

Infanticide by males and its implications

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CAMBRIDGE
UNIVERSITY PRESS

PUBLISHED BY THE PRESS SYNDICATE OF THE UNIVERSITY OF CAMBRIDGE
The Pitt Building, Trumpington Street, Cambridge, United Kingdom

CAMBRIDGE UNIVERSITY PRESS
The Edinburgh Building, Cambridge CB2 2RU, UK
40 West 20th Street, New York, NY 10011-4211, USA
10 Stamford Road, Oakleigh, VIC 3166, Australia
Ruiz de Alarcón 13, 28014 Madrid, Spain
Dock House, The Waterfront, Cape Town 8001, South Africa

<http://www.cambridge.org>

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First published 2000

Printed in the United Kingdom at the University Press, Cambridge

Typeface TEFLEXicon 9/13 pt *System* QuarkXPress® [SE]

A catalogue record for this book is available from the British Library

Library of Congress Cataloguing in Publication data

Infanticide by males and its implications/edited by Carel P. van Schaik and Charles H. Janson

p. cm.

Includes bibliographical references.

ISBN 0 521 77295 8 – ISBN 0 521 77498 5 (pbk.)

1. Primates–Behavior. 2. Infanticide in animals. I. Schaik, Carel van. II. Janson,
Charles Helmar.

QL762.5 I535 2000

599.8'156–dc21 00-020380

ISBN 0 521 77295 8 hardback

ISBN 0 521 77498 5 paperback

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The holy wars about infanticide. Which side are you on? And why?

Introduction

The topic of infanticide has been a staple theorem of sociobiology ever since this discipline – the study of social behavior from an evolutionary perspective – was born two and a half decades ago (Wilson 1975). The killing of conspecific young is still hotly debated. Does it occur at all, does it reflect an adaptation, a pathology or even a political agenda? Infanticide – observed among such varied taxa as birds, rodents, carnivores, pinipeds and primates (Hausfater & Hrdy 1984; Parmigiani & vom Saal 1994) – therefore remains a litmus test upon which the validity of a sociobiological interpretation of behavior depends. I attempt to trace some intellectual roots of the controversy: those of defenders of adaptationist explanations, those of critics from within the paradigm of evolutionary biology, and those of critics who operate from other paradigms such as the social sciences. My ultimate aim is to defend the adaptationist interpretation as a valid and fruitful approach, while acknowledging that its narrative is anchored in a time-dependent framework of interpretation.

Cute and brute

People are fascinated by animals, not least because people are, in their own right, animals who can empathize with similar organisms. The average viewer of a natural history documentary will feel good if a monkey mother cuddles her newborn: “It’s so cute.” But different emotions flare up if, over television dinner, wild chimpanzees eat an infant of their own kind: “It’s so brute.”

These complementary sets of emotions are readily served by our brains

and will often grow into thinly veiled judgments. The cute stuff animals do is “natural” because we like it, whereas the brute stuff is “animalistic” because we do not like it. Nevertheless, egg cannibalism in wasps will upset us less than seeing a little chimpanzee being torn apart. The repugnance is stronger if we are phylogenetically close to the victim.

Natural scientists are, of course, supposed to shrug their shoulders no matter what behavior is at stake and take refuge to the advice offered by David Hume in his 1740 *A Treatise of Human Nature* not to stroll from “Is to Ought”, or else be in danger of committing the *naturalistic fallacy*. Still, scientists are governed by the same mechanisms of empathy that lay nature lovers possess. I will not forget 9 July 1981, the first time I witnessed a male monkey sinking his canines into an infant I had grown fond of during a study of hanuman langurs in India. Later in the fieldwork I shouted and threw stones at the aggressor. It did not prevent infant-killing. The first attack took me by surprise. My academic mentor, Christian Vogel of Germany’s Göttingen University, had instilled in me disapproval (Vogel 1979) for the “out-of-America” hypothesis that infanticide occurs regularly amongst langurs and is caused by male competition over females (Hrdy 1974). Vogel’s views still reverberated with the idea that animal behavior serves the good of the species. Accordingly, monkeys were expected to perform “group serving” and “group bonding” acts (Vogel 1976). As an evolutionary biologist, Vogel represented a *within-paradigm critic*. Data subsequently gathered by his students and Indian colleagues changed Vogel’s *Weltanschauung* radically: he transformed into a vigorous defender of the theorem that infanticide amongst animals including humans reflects evolutionary adaptation (Vogel 1989), such as exploitation of the infant for cannibalistic purposes, or parental manipulation of progeny (cf. Hausfater & Hrdy 1984; Parmigiani & vom Saal 1994). With respect to langurs, the theory (Hrdy 1974) maintains that infanticidal males increase their relative genetic representation in future generations by eliminating unweaned offspring of other males, particularly those of their predecessors as harem residents in populations with one-male/multifemale group structures. Infanticide will shorten the waiting time of a new male until he can impregnate a female, because the loss of an infant terminates the period of temporary infertility associated with lactation. In addition, infanticide may be adaptive if it reduces resource competition for a male’s kin.

I for my part learned to rationalize the gruesome events (Sommer 1987, 1994, 1996; Böer & Sommer 1992). I now publicly lecture and write about

infant-killing in more or less the same way as about grooming, presenting both as functional behaviors. However, occasionally somebody from the audience or readership will call me a fascist (cf. Schües & Ostbomk-Fischer 1993: 17). I tend to reply that few people hold meteorologists responsible for the destruction and grief caused by tornadoes; by the same token, I should not be held morally accountable for the aggressive behavior of the monkeys that I study.

This excuse is an easy escape when dealing with benign minds who accept that they are committing the naturalistic fallacy. However, the route from “Is to Ought” is a two-way street and various apostles actually travel in the opposite direction: from “Ought to Is”. They preach that our values construct the reality around us, and that it is imperative to possess the right values. Cute mother–infant interactions are OK, acceptable testimony to how the world should be, but brute male–infant interactions are not OK because reports about aggression are borne out of aggressive minds and breed more violence. This can be labeled as the *moralistic fallacy*: what should not be, cannot be.

Donna Haraway, American scholar of History of Consciousness, figures prominently as an *outside-paradigm critic* sympathetic to such conviction: “To center the debate on the biological meanings of infanticide among primates too easily plays into the culturally overdetermined lust for sexualized violence” (Haraway 1989: 311). There is some truth to this if we look at how the popular media disseminate findings about infanticide: as a story about sex and crime in which the theory is often not only trivialized but distorted. Headlines of, for example, German magazines were only at times acceptable (“Der neue Chef des Harems tötet seine Stiefkinder” [The harem’s new boss kills his stepchildren]), but more often barely bearable (“Affen morden ihre Kinder” [Monkeys murder their young], “Mord im Harem” [Murder in the harem]) and at times blatantly sensational (“Anklage Mord” [Accused of murder], “Das Killer-Gen” [The killer-gene]; “Blutrünstige Rivalen” [Bloodthirsty rivals]) (references in Sommer 1996). But then, purging language and employing euphemisms will not in itself foster desired political change. It may, on the contrary, just cover up fields of conflict. Moreover, any paradigm can be used to incite a war – prime examples being such diverse ideologies as the Christian doctrine to love one’s neighbor, Buddhist belief in the vanity of life, or Marxist utopias of equality. I cannot see what harm talk about infanticide has done, but the fear that it *could* certainly generates much of the heat of the debate.

I will demonstrate how transgressions from “Ought to Is” and “Is to Ought” are committed by all parties involved: defenders and critics from within and outside the paradigm.

Nature revisited

The founding fathers of classical ethology – Nikolaas Tinbergen, Karl Ritter von Frisch, and Konrad Lorenz – did not offend the public with gory stories about a brute natural world. Non-human animals, if anything, were the better humans: they acted for the good of the group and did not kill each other with the ease that humans exhibit. Konrad Lorenz made such cultural pessimism explicit in *Das sogenannte Böse* (*On Aggression*, but literally translated as “The so-called Evil”, 1963; see also Lorenz 1955). The Austrian ethologist embodied a modern version of Jean Jacques Rousseau, and the opening sentence from *Émile ou de l'éducation* (1762) could have been written by Lorenz himself: “Everything is good as it comes from the hands of the maker of things; everything perverts under the hands of man”.

Such romanticism fueled 19th century portrayals of indigenous peoples as “noble savages”, living in peace with themselves and their environment. Visions of politically and ecologically correct *Naturvölker* (nature peoples) are still nurtured by well-meaning Green idealists, particularly in continental Europe. One can easily criticize such back-to-nature missionaries by, for example, pointing out that the rates of homicide in the Highlands of Papua New Guinea are several times higher than in the streets of Los Angeles (Daly & Wilson 1988), and that native Americans had all but wiped out large game before European settlers arrived (Kay 1994).

The good-for-the-whole paradigm in animal behavior came to a rapid demise when field studies such as those on lions, langurs and chimpanzees reported killings amongst conspecifics (Hrdy 1974; Bertram 1975; Goodall 1986). The parallel rise of gene-centered sociobiology and modern behavioral ecology in the 1970s and 1980s was fostered by eloquent treatises accessible to the general public (Wilson 1975; Dawkins 1976). Efforts to debunk romanticism and group selectionism coincided with an unavoidable emphasis on “nature red in tooth and claw”. Killings among conspecifics were compatible with the revised interpretations of social evolution, since animals were understood to maximize their individual reproductive success without taking the good of the group,

population or species into account. Seemingly “nice” behaviors were also reinterpreted as only phenotypically altruistic. Maternal care, therefore, became genetically as selfish as cooperation amongst kin or the exchange of “reciprocal altruism” amongst non-relatives. Sociobiology thus equalized, categorizing both brute *and* cute stuff as “selfish”, “deceptive” or “spiteful”.

Frans de Waal (an ethologist of Dutch origin with perhaps a soft spot for classical ethology *sensu* Tinbergen) has complained that efforts to purge the study of animal behavior from group-selectionist hues has rendered it almost unacceptable to speak about “friendly” interactions. Purist sociobiologists would rather have him talk about “affiliative” interactions and relabel a “reconciliation sealed with a kiss” amongst apes as “postconflict interaction involving mouth-to-mouth contact” (de Waal 1996: 18f). De Waal tends to dwell on the niceties of animal societies – “Survival of the kindest” (1998) – and is perhaps in danger of neglecting a chicken and egg problem: that aggression is not caused by reconciliation but that reconciliation is caused by aggression, which is hence always *a priori*. Still, he rightly identifies the exaggerated swing of the pendulum to a behavioral ecology devoid of group-selectionist interpretations (not withstanding the fact that intergroup competition may sometimes select for group-benefiting behaviors; cf. Sober & Wilson 1998).

Despite de Waal’s complaint, which some might view as merely semantic, there is still fundamentalistic resistance in the scientific community to the idea that animals act selfishly – particularly when it comes to infanticide.

Non-adaptationist explanations: critique from within the paradigm

Outspoken critics of adaptationist theories about infanticide are nowadays rare among behavioral ecologists. Still, their voices are clearly heard because they receive disproportionate attention concordant with the status of a minority. Arguments and counterarguments are often based upon particulars of reports and alleged data errors. As important as details are, I will also highlight general strings of argumentation.

Adaptationist hypotheses are typically debunked by declaring that favorable evidence distorts what goes on in nature. This attempt is two pronged: one can question the database or declare that infant-killings are maladaptive. Both positions have been invoked most prominently by

Phyllis (Jay) Dolhinow – the pioneering researcher on wild langurs (Jay 1963) – and her students and co-workers at the University of California at Berkeley (e.g., Dolhinow 1977; Curtin & Dolhinow 1979; Boggess 1984; Fuentes 1999). This group has recently been joined by Robert Sussman from the Washington University in St Louis. He and his student Thad Bartlett plus coworker James Cheverud are responsible for two papers (Bartlett *et al.* 1993; Sussman *et al.* 1995) and an accompanying press release (which resembles an anti-adaptationist pamphlet) proclaiming “Infanticide more myth than reality” (Aronson 1995). In 1998, Sussman became editor of *American Anthropologist*, which had published his original retort to the sexual selection hypothesis as applied to non-human primates. The rejectionist platform was broadened when the journal soon thereafter carried an article entitled “Infanticide by male lions hypothesis: a fallacy influencing research into human behavior” (Dagg 1999a). The paper listed “faulty science” as a keyword.

Such publications undermine the credibility of certain behavioral researchers, including myself. The primates article therefore became the subject of a rebuttal (Hrdy *et al.* 1995); the lion article provoked a letter, signed by 17 defenders of adaptationist explanations, sent to Sussman in his position as journal editor (Silk & Stanford 1999).

This development can be viewed as testimony to scientific progress that lives from the exchange of arguments and counterarguments. However, scientists do not simply sit down and weigh evidence. The specific academic climate in which we were brought up rewards or punishes specific opinions, and our views therefore tend to be biased. Not denying self-interest, I will take issue with Sussman and colleagues – since they explicitly referred to my own work (see also van Schaik, Chapter 2). The exercise shall demonstrate how evidence is inflated if we sympathize with explanations and how evidence is downplayed if we dislike explanations.

A “careful” literature examination found 48 “observed” infanticides amongst wild primates (Bartlett *et al.* 1993: 960) – but failed to list criteria for what counts as “confirmed”. For example, included is the fate of little langur M1.4 at Jodhpur, India, whose serious wounding I witnessed and who later disappeared. However, other such circumstantial evidence is often excluded from the sample. The authors’ tally for Jodhpur thus adds up to 13 infanticides – whereas I counted more than 50 (Sommer 1994). Similarly, only 3 recognized infanticides among mountain gorillas compare with 17

or so cases considered by other researchers (Fossey 1984; Watts 1989). What counts as “unequivocal” evidence is obviously largely in the eye of the beholder.

Captive primates are only included selectively. Killings that allegedly refute the sexual selection theory are cited (Bartlett *et al.* 1993: 978), while supporting cases are ignored (e.g., Böer & Sommer 1992).

The review states that “many instances of infant killings are confounded by unnatural conditions”, citing the killing of resident males by humans or the darting of a mother preceding an infanticide (Bartlett *et al.* 1993: 981). Alternatively, one could value such cases as strong support for adaptationist explanations. In fact, an early critic of the sexual selection theory lamented the lack of (experimental) manipulation of group structures (Schubert 1982). In fact, the authors may have ethical concerns about lethal experiments (as do I and other researchers, e.g., Hrdy 1982: 248). But then, evidence in which field researchers protected infants from attacks is also excluded (Bartlett *et al.* 1993: 985).

Some chimpanzee infanticides are omitted “because of the poor observation conditions” (Bartlett *et al.* 1993: 985). However, study sites with good observations are labeled as “urban-like setting” (P. Dolhinow, cited in Aronson 1995: 2). Similarly, habitats without predators count as “disturbed”. But when predators abound, they, rather than new incoming primate males, are considered to be the infant-killers (Bartlett *et al.* 1993: 968).

My observation of a dog snatching an infant langur that fell from a tree after being seriously wounded by a new male (Sommer 1987: 178) begs for the simple interpretation that the attacker caused the death. Instead, my description is rated “significant because it highlights the possible role of feral dogs in the deaths of infant langurs” (Bartlett *et al.* 1993: 968). One could as well consider the role of crows that also scavenge on dying infants.

The authors stress that “the most salient characteristic of the infanticidal episodes . . . is the generalized, overt aggression exhibited by adult males” (Bartlett *et al.* 1993: 984). Moreover, males suffering injuries during invasions into troops provide evidence that “infants were not the sole attack target” (R. W. Sussman, cited in Aronson 1995: 3). However, this is exactly predicted by the sexual selection hypothesis, according to which infant-killings occur in connection with aggressive competition amongst males for access to females. More importantly, how can a male kill an infant without being aggressive? The critics demand non-aggressive aggression. In

addition, it looks as if they confuse a proximate mechanism (aggression) with an ultimate function.

The authors of the review suggest that “infants often place themselves in danger by their own actions” – such as clinging to their mothers during attacks (Sussman *et al.* 1995: 149). However, how likely is it that unweaned infants will *not* cling to their mothers when aggressive males approach? I witnessed time and again that males singled out infants, even wrenching them from their mother’s breast. Female defenders hardly ever suffered a scratch, even if they wrestled with the highly aggressive male – presumably, because females are potential mates (see photos in Sommer 1987).

The review concludes that infanticide amongst wild primates failed “to support the interpretation of infanticide as a primate-wide adaptive complex”; moreover, of the 48 infanticides that were accepted as “observed”, 88% were allegedly “not compatible” with the sexual selection hypothesis (Bartlett *et al.* 1993: 958; Sussman *et al.* 1995: 149; a colporteur is Dagg 1999b: 20). This is gravely misleading. Incompatible would be cases in which fathers killed their own offspring or when males killed weaned offspring. However, information about the genetic relationship between killer and victim is in many cases simply incomplete. Such cases do not at all contradict the hypothesis.

Using such distorted evidence, the anti-adaptationist group condemns the widespread acceptance of the sexual selection hypothesis as “academic mythology” (Bartlett *et al.* 1993: 984) or, even bolder, as “myth of legendary proportions” (P. Dolhinow, cited in Aronson 1995: 2). Similar statements are reiterated by the major European pocket of resistance against adaptationist explanations: Austrian ethologist Irenäus Eibl-Eibesfeldt, a loyal disciple of his academic mentor Konrad Lorenz (Eibl-Eibesfeldt 1984, 1997). Early in his career Eibl-Eibesfeldt wrote an article with the programmatic title “Warum sich Tiere nicht töten” (Why animals don’t kill each other; Eibl-Eibesfeldt 1965). Ironically, he initially referred to the critique of infanticide by my own supervisor (Vogel 1979) in concluding that “an often cited example for the recklessness of genetic selfishness turns out to be the product of premature conclusions” (Eibl-Eibesfeldt 1984: 125; translated by V.S.). This statement was reiterated 14 years later, citing the review conceived at Washington University as additional evidence (Eibl-Eibesfeldt 1997: 142).

Eibl-Eibesfeldt is uneasy about the occurrence of infanticide because it contradicts the basic assumption of classical ethology that an “aggression

drive” – essentially an adaptive “instinct” – is bridled by an “inhibition to kill conspecifics” that ensures that the species survives. The frequent killings amongst humans cannot easily be reconciled with this concept. The term “pseudospeciation” is therefore introduced to explain why homicide is not inhibited: “de-humanizing” does the trick. Interestingly, Jane Goodall borrowed this idea, stating that wild chimpanzees “dechimpized” their opponents before killing them (Goodall 1986: 532). Obviously, “pseudospeciation” rests on the postulate of a “killing-inhibition”. Both terms are as obsolete as the classic concept of “instincts” (see Zippelius 1992). Otherwise, we would need terms such as “de-lionization”, “de-langurization”, and “de-mouseation.” Eibl-Eibesfeldt muses that infanticide is a pathology: chimpanzees (and humans) have perhaps undergone rapid brain expansion, during which some fine-tuned control mechanisms were lost (Eibl-Eibesfeldt 1997: 143).

A similar “loose screw argument” was initially employed by Dolhinow (1977). Infanticide amongst langurs was considered to be “aberrant”, caused by the stressful conditions at “atypical” sites (see also Aronson 1995: 2). I believe this competing hypothesis has outlived its justification for the following reasons. (1) I could never detect a hint of “pathological” behavior during my own observations of langur infanticide; the killers were apparently healthy individuals acting under highly predictable circumstances (Sommer 1987). (2) Langur infanticide has meanwhile been observed at sites with minimal human influence. Provisioning in particular – believed by Dolhinow to cause “crowding” – is absent from, for example, Kanha, India (Newton 1986), and Ramnagar, Nepal (Borries *et al.* 1999b). (3) Other primate species commit infanticide in the wild at undisturbed sites (for a review, see Bartlett *et al.* 1993). (4) Current DNA analyses for Ramnagar langurs show that male attackers were not related to their victims (Borries *et al.* 1999b).

The interpretation of infanticide as a pathology has its roots in typological reasoning. Animals are believed to have a species-specific repertoire of behavior. Those who deviate from such “universals” are consequently “deviant”, “maladaptive”, “pathological” or “abnormal”. “Overcrowding” (Curtin & Dolhinow 1979) is a corresponding misnomer since no God-given gold standard for population density exists. Animals will reproduce rapidly until a habitat’s carrying capacity is reached. Different behaviours might be employed under low and high densities and some individuals will do better than others (cf. Moore 1999). One cannot have it both ways and rightly emphasize that “one genotype can

produce a wealth of different phenotypes” (Dolhinow 1999: 194) while at the same time hang on to the dichotomy of “normal” versus “atypical” (Dolhinow 1999: 195). It is therefore quite peculiar if not self-contradictory that Dolhinow (1994) tends to stress the problematic “reification” of social systems, pointing towards the tremendous “intraspecies behavioral variability” of primates. This resembles modern behavioral ecology, which has done away with the emphasis on “uniformity” and the labeling of alleged “asocial” behavior as “sick”. Instead, selection is believed to produce individuals who are capable of strategic and tactic responses, resulting in behavioral polymorphisms and “plasticity in primate societies” (Fuentes 1999: 183). A behavior such as infanticide is therefore not necessarily an all-or-none phenomenon, but may or may not occur – depending on varying socioecological circumstances. Such a theorem has room for non-conformist life trajectories.

The British ethnologist Alfred Reginald Radcliffe-Brown (1881–1955) helped to pave the way to (since outdated) group-selectionist ideas. He maintained that the health of a society could be measured by how well its members were integrated, societal structure being “like that of the organic structure of a living body” (Radcliffe-Brown 1952, cited in Jay 1963: 224). The analogy to body cells suggests that the individual within society has little value. Dolhinow applied this to societies of non-human primates where age and sex classes fulfill roles to support the “functionally integrated structure”. Variability in behavior may lead to “temporary alterations in the ‘normal’ structure” but such “disequilibrium” will soon give way to the original “equilibrium” (Jay 1963: 239). This catechism of harmonious togetherness knows no place for acts that do not foster group cohesion.

Tragically, Nazi ideology was built on similar convictions, and Konrad Lorenz himself lent pseudoscientific credibility to it. He, who held a degree in medical sciences, proposed to eradicate “asocial” members from the “supra-individual organism” of the Deutsches Reich, just as a doctor has to cut out cancerous cells from individual bodies (Lorenz 1940, cited in Müller-Hill 1984). There is certainly no straight line from the Nazis via Lorenz to the rejection of the idea that so-called “selfish” behavior is pathological. However, it is worth noting that group-selectionist (= systemic) arguments can be as easily perverted into fascist currency as can gene-centered (= individualistic) views (cf. Segerstråle 1992, 2000).

Rather unspecified political fears were expressed by Phyllis Dolhinow: “You are playing with fire when you use such a loaded word”

as infanticide (cited in Aronson 1995: 3). She stops short of transforming into an outside-paradigm critic; but Anne Innis Dagg (1999a) in her attack on adaptationist explanations clearly does become one. The sexual selection hypothesis “resonates with Western culture in which many people accept male dominance and aggression and condone in part the control of female sexuality by men”. She commits the moralistic fallacy by stating how the world ought to be: “There is little hope of changing society if social behavior is considered as a genetic trait”. Dagg is not very specific about which of the multitude of existing societies she would want to change and how and why, but her agenda is obviously an anti-conservative and progressive Western platform: “Sociobiology clearly treads on dangerous ground” and “locates us in a biological fatalism that a correct cultural interpretation would avoid” (Dagg 1999a: 940, 947–8). Dagg (1999a: 947) categorizes previous research about infanticide as “not good science”. The moral flavor of her “good” is the subject of the following section.

Science is politics: critique from outside the paradigm

The modern evolutionary paradigm maintains that the structure of animal societies is delicately balanced between cooperation and competition – with infanticide as one consequence. Is this really a hard and fast “objective” approach to the study of nature? Scientists may believe that their methods safeguard them from being subjective. However, topics certainly become fashionable (or unfashionable) with the prevailing *Zeitgeist*.

The theory of evolution is linked to the 19th century mantra of “progress”, fostered by Victorian optimism after the so-called civilized nations had gone through a period of “enlightenment”.

Darwin did not dwell on the selective forces reflected in the morphology of genitals – probably muffled by the mores of Victorian England. It took a century, and perhaps the so-called “sexual revolution”, until genital sexual selection became a recognized area of research (cf. Dixson 1998).

“Overcrowding” was a buzzword in the 1960s (Calhoun 1962) when fears about human population growth flared up. Early reports about infanticide amongst monkeys were also linked to this concept (for critique of underlying assumptions, see Moore 1999).

“Tool-making” defined early humans in much of 19th and early 20th

century palaeoanthropology – in all likelihood a reflection of the abundant technology associated with the industrial revolution. Likewise, theories of “Machiavellian intelligence” as a means to manipulate others (Byrne & Whiten 1988) flourished when information technology became widely available.

Still, average scientists tend to be naive positivistic empiricists, convinced that “results” describe “reality” more or less objectively. Disciplines more open to meta-theories such as philosophy or theology point out that all measurement depends on categories. Otherwise, we would have nothing to quantify. But do categories really exist or are they in some ways social or linguistic constructs? Consider, for example, “species”. The borders of this category are hard to define in a longitudinal phylogenetic perspective, but even cross-sectional perspectives reveal blurred edges such as interbreeding with other “species”. Categories are certainly useful but the heuristic importance is not in itself proof that such entities exist *per se*. The philosophical tradition that argues in favor of the existence of categories has its roots in Plato’s teaching about “ideas”: perfect [“ideal”!] “horsehood” or “beautihood” exists independent from the imperfect specimen of horses or beautiful beings we see here on earth. This tradition is, somewhat confusingly, not called “idealism” but “realism” – because the categories are believed to be real entities. The opposing school according to which categories are simply “names” (Latin *nomen*) is called “nominalism”.

If reification troubles physics (does “gravity” exist, or an “atom”?) then the conversion of abstractions into concrete entities poses even more of a problem for biological sciences (is “intelligence” or “race” something concrete?; cf. Segerstråle 1992). The debate about infanticide is heavily infested with essentialized terms such as “species”, “population”, “instinct”, “social system”, “selection”. Thus, even natural scientists should warm to the idea that we not only define the borders of the world by naming the parts we find in our environment, but that we in certain ways also “create” worlds.

This has implications for the judgment about the vigorous attack on the *naïveté* of natural sciences that evolved in the late 1980s from the left-leaning postmodern movement (cf. Haraway 1989). For scientists, the choice and usage of certain categories to “foreground” (Haraway 1989: 311) could simply be a matter of heuristics that employ reductionism and simplification. However, postmodern outside-paradigm critics assume that such choices are contaminated with politics. For example, evolution-

ary biologists who believe that they can *reconstruct* the natural history of infanticide can be *de-constructed* as promoting male violence against females. To unmask hidden agendas is to teach that the truths about the world are moral truths. Scientists may postulate that they try to distinguish true from false, but according to postmodern critique any construction of reality is a decision between good and evil (for the following, cf. Cartmill 1998).

To stress that humans are a specific kind of animal, that they are influenced by genetic information and that there was no right or wrong for the billions of years it took life forms to evolve on earth is a deeply suspicious notion for postmodernists: It restricts the (allegedly) unlimited freedom of humans to shape their destiny. Humans are believed by postmodernists to be utterly different from other animals – an idea that is not easily compatible with that of evolution. Such a *Weltanschauung* has been labeled “secular creationism” because the dislike for evolutionary theory is similar to that found in religious creationism where evolution is unacceptable because it “demotes” humans to the status of beasts. The Christian Right ideology of creationism is virtually endemic to North America. In contrast, the multicultural Leftist ruminations have not only infiltrated Europe but were in large part conceived in France (e.g., Derrida 1970; cf. the counterattack mounted by Sokal & Bricmont 1998).

As diverse as these secular and religious discourses may be, they are united in their dislike for Darwinism and follow the moralistic fallacy. Scientists may find the accusations entirely silly, but the basic idea of a feedback in which science is influenced by and itself influences the given contemporary political climate is not unwarranted. Thus, even if science does not have an explicit political agenda, it may have one implicitly. I will now evaluate some typical accusations raised by postmodernists.

Accusation: science is sexist

Postmodernists argue that the patriarchal structure of science places rationality above feelings and thus facts above values, rendering it sexist. Behavioral ecology should not have much of a problem with this because women and the particular views of women, especially on male-biased interpretations, became deeply influential in the development of this field. For example, the orthodox concept that male mammals are promiscuous (because their reproduction is limited by access to fertile females) and that females are monogamous (because they do not gain anything from copulations with multiple partners) has been replaced with a whole

repertoire of reproductive strategies that have little to do with the idea that females are “passive” (Hrdy 1981). Thus females may be inclined to seek extra-pair copulations in order to accrue better genes, to elicit additional parental investment or to confuse paternity in order to minimize the risk of (*sic*) infanticide.

One consequence of these discoveries is the “re-education” of male researchers who will now readily admit the nastiness of the reproductive strategies of their own “demonic” sex (Wrangham & Peterson 1996). But male-ist *mea culpa* might be unnecessary because notable “Darwinian feminists” such as Sarah Blaffer Hrdy, Barbara Smuts or Patricia Gowaty disagree with the postmodern paradigm by arguing that females are not innately “better” than males. They merely compete by other means – such as female choice or inciting male–male competition (Gowaty 1997a). Females will even tip the balance of physical power if they can get away with it – as is illustrated by female coalitionary dominance over males in perhaps our closest living relatives, the bonobos (Parish 1996). Finally, even infanticide committed by *females* became a resilient feature (Digby, Chapter 17).

Nevertheless, Haraway warns that to center debates on the biological meanings of infanticide “could be dangerous territory on which to build feminist approaches to science” because “women rarely control the field structure of interpretation” (Haraway 1989: 311, 418). This brings us to the ultimate “evil” of behavioral ecology: competition.

Accusation: science is capitalistic

Behavioral ecologists are most easily assailed in this arena given that game-theoretical approaches and the method of cost–benefit analysis are direct descendants from economics (cf. Maynard Smith 1982). Centers such as that for Economic Learning and Social Evolution at University College London evidence this basic link. One could even subsume all of sociobiology and behavioral ecology as a subdiscipline of economics – although the primatologist stumbling through a muddy rain forest hoping to glimpse who attacked whom does not easily pass as a stockbroker. Still, sociobiologist fieldworkers are likely to come up with a net-benefit interpretation for the observation. Undoubtedly, sociobiological arguments about infanticide are “firmly anchored in the narrative of genetic investment” (Haraway 1989: 430).

The German sociologist Max Weber (1864–1920) connected capitalism with certain religious traditions. He drew attention to the Christian

concept of predestination fostered by the Swiss reformatory Johannes Calvin (1509–1564). Calvin disagreed with the Catholic church on how to obtain salvation. Catholicism taught the principle of “ut . . . ut”, which states that a believer could acquire eternal life through a mixture of God’s grace *and* good deeds. Calvin rejected this idea – for God would not be absolutely free if He allowed Himself to be influenced by the virtuous deeds of His creatures. Thus the fate of humans was already predestined by God’s inexplicable decisions even before birth: for either eternal salvation or eternal damnation. It did not matter what one did. Such conviction came in handy for early capitalists who argued that their own riches were testimony of God’s choice; helping the poor would mean to revolt against the divine plan. Such thinking was fertile soil for the social Darwinism of the 19th and 20th century, except that God’s will was substituted by the selection processes of evolution (cf. Mühlmann 1984). Evolutionary biologists would be naive to assume that their ideas about mechanisms of adaptation are not at least subtle extensions of such histories of intellect.

Accusation: science is oppressive

Postmodernists state that science oppressively demands that we argue in an approved way and that science is thus part of a conservative strategy to suppress political progress. One can raise the counterargument that scientists span a broad range of political thought or even conclude that sociobiology is a Communist conspiracy. After all, J. B. S. Haldane and John Maynard Smith were members of the British Communist Party, Robert Trivers was involved in radical Black politics, and “E. O. Wilson and most other leading sociobiologists are left-center liberals or social democrats” (van den Berghe 1981: 406). However, facts will matter little because the postmodern critique assumes an “inherent connection between political motivation and bad science” (Segerstråle 1992: 203). Any meaningful dialogue ends if postmodern critique voices such totalitarian views.

Interestingly, argued from within the paradigm, science is designed to be non-oppressive and open to heretics. Ideally, and often factually, those are rewarded who overthrow a reigning paradigm by convincing their peers that a competing hypothesis offers better explanatory value. Such a struggle is reflected in the exchange of arguments between adaptationist and non-adaptationist explanations of infanticide (data artifact, pathology, by-product). The latter are notoriously annoying, upsetting, and

unreasonable for me (and others). Still, my opponents have – and should have! – the freedom to criticize my work, which will hopefully spur me to deliver stronger arguments or new data.

However, deriving testable predictions from competing hypotheses has its gray zones. For example, the often reiterated Popperian theorem that the possibility of “falsification” is the cornerstone of proper scientific method is limited. For instance, one cannot falsify the assumption that something (God perhaps, or the death star) does *not* exist – if it *really* does not exist – because the falsification would have to show its existence. Non-adaptationists may at times feel that they are trapped in a similar circle. For example, Phyllis Dolhinow (pers. comm.) complained that journals tend not to accept articles that provide “negative evidence”, i.e., reports on resident male changes in primates groups that did not lead to infant-killings.

Moreover, hypotheses multiply once a paradigm “goes baroque” (typically, before it “goes broke”. . .). Behavioral ecology may be approaching this era. Ironically, it is more difficult to reject certain hypotheses once the empirical basis is broadened, because living systems are complex. For instance, half a dozen explanations are now available for why infanticide might be adaptive, and one can almost never go wrong with a reference to resource competition. In addition, infanticide has advanced to a mega-paradigm in that the risk of infant-killing is contemplated to be the very reason for male–female associations itself (van Schaik & Dunbar 1990; van Schaik & Kappeler 1997). Consequently, and somewhat paradoxically, infanticide is relatively rare or may even have become extinct because the severe threat bred sophisticated counterstrategies, as perhaps happened in gibbons (Reichard & Sommer 1997) and bonobos (Parish 1996). Similarly, thousands of hours of focal animal sampling in downtown London may never yield a single datum point about a pedestrian killed by a car; still, nearly all movements of *Homo sapiens* in this habitat reflect avoidance of vehicles. We are therefore advised to not confuse rates of mortality with the risk of being killed (Dunbar 1988). So it seems that the study of infanticide is slowly evolving into a study of events that do – more often than not – *not* happen.

Amongst the fallibles

The confrontation with outside-paradigm critics does not lend itself to solutions because the study of nature cannot be free from subjective constraints. In physics, Heisenberg’s uncertainty principle states that one

cannot observe something without influencing its path because the minimum amount of energy needed for a measurement is a photon, which will inevitably influence the very thing it is supposed to measure. The measured entity will, by the same token, influence the measurer. Thus “objectivity is a subject’s delusion that observing can be done without him” (Heinz von Foerster, cited in von Glasersfeld 1992: 31). Our epistemology is left with, at best, a “hypothetical realism” (Lorenz 1973). As a consequence, natural scientists have no monopoly on what is a correct world-view. By the same token, postmodernist schools can be light-handedly caught in their own de-constructionist nets: Time dependency is definitely characteristic of late 20th century agendas on political and social equality. To declare that one holds the holy grail of “a correct cultural interpretation” (Dagg 1999a: 947f) is self-righteous and approaches totalitarianism.

I do not hesitate to conclude that natural scientists, too, are not immune against the two fallacies. We commit moralistic fallacies with transition from “Ought to Is” when formulating hypotheses: the hypothetico-deductive method cannot work without *pre hoc* categories, and these tend to be derived from a capitalistic cost/benefit approach to nature. Similarly, we commit naturalistic fallacies and transgress from “Is to Ought” when we like or dislike what study-animals do. Who could remain unmoved if our closest primate relatives engage in conspecific killings? Understandably, primatologists sometimes intervened in infanticidal attacks (cf. Goodall 1986). The problem is most obvious with respect to the widespread practice of child abuse and infanticide amongst humans, since few behavioral ecologists will *not* wish this pattern to decrease. Such moral agenda is even employed to justify the study of infanticide: “If you know these things better, you know what to do, take certain measures, counsel people. It arms us” (Carel van Schaik, quoted in Zimmer 1996: 78). The position resembles that of Darwinian pioneer Thomas Henry Huxley, who in 1888 proclaimed that humans could not learn anything from nature except that it is full of evil. The “good natured” heritage that de Waal (1996) stressed is just the flipside of this coin, which will never become outdated as long as humans interact. Consequently, we probably cannot develop rational ethical choices without recurring to nature (Liedtke 1999).

Does this mean that empiricist approaches to infanticide are futile? Outside-paradigm critics may think so, whereas I do not. Behavioral ecology reflects a certain type of culture to which members of the scientific community adhere, and this set of rules produces impressive

results for those who “believe” in it (“emic perspective”). For me, this is enough justification for my work. We cannot – and should not! – expect a synthesis or reconciliation with a paradigm that operates under different rules (“etic perspective”).

Perhaps, there can one day be the “new holism” E. O. Wilson (1975: 7) hoped for and still dreams about (Wilson 1998) – enabled by evolutionary theory. In the meantime, I at least, do not want my world to be stirred into a gigantic homogenization of paradigms. My desire is probably evidence for the trend to base ethics on esthetics. The powerful proximate mechanisms of my neurochemistry make me cherish a world full of beautiful biodiversity – and mind-boggling tradidiversity. Live, let live and – one hopes – be allowed to live: a wisdom strangely obtained from the study of infanticide.

The within-paradigm critique of the study of infanticide is a conflict that in principle can be resolved. Opponents subscribe to similar if not identical sets of rules that include careful comparison or experiment. We will have to design conflicting hypotheses in such a way that their predictions will become mutually exclusive. This is relatively easily said and done. Alas, the hard part is to test the predictions under natural conditions because observations leave much room for subjective interpretation: one observer’s “goal-directed attack” might be another’s “generalized aggression”. Similarly, DNA analyses for langur monkeys at Ramnagar, Nepal (Borries *et al.* 1999b), that exclude attackers of infants as fathers can be interpreted as supportive for the sexual selection hypothesis. Alternatively, one can raise the bars and reject the sample size of this study as too small or point out the lack of evidence for other langur populations or other primate species. In any case, we should not fool ourselves that in the end certain hypotheses can be proven or – à la Dagg (1999a: 947) – “disproven”. Like it or not, we will have to resign ourselves to “plausible” and “implausible” explanations.

Yet, on the whole, I am optimistic about this process – as much as I may be upset about notorious counterarguments. But this is probably true for my opponents as well. I view the science I conduct as a sports match – certainly not free from emotional and economic involvement, but subject to mutually agreed rules. My arguments may ultimately succumb. However, until then, I will defend my conviction with passion – in this case the belief that the pattern of infanticide has been brought about by selective forces.