Cambridge Archaeological Journal 5:1 (1995), pp. 75-114

The Human Symbolic Revolution: A Darwinian Account

Chris Knight, Camilla Power & Ian Watts

By 50,000 years ago, the effects of a 'symbolic explosion' – an efflorescence of human art, song, dance and ritual – were rippling across the globe. Applied to archaeological evidence, standard neo-Darwinian theory offers new understandings of this improbable event. The present article defines 'symbolism', models quasi-ritual behaviour in late archaic Homo sapiens, *extends the argument to the emergence of anatomically modern humans and concludes with preliminary tests against archaeological, ethnographic and rock art data.*

The brain constructs from sensory inputs its own illusory version of the world outside. Unlike the real world, the illusory one is egocentric, its coordinates measured from the 'here' and 'now' of individual experience. Environmental features are picked out and interpreted; schemata for recognizing feeding and/or mating opportunities may be disproportionately developed.

While cognition in all animals is distorted by such motivational bias, among humans a further distorting process occurs. Through exposure to ritual, art and other external memory stores, every individual constructs, in addition to the cognitive map just described, a personalized *copy* of a *communal* map, access to which defines membership of a symbolic community.

Among modern humans, all behaviour and all cognition occurs in the context of this additional map – 'a web of beliefs, rules, and values that gives all things and all actions symbolic cultural meaning' (Chase 1994). Implanted by external public pressure, the motivational bias of such communal cognition is sociocentric, countering the bias of egocentric vision. Onerous social duties are presented, paradoxically, as attractive, while opportunities for sexual self-indulgence are marked 'danger' or 'taboo'.

The representations central to the communal map are intangibles, without perceptual counterparts. *'God', 'Unicorn'* and *'Totem'* are among the possibilities. 'Symbolic culture', as Chase (1994) puts it,

> ... requires the invention of a whole new *kind* of things, things that have no existence in the 'real' world but exist entirely in the symbolic realm. Examples are concepts such as good and evil, mythical inventions such as gods and underworlds, and social constructs such as promises and football games.

If speech in the modern sense is distinguished by uniquely complex formal features, it is because these are the specialized design hallmarks of a system for communicating about non-perceptible worlds (Bickerton 1990).

Symbolic culture facilitates co-operation beyond that explicable directly from kin selection or reciprocal altruism theory (Chase 1994); it is such uniquely human co-operation that underwrites the effectiveness of speech (*cf.* Carrithers 1990, 202; Bennett 1976; Grice 1969). This needs stressing because the reverse argument has recently become current among archaeologists, palaeoanthropologists and linguists debating human origins. Bickerton (1990) postulates a 'neural macromutation' thanks to which the descendants of 'African Eve' spontaneously produced 'syntacticized language', capable for the first time of reference to morally authoritative intangibles. 'The gods' and those human co-operative endeavours which depend on their authority are pictured by Bickerton as *products* of speech. Speech as a biological given is then credited with generating religion, art, myth and symbolic culture as a whole (*cf.* Cavalli-Sforza *et al.* 1988,6006; Mellars 1991).

Such 'word-magic' scenarios treat speech as an independent variable, unconditionally superior to alternative systems of communication. Darwinian theory, however, does not recognize superiority/inferiority in the abstract – only selection pressures. Speech involves not only benefits but potential costs; among these must be counted the dangers of excessive reliance on uncorroborated information *from* others and – conversely – the risks of entrusting valuable information *to* others. Primate 'tactical deception' theory (Byrne & Whiten 1988) would not predict such trust; within the terms of this paradigm, it is anomalous. The human capacity for speech is a specialized biological adaptation which – no less than stereopsis in monkeys or echolocation in bats – must have evolved through standard processes of Darwinian natural selection (Lieberman 1991; Pinker & Bloom 1990; Pinker 1994). But postulating sudden macromutations is not Darwinism.

Darwinian signal evolution theory

Politics and power relations are inevitably involved in communication. Krebs & Dawkins (1978; 1984) broke new ground by abandoning assumptions about truthfulness and defining communication as 'the means by which one individual, the actor, exploits the muscle power of another, the reactor'. According to this body of theory – as amended and refined by Zahavi (1987), Guilford & Dawkins (1991), Maynard Smith (1991; 1994) and others – signals in the animal world can be costly or cheap. They can also be deceptive or honest. In conflict situations, reactors do not assume honesty in each other's signals. Zahavi (1987) has shown that a signal's discernible *costs* can nonetheless guarantee its honesty by tapping and hence testing the very reservoir of quality that the signal is attempting to advertise. Signals can, then, be 'honest' but only about the physical quality (health, stamina, strength etc.) of those emitting them. Signals dependent upon convention – always less costly than full display – may permit external reference but afford opportunities for cheating and so tend to be ignored. Deceivers may still use occasional coded signals provided cheating is sufficiently rare. But where conflict is endemic, coded signals become phased out and even cheats must opt for what *look like* self-evident, costly displays of signaller quality. Advertisers typically emit conspicuous, multi-media, repetitive signals ('peacock tails', aggressive 'threat' displays etc.) designed to exploit the perceptual biases of receivers and thereby overcome their 'sales resistance'. Such signals, being costly and redundant, bear all the hallmarks of 'ritualization' (Krebs & Dawkins 1984).

Where interests are shared, however, everything is different. Signallers and receivers may cut mutual costs, including the costs of time-consuming signal evaluation (Guilford & Dawkins 1991), by allowing an element of convention into even competitive 'quality advertising'. Coded warnings of predators can evolve between group-living animals since they are likely to be believed. If it pays the listener to respond to a signal, the listener will be straining its ears or eyes so that the signaller has no need to produce a loud blast of sound or bright colours (Krebs 1991). The outcome is signals of diminished amplitude and conspicuousness – 'conspiratorial whispering' (Krebs & Dawkins 1978; 1984; Krebs 1991).

The great apes appear to lack even the limited repertoire of coded calls possessed by the much smaller-brained vervet monkeys (Cheney & Seyfarth 1985; 1990). How should we explain this paradox? The most remarkably coded calls of vervets are those used in warning against predators – quintessentially an area of *common* concern. Chimpanzees are threatened by few predators. If they *did* evolve coded signals for other purposes, these would be largely under voluntary control. Given the general competitiveness of ape social life, 'Machiavellian' deception (Byrne & Whiten 1988) would consequently be routine – and few would believe any call not accompanied by perceptible evidence. But to endorse each call perceptually would be costly, undermining the point of using a code. This may explain why, despite certain symbolic *capacities* as revealed in human language-training programmes (e.g. Savage-Rumbaugh & Rumbaugh 1993), chimpanzees in the wild communicate only in *perceptually verifiable* ways, relying not on coded signals but on emotionally expressive vocal and other gestures which are difficult to fake (Burling 1993) and whose more subtle meanings can be contextually *inferred.*

Deception: individual and collective

Darwinian 'Machiavellian intelligence' theory, then, expects large-brained primates to exploit the gullibility of their conspecifics, precluding by deceptive use any incipient reliance on coded signals. This dynamic contradicts an alternative scenario, according to which primate tactical deception, far from undermining symbolic behaviour, constitutes an incipient manifestation of symbolism.

Take the reported case of a subadult male baboon observed harassing a youngster before being pursued by the victim's adult protectors. Unexpectedly, the culprit suddenly stared as if watching a distant predator; although this did not exist, his pursuers had to check first. The deceiver thereby distracted attention long enough to make his escape (Byrne & Whiten 1985).

It might be claimed that the fictional predator amounted to a 'symbolic' one. But it was not. Having checked by looking at the horizon, the victims of the deceit lost all interest in the unreal phenomenon. The fiction expired at that point. The human symbolic domain by contrast is a realm of *indefinitely* maintained *collective* 'deceits', *collective* 'fantasies' (*cf.* Sperber 1975, 93-5). It is as if the gang of baboons in our example all looked, saw no predator – but then joined in with their deceiver in pretending to see one. Clearly, they would not be predicted to do this unless they had some collective interest in perpetuating the fantasy, for example in the course of deceiving a third party. There are no primate examples of this.

It is the individualism of primate 'tactical deception', not deception as such, which precludes the evolution of call systems resembling speech. Vervet monkeys, it was noted, have specific alarm calls for distinct species of predator. Each call triggers an act of perceptual identification of the predator, prompting automatic escape reactions almost as if the creature had been seen (Cheney & Seyfarth 1985; 1990). The fundamental peculiarity of the sounds of human speech, however, is that they activate representations of a kind unknown among primates: namely *collective* ones (Durkheim 1965 [1912]). When we speak, we use coded terms to successively trigger imaginative acts of identification of pre-existent constructs, each distributed throughout the speech community, the more morally authoritative ones (*'God', 'Supernatural Potency'* etc.) being central to the whole system. Only humans can gossip about unicorns and hence about anything imaginable (Bickerton 1990; Kendon 1991).

For a signaller to refer or 'point' to something, whether gesturally or using a code, the receiver must be able to 'see' what is meant; the parties must share experience of the same world. If the reference is to something imaginary, then all involved must inhabit the same *imaginary* world. Vervets and other primates live in the real world. Their calls and gestures concern on-going social/ environmental events. Primates know of no gods, and seemingly need none. There is therefore nothing for them to vocalize about – except on-going *real* events and experiences, for which purpose a gesture/call system (Burling 1993) suffices. Since primates lack a *constructed* world of commonly acknowledged, morally authoritative intangibles, reference to them – syntactical or otherwise – is simply unthinkable.

Neither the primate nor the adult *Homo* brain is designed to confuse fantasy with reality. In evolutionary terms, that is a novel function. In this light, it seems misguided to imagine that even highly intelligent archaic humans who stumbled upon the supposedly self-evident 'advantages' of symbolic language would necessarily have appreciated those advantages. Whatever their undoubted symbolic *capacities* (Marshack 1989; Soffer 1992), unless they already had powerful reasons for sharing pure fantasies, the *point* of making the switch to true language would have escaped them. Symbolic speech would have been redundant for the same reason that teaching chimps how to say 'God' is a waste of time. For evolving late archaic *Homo sapiens,* choosing a verbal label for a construct such as *'Spirit'* or *'Supernatural Potency'* would have been a small challenge compared with the difficulties inherent in establishing a construct of this kind in the first place. More than a linguistic revolution would have been required, after all, to generate the *need* for constructs of that kind.

To summarize: primates *CAN:*

- Vocally label reality
- Individually deceive
- Fantasize.

They *CANNOT:*

- Collectively deceive
- Label their fantasies.

These capacities and incapacities are connected. It is *because* primate deceptions are not communal that they are not cryptically communicable. Private fictions, private fantasies, are simply not the kinds of things to which agreed, collective labels can be attached. Our task, then, is to elucidate the conditions necessary for *collective deception* to evolve, and since no better paradigm exists – to do so within a neo-Darwinian, behavioural ecological framework.

Encephalization: costs and female strategies

If the story of human evolution is encephalization, the materialist subtext must be how females fuelled the production of increasingly large-brained, burdensome offspring. We know the basic answer: in the course of *Homo's* two million years of evolution, mothers extracted what by primate standards are unprecedented levels of male energetic investment. Models which address the fine-grained evolutionary mechanisms by which females drove this process may yield fruitful lines of enquiry, explicating not only some basic features of human reproductive physiology, but also underlying motives to symbolism.

The extra costs arising from encephalization include the metabolic demands on the mother for sustaining brain growth in the infant (Foley & Lee 1991), and the increased energetic requirements of foraging for a higher quality diet. Humans compensate for their expensive brains by having correspondingly small, and energetically cheap, guts (Aiello & Wheeler in press). This is only compatible with high quality diet, in turn requiring larger foraging areas (Clutton-Brock & Harvey 1977), with increased overall energy expenditure (Leonard & Robertson 1992).

Because hominine mothers bore these escalating costs, we must suppose that it was females who developed strategies to meet them. As maternal energy budgets came under strain, natural selection would have acted on two key areas of reproductive physiology: firstly, on life history variables (*cf.* Foley & Lee 1991) – factors critical in partitioning the energy costs of reproduction – and secondly, on features of the reproductive cycle.

To drive up male investment, females needed to counter male philandering strategies. A roving or philandering male should aim to reduce the time spent searching for a fertilizable female, and then reduce the time spent waiting for access to her at her fertile moment. The human female appears ‘welldesigned’ to waste the time of philanderers by withholding accurate information about her true fertility state.

Concealment of ovulation and loss of oestrus with continuous receptivity have eliminated any reliable cue by which to judge whether a female is likely to have been impregnated. The longer a male must remain with one female to ensure fertilization, the smaller his chances of fertilizing another within one breeding season (Dunbar 1988, 160). These features are not unique to humans as against other primates (Hrdy 1981, 158). In some species with apparently concealed ovulation, however, males can track female cycles through olfactory cues (e.g. cotton-top tamarins: Ziegler *et al.* 1993). Studies of variation in human ejaculates (Baker & Bellis 1993, 880) confirm that men are unable to track women’s periovulatory periods. Ovulation in women is well concealed.

Alexander & Noonan (1979) argued that, through concealment of ovulation and continuous receptivity alone, males would have been forced into prolonged consortships, that even in multi-male group contexts, pair-bonds would have been reinforced, and hence males would have had greater confidence in paternity, leading to greater parental investment. Against this, Hrdy (1981) proposed that these features could function in a context of promiscuity to deprive males of information about paternity, so reducing risks of infanticide. These hypotheses are not mutually exclusive. While ovulation may be concealed initially to counter infanticide risk, once evolved this feature can be exapted to serve a female strategy of increasing male investment (see Sillén-Tullberg & Moller 1993).

Another key means of thwarting philanderers is reproductive cycle synchrony. Knowlton (1979) formulated a general model of synchrony as a strategy by the sex which invests most in offspring to secure greater parental investment from their mates. If females synchronize their fertile moments, no single male can cope with guarding and impregnating any group of females. Local, previously excluded males are attracted into groups by potentially fertile females (*cf.* Dunbar 1988, 140-42). More males become available to the synchronizing females.

Cycle synchrony, manifested as synchrony of menstrual onset, has been documented among humans (McClintock 1971, and see Graham 1991; Weller & Weller 1993 for refs.). Several of these studies have been criticized on methodological grounds, however, (see Wilson 1992). Knowlton, following Maynard-Smith (1977), Ralls (1977) and Emlen & Oring (1977), observes that ‘the spread of female synchrony is likely to increase the ESS [evolutionarily stable strategy] for male parental investment because the payoffs to searching for new mates are reduced’ (1979, 1029). Given the large range sizes postulated for Pleistocene hominines (Leonard & Robertson 1992), fertile moments would need to be synchronized widely across populations rather than being synchronized only within local troops each on a different cycling schedule – the pattern observed in various baboon species (Dunbar 1988). The modern human female appears well designed for such widespread cycle synchrony, since she has the capacity for cyclicity linked to an environmental cue. Her mean length of menstrual cycle corresponds to the mean lunar synodic period at 29.5 days (Gunn *et al.* 1937; McClintock 1971; Vollman 1977; Cutler *et al.* 1980); and her mean length of gestation at 266 days is a precise nine times multiple of the mean lunar synodic period (Menaker & Menaker 1959, and see Martin 1992, 263-4). In ancestral populations, the probability of overlap or coincidence of female fertile periods would be enhanced by seasonal and ecological factors affecting nutritional status and fertility rates (Ellison *et al.* 1989; Ellison 1990). Significant seasonality of births has been documented for hunter-gatherer populations (Wilmsen 1978; Hurtado & Hill 1990).

Turke (1984; 1988) was the first to focus on 'ovulatory synchrony' in hominine mating systems. He argued that ovulation concealment (with continuous receptivity) functioned to draw males into longer consortships, depriving would-be philanderers of accurate information about fertility. A pattern of ovulatory synchrony in local populations had the further effect of punishing male attempts to philander. This should drive the ratio of sexually active males to females in groups towards one-to-one. Sustained male / female bonds on a one-to-one basis should increase paternity confidence and inclination of males to invest.

Such one-to-one sex ratios can only be guaranteed by extreme reproductive synchrony, with local females synchronizing all reproductive events - ovulation, conception, gestation, birth, lactational amenorrhea and return to fecund cycling. In these conditions, to maximize fitness, males have no alternative but to invest in current partners. Such a strategy would be costly to females in populations with high infant mortality, however, and would be virtually impossible to sustain where females were using continuous sex receptivity to bond with males (Foley & Fitzgerald 1994; Aiello & Arthur 1994). Extreme synchrony would not be evolutionarily stable for hominines. But a seasonal form of synchrony, restricting most female fecund cycles to a certain period of the year, would incur minimal costs to synchronizers and would still function to counter male attempts to philander (Aiello & Arthur 1994). The mechanism for such a seasonal strategy could be nutritional status affecting energy balance and ovarian function (*cf.* Ellison 1994), rather than any restrictive pattern of sexual activity.

Seasonal ovulatory synchrony with continuous receptivity would effectively ensure at least as many males in breeding groups as fecundable females. But it would not prevent males from deserting lactating females in favour of cycling females. Where a degree of synchrony was an important female strategy for undermining male philandering, we would predict minimizing of any signal that divulged information about prospective fertility. If synchrony is not going to be perfect, then the least females can do is not advertise the fact.

But one reproductive signal *has* been amplified in the course of human evolution – women's profuse menstrual bleeding.

The problem of menstrual bleeding

Signalling was not the primary function of menstruation (see Profet 1993 for the only functional hypothesis yet developed). But once ovulation was concealed and oestrus lost in the human lineage, menstruation would have taken on significance as a cue. It is not an accurate indicator of fertility, because it occurs at the non-fertile time of the cycle. But it is a good indicator of *impending fertility.* Because menstruation is the *only* cue giving males positive information about female reproductive condition then we would expect that hominine males came under selection pressure to respond to that cue. This is not the case with chimpanzee males who have more reliable information concerning female fertility state in 'loud' oestrus signals.

Because menstruation is not accurate about timing of fertility, the information is not very useful for philanderers. Menstruation should make a female attractive to males who are prepared to wait around and mate-guard. Concealment of ovulation withdraws from males any information about when to bring mate-guarding to an end. So the male who responds to menstrual cues has to spend time with the female to increase his chances of paternity.

This implies that pronounced menstrual bleeding functioned to attract extra male attention, procuring *mating effort* in the form of protection, some food-sharing, grooming and coalitionary support. But the signal does not necessarily secure genuine *parental effort* from males. Once a female was pregnant, she risked losing that extra male attention to other menstruating females in the vicinity. The 'loud' menstrual signal threatens to destabilize a synchrony strategy in local populations, by marking out those females who may be impregnated in the near future. The menstrual 'flag' could have encouraged mate desertion, incited male competition, and led to some monopoly of fertile consortships by dominant males.

Male mating effort, associated with relatively high rates of mate desertion, may have been adequate for females during earlier phases of hominine evolution, when encephalization was proceeding at a relatively slow rate. The increased energetic costs of the phase of brain expansion associated with early *Homo* could have been offset by such factors as reduction in gut size (Aiello & Wheeler in press) and increased female body size (Aiello in press; Power & Aiello in press). Reduction in sex size dimorphism may be better explained by female requirements to meet reproductive and thermoregulatory costs, than by significant changes in mating strategy at this stage *(cf.* McHenry in press). In modelling the energetics of hominine encephalization, Foley & Lee (1991, 70) suggest that early *Homo* mothers did not rely on systematic male provisioning to meet their increasing reproductive costs. Left to themselves, mothers could find the additional energy from 'higher quality diet, from feeding for longer each day, or from maintaining lactation over a longer period'. Selection for 'slower rates of development and an extended period of immaturity' (Foley & Lee 1991, 70) enabled mothers to spread their energy load. Development of the fat stores characteristic of human females may have subsidized the costs of lactation, especially during periods of resource stress (Prentice & Whitehead 1987).

Shipman & Walker (1989, 388) model early *Homo erectus* patterns of foetal and postnatal brain growth, comparative to chimps and modern humans, on the basis of the cranial capacity and pelvic diameter of the adolescent male KNM-WTI5,000 (dated to 1.6 mya). They infer an incipient human-type pattern of altriciality, which they link to shifts towards higher quality diet. At later stages, during the exponential increase in brain size associated with archaic *Homo sapiens (cf.* Leigh 1992; Aiello in press), the postnatal trajectory of foetal brain growth became progressively extended (secondary altriciality). Intensified energetic costs during lactation would have confronted mothers with problems in balancing energy budgets. This demanded higher quality diet and more reliable supplies, which ultimately meant extracting greater energetic investment from males.

Deceptive sexual signalling

Assuming that archaic *Homo sapiens* females required more reliable male parental effort, how could they have resolved the problem of the salience of menstrual bleeding as a signal of impending fertility?

The logical solution is to cheat. Within kin coalitions, non-menstruating females could do this by 'borrowing' the blood of menstruants. Confusing the information available to males by displaying the same reproductive signal at the same time, females could then retain both the attractions of menstruation as an indicator of impending fertility and the advantages of synchrony for maximizing male parental investment. On this basis we would expect females, within kin-coalitions, to *manufacture* synchrony of signals whenever a member was actually menstruating – a strategy we term 'sham menstruation'. We might then expect them to select additional cosmetics – blood-coloured pigments for use in body-painting – to augment their 'sham' displays.

Such co-ordinated body-painting would constitute 'advertising' for extra male support. Provided females maintained solidarity, even if males knew which individuals were actually menstruating, they would not be able to use the information. If males attempted to fight for access to a particular female they would incur heavy costs and there would be no benefit since the female is not immediately fertile.

'Sham menstruation' is a strategy of 'counter-dominance' *(cf.* Erdal & Whiten 1994) in that female coalitions resist attempts by would-be dominant males to discriminate between fertilizable and nonfertilizable females. As a mechanism of sexual selection, it favours investor males over philanderers. 'Sham menstruation' also models behavioural adaptations prefiguring symbolic and ritual activity. What was a signal belonging to an individual, capable of extracting energy from males on a one-to-one basis, has become collectivized among a coalition of females, and amplified, broadcasting information which males cannot afford to ignore. Quite simply, males are only interested in *positive* cues to fertility. 'Sham menstruation' is an amplified indicator of the presence of an imminently fertile female in the vicinity. The female coalition now has a powerful signal for manipulating males.

Ostentatious cosmetic display surrounding a menstruating relative would be costly to a female coalition. That very costliness *(cf.* Zahavi 1987) would enable relatives to prove the health and fitness of any menstruating individual including, importantly, the level and quality of coalitionary support available to her. The trait of cosmetic signalling at menstruation could become exaggerated under pressure of competition between female coalitions for male investment *(cf.* Pagel 1994, on the evolution of prominent oestrous swellings as sexually selected traits that reliably advertise quality in female primates). Alternatively, it may evolve as a runaway process of sexual selection *(cf.* Fisher 1930) motored by male choice for female cosmetic display.

On another level, since some females who are not imminently fertile pretend to be, the signal is deceptive. Unlike primate tactical deception, which is always individualistic and egocentric (Byrne & Whiten 1988), the deception in this case is incipiently sociocentric, being maintained by a collective. As such, it represents a vital step towards sustaining an *imaginary construct* and sharing that construct with others – that is, establishing symbolism.

So long as deceptive displays are staged only because a local female is really menstruating, such signals are not displaced but tied to here-and-now context. Symbolic cultural evolution takes off when cosmetic displays are staged as a default – a matter of *monthly, habitual performance,* irrespective of whether any local female is actually menstruating. Once such regularity has been established, females have effectively created a *communal construct o 'fertility' or 'Blood'* – no longer dependent on its perceptible counterpart. Body-painting within groups repeatedly creates, sustains, and recreates this abstract construct. Such energetically costly repeated ritual must be linked to the level, regularity and kind of male provisioning effort it engenders. We therefore predict that data interpretable as evidence for regular female ritual performance will correlate with the onset of a symbolically structured sexual division of labour.

'Home base'/'sex strike' strategy

Our model rests on the value to females of cosmetic menstrual signalling *to attract and retain male support.* But, no matter how amplified, the signal would not motivate males to embark on long-distance, logistic hunts or foraging expeditions; on the contrary, it should promote mate-guarding. 'Sham menstruation' would function to mobilize male mating effort on condition there were sufficient gatherable resources in the vicinity to permit area-intensive foraging. It is consistent with fairly similar foraging strategies between the sexes, where females travel with males for hunting of no more than small to medium game. For females burdened with increasingly dependent offspring, however, one of the key means to alleviate reproductive stress would be to reduce activity levels, especially energetic costs of travel (Prentice & Whitehead 1987).

Other than reduction in mobility, conceivable solutions to the problem of female resource stress include extra female robusticity and increased female foraging autonomy from males, as some archaeologists (e.g. Binford in Fischman 1992; Soffer 1992) posit for the Neanderthals in Europe. Another option might be to stay in biotically rich environments permitting area-intensive foraging by relatively self-sufficient females *(cf.* Soffer 1992 on eastern European Neanderthals). Our point is that any increased seasonal dependence on meat will increase female energetic costs by compelling them *either* to hunt for themselves *or* travel alongside mobile male hunters. A radical solution would be to cease 'chasing after' game animals or those hunting them and instead *make the meat move.* This would involve investing more heavily in camp sites and refusing sex to all males except those returning 'home' with provisions. Knight (1991) has termed this the 'sex strike' option. We posit that pressures to transform 'sham menstruation' into such a 'sex strike' strategy would have built up during a glacial rather than interglacial cycle. Reproductive stress motoring 'sham menstruation' may have become most acute in the period 160-140 kya, the height of the Penultimate Glacial cycle (Jouzel *et al.* 1993).

Lack of adequate dates and data precludes specifying whether humans during the Penultimate Glacial had already switched to a 'sex strike' strategy with the implication of 'home base' behaviour. But when we do find archaeological evidence for structured hearths or other signs of investment in home bases (Stringer & Gamble 1993, 154-8), an implication is that females were establishing greater residential stability, with the corollary that they were motivating their mates to take on proportionately more of the energetic costs of foraging.

The idea that well-built dwellings and structured hearths were established under female pressure may seem a novel one. But without such pressures we would not expect males to be motivated to return regularly to the same base camp – a commitment which would drastically restrict the foraging range of any hunting band which had to move everywhere *as a group.* The costs of such a commitment could have been outweighed by benefits only given some sexual-political arrangement liberating males to depart and (where necessary) stay out overnight on long-distance hunting trips, free of sexual distractions and childcare encumbrances. Such a 'logistic' configuration *(cf.* Binford 1989) involves a restructuring of sexual relations and therefore implies a definite female *strategy,* in which sexual access is made dependent on males' bringing meat 'home'. Some such pattern is standard among contemporary hunters and gatherers (Knight 1991, 139-53; Collier & Rosaldo 1981 and refs.). Recent ethnographic work confirms that hunters focus on large game because marital *and extramarital* relations are best pursued by such means (Hill & Kaplan 1988; Hawkes 1990; Hawkes *et al.* 1992).

It might be objected that all this is to overestimate the significance of female strategies. Why not give male priorities equal weight?

Any focus on evolutionary *change* obliges us to adopt something of a female bias. This is because according to standard socioecological models, changing ecological variables drive changes in mammalian mating systems via changes in *female* strategies, not male ones (e.g. Jarman 1974). Primates are no exception (Crook & Gartlan 1966; Clutton-Brock & Harvey 1977; Wrangham 1980). Male primates tend to prioritize the search for fertile females over the search for food. Mothers, by contrast, distribute themselves, independently of males, in accordance with ecological variables, prioritizing *resources* to feed their young. Within limits set by phylogenetic history, the mating system which emerges depends, consequently, on whether females forage in isolation or in groups and on the nature of interfemale resource competition (van Hoof & van Schaik 1992). Whether such factors as paternity certainty are high or low depends, then, on a mating system whose essential features have been for the most part female-defined.

Unless there exist good reasons for making an exception, we must assume that human groups during the Pleistocene conformed with standard models. Not only Plio-Pleistocene processes but those leading up to the 'symbolic explosion' require analysis in the light of Darwinian theory. In particular, researchers locating the emergence of human symbolic culture in a 'social revolution' (Gamble 1993; Stringer & Gamble 1993; 1994) should suspect *female* strategies driving this *(cf.* Power & Aiello in press).

Perhaps the most decisive advantage of a 'female manipulation' model is that it can explain the emergence of collective deception – hence ritual, language and symbolic culture generally. It is this which previous male-biased models have been unable to achieve.

Dynamics of 'sex-strike'

In order to develop predictions which can be tested, we turn now, to the internal dynamics of 'sex-strike' *(cf.* Knight 1991) as an abstract model. The model is testable because it is difficult, conceptually, to make it work. Indeed the constraints appear tight enough to exclude all but a singular configuration which in principle *could* have worked.

Firstly, the strategy implies intense levels of sexual solidarity. When a female is defying a 'lazy' male partner and is determined to make him link up with other males and hunt, she must prevent him from finding sex elsewhere, organizing – jointly with local females – a boycott of him unless or until he departs and returns with meat. What factors will influence the timing of such action? Any regular pattern must harmonize with female reproductive rhythms. Fitness considerations preclude signalling 'No!' while ovulating. If signals are to be both *collective* and *negative,* menstrual periods (real and sham) must overlap. Joint periodic resistance to male advances should generate female solidarity and interdependence, optimizing menstrual synchrony within each camp *(cf.* Graham 1991). Using blood and/or pigments, menstruating and non-cycling females now implicate one another in the same action, whole groups signalling 'menstruation' together. To the extent that this announces regular periodic 'strike' action, 'taboo' now becomes a property of this signal.

But how to prevent males secretly eating their own kills out in the bush? To draw on the symbolic configuration already in place to prevent this, females can exploit the natural fact that *hunted game animals visibly bleed.* This would have been difficult without a previous history of 'sham menstruation', establishing that red colorants of one kind could substitute for colorants of another. But given such a tradition, the blood of the hunt as a public, communal construct signals 'menstrual blood', the symbolism of this prompting comparable avoidance.

Women could not have benefited economically from such blood taboos unless they had some means to *remove* visible blood from raw meat. Since we have specified women as those most heavily investing in campsites, they would be the most reliable custodians of cooking fire. With such fire under domestic control, women have an important resource complementing the efficacy of blood taboos. Men who have just killed a game animal are inhibited by the blood from eating it. To remove its 'rawness', they must bring the meat home to be 'cooked'-whereupon it passes into female hands *(cf.* Lévi-Strauss 1970). Given such arrangements, cheating by hunters should be minimized, and reliable provisioning will permit the formation of relatively large and stable residential groups.

To counter outgroup male attempts at rape, harassment or other threats to their periodic strike action, females must draw on the support of coalition allies including male offspring and kin. To prevent highly mobile males from sexual cheating, females must maintain synchrony not just locally but across the landscape. This implies phase-locking to a universally accessible external natural clock. The only clock of appropriate periodicity is the moon. This compounds the improbability of the model. The whole system can only work if collective hunting is a periodic work/rest activity governed by a *monthly* on/off rhythm, with the proceeds of each large, ceremonially prepared 'special' hunt augmented during the rest of the month with food from less organized kinds of foraging/scavenging.

Lunar/menstrual time is most simply structured through bisection, yielding a waxing and a waning half of each month. A strike is an all-or-nothing event, either 'off' or 'on', giving two possibilities: 'on' during waning moon while 'off' during waxing, or vice versa. Action during waning moon would schedule the climax of hunting, butchering and transporting activities within the darkest portion of each month. Since this would limit the effective day length available to complete such activities, we are led to posit the reverse polarity – strike action during waxing moon, climaxing with the return of the hunt by or around full moon. As 'on' switches to 'off' at this point, fires are lit, meat is cooked and marital relations resumed (Fig. 1). Ritual signals cross-culturally should reflect this binary on/off logic, 'on' coinciding with crescent moon, 'off' with the moon's waning *(cf.* Knight 1987; 1991).

Symbolic revolution as 'counterdominance'

Current models associate the rapid evolutionary expansion of the hominine brain with greater Machiavellian intelligence (e.g. Dunbar 1993). An authoritative recent suggestion is that hunter-gatherer type egalitarianism was eventually established because the capacities of dominant individuals to exploit subordinates became increasingly well matched by group members' 'counterdominance' capacities; under such conditions, a strategy of 'playing fair' – resisting dominance by others while not fighting for dominance oneself – became evolutionarily stable (Erdal & Whiten 1994).

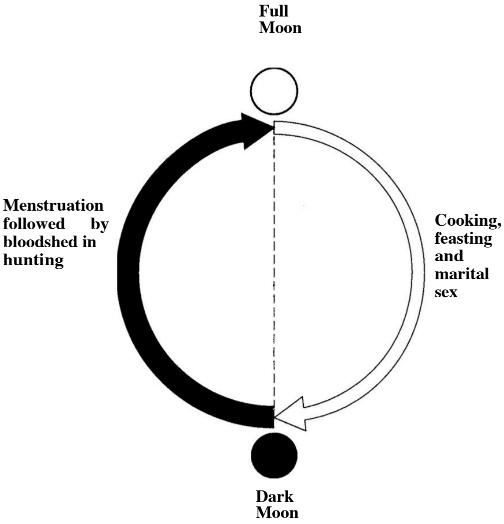


Figure 1. *Lunar-phased alternation between work and rest, ritual and profane time for a model early Upper Pleistocene community.*

There are grounds for linking counterdominance with an increasing reliance on low-cost vocal-auditory communication in place of energy-expensive gesture and manual grooming *(cf.* Aiello & Dunbar 1993). We have seen how, in conflict situations, low cost signals are mistrusted and ignored by listeners *precisely on account* of their low costs. If we accept that Pleistocene humans nonetheless *did* rely increasingly upon efficient vocal-auditory communication (e.g. Lieberman 1991), we can only assume that through counterdominance strategies minimizing such sources of conflict as inter-male reproductive differentials (Turke 1984; 1988; Power & Aiello in press), humans were establishing unusually extensive, stable, kin-based coalitions fostering corresponding levels of mutual trust. Provided *internal* deception remained relatively rare, conditions for the evolution of conventional signalling might have been established, yielding a growing 'proto-language' of coded terms for social! environmental reference *(cf.* Bickerton 1990).

Darwinian theory, in any event, locates the evolutionary roots of speech in selection acting upon *cooperative* individuals. Over the generations, listeners in the human case have evidently *needed to know:* otherwise they would not have evolved such specialized neurophysiological adaptations for decoding messages accurately at low amplitudes, requiring minimal redundancy and at astonishingly high speeds (Lieberman 1991; Pinker & Bloom 1990 and refs; Pinker 1994). Admittedly, the system *can* be used for lying; indeed, reliance upon speech renders listeners highly vulnerable. But in view of the low cost, conventional design features of the system, internal deception can be ruled out as a factor driving its evolution.

By contrast, *ritual* in human cultures demands seemingly disproportionate energetic investments (Sperber 1975). Like their animal counterparts, human rituals are loud, multi-media displays, stereotyped and prone to massive redundancy (Rappaport 1979, 173-246). They are also characteristically illusion-inducing or 'deceptive' (Sperber 1975; Lattas 1989). The difference is that animal manipulative displays are individualistic and competitive, whereas their most potent human counterparts in traditional cultures are quintessentially *collective* performances. They demarcate social relations of power, identifying groups with common interests and setting them in opposition to other groups (Leach 1954; Cohen 1985). But despite this collectivity, they are also highly manipulative performances. Human groups throughout recorded history have exploited others by using elaborate ritual to overcome their victims' 'sales resistance'. To the extent that such performances convey honest information, it is in ostentatious display of their very costliness – demonstrating the organizers' ability to bear such costs *(cf.* Zahavi 1987). We might infer – in view of the energy expended in relation to the paucity of reliable information conveyed (Sperber 1975, 8) – that such rituals arose as *coalitions of conspirators* strove to 'exploit the muscle power' of others who tended to resist the message.

We are now in a position to begin putting all this together. Hunter-gatherers (and by implication humans since the origins of symbolism) produce both speech and ritual- both co-operative and exploitative signals. If we attempt to relate these two patterns within an evolutionary framework, they appear at first sight to be mutually incompatible. If the relationships at the root of symbolic origins were exploitative, how could they have been cooperative at the same time?

Our earlier exploration of the 'sham menstruation/ sex-strike' model suggests a solution. What was exploited – as the 'Human Revolution' (Mellars & Stringer 1989) got under way – was the muscle-power of males. Those who benefited were females and their offspring.

The first symbolic construct

Provided humans have confidence that they share the same gods, mutual reference to them – we have seen – is theoretically possible. But how did humans acquire such confidence? Appeals to 'speech' in this context *(cf.* Bickerton 1990) lack force. While cryptic vocal signals can *trigger* morally authoritative constructs, they are powerless to do the initial work of implanting and replicating them. To rely on coded signals to implant a construct such as *'Supernatural Potency'* would be hopeless – as futile as using cryptic nods and winks to instil belief in *'God'*. Intangibles, by definition, are not self-evident. One cannot just refer to them. They have first to be emotively *experienced.*

The only known agency capable of ensuring this is *communal ritual* (Durkheim 1965 [1912]; *cf.* Gellner 1992, 36-7). Social anthropologists in traditional cultures have extensively documented the elaborate ways in which collective representations such as 'God' are emotionally charged, made fearsome and given both structure and experiential authority. Ritual collectivizes dreams. Representations of events in the inner world are externalized, socially standardized and replicated. To the extent that a performance has succeeded, every participant should share the same set of fantasies. *Subsequent* to their ritual implantation, these variegated gods, goblins and other beings then comprise an entire world of illusion, within which everyone is immersed. From then on, cryptic mutual reference to these particular illusions can work.

It now possible to explain the goblins. Knauft (1994) notes the obsequious 'submission' displays central to the signalling repertoire of the social great apes, contrasting them with the egalitarian 'don't mess with me' norms of human hunter-gatherers. 'Aversion to submission in human evolution', comments Knauft (1994, 182), 'both between males and between males and females, is particularly important'. The key ape-human distinction, he concludes, 'may be the apes' *willingness to demonstrate subordination'*. Such insights accord closely with 'sex strike' theory *(ef.* Knight 1991). Female mammals, including primates, recurrently signal subordination with a gesture known as 'presenting', in which the most vulnerable organs are offered to a dominant individual for inspection or for copulation (real or simulated). On this basis, we would predict 'counterdominance' to be signalled by reversing such signals. Courtship ritual in the animal world is central to the functioning of the specific mate recognition system (Paterson 1978; 1982); minimally, this involves signalling to prospective partners, 'right species; right sex; right time'. Systematic reversal yields *'wrong species/sex/time'*. This, then, is the predicted signature of counterdominance – the political dynamic driving menstrual inviolability, taboo and the potency of the ritual domain.

It need hardly be stressed that for human females to signal that they were in fact *males,* and of a *non-human species,* would be a fantasy not easy to convey. To overcome listener-resistance, such signalling will therefore be amplified rather than whispered. Transmission will involve energetically expensive, repetitive, highly iconographic pantomime. *Between* conspirators, on the other hand, the reverse logic will apply. Interests being shared, 'conspiratorial whispering' – i.e. low-amplitude, energy-saving, highly encoded signalling – should suffice (Krebs & Dawkins 1978; 1984). Such whispering will govern the staging of those public, amplified and deceptive signals expressive of counterdominance.

The corresponding fantasies, being shared, will *be communicable for the first time.* Coded vocal calls, previously capable of labelling only demonstrable things, can now for the first time label constructs. The model specifies the first symbolic construct as women's assertion of their ritual inviolability through metamorphosis into *'wrong time/wrong sex/wrong species'.* Monstrous therianthropic fictions of this kind are the first 'gods'. Humans now have access to a sociocentric, communal cognitive map whose motivational biases reverse those of ordinary cognition.

In this model, resistance is simultaneously both political and economic – a means of compelling *potential* dominant males to depart, join with other males, hunt and bring back meat. 'Ritual' is this periodic assertion of collective, coalitionary counterdominance – an energy-expensive, amplified way of signalling 'no'. But what is a 'no' to dominance is experienced positively – as a whispered 'yes' to synchronized action – within the counterdominance camp. Speech is this 'yes'. Within this perspective, the two most easily distinguished ethnographically documented ways of communicating – one narrowly vocal, the other exaggeratedly gestural, one 'speech', the other 'ritual' – appear not as successive stages but as interdependent poles of one and the same symbolic domain. Speech is 'gossip' about the representations implanted by *ritual;* ritual generates an entire world of amplified deceptions which being standardized and communal – can now be gossiped about for the first time.

An advantage of this model of speech evolution is that it explains the 'leap' *(cf.* Bickerton 1990) to syntactical complexity. For as long as humans were referring only to *demonstrable* phenomena, complex grammar could not have evolved. Why burden vocalizations with tense and case if spatio-temporal relationships can be gesturally demonstrated or inferred from living context? If the calls which accompany gestures can remain cryptic and unelaborated like knowing nods and winks, each rich with contextual significance – why pack tight, context-independent specifications into long vocalized sequences? Simple calls – each accompanied by gesture and kept for use only in the appropriate time and placewill communicate all that is required.

But apply coded vocal labels to 'the gods' *out* of space and *out* of time – and perceptual recourse fails. Nothing under discussion can now be perceived. The audience is in the dark, unable to *infer* from visible context how such intangibles relate to 'this world' of experience *in* space, *in* time. To specify, one coded call must follow another, each narrowing the range of possible interpretations until sufficient precision has been achieved. While ritually structured life has become the ultimate context and source of meaning, the *immediate* semantic context of each call is now neither life nor gesture – but *preceding and following calls.*

Ritual 'conspiracy' requires not only publicity but also concealment, driving a new dependency on secret codes which are not only cheap but can be *arbitrarily changed* so as to exclude the targets of deception (Englefield 1977, 123). Gesture, being largely self-evident, is in this context a handicap and is therefore rejected in favour of vocal reliance – yielding a system of wholly coded communication effective even in the dark. 'Syntactical complexity', previously a property of motor control and comprehension of mimetic gesture (Armstrong *et al.* 1994; Donald 1991), now invades the vocal-auditory channel, controlling sequences of representations *as if* they were bodily movements *(cf.* Johnson 1987). Exapting neurophysiological capacities for handling a call system still heavily embedded in gesture *(cf.* Burling 1993; Savage-Rumbaugh & Rumbaugh 1993; Goldin-Meadow 1993), syntactical speech now explosively evolves.

Testing the model: the archaeological record

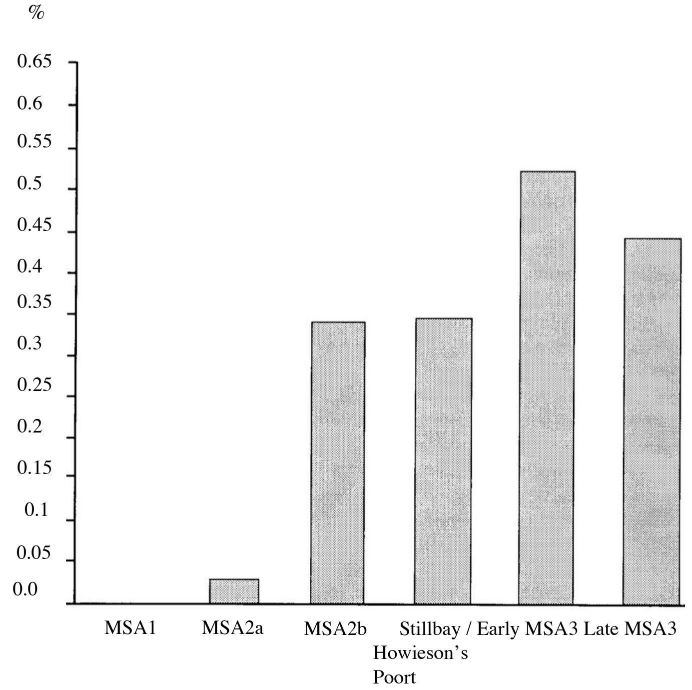
This model of symbolic origins generates predictions potentially falsifiable across a range of disciplines. Here we consider (1) the archaeological record of pigment-use; (2) ethnographic traditions of 'female inviolability'; (3) the specific content of rock art.

We have proposed that the earliest collective constructs – premised on a transition from 'sham menstruation' to 'sex strike' – were amplified menstrual signals, and that these underpinned the emergence of a sexual division of labour. On this basis, red pigments should be the focus of the earliest symbolic tradition. The ochre record should document an initial period of *sporadic* ochre use ('sham menstruation' prompted only by the local incidence of real menstruation) followed by an explosion in such use (reflecting *regular monthly* body-painting, regardless of real menstruation) coinciding with the first evidence for a sexual division of labour.

There are no more than a dozen instances of hominine use of iron oxides predating the Upper Pleistocene (Beaumont 1990a, b, c, d; Bednarik 1990; Clark 1950; Cooke 1963; Jones 1940; Marshack 1981; Roebroeks 1988; Thévenin 1976; Walker 1987). No instance is thought significantly to predate 250-300 kya; seven are from Late Acheulian, Fauresmith and early Middle Stone Age contexts in sub-Saharan Africa. We conclude that the earliest use of iron oxides was by archaic grade *Homo sapiens* rather than *Homo erectus* (contrary to Marshack 1981; Cord well 1985; Velo & Kehoe 1990). While this behaviour is geographically widespread, it is neither regular nor frequent; in all cases we are dealing with either a single or only a few pieces.

From the beginning of the Upper Pleistocene, we have divergent pictures of iron oxide use in Africa and Europe. Focusing on the Middle Stone Age of southern Africa (south of the Limpopo), we adopt Volman's (1981; 1984) chronological scheme based on informal changes in lithic technology and typology.

The MSA1 is thought to span the greater part of Oxygen Isotope Stage 6, the penultimate glacial c. 190-130 kya. Volman suggests that the MSA2a may begin towards the end of Stage 6 (a glacial maximum:]ouzel *et al.* 1993), but it is best known from the beginning of the Last Interglacial, associated with some of the earliest fossils approximating an anatomically modern form. The beginning of the MSA2b is not fully resolved at the two best dated sites of Border Cave and Klasies River Mouth and may lie anywhere between 11S-105 kya. **It** is contemporary with or immediately predates the proposed initial migration of modern humans out of Africa, and lasts for the duration of the Interglacial in the broad sense. The Howieson's Poort dates to



Approximate Chronology	>140	*c.130-110*	*c.110-75*	*c.75-55*	*c.55-35*	*c.35-20*	kyBP
Total lithic assemblage	11,860	60,958	147,198	606,581	186,523	308,987	
No. of sites with unselected samples	2	9	10	14	6	8	

Figure 2. *The symbolic explosion. Ochre frequency as a percentage of total lithic and pigment assemblages in southern African MSA cave/rockshelter sites.*

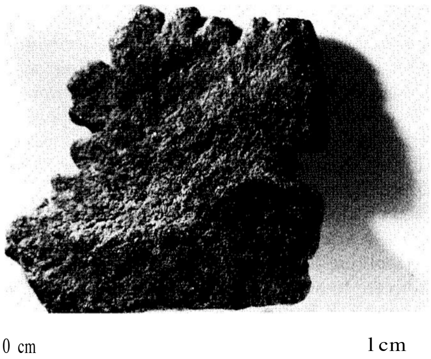


Figure 3. *Notched red ochre from Hollow Rock Shelter, southwestern Cape. Stillbay industry, Unit SA IIIB, Square AD14. Excavated by Ursula Evan 1993.*

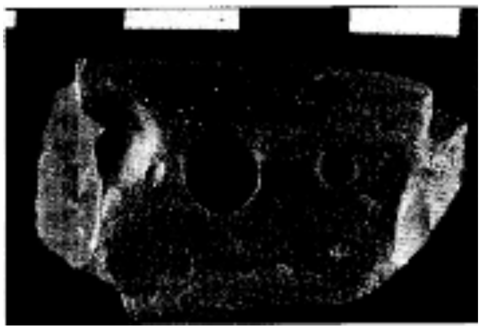


Figure 4. *Drilled and ground red ochre from Klasies River Mouth Shelter 1 A. Howieson's Poort level* 21, *Square B2. Singer and Wymer excavations.*

early stages of the Last Glacial, beginning about 75 kya. We have modified Volman's scheme by subdividing the MSA3, the later stage of which, from about 35 kya, includes contemporaneous classic MSA, early LSA and genuinely transitional industries (Wadley 1993).

Figure 2 represents the aggregated frequency of pieces of ochre as a percentage of unselected lithic and pigment assemblages from caves/rockshelters over the course of the MSA in southern Africa.

Given that ochre occasionally occurs in earlier Fauresmith contexts in the region (Beaumont 1990a; 1990d) and in early MSA contexts just north of the Limpopo Ganes 1940; Cooke 1963), the absence of pigments in the MSA1 can probably be attributed to the negligible sample size. In the majority of MSA2a assemblages, ochre is absent, or only present at very low frequencies; but in two sites there are significantly larger quantities than in any previous context worldwide. We can infer that among some groups, at least, ochre was now in regular use. But the explosion in the use of red ochre with the MSA2b is remarkable, an increase greater than an order of magnitude. From this point on, *copious* amounts of ochre are ubiquitous in cave/rockshelter sites (Watts in prep.). If we remove the factory site of Olieboompoort as a statistical outlier, we can say that from the MSA2b onwards the sample means for each time period pass the 95 per cent confidence limit that they are not attributable to sampling variation alone (Watts in prep.).

The mining of specular haematite at Lion Cavern in Swaziland is believed to date back to the MSA2b (Beaumont 1973; Boshier & Beaumont 1972; Volman 1984). Approximately 99.5 per cent of all MSA pigments are some form of iron oxide, while about 94 per cent have a red streak – despite other metal oxides and other forms of iron oxide being readily available in several regions (Watts in prep.). A low but recurring proportion of utilized pieces have been reduced to produce small working surfaces, edges and points, rather than maximizing the worked surface area, indicating that some pieces were directly applied to demarcate clearly areas or designs of colour. Several MSA2 and Howieson's Port assemblages yield unique pieces of red ochre which have been notched (Fig. 3), drilled (Fig. 4), or scored with simple 'patterns' not explicable in terms of the production of ochre powder. The 'patterns' consist of parallel, convergent, and perpendicular lines, triangles and complex meanders (Fig. 6). The notched pieces of ochre in particular, from Stillbay and Howieson's Poort contexts, strongly suggest symbolic traditions.

The only parallel to the explosion in ochre use witnessed in the MSA2b occurs 60-70 ky later, with the Eurasian Middle/Upper Palaeolithic transition. For the rest of Africa this may simply reflect the less intensive history of research, as ochre is reported from a number of Last Interglacial or later MSA/ Middle Palaeolithic assemblages in eastern and central Africa (e.g. Wendorf *et al.* 1993; Clark & Williamson 1984; Phillipson 1976; Mehlman 1979; Dart & del Grande 1930/31; Cooke 1957; 1963; 1971; Leakey 1943; Jones 1933; 1940).

The contrast between this African data and the pre-Upper Palaeolithic European data is marked. Couraud (1991) lists 17 French Middle Palaeolithic or Mousterian sites where pigments have been reported. Many of these were excavated in the early decades of this century, so details are scanty, but a few generalizations are possible. The vast majority appear to come from late Mousterian contexts, between approximately 70-34 kya. The quantities involved are generally slight compared to both the southern African MSA sites and the Eurasian Upper Palaeolithic. Equally striking is that the predominant metal oxide is black manganese (Couraud 1991, 38). Apart from the Tata plaque, we are aware of no pre-Châtelperronian materials resembling the South African material illustrated here. It is only during the Châtelperronian, when Neanderthals are widely believed to have been in direct competition with the newly arrived 'modems', that we witness a dramatic increase in the use of red ochre, best illustrated at Arcy-sur-Cure (Couraud 1991).

Ochre use: competing hypotheses

Since 1978 a number of functional hypotheses have been proposed for ochre use (e.g. Keeley 1978; 1980; Audouin & Plisson 1982; Moss 1983; Vela 1984; Cord well 1985; Lavallee *et al.* 1985; Bahn & Vertut 1988; Dumont 1988; Couraud 1991). Foremost among these has been the hide-preservation hypothesis (Keeley 1978; 1980), which draws on Mandl's (1961) work on the preservative effect of metal oxides in inhibiting the bacterial breakdown of collagen. Bahn (Bahn & Vertut 1988) has taken Keeley's arguments further, suggesting that such functional uses preceded any symbolic use.

In an unpublished but comprehensive critique of what had become a substantial body of archaeological research attempting to demonstrate a technical role for ochre in hide processing, Volman (1988) argued that both the experimental and ethnographic precedents which had been used to support this archaeological interpretation were highly misleading. He also argued that the microwear studies of ochred lithics did not support the claim that scrapers were used on already ochred hides. It is worth noting that the few artefacts in question were all from Upper Palaeolithic or later contexts; consequently, even if we were to accept Keeley's interpretation, it would not support Bahn's suggestion that such uses preceded any symbolic use.

Rather than expand on Volman's critique, we will draw out some other implications of the hide-protection hypothesis.

Iron oxides neutralize the action of collagenase, an enzyme responsible for the breakdown of collagen (Keeley 1980; Cordwell 1985, 40). Mandl (1961, 196) noted, however, that *all* metal oxides inhibit collagenase. We would expect, therefore, that iron oxide frequency relative to other metal oxides, such as manganese, would reflect local availability. Within the class of iron oxides, haematite and red ochre frequency should also reflect local availability relative to the wide range of other iron oxides, such as magnetite, which do not produce a red streak. While the pre-Châtelperronian classic Neanderthal data may meet this expectation, the same cannot be said of the southern African data.

Ethnographic data on the use life of different kinds of hide garments in the Kalahari (Silberbauer 1981, 225-6) indicate that we

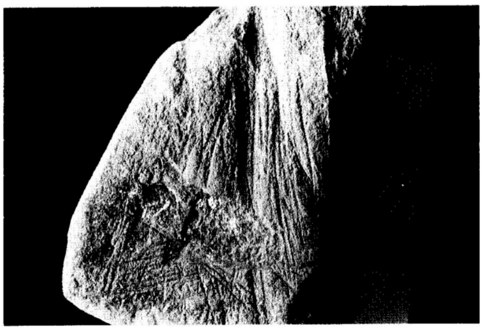


Figure 5. *Gouged red ochre. Klasies River Mouth Cave 1, MSA I (MSA2a). Layer* 33. *Singer and Wymer excavations.*

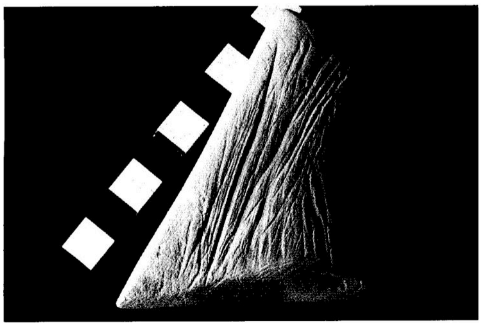


Figure 6. *Ground and gouged red ochre. Klasies River Mouth Cave* 1. *MSA II (MSA2b). Layer* 26. *Singer and Wymer excavations.*

would have to presume a very rapid rate of bacterial decay to outstrip the documented rates of normal wear and tear.

If pigments were being applied to large surface areas such as hides, then whether they were powdered or rubbed directly onto the surface, we would expect a form of utilization which maximized the surface area of any single ground or rubbed facet. But some of the southern African pieces have sharp points and honed edges.

The hide preservation hypothesis has at least two implications for the temporal patterning in the frequency of ochre in the archaeological record. If widely dispersed archaic *Homo sapiens* were aware of some technical application for iron oxides (to account for the late Middle Pleistocene record), we might predict a gradual curve in the spread of such a utilitarian innovation. It is inconceivable that either the MSA2b explosion or that witnessed across the European Middle/Upper Palaeolithic transition could be explained by a sudden desire or need to utilize pre-existing knowledge of how to extend the use life of cloaks and coverings. The second temporal implication is that the frequency of ochre use should correlate with climatic conditions. We would expect increased use of hide clothing and, therefore, higher ochre frequencies in colder periods. There is no such correlation. Although the explosion in ochre use in the MSA2b correlates with a cooling during the Interglacial, there is no subsequent data to indicate that this was a recurrent pattern. Where fine grained data are available, as with the charcoal analysis from Rose Cottage Cave in Lesotho (Harper 1994), the correlation is the reverse of that expected, with the lowest ochre frequencies coming from the coldest part of the Howieson's Poort, despite intense occupation throughout the Howieson's Poort levels.

While we cannot state that pigments were never used to preserve hides, we can be confident that any such use was marginal or secondary. We seem obliged, therefore, to fall back on the old 'invisible' archaeological stand-by that Middle Palaeolithic/ MSA peoples used ochre primarily to decorate their own bodies and possibly other organic materials, a conclusion also reached by Volman. This regular and copious use of red pigments, witnessed from at least 110 kya in Africa – and possibly extending back as a regular but less copious practice to c. 140 kya – should be regarded as the earliest symbolic tradition *(cf.* Soffer 1992; Chase & Dibble 1987; 1992).

We have presented a neo-Darwinian processual model which would predict symbolism's emergence in this form. To that extent, we are able to go beyond appeals to the universal salience of red (Velo & Kehoe 1990; Berlin & Kay 1969) and the earlier symbolist position which drew on random ethnographic precedents to suggest menstrual blood as the ultimate source of such salience (Dart 1968; Boshier & Beaumont 1972; Wreschner 1975; 1980). We have shown why symbolism presupposes ritual traditions. Integrating the above data with the model's predictions, some type of 'sham menstruation' behaviour may have been common to all archaic *Homo sapiens.* But it is in Africa, approximately coincident with the evolution of early modern humans, that this initially context-dependent strategy was raised to the level of habitual performance, establishing a symbolic ritual tradition.

A symbolically structured sexual division of labour?

Focusing on Eurasian data, Stringer & Gamble (1993) suggest that the foundations of a 'modern' hunter gatherer behavioural repertoire were laid in a 'pioneer' phase of colonization by anatomically moderns in the period 60-40 kya. They argue that before this there is no convincing evidence for the organization of behaviour according to symbolic codes. Instead of campsites, they see locations repeatedly used for the processing of animal carcasses. They see ash spreads instead of structured hearths, limited technical facilities for food processing, and no symbolic traditions.

Models positing a recent African origin for modern humans will have to pay greater attention to the limited MSA record than hitherto (Willoughby 1993). Many of the archaeological features which Stringer & Gamble identify with 'moderns' campsites would not be expected in low-latitudes *(cf.* Brooks & Yellen 1987; O'Connell *et al.* 1991; Binford 1980; 1990). What evidence there is suggests that from the Last Interglacial, MSA peoples used caves/rockshelters as campsites (e.g. Deacon 1992; Henderson 1992; Opperman & Heydenrych 1990). The structure of hearths in oxygen isotope Stages 5 and 4 (Henderson 1992; Deacon 1983, 174) are essentially the same as described by O'Connell *et al.* (1991) for the Hadza. It has been claimed (Stiner 1993, 71) that the MSA, like the Mousterian, lacks elaborate food processing facilities such as grinding and pounding implements.

While largely true for Europe and the Near East, this is incorrect for Africa, where MSA/Mousterian groundstone assemblages have been widely reported, almost exclusively from Last Interglacial and later contexts (e.g. Clark 1971, 1216; Beaumont 1978, 271; Brooks *et al.* 1990; Deacon 1983, 173; Deacon *et al.* 1987,149; Evans 1993; Kuman 1989,69; Louw 1969; Mason 1962, 273; Mason 1988, 301, 648-58; Tobias 1949; Wadley & Harper 1989; Wendorf *et al. 1993).* The use of ostrich eggshells as water containers with clear implications for logistical organization was once believed to be restricted to the LSA, but recent research indicates that this practice also extends back to the MSA2b (Vogelsang 1993). Further afield, it has been argued (Lieberman & Shea 1994) that early modems in the Levant practised greater seasonal scheduling of residential moves than Neanderthals in the same region, possibly reducing anatomical stress.

In terms of animal procurement strategies, Neanderthals and early modern humans may have been on a par. Both were perfectly competent at selectively hunting prime-aged adult animals in the medium body-size range and sometimes larger (e.g. Klein 1989; Brink 1987; Gautier 1993 *re* Africa; *cf.* Stiner 1993 for a general review). There is nothing to suggest, however, that early modern human hunting and scavenging activities were spatially and temporally separated in the manner which has recently been inferred from some Neanderthal data (Stiner & Kuhn 1992). More importantly, at issue are the sexpolitical relations determining access to these resources. Such relations may be beyond our grasp, but in the light of ‘sex strike’ theory, African archaeological and ethnographic data offers some interesting insights.

Our model accords dry-season nutritional stress a critical role in shaping the coalitionary strategies of African early modem human females; for internal reasons, it further specifies full moon as the optimal time for the return of the successful hunt.

Among extant Khoisan and Hadza hunter-gatherers, the most productive form of hunting consists of dry-season night-stand hunts over game trails leading to remaining water sources. Such techniques are further restricted to the nights leading up to full moon (Brooks & Yellen 1987; Bunn *et al.* 1988; Crowell & Hitchcock 1978; Hawkes *et al.* 1992). Although poisoned arrows are generally used, spears are kept in the blind both to finish off the wounded animal and as a protection against nocturnal predators (Crowell & Hitchcock 1978, 38). We may infer great antiquity for this strategy. A number of African and Levantine Middle Palaeolithic/MSA pan-margin assemblages have been identified as specialist hunting locations (Brink 1987; Brooks & Yellen 1987; Brown 1988; Gilead & Grigson 1984; Rabinovich 1990; Wendorf *et al.* 1993). Where season of use has been inferred, it has invariably been the dry-season (Gilead & Grigson 1984, 93; Rabinovich 1990, 212; Wendorf *et al.* 1993,571). Although Wendorf *et al.* (1993,570) propose that such sites were only used diurnally because of the danger from predators, this can be countered on the grounds that the predator defence used by contemporary night-stand hunters – a hafted spear – would have been available to MSA forebears. Furthermore, the productivity of this form of hunting is based on exploiting the nocturnal drinking habits of a number of ungulate species.

Neither the campsite evidence nor that relating to hunting strategies points conclusively to a sexual division of labour. There are limited morphological data, however, which have been interpreted as indicating greater division of labour by age and possibly by sex among Levantine early modern humans than among broadly contemporary Levantine Neanderthals. Trinkaus’ comparative study of femoral neck-shaft angles led him to suggest that early modern human children in the Qafseh-Skhul group may have participated far less in day-to-day foraging-related mobility than their Neanderthal counterparts occupying the same region. This implies that they were receiving more provisioning and care from a subset of the adult population in or near the sites (Trinka us 1993; 1994).

Both Trinkaus’ femoral neck-shaft analysis and Lieberman and Shea’s (1994) seasonality research can in turn be related to functional analyses of skeletal remains. These indicate generally lower levels of strength, endurance and use of the anatomy to perform habitual tasks among the Qafseh-Skhul group than any Near Eastern Neanderthals (Trinkaus 1993, 397 and refs.). Taken in isolation, such functional differences could be attributed to the different environmental constraints on the evolutionary histories of the two groups. But taken together, significantif poorly understood – differences in social organization are implied. While Trinkaus’ analysis is suggestive rather than conclusive, reduced levels of mobility among age-specific sub-sets of the population would be predicted by our model, in which females resort to strike action precisely to minimize the energetic costs of foraging-related travel. The technology and subsistence strategies of the two hominine groups show little differentiation. How, then, could a sub-set of the adult early modern human population have afforded to spend more time at home caring for children – unless those carers themselves were receiving greater provisioning from further afield?

Finally, we reject the assertion that there is no evidence for symbolic traditions prior to c. 60 or 40 kya. While in Europe such a claim appears well founded, in southern Africa evidence for symbolic traditions extends back approximately 100 kya. This is quite independent of our interpretation of the ochre record from c. 130 kya. Serially notched bones were recovered from MSA2b levels at Klasies River Mouth (Singer & Wymer 1982) and Apollo 11 (Wendt 1974). While a number of engraved bones are reported from the European Middle Palaeolithic (*cf.* Bednarik 1992), the markings on these appear to be unique. There is nothing to indicate that they are part of a wider system that used repetition of design and shape as symbols for action; it has been suggested that some may be the unintentional by-products of other activities (Chase & Dibble 1992). What is striking about the southern African serially notched artefacts, by contrast, is their formal similarity and the temporal depth of the tradition – similar pieces have been recovered from MSA3, early LSA, and Holocene LSA contexts (Marshack 1978; Watts in prep.). Regardless of their specific interpretation, these fulfil the criteria set by Chase & Dibble for a symbolic tradition. Mention should also be made of the engraved ostrich eggshell fragments from Howieson's Poort levels at Diepkloof (Yates pers. comm.) and from Apollo 11 (Wendt 1974); and the perforated *Conus* shell associated with the MSA2b infant burial at Border Cave (Cooke *et al.* 1945; Beaumont *et al.* 1978), believed to date to c. 80 kya (Grun *et al.* 1990). It is worth emphasizing that the only *unequivocally* symbolically elaborated burials predating the Upper Palaeolithic are of early modem humans (Cooke *et al.* 1945; McCown *1937 re* Skhul V; Vandermeersch 1970 *re* Qafseh 11).

In summary, we suggest the following:

2. from the onset of the Last Interglacial, MSA caves/rockshelters were used in ways more closely approximating campsites than Stringer & Gamble's interpretation of pre-'pioneer' phase cave assemblages;
3. from the MSA2b, the regular use of grindstones indicates more complex processing technologies among early modern humans than among Neanderthals;
4. there is indirect evidence that lunar phase-locked hunting was a significant component in early modern human animal procurement strategies during the dry season, when females would have been under greatest nutritional stress;
4. there is limited morphological evidence to suggest greater division of labour among early modern humans than among Neanderthals;
5. early modem humans prior to c. 60 kya were already organizing behaviour according to symbolic codes, with symbolic traditions including recording systems, symbolically elaborated burial and, above all, red ochre body painting.

Ritual traditions of 'female inviolability': a timeresistant syntax

Our model predicts *periodic female inviolability* as a discernible focus of early hunter-gatherer ritual traditions. Menstrual taboos meet this expectation, being sufficiently widespread and invariant to indicate extreme antiquity (Knight 1991 and refs). Predictably, the taboos recurrently concern hunting luckthe severity of observances varying cross-culturally with dependence upon hunting (Kitahara 1982).

Ritual potency more generally is predicted to display everywhere a characteristic signature, revealing its ancestry in menstrual inviolability. Power should be switched 'on' by one set of signals, 'off' by another:

ON	OFF
Loud signals	Weak signals
Waxing moon	Waning moon
Seclusion	Availability
'Other world'	'This world'
Night	Day
Wet	Dry
Bleeding/ raw	Cooking/ cooked
Hunger /being eaten	Feasting
Flesh taboo	Flesh available
Production	Consumption
Kinship	Affinity
Gender inversion	Heterosexual polarity
Animality	Humanity

This is a tight set of constraints. It means, for example, that a menstruant ('on') may amplify 'blood' by signalling 'hunger', 'kinship intimacy', 'gender inversion' and/or 'animality' (all 'on'). But she *cannot* enhance her potency by being seen in bright light, on dry ground, with her marital partner or by a cooking fire (all 'off'). From one culture to another, political factors will naturally alter ideological *meanings,* that is, the positive or negative valuation of terms. Menstruation, for example, may appear as 'supernatural potency' or as 'pollution' according to women's political status. But through all such variation, we expect ritual traditions relentlessly to define menstrual potency as incompatible with feasting, strong light, cooking *or any other signal from the 'off column.* We term such formal consistency – unchanging across all cultures and all historical periods – the *time-resistant syntax* of symbolic ritual and myth.

Focused as it is on origins, the model does not necessarily predict the survival into the present of *behavioural* aspects of the posited 'initial situation'. For example, we do not expect lunar cycle menstrual synchrony under contemporary conditions. Nor do we necessarily expect hunting behaviour to climax around full moon. The model concerns public, ritual signals, and their discursive reflection in myth. Wherever surviving myth and ritual have *anything to say* about the raw and the cooked, the moon, menstruation, abstinence, hunting and so forth, we expect the rules to match the improbable specifications of our model. Social anthropologists, notably Lévi-Strauss (1970; 1973; 1978; 1981), have documented widespread mythic and ritualistic preoccupation with precisely such signals, rendering our predictions empirically vulnerable.

We tum, now, to a fine-grained, specific ethnographic test. Southern African archaeologists widely agree that significant continuities in San material culture extend back about twenty-five thousand years the duration of the Later Stone Age. Moreover, mtDNA and nuclear DNA studies indicate greater time-depth for Khoisan lineages than for any other aboriginal populations in the world (Vigilant *et al.* 1991; Soodyall & Jenkins 1992; Wainscoat *et al. 1986;* Bowcock *et al.* 1991). In addition to the Khoisan, our discussion will include the Hadza since they are the only extant low latitude hunter-gatherers whose hunting is primarily focused on big game (Hawkes *et al. 1992).*

Hunting, sex, menstrual observances and lunar periodicity

The dry season marks the phase of Hadza social aggregation associated with their most sacred rituals – the *epeme* dances held on each night of the dark moon for the duration of the aggregation. All camp fires *are* extinguished and the women call upon each man in turn to dance, referring to him exclusively in consanguineal kinship terms (Woodburn 1964; 1982). In Hadza belief, women synchronize menstruation with dark moon (Woodburn pers. comm.), hence at the time of *epeme* rites. Dorothea Bleek's (1930, 700) account, in which women are said to become 'ill' when they see the moon, also links menstruation with the new moon's appearance. The dance emphasizes gender segregation cross-cut by kinship solidarity; it is believed to ensure health and success in forthcoming hunts, when portions of the fattiest meat will be offered in bride service. A coherent pattern emerges from the following: (1) Hadza men should neither hunt nor have sex during their wives' menstrual periods (Woodburn 1964; 1982); (2) the most successful hunting in the dry season occurs around full moon; (3) menstruation occurs normatively at dark moon, concurrently with the most sacred ritual. The 'improbable' specifications of our model are here met not only on the level of ritual syntax, but also in actual hunting practice.

Historical and ethnographic accounts explicitly refer to full-moon hunting by Khoisan (e.g. Livingstone 1857,165; Wikar 1935, 103; Potgeiter 1955, 11); other accounts describe nocturnal hunts where some lunar phase-locking has to be presumed (e.g. Stow 1905, 149-50; Livingstone 1857, 161-2; Bleek & Lloyd 1911, 283; Valiente-Noailles 1993, 63). The literature is replete with references to beliefs linking success in the hunt to lunar periodicity (e.g. Kohler 1978/79; Viegas Guerreiro 1968, 97, 297; Esterman 1976, 17; Silberbauer 1965,101; Bleek 1920-21, 302, 328, 455, 469; Lloyd V6,5206-40). Where details are given, it is always the waxing phase which is associated with hunting success.

Like the Hadza, Khoisan groups normatively link menstrual and lunar periodicities (Shostak 1983, 163; Biesele 1975 I, 164; Schmidt 1979, 62; Bleek 1928, 122; Lloyd VI-2, 4001, 4059; Valiente-Noailles 1993, 94-7). Where details are given, the dark moon is associated with menstruation and a more generalized conception of 'death' and 'illness'. The linkage is not only metaphorical but structures ritual practice. The /Xam, the !Xu, and the Kua would not release a menarcheal girl from seclusion until the appearance of the new moon (Lloyd VI-2, 4001-2; Bleek 1928, 122; Valiente-Noailles 1993, 94-7). There is some indirect evidence for behavioural menstrual synchrony (Metzger 1950, 73; Heinz 1966, 116), although the only firm evidence concerns belief. Among the Zu/'hoasi (formerly the !Kung), it is said 'that if a woman sees traces of menstrual blood on another woman's leg or even is told that another woman has started her period, *size* will begin menstruating as well' (Shostak 1983, 68).

As with the Hadza, the Khoisan widely warn men against hunting while their wives are menstruating – to do so is to risk becoming the prey (e.g. Biesele 1993, 93). Nor should a man have sex at such a time (Shostak 1983, 239). He should avoid sex also when due to hunt large game (Biesele 1993, 196; Schmidt 1986, 330--31; Schultze 1907, 203) or if about to resume tracking the spoor of a wounded animal (Marshall 1959, 354 fn. 1; Kohler 1978-79, 26). Linked with beliefs about the effects of lunar periodicity on hunting and menstruation, all this suggests an underlying pattern of abstinence from sex or hunting during menstruation around dark moon, prior to big game hunts in the waxing-to-full moon period when light is optimal. During an eclipse of the moon some groups would abandon all hunting (e.g. Hahn 1881, 89). Andrew Smith gives an account from nineteenth century Khoisan in the northern Cape illustrating how marital sex could be made collectively conditional upon men returning home with meat:

> The Bushmen when they will not go out to steal cattle, are by the women deprived of intercourse sexual by them and from this mode of proceeding the men are often driven to steal in opposition to their better inclination. When they have possessed themselves by thieving of a quantity of cattle, the women – as long as they (the cattle) exist – appear perfectly naked without the kind of covering they at other times employ. (Smith notebook 4, p. 77 rev.)

The most appropriate term for such collective action would be ‘sex strike’.

‘Wrong sex, wrong species’ in southern Africa

Instead of signalling *‘right* species/sex/time’, females on ‘sex strike’ – we have argued – should signal *‘wrong* species/sex/time’. In asserting their ritual inviolability, women should construct around their blood-signal *(‘wrong time’)* communal fantasies of being ‘male’ and ‘animal’. The menstruant should now be in a paradoxical position. She initiates the female collective ritual prompting males to prepare for hunting. Yet in ‘changing species’ she becomes the quarry itself, her blood automatically constructed as the blood of the wounded game.

How do these expectations match the ethnographic data? Metaphorically and in ritual practice, Hadza and Khoisan menstruants become hunters that is, symbolically ‘male’. The Hadza say ‘She has shot her first zebra’ (Woodburn pers. comm.); the Zu/’hoasi say ‘She has shot an eland’ (Lewis-Williams 1981,51). Yet despite being cast as ‘the hunter’, upon emergence the Zu/,hoasi and /Xam menstruant must *play tire part* of the antelope, lowering her eyes so the animal will not see the hunters’ approach (Bleek & Lloyd 1911; Lewis-Williams 1981). A !Xo menarcheal girl shoots a ritual arrow at a gemsbok shield suspended at the back of the seclusion hut to bring luck to the weapons; on emergence, her face is painted with a gemsbok mask (Heinz 1966). Solomon’s (1992; 1994) analysis of gender in San rock art focuses on the recurring motif of ambiguously female figures with menstrual flows – ambiguous because of their therianthropic features and/ or male attributes such as a penis or hunting equipment (Fig. 7).

Returning to the Hadza, *epeme* is not only the name of the dark-moon ritual but also refers to portions of fatty meat exempted from bride-service obligations and reserved for secret consumption by men in the name of ‘God’. A Hadza narrative (Woodburn 1964) portrays the original owner of the sacred *epeme* meat as an old woman, Mambedaka, who dressed as a man, hunted male zebra, and wore the penis of her kills, using this to have sex with her ‘wives’. Male hunters had to surrender their meat (the *epeme)* to Mambedaka for distribution among these women. Men were excluded from any share until the old woman’s violent overthrow. Mambedaka playing ‘God’ with her zebra’s penis matches precisely the *‘wrong sex, wrong species’* construct central to our origins model (for similar myths of ‘primitive matriarchy’ worldwide see Knight 1991).

Probably the best known and most widespread aspect of Khoisan menarcheal ritual is the ‘Eland Bull Dance’, performed during the final days and nights of menstrual seclusion and upon the emergence of the ‘new maiden’ (e.g. Lewis-Williams 1981; Valiente-Noailles 1993). Men are sometimes totally excluded; alternatively, one or two older men in affinal relation to the menstruant may play the bull. In an elaborate ‘wrong species’ pantomime, the women mimic the courtship behaviour of eland cows, dancing around the menarcheal girl, exposing their buttocks to the ‘Bull’. The Zu/’hoasi explicitly identify the menarcheal girl as this ‘Bull Eland’ (Power 1993). In conformity with predictions, the Khoisan consider such displaced ‘animal sex’ – in effect, ‘anti-sex’ – *good for hunting luck.* ‘Human’ or heterosexual marital sex, here as throughout the world, undermines hunting luck.

When do Khoisan use ochre?

Ochre and haematite were widely used in Khoisan menarcheal observances. Among the /Xam the most socially inclusive use occurred in such ritual contexts (Lloyd VI-I, 3969 rev-3973), when the ‘new maiden’ presented all the women of the band with lumps of haematite for decorating their cloaks and faces. A !Kung (Zu/’hoasi) new maiden had

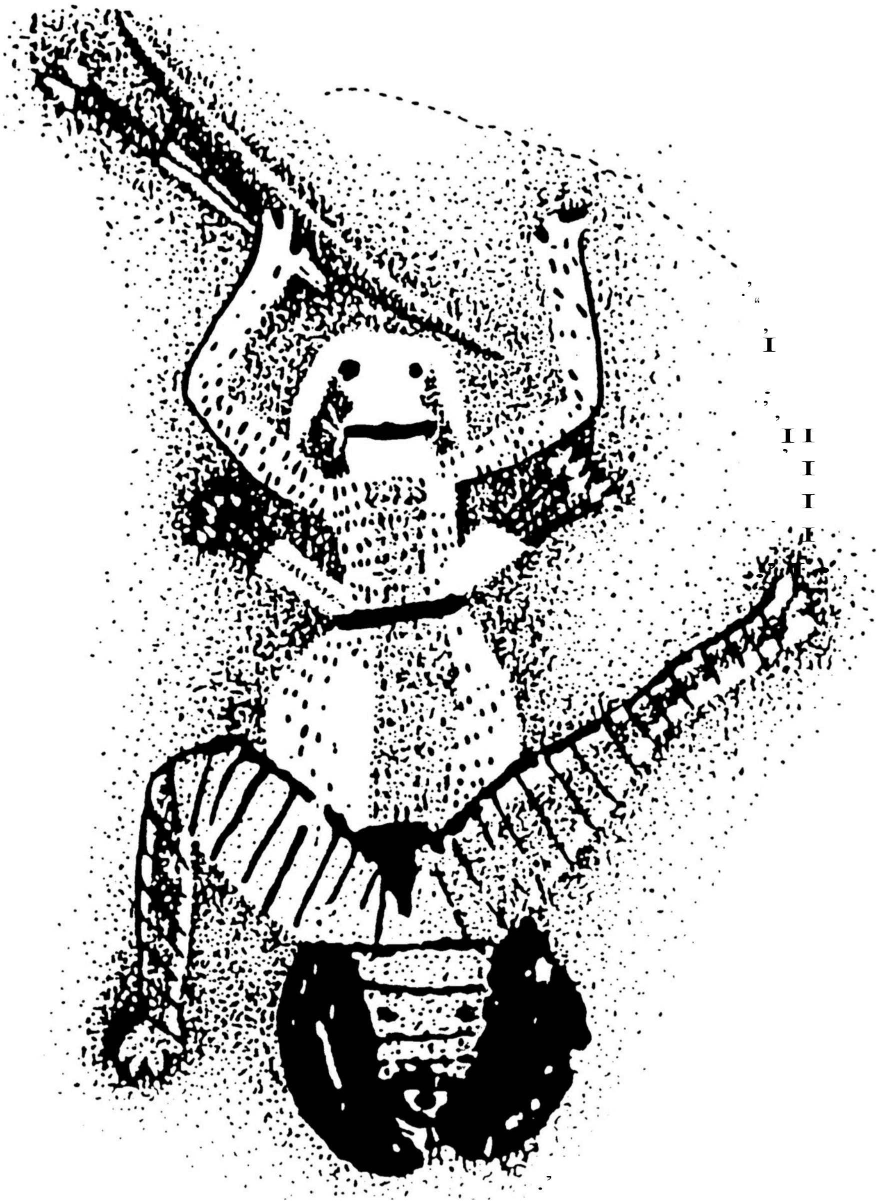


Figure 7. *'Wrong sex/wrong time/wrong species'. Rock painting at Wilcox's Shelter, Drakensberg Mountains, Natal. (Drawing by Anne Solomon after Lewis-Williams & Dawson* 1989, 173.)

a red ochre design painted on her forehead and cheeks (Marshall 1959). The *G/wi* or *G//*ana new maiden would be cut during seclusion; a mixture containing her mother's blood would be rubbed into the cuts (Valiente-Noailles 1993,96). /Xam and !Kung new maidens treated adolescent boys with red pigment to protect them from accidents when out hunting (Lloyd VI-I, 3969 rev 3973; Lewis-Williams 1981). Commenting on Khoisan in Namibia, Fischer (1913) remarked:

> Only the poor ones still paint themselves (with red iron oxides) and then rarely, especially at the time of menstruation (cited in Rudner 1982, 218).

Similar uses of red pigment, whether animal, vegetable or mineral, are reported for menarcheal rites among most other Khoisan hunter-gatherer groups (e.g. Silberbauer 1965; Thomas 1960, 210; Viegas Guerreiro 1968, 223, 226).

Cosmetic manipulation of menstrual signals with 'blood' triggering periodic seclusion within 'another world' – provides a template from which other patterns of ritual can be derived. Most Khoisan ochre/haematite uses were ritual, the pigments being prominent in rites of transition, healing dances, rain magic (Kaufmann 1910, 158), hunting magic (Viegas Guerreiro 1968, 102, 104-5; Bleek & Lloyd 1911,359,363; Kohler 1973) and rock painting (How 1962). Ochre processing was characteristically a women's activity: a metaphor in Zu/'hoasi oral narratives for impending ritual action was hearing the sound of women pounding red ochre in camp (Biesele 1993, 163, 196). Several anthropologists working among the Zu/'hoasi have remarked that rituals of transition – notably marriage, a hunter's first kill ceremony and a woman's initiation into the healing potency of *n/llm* – share much with the syntax of menstrual ritual (Marshall 1959, 356; Lewis-Williams 1981; Katz 1982, 172; Biesele 1993, 153). Marshall (1959,359) extends such structural parallels, arguing that Zu/'hoasi marriage ritual 'undoubtedly derives' from menarcheal ritual. In various Khoisan groups, men's first kill and initiation rituals (Hewitt 1986, 131-3,199-201) can be similarly derived. The /Xam explicitly likened the adolescent hunter to a menarcheal girl (Lloyd VI, 4386).

Where ochre was used by Khoisan to treat hides, it was not to preserve them but for decorative effect; neither is there evidence that application of ochre to the body was for utilitarian protective purposes (Watts in prep.). Together with our critical assessment of the principal functional hypotheses of prehistoric ochre use, such ethnographic data permit greater confidence in inferring the operation of similar relations of relevance (Lewis-Williams 1991; Wylie 1988) in the southern African early Upper Pleistocene. Functional uses of iron oxides were then, and remain today, subordinate to ritual and symbolic ones.

Whether in southern Africa or elsewhere, evidence that ritual focused on marital relations would falsify our model. Pair-bonding should *not* be a focus of rock art. The model predicts instead all-female groups engaged in dancing, or all-male groups

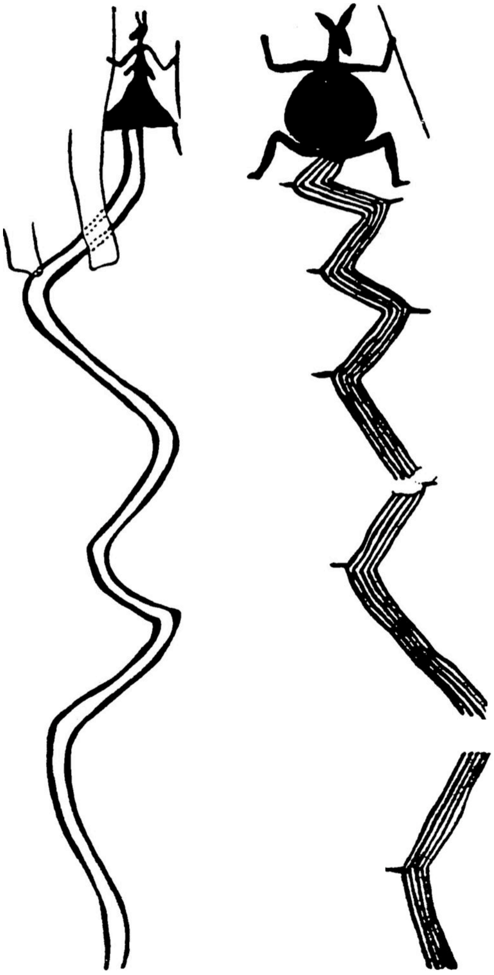


Figure 8. *Marandellas, Zimbabwe. Female therianthropes, complete with meanders. (After Frobenius* 1931, 13, 14.)

Figure 9. *Dance with crescent and flow. Manemba, near Matuka, Zimbabwe. (After Garlake* 1987, *fig. 78.)*

hunting or preparing for hunting. 'Art' traditionally expresses *ritual* priorities (Harrison 1922; Lewis-Williams 1981); given our understanding of how ritual originates, we expect rock artists traditionally to be concerned to fantasize 'animal' roles. If human females are the focus, these should not be 'sociobiological' – that is, narrow-waisted, big-hipped, big-bosomed, nubile and available *(cf.* Low *et al. 1987).* Rather, they should be 'ritually potent'. They should include figures interpretable as menstruating, pregnant and/ or nursing; as gender-ambivalent; and as pantomiming 'animal' courtship behaviour or attributes (Figs. 8 & 9).

Conclusion

What was the original function of ritual? How did it arise? What specific representations did it sustain? Since Durkheim (1965 [1912]) advanced his theory of ritual as the generator of collective representations, the social sciences have resisted exploring the evolutionary implications of his model, distancing themselves from all evolutionary questions in face of the perceived reductionism of evolutionary theory. Impatience with Darwinism's simplistic treatment of symbolic data has been one of the few unified stances in twentieth-century social science.

Absenteeism by social scientists is no longer justifiable. Nee-Darwinism now has a sophisticated body of theory differentiating cultural from biological levels of evolution. Social anthropologists remain those best acquainted with the dynamics of ideology, ritual and symbolism. Clearly, no further advances can be made in reconstructing symbolic origins without harnessing Darwinism and the humanities to work together.

Our focus here has been on the ritual function of cosmetics. Symbolism arose as a response to increasing levels of reproductive stress experienced by females during the rapid phase of encephalization associated with archaic *Homo sapiens.* Once reliable fertility signals had been phased out, menstrual bleeding was left as the only cue offering males positive information on which females were imminently fertile. Because pronounced menstrual bleeding was valuable for extracting mating effort from males, even non-cycling females 'cheated' by joining in with menstruating relatives whenever blood was flowing, painting up with red pigments to signal 'imminent fertility'. Dance and associated body-painting of this kind long antedates the production of representational imagery on inanimate surfaces. At the point where 'collective deception' was established, such use of movement and pigments constituted symbolism. The archaeological record of ochre use in southern Africa, interpreted in the light of San and Hadza ethnographic data, matches the predictions of this model.

'Sham menstruation' involved keeping males continuously in the vicinity – an adaptive strategy provided *local* resources remained abundant enough to permit *area-intensive* foraging, utilizing relatively restricted home ranges. When hunting had to be conducted *at a distance,* involving extended journeys and overnight stays, females changed strategy accordingly, signalling ‘No meat, no sex!’ on a monthly basis as supplies of meat ran low. We have shown how ‘sex strike’ theory yields predictions illuminating an array of puzzling features of symbolic culture, including the detailed syntax of menstrual ritual and associated mythology.

The signal of menstruation, appropriated from an individual *by a collective,* communicated for the first time a ‘symbolic construct’. The earliest representation of ‘divinity’ was an amplified and deceptive signal indicating *‘wrong sex; wrong species; wrong time’*. We see symbolic culture at origin as the set of all such deceptive signals, designed to exploit male labour power. Speech is cryptic mutual reference to such ‘gods’.

Finally, we have argued that red ochre/haematite from the MSA2 onwards was first regularly used in the symbolic structuring of a prototypical sexual division of labour. The Upper Pleistocene MSA obviously does not share in the elaboration of symbolic culture witnessed in the LSA; but all the essential elements appear to be in place by, or shortly before, the Last Interglacial- approximately coincident with the evolution of anatomically modern humans. This ritual and symbolic evidence, at so early a date, lends support to at least some version of the ‘Out of Africa 2’ model of human origins.

Acknowledgements

Financial support from the University of London Central Research Fund, the British Academy and the L.S.B. Leakey trust for research in southern Africa is gratefully acknowledged. For access to ochre collections, thanks are due to Aron Maze!, Natal Museum; Lyn Wadley, Department of Archaeology, University of Witwatersrand; Peter Beaumont, McGregor Museum, Kimberley; John Kinnahan, State Museum, Windhoek; Graham Avery, South African Museum Department of Archaeology; Royden Yates and Ursula Evans, Department of Archaeology, University of Cape Town; and Hilary Deacon, Department of Archaeology, University of Stellenbosch. Thanks are also due to Eric Wendt for permission to examine material from Apollo 11. The South African Museum Library and the African Studies Library gave permission for access to unpublished notebooks. Thomas Volman generously gave permission to cite an unpublished manuscript. The following have been very helpful with discussion: Leslie Aiello, Catherine Arthur, Robbins Burling, Robert Foley, Andy Garrard, Adam Kendon, Paul Turke and James Woodburn.

Chris Knight Department of Sociology University of East London Longbridge Road Dagenham Essex RM82AS

Camilla Power & Ian Watts Department of Anthropology University CollegeLondon Gower Street London WCIE 6BT

Comments

From Ben Cullen, Department of Archaeology, University of Wales, Lampeter

Regardless of whether one agrees with every particular of their position, one cannot deny that Knight, Power & Watts have produced a superbly researched, meticulously argued and clearly presented piece of work. The article owes much to Knight’s impressive tome *Blood Relations,* itself a culmination of many years of research. In support of their argument, the authors amass so much information – much of it already accepted in the disciplines from which it is derived – that those who wish to resist their conclusions will have to advance an alternative explanation of some kind.

Apart from the introduction and conclusion, which define culture and recapitulate the logic of the paper respectively, each of the 16 sections focuses on one piece of the blood relations jigsaw puzzle, building the overall picture piece by piece. There are, nonetheless, a few questions which could be asked of the authors: If culture is defined as a communal map, where does individual cultural variation fit in? Is every copy a perfect copy? Does the model preclude female hunting and male caring? Why does the sham menstruation strategy in archaics not lead to an earlier fully modern cultural capacity? Why does the theory lend nominal support to Out of Africa 2? Is it because local archaic females lack the fully modern cultural capacity required to join the coalition during the wave of advance? But this is not an article for nitpicking. Even if a few of the judgements presented turn out to be incorrect there is definitely something important here that needs to be addressed.

When assessing any model based around neo-Darwinian principles there are at least two constructs which a reader who is not familiar with evolutionary theory needs to be made aware of. The first construct is the distinction between genetic and cultural process. It is impossible to assess the value of a neo-Darwinian explanation unless one is able to determine whether the model is explaining a series of genetic changes, or a series of cultural ones. Quite different bodies of theory would be employed in each case. As I understand it, Knight, Power and Watts are advancing an explanation for a series or complex of genetic changes, namely those final changes which they believe were required to transform archaic hominids into people who possessed an identical cultural capacity to ourselves. They are arguing that this final shift is partially responsible for the symbolic explosion they associate with the Middle to Upper Palaeolithic transition. Yet culture itself pays an important role in this process. The explanation advanced by the authors therefore involves a complicated interplay of cultural and genetic forces, but it is not a theory of cultural change.

The second construct to be borne in mind (assuming genetic change toward a fully modern cultural capacity is what is being explained) is the 'as if' principle of neo-Darwinian theory, as discussed by Dawkins. He writes (1989, 47): 'In practice it is usually convenient to regard the individual body as an agent "trying" to increase the numbers of all its genes in future generations'. Such shorthand is sometimes convenient because it cuts through the 'proximate' causes or objectives of genetically-reproduced behaviour (drives, desires, patterns of reasoning, etc.) to the 'ultimate' cause or objective, namely reproductive success (Betzig 1986). However, since people do not often go around thinking about 'reproductive success', much recent sociobiological research has concentrated on the 'proximate' mechanisms of motivation and reasoning, under the name of 'algorithms' and 'modalities' (e.g. Cosmides 1989; Mithen 1993; Steel in press); generalized patterns of thinking which might be expected to lead to reproductive success under certain social or ecological circumstances. The arguments of Knight, Power & Watts are best understood, I believe, within this algorithm/modality tradition, with the modalities in question being the domains of ritual and language.

These final genetic changes are psychologically manifest not as specific concepts (such as 'no meat, no sex, as I need your help to share the cost of raising my bigger-brained offspring, and be reproductively successful'), but as a double-barrelled algorithm or bimodality which integrates and explicitly interrelates the two capacities for ritual and language. This is one of the special innovations of the article. Previous models have seen the origin of the cultural capacity in one element, for example language (e.g. Cavalli-Sforza *et al.* 1988; Mellars 1991), which then produces the other aspects of the symbolic revolution as byproducts. By contrast, the authors argue that the two capacities of language and ritual are elaborated together, as two different skills are brought to bear on the same task – collective deception. Language enables the organization of the collective, while ritual becomes the vehicle of deception.

What makes the blood relations model so ingenious is that it sees collective deception as the outcome of a signalling paradox. Animal signalling normally involves evolution toward either conspiratorial whispering or loud deception, in social contexts of shared interest or competition respectively. But collective deception involves the concurrent elaboration of both kinds of signalling. Knight, Power & Watts then show how, in keeping with neo-Darwinian signal theory, this paradoxical human social situation arises where *both* shared interest and competition are endemic.

In summary, the value of the blood relations model can be understood as a series of syntheses of problems which have previously been held distinct. First, Knight, Power & Watts present a model of a fully modern cultural capacity as the synthesis of the two modalities of ritual and language, rather than positing one as the cause of the other. Second, in seeking an explanation for this double algorithm, they present a model which explicitly integrates the two extremes of conventional neo-Darwinian signal theory – loud, apparently verified deceptions and accurate, conspiratorial whispers – into one adaptive strategy. Third, the authors actually specify a plausible context in which just such an integrated double capacity might be required by the same individuals: where a coalition of individuals organized itself with honest, conspiratorial whispers, performing loud, collective rituals which effectively deceived those who were excluded from the coalition.

From Alan Barnard, Department of Social Anthropology, University of Edinburgh, Adam Ferguson Building, George Square, Edinburgh, EH8 9LL

The Darwinian evolutionism of Knight, Power & Watts is apparent here, but in another sense their work is better characterized as revolutionist rather than evolutionist (as indeed their title implies). By this I mean that their particular feminist brand of unilinear evolutionism, like Freud's (1950 [1913], 140-61) masculinist but also Darwinian version, depends not so much upon a sequence of events as on the single event which marks the beginning of symbolic culture. Everything, in their view, depends on the evolution of the capacity for collective deception, and on whether there was an actual 'sex strike' by a group of females at some specific time prior to 60 kya. This is how they themselves wish their theory to be judged.

I find the present article among the most convincing of all the works of the three authors. However, I do not hold to their implicit supposition that belief in a literal sex strike is at all essential for making good use of their brilliant analysis. The ethnographic material they report does not necessarily represent a set of survivals of the original collective deception. Nor does their theory represent a testable hypothesis, Le. one which is falsifiable through either archaeological or ethnographic research. What is interesting is that, irrespective of the unfalsifiability of their hypothesis, the research programme these authors have pursued is as provocative and enlightening as any now on offer in either social anthropology or theoretical archaeology. Like Darwin, they have chosen Africa as the cradle of both humanity and culture, a notion which runs counter not only to earlier, and today less-tenable, theories of the physical origin of *Homo erectus* and *Homo sapiens,* but also to the traditional social anthropological emphasis on Australian ethnography as the best model for early culture. The connections Knight, Power & Watts make between the origins of symbolic behaviour and the southern African archaeological and ethnographic record should be of great interest, both to southern African area specialists and to all archaeologists who confront issues such as the migration of *Homo sapiens* from Africa or the relevance of ethnographic analogy for the construction of models of cultural origins.

For me, it does not matter either whether there really once was an original, collective sex strike, because the arguments Knight and his colleagues have been putting forward over the last decade have led to re-interpretations ritual, rock art, and ochre use, among other things, which are of intrinsic interest. They shed light on issues such as the relative significance of ritual, other forms of communication, kin ties, and social contracts, all of which have been the object of decontextualized theoretical debate for over a hundred years. Knight, Power & Watts have here placed these debates more firmly in an archaeological context, though consideration of the issues they discuss is not dependent on a strict adherence to every detail of Knight's (1991) theory.

Evidence of menstrual symbolism in rock art, for example, may as easily relate to menstrual taboos and their association with hunting, which could either be specific to given culture areas or to given stages of social evolution, or, in a wider sense, general in the human psyche (if now hidden in the Western subconscious).

My own area of knowledge is Khoisan ethnography. The Khosian data discussed here are, on the whole, incontrovertible, though one correction may be of interest. Among the Nharo (Naron), it is not affine but rather a senior male of the 'grandfather/ cross-cousin' category who in female initiations dances the part of the Eland Bull. This is significant because although the 'grandfather' cannot himself have a sexual relationship with the initiate, he nevertheless comes from the category which includes those who can. Thus the dance symbolizes the tension between sexual availability and non-availability, as well as between female and male (the initiate also being equated with the Bull), and between man chasing eland and woman being chased by man.

From R.I.M. Dunbar, Department of Psychology, University of Liverpool, P.O. Box 147, Liverpool L69 3BX

Naive linear evolutionary accounts of human behaviour were rampant during the latter part of the last century. The reaction against them was no doubt justified. Biologists took equal umbrage at similar phenomena within their own discipline. Unlike the biologists, however, it seems that social scientists attempted to sweep history firmly under the carpet and out of sight. Unfortunately, such tactics are to no avail. It is naive to assume, as most anthropologists and sociologists implicitly do, that human behaviour has been as it is now since before the dawn of time – or that, if things were once different, the tortuous historical sequence of how it got from some ancestral state to its current form is not an interesting question or one that is open to investigation (the error into which the bewildered advocates of postmodernism have fallen).

Any account that seeks to explain why (as opposed to 'how') a particular historical development took place must be a Darwinian one: despite a century and a half of intensive searching, we have failed to find any suitable alternatives to Darwin's theory of evolution by natural selection (at least in its modern form, with genetic and cultural drift and other 'non-Darwinian' components added in).

Our problem in dealing with the past, however, is that it becomes difficult to be sure that we have understood the conditions that prevailed at the time. It is in the environmental forcing of behaviour that the Darwinian processes work. Knight *et al.* argue forcefully that culture arose through the need to coordinate behaviour within the group (and specifically to coordinate the behavioural strategies of females in order to force the males to provide them with high quality foods from hunting). I am not competent to comment on either the archaeological or the ethnographical evidence they adduce in support of their hypothesis, but (pared of the evocative languages which Knight *et al.* sometimes use to describe them) the biological components at least make some sense and are at least as plausible as any alternatives that have been offered.

The proximate cause of that need is reasonably clear: the extraordinary cost of reproduction for large-brained humans. The point of origin of these behaviours and the cognitive skills that underpin them cannot predate the time at which hominid brain size exceeded the levels at which females could service their own needs. Were they to do so, the hypothesis advanced here would collapse. What we cannot yet say is what ultimate factors forced these developments in the first place. We know that they must have had something to do with dramatically increasing group sizes (Aiello & Dunbar 1993), but we have yet to come to serious grips with the ecological factors which drove the increase in group size.

Knight, Power & Watts will surely be the first to admit that they cannot have provided all the answers. Nonetheless, their service to the community will be enough if it forces us to focus on the substantive issues. In the immortal words of the biologist Robert MacArthur, in science it is often more important to be interesting than to be right.

From Brian Boyd, Paul Pettitt & Mark White, Department of Archaeology, University of Cambridge, Cambridge, CB2 3DZ

We congratulate the authors for a stimulating and innovative model which attempts to define the nature of the 'symbolic revolution'. As the authors note, testing this model against the archaeological database is critical to its success. We do not feel that the evidence presented is as clearly supportive as the authors believe and that more parsimonious explanations are passed over in favour of symbolic interpretations.

Absolute numbers of archaeologically recovered ochre fragments are crucial to the model's success. The presented data comes entirely from caves and rockshelters, most of which have extremely complex depositional histories, often involving major erosional episodes. As Volman (1984) notes, most MSA horizons have been reduced to lag deposits and even the thinnest of these are probably compacted occupational palimpsests. Moreover, haematite is widespread in limestones and ferruginous quartzites and often forms in sedimentary contexts (Teodorovich 1961; Berry *et al* 1983). It is, therefore, a ubiquitous element of cave/rockshelter deposits; and the presence of plaques and crayons may simply be the result of haematite's natural tabular or columnar habit. In this sense we wonder how reliable absolute counts of fragments are in reflecting an 'explosion' in the use of ochre, and how certain are the authors that the majority of the haematite has been anthropogenically, rather than autochthonously, introduced? Could the under-representation of ochre from Early Stone Age sites merely be the result of the lack of cave sites and general poor preservation of deposits from this period? If so, then the 'dramatic' increase in the number of fragments from around 110 kyr may be illusory. Additionally, the sampling problems associated with the MSAI and the possibility that Last Interglacial (= MSA2a) depositional histories differed widely from subsequent cold stages makes it very difficult to sustain statements about the appearance of this mineral in the archaeological record. Given the vast periods of time represented by the deflated cave deposits, the absolute numbers of ochre fragments are extremely low. Numbers of crayons and utilized pieces are even lower. Is the available archaeological resolution really fine enough to support the model?

If we assume that the haematite has been humanly introduced, the problems concerning the more utilitarian uses of ochre need to be addressed more fully. The authors deal only with hide preservation and suggest that with such a use the surface area of ochre fragments would be maximized. This would only hold, however, if the ochre was applied directly to hides. If the ochre were powdered, perhaps by abrasion with flakes, one would expect to find the striated fragments seen in Figures 3 to 6. Powder could be mixed with fat or other substances before application as a paste *for many protective uses* (Bahn & Vertut 1988,69-70). Can the authors confidently eliminate these more utilitarian uses of ochre? If they cannot, then the archaeological application of their model is significantly weakened.

Further, the authors claim that there exists 'indirect evidence that lunar phase-locked hunting was a significant component in early modem human animal procurement strategies during the dry season, when females would have been under greatest nutritional stress' (p. 93). What form does this evidence take? The notion of the lunar cycle as some kind of biological and social determinant requires comment. Firstly, the authors assume that women's menstrual cycles are somehow locked into the lunar cycle; but we do not know either the timing or duration of Neanderthal or early modem human menstrual cycles, nor their periods of gestation, etc .. It is quite conceivable that they differed considerably from modern populations in these aspects. We argue, therefore, that the menstrual cycle was not integral to the structuring of 'animal procurement strategies'. If hunting practices are 'periodic' in any way, then surely they are structured not according to human biological cycles, but to the seasonal cycles of animals as they move through particular landscapes.

Finally, it seems ironic that a model which attempts to produce a 'female-led' symbolic revolution ultimately falls back on stereotypical perceptions of the man-the-hunter /women-at-home dichotomy so characteristic of evolutionary, processual and structuralist perceptions.

From Sue Taylor Parker, Department of Anthropology, Sonoma Satate University, 1801 East Cotati Avenue, Rohnert Park, CA 94928, USA

On the basis of Chase's concept of 'symbolic culture' the authors of this paper declare that their task is ' ... to elucidate the conditions necessary for collective deception to evolve, and – since no better paradigm exists – to do so within a neo-Darwinian, behavioural ecological framework' (p. 77). In a larger frame their aim is to explain the evolution of symbolic culture as a strategy females devised to garner greater male parental investment in the face of the extra energy costs arising from increased encephalization. According to their multi step scenario, females first evolved concealed ovulation, then menstrual synchrony, then menstrual displays, then kin-based sham menstruation, and finally sex strike.

More specifically, they argue that once menstruation evolved as a display or signal of impending fertility (see Piage & Paige 1981 for a quite different treatment of this theme), it became the target of mimicry through deceptive borrowing of menstrual blood or use of blood-coloured pigments (sham menstruation) by coalitions of female kin. According to their model, the critical juncture for symbolic cultural evolution came when displays became ' ... a matter of *monthly habitual performance,* irrespective of whether any local female is actually menstruating. Once such regularity has been established, females have effectively created a *communal construct of 'Fertility' or 'Blood'* no longer dependent on its perceptual counterpart: (p. 81). Finally, in the sex strike, female kin acted as ritual conspirators by collectively withholding sex from men during a monthly cycle preceding special hunts. Females did this by ritually signalling the reversal of the 'specific mate recognition system' *'wrong species/sex/time'* sometimes through 'monstrous therianthropic fictions' which constituted the first gods.!

The authors frame their problem in a set of questionable presuppositions. They invoke encephalization as a background condition which requires no explanation. They also presume that the representations of symbolic culture are necessarily religious and fantastical in nature. In relation to linguistic and cognitive models, their discussion of symbolic culture and speech is underdeveloped. It therefore tends to conflate various levels of complexity. They fail to acknowledge, for example, that symbol use is cognitively simpler than language: developmentally, use of symbols emerges during the second year of life and presages the onset of language development (Piaget 1962; Bates 1979). Negation precedes full grammatical language which develops slowly and is only complete in adolescence (e.g. Menyuk 1977). Comparative data suggest that great apes are capable of learning and using symbols though they are incapable of full grammatical language.

Given the common ancestry of great apes and humans, it is therefore reasonable to infer that symbol use evolved early in hominid evolution and long preceded the emergence of full fledged language (Parker 1985). If this is true, then the authors' task is to explain the selection pressures favouring the emergence of full propositional language rather than symbol use. Presumably, selection for propositional language involved messages more various and complex than the simple ritual reversals 'right/wrong' which the authors propose. A plausible candidate for such reproductively related selection pressures on language and cognition in the Upper Palaeolithic is planning and execution of alliances for resource and mate exchanges among families in adjacent groups (e.g. Gamble 1986; Parker & Milbrath 1993). Planning such alliances requires new kinds of entities (Le. promises of reciprocation) which are not fantastical. It also provides ample opportunities for the collective deception the authors propose to explain.

In touting their model, the authors claim that ' ... the most decisive advantage of [their] "female manipulation" model is that it can explain the emergence of collective deception – hence ritual, language and symbolic culture generally. It is this which previous male-biased models have been unable to achieve' (p. 82). I question this on theoretical grounds.

First, the 'female manipulation model' does not accord with sexual selection theory: the authors' claim that 'Any focus on evolutionary change obliges us to adopt something of a female bias. This is because according to standard socioecological models, changing ecological variables drive changes in mammalian mating systems via changes in female strategies not male ones' (p. 81). This claim is anomalous given that among closely related species, males commonly differ more in sexually selected characteristics than females do (e.g. Eberhard 1985; Kummer 1970). This phenomenon can be understood in relation to the role of female choice (= female manipulation?) and male competition in species divergence in male characters during speciation (e.g. West-Eberhard 1983; Parker 1987). Furthermore, it is clear that the major shift in reproductive strategies in hominids occurred in males rather than females, first, in the shift to greater parental investment in the form of provisioning of famale and offspring especially with hunted meat, and second, in the formation of intergroup alliances particularly through exchanges of wives.

Second, the authors' model of collective or co-operative female strategies is also anomalous in failing to counterpose alternative strategies characteristic of competitors within the population. The authors might, for example, have proposed that females with kin who cooperated in the sex strike might have garnered greater paternal investment than females in kin groups who failed to do so. One difficulty with this model lies in its neglect of female competition and the potential for cheating by individual females. Another difficulty lies in its assumption that fertile females lived in extended kin groups. Given the more common pattern of male philopatry and female dispersion in great apes and humans this is a questionable assumption (e.g. Pusey & Packer 1987).

Finally, their menstrual model neglects to counter relevant ethological and ethnographic theory and data which suggest that the ritual and reproductive significance of the colour red is over-determined (e.g. Brain 1979). Red is not only the colour of menstrual blood and the colour of sexual organs, it is also the colour of blood of injury, the colour of animal blood, the colour of heated objects, the colour of anger and excitement. Many animal species display red in courtship and territorial displays. From this perspective the use of red pigments by early humans is almost predictable and need not be tied uniquely to menstrual blood.

Despite the limitations of the authors' model, it has the sanguine effect of drawing attention to the possibility that menstruation might have been a ritualized display of fecundity in human females, and that red pigments might have been used by humans to mimic physiological states including menstruation – in various kinds of reproductive rituals.

Note

1. It is interesting to note that the 'species recognition model' that the authors invoke in relation to the base meaning 'right/species/sex/time' is founded on the old 'lock and key model' of animal genitalia and courtship which preceded the sexual selection model for speciation and courtship and copulation (e.g. Eberhard 1985; West-Eberhard 1983).

From Lauara Betzig, Co-Director Evolution & Human Behavior program, Museum of Zoology, University of Michigan, Ann Arbor MI 48109-1079, USA

This is a complicated piece of work. At the most general level, I think Knight, Power & Watts are absolutely right. They write: 'Impatience with Darwinism's simplistic treatment of symbolic data has been one of the few unified stances in twentiethcentury social science', and that 'Absenteeism by social scientists is no longer justifiable'. Darwin's is the only good theory of life we have; any good theory of human life – symbolic or otherwise -will have to reckon with it.

I think several more specific points in the article are insightful. Among them: rituals, as 'loud, multi-media displays, stereotyped and prone to massive redundancy' are likely to be deceptive, while speech, 'at low amplitudes, requiring minimal redundancy and at astonishingly high speeds' is more likely to be co-operative. And: ritual, about 'gods' or other things *'Out* of space and *out* of time', should have selected for complex syntax.

At the nitty-gritty level, however, I'm muddled. Whether or not women really do synchronize menstruation is a moot question (e.g. Wilson 1992); in at least one well-studied contemporary group, the Dagon, menstrual status is honestly advertised by use of menstrual huts (Strassmann in press); and most human females, in natural fertility societies, are pregnant, lactating, pre-menarcheal or post-menopausal most of the time (Short 1976). Not being a man myself, I'm not sure how well the application of ochre to a pregnant, lactating, pre-menarcheal or post-menopausal woman would mimic the flow of menstrual blood. And it isn't clear how likely women would be to deceive men *collectively*. Signals of fertility status have been, on the contrary, seen as aspects of female-female competition (Pagel 1994). If being one of the rare, fecund able ones makes it easier for me to attract a better defender, a better baby-sitter, a better provider, and/or better genes, then why should I lend my blood – or my ochre – to my sister?

Reply from Chris Knight, Camilla Power & Ian Watts

Locating us within the neo-Darwinian 'algorithm/ modality' trend in evolutionary psychology, Cullen believes we have solved a difficult jigsaw puzzle. The pieces fit; no other solution has been proposed. There remain bumpy areas – but any hostile critic must demonstrate some alternative solution not just to parts but to the whole puzzle.

Barnard finds our research programme 'as provocative and enlightening as any now on offer in either social anthropology or theoretical archaeology'. Faithful to a tradition in social anthropology which denies the testability of any evolutionary model, he is less interested in whether our revolution 'literally' occurred. We align ourselves with the sciences in rejecting untestable theories. Dunbar concludes 'it is often more important to be interesting than to be right'. But we are not too interested in wrong hypotheses. Had we been wrong – had females not 'literally' pursued the strategies we model – we doubt we could have seemed interesting at all.

Working from purely Darwinian premises, we specify the initial signature of ritual potency, and predict that subsequent ritual elaborations should replicate that signature. Barnard thinks equally good use could be made of our 'brilliant analysis' even were its central tenet wrong. We disagree. It is because our model approximates the correct initial conditions that we can achieve a fit between our predictions and detailed Khoisan mythico-ritual data which Barnard endorses as 'on the whole incontrovertible'. Any competing model of cultural origins must achieve as good a fit.

Boyd *et al.* claim an absence of 'clearly supportive' archaeological evidence. Absence of evidence since it cannot falsify anything – lacks force as an argument. Our model will fall under the weight of positive evidence it cannot allow – examples being pre-hunt rituals prescribing indulgence in marital sex; menstrually potent women cooking meat; rock art traditions focused on the human pair-bond. We await falsification of the predictions our model actually specifies.

Our 'time-resistant' mythico-ritual syntax owes much to Lévi-Straussian structuralism; *contra* Barnard, it is not to be equated with earlier doctrines concerning 'primitive survivals'. To Barnard, the connections we discern are of interest to archaeologists evaluating the relevance of ethnographic analogy, but this seems at odds with his own position on testability. What relevance can any ethnographic analogy have for archaeologists investigating earliest symbolism, if no model of origins can be falsified using ethnographic data? We are not playing intellectual games. We take as axiomatic that a theory of symbolic origins must be tested against symbolic cultural data. Does our model provide the most parsimonious explanation for the abundance of red ochre in the southern African MSA record, for the 'wrong sex, wrong species' configuration in African hunter gatherer initiation rituals, for the persistent mythico-ritual linkages between the moon, menstruation and hunting luck?

Boyd *et al.* query the robustness of the archaeological iron oxide data. We acknowledge that this data needs to be refined, but we are confident that it is, at a gross level, reflecting past human behaviour. Taking their points in turn, we focused on cave/ rockshelters because there are so few MSA open site excavations and just a handful that can be tied into Volman's informal techno-typological chronological scheme. Lagged deposits do not represent the majority of our sample but about one third. The processes resulting in lagged deposits remove organic materials and in some circumstances may remove small lithic and ochre fragments. They do not appear significantly to affect the representation of ochre relative to lithic material. Where sites have both MSA2a and 2b assemblages, while different environmental conditions can sometimes be demonstrated, similar depositional histories generally obtain (e.g. Klasies River Mouth and Border Cave). Regarding the translation of absolute numbers into a relative percentage frequency, we note that the range of values from 0 per cent to around 11 per cent is similar to that reported ethno-archaeologically in the Western Desert of Australia (Nicholson & Cane 1991). The aggregated frequencies from the MSA2b onwards are only six to four times lower than the 2 per cent reported in Australia, which could easily be accounted for by the absence of any small lithic debris in the Australian inventories.

Was this ochre humanly introduced? Boyd *et al.* do a disservice to the professionalism of southern African archaeologists, who routinely make distinctions between ferruginous crusts, ferruginous deposits and pieces of ochre. Equally, when these archaeologists use the term 'crayon' or 'pencil' they do so to refer to particular end products of human modification. One of us examined material from 13 of the 28 sites used in the sample. Visible signs of modification accounted for between 8 and 30 per cent of most assemblages. It is precisely because ochre is so common in quartzites, limestones, shales, mulls, etc., that archaeologists can compare archaeological frequencies across, for example, the Middle-to-Upper Palaeolithic transition (e.g. Couraud 1991). We are merely doing the same in an African context. In virtually all the assemblages examined, a wide variety of forms of iron oxide were encountered, many of which were clearly from different sources. While the majority of the material examined was almost certainly of local origin (as with lithic raw materials), a variable proportion is clearly exotic. At Boomplaas, for example, the dominant pigment is a shale believed to have been transported c. 70 km (Deacon pers. comm.). In short, we are reasonably confident that less than 1 per cent of the total material examined was of 'autochthonous' rather than anthropogenic origin.

We focused on the hide-preservation hypothesis because this is the one that has most concerned archaeologists for the last 17 years. Many of the criticisms we raised against this as a general explanatory model apply equally to other protective uses. The overwhelming majority of utilized pieces were ground; neither the grinding of pigments nor the possible mixing with fats (not archaeologically discernible) would discriminate between hide processing and body painting. Our point was that hide processing would not account for the minority of ground pieces with sharp points, honed edges, etc .. As far as the scored pieces are concerned, we were simply drawing attention to the non-random nature of some of the marks and possible patterns. That such scoring would produce a powder is not in doubt. Finally, we were not ruling out utilitarian functions for ochre, but arguing that if these were the only uses, the full range of data encountered would not be explained. Our conclusion was that more utilitarian uses of iron oxide were secondary to ritual body painting.

Boyd *et al.* believe Neanderthal gestation lengths may have diverged from those of modern women. The evidence of the Kebara 2 skeleton (Rak & Arensburg 1987), found with a pelvic canal of the same diameter as in modem humans, shows that Neanderthal mothers cannot have had a period of gestation longer than we do because otherwise their babies' heads would have been too large at birth. Boyd *et al.* surely are not suggesting that Neanderthal gestation lengths were shorter? We reject their further suggestion that 'early modern humans' members of our own species – can differ from the species' mean gestation or menstrual cycle length!

Citing only our summary while ignoring the substantial archaeological and ethnographic references we provide on the previous page, Boyd *et al.* request 'evidence' for lunar phase-locked hunting among early humans in Africa. Historical and ethnographic local precedents suggest that any specialist hunting at dry-season pan-margin sites was nocturnal, hence necessarily restricted to moonlit nights up to and around full moon. Boyd *et al*. fail to appreciate that it is not Knight *et al*. who are inventing a link between lunar/menstrual periodic rituals and hunting schedules. It is hunter-gatherer models, Khoisan and Hadza, which actively assert this linkage as an ideal. The final throwaway remark of Boyd *et al.* merely panders to academia's current canons of political correctness.

Alone of the commentators, Parker flatly opposes our model, referring readers instead to her proposed alternative (Parker 1987; Parker & Milbrath 1993). This is a version of what Richards (1987) has dubbed the 'prostitution' theory – a narrative central within old-fashioned popular Darwinism (*cf* Morris 1967; Ardrey 1969; Tiger & Fox 1974). Since our model was an assault on precisely this outdated, male-biased paradigm, we expected no support from this quarter.

In Parker's alternative, females compete to offer sexual favours to dominant males, 'sexual selection for increasing male control of females through provision of meat' explaining successively 'bipedalism' and 'language' (Parker 1987). Females are enticed by early *Homo* males to special butchery-sites where they solicit provisions in exchange for sex, inciting males to aim missiles at one another in their struggles for control (1987, 246). Parker pictures 'rules' and 'language' as additional weapons in the armoury of such sexually aroused males, deployed increasingly in place of physical weapons (1987, 245-7). Over time, all this is said to explain 'reduced canines', 'hunting', 'alliances', successive levels of 'planning' – and symbolism culminating in 'propositional language' (Parker 1987; 1993).

In a palaeoanthropological 'Just So' story, one thing is strung alongside another to make a narrative, the only constraint being to end up with contemporary Man. If 'selfish gene' Darwinism has accomplished anything, it has been to curb such narrative freedom by insisting on sociobiology's calculus of *fitness benefits and costs* (Hamilton 1964). Parker nowhere counts the *costs* to 'females burdened by encephalized offspring of the kind of male sexual behaviour she posits. We are at a loss to understand how long-term male investment could have emerged from the dynamic of male competitive struggle and short-term promiscuous exchange which she envisages. Why should such competitive females have lost their oestrus signals? How can insecure males prioritizing mate-guarding of females afford to take time out to hunt big-game? As for 'language', given the costs of unreciprocated honest signalling, we fail to comprehend how any kind of shared symbolic domain could have been founded in such scenes of sexual chaos (Knight 1991, 183-90).

Parker's assertion that 'the major shift in reproductive strategies in hominids occurred in males rather than females' is baffling. Admittedly, prior to the emergence of selfish-gene Darwinism, it was difficult to 'see' female strategies (Haraway 1989, 1769): the functionalist/ group-selectionist paradigms of the 1960s treated females as appendages. Any increased male parental investment would automatically be attributed to males as the 'dominant' sex. Selfish-gene Darwinism has exposed the inadequacy of such methodologies. Positing 'greater parental investment in the form of provisioning of female and offspring especially with hunted meat' lacks force as an evolutionary argument unless the fitness costs to males of such commitment are taken into account.

The thrust of our discussion of human brain evolution was to *explain* what drove the novel scale of male investment. We can infer profound changes in hominine female sexual strategies from the considerable changes which have occurred in female reproductive physiology. Instead of belittling the contribution of either sex, our model addresses the *interaction* of conflicting male/ female reproductive strategies. Parker prefers to picture females as sexual pawns in male political schemes of alliance formation and mate-exchange *(cf.* Lévi-Strauss 1969). Standard socioecological models show that since females must provision their offspring – whereas males may or may not – the *general* pattern is for female strategies to respond more directly than male strategies to factors such as climate change or resource distribution, female grouping patterns in turn affecting the behaviour of males. We note that Parker finds this tenet of modem Darwinism 'anomalous' in the light of speciation and sexual selection theory!

Parker questions the importance of female coalitionary strategies in hominid social evolution. Neglect of female coalitionary behaviour in species where ancestral populations have moved into more open, predator-risky environments, with groups increasing in size, would not accord with current theory of primate social systems *(cf.* Dunbar 1988). Effective female alliances among primates need not rely on kin-bonding (e.g. sexual bonding by unrelated female bonobos for defence of resources: Parish 1994). But the evidence for enduring bonds and co-operation among African hunter-gatherer kinswomen is very strong. Hadza society to this day is defined essentially by the strength of the mother-daughter relationship (Woodburn 1964). In recent historic ethnography, brideservice was the norm among those San groups still primarily reliant on hunting and gathering, implying a degree of shared residence between mother and daughter for lengthy periods after marriage (e.g. Barnard 1992, 44, 55, 69, 80; Marshall 1959, 352). Efe women of the Ituri forest rely on task-sharing even to the extent of breastfeeding each other's infants (Tronick *et al. 1987).* Hawkes (in press) cites the comparative study by Vigilant *et al.* (1989) of mtDNA and nucleic DNA evidence which shows greater residential stability of matrilines, implying female philopatry on an evolutionary timescale for African *Homo sapiens.* Geneticists have thus knocked the final nail into the coffin of the 'patrilocal band' model once favoured by Radcliffe-Brown (1930--31) and Lévi-Strauss (1969) – a model in retreat since the 1960s (e.g. Hiatt 1962; Woodburn 1968).

Parker chides us for failing to realise that 'symbol use is cognitively simpler than language'. In fact, we stress how 'conventional signalling' – presumably what Parker means by 'symbol use' – is cognitively so simple it can be grasped by bonobos. Far from 'conflating' levels of complexity, we isolate conventional signalling and symbolic ritual as each simpler than speech, yet in combination generating it,

Parker's confusion about what constitutes 'symbol use' is betrayed when her argument leaps from discussing common ancestry with the great apes to alliance formation in the Upper Palaeolithic. She claims 'symbol use' as an early hominid development on the grounds that living apes can be taught quasi-symbolic skills by modern human trainers. It is time this fallacy died a death. *Capacity* is one thing, *performance* another (*cf.* Soffer 1992). Given that apes have demonstrated symbolic capacities, why don't they use them in the wild? Parker does not engage with our crucial point: *where conventional signalling* is *voluntary, its low-cost features invite heavy abuse through tactical deception.* If chimpanzees fall short of mustering even a conventional signalling system, it is because there is insufficient basis in their social life for the generalized trust required for such a system to work.

We agree with Parker that rituals are means of allocating sex partners and resources – matters critical to reproductive interests. Our question is why humans, in dealing with material problems of reproduction and alliance, should mobilize communal fantasies in defence of 'moral' authority. What short-term fitness benefits could an individual gain by entertaining such fantasies, and what benefits would other individuals gain by sharing them? Parker fails to answer this question, missing its theoretical significance since she fails to appreciate that symbolic communication has costs at all.

Parker considers establishing ritual distinctions of 'right' and 'wrong' a simple matter not requiring complex 'propositional language', and criticizes us for presuming 'that the representations of symbolic culture are necessarily religious and fantastical'. According to Parker, the 'planning and execution of alliances' for male sexual control over females would have required 'new kinds of entities (i.e. promises of reciprocation) which are not fantastical'. We follow Chase (1994) in defining a 'promise' as in principle no different from a 'goblin'. Can it be touched, tasted, kicked? Would a chimp be able to 'see' it, verbally label it, communicate about it? If not, why not? *How can it be tangibly, visibly, represented?* Is there a 'non-fantastical' way? Parker thinks so, but we are mystified. Where is her ethnographic data on how 'promises' are made binding? Veins are cut, animals sacrificed at the transaction (Girard 1977; Hubert & Mauss 1964). By such means are the gods visibly involved. Can Parker tell us of oaths which can be sworn without summoning up the sacred powers?

We stand by our argument – and here Betzig expresses her interest and agreement. The 'sacred domain' of fantastical beings constitutes precisely that 'displaced' level of reference required to drive the elaboration of complex symbolic communication. Without the gods – no right, no wrong, no syntax, no speech.

While Cullen sees our solution as simple and elegant, to Betzig it all appears 'complicated'. Both Parker and Betzig believe female coalitionary strategies would be undermined by inter-female competition and 'cheating'. Any selfish-gene model must certainly take account of this. On the other hand, Parker and Betzig cannot have it both ways. *They* must adequately take account of male competition and cheating in the form of philandering which undermines male tendencies to invest. For high-ranking males, fidelity/investment has high opportunity costs. But females can influence the payoffs of mate desertion through a strategy of reproductive synchrony (Knowlton 1979; Turke 1984) – the viable form for hominids being seasonally-based ovulatory synchrony (Aiello & Arthur 1994). By reducing the opportunities for philandering, female reproductive synchrony makes investment a better option for males with high reproductive fitness.

Whether menstrual synchrony has been demonstrated in humans remains unclear. Weller & Weller (1993, 437) answer some of Wilson's (1992) criticisms in their review of synchrony studies. Ovarian cycle synchrony has certainly been demonstrated in other female mammals, including primates (Wallis 1985). It would be strange if humans were anomalous in showing no tendency to ovarian cycle synchrony.

Biological seasonal synchrony, however, can never fully eradicate mate desertion. 'Sham menstruation' is an extension of synchrony in an artificial form. Betzig asks, Would males really be confused? Human males respond to sexual signals in the form of pornography which they know perfectly well is not real. What matters is not conscious knowledge, but the proximate mechanisms involved. At the level of such mechanisms, amplified sham menstrual signals will convey 'there are imminently fertile females around', so attracting male attention. But the signals also say 'we are in control of access to these imminently fertile females, and there is no benefit in raping anyone of us, because no one is fertile right now'. Sham menstruation is a strategy of resistance and deterrence, driving up the costs of philandering relative to investing.

Betzig asks why any rare, fecundable female would share her blood or ochre with her sister. But how can she *stop* her sisters, mum or aunties using cosmetics to mimic *her* menstrual signal? She is outnumbered and surrounded! Exactly because females are competing to display these signals, 'sham menstruation' is generated in the first place. Once a female coalition co-ordinates these signals, it has a powerful means of manipulating males because males cannot afford to ignore any positive cue of female fertility. The competitive dynamic between coalitions drives elaboration of cosmetic displays. Ultimately, both sham menstruation and its emergent symbolic form of sex-strike are *moralistic* strategies, in the sense of Boyd & Richerson (1992), where not only non-cooperators are punished, but those who do not punish non-cooperators are also punished. Rigorous menstrual taboos and seclusion huts are exactly what would be predicted by sex-strike theory (Knight 1991); but these remain mechanisms for advertisement of menstrual status. The way menstrual signals and the ideology surrounding them are manipulated will vary between such societies as the Dogon (polygynous cultivators) and the San (substantially monogamous hunter-gatherers). San and Hadza women assert an ideology of synchrony of signals, through ritual action.

Parker acknowledges that menstruation could have formed the basis for ritualized displays of female fecundity, but argues that the use of red pigments 'is almost predictable and need not be tied uniquely to menstrual blood'. In Khoisan ideology, redness is undoubtedly over-determined, with overlapping connotations of 'fertility', 'rain', 'blood' (human and animal) and 'ritual power'. The pounding of ochre – a task performed by women – remains a metaphor for ritual action in general. In our article, we have gone to great lengths to explain why the 'morally authoritative intangibles' central to the symbolic domain could not possibly have had narrowly perceptual meanings. Red body-paint in our model does not *mean* anything as visible, viscous or tangible as real blood. What is referred to ('meant') is something 'imaginary' – 'supernatural potency' which in this case links and merges a range of subordinate meanings including, 'dance', 'fertility', 'hunting luck', 'death', 'rebirth' and many others. We have proposed coalitionary behaviour revolving around menstrual signals as a pre-adaptation for such collective deception. We would invite Parker to 'tout' a male-biased model for symbolic cultural evolution which generates comparably fine-grained and robust predictions testable against ethnographic symbolic data.

We thank everyone for making this a stimulating debate. Our challenge to orthodoxy in defining symbolism as a set of 'collective deceptions' – a special case of primate 'tactical deception' – acknowledged by all commentators. No-one disputes the problem: it is to provide a Darwinian evolutionary explanation for deception of this collective kind.

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