295-296).

The results are summarised in tables 4-12 and 4-13.

Table 4-12: Partners in Premarital Sexual Affairs

Subject/partner	Males	Females
Same peer group	**0**	**0**
Other peer groups	**1**	**1**
Educated in the kibbutz	**3**	**3**
Adults of the kibbutz	**2**	**7**
Adults of other kibbutzim	**4**	**4**
From outside the kibbutz	**12**	**4**
Total	**22**	**19**

[a] Twenty-two premarital sexual affairs of 30 native males and 19 affairs of 20 native females have been recorded.

Shepher found only a single instance of romantic attachment between two members of the same peer group, one involving a boy who only moved into the kibbutz at the age of 10 (Shepher 1971, 296). Otherwise, not a single case of heterosexual relations was recorded. Shepher stressed that the avoidance is not the result of pressure from either educators, parents and kibbutz authority on the one hand or peers on

Table 4-13: Partners in Marriages

Subject/partner	Males	Females
Same peer group	0	0
Other peer group	0	0
Educated in the kibbutz	0	1
Adults of the kibbutz	2	2
Adults of other kibbutzim	1	3
From outside the kibbutz	4	1
Total	7	7

* Seven marriages of 30 native males and seven marriages of 20 native females.

the other (Shepher 1971, 296). Thus, the one recorded affair "... was benevolently accepted by the peer group, the educators, and the parents, and when it was discontinued, everybody regretted it" (Shepher 1971, 296).

Shepher analysed the data of a complete census of the adult second generation in the three big kibbutz federations (a population which accounts for 97.5% of the second generation adults in the entire kibbutz movement) (Shepher 1971, 295; 1983, 56-57. Two of the federations have since merged). Of the 2769 marriages analysed only in five cases were the spouses in the same peer group, but never for more than two years while they were younger than six years of age, a rather unusual situation in a system in which most children remain in one peer group for the duration of their upbringing (Shepher 1971, 297-298; 1983, 57-60).

Based on his experience in Ya'ara, and following the rationale quoted above, Shepher concluded that in the large population of kibbutzim continuous co-socialisation in a critical period of before 6 years of age results in individuals not having sexual relations or marrying (Shepher 1971, 299).

This situation does not pass unnoticed by the kibbutzniks. The lack of mutual sexual attraction among members of the same peer group in a kibbutz is an accepted fact of life by both adolescents and adults (Shepher 1971, 296-297).

Consider the following dialogue which was quoted by Shepher:

> "D.14, male, 22 years old, comes from the army on leave. He wears a uniform; tall, beautiful youngster. He meets his classmate D.1, also 22. The girl is dressed beautifully for the festival dinner of the eve of the Shabbat. The two youngsters shake hands, giving signs of greatly enjoying the meeting. D.14 stares at D.1 and says, 'What a pity that we are classmates! We would make a beautiul couple!' D.1 answers, 'Why, that would be a blaze!' Both laugh heartily and go on their way" (Shepher 1971, 297).

Shepher noted that the realisation of this lack of sexual interest among peers had been perceived and discussed in the kibbutz movement ever since the 1940s when the second generation came of age (Shepher 1983, 52).

Shepher's study lends support to the Westermarck effect, but does it conclusively prove it? The fact that sexual relations are not banned or tabooed does not mean that the cultural constructions of sexuality and of peerhood do not entail sexual avoidance of the peers with whom one had been co-socialised. The mere fact that kibbutzniks are aware of the fact that membership in the same peer-group results in sexual indifference (see dialogue above) means that the role of peer, and hence the cultural construction of sexuality and of peerhood do entail sexual avoidance of one's peers. Shepher argued that this is a result of the Westermarck effect, not its cause. It would be hard put to prove it, though, because all one could point at is a correlation between the two.

Critics often cite Kaffman's article in an attempt to discredit Shepher's findings (e.g. Kitcher 1985; Leavitt 1990; 982). Kaffman claimed (but provided no figures) that sexual relations do occur between individuals who were brought up in the same peer group from infancy. However, he too considers such relations remarkably rare and short lived (Kaffman 1977, 215-216; cf Bixler 1981, 639-640; Shepher 1983, 61-62). The additional fact that no marriage between co-socialised had been found (Shepher 1983, 61-62), and the fact that the lack of romantic attachment between co-socialised is well known both to kibbutzniks and to social scientists make this reliance on Kaffman insufficient to discard the Westermarck effect.

A different objection that might be raised against Shepher's findings is that the phenomenon of rare sexual encounter is a non-issue. If the probability that an individual will be attracted to any other individual is represented by q, the probability that mutual attraction will occur is q+[2] which could be very low.

There are a few reasons to discard this objection. One is that a flat figure such as q is not a realistic expression of the probability that two individuals will be attracted to one another. Circumstances surrounding the interaction of any two individuals contribute a lot to the process of sexual attraction.

In addition, in the only other equivalent situation, that of Roman Egypt, it is estimated that a third of those who could marry their siblings apparently did. This is hardly the rate at which erotic association between the co-socialised in the kibbutz occurs. At any rate, the objection can be turned into a prediction. If an individual who is involved in a romantic relationship with his/her peers in the kibbutz, or who is involved in incest elsewhere simply has a high mean q, in other words, if for whatever reason, it is the probability that s/he will be sexually attracted to other individuals that make all the difference, we would expect these individuals to be involved in many more sexual liaisons than their counterparts who show a low level of q and avoid their peers or siblings. This could be tested quantitatively in a survey.

One could also argue that the peers are defined and define themselves as siblings according to the

standards of the broader society, thus creating a situation in which the incest taboo was extended through the model and notion of siblinghood. Wolf mentioned this possibility in relation to the minor marriage (see section 4.3.1 above). This is also the accepted wisdom in the kibbutz movement (Shepher 1983, 52). This suggestion runs into some difficulty, though. The mere fact of a couple being in the same peer group should be sufficient for the process of extension of the incest taboo; however, the number of instances which Shepher cited in which co-socialisation lasted for less than two years under six years of age and which resulted in marriage (Shepher 1983, 57-59) suggest that membership in the peer group is not in itself enough to de-eroticise the relationship (Shepher 1983, 59). It could, of course, be that the late-comers were treated as outsiders and not as "real" members of the peer group. Shepher does not provide these data.

Shepher's study, then, falls short of proving conclusively that independently of cultural construction childhood proximity causes adult sexual indifference. The possible role of cultural constructions and the extension of siblinghood in this instance remain unprobed.

4.3.3. Patrilineal Parallel cousin marriages in Lebanon

Justine McCabe undertook field-work in a Sunni village in the south of Lebanon in the mid-seventies (just before the outbreak of the civil war). The study she conducted was similar in its logic to Wolf's study of the Chinese minor marriage. She focused on patrilineal parallel cousin marriages. Such marriages are enjoined in vast segments of the population all over the Middle East, where it is the "proper" thing to do (McCabe 1983, 50-51, 57).

McCabe called the village she had studied *Bayt al'Asir*. Out of 92 households in the village 8 (8.7%) were made up of extended families, 2 (2.2%) of stem families, and the majority of households (78.3%) of nuclear families (McCabe 1983, 57-58).

The nuclear families belonging to a particular family tended to concentrate in a particular area and form extended family clusters of households. The clustering followed the patriline, and formed a picture of a patrilineal and patrilocal community (a very common situation throughout the Middle East). These clusters of nuclear families were very closely knit in *Bayt al'Asir* and were effectively large extended families. People moved freely in and out of their relatives' homes. The children and infants of the extended patrilineal family spent most of their waking time together. Housekeeping and child rearing were shared among the nuclear families belonging to one extended family. This created close childhood propinquity between offspring of male siblings (McCabe 1983, 58).

McCabe suggested that the way juvenile parallel patrilineal cousins treat one another is practically identical to the way siblings do (McCabe 1983, 58-59) and to that which Westermarck hypothesised would

lead to sexual indifference at the onset of puberty (McCabe 1983, 52 ff.). She summarised her analysis in the following table, table 4-14 (table 1 in McCabe 1983, 59).

Table 4-14: Comparison of Sister-Brother and FBD-FBS Relationships in *Bayt al'Asir*

Characteristics common to sister-brother relationships	Characteristics common to FBD-FBS relationship
1. Constant interaction from birth, including eating, sleeping, and performing other bodily functions in the same house.	1. Constant interaction from birth including eating, sleeping and performing other bodily functions in each other's homes, if these are not one and the same.
2. Intimate heterosexual exploration/play as babies/very young children.	2. Intimate heterosexual exploration/play as babies/very young children.
3. Little sisters are often caretakers of their (even slightly) younger brothers.	3. Little FBDs are often caretakers of their (even slightly) younger FBSs.
4. The childhood through adolescence relationship is affectively characterized by informality, candor, teasing, tattling, quarreling, laughing, joking.	4. The childhood through adolescence relationship is affectively characterized by informality, candor, teasing, tattling, quarreling, laughing, joking.
5. Brothers are physical guardians of their sisters.	5. FBSs are physical guardians of their FBDs.
6. Brothers are moral guardians of their sisters	6. FBSs are moral guardians of their FBDs.
7. Brothers' personal/family honor varies with sisters' virtue.	7. FBSs' personal/family honor varies with their FBDs' virtue.
8. A degree of respect/deference is accorded brothers by their (particularly younger) sisters.	8. A degree of respect/deference is accorded FBSs by their (particularly younger) FBDs.
9. Brothers and sisters close in age are often confidants.	9. FBSs and FBDs close in age are often confidants.

Following Wolf's studies, McCabe compared the fertility and divorce rates that occurred in patrilineal parallel cousin marriages with their equivalents in other marriages. She presented her results in the following tables 4-15 and 4-16 (tables 2 and 3 in McCabe 1983, 62).

She found that divorce rates were four times higher in patrilineal parallel cousin marriages, and 23% fewer offspring were produced in such marriages (McCabe 1983, 61-63). McCabe suggested that these findings are testimony to a reduced sexual attraction between the spouses. This, she argued, demonstrates the working of the Westermarck effect (McCabe 1983, 61-63).

Some objections to McCabe's conclusions have been raised. It should be noted that the numbers are not always statistically significant. Three cases of divorce are hardly a sound basis for analysis.

Peter C. Dodd and E. Terry Prathro (1985) criticised McCabe's study on the basis of its rather small sample. They provided evidence from studies of other communities in the Middle East and other statistical evidence that show results different the McCabe's (Dodd and Prathro 1985, 133-135). They also claimed that their evidence does not disprove Westermarck's hypothesis because their observations in Lebanese villages led them "... to believe, that the infant and childhood relations between parallel first cousins are often close but rarely intimate" (Dodd and Prathro 1985, 135).

Table 4-15: Number and Percent of All First Cousin and Other Marriages Ending in Divorce

	FBD	MBD	FSD	MSD	Distant relatives/ no relation	All non-FBD marriages
Total number of marriages[a]	23(19.65%)[b]	12(10.25%)[b]	7(5.98%)[b]	7(5.98%)[b]	68(58.11%)[b]	94(80.34%)[b]
Number ending in divorce	3	0	0	0	3	3
Percent ending in divorce	13.04	0	0	0	4.41	3.2

[a] Excluding "engaged" couples
[b] Percent of all marriages

Table 4-16: Average Number of Children By Type of Marriage

Years of marriage in five-year intervals	FBD (N = 22)	MBD (N = 12)	FSD (N = 7)	MSD (N = 7)	Distant relatives/ no relation (N = 67)	All non-FBD marriages (N = 93)
1st (1-5)	1.272 (28)	1.833 (22)	1.714 (12)	1.714 (12)	1.552 (104)	1.703 (150)
2nd (6-10)	1.150 (23)	1.166 (14)	1.142 (7)	1.500 (9)	1.464 (82)	1.318 (112)
3rd (11-15)	.947 (18)	1.727 (19)	1.600 (8)	1.600 (8)	1.378 (51)	1.576 (86)
4th (16-20)	.875 (14)	1.625 (13)	.800 (4)	.800 (4)	1.066 (32)	1.072 (53)
5th (21-25)	.700 (7)	.285 (2)	.750 (3)	.750 (3)	1.000 (23)	.696 (31)
Total fertility rate	4.944 (N = 90)	6.636 (N = 70)	6.006 (N = 34)	6.364 (N = 36)	6.460 (N = 292)	6.365 (N = 432)

[a] "Children" refers to all pregnancies, including live births, miscarriages, abortions, and stillbirths.

McCabe argued that the information that Dodd and Prathro had provided was obtained from urban communities in which childhood proximity could not take place since the residential patterns in these places were significantly different to those in *Bayt al'Asir* (McCabe 1985).

In addition, an alternative explanation of McCabe's findings could be that marriages other than the patrilineal parallel cousin marriages would tend to be based on personal preferences more often than patrilineal parallel cousin marriages because the former were not the prescribed form of marriage. The latter are *the* proper conjugal unions. This may lead to such marriages being undertaken with consideration other than the preferences of the bride or the groom in mind, such as prestige. McCabe did not indicate whether relationships between young men and women were such that romantic relationships and personal preferences could normally develop. Nor did she describe how arrangements were actually made, and who made them. Perhaps segregation of members of both genders from one another made such relationships impossible. Nor is it clear how strongly families insisted that patrilineal parallel cousin marriages should be conducted, or whether the older generation had the power or the authority to coerce the young into marriages they might not have wanted.

Another type of evidence which could support McCabe's work would have been an analysis of the age gap between the spouses of patrilineal parallel marriages. It would be expected that the age gap in the couples that were affected by the Westermarck effect would be rather small. Had it been substantial (for instance, ten years or more) sexual difficulties would not have been expected to arise because the spouses may not have been very close to each other in childhood. However, McCabe does not provide information such as ages of spouses.

One important difference between the kibbutz and minor marriage studies on the one hand, and the Lebanese on the other, is that in the former we are confronted with anecdotal evidence and stories from the Taiwanese and kibbutzniks about the effects of minor marriages and of peer-group co-socialisation respectively. The statistical analysis only demonstrates quantitatively what is known to the Taiwanese and kibbutzniks qualitatively (see above). If indeed patrilineal parallel cousin marriages are sexually troubled, one would expect the local people to be aware of this problem the same way the kibbutzniks and the Taiwanese are aware of the results of co-socialisation in their communities. I could find in the literature no mention of cultural conventions or beliefs in the Middle East to the effect that patrilineal parallel cousin marriages are particularly problematic. This should be a source of further concern about McCabe's conclusions, as the type of upbringing she describes is not radically different from other villages in the Middle East. It is a pity that McCabe does not present her readers with the way the villagers themselves explain the results of her statistical analyses.

It may still be that in *Bayt al'Asir* the Westermarck effect does take place. But the objections raised are strong enough to cast doubt on McCabe's analysis.

4.3.4. Father absence during daughter's infancy as a contributing factor to F-D incest in the USA

The only study explicitly seeking to demonstration of the working of the Westermarck effect on inter-generational incest was given by Hilda Parker and Seymour Parker. Their study compared a sample of 56 fathers (biological and non-biological) who were known to have sexually abused their minor daughters with a control group of 54 fathers (biological and non-biological too) who had no record of child sexual abuse (Parker & Parker 1986, 536). The control group was taken from the same penal or social-psychiatric facilities in similar proportions and age as the study group. The selection of members of both groups was restricted to individuals who had lived in the household of their abused daughter as partner of the mother for at least six months, contributed economically to the household, and performed a paternal caretaking role towards the daughter (Parker & Parker 1986, 536).

One of the factors that was examined was the time spent at home by the fathers when their daughters were three years old or younger (Parker & Parker 1986, 538-539).

Based on the following table 4-17 (table 3 in Parker & Parker 1986, 539),

Table 4-17: Time Spent in Home by Father During First 3 Years of Daughter's Life by Abuser Status of Respondent

Table 3

TIME SPENT IN HOME BY FATHER DURING FIRST 3 YEARS OF DAUGHTER'S LIFE BY ABUSER STATUS OF RESPONDENT

	ABUSERS		NONABUSERS	
TIME AT HOME	f	%	f	%
Almost all the time	19	34 (53)*	38	70 (75)*
Most of the time	4	7 (11)	9	16 (18)
Part of the time	7	12 (19)	1	2 (2)
Almost never	6	11 (17)	3	6 (5)
Not present in home	20	36	3	6
Total	56(36)	100(100)	54(51)	100(100)

$\chi^2 = 23.65$ (corrected), 4 *df*, $p < .001$
($\chi^2 = 18.42$ (corrected), 3 *df*, $p < .001$)*

* Percentages and summary statistics enclosed in parentheses are based on the subsamples of fathers who were present during the first three years of life of daughter.

Parker & Parker concluded that absence from home is a very significant factor in determining whether a father would be an abuser or not. This accords well with Westermarck's hypothesis (Parker & Parker 1986, 539).

Further, the degree of the fathers' involvement in child-care and nurturant activities during the first three

years of their daughters' lives seemed to distinguish sexually abusive from non-abusive fathers (Parker & Parker 1986, 539-540), as shown in the following table 4-18 (table 4 in Parker & Parker 1986, 540).

Table 4-18: Number of Child-Care and Nurturant Activities Performed Frequently by Father during First Three Years of Daughter's Life by Abuser Status of Respondent

	ABUSERS		NONABUSERS	
NUMBER OF ACTIVITIES	f	%	f	%
None	19	34 (53)*	12	22 (23)
One	9	16 (25)	12	22 (23)
Two	5	9 (14)	7	13 (14)
Three or more	3	5 (8)	20	37 (39)
Not present in home	20	36	3	6
Total	56(36)	100(100)	54(51)	100(100)
	$x^2 = 27.69$, 4 *df*, $p < .001$ ($x^2 = 12.96$, 3 *df*, $p < .01$)*			

* Percentages and summary statistics enclosed in parentheses are based on the subsamples of fathers who were present during the first three years of life of the daughter.

Even when present at home, sexually abusive fathers were much less frequently involved in caring and nurturant activities (Parker & Parker 1986, 540).

The risk associated with step-parenting is widely recognised in the literature on child sexual abuse (e.g. Gordon and Creighton 1988). In Parker & Parker's sample too, stepfathers did seem to be significantly more inclined to be involved in child sexual abuse (Parker & Parker 1986, 541). However, as table 4-19 (table 5 in Parker & Parker 1986, 542) shows

this can be explained by the factor of presence at home during the daughter's first years of life. Stepfathers who were present followed the behavioural patterns of biological fathers who were present. Stepfathers who were not, followed the patterns of biological fathers who were absent from home in the early years of their daughters lives. Stepfatherhood as such was not significant in Parker & Parker's results (Parker & Parker 1986, 541-542).

Parker & Parker performed multiple regression analyses and concluded that the explanatory power of the factor of time spent at home during daughters' early childhood was totally absorbed by the factor of father involvement in child-care and nurturance (Parker & Parker 1986, 543).

{{Table 7 Parker & Parker 1986, 544}}

Parker and Parker pointed out a correlation, not a causal relation. The possibility of one factor effecting the correlation should not be overlooked. One such factor could be the ability of the father to empathise. It

Table 4-19: Biological Status of Father Related to Abuser of Those who Were in the Home during the First Three Years of Daughter's Life

BIOLOGICAL STATUS	ABUSERS		NON-ABUSERS	
	f	%	f	%
Biological father	30	83	45	88
Nonbiological father	6	17	6	12
Total	36	100	51	100

$\chi^2 = .10$ (corrected, 1 *df*, $p = .70$)
Fisher's Exact Test (two tail), $p = .54$

could be that a person who can easily empathise with others will tend to partake in child-rearing and avoid child sexual abuse (the partaking in child-rearing and the avoidance of child sexual abuse being independent of one another) because he can appreciate the hardships of looking after the household, and can relate to the trauma his daughter might suffer as a result of his sexually abusing her. However if such a factor were effecting the correlation, we would expect that absent stepfathers would be significantly less represented among abusers than biological fathers, as stepfathers are not involved in child rearing because they were mostly not part of the nuclear family at the time the daughter was an infant. A third factor like inability to empathise is not the cause of their absence, and there is no reason to assume that such a factor would be more common among stepfathers than among the general population. In other words, since it is to be expected that the population of absent stepfathers would not be very different from the general population in the effect of a possible third factor, the pattern of abuse among absent stepfathers should resemble the patterns among the general population of biological fathers, absent or present, and not to the patterns of absent biological fathers. This is not the case in Parker & Parker's study (Parker & Parker 1986, 541-542). Nonetheless, this conclusion should be hedged because the number of stepfathers is by no means large (46% of the abusers and 17% of the non-abusers [Parker & Parker 1986, 541] which adds up to 26 among the abusers and 7 among non-abusers).

Parker and Parker's study lends support to Westermarck's hypothesis concerning the relations of fathers and daughters. Their conclusions should be treated with caution, though, on two grounds. One is the size of the sample which is not very large (56 abusers and 54 in the control group) and the fact that the results

are yet to be replicated. The second has to do with the definition of sexual abuse. Parker & Parker analysed a sample of convicted sexual abusers. Earlier I pointed out that studies of child sexual abuse are of little value for the study of incest as they include relatives other than primary kin and acts other than intercouse, and exclude consensual incest. In this case the study refers to fathers who have sexually abused their daughters, a situation which makes the first and third objection irrelevant. However, the difference between sexual intercouse and sexual abuse is still a problem particularly as Parker and Parker did not indicate the age of the victims when abuse took place. We therefore do not know whether that abuse took place when daughters were pubescent, in line with the formulation of the Westermarck effect, or not.

Not one of the studies just reviewed shows proximity in childhood to be the only possible factor explaining apparent sexual indifference at puberty. However, coupled with the qualitative data described before, the quantitative studies, Shepher's and Wolf's in particular, make it reasonable to believe that unrestricted proximity produces sexual indifference at puberty.

Clearly, more research is needed. I do not think that a study that will conclusively rule out the working of cultural construction is possible. This is because whenever a pattern of sexual indifference occurs it will most probably be realised by the members of the society in question and be subsequently mirrored in and incorporated into cultural constructions. This may have happened in the kibbutzim. The only possible conclusive examination of whether cultural construction preceded social practice or vice versa would be a historical one which would examine the development of social practices like peer group upbringing or minor marriages from their origins, and examine the parallel development of cultural constructions. This ideal study may not be possible in practice because data of cultural constructions and the origins of such social practices may not be sufficient.

However, if an absolutely conclusive proof of the independent effect of childhood proximity is unlikely the proposition that childhood proximity effects sexual indifference is falsifiable. If situations of unrestricted physical proximity as described by Shepher and Wolf occurred and were shown to be followed by no pattern of sexual indifference, Westermarck's hypothesis would be called into question. Until such evidence is provided, every further study that identifies a correlation between childhood proximity and sexual indifference at puberty will increase the confidence in the Westermarck effect.

With this in mind, I will proceed to discuss some analyses which assume the Westermarck effect has been demonstrated, and which attempt to explain its ontogenetic and proximate aspects.

4.4. The ontogeny of the Westermarck effect

In his 1971 article Shepher concluded that sexual avoidance in kibbutz peer groups is a case of negative sexual imprinting (Shepher 1971, 299 ff.): humans who are co-socialised in a situation of unrestricted physcial proximity for over two years in the first six years of their lives are being sexually negatively imprinted on one another (Shepher 1971, 300).

Shepher based his analysis on W.H. Thorpe's definition of imprinting which was published in the mid 1950s (Thorpe 1956; Shepher 1971, 300). For a classification of a behavioural process to be classified as imprinting according to Thorpe, four criteria needed to be met. The period during which the organism is imprinted needs to be a very definite and brief, the behavioural effects of imprinting need to be enduring, the effect of imprinting should be similar across the species, and a time gap should exists between imprinting and its behavioural consequences.

Classifying the Westermarck effect as imprinting according to these definitions tells us very little about the ontogenetic process which is involved. Since Thorpe published his work, the study of imprinting has developed and been refined by the study of sensitive periods (Bateson 1979; Bateson & Hinde 1987; Bornstein 1987; Bornstein 1989). In order to facilitate a comparative study of sensitive periods, and help understand the proximate mechanisms underlying them, Marc H. Bornstein (1989) devised a general framework for analysing such phenomena. In this section I will use Bornstein's framework to show how very little is known about the process/es dubbed here the Westermarck effect. That not much is known about the particular ontogenetic aspects of the Westermarck effect is not surprising, as there is a great limit on the types of data that could be obtained in ethnographic research. I suggest that this framework can be used as basis for developing and testing possible accounts for the ontogeny of the Westermarck effect. This may require more experimental studies as well as more elaborate statistical analyses of data.

Under the heading of structural characteristics Bornstein discussed 14 characteristics of sensitive periods. These are in turn divided into four sets.

One of these sets focuses on the temporal and intensive aspects of the sensitive period, another set focuses on the actual experience which operates to effect the change in the organism during the sensitive period, the third set includes the characteristics of the behavioural outcomes of the experience in the sensitive period, and the fourth includes both the extent to which variation occurs in the first three sets, and the extent to which the process is modifiable. I will now describe the actual characteristics involved, what they mean to incestuous behaviour, and what if anything can the studies of the Westermarck effect conducted thus far tell us about these characteristics.

The set of temporal and intensive aspects of the sensitive period includes five characteristics.

Developmental dating -- This is the question of how many sensitive periods occur in the life cycle of individuals and when they occur (Bornstein 1989, 181-182). Shepher would suggest that the sensitive period occurs in childhood, before puberty. If imprinting on the part of parents is considered part of the same phenomenon, sensitive periods would have to occur in further stages in the life cycle as well. Focusing for the moment on sibling incest avoidance, the question of whether only one sensitive period occurs in childhood, or whether there is a series of periods in which separate sensitive periods occur, cannot be answered by the data we have which was reviewed in the previous section. The same situation is true, of course, for parent-offspring dyads.

Onset and offset -- the rise and decay of sensitive periods (Bornstein 1989, 182). Shepher estimated that the sensitive period occurs between birth and six years of age, and not beyond the age of six. His estimation ignored those individuals who did not marry peers with whom they had been co-socialised for less than two years in the first six years of their lives, like those who had been introduced into the peer group at a late stage. The sensitive period in these individuals may have continued for many more years (Bateson 1983b, 102). In addition, there is the possibility that other factors confounded with late entry into the peer group and allowed the formation of erotic relations between the late entrants and their mates thus contravening the effects of the changes that do nonetheless occur during the sensitive period (e.g. newcomers being conceived of and perceived as outsiders). In other words, the different behavioural outcome in late entrants or other individuals who had arrived after six years of age in the kibbutz do not necessarily mean that the sensitive period is over by the age of six (Bateson 1983b, 102; Bateson 1979, 473). Perhaps characterising the sensitive period in terms of years since birth is misleading, and the sensitive period is better measured in terms of developmental processes: it could be that some other ontogenetic event affects the onset and offset of sensitive periods which happens with some children at six years of age, and with others at eight, ten or any other age. Further, none of the studies reviewed above gives any indication of whether the sensitive period is simply turned on and off, or whether the onset and offset are gradual processes.

Duration -- "the temporal window of susceptibility" (Bornstein 1989, 182-183). On the basis of the five cases of peers who did marry, Shepher argued that proximity of for a period of over two years is needed for sexual indifference to develop at puberty (see above). Again, one could ask if the situation is the same for all the individuals whom he studied. Perhaps the ten individuals (just over .3% of the sample of 2769) for whom less than two years of exposure was not enough were abnormal. One could also ask whether the two years of exposure need to be continuous, or could be broken up into several separate periods.

Asymptote -- sensitivity during the sensitive period (Bornstein 1989, 183). Shepher's and Wolf's data as well as Parker & Parker's are insufficient to allow conclusions as to whether sensitivity during the sensitive periods is best described as a plateau, a peak, twin peaks, or any other way. Perhaps there is a peak in the sensitive period of co-socialisation during which six months of exposure would be sufficient, but at an earlier or later stage two years would not be enough. Since the question of whether there are fluctuations cannot be answered, the temporal aspects of such possible fluctuations cannot be determined.

The studies reviewed earlier yield just as few conclusions about the consequences of the sensitive period.

Outcome -- "*The consequence of experience sustained during the sensitive period in terms of normative development of the species* [italics in the original]" (Bornstein 1989, 185-186). In the case at hand the observable behavioural outcome is sexual indifference which reduces the probability of close inbreeding taking place. However, the exact changes that occurred as a result of the experience at the sensitive period are unknown, and depend on the understanding of the proximate and ontogenetic mechanisms underlying the whole process. The studies discussed above do not help in identifying these mechanisms.

Manner -- *How the influence of experience during the sensitive period affects the outcome* [italics in the original]" (Bornstein 1989, 186). The experience may induce the outcome if the latter is not otherwise present. Alternatively, it may attune or maintain it (Bornstein 1989, 186). The studies reviewed give no indication as to whether an infantile lack of mature sexual attraction is maintained into adulthood (which is what Westermarck and Havelock Ellis hypothesised, Westermarck 1921, 193), or whether adult sexual attraction is inhibited when mature sexuality develops.

Outcome conditions -- "... when and under what circumstances in development the influence of experience manifests itself relative to the onset of the sensitive period and relative to the onset of the experience itself" (Bornstein 1989, 186). It is impossible to answer this question before the outcome is made clearer. The fact that the outcome is identified as a non-event as it were, namely the absence of sexual attraction, makes it even more difficult to approach this question.

Duration -- how long the outcome endures (Bornstein 1989, 186-187). Shepher and Wolf focused on the years immediately after puberty. Their data give no indication as to whether sexual indifference brought about by co-socialisation, wears off after long periods of time, like twenty years or more.

Two characteristics comprise the set which focuses on the phylogenetic and ontogenetic time scales.

Variability -- refers to the range of the characteristics described so far within and among species (Bornstein 1989, 187). The lack of series of systematic studies of the Westermarck effect precludes any possibility of answering this question or the next one, that of

Modifiability -- the degree to which the above described characteristics may be altered either during the sensitive period or subsequently (Bornstein 1989, 187-188).

Many of these characteristics cannot be properly described because of the limited scope of permissible experimental manipulation of human subjects. Some characteristics can be better studied in future studies. My point in describing the lack of data about the Westermarck effect is simply to illustrate how premature speculation concerning the exact mechanisms of the Westermarck effect are, and to point out what should be at the back of the minds of those who undertake to study incest avoidance further.

This leads me to the set which focuses on the mechanisms involved in the change that occurs during the sensitive period, i.e. the proximate and ontogenetic factors behind the sensitive period.

Experience -- the nature of the effective stimulus event (Bornstein 1989, 183-184). Ever since Fox's 1962 article, people studying the Westermarck effect have seen unrestricted physical proximity as the effective stimulus event. However, the question of what in the situation of such proximity is relevant is impossible to answer. Further, one cannot discard the possibility that at different stages during the sensitive period different aspects of the proximity are important, or that at one stage alternative experiences are effective, or that at any particular stage more than one aspect of the situation of unrestricted physical proximity are necessary. The two years which Shepher stated as the minimum duration of experience necessary may, perhaps, be shortened if various aspects of unrestricted physical proximity, e.g. sex play, were intensified. Perhaps the whole duration of the sensitive period could be altered if the intensity of aspects of such a proximity were modified.

System -- the change that occurs to enable a sensitive period to arise. Studies so far allow no insight into what happens at what level (Bornstein 1989, 184-185). Is the hypothesised imprinting happening at a neurological level (e.g. changes to the brain as a result of the experience), a physiological level or a psychological level (e.g. repression of desires into the subconsciousness)? If some form of arousal is part of the effective stimulation, does "imprinting" occur at the physiological level of hormonal secretion, at a psychological level through some memory of the arousal and so forth? Perhaps more than one level in an individual is affected.

Pathway -- The means through which the system is affected (e.g. which sensory system). The distinction between pathway and system is very subtle (Bornstein 1989, 185). At any rate, neither aspect can be gauged from the ethnographic studies of the Westermarck effect.

It strikes me that the only possible conclusion to be drawn at this stage is that the Westermarck effect involves a sensitive period which probably does occur. However not enough data exist to draw further

conclusions about the nature of this sensitive period. More research is needed into the phenomenon with the view of devising a profile of the sensitive period according to Bornstein's framework.

Notwithstanding the apparent lack of data and little understanding of the nature of the Westermarck effect, scholars have attempted to speculate about the nature of the proximate mechanisms involved. Even the most plausible and sensible of those speculations need to be treated with caution as what they attempt to explain is not more than a likely mechanism which only probably exists, and which is very little understood. There is no way yet of determining whether any one or perhaps some combination of these elements is involved. (Bateson suggested that in quails, two processes are involved, namely imprinting and habituation (1983b). Intuitively, it is hard to imagine that the process in humans is any less complex.)

Several possible mechanisms were discussed in the literature. In 1971 Shepher speculated, following Westermarck himself, that the mechanism involved was neuro-psychological: models of relationships implanted in the nervous system in some pre-sexual ontogenetic stage (Shepher 1971, 302-303; Westermarck 1921, 193). A similar suggestion which did not confine itself to the neurological level was that co-socialisation and the relationship between kin inhibits possible relationships of male dominance and masculine agonistic behaviour. The scholars who advanced this suggestion believed this dominance to be an important contributing factor to the occurrence of sexual relations (Abernathy 1974, 813-815; Kortmulder 1974; Frances & Frances 1976, 237-238, 243; Parker 1976, 292-295; Erickson 1989). In 1983 Shepher seemed to suggest that an actual ontogenetic process of imprinting (as opposed to a mere characterisation of another process as following some structural characteristics dubbed imprinting) was responsible for the Westermarck effect (Shepher 1983, 61). Fox argued that negative conditioning occurred through the frustration caused by the children's physiological inability to consummate sex-play (Fox 1980, 22-25). This, however, is not a likely explanation because sex play did not seem to end in frustration in the examples he described (e.g. Fox 1980, 39, 41) or in the kibbutzim that Shepher studied (Shepher 1983, 61). In addition, there is evidence that infants quite normally experience orgasm even when they do not consummate their sex-play (Hyde 1986, 295-296). Wolf suggested that negative conditioning occurred through pains of socialisation that are associated with the presence of the co-socialised individual (Wolf 1966, 892-897), but Shepher argued that socialisation in the kibbutzim could not be characterised as painful, and therefore Wolf's suggestion could not account for the Westermarck effect (Shepher 1983, 60-61). Some suggested that a process of stimulus satiation was involved in the process (Parker 1976, 294-295; Parker 1984; Parker & Parker 1986, 545; Demarest 1976, 334-337).

4.5. Conclusion

In the last few sections I argued that there is no conclusive proof that unrestricted physical proximity at childhood leads independently of cultural construction and other factors, to sexual indifference at puberty. I doubt that conclusive proof is possible, because a situation in which cultural construction would be oblivious to or conflict with prevailing behavioural tendencies is unlikely. Perhaps a historical study of the origins and development of peer group socialisation in the kibbutzim, of minor marriage in Taiwan or of any other situation in which the Westermarck effect may be demonstrated, might actually show decisively whether cultural construction preceded social practice or vice versa. Nevertheless, even without such a historical study, when taking an overview of the ethnographic data one may conclude that it makes sense to assume that the Westermarck effect does take place.

Assuming for the moment that the Westermarck effect does operate, the exact nature of the way co-socialisation effects sexual indifference is still a mystery.

More studies of the Westermarck effect are needed for various reasons. Foremost is the need to increase the credibility of the suggestion that it exists. The more evidence mustered from as many societies as possible will increase this credibility. This could be done through identifying social settings in which the effects of childhood proximity on pubescent erotic relationships can be assessed, and the Westermarck effect tested. Further, careful observational studies of the processes involved in the Westermarck effect are needed to isolate the factors that are involved in the sensitive period and the effect of the experience during this period. This is a necessary step towards understanding the proximate and ontogenetic factors behind the Westermarck effect. Once better understood, the proximate and ontogenetic factors could help describe the evolution of the Westermarck effect in humans' natural history.

Researchers have reported that sexual relations between co-socialised apes or between apes and their mothers are very rare, much rarer than would be expected had they been ordinary troop members (see section 3.2 above). This suggests that the Westermarck effect may take place in non-human primates as well. If this is so, it may be that the proximate and ontogenetic processes in humans and apes are, at least in part, homologous. Further research on non-human primates may help direct research on humans, particularly as experimental manipulations of childhood experience are impossible with humans, but to an extent acceptable in primates.

To prove conclusively that natural selection is, or was, effective in shaping or maintaining the Westermarck effect, it will need to be established that under the circumstances in which our ancestors presumably acquired the Westermarck effect, when unrestricted physical proximity did not result in sexual

indifference, an individual's genetic fitness was reduced (e.g. through reduced reproductive success). It will also need to be shown that the proximate cause of the Westermarck effect is indeed genetically biased. It is unrealistic to expect this kind of proof. But the more data about the Westermarck effect that is gathered, the more educated the guess about its natural history will become.

Chapter 5

Strategies of Incestuous Behaviour

The analytical level of behavioural strategies is far removed from specific proximate causes of behaviour. This distance makes the identification of the factors that shape these strategies, of the history of their development and of the part played in it by natural selection a very difficult task. Some scholars have tried to apply sociobiological logic to the question. Below I will present their case. First I will summarise the argumentation. I will reserve my own comments on the argumentation to the latter parts of this chapter.

5.1. Hypothesis and rationale

Based on the contention that close inbreeding ($r \geq .25$) is mal-adaptive and on the strong possibility that the Westermarck effect was shaped by natural selection, it is tempting to try to analyse the extent to which natural selection has affected the cross-cultural, etic[10] tendency to avoid incest.

The near universality of the tendency to avoid close inbreeding among humans ($r \geq .25$) may be argued to add strength to the suggestion that incest avoidance is strongly influenced by natural selection, and that it is a basic characteristic of the human species.

Shepher and van den Berghe suggested that the proximate mechanisms behind human incest avoidance include negative sexual imprinting among the co-socialised (see above chapter 4), negative sexual imprinting in the younger partner in intergenerational incest (van den Berghe 1983, 95-97), external physical prevention owing to demographic causes (Shepher 1983, 109-121[11]), and the prohibition of incest (Shepher 1983, 109-121).

One possible approach towards tackling the question of the role of natural selection could be to identify

[10]Etic and emic are two terms which are often contrasted with one another. Etic, by analogy with phon*etic*, refers to analyses conducted in a conceptual framework defined by the observer. Emic, by analogy with phon*emic* refers to analyses conducted within a conceptual framework of native people/s (see Brown 1991).

[11]Shepher's analysis was based on Mariam Kreiselman Slater's 1959 article (Shepher 1983, 111). Slater's argument (1959; the originator of the theory that demographic conditions would have prevented the possibility of incest was Wilson D. Wallis, 1950) was analysed by Ruth C. Busch and James Gundlach who have shown it to be faulty on methodological grounds. The calculations performed by Slater were based on the assumption that all members of society live the average life span. This crude model was unrealistic and based on wrong assumptions, and when a more sophisticated model was used, Busch and Gundlach showed Slater's calculations to be invalid (Busch & Gundlach 1977).

proximate (ie causal) mechanisms that contribute to incestuous strategies and then proceed to analyse them for their adaptive value and possible natural history. This strategy was adopted in relation to the study of the Westermarck effect that was reviewed in the previous chapter.

However, attempts to identify the role of natural selection in specific proximate causes of incestuous strategies seem frustrated by the limited understanding of such causes.

Shepher, van den Berghe and Welham, some of whose contributions will be discussed here, have attempted to overcome such difficulties by setting the question of proximate mechanisms aside, and showing that human incestuous behaviour, that is, the patterns of practice and avoidance of incest, conform to predictions that assume that individual humans act so as to maximise their genetic fitness. These authors felt that if they were successful, the possibility that natural selection has shaped incestuous strategies would be very strongly supported (e.g. Shepher 1983, 85-86).

The attempts to demonstrate the role of natural selection in shaping the incestous strategies have focused on the patterns of the practice of incest, rather than its avoidance.

5.2. Reproductive strategies and incestuous behaviour

One line of argument stemmed from the theory of parental investment. Parental investment theory states that male and female reproductive strategies differ (Trivers 1972; Wilson 1980, 162-164). When a parent invests in an offspring, his/her ability to invest in another offspring is reduced. In humans (and in most other mammals too) it is said that the normal situation is that mothers invest more in their offspring than fathers do. Their investment includes a long period of gestation, a period of lactation and so forth. In addition, the production of gametes differs between the genders in both number and size: females produce much fewer and bigger gametes. This makes female investment in pregnancy higher than male investment. Therefore, women are said to be more committed to ensuring that their investment choice is good (For a sociobiological exposition see Trivers 1972; for an explicit biosocial statement in the context of incest see van den Berghe 1983, 97, and Shepher 1983, 92 ff.).

Shepher, van den Berghe, and their colleagues argued that these unequal patterns of parental investment lead to a situation whereby female sexual promiscuity, which includes incest, is more heavily selected against than male sexual promiscuity and incest. This is so because female investment in offspring is greater, which means that female losses from bad investments would be more significant. Hence they argued that human females select their sexual partners very carefully and are sexually much less promiscuous and resist incest more strongly than do their male counterparts. Following from this is the conclusion that in the dyads in which females dominate and control the occurrence of incest, incest would

be rarer than in those in which males dominate. Thus, M-S incest should be extremely rare, F-D incest most commonly practised, while sibling incest is somewhere in between (van den Berghe 1983, 97; Shepher 1983, 94, 125-128; Dawkins 1989, 163-164).

Welham added the aspect of paternity confidence to the possible factors which may encourage fathers to initiate incestuous relations with their daughters. He pointed out that paternal investment is costly too. A man whose mate's offspring are not his own is faced with three alternatives: he can abandon his mate; he can practise infanticide; or he can mate with the unrelated female offspring of his mate's thus recovering as much benefit as possible from his investment. Hence Welham predicted that the incidence of F-D incest is inversely related to the degree of paternity confidence, and that the risk of child sexual abuse (which he does not clearly distinguish from incest and reproduction) is invariably greater from paternal male relatives than from their maternal counterparts (Welham 1990, 97, 99, 102 ff.).

Welham used this explanation to account for another phenomenon, namely girls' age at the onset of incestuous relations. He cited several studies that show that the physiological development of a young girl, menarche, and the development of secondary sexual characters, for instance the development of breasts, are the instigatory factors in most cases of F-D incest in the "West" (See figure 5-1 [figure 1 in Welham 1990, 101]. Welham relies on Justice & Justice 1979, Meiselman 1978, Maisch 1972) (Welham 1990, 108).

Figure 5-1: Age of Females at Onset of Incest

FIGURE 1. A. Distribution of the onset of incest (see text for definition) by male perpetrators with respect to age of the female victim (N = 70; data from Maisch [1972]). B. Rate (number of reported cases per 100 children) of all forms of female sexual abuse by age of onset (data from Finkelhor [1986], Table 2).

According to Welham this is because the father strives to minimise the costs of incest to himself. When

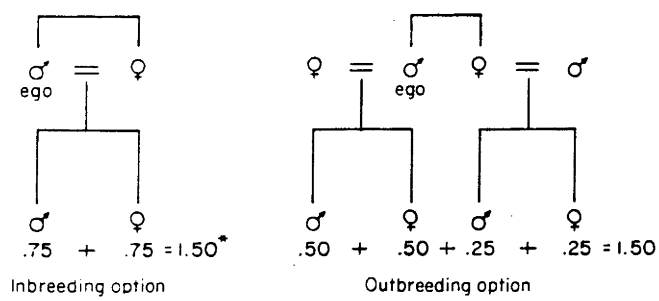
the female is too young to conceive it would be a total waste of resources. Waiting too long might result in the daughter's mating with someone else. This is the reason timing is crucial (Welham 1990, 108).

Shepher invoked the theory of inclusive fitness originally developed by W.D. Hamilton (Hamilton 1964) to reach similar conclusions concerning the rarity of incest, and more importantly, the relative rates of incest in the various dyads. He constructed models of the various incestuous situations and measured the share of ones genes which will be passed on to future generations if one adopted any of various strategies of incest. I will present his analysis here.

Shepher decided, for the sake of simplicity, to fix the number of offspring of every female at two (Shepher 1983, 94). He then described and analysed each possible dyad.

The first instance he tackled was B-Z incest in a monogamous situation shown in figure 5-1 (figure 7.1 in Shepher 1983, 95).

Figure 5-1: Brother-Sister Incest Compared with Outbreeding Option: Monogamic [sic] Situation



Shepher concluded that there are no gains to be made by either brother or sister from such incestuous relations. Further, since the brother can have more offspring from polygyny, the sister would lose more than her brother if the latter had the choice of mating with other females and did not (Shepher 1983, 94).

In a polygynous situation, shown in figures 5-2, and 5-3 (figures 7.2 and 7.3 in Shepher 1983, 95, 96), there are some gains to be made from incest if the female has no other option for mating.

If siblings can mate with others, neither one will gain further genetic representation. Further, Shepher pointed out that males would increase their inclusive fitness by .5 more than their sisters.

Also in polyandry, which is represented in figure 5-4 (figure 7.4 in Shepher 1983, 96), the only situation which would encourage incest would be if the brother cannot outbreed.

Figure 5-2: Brother-Sister Incest: Polyginic [sic] Situations, Male Ego

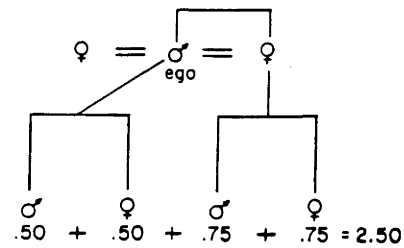


Figure 5-3: Brother-Sister Incest: Poliginic [sic] Situation, Female Ego

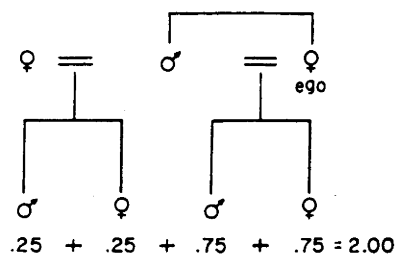
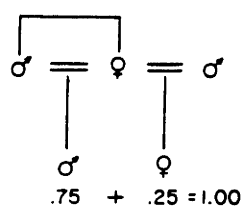


Figure 5-4: Brother-Sister Incest: Polyandric Situation, Male Ego



For the male this represents a loss compared with outbreeding. The female loses .25 relative to a situation in which the male would be outbreeding, but gains .25 in comparison with a situation in which the male is not breeding at all.

Shepher summarised his findings in the following table, table 5-2 (table 7.3 in Shepher 1983, 100).

to which he added the following note

Table 5-2: Comparison between Brother-Sister Incest and Outbreeding

	Situation 1 (monogamic)	Situation 2 (polygynic)		Situation 3 (polyandric)	
	Male and female	Male	Female	Male	Female
Incest	1.50	2.50	2	1	1.25
Normal	1.50	1.50	1.50	1.50	1.50
Balance	0	+1	+.50	−.50	−.25

Note: The assumption in each case is that a female has two offspring. In Situation 2, female has no outbreeding option; in Situation 3, male has no outbreeding option.

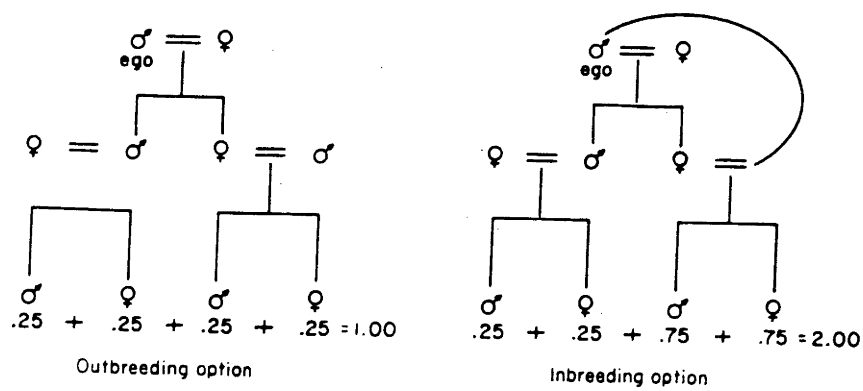
"Outbreeding can be polygynous or polyandrous; the difference is not especially important. Thus in a polygynous society, the male would normally have $2n$ •.50 [genes] from his wives +.50 from his sister. If his sister cannot mate, and he therefore mates with her and his own wives, his gain will be 1.00. Conversely, in a polyandrous situation, a male would normally have .75 (one of his own; one of his brother; polyandry is usually adelphic) and .50 from his sister's polyandric marriage, which equals 1.25. He still loses" (Shepher 1983, 95).

I shall return to this note later.

Shepher summarised his models so far by stating that gains are to be made only when the sister cannot mate non-incestuously, and even then, brothers gain more. Hence sibling incest would be restricted to exceptional and rare circumstances, and more strongly avoided by females (Shepher 1983, 95).

In F-D incestuous situations both gain as much as they would under condition of non-incestuous mating in terms of genetic representation in future generations. The assessment of genetic gains are applicable here to both father and daughter and presented in figure 5-5 (figure 7.5 in Shepher 1983, 96).

Figure 5-5: Father-Daughter Incest Compared with Outbreeding Option

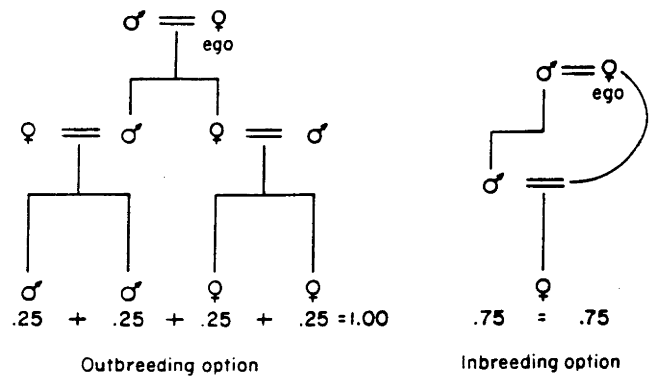


Therefore F-D incest will meet less objection than other forms of incest. Since the father has already

produced his own offspring, and since the daughter will have to invest in their common offspring more than he, she will still have more to lose than he does. Hence, the daughter would tend to resist incest more than her father. If the father can find an additional mate to his original wife, he will not gain by preferring to mate with his daughter (Shepher 1983, 95-97).

Figure 5-6 (figure 7.6 in Shepher 1983, 97) shows the consequences of M-S incest.

Figure 5-6: Mother-Son Incest Compared with Outbreeding Option



The mother is in a polyandrous situation. The son will only be able to gain one child, and no nephews or nieces (Shepher 1983, 97).

Shepher concluded that his analysis demonstrates that on genetic grounds M-S incest should be rarest, F-D most common, and sibling incest intermediate (Shepher 1983, 97).

Shepher's analysis shows that when properly calculated, one's inclusive fitness will not normally be increased by inbreeding and passing on more genes to one's offspring. This is so because of the gains in inclusive fitness through passing on more genes to one's offspring is balanced out by the loss of inclusive fitness through not passing on genes to ones nieces and nephews (see Grafen 1982 for the general point on the calculation of inclusive fitness).

The application of sociobiological reasoning to incest avoidance thus brings together different empirical data, namely, differential rates of the practice of incest based on the gender of the dominant partner (assuming that that partner usually is the F in F-D incest, and the M in M-S incest) and the age of girls in common cases of F-D incest, inbreeding depression, close inbreeding avoidance among non-human mammals, and the near universality of the tendency to avoid incest.

5.3. Predictions made by Shepher, van den Berghe and Welham

In order to demonstrate the power of their paradigm, Shepher, van den Berghe and Welham make predictions based on their analyses. They aim to show that their theories are falsifiable and agree with the empirical reality (e.g. Shepher 1983, 125; Welham 1990, 108-109).

Based on his analysis, Shepher both retrodicted and predicted that further research will continue to point at a differential frequency in the various incestuous dyads, namely that F-D incest is most common, B-Z intermediate, M-S rarest. He also predicted that females will be found to resist incest more strongly than males do, and that in extreme situations (e.g. isolation), incest will be more likely to occur. In addition, since the proximate mechanisms Shepher proposed for incest avoidance depend on statistically prevalent social situation (e.g. Westermarck effect's dependence on unrestricted physical proximity in childhood [see chapter 4 above]), Shepher predicted that when these situations do not occur, inhibition will not follow, and the likelihood of incest will increase (Shepher 1983, 128-130).

Virilocal residence causes female dispersal, and hence offers some protection from incest to the females. Van den Berghe predicted a higher incidence of F-D and B-Z incest in uxorilocal societies than in neolocal or virilocal societies (van den Berghe 1983, 99).

It is not clear why there should be differences in the rates of B-Z incest in the virilocal and uxorilocal systems, as the emigration of males or females should equally reduce the possibility of sibling incest.

However, based on considerations of natural selection the opposite prediction concerning F-D incest could also be made. If one assumes that incestuous behaviour is affected by natural selection one should expect the selective pressure against F-D incest in a virilocal situation to be lower and hence tendency towards incest to be higher, because the opportunity for its occurrence is lower. Thus, if the rates of incest at the age before women are transferred are compared, it should be higher in a virilocal society because the selective pressure against incest in the former case would be lower because the opportunity for incest would be lower than in the latter case. A similar prediction can be generated concerning the relationship between uxorilocality and M-S incest rates.

Welham predicted that all other conditions affecting the costs of incest (like degree of inbreeding) being equal, societies with low paternity confidence will have higher rates of incest than those with higher paternity confidence. Further, if paternity confidence is equivalent, then societies with higher coefficients of inbreeding will have higher rates of incest compared with more outbred populations because the selective pressure against incest would be lowered with high rates of inbreeding (here Welham accepts the theory that inbreeding over generations cleanses the gene pool and reduces the genetic load, but his

prediction will also follow from the argument that in societies with a high mean coefficient of inbreeding there will be a lower marginal increase in homozygosity in close inbreeding [see above]) (Welham 1990, 97, 108-109).

However, the putative inter-cultural correlation between paternity confidence and incest could be accounted for by a third factor as well, namely conformity to rules. In societies in which rules are adhered to more rigorously, we could expect fewer people to break the rules concerning adultery, and fewer people to break the rules concerning incest avoidance. In societies in which conformity to rules is weaker, it could be expected that both rules against adultery and rules against incest would be more frequently broken.

It seems that most of the predictions made by Shepher, van den Berghe and Welham are of little use, no matter how well they correspond with reality. Shepher's prediction concerning the relative rates of incestuous dyads is very important, though. If borne out by empirical studies, it would require the opponents of human sociobiology to explain the finding. If not confirmed by empirical studies, the failure of the prediction would cast serious doubt on the sociobiological rationale of incest avoidance. There are few empirical data that can test Shepher's prediction, but there is some information that suggests that Shepher's prediction may not correspond with reality. I now turn to examine this information.

5.4. Some empirical concerns about the sociobiological theory

The human sociobiological analysis of incestuous strategies (be it the avoidance of incest or its perpetration) was based on an accepted understanding that F-D is by far the most prevalent incestuous dyad, followed by B-Z with M-S lagging way behind (see section 5.2 above). These figures were derived from studies of *reported* incidents of incest in the "West" (Shepher 1983, 126). There are good empirical grounds to cast doubt on this basic understanding.

First, it is likely that in Roman Egypt B-Z sexual intercourse was more common than F-D (see chapter 1 above). Shepher seems to have been unaware of the situation in Roman Egypt altogether. It is not mentioned either in his 1971 article or in his 1983 book. Bixler considers this instance an exception (Bixler 1982a).

In addition, there are statistics both from America and Japan that challenge Shepher's prediction. Shepher and his colleagues seem to be largely unaware of these data. (These studies were not cited by van den berghe, Shepher or Welham. Some aspects of Gebhard and his colleagues' study [see below] were briefly discussed by Bixler [1981, 642], though no reference was made to their analysis of incest between primary kin in the various heterosexual dyads).

Paul H. Gebhard and his colleagues from the Institute of Sex Research embarked on a large-scale analysis of sex offenders (Gebhard et al. 1965). Their study is unique in that it included a sample of a segment of the general population rather than relying on estimates of the rates of incest from reported cases (Hyde 1986, 496). It compared three sample groups as follows: 1356 convicted sex offenders; 888 inmates in prison who had never been convicted of sex offences; and a control group of 477 who had never been convicted of anything beyond traffic violations. All groups were of white males. The prison group of non-sex-offenders and the control groups are the relevant to the current discussion. The researchers estimated that the control group approximated the sexual behaviour of white urban American males with less than college education and no prison experience, and that the prison group was not badly biased (Gebhard et al. 1965 1, 27, 36-37).

406 members of the control group had been studied for their possible incestuous experience. None reported having been involved in F-D or M-S incest, and only one reported being involved in B-Z incest. In the prison group fourteen reported they had been involved in B-Z incest and one in M-S, while no F-D incest was reported (The data are arranged in percentages in Gebhard et al. 1965, 572, table 63).

I would not go as far as claiming that this demonstrates that in the US full sibling incest is more common than F-D (but see Hyde 1986, 496). The sample is small, and the results need to be replicated in similar surveys. The reporting of the individuals studied may have been biased. However, this study does strongly suggest that the situation in the wide population may not be reflected by reported incest , and that the reporting of incest which informs studies like those quoted by Shepher may well be biased so that the actual practice of full-sibling incest is under-reported, resulting in a distorted perception of the relative occurrence of incest in the different dyads.

Reported incidents of incest in Japan do not concur with Shepher's and van den Berghe's expectations either. M-S and older Z-younger B are by far the most commonly reported there. F-D incest is rare among reported cases of incest (Kawana 1980; Minami 1984[12]; Hartcher 1989).

Peter Hartcher, a reporter for a Sydney tabloid newspaper, quoted statistics he had collected from Japanese counselling centres according to which M-S incest is among the most common sexual problems in Japan, second only to extramarital affairs (Hartcher 1989, 1). Seven of every eight cases of incest referred to one centre were mother-son incest. F-D incest accounted for just one or two per cent of the incest cases handled by counselling centres (Hartcher 1989, 1).

[12]Citations of Minami's and Kawana's books are based on translated summaries.

Kimi Kawana reported on data she had obtained from the Contraception Information Phone Service Centre in Tokyo which provided counselling services on sexual and contraceptive matters. Of 15,000 calls received between October 1978 and September 1979 412 were related to incest. They were arranged into the following table, table 5-2, (which is based on table 1 in kawana 1980, 25).

Table 5-2: Incestuous Relationships and Desires as Reported to a Japanese Counselling Centre

Male -- Incest	Total -- 382 cases	100.0%
with older sister	155	40.6%
with younger sister	51	13.4%
with mother	110	28.8%
with daughter	10	2.6%
with aunt	48	12.6%
other	8	2.1%
Male -- Incestuous Wishes	Total -- 144 cases	100.0%
with older sister	64	44.4%
with younger sister	28	19.4%
with mother	45	31.3%
with aunt	7	4.9%
Female -- Incest	Total -- 30 cases	100.0%
with older brother	8	26.7%
with younger brother	7	23.3%
with father	12	40.0%
other	3	10.0%
Female -- Incestuous Wishes	Total -- 10 Cases	100.0%
with older brother	4	40.0%
with younger brother	6	60.0%

This table cannot be taken at face value. Men loom large among the people who approached the Centre, whereas very few women contacted the Centre for problems that relate to incest. One may wonder whether it is that women are more than twelve times less likely to be subjected to any form of incest than men are, and more than fourteen times less likely to entertain incestuous wishes, or whether women are less likely to call such centres. The assumption that the reporting of incest is gender biased is strengthened when considering the reporting of F-D and M-S incest by the mother or father. Ten fathers reported having incestuous relations with their daughters (compared with twelve daughters reporting incest with their fathers), whereas no mother reported having incest with her son (as opposed to 110 sons who reported having incestuous relations with their mothers). An inevitable conclusion is that for whatever reason women are less likely than men to report incestuous relations and desires. In addition, the fact that the data were obtained from anonymous phonecalls should be borne in mind too, as this casts further doubt on the reliability of the calls (cf Spink & Tutt 1989).

F-D incest was rarely reported by those who contacted the Centre. This may well be attributed to the gender bias in the reporting. Nonetheless, theories that assume that females resist incest more strongly than

males would predict that in B-Z incest, younger sisters would be the ones more commonly involved in incest or that the incidence of sibling incest involving younger and older sisters would be equal. This is obviously not the case here. Since both the figures on older and younger sisters come from men, a gender bias in reporting cannot account for the differing rates of reported incest involving older and younger sisters (the number of women who reported full sibling incest -- fifteen, is too small for generalisation). One cannot rule out the possibility that men who were the initiators of incest are much less likely to seek counselling than those who were victims of incest. Perhaps this explains, at least in part, the overwhelming reporting of older Z-younger B and M-Z incest.

The question of who initiates the incestuous act is crucial. Shepher and van den Berghe would predict that males do. In the representative examples of M-S incest described by Kawana (e.g. 1980, 29-30, 65), and according to both Kawana and Minami it is the mother who typically initiates and maintains M-S incestuous relations (Kawana 1980; Minami 1984, chs 1, 2).

There is a growing body of evidence from the "West" that M-S incest is under-reported. In a recent study in Britain Anne Peake compared statistics on child sexual abuse (though not necessarily incest) collected by police with statistics collected by professionals in confidential surveys. Her results suggest that sexual abuse of boys is under-reported. She proposed that various social factors are responsible for this situation, factors like boys' being expected to be self reliant (Peake 1989). As stressed before, child sexual abuse and incest are very different concepts. However, Peake's result at least raise the possibility that M-S incest is under-reported. Under-reporting of M-S sexual abuse seems to be the situation in Australia (Crisp 1991).

It seems that an essential element of what Shepher and his colleagues perceived to be the common reality, namely the putative relative rates of the practice of incest, may prove to be wrong. At present, the situation is still unclear, and research into the rates of incest in its various dyads is urgently needed.

5.5. Some theoretical concerns

In addition to the empirical concerns described above, There are some weaknesses in the way parental investment theory and the concept of inclusive fitness were used by Shepher, van den Berghe and Welham. As for the assumption of unequal parental investment in humans, it is true that when focusing on gametes, gestation and lactation, the female invests more in rearing the child. However, the male may also expend energy in competing with other males for a female; in providing for her and ensuring her survival in the period of gestation; and for the duration of dependence of the offspring on maternal care in ensuring that no other competing male takes over the female and disposes of his offspring to make way for the competitor to invest. In addition, paternal care may be important in increasing the probability of future reproductive

success of offspring in developing social skills in the young. Thus, in humans, the question of who invests more in offspring and of how different the investments of men and women are should be empirically studied in different cultures and in different situations, and not presupposed.

Shepher's models of inclusive fitness are useful insofar as they demonstrate that normally a gene for incest has no higher a probability for spreading in a population than a competing allele which does not favour incest. Essentially this is because the increase in the coefficient of inbreeding of offspring of incest is matched by the absence of nephews and nieces (see section 5.2 above). However, Shepher's analysis of the relative losses of males and females are essentially flawed and stem from the assumptions at the basis of his models, not from the modelling itself. Shepher assumed that the number of offspring in a dyad is fixed by the number of offspring a female can rear, and that males can produce an infinite number of offspring limited only by the maximum capacity of the females that are available for mating (see the note quoted from Shepher above). That this is so in all or most societies is not at all obvious. These assumptions create the situation in which females stand to lose more from incest.

Another misconception is Shepher's confusion of sex and marriage (cf Fox 1967, 54). The fact that two individuals are married does not necessarily mean that the male is the genitor of the female's offspring. This confusion is particularly evident in the note added to table 5-1 quoted above (e.g. the comment on polyandry being usually adelphic).

The focus on the practice of incest in order to analyse incestuous behaviour which is typically the avoidance of incest raises some difficult questions. An important consideration that Shepher, van den Berghe and Welham have not addressed is the possibility that the causes of the *practice* of incest may be very different in nature to those behind the *avoidance* of incest. Their explanation of the relative strength of the drive towards incest may explain the difference between individuals who practice incest at different rates, but fails to account for those who avoid it altogether. For instance, the rationale that explains why men stand to gain more, or to lose less, from incest may explain why men who practise incest, do so more often than women who initiate incestuous relations. But this explanation does not account for the majority of men and women who avoid incest altogether. It could be that most humans avoid incest because of some hypothetical mechanism called "conformity." In each society there may be a certain part of the population which lacks this mechanism. Among those, gender may well play an important role in determining the rate of the practice of incest. This does not mean, though, that gender plays any role in the avoidance of incest among the rest of the population.

Another point should also be kept in mind, namely that statistics of the practice of incest are not necessarily a numerical expression of the relative inclination towards various forms of incest. In order to

give a fair assessment of incestuous tendencies, actual rates of incestuous activity in the various dyads need to be calculated against the opportunities of its practice in those dyads. If an inclination towards one form of incest is twice as strong as the inclination towards another, and yet there are twice as many opportunities to carry out incestuous relations of the second type, we may end up with statistics that show that both types of incest are practised at identical rates. Statistics about the opportunity of incest should be provided alongside statistics on its practice for analysis. An evolutionary study of incest avoidance should ideally, then, include a study of the changing opportunities for incest. This point is important because it could be argued that mate selection strategies had been fine-tuned at an earlier phylogenetic stage (ie an earlier stage of the species' natural history), and as social conditions changed to create new opportunities for incest and eliminate old ones, the emergent empirical picture was distorted. Thus, the prevalence of female-initiated incest in Japan, if indeed it transpires that female-initiated incest is more common than male-initiated incest, can be explained away as a situation in which new forms of proximity between M-S have arisen, changed the environment, and consequently caused incestuous strategies not to conform to the predicted patterns of behaviour.

As for predictions of the form advanced by Welham, these could prove useful. It would be helpful to generate predictions of the nature "everything else being equal ... then" to test the hypothesis that natural selection plays even a partial role in shaping human incestuous behaviour. Such predictions can be tested both within cultures or on a large scale comparative intercultural analysis. If a prediction is borne by the results of a survey, it is possible that natural selection has affected the mechanisms behind the behaviour in question. However, such predictions need to be based on sound reasoning and not be derivable from alternative theories, which I fear is not really the case with the predictions at hand (see section 5.3 above), and tested out.

Another line of criticism was put forward by Kitcher. He argued that human sociobiologists who had written on incest avoidance did not rule out the possibility that explanations other than those proposed by them are responsible for human incestuous strategies. Kitcher suggested a possible alternative explanation. He pointed out that cultural forms of transmission can account for the spread of the tendency to avoid incest, and it may have derived from an original situation in which incest was avoided because of environmental factors or causes other than individual genetic fitness-maximisation (Kitcher 1985, 203-205). However, Shepher, van den Berghe and Welham were not describing actual evolutionary processes or natural histories that resulted in the behaviours they describe. A minimalistic argument could be advanced, namely that genetic factors did not necessarily shape the whole of these behaviours, only those behaviours that were mal-adaptive and consequently modified by natural selection through mechanisms such as the Westermarck effect. To use the metaphor of the genes holding culture on the leash

(Lumsden & Wilson 1981, 13) it could be argued that so long as the dog is walking spontaneously in the right directions it needs no correction. This does not mean that the master is not ultimately responsible for the direction. Thus, the relevance of natural selection to behaviour does not require that proximate mechanisms should have all been selected or should be somehow genetically grounded if the behaviour is adaptive. In addition, the fact that other primates seem to avoid close inbreeding (see section 3.2 above) suggests that in a pre-cultural evolutionary stage inbreeding was probably avoided by hominids owing to natural selection. It is therefore plausible that such a behaviour was further transmitted even if only cultural transmission is involved. Whereas the contribution of natural selection in early, pre-cultural as it were, hominid evolution seems plausible, the crux of the debate is whether human incest avoidance has been maintained by natural selection in our recent evolutionary past, after humans have become distinctly cultural animals. Ideally, to substantiate a claim to the relevance of natural selection along these minimalistic lines, one will have to provide a credible description of incestuous behaviour, demonstrate empirically that the strategies of incest avoidance are adaptive, and finally demonstrate that in cases where mal-adaptive behavioural strategies arose, they were selected against by natural selection. In the latter case natural selection would act on the gene pool, and genetically grounded mechanisms would be selected for. If such instances are not known, as seems to be the case with human incest avoidance, it is impossible to determine whether natural selection is relevant (cf Caro & Borgerhoff Mulder 1987).

At any rate, Shepher, van den Berghe and Welham did not pursue this line of argument. Shepher and van den Berghe explicitly committed themselves to the position that the proximate and ontogenetic factors behind human incest avoidance were selected for and shaped by natural selection.

There are also theoretical difficulties in studying behavioural strategies and tendencies (cf Kitcher 1987, and see responses there). Incestuous strategies may be affected by other proximate mechanisms which may have survival value in their own right, but whose effect on incestuous behaviour is to reduce genetic fitness. One example could be conformity to rules, which may drive people to avoid incest in situations in which it is genetically profitable, or risk taking behaviour which according to some sociobiologists is advantageous in males (e.g. Zahavi 1975) and which can push young males to defy rules because they are rules, including those banning incest.

My point is that even if incestuous strategies were shown to be mal-adaptive, the role of natural selection in their shaping would not be disproved. Equally, if incestuous strategies were conclusively shown to be adaptive, it would still be very hard to discern the exact role, if any, played by natural selection in their evolution. The alternative hypothesis of a genetically neutral, purely cultural transmission cannot be ruled out. The information we have on human natural history is very scarce, and generally, there is a great

difficulty in identifying the role played by natural selection in the evolution of any human behaviour without understanding the proximate or ontogenetic factors behind the particular behaviour (Caro & Borgerhoff Mulder 1987).

5.6. Conclusion

To sum up this chapter, I believe that there is little promise in the attempts to study incestuous strategies the way Shepher and van den Berghe propose. There are serious theoretical difficulties in their project, and the statistical picture they take for granted is far from evident. Data about the general behavioural tendency is obviously lacking, and research is needed into the prevalent forms of incestuous behaviours in different societies at different times. (This is clearly much easier said than done.) It seems to me that judgement on the role played by natural selection will have to be deferred until the proximate and ontogenetic factors behind human incest avoidance are better understood.

Chapter 6

Summary and Conclusions

In this thesis I have tried to assess the current state of human sociobiological analyses of incestuous behaviour, and suggest directions for further anthropological research.

Chapters 2 and 3 focused on propositions that are fundamental to the human sociobiological treatment of human incestuous behaviour. The proposition that incest avoidance is universal was examined. This proposition was taken to mean that most humans in most societies avoid incest. The very paucity of the exceptions found underpins the emerging picture of universality of incest avoidance. The proposition that inbreeding depression occurs in humans and constitutes selective pressure against close inbreeding was also examined. The occurrence of inbreeding depression was found to be well supported by empirical evidence. It is plausible that inbreeding depression has constituted selective pressure against close inbreeding by reducing reproductive success, but, at least theoretically, scenarios could be envisaged in which inbreeding depression would not reduce reproductive success. Therefore, more data are needed on the circumstances of human evolution, data that could be used to assess the effect of inbreeding depression on reproductive success. Such data include the actual determinants of the number of offspring, parental investment required to secure the maturation of offspring and so forth. In addition, studies of the exact effects of inbreeding depression in various present human populations as well as in non-human primate populaions may form a basis for educated guesses about the effects inbreeding depression might have had in past stages of human evolution. Once the exact effects of inbreeding depression are better known, more precise arguments about the effect of inbreeding depression on reproductive success could be devised.

Also examined was the proposition that other species, particularly non-human primates, avoid close inbreeding too. It was found that this proposition is supported by observational studies of primates. This proposition has implications for the study of incestuous behaviour. As most, if not all primates seem to avoid close inbreeding, it is plausible that early hominids avoided close inbreeding too. Further, it is possible that similar selective pressures may have been involved in inbreeding avoidance in non-human primates and in incest avoidance. These two possibilities should inform further theorising on the natural history of incest avoidance. A third implication is that it is possible that homologous proximate and

ontogenetic processes are involved in non-human primate close inbreeding avoidance and in incest avoidance. This means that observations and experimental studies of the former are relevant to the latter, and can be legitimately used to generate hypotheses concerning incestuous behaviour.

Chapter 4 moved to analyse the Westermarck effect which may play a role in the process behind incest avoidance. The studies reviewed suggest that childhood co-socialisation in a context of unrestricted physical proximity does lead to sexual indifference at puberty. However, the number of studies conducted into the Westermarck effect is by no means large. More demonstrations of this phenomenon will increase our confidence in Westermarck's hypothesis. The independence of those effects of co-socialisation which are independent of cultural constructions is unlikely to be demonstrable, as in all societies cultural constructions will most probably reflect the effects of typical child rearing. Hence, all that could be expected from future ethnographic work on the Westermarck effect is further demonstration of a correlation between childhood proximity and adult sexuality, preferably in the absence of explicit taboos or strong prohibitions; conclusive proof of the independent effects of childhood proximity on adult sexuality is unlikely to be attained.

As for the ontogeny of the Westermarck effect, further research is needed before anything beyond speculations could be said about the experience necessary and its exact effects. I used Borenstein's (1989) scheme for research into sensitive periods to point out the questions that should be answered in order to further our understanding of the ontogeny of the Westermarck effect. The further work needed includes more sophisticated statistical analyses of cases like the kibbutzim. This would yield a better understanding of the nature of the sensitive period required for the Westermarck effect. A more careful study of sexual indifference in minor marriages can be a step towards finding out further important information about the effect of co-socialisation. Does sexual indifference wear off with time? Does it increase with time? Does it remain constant? Answering such questions can be a great step forward in deciphering the proximate and ontogenetic processes involved in the Westermarck effect. If non-human primates are found to demonstrate a Westermarck effect too, experimental manipulations of primates as well as observational studies may inform further studies of humans.

In Chapter 5 I discussed human sociobiological accounts of incestuous strategies in terms of fitness-maximisation. Some of the facts assumed by such analyses, like the ratios of incest at its various dyads, differential rates of parental investment and so forth, were called into question. The empirical reality which informs these analyses was called into question. It is apparent that not enough information exists about human incestuous behaviour, and that more research is needed into the absolute rates of the practice of incest and into the rates at which it is practised in various dyads. We need to know how often incest

normally occurs in various societies, as well as in which dyads and in what relational contexts does incest typically occur. Thus, studies in Japan may probe the possibility that incest in female-dominated dyads occurs more often than incest in male-dominated dyads. Studies of the general population in the "West" may examine whether B-Z incest occurs more commonly than F-D incest. Other relevant empirical questions include the determinants of numbers of offspring families have, the distribution of parental investment between genders in different contexts, the opportunity for incest in the various dyads.

My criticism of the application of considerations of natural selection to human incestuous strategies was not confined to the empirical. The attempts to apply considerations of parental investment and inclusive fitness to incestuous strategies are fraught with theoretical difficulties like unrealistic assumptions (e.g. in Shepher's modelling of the effects of incestuous matings on Inclusive fitness), extrapolating from one phenomenon (assumed ratios of the practice of incest in various dyads) to a possibly unrelated one (total avoidance of incest), and so forth. I argue that to bypass the proximate and ontogenetic levels of analysis is not a sound way of pursuing an analysis of human incestuous behaviour, and that the most profitable line of investigation would be that focusing on the proximate and ontogenetic factors behind human incest avoidance. Once our understanding of the the ontogenetic and proximate factors is improved, and once more empirical data exist about actual practices and about the circumstances of hominid evolution, a more concerted analytic effort could be put into the interpretation of the ultimate and phylogenetic aspects of the ontogenetic and proximate factors that produce the emerging picture of incestuous behaviour.

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