Anim Cogn (2003) 6 : 293–299
DOI 10.1007/s10071-003-0197-4

ORIGINAL ARTICLE

Frans B. M. de Waal

Silent invasion: Imanishi's primatology and cultural bias in science

Received: 29 July 2002 / Revised: 5 November 2002 / Accepted: 1 September 2003 / Published online: 10 October 2003

When it comes to our relation with nature, there is no escaping the tension between perception and projection. What we discover in nature is often what we put into it in the first place. Consequently, the way naturalists have contributed to humanity's know-thyself mission can be understood only in the context of the stained glasses through which they stare in nature's mirror. Given that these glasses cannot be taken off, the next best thing is to compare alternative ones.

The present essay explores cultural bias in the context of my own little corner of science, which is the behavior of monkeys and apes. Inasmuch as the way we look at other animals reflects the way we look at ourselves, the study of animal behavior is subject to far greater cultural preconceptions than many other fields of the natural sciences. For example, one can look at organisms as cooperative ventures – both on the inside, among cells within the body, and on the outside, when animals cooperate to survive. But one can equally well stress cut-throat competition and so-called "selfish" genes. It is not hard to support either position, but in the West we certainly love to depict nature as red in tooth and claw.

The founder of Japanese primatology, Kinji Imanishi (1902–1992), saw nature as inherently harmonious. Species fit together in a large organic whole, each species finding its own niche. That the contrast with the Western approach persists today was related to me by Tijs Goldschmidt, a Dutch biologist, who works on cichlids in Lake Victoria (Goldschmidt 1998). Goldschmidt once visited Japanese colleagues working on the same fish family in nearby Lake Tanganyika. Whereas Goldschmidt's own team explained the species explosion in these lakes in terms of competition and mutual exploitation, the Japanese saw it in terms of complementary roles within the ecosystem. While the two teams agreed on the data, they operated on the basis of strikingly different outlooks.

East–West disagreements about the naturalness of competition versus cooperation go back at least to the late 19th century debate between Thomas Henry Huxley and Petr Kropotkin, in which the former took a "gladiatorial" view of nature and the latter advocated a more synergistic model (Todes 1989; de Waal 1996). These disagreements rarely show a clear winner. They rather tend to have the flavor of the-glass-is-half-full versus the-glass-is-half-empty debate.

A prime instance of such a debate was the reaction to Imanishi's views by British paleontologist, Beverly Halstead, who found it necessary to travel all the way to Japan to set the old master straight. In 1984, armed with a heavy load of prejudice and admitting no firsthand knowledge of Imanishi's writings, Halstead came to confront Imanishi. In his unpublished English manuscript (a copy of which can be found in the Kyoto University Library), he noted: "In my Western way, I came to Kyoto, the home of Imanishi and his School seeking the man and his ideas, but I came as an avowed opponent" (Halstead 1984, unpublished manuscript). After handing the 82-year-old emeritus professor a gift – a bottle of whisky – he presented him with a document translated into Japanese containing statements such as "Imanishi's evolution theory is Japanese in its unreality" and "You see the wood, but the trees are not in focus". No wonder that Imanishi's face, as Halstead recalled, betrayed profound regret at having agreed to the meeting.

What could have compelled Halstead to be so rude? Why, upon his return to Britain, did he write an article that trashed not just Imanishi's views, but his entire country as well? How did *Nature* dare run it with the following patronizing introduction: "The popularity of Kinji Imanishi's writings in Japan gives an interesting insight into Japanese society" (Halstead 1985)? Could not the same subtitle be applied to, say, Darwin's theory? As has been pointed out many times, it can hardly be coincidental that ideas about free-market capitalism and the struggle for existence arose at the same time in the same place. The common habit of framing evolutionary questions in terms of costs and benefits leaves little doubt about this connection.

The first Imanishi-Itani Memorial Lecture of Primatology, Kyoto, 18 February 2002

F. B. M. de Waal (✉)
Living Links, Yerkes Primate Center, Emory University,
GA 30322 Atlanta, USA
Fax: +1-404-7273270,
e-mail: dewaal@emory.edu

What we have here, then, is the familiar case of one culture perceiving another's biases more acutely than its own. Even if Imanishi's ecological and evolutionary ideas are now considered problematic, he and his followers were right about quite a few other things. This is clear from the fundamental change that has taken place in the study of animal behavior over the past few decades: Western scientists have on a grand scale adopted Japanese concepts and approaches, albeit often ignorant of their origin. It is unclear if they have also embraced the underlying outlook, which is rather far removed from their own, yet they certainly have been receptive to the observation techniques and specific concepts, such as individualized relationships and cultural transmission, first employed within Japanese primatology.

Imanishi's legacy

Imanishi was an extraordinarily prolific, widely known author in the life sciences: the Stephen Jay Gould of Japan. He started out as an entomologist, but was also an ecologist, anthropologist, primatologist, mountaineer, and philosopher. He received an official faculty position – in the humanities, not the sciences – only after he was approximately 50 years old. Being of wealthy descent, he could do whatever he wanted without the obligations that come with salary. He had only one room at Kyoto University with no furniture other than a low desk at which he wrote his books sitting in lotus position on a tatami: an ascetic, cultured man of immense influence.

Apart from being a Himalayas climber, Imanishi had two main interests. One was the interconnectedness among all living things and the environments in which they are found. Even though he rarely mentioned those who influenced him, he was widely read and elements of his approach are traceable to outside influences, ranging from Jacob von Uexküll to Petr Kropotkin, and perhaps most of all Kitaro Nishida, founder of a school of philosophy that was particularly influential in the 1930s and 1940s. I cannot judge this for myself, as my information comes from secondary sources, but Imanishi's emphasis on intuition and perception of the whole, his dislike of reductionism, and his view that the individual is secondary to the society probably derived from Nishida, the Kyoto philosopher of "nothingness," who used to think deep thoughts while strolling along a rustic little river lined by cherry trees – still known as the philosopher's way – that runs past the university campus (Yoshimi 1998).[1]

Imanishi did most of his research on mayfly larvae in the much larger Kamogawa River that runs through Kyoto's heart. His work on aquatic life led him to develop the idea of habitat segregation, meaning that different but related species select their own distinct lifestyles and microhabitats, which allows them to coexist harmoniously in the same environment. Imanishi did not seek to explain how segregation might have come about, and was vehemently opposed to explanations that involved strife.

The second interest and lasting legacy of Imanishi concerns the study of primate behavior. Here, the approach was very innovative thanks to the absence of human–animal dualism. Being the product of a culture that does not set the human species apart as the only one with a soul, Imanishi had trouble with neither the idea of evolution nor that of humans as descendants of apes. To the Buddhist mind, this is eminently plausible, even likely, and has nothing insulting about it (Asquith 1991; Sakura 1998; Matsuzawa 2003).

Plato's "great chain of being", which places humans above all other animals, is absent from Eastern philosophy. In most Eastern belief systems, the human soul can reincarnate in many shapes and forms, so all living things are spiritually connected. A man can become a fish and a fish can become a god or goddess. The fact that primates, our closest animal relatives, are native to China and Japan, has only helped to strengthen the belief in the interconnectedness of life. Unlike European fables, which are populated with ravens, rabbits, foxes and the like, Japanese and Chinese folk tales and poetry are laced with references to gibbons and monkeys, such as the three wise macaques of Tendai Buddhism ("see no evil, hear no evil, speak no evil").

Feeling humility towards animals affects the way we study them. The study of animal behavior in Japan has never been contaminated by feelings of superiority or an aversion to acknowledging humanlike characteristics in animals. According to Imanishi's most respected student, the late Jun'ichiro Itani (1926–2001): "Japanese culture does not emphasize the difference between people and animals and so is relatively free from the spell of anti-anthropomorphism ... we feel that this has led to many important discoveries." (Itani 1985; see also Asquith 1984, 1986). Thus, Japanese primatologists plotted kinship relationships over multiple generations, assuming that animals must have a complex family life, just like we do. They started all of this before any Western scientists thought of it (e.g. Kawamura 1958; Yamada 1963), and years before William Hamilton (1964) developed kin selection theory. Kinship networks were a true discovery, perhaps the greatest of Japanese primatology (Reynolds 1992).

In fact, the smooth acceptance of one major aspect of evolutionary theory – the continuity among all life forms – meant that questions about animal behavior were, from the start, uncontaminated by the human/animal divide assumed in the West. As a result, Imanishi's students moved ahead rapidly with a distinctly anthropological agenda: by studying other primates, they sought to understand the origins of the human family and society. In all of this,

[1] In the fall of 1998, I visited China and Japan for several months on a scholarship provided by the Japan Society for the Promotion of Science (JSPS). During this time, I spoke with many different primatologists and other academics both at my home base, Kyoto, and throughout Japan, from the north to the south. This included a discussion with the late Jun'ichiro Itani, who offered me a firsthand glimpse of Imanishi's early ideas and their reception both in Japan and in the West. It also included a visit to Koshima and discussions with Satsue Mito, who from the start assisted the studies of potato-washing on the island. The reader is referred to de Waal (2001), for a more comprehensive account of my impressions of Japanese science and the connections and differences to the Western approach.

Imanishi was well ahead of the celebrated Western paleontologist, Louis Leakey, who developed a similar agenda. Leakey sent several primatologists out to study great apes in the wild in the belief that these animals could provide us with information about the earliest stages of human evolution. But by the time he did so, in the 1960s and 1970s, the questions and techniques that would prove useful in this endeavor had already been developed by Japanese primatologists, who had individually identified their monkeys and followed them long enough to understand the extraordinary complexity of primate society, and the degree to which every group was different. Most importantly, Imanishi had formulated the question of animal culture in a way that invited further study (see following discussion).

But instead of comparing Imanishi with Leakey, the more appropriate parallel is with Ray Carpenter, the American primatological pioneer. Carpenter was a trained physiologist, but also a first-rate behavioral scientist who preferred the field over the laboratory. He worked on rhesus macaques released on the Caribbean island of Cayo Santiago as well as on wild howler monkeys and gibbons. He was interested in social relationships and drew sociograms that mapped group structure (Carpenter 1964). He did not go nearly so far in this as the Japanese primatologists, who were able to distinguish over a 100 monkeys and trace their family ties over generations, but Carpenter shared with them a distinctly "sociological" outlook. It is not surprising, therefore, that Carpenter was the first Westerner to become a staunch supporter of Japanese primatology (see Fig. 1).

Fig. 1 Kinji Imanishi (1902-1992) and Ray Carpenter (1905-1975), in front of Pennsylvania State University, where Carpenter was working during Imanishi's visit to the United States. Carpenter is holding the first ever issue of the journal *Primates*, the oldest journal in its field, now published by Springer-Verlag. The photograph was taken by the late Itani in 1958. Copyright: Jun'ichirou Itani Memorial Visual Archives of the Primate Research Institute, Kyoto University. With special permission from Tetsuro Matsuzawa

Imanishi's approach to primate behavior amounts to a paradigmatic shift that today has been adopted by all of primatology and beyond. Thus, if we no longer perceive anthropomorphism as the problem it once was (Mitchell et al. 1997; de Waal 1999), and if students of long-lived animals in the field – whether they watch dolphins or elephants – routinely identify individuals and follow them over their life span (de Waal and Tyack 2003), then we are employing techniques from the East initially mocked and resisted by the West.

Language barrier

In 1958, Imanishi and his students toured American universities to report their findings. They encountered a great deal of ridicule for humanizing their subjects, and profound skepticism about the ability of mere humans to distinguish all those monkeys. People found it hard to believe that such a feat was even possible, expressing disbelief in front of their visitors (Itani, personal communication). We should also not forget that it has been only a few decades since university professors in the West used to warn primatology students against the atheoretical approach, the anthropomorphism, and the general lack of relevance of papers by Japanese colleagues, while discouraging reference to any of this literature (Asquith 1996).

So, how is it possible that the basic tenets of Imanishi's school are now all but taken for granted in the West? To understand how this "alien invasion" of ideas could have taken place under our noses, we need to look at Eastern culture, and also appreciate how linguistic monopoly affects science (Gibbs 1995).

The answer to the first question is, as we have seen, that Eastern science had no fondness for the traditional Western human/animal dualism. The advantages of ignoring this dualism were immediately obvious to open-minded scientists, such as Carpenter, who helped speed up a process in the West that might have occurred anyway.

The answer to the second question lies in language (Bartholomew 1998; Asquith 2000). It is hard for non-English speakers to make themselves heard in an English-speaking world. Since English is not my native tongue, I am familiar with the effort involved in writing and speaking another language – even though my native Dutch is probably the closest any other language comes to English. As a student, all of my textbooks were in two foreign languages (German and English), and when I later started writing papers I spent an inordinate amount of time searching through thick dictionaries to express my thoughts in English. This effort, which has to be multiplied by ten for Japanese scientists, is blithely ignored by native English speakers.

The typical native English speaker is monolingual. Lack of familiarity with other languages makes him or her imagine that these must be copies of English. Other languages have not only different words and grammars, they also represent different worldviews. They are conceptually different, so that many expressions and nuances are un-

translatable. For example, Dutch is richly sprinkled with diminutives – nonexistent in most languages – which convey a cuteness befitting a small, tidy country. And the way the French talk about food is not even imaginable for most English-speakers. The differences go far deeper than this, though: they determine the way we construct reality. Thus, if Chinese words often serve as both nouns and verbs, this makes it natural for the Chinese to see objects as events, and to understand the world as consisting of processes instead of entities.

English, which through an accident of history has emerged as the world's dominant tongue, is a perfectly fine language. To have a single language for international papers and conferences is far preferable, in my mind, than to have a number of competing languages. So, English itself is not the problem: the problem is the attitude of native English-speakers.

Naturally, you speak your own language faster than any other. This can make it impossible for those who are not native English speakers to keep up at international meetings. It is worse on those occasions when an English speaker does not pull any punches while debating a scientist whose English is poor. I have seen it happen often. The English speaker rises from the audience, articulates a penetrating question, sometimes with a joke mixed in, and barely takes the time to listen to the clumsily phrased reply of his opponent. Since native English speakers dominate all discussions, they form a class of great minds strutting around in the secure knowledge that no one will dare challenge them.

Good ideas formulated in bad English either die or get repackaged. Once the idea has been moved into the domain of good English, its origin tends to be forgotten. It is a bit like a movie interpretation of a French play (e.g. "The Birdcage" – "La Cage aux Folles;" "Beauty and the Beast" – "La Belle et la Bête"): once the movie has come out, the vast majority of people believe that the idea must have arisen in Hollywood. Once expressed in English, an idea becomes English or American. This is a natural process that probably applies to any language, yet in science we should give intellectual credit where credit is due.

So, one reason Eastern thinking could creep into the study of animal behavior unnoticed is that it filtered into the literature through awkward formulations and translations that native English speakers found easy to improve upon. In the process, they proceeded to erase part of the credit for the new ideas. Hence, even though Imanishi put us on track of animal culture – which is now about the hottest topic in our field – Western scientists rarely, if ever, mention his name in this context.

Animal culture

As far back as 1952, when European ethologists were working on instinct theories and American behaviorists were rewarding rats for pressing levers, Imanishi wrote a paper that criticized established views of animals (Imanishi 1941, 1952). He inserted a debate between a wasp, a monkey, an evolutionist, and a layman, in which the possibility was raised that animals other than ourselves might have culture. Hirata et al. (2001) provide a translation of a portion of this imaginary debate. The proposed definition was simple: if individuals learn from one another, their behavior may, over time, become different from that in other groups, thus creating a characteristic culture (Itani and Nishimura 1973; Nishida 1987).

This approach brought culture down to its lowest common denominator: the social rather than genetic transmission of behavior. It was confirmed within a few years of the book's publication, from observations of Japanese macaques washing sweet potatoes on Koshima island. We now know that cultural learning is widespread, and includes birdsong (e.g. West et al. 2003), the use of tools by chimpanzees (e.g. Whiten et al. 1999), and the hunting techniques of whales (e.g. Rendell and Whitehead, 2001). New examples are discovered almost daily.

Until recently, however, Western scientists have resisted the idea of animal culture, mainly by insisting on highly specific mechanisms of social transmission – such as teaching and imitation – that many animals may not show, and that even human culture may not rely on to the degree that these authors assume (Premack and Premack 1994; Tomasello 1994). In a direct challenge to Imanishi's school, Galef (1992) wrote an influential critique of the Koshima studies in which he claimed that (1) food provisioning on the island may have been conducted selectively so as to reward individuals who showed desirable behavior, such as potato washing, and (2) individual learning might suffice as an explanation of the spreading of the habit. Galef, who himself never set foot on Koshima, relied on a couple of sentences in Green (1975), who did visit there in 1968 and 1969.

By that time, staff on Koshima would occasionally accommodate people who wanted to see monkeys perform potato washing. They did so by feeding the monkeys close to the ocean and making sure the best "performers" were at hand. Green (1975), who attended only one potato feeding during his entire stay, was aware that the method of provisioning was intended to benefit tourists and visiting researchers, such as himself. His anecdotal account from more than 15 years after the start of potato washing cannot possibly tell us how the habit originated or spread. For this, we need to turn to the careful documentation by Kawai (1965), which covered a much earlier period in which few outsiders ever showed up on Koshima.

My own discussions with Satsue Mito (who conducted the potato feeding in the early years), raised serious doubts that the procedures imagined by Galef (1992) had ever been applied (de Waal 2001). First of all, this would not have been logical. For a behavior to spread, it is critically important that individuals who do not show the behavior get an opportunity to do so. Hence the need to provide sweet potatoes to non-washing monkeys. Second, one cannot feed a troop of monkeys any way one wishes without causing incredible turmoil. One needs to feed "down" the hierarchy. If one were to feed low-ranking and young mon-

keys before the rest, they would get harassed to a life-threatening degree by the higher-ups. Mito understood this, and fed the adult males and high-ranking females first. Despite this provisioning technique, potato washing started with the young and low-ranking individuals. In fact, the older males never learned even though they were fed first.

It is furthermore obvious from the excellent reviews by Kawai (1965), Watanabe (1994), Hirata et al. (2001), and others, that potato-washing spread among the monkeys in a manner consistent with the troop's social relationships. Thus, the first individuals to show the behavior after Imo, a juvenile female, had started the habit were her age peers and mother. What would be the probability of this happening by chance? In a post hoc analysis of extant data, the speed of learning appeared consistent with a social transmission model. Lefebvre (1995) argued that social learning can be distinguished from individual learning by the rate with which a novel behavior spreads. Social learning can be expected to produce an accelerating function, as the increase in the number of practitioners of the new behavior increases the likelihood of naïve individuals being exposed to it, whereas individual learning can be expected to produce a linear function. Based on these assumptions, Lefebvre concluded that most analyzable primate traditions were consistent with social learning. There is no urgent reason, therefore, to abandon potato-washing on Koshima as an example of social transmission of a specific habit in a monkey population (de Waal 2001).

This is not to say that the issue brought up by Galef et al. of the mechanism of transmission is irrelevant. Whereas biologists consider mechanism secondary to function, certainly at the level of the definition of a phenomenon (e.g. we do not define locomotion or respiration by the way it is accomplished), the various ways in which cultural transmission is achieved remain a puzzle, and demand full attention. It should be added, though, that learning psychologists, coming out of a tradition in which single animals are tested on artificial tasks, may not have the best approach available to tackle this puzzle. This can be illustrated by Galef's (1992) confident claim that imitated acts will automatically extinguish if they fail to deliver rewards.

Whereas this is logical from a reinforcement perspective, we know a lot of examples of culturally transmitted behavior without any reward value. Just two examples are the stone handling of Japanese monkeys on Arashiyama, which appears to be a functionless activity (Huffman 1996), and the nut-cracking attempts of immature chimpanzees. Infant and juvenile chimpanzees lack the skill, strength, and coordination to crack nuts, yet in their games combine stones and nuts in a manner that eventually leads to the cracking of edible nuts. Their first successes occur only after many years of unrewarded activity, however (Inoue-Nakamura and Matsuzawa 1997). In fact, contrary to Galef's claim, extrinsic reinforcement may have little relevance for the spreading of cultural habits (de Waal 2001).

An ecologically valid approach to social learning would require that we consider the context in which it takes place in nature. For example, the currently popular imitation experiments, in which nonhuman primates or other animals face models of another species (always ours), or witness a single demonstration of a novel act, are miles removed from the circumstances under which social learning evolved. In nature, observers and models belong to the same species and usually copy behavior after having witnessed it over and over again. This is the sort of learning context that we need to know more about.

Rather than focusing on reinforcement, new conceptualizations assume that social learning rests on identification with models and an urge to act like them. Such conformist assumptions underlie de Waal's (1998, 2001) Bonding- and Identification-based Observational Learning (BIOL) as well as Matsuzawa et al.'s (2001) education by master–apprenticeship. These theories have no trouble explaining why imitation experiments with human models have occasionally failed: they assign a critical role to closeness between the observer and its behavioral model, which, together with a desire to behave like others, promotes the behavioral convergence of which culture is made. The proposed mechanisms are socio-emotional as much as cognitive.

Assumptions versus theory

In her illuminating comparisons of Western and Japanese primatology, Asquith pointed out many useful distinctions, such as the aforementioned lack of human–animal dualism in Eastern religion. Asquith does not believe, as Halstead (1985) did, that Darwinism was ever rejected by Japanese scientists, but rather that Western scientists tended towards either-or thinking (i.e., one is either for or against an idea), whereas their Japanese colleagues simply drew on the most appealing elements from evolutionary theory, ignoring the rest (Asquith 1986, 1991).

Japanese primatology was never as atheoretical as some Western scientists believed. My impression is rather that not everyone in the world is comfortable formulating ideas in the fashion popular in the West. We set up theories that are testable, meaning that they can be falsified. For us, science is a confrontational process that seeks to decide who is right and who is wrong. It takes a certain mind-set to operate in this manner. Japanese scientists have no lack of assumptions or expectations about the world, but are reluctant to propose them in a way that invites disagreement.

Japanese primatologists hold clear expectations, which is evident from a little-known historic example from field primatology. Until the late 1960s, the Western view was positively Rousseauian: apes were autonomous "noble savages", free of social ties and obligations. All they did was travel in haphazard combinations from one fruit tree to the next. The ever-changing parties of chimpanzees that researchers were encountering in the forests of Africa seemed to confirm that they lacked a coherent group life.

While Jane Goodall was describing female chimpanzees and their dependent offspring as the only bonded

units (van Lawick-Goodall 1968), a Japanese team working a mere 130 km south of Gombe was working under quite different assumptions. How could a species that supposedly fills the gap between ourselves and other animals lack a complex social life, they wondered. Eventually, through persistent field observations, they cracked the puzzle and showed that chimpanzees live in so-called unit groups with a stable membership (Nishida 1968; Takasaki 2000).

Western scientists soon replaced the term "unit group" by "community," which did not facilitate the retention of the discovery's origin. The male-bonded society of the chimpanzee is now a staple of primatological knowledge – there is ample evidence for territorial aggression between different communities (e.g., Goodall 1986) – but the initial discovery came from a firm conviction that chimpanzees could not be nearly as "individualistic" as Western science had made them out to be.

The important point of all this is that Imanishi's views, even though not phrased as formal theory and reaching the West only with great delay, have clearly won out. Views that were at odds with traditional Western dualisms (e.g., animal/human, nature/culture) slipped into our thinking unnoticed along with observation techniques of great value. This silent invasion from the East helped the West chuck some of its cultural baggage. The way this happened, however, and the general lack of acknowledgment, hint at the difficulties other cultural and linguistic groups experience when they try to find a voice in science.

We should not be deceived by the West's scientific hegemony: it is as unrealistic as that of one country thinking it can order the rest of the world around. The study of nature cannot be left to a single priesthood who all think the same. Each culture is too wrapped up in its own relation with nature to step back and see it as it is. To gain a full picture requires scientists with all kinds of backgrounds, who together take on a task equivalent to comparing the images in a range of fun-house mirrors. Somewhere in that heavily distorted information resides the truth.

Acknowledgements This essay could not have been written without my Japanese colleagues during my pleasant stay in their country. I am immensely grateful to Toshisada Nishida and Tetsuro Matsuzawa, who invited me to Kyoto in 1998 and 2002, respectively, and to the late Jun'ichiro Itani, Pamela Asquith, Toshikazu Hasegawa, Mariko Hiraiwa-Hasegawa, Michael Huffman, Suehisa Kuroda, Satsue Mito, Osamu Sakura, Hideko Takeshita, Kunio Watanabe, Soshichi Uchii, and many other colleagues for helpful discussions. I also thank three anonymous referees of this journal for constructive comments and supplemental references to the literature.

References

Asquith PJ (1984) The inevitability and utility of anthropomorphism in description of primate behaviour. In: Harré R, Reynolds V (eds) The meaning of primate signals. Cambridge University Press, Cambridge, pp138–176

Asquith PJ (1986) Anthropomorphism and the Japanese and Western traditions in primatology. In: Else JG, Lee PC (eds) Primate ontogeny, cognition, and social behavior. Cambridge University Press, Cambridge, pp 61–71

Asquith PJ (1991) Primate research groups in Japan: Orientations and East–West differences. In: Fedigan L, Asquith PJ (eds) The monkeys of Arashiyama. Thirty-five years of research in Japan and the West. SUNY Press, Albany, NY, pp 81–98

Asquith PJ (1996) Japanese science and western hegemonies. Primatology and the limits set to questions. In: Nader L (ed) Naked science. Anthropological inquiry into boundaries, power, and knowledge. Routledge, New York, pp 239–256

Asquith PJ (2000) Negotiating science: internationalization and Japanese primatology. In: Strum S, Fedigan LM (eds) Primate encounters. University of Chicago Press, Chicago, pp 165–183

Bartholomew JR (1998) Japanese Nobel candidates in the first half of the twentieth century. Osiris 13:238–284

Carpenter CR (1964) Characteristics of social behavior in nonhuman primates. In: Carpenter CR (ed) Naturalistic behavior of nonhuman primates. Pennsylvania State University Press, University Park Pa., pp 358–364

Galef BG (1992) The question of animal culture. Hum Nat 3: 157–178

Gibbs WW (1995) Lost science in the Third World. Sci Am 273: 92–99

Goldschmidt T (1998) Darwin's dreampond. MIT Press, Cambridge, Mass.

Goodall J (1986) The chimpanzees of Gombe: patterns of behavior. The Belknap Press of Harvard University Press, Cambridge, Mass.

Green S (1975) Dialects in Japanese monkeys: vocal learning and cultural transmission of locale-specific vocal behavior? Z Tierpsychol 38:304–314

Halstead LB (1985) Anti-Darwinian theory in Japan. Nature 317: 587–589

Hamilton WD (1964) The genetical evolution of social behaviour. J Theor Biol 7:1–52

Hirata S, Watanabe K, Kawai M (2001) Sweet-potato washing revisited. In: Matsuzawa T (ed) Primate origins of human cognition and behavior. Springer, Tokyo Heidelberg Berlin, pp 487–508

Huffman MA (1996) Acquisition of innovative cultural behaviors in nonhuman primates: a case study of stone handling, a socially transmitted behavior in Japanese macaques. In: Heyes CM, Galef BG (eds) Social learning in animals: the roots of culture. Academic, San Diego, pp 267–289

Imanishi K (1941) Japanese view of nature. The world of living things. Translated 2002 by Asquith PJ (ed) Kawakatsu H, Yagi S, Takasaki H. Routledge Curzon, New York

Imanishi K (1952) The evolution of human nature. In: Imanishi K (ed) Ningen. Mainichi-shinbunsha, Tokyo, pp 36–94 (Japanese)

Inoue-Nakamura N, Matsuzawa T (1997) Development of stone tool use by wild chimpanzees. J Comp Psychol 111:159–173

Itani J (1985) The evolution of primate social structures. Man 20: 593–611

Itani J, Nishimura A (1973) The study of infrahuman culture in Japan: a review. In: Menzel EW (ed) Precultural primate behavior. Karger, Basel, pp 26–50

Kawai M (1965) Newly-acquired pre-cultural behavior of the natural troop of Japanese monkeys on Koshima islet. Primates 6: 1–30

Kawamura S (1958) Matriarchal social ranks in the Minoo-B troop: a study of the rank system of Japanese monkeys (in Japanese). Primates 1:148–156

Lawick-Goodall J van (1968) A preliminary report on expressive movements and communication in the Gombe Stream chimpanzees. In: Jay PC (ed) Primates: studies in adaptation and variability. Holt, Rinehart and Winston, New York, pp 313–519

Lefebvre L (1995) Culturally-transmitted feeding behaviour in primates: evidence for accelerating learning rates. Primates 36: 227–239

Matsuzawa T (2003) Koshima monkeys and Bossou chimpanzees: culture in nonhuman primates based on long-term research. In: de Waal FBM, Tyack PL (eds) Animal social complexity: intelligence, culture, and individualized societies. Harvard University Press, Cambridge, Mass. (in press)

Matsuzawa T, Biro D, Humle T, Inoue-Nakamura N, Tonooka R, Yamakoshi G (2001) Emergence of culture in wild chimpanzees: education by master-apprenticeship. In: Matsuzawa T (ed) Primate origins of human cognition and behavior. Springer, Tokyo Berlin Heidelberg , pp 557–574

Mitchell RW, Thompson NS, Miles HL (1997) Anthropomorphism, anecdotes, and animals. SUNY Press, Albany, NY

Nishida T (1968) The social group of wild chimpanzees in the Mahali Mountains. Primates 9:167–224

Nishida T (1987) Local traditions and cultural transmission. In: Smut BB, Cheney DL, Seyfarth RM, Wrangham RW, Struhsaker TT (eds) Primate societies. University of Chicago Press, Chicago, pp 462–474

Premack D, Premack AJ (1994) Why animals have neither culture nor history. Companion encyclopedia of anthropology. In: Ingold T (ed) Routledge, London, pp 350–365

Rendell L, Whitehead H (2001) Culture in whales and dolphins. Behav Brain Sci 24:309–382

Reynolds V (1992) The discovery of primate kinship systems by Japanese anthropologists. In: Itoigawa N, Sugiyama Y, Sackett G, Thompson R (eds) Topics in primatology, vol 2. University of Tokyo Press, Tokyo, pp 79–87

Sakura O (1998) Similarities and varieties: a brief sketch on the reception of Darwinism and sociobiology in Japan. Biol Philos 13:341–357

Takasaki H (2000) Traditions of the Kyoto School of field primatology in Japan. In: Strum S, Fedigan LM (eds) Primate encounters. University of Chicago Press, Chicago, pp 151–164

Todes D (1989) Darwin without Malthus: the struggle for existence in Russian evolutionary thought. Oxford University Press, New York

Tomasello M (1994) The question of chimpanzee culture. In: Wrangham R, et al (eds) Chimpanzee cultures. Harvard University Press, Cambridge, Mass., pp 301–317

Waal FBM de (1996) Good natured. Harvard University Press, Cambridge, Mass.

Waal FBM de (1998) No imitation without identification. Behav Brain Sci 21:689

Waal FBM de (1999) Anthropomorphism and anthropodenial: consistency in our thinking about humans and other animals. Philos Topics 27:255–280

Waal FBM de (2001) The ape and the sushi master. Basic Books, New York

Waal FBM de, Tyack PL (2003) Animal social complexity: intelligence, culture, and individualized societies. Harvard University Press, Cambridge, Mass. (in press)

Watanabe K (1994) Precultural behavior of Japanese macaques: longitudinal studies of the Koshima troops. In: Gardner RA, Chiarelli AB, Gardner BT, Plooij FX (eds) The ethological roots of culture. Kluwer, Dordrecht, pp 81–94

West MJ, King AP, White DJ (2003) Discovering culture in birds: the role of learning and development. In: Waal FBM de, Tyack PL (eds) Animal social complexity: intelligence, culture, and individualized societies. Harvard University Press, Cambridge, Mass., pp 470–492

Whiten A, Goodall J, McGrew WC, Nishida T, Reynolds V, Sugiyama Y, Tutin CEG, Wrangham RW, Boesch C (1999) Cultures in chimpanzees. Nature 399:682–685

Yamada M (1963) A study of blood-relationship in the natural society of the Japanese macaque – an analysis of co-feeding, grooming, and playmate relationships in Minoo-B-troop. Primates 4:43–65

Yoshimi K (1998) Imanishi Kinji's biosociology as a forerunner of the semiosphere concept. Semiotica 120:273–297