LABORATORY PRIMATE NEWSLETTER

Volume 6, Number 4

October, 1967

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CONTENTS ISSUE: VOLUMES 5-6

POLICY STATEMENT
(Revised January, 1967)

The primary purpose of the Laboratory Primate Newsletter is to provide information on maintenance, breeding, and procurement of nonhuman primates for laboratory studies. A secondary purpose is to disseminate general information about the world of primate research. Requests for information, for special equipment, or for animal tissues or animals with special characteristics will be included in the Newsletter. As a rule, the only research articles or summaries that will be accepted for the Newsletter are those that have some practical implications or that provide general information likely to be of interest to investigators in a variety of areas of primate research. However, special consideration will be given to articles containing data on primates not conveniently publishable elsewhere. General descriptions of current research projects on primates will also be welcome.

It should be kept in mind that the Newsletter is not a formal publication and it is not obtainable in most libraries. Therefore citation of Newsletter notes or articles should be limited to special circumstances. This also means that inclusion of material in the Newsletter does not preclude its publication in a journal. As a rule, authors of longer articles will receive two extra copies of the issue in which the article appears; reprints will not be supplied under any circumstances.

The Newsletter appears quarterly, and the mailing list is open to anyone in the primate field expressing an interest. There is no charge for new issues and back issues for the current year. Back volumes will be furnished free of charge to any library operated by a nonprofit organization with the understanding that they will be kept in the library. Individuals may purchase Volumes 1, 2, and 3 for $4.00 per volume, and Volumes 4 and 5 for $2.00 per volume. (Please make checks payable to Brown University.)

Preparation of articles for the Newsletter.--Articles and notes should be submitted in duplicate and all copy should be double spaced. Articles in the reference section should be referred to in the text by author(s) and date of publication, as for example: Smith (1960) or (Smith & Jones, 1962). Names of journals should be spelled out completely in the reference section. Technical names of monkeys should be indicated at least once in each note and article. In general, to avoid inconsistencies within the Newsletter (see Editor's Notes, July, 1966, issue), the scientific names used will be those of Fiedler [In H. Hofer, A. H. Schultz, & D. Starck (Eds.), Primatologia. Vol. 1. Basel, Switzerland: Karger, 1956. Pp. 1-266].

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Acknowledgment

The Newsletter is supported in part by U. S. Public Health Service Grant MH-07136 from the National Institute of Mental Health.

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CHIMPANZEE ECOLOGY AND LABORATORY MANAGEMENT: REPLY TO COMMENTS BY G. H. BOURNE AND BY J. MOOR-JANKOWSKI

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I was gratified that my attempt (Kortlandt, 1966) to provoke a discussion on certain issues regarding laboratory animal management succeeded so well. Before replying to Bourne's (1967) and Moor-Jankowski's (1967) comments on the ecological, technological, and behavioral aspects of the problem, I would like to put right some points Moor-Jankowski made which may obscure an objective discussion.

I. Some Corrections

A. I regret that Moor-Jankowski, in his attempt to refute my views, misquoted and distorted my statements no less than 10 times. The instances were: (1) It was incorrect to assert that I "confused" the requirements of zoos and laboratories (Moor-Jankowski, 1967, p. 4); on the contrary, for example, I criticized solitary confinement of chimpanzees explicitly "if this is not necessary for the research involved" (Kortlandt, 1966, p. 4; italics added). (2) According to Moor-Jankowski, "Kortlandt appears to be unaware of achievements and developments...in the United States in the field of primate maintenance and experimentation" (p. 4); whereas I actually stated, "There is a growing awareness among American laboratory scientists that there is much wrong with the current care and management practices," (p. 2) etc. and referred extensively to the pioneering work on this issue undertaken at the 6571st Aeromedical Research Laboratory, Holloman Air Force Base, N.M. (pp. 2 & 4). I could have added, e.g., the interesting chimpanzee accommodation projects started by Charles B. Ferster (partly described by Hammer, Ferster, & Randolph, 1967) and Robert L. Hummer, both of which aim at getting rid of the current cage confinement system and with which I was familiar when they were still in the design stage. (3) Moor-Jankowski incorrectly supposed that I was "totally unaware" of Yerkes' climatological survey of the United States to find the most appropriate chimpanzee laboratory site (p. 4); whereas actually my criticism was that until 1964 nobody ever went to Africa to obtain a "chimpomorphic" climatological survey at the natural living sites (p. 1; italics added). I could have added that Nissen's figure for the average maximum temperature from February to April in his study area in Guinea (Nissen, 1931, p. 12) was not only an "anthropomorphic" one, but, moreover, incorrect, about 7°C too high. (4) It was incorrect to state that I asserted that "in the United States ...the animals are taken care of by the least-educated class of workers available" (Moor-Jankowski, 1967, p. 5). I only stated that, in the primate laboratories and zoos in the United States which I had visited when I wrote the paper (12 in total, not including the new Yerkes and Delta Regional Primate Research Centers), the keepers "often belonged to the least-educated class..." (Kortlandt, 1966, p. 1; italics added).

(5) It was not I who called American zoo keepers "some kind of cowboy," as Moor-Jankowski claims (p. 5); on the contrary, I stated that, "In the United States...except at the very best zoos, one often has the impression that the keeper is regarded as some kind of cowboy" (p. 2; italics added), in contrast with the high esteem often enjoyed by zoo keepers in Europe, particularly Central Europe. (6) I did not state that, "an animal keeper, rather than the veterinarian, should be assigned the responsibility for the animal care" (Moor-Jankowski, 1967, p. 5), but that, "In Europe (particularly in Germany and Switzerland)...the keeper is a great authority...who is primarily regarded as responsible for the condition of the animals..." (p. 2; italics added), and I added that this policy worked well in the framework of the existing traditions with regard to animal keeping and breeding. (7) It was incorrect to suppose that I was "unaware of the large number of chimpanzees that have been bred in laboratories in the United States and of the new concept of laboratory breeding" (p. 6); on the contrary, my complaint that "we have no standardized data on natality and mortality rates of chimpanzees in captivity..." (pp. 5-6) was meant to criticize the fact that, during my visits to primate research laboratories in the United States, nobody could provide me with demographic statistics in such a form that current methods of population analysis could be applied to them, whereas we do have such data, though in rough form, on wild populations (Kortlandt, 1966, p. 6; also see below). (8) Moor-Jankowski attempted to refute my position that the best conditions for keeping animals in captivity are, generally speaking, those which approximate their ecological wildlife conditions by pointing to the very bad condition in which animals imported by commercial dealers from Africa into the United States are usually delivered (pp. 6 & 7). However, I stated myself that, "the exported animals [from Africa] have been infected with all the contagious and intestinal diseases occurring in all the villages they have passed through," owing to the current extremely primitive capturing and trading practices (p. 8). For a fair comparison, Moor-Jankowski should have referred to the health condition of newly-captured apes (Rahm, 1967). (9) The experiences at the Holloman Air Force Base do not prove that my statement on peaceful co-existence was a "rash assertion" (Moor-Jankowski, 1967, p. 7); on the contrary, they demonstrate rather convincingly the correctness of my statement on the necessity of escape routes, screens, hide-outs, etc., as technical devices to establish peaceful co-existence (Kortlandt, 1966, pp. 4-5; see also van Hooff, 1967). (10) Moor-Jankowski's reference to the figures of the Institut Pasteur in Guinea as an example of a less favorable mortality rate in a "European institution" as compared with well-run laboratories in the United States (p. 7) is, of course, entirely away from the point. First, I explicity stated that most of this mortality occurred within a few weeks after purchase of the individuals involved (i.e. newly-captured chimps) from native African dealers (pp. 7-8); secondly, in my praise of certain concepts in animal keeping I referred explicitly to non-Latin European countries (p. 2); and thirdly, the keepers at the Institut Pasteur consisted entirely of native African personnel without any substantial education or hygienic training. If the figures of the Institut Pasteur prove

anything about management problems, they corroborate at least my position that good keepers sometimes seem to be more important than a good veterinarian and medical staff for obtaining high survival rates. Of course, I admit that no definitive proof is available as long as no standardized vital statistics are available, and that is why I criticized the total absence of such statistics (survival tables, fertility figures, etc.).

B. Moor-Jankowski reproached me for my statement that "particularly in Central Europe, there is a widespread cultural tradition of keeping and breeding all kinds of animals at home, just because many people love animals" (p. 2), claiming that it is "entirely gratuitous and reveals a rather prejudiced attitude" (Moor-Jankowski, 1967, p. 4). He also accused me of having presented "a great deal of misinformation and some rash assertions unsupported by facts" (p. 4) as well as "many examples of gratuitous and meaningless remarks" (p. 5) when I drew comparisons between the United States and (non-Latin) Europe, particularly with regard to the standards of zoos and animal keepers. I admit that my statements on cultural differences were based to some extent upon personal impressions gained in social and scientific contacts when visiting zoos and laboratories, but this does not necessarily prove that they were "gratuitous" and "prejudiced." One does not necessarily need figures to be aware that, for example, among the field sports, bird watching is more popular in Great Britain, and hunting (i.e., animal killing, after all) in the United States. Nor does one need figures to perceive that in a talk with a Central European experienced zoo keeper one may use such technical terms as, for example, "releaser," "imprinting," "displacement activities," etc., whereas this is rarely possible in the United States. Yet there is some hard factual evidence as well; (1) Historically, new trends in animal keeping emerged almost entirely in Europe. The use of "freianlagen" came from Germany. The only existing scientific journal dedicated to zoo problems, Der Zoologische Garten, is a German one and was founded in 1860. The first scientific book on zoo keeping was written in Switzerland by Hediger in 1942. The International Zoo Yearbook is a British publication, started in 1959. Nothing comparable ever emerged from the United States. (2) The "naturalistic" trends in animal psychology, ethology, and sense physiology, characterized by a predominance of interest in animals as such (rather than as so-called research tools, or as hunting objects), and based upon a combination of field work and raising wild animals at home or in the laboratory, were developed almost entirely in West and Central Europe. Scientific wildlife management, on the other hand (i.e. mainly game management for hunting purposes), was developed largely in the United States and Canada. The difference is indicative of dissimilarities in cultural backgrounds. (3) In order to measure national differences in interest and attitude towards animals objectively, we may analyze the data in The International Zoo Yearbook (Jarvis, 1965) by relating them to the figures in the Statistical Yearbook of the United Nations (1965). In Table I, I have done this for the United States and for the entire array of non-Latin (and predominantly non-Latin) West and Central European

countries from Ireland to Poland. For the purpose of the table a "zoo" was defined as an institution accommodating 100 or more animal species, including 30 or more mammal species; and the average admission fee was assumed to be the average of the adult's and children's fees. In the list, the United States ranks among the lowest in almost every respect, and particularly in terms of what the public is prepared to pay to see a good animal collection. It may be added that all Latin European countries have much lower figures, both in terms of zoo density per 1,000,000 inhabitants (e.g., France 0.06, Italy 0.08, Spain 0.06) and in terms of expenditures relative to the national income. On the other hand, there are six countries not mentioned in the list which have a higher zoo density per 1,000,000 inhabitants than the United States, and as a rule quite high figures in other respects as well. Five of them belong to the former British colonial sphere of influence, and the sixth is Israel. Thus the figures fully confirm my statements about the cultural attitudes and interests concerning animals, as found in (and emanating from) non-Latin West and Central Europe. Incidentally, more evidence could be added.

Table I

Survey of Zoos in Non-Latin West and Central Europe and in the United States

Country	Average number of:				Part of nat. income spent for attend-ance fees (0.000001)
	zoos per 1,000,000 inhabit.	specimens per zoo	species per zoo	mammal species per zoo	
Czechoslovakia	0.64	1351	236	67	22
East Germany	0.47	2116	362	96	62
Denmark	0.42	1724	464	109	60
Netherlands	0.41	2616	634	106	89
Ireland	0.35	838	252	67	51
West Germany	0.34	2604	455	93	54
Switzerland	0.34	1855	417	81	42
Great Britain	0.28	1586	395	97	38
Poland	0.22	1392	224	83	15
Austria	0.14	3090	763	98	13
United States	0.25	1303	336	90	9

C. Moor-Jankowski's statement that "the career of a certified animal technician simply is non-existent on the European continent" (p. 5) is incorrect. In the Netherlands, at any rate, 2-year and 3-year evening training courses for laboratory animal keepers and technicians are given by the

universities in Amsterdam, Groningen, Leiden, Nijmegen, and Utrecht. Furthermore, a 3-year evening training course for zoo animal keepers is given by the Amsterdam Zoo. Three of these courses have existed for many years.

D. I could add more evidence to demonstrate the incorrectness of Moor-Jankowski's critique of my paper. However, instead of going on in this way, I would like to state that my comments on the standards of animal care in zoos and laboratories in the United States were based upon impressions gained on trips that I made during the years 1962 through 1963. In 1966, I visited the new Yerkes and Delta Regional Primate Research Centers, and I wish to state here that I am very much impressed by the standards that have been achieved there. Furthermore, I agree with Moor-Jankowski insofar as I also feel that "impressions" are in themselves insufficient evidence. This is why I proposed that the 6571st Aeromedical Research Laboratory, U.S. Air Force, invite Mr. van Hooff, research scientist at the Laboratory of Comparative Physiology and co-director of the Arnhem Zoo, to undertake a more thorough survey of these and related subjects in the three leading chimpanzee laboratories and in some leading zoos in the United States. It is a pleasure for me to quote his conclusion on the personnel issue: "A difference in training between American and European animal keepers has not been found," though, "it is difficult to get a good impression," owing to differences in organization, etc., between institutes (van Hooff, 1967).

II. Ecological Aspects

A. Natality. Both Bourne (1967, p. 9) and Moor-Jankowski (1967, p. 6) refer to the breeding results in laboratories as a reason for satisfaction with the present standards of laboratory chimpanzee management. However, no valid data are available. We only know that the average chimpanzee population at the Yerkes Laboratories and the Yerkes Center since 1930 amounted to 55 individuals (van Hooff, 1967), and that the average birth rate was nearly 5 per year. If we roughly estimate that about two-thirds of the average population were adults, and that one-half of these were females, it would follow that, on the average, nearly 20 adult females were available for reproduction. Taking into account that, at Yerkes, most babies were taken away from the mothers shortly after birth, a natality rate of 15 instead of 5 per annum would seem feasible, at least in principle. (In this case a comparison with wildlife conditions would not be valid because a nursing mother in the wild does not ovulate, or, strictly speaking, shows no genital swelling, until her baby has attained an estimated age of 2 to 3 years.)

Furthermore, as long as we have no figures for the net increase of the stock and for the number of sacrifices, Bourne's statement that the Yerkes colony could have been perpetuated at a birth rate "very much less" than the one quoted, seems difficult to reconcile with the fact that, in 37 years, only about one-half of the chimpanzees that

live, or have lived, at Yerkes have been born there. Thus, for the time being, until we will have standardized figures for the fertility and the net reproduction rate among those females which were used in breeding programs, the crude data available plead against Bourne's and Moor-Jankowski's argument on this issue.

B. Mortality. Contrary to Bourne's conclusion, the fact that chimpanzees at the Yerkes Laboratories have attained ages up to 45 and 46 years, does not prove that the overall mortality rate should be considered as satisfactory. In the wild, such ages must be quite normal. In the populations observed by Goodall (1965), Reynolds and Reynolds (1965), and me[1], the ratio of the number of females over puberty relative to the number of youngsters under puberty was about 1 : 1.10, with little variation between the three samples (see also van Hooff, 1967). Assuming that the sex ratio among youngsters was 1 : 1, it follows that the ratio of the numbers of females over puberty to females under puberty was 1 : 0.55. If we assume that puberty occurs at the end of the 8th year of life, that the maximum age is 50 years, and that the chances of death are the same in all classes up to 49 years old, then demographic equilibrium would require the annual survival rate (r) to be:

$$0.55\ r^{50} - 1.55\ r^{8} + 1 = 0$$

i.e., r = 0.952. It follows that the average age expectancy of those females that do pass puberty must, in this case, amount to:

$$\Sigma[(k + 1/2)\ r^{k}]\ /\ \Sigma\ r^{k} = 22 \text{ years},$$

with summation extending from k = 8 to k = 49 years old. The age class of 40-49 years old would then constitute 9% of the total adult female population.[2] Actually, certain field data suggest that the death risks among youngsters are very much higher than those among adult females. It follows that, in order to maintain the population equilibrium, those females that do pass puberty must attain an average age span substantially longer than 22 years, i.e., somewhere between 25 and 30. This implies at the same time that the lower number of females available for reproduction in the age classes under 22, owing to the non-exponential course of the age pyramid, must be compensated by a proportionally longer survival of those which die later than the average, including at least the age classes high in the thirties, and probably those in the forties. If otherwise, the species would necessarily die out. Thus we may expect that ages of well over 40 probably occur quite commonly in the wild.

This conclusion is supported by the fact that, in captivity, females

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[1] Kortlandt, A. Computation of the longevity and mortality from the age composition of chimpanzees in the wild. (Available on request)

[2] The writer expresses his thanks to the Mathematical Center at Amsterdam for the computations.

up to ages of at least 40 years still show no indications of a reduction in fertility (Riopelle & Rogers, 1965). Furthermore, in the wild, chimpanzees which look very old and which are apparently handicapped by the infirmities of old age, may nevertheless survive for years. The male "Grandad" which I observed in the Congo in 1960, 1963, and 1964, and which, at any rate, looked definitely older than any chimp I have seen at the Yerkes Laboratories and in zoos, i.e., well over 40, avoided unnecessary climbing and physical exertion, had a sagged face and a bent back, and suffered in 1964 from one rather stiff leg (see photographs in Kortlandt, 1962, 1967a). Yet he seemed to be in a well-fed condition and maintained a quite high position in the social hierarchy, perhaps because he acted as the chief security inspector. He took at least a part of his food away from other apes (including from males in the prime of life), and conversely he once gave away some food to a begging lower-ranked adult female. He also participated in reproduction. Thus we may assume that the chimpanzee social system in the wild protects the aged.

As mentioned already, we do not have any usable mortality data on laboratory chimpanzees. There are, however, some data on the zoo stock (van Hooff, 1967). The average number of chimpanzees born in zoos during recent years amounted to about 20 per year. Yet, according to a census carried out in 1962, not more than 47 out of a total of 504 chimpanzees living in zoos had been born in captivity. These figures are distressing. Neglecting the trend factor, they come down to an average life expectancy of less than 2-1/2 years. Primate laboratories certainly do better, and probably even much better, but this will become evident only if the data, including the number sacrificed and the number of accidental deaths, are gathered and published in the form of a life table. For the time being, the evidence available, as well as the general appearance of chimpanzees in the wild, indicates that Moor-Jankowski's statement that chimpanzees "do much better in American laboratories than they do in the wild in Africa" (1967, p. 7) is definitely amiss.

C. Environmental conditions. I cannot understand why Moor-Jankowski (1967, pp. 4-6) contests my basic assumption that the best conditions for maintaining animals in captivity are, as a rule, those which equal or approximate the wildlife conditions. Animals have adapted to their natural habitats and foods in the course of selection through hundreds of thousands of generations. So we may be pretty sure that, conversely, their natural habitats and foods must be almost identical to the optimum ones as a rule. Moreover, this indirect inference from the logic of evolutionary processes is corroborated by the fact that cases in which a wild animal species thrives well at another continent than where it has evolved (like the rabbit in Australia, the grey squirrel in Great Britain, and the house sparrow in North America) have occurred relatively rarely in comparison with the numerous attempts at species introduction and the countless accidental escapes of pets. Moreover, in most cases of really successful settlement, both the climate and the

vegetation in the areas of origin and of settlement were very similar to one another as a rule; except, however, in Australia, which represents a special case (Niethammer, 1963; de Vos, Manville, & van Gelder, 1956). Another argument to the same effect is that very few animal species have proved to be fit for domestication (Angress & Reed, 1962; Boettger, 1958; Herre, 1958).

The same principles apply to behavioral adaptation to environmental conditions. Thus, if chimpanzees in the wild actively avoid exposure to temperatures over 80°F, we have simply to assume that this behavior has evolved because those individuals which did not behave in this way gradually have died out. Similarly, if chimpanzees in the wild eat much roughage, mostly produce firm feces, and do not show coprophagy as a rule (except, however, at one observation site in Guinea; see van Zon & van Orshoven, 1967); whereas chimpanzees in captivity mostly produce soft feces and often show coprophagy, it seems self-evident that a remedy may perhaps be found by giving them a more natural, i.e., more fibrous type of diet. Some positive evidence to this effect has been produced experimentally by Nees, Derse, Rodaidek, & Regel (1965). They also found that on a diet that produces better feces and suppresses coprophagy the apes became less irritable, more manageable, and more cooperative in experimentation. One wonders why it has taken until 1965 for this to be discovered, and why no more extensive research on this issue has been done. Also one wonders why nobody seems to have taken up Reichenow's (1920) interesting suggestion, made a relatively long time ago, that the intestinal diseases that turn up in chimpanzees and gorillas after capture may, to a large extent, be due to the disappearance of Troglodytella from their digestive tract. (This ciliate occurs abundantly in these apes in the wild, but rarely in captivity; and, according to Reichenow, it may exert a cellulose-digesting function, since it belongs to the Ophryoscolecidae, many of which seem to exert such a symbiotic function in ungulate digestive tracts.) Maybe the existence of these rather conspicuous gaps in our existing knowledge is due to the current neglect of the ecological aspects of animal keeping which I criticized in my paper.

This neglect is by no means surprising, after all, since both human and veterinary medicine have developed primarily to cure individuals belonging to genetic strains that have been selected under heavy domestication pressure for thousands of generations. How far the influence of selection under domestication conditions may go becomes obvious if one compares the ecology, physiology, and behavior of ordinary laboratory rats with those of their wild ancestors under comparable laboratory conditions, or those of dogs with those of wolves (see, e.g., Barnett, 1963). I therefore fully maintain my criticism that it is biologically out of place to manage chimpanzees by means of the same principles and standards that have proved to be adequate in many respects for domesticated animal races, including man.

III. Technological Aspects

On these I may be briefer because they have been dealt with extensively by van Hooff (1967). Some points, however, are worthwhile mentioning:

A. Moor-Jankowski reproaches me for not citing "a single item of evidence" to substantiate my critique. He will certainly understand that I thought it would be more fruitful to fill my paper mainly with constructive recommendations for the future, rather than with a long enumeration of faults that have been made in the past. Suffice it here to state that when I visited primate laboratories in the United States, I happened to witness, myself, among other things, some rather hair-raising escape incidents that could have been easily prevented by a reasonably well-thought-out design of the confinement system and the door locks. This example may show that my critique was not entirely without any foundation.

B. I fully agree with Bourne (1967, p. 9) that, from the viewpoint of natural ecology and behavior, there is no need for air-conditioning if the chimpanzees have constant access to indoor quarters where the temperature does not rise above 80°F. Such a situation, however, does not exist at all chimpanzee laboratories in the United States. Incidentally, I apologize for stating incorrectly the number of rooms with air-conditioning at the former Yerkes Laboratories.

C. Both Bourne (1967, p. 8) and Moor-Jankowski (1967, pp. 6-7) objected to the system of free-ranging colonies advocated by me, because caging is necessary to ensure regular access to the animals for experimentation. However, the system applied at the Chimpanzee Consortium at the Holloman Air Force Base, in which the apes have to pass a turnstile device on their way from the outer enclosure to the feeding and dormitory room, and vice versa, allows quick and easy isolation. From the turnstile the trapped animal can be moved into a transport cage without any difficulty. During my visits the system worked quite well and efficiently, except with regard to one chimpanzee which was ostracized by the group. For such cases, a trapping cage baited with food at the other end of the enclosure might help.

IV. Behavioral Aspects

I have some difficulty in understanding Bourne's statement: "We are well aware that the keeping of our chimpanzees under relatively confined conditions produces animals which are psychologically different from those kept either in the wild or in large enclosures, but there is no evidence that this invalidates any of the experimental studies carried out on them so far" (Bourne, 1967, p. 8). The absence of proof that a procedure is invalid, however, does not constitute proof that it is valid. When we use chimpanzees as human substitutes, we are primarily interested in research results applicable to non-confined humans. Re-

sults which have been obtained only under conditions of confinement may not be applicable. We still have very little knowledge of the influences exerted by confinement. The statements in my paper were based upon the fact that the chimpanzees kept in the large enclosure at the Chester Zoo behave virtually normally (measured by the standards of wild-life behavior), whereas chimpanzees kept in cages quite often show such "neurotic" symptoms as dullness, stereotyped behavior, coprophagy, bad temper, and viciousness. The facts were plain enough. Since then, however, it has been found that, when a chimpanzee infant in the wild loses its mother, it may become "neurotic" too (van Lawick-Goodall & Harris, 1967), and furthermore, that sexually aberrant behavior and coprophagy also occur in the wild (van Zon & van Orshoven, 1967). In addition, it has been found that the overall level of aggressiveness among the chimpanzees in the large enclosure at the Holloman Air Force Base is (or, at any rate, was in September, 1966) much higher than it is in the wild (van Hooff, 1967), and, if I may rely upon my memory, also much higher than it is at the Chester Zoo (or was in 1958). For the time being we do not yet know whether the high aggressiveness among the Holloman chimpanzees will eventually decline to a more normal level, or whether it represents a lasting effect of the preceding long-term confinement. We have to wait and see. Nevertheless, though I may perhaps have somewhat overestimated the "neuroticizing" and stress-producing influences caused by confinement per se (as is suggested by the new wildlife observations), it remains sufficiently obvious that, apart from the factors that have been studied extensively by Harlow and by Mason, confinement may exert such a "neuroticizing" and stress-producing influence that it will invalidate at least some current research results. The point is: we have yet to find out which results obtained under confinement conditions are valid for non-confined apes as well, and which are not.

For the rest, I admit to Bourne that my paper was to some extent prompted by my wish that more semi-naturalistic behavior research would be done with the invaluable stock of primates available at American laboratories. I disagree entirely with him that the only appropriate place for naturalistic behavior research is in the wild. For example, it is impossible in the wild to assess to what extent the differences in behavior between the chimpanzee and the orangutan are genetically determined, because the structure and stratification of the rain forests in their respective natural habitats differ widely from one another. Another example is that the ethological method of motivation analysis (with its important psychiatric and psychosomatic implications) requires standardized observation conditions which cannot be achieved in bush and forest habitats. And finally, we should not forget that the founder of contemporary ethology, Oskar Heinroth, was not a field worker, but a deputy director of a zoo who liked to study the behavior of animals kept in naturalistic enclosures. Similarly, Lorenz has never been a field biologist, but a man who liked to have and study tame animals ranging as freely as possible in and around his home. All this may sound rather romantic, but, at any rate, it eventually evolved into a

new branch of science. (See also Kortlandt, 1967b).

V. Final Remark

My critics commented upon only that half of my paper which was dedicated to the problems of laboratory management. I would be grateful if this implies that everybody agrees with the other half, in which I expressed and documented my grave concern about the rapid rate of extermination of chimpanzees, about the extremely primitive and destructive ways of procurement of laboratory specimens, and about the serious threat to the continuation of field work which is caused by the activities of commercial chimpanzee dealers. Since then I have been informed that a dealer in Sierra Leone is trying to find out at which sites in Guinea the members of the Sixth Netherlands Chimpanzee Expedition have worked so successfully recently. I can, therefore, only repeat what I consider, myself, to be the main issue of my paper: "...a scientist who at present buys a chimpanzee from a country where they are not eaten and where field work is possible, unless he is sure that it has been captured by means of a drug or a trap, is like a scientist who pollutes the water of a fishery-research lake," (p. 9); "The medical interests involved in chimpanzee ecology are too great to tolerate the prospect that this branch of research will be undone by the commercial suppliers of mainly medical research laboratories" (p. 11). Whatever controversies may have arisen on the other issues of my paper, I hope that everybody shares my concern on this point, and that in the near future some effective steps will be taken to turn back the present disastrous trends in the chimpanzee trade.

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PRIMATE BIOLOGY PROGRAM INAUGURATED AT THE SMITHSONIAN INSTITUTION

A new program of primate biology has recently been inaugurated at the Smithsonian Institution, Washington, D. C. It will be directed by J. R. Napier who has just been appointed a Curator in the Division of Mammals at the U.S. National Museum. The program is intended to be international in approach, having an office in London [Unit of Primate Biology (Smithsonian Institution), Royal Free Hospital School of Medicine, London] as well as in Washington. The work of the program, which will embrace both education and research, will be shared between the two centers as their national resources and facilities dictate. Particular attention will be given to such aspects of primate biology as systematics, laboratory identification methods, and population dynamics of free ranging primates.

PREMATURE CHIMPANZEE TRIPLETS AT YERKES

Geoffrey H. Bourne

Yerkes Regional Primate Research Center, Emory University

One of the Yerkes chimpanzees, Flora, gave birth to premature triplets on July 5. The details of the parturition and length of the survival are described below.

Parturition

Flora gave birth to premature triplets on July 5, 1967. The first infant (No. 376), a female, was born at 4:25 p.m. and weighed 550 gm. The second (No. 378), also a female, was born at 4:30 p.m. and weighed 520 gm. These infants were removed immediately without struggle from Flora's cage, placed in an Isolette on heating pads in the nursery, mouth and pharynx were suctioned, and oxygen was administered. Both infants showed Cheyne-Stokes respiration and appeared to have died several times. No. 376 died at 8:00 p.m. on July 5. No. 378 received 15 ml. of warmed 10% Dextrose I.P. Postmortem examination revealed almost complete atelectasis of the lungs in both infants. No. 378 also showed considerable subdural hemorrhage. No. 378 died at 4:45 a.m. on July 6. The third infant (No. 289) was stillborn; it was a male and weighed 635 gm. It was found in Flora's cage at approximately 11:30 p.m., on July 5.

The male fetus was attached to a separate placenta. The two females shared a separate common placenta and probably a single chorion. It is therefore probable that the females were uniovular twins. It was estimated that Flora was 5 to 6 months pregnant (the infants were at least 1 month premature).

Background

Flora was born January 28, 1941 at Yerkes Laboratories of Primate Biology, Orange Park, Florida. Her parents were Frank and Alpha. Frank's father, Pan has sired one set of twins. (See Table I for summary of ancestry of triplets.) Her first menstruation occurred on September 17, 1948, when she was 7 years, 7 months, and 20 days of age. She had given birth to nine offspring prior to the birth of the triplets, including two sets of twins (Table II). All were born at Orange Park.

The father of the triplets was Hal who was born October 7, 1935 at Orange Park. His parents were Jack and Josie. He has sired a total of 18 offspring, including two sets of twins (not by Flora) and one set of triplets.

Table I. Triplets' Ancestry

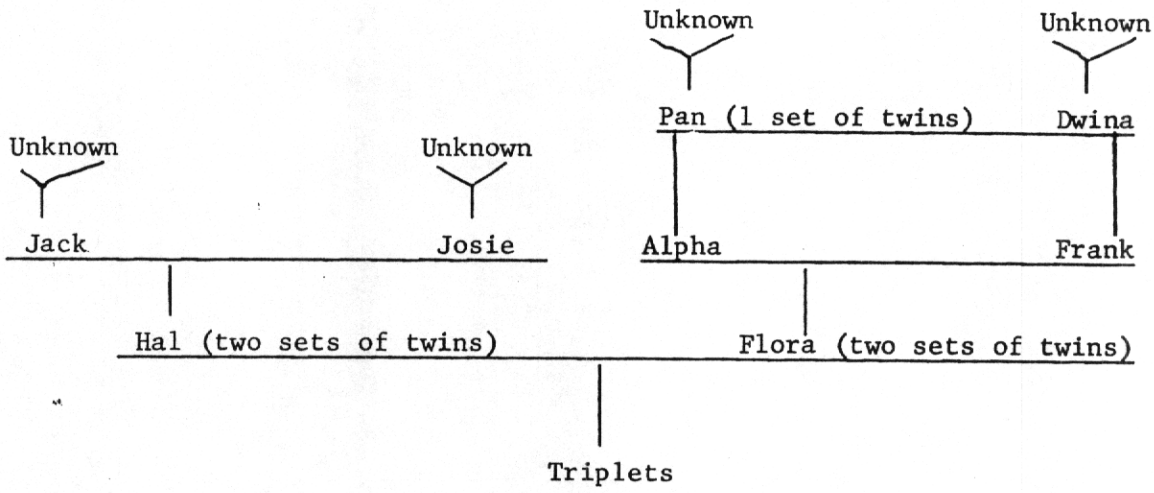

Table II. Flora's Offspring Prior to Triplets

No.	Name	Sex	Date of Birth	Father
149	Pandor	M	5/25/55	Web
161*	Born 9/24/56 Died 9/27/56	M	9/24/56	Jent
174*	Born 9/24/56 Died 9/27/56	F	9/24/56	Jent
190	Reba	F	11/3/58	Web
202**	Mary	F	7/7/60	Jent
204**	Martha	F	7/7/60	Jent
252	Flo	F	8/4/61	Web
308	Hora	F	2/20/63	Hal
271	Stillborn	M	3/12/65	Hal

*First pair of fraternal twins. **Second pair of fraternal twins.

TRAINING OF SQUIRREL MONKEYS TO PERFORM A PULLING-IN TASK

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It has come to our attention that some investigators have experienced difficulty in shaping the instrumental behavior of squirrel monkeys (Saimiri sciureus). We felt, therefore, that it might be helpful to present a detailed description of a procedure which has been successful at the University of Washington for training squirrel monkeys to perform a "pulling-in" task similar to Klüver's (1933). Since the procedure to be described here is specific to the apparatus used, a short description of that apparatus will be given first.

The apparatus consists of two compartments separated by horizontal and vertical bars. The animal to be tested is in one compartment. The bars allow the animal to reach into, but not to enter, the other compartment. This compartment contains two small carts on wheels, to which are attached string and small rings. The latter make it easier for the monkeys to pull the carts. There is a foodwell in each cart. When testing the animals, these foodwells are usually covered by blocks.

The animals are placed on a 23-hour food deprivation schedule prior to the beginning of shaping and are maintained throughout training at approximately 85% of their ad lib body weight. Food reward in most cases consists of one-half a banana pellet. Occasionally banana pellets appear to make an animal ill or else it refuses to work for them. In these cases, small pieces of their normal laboratory chow are substituted as rewards. Peanuts or dried currants are also effective.

Before the beginning of each test session the animal compartments are checked for food pellets and other extraneous objects. The area should be clean and contain nothing which might distract the animal.

For the typical two-choice learning problem, the foodwell of one of the two carts is baited with food and the foodwell of each cart is covered with a block. The strings attached to the carts are placed within reach of the animal. In order to obtain the food, the animal must reach for and pull in the cart containing the food, displace the block, and pick up the food pellet. To shape the necessary sequence of responses requires approximately 5 to 7 daily test sessions with 25 to 30 trials per session. The following are the steps used for this shaping:

Step 1: With the carts immediately in front of the bars separating the two compartments, the animal is placed in the apparatus. The experimenter then places a food pellet on one of the carts, in full view of the animal. If the animal does not pick up the food pellet immediately

and eat it, the experimenter picks up the pellet and offers it to the animal. Sometimes an animal still does not take the food. In such cases, if the animal seems to be especially fearful, it may help if the investigator departs for 10 minutes or so, permitting the animal to remain alone.

Once the animal begins taking the food regularly, the experimenter resumes placing the food on the carts. One of the two carts is baited with food on each trial, with each cart used equally often over a series of trials. The experimenter usually begins by placing a food pellet on top of the cart (again, while the animal is watching). In the beginning, it is often necessary to draw the animal's attention to the food pellet by pointing to it. If the animal still fails to respond, shaking or tipping the cart may draw its attention to the pellet. If the animal will not take the food from the carts, then the carts are removed and the food placed on the floor in front of the animal. Once the animal is responding consistently again to the food, placement of the food on the carts is tried again.

In general, by the end of the first session, the animal should be readily picking up food pellets in the apparatus, and, by the end of the second session, is should be consistently taking pellets from both carts.

Step 2: For this step the experimenter begins to gradually move the carts away from the bars, requiring the animal to reach further for the food pellet. If the animal does not respond immediately, the carts are pushed forward again. The objective of this procedure is to reinforce a reaching response which can subsequently be transferred from reaching for the cart to reaching for the rope to pull the cart. By the end of the third session, the animal should use its full reach to obtain the food.

Step 3: The objective of this step is to get the animal to use the rope to pull in the cart with the food. Begin with the carts just within reach of the animal. If the animal responds readily, the carts are then very gradually moved out of his reach. If the animal does not use the rope, the wagon is pushed back within reach of the animal. It does no good to point to or hand the rope to the animal. The best procedure appears to be to wait until the animal accidentally grabs the rope. However, it is important that the rope be in full view and readily accessible to the animal. It normally takes the animal about two sessions to complete this step.

Step 4: The objective of this step is to train the animal to retrieve the food when the cart is located the maximum distance away. When the animal consistently pulls the rope to get the cart, gradually move the cart farther away. By the end of the fifth session the animal should be readily responding to the baited cart when it is placed as far back in the apparatus as possible.

Step 5: The main objective of this step is to train the animal to choose the wagon containing the food after it has been placed in the foodwell and covered with a block. This is accomplished by gradually covering the food pellet with the block on successive trials. During these trials the foodwell is always baited within sight of the animal. The experimenter may have to put something in front of the bars to prevent the animal from responding before the pellets have been properly placed. In doing this, it is especially important that only the animal's reach is blocked and not its view. Moreover, any of the experimenter's movements involved in this, or any other, procedure must be done carefully so as to avoid disturbing the animal. Should the animal develop a position habit, i.e., reaching only for the cart on one side, the food pellets are placed only on the cart on the opposite side for a series of trials. The rope attached to the unbaited cart is placed out of reach of the animal during these trials.

The procedure used at the end of the previous day's session is usually used on the first few trials of each new session. The average length of a daily session is between 15 and 20 minutes. By the end of about seven sessions the animal should be ready to start on a delayed response test or discrimination problem. For example, a delayed response problem may be set up by introducing an opaque screen between the compartments after the animal observes the placement of the food pellet on one of the carts, but before he is allowed to make the responses necessary to get the reward. Before this problem is introduced the monkey must be accustomed to the use of the opaque screen.

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A NEW ORGANIZATION: THE PRIMATE SOCIETY

A new Society dedicated to the advancement of research in primate biology was inaugurated at a meeting held at the Zoological Society of London on April 22, 1967.

The officers of the Society were elected as follows: President, J. R. Napier; Hon. Sec., Richard P. Michael; Treasurer, Richard Andrew.

Anyone interested in becoming a member is invited to communicate with the Honorary Secretary, Richard P. Michael, Institute of Psychiatry, 3, de Crespigny Park, Denmark Hill, London, S.E.5, U.K.

REQUEST FOR PRIMATE MATERIAL: WHOLE LUNGS

In a continuing investigation of the lung mite, Pneumonyssus simicola, of the rhesus monkey (Macaca mulatta) the need for specimen material has become critical. This laboratory is therefore eager to obtain whole lungs from these monkeys. If any specimens are available, this laboratory would pay expenses for packing and shipping, as well as procurement costs. The lungs should be fresh frozen and shipped on dry ice.

Any help given in obtaining material for this project would be greatly appreciated. Inquiries regarding this matter should be addressed to: Mr. James C. Drake, Division of Pre-clinical Pharmacology, Room 214, Microbiological Associates, Inc., 5221 River Road, Bethesda, Maryland 20016; or telephone collect (301) 654-3400, Ext. 375.

REQUEST FOR PRIMATE MATERIAL: REPRODUCTIVE ORGANS

Reproductive organs of adults and fetuses of any species of non-human primates are requested for research on reproductive biology. All gifts of material and institutions will be acknowledged, and reprints of any publications arising from this study will be sent without request to all contributors. Cost of postage and packing will be refunded if requested.

Investigators who can supply this material from animals of either known or unknown age are requested to use the following procedures: 1. Fix reproductive organs in 10% formalin in an individual container. 2. Supply the following data: (a) dates of birth and death, if available, (b) sex, (c) circumstances of death (e.g., killed/healthy; died/emaciated), (d) weight at death, if known. 3. Mail the material within 3 months of fixation. Indicate sender's name and return address.--E. S. E. Hafez, Reproduction Laboratory, Washington State University, Pullman, Washington 99163.

RABIES VACCINATION OF NONHUMAN PRIMATES*

Dr. Monroe A. Holmes, public health veterinarian, Oregon State Board of Health, recently reported that the veterinarian in charge of the animal colony of the Oregon Regional Primate Center receives from time to time shipments of nonhuman primates from various sources, many out-of-state. The animals are received with interstate health certificates which indicate vaccination with "dog rabies vaccine" of the live virus type, presumably the low egg passage (LEP) modified live virus chick embryo origin rabies vaccine. Dr. Holmes suggested that action be taken to remind veterinarians that in such cases live virus vaccine should not be used.

Editor's Note*: Dr. Holmes has presented a problem that may confront veterinarians in other states. Most states require that health certificates accompany shipments of imported animals. In most cases, it is doubtful whether rabies vaccine is needed in nonhuman primates; when it is, the LEP modified live virus chick embryo origin rabies vaccine should not be used. We recommend that only a licensed, inactivated rabies virus vaccine be used in nonhuman primates; these vaccines are also recommended for other wildlife species.

* * *

TOOL-USING BEHAVIOR OF GREAT APES BEING STUDIED

A program of research investigating implementation or tool-using behavior in gorillas, chimpanzees, and orangutans is underway at the San Diego Zoo. Measures of initial responsiveness to visual and tactual stimuli have been obtained on three members of each species. During one condition in which responsiveness was measured, observations of the kinds of hand-movements employed in manipulating an object were made. The types of implementation problems currently being investigated are: dipping a tool into a hole, hoeing or raking, and striking. Food incentives are used in each of these problems. Other, more difficult, types of problems in which the tool does not directly contact the goal object have been planned. None of the data have yet been published. Inquiries or comments are cordially invited.--Christopher E. Parker, Psychology Department, San Diego State College, San Diego, California 92115.

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*From CDC Veterinary Public Health Notes, July, 1967, prepared by the Veterinary Public Health Section of Epidemiology Program of the National Communicable Disease Center, Atlanta, Georgia.

RECENT BOOKS AND ARTICLES*
(Addresses are those of first authors)

Books

The subhuman primate: A guide for the veterinarian. Whitney, R. A., Johnson, D. J., & Cole, W. C. Edgewood Arsenal, Maryland: Research Laboratories, Dept. of the Army, Edgewood Arsenal, 1967. (Available from the Clearing House for Federal Scientific and Technical Information, U.S. Department of Commerce, Springfield, Virginia 22151. Microfilms of the booklet are also available for $.65 a copy from the Clearing House.)

An introduction to the essentials of primate care and primate medicine. This booklet is designed to provide quickly a broad familiarity with the primate and the problems of maintenance of primates in research facilities. Chapter titles are: Procurement of primates; Housing and care; Preventative medicine and public health; Restraint, anesthesia, specimen collection, and drug administration; Nutrition and vitamin deficiencies; Generalized infectious diseases; Localized infectious diseases and parasites; Physiological data; Classification of primates; Regional primate centers; Suggested library.

Zoonoses of primates. Fiennes, R. N. London: Weidenfeld & Nicolson, 1967, or Ithaca, N.Y.: Cornell Univ. Press, 1967.

Primate ethology. Morris, D. (Ed.). London: Weidenfeld & Nicolson, 1967.

Primate behavior as studied by scientists trained in the tradition of the Lorenz-Tinbergen school of comparative ethology. The chapter titles are: Introduction: the study of primate behaviour; The facial displays of the catarrhine monkeys and apes; Socio-sexual signals and their intra-specific imitation among primates; Allogrooming in primates: a review; Play behaviour in higher primates: a review; Variability in the social organization of primates; Comparative aspects of communication in New World primates; The effect of social companions on mother-infant relations in rhesus monkeys; Mother-offspring relationships in free-ranging chimpanzees; and An ethological study of some aspects of social behaviour of children in nursery school.

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*References in this section without summaries have in many cases been taken directly from the Current Primate References prepared by The Primate Information Center, Regional Primate Research Center, University of Washington.

Disease

Vitamins D_2 and D_3 in New World primates: influence on calcium absorption. Hunt, R. D., Garcia, F. G., Hegsted, D. M., & Kaplinsky, N. (New England Reg. Primate Res. Center, Southboro, Mass.) Science, 1967, 157, 943-945.

It was demonstrated that, in Cebus albifrons monkeys, vitamin D_3 promotes the intestinal absorption of calcium-47 and that vitamin D_2 does not increase absorption above that seen in monkeys deficient in vitamin D. These data support previous observations that vitamin D_2 is not effective in preventing metabolic bone disease in this species.

Multiple helminth infections in cinnamon-ringtail monkeys (Cebus albifrons). Garner, E., Hemrick, F., & Rudiger, H. (Dept. Pharmacology, Res. Div., Hoffman-La Roche, Inc., Nutley, N. J. 07110) Laboratory Animal Care, 1967, 17, 310-315.

A group of 16 adult cinnamon-ringtail monkeys (Cebus albifrons) were necropsied after a period of 12 weeks. Fourteen monkeys of this group were noted to be infected with from 2-4 different species of helminths. The various species were identified as follows: Dipetalonema caudispina (a filarial worm), Prosthenorchis elegans (a thorny-headed worm), Athesmia foxi (a liver fluke), and Oochoristica megastoma (a tapeworm). The pathological changes caused by two of these parasites are discussed.

Comparative pathological analysis of the incidence and nature of tumors in different mammals. Yakovleva, L. A. (Inst. Exptl. Pathol. & Therapy, Acad. Med. Sci., U.S.S.R., Sukhumi, U.S.S.R.) Vestn. Akad. med. Nauk, 1966, 21, 65-74.

Physiology and Behavior

Das sogenannte urinmarkieren von totenkopfaffen (Saimiri sciureus) in abhängigkeit von umweltbedingten und emotionalen faktoren. Castell, Von R., & Maurus, M. (Max-Planck-Inst. für Psychiatrie, Abteilung für Verhaltensforschung, München, Germany) Folia Primatologica, 1967, 6, 170-176.

The urine washing of squirrel monkeys is described and discussed. (1) Urine washing was performed by male and female monkeys equally with considerable differences among individuals. (2) During the highly aggressive phase at the beginning of group fusion, the losing group performed urine washing half as often as the winning group. (3) When the cage floor was wet, the frequency of urine washing was about one-tenth of the frequency when the cage floor was dry. (4) The presence of sand on the cage floor and the sittingbars increased the frequency of urine washing 4 to 17 times. Emotional factors influenced the frequency of

urine washing much less than did changes in physical factors. Urine washing is evidently very important for cleansing and moistening of the palms and soles of squirrel monkeys.

A list of chromosome numbers in primates. Borogadnkar, D. S. (Johns Hopkins Sch. Med., Baltimore, Maryland) Journal of Heredity, 1966, 57, 60-64.

Urinary estrogen excretion patterns in pregnant rhesus monkeys. Hopper, B. R. (Biol. Lab., Nat. Inst. Child Health, Nat. Inst. of Health, Bethesda, Md. 20014) Steroids, 1967, 9, 517-527.

Urinary estrogen excretion patterns were determined by means of gas chromatography during early, middle, and late pregnancy in the rhesus monkey (Macaca mulatta). Estrone, estradiol-17β, and estriol levels increased from early to late pregnancy. Average increases were: estrone, 2.6 fold; estradiol-17β, 4.7 fold; and estriol, 4.5 fold.

The mycoflora of the subhuman primates. II. The flora of the rectum and vagina of the baboon in captivity. Al-Doory, Y., Kalter, S. S., & Frederickson, M. (Southwest Found. Res. & Educ., San Antonio, Texas) Mycopathologia et Mycologia Applicata, 1967, 31, 332-336.

For documentation of the mycoflora of the baboon, 127 vaginal and 165 rectal swabs were taken from males and females in captivity. A total of 176 and 171 yeast isolates were obtained from the vagina and rectum respectively. Candida was found to be the yeast most frequently found in both of these sites. C. albicans formed 14.6% of the rectal yeasts and 7.9% of the vaginal yeasts. No significant differences were found between the rectal isolates from the males and those from the females in the baboons. Furthermore, there is a strong indication that the mycoflora of both the rectum and the vagina of the baboon is similar to that of human beings, from both a qualitative and a quantitative point of view.

Facilities, Care, and Breeding

Preservation of primate spermatozoa by freezing. Roussel, J. D., & Austin, C. R. (Delta Reg. Primate Res. Center, Tulane U., Covington, La.) Journal of Reproduction and Fertility, 1967, 13, 333-335.

The effect of freezing and storage of primate spermatozoa in liquid nitrogenvapor for 3 days was assessed. Semen was collected from the following species: Macaca mulatta (rhesus), M. speciosa (stumptail), Erythrocebus patas (patas), Cercopithecus aethiops (African green), and Pan troglodytes

(chimpanzee). There were no significant differences among the species investigated in the proportions of spermatozoa surviving after freezing, and the survival rate obtained was similar to that reported for human spermatozoa.

Semen collection in the squirrel monkey. Bennett, J. P. (Biol. Res. Dept., Brit. Drug Houses Ltd., Godalming, Surrey, England) Journal of Reproduction and Fertility, 1967, 13, 353-355.

A rectal probe to stimulate ejaculation of semen in Saimiri sciureus is described.

Artificial insemination of the squirrel monkey. Bennett, J. P. (Biol. Res. Dept., Brit. Drug Houses Ltd., Godalming, Surrey, England) Journal of Endocrinology, 1967, 37, 473-474.

The induction of ovulation in the squirrel monkey (Saimiri sciureus) with pregnant mares serum (PMS) and human chorionic gonadotrophin (HCG). Bennett, J. P. (Biol. Res. Dept., Brit. Drug Houses Ltd., Godalming, Surrey, England) Journal of Reproduction and Fertility, 1967, 13, 357-359.

Reproduction in green monkeys (Cercopithecus aethiops pygerythrus). Chernyshov, V. I. (Inst. Poliomyelitis & Virus Encephalitis, A. M. N., Moscow, USSR) Vop. Antrop., 1966, 23, 144.

Between 1963 and 1965, 2457 green monkeys (1671 females and 786 males) from Kenya were studied. They were housed in a facility in the outskirts of Moscow. The age composition of these imported monkeys provides some idea of the age composition of the natural population, and study of the genitals provides evidence on reproduction in vervets under natural conditions. In the majority of cases the females with newborns were in shipments that arrived between November and May. Pregnant females arrived primarily between May and January. This suggests that the maximum number of successful matings in vervets occurs between April and June. All this makes doubtful the concept which is widely spread in primatological literature that vervets multiply throughout the year in their natural habitat. However, green monkeys kept in the laboratory can reproduce during every season of the year, apparently as a result of the specific influence of laboratory conditions on the reproductive cycle.

The care and management of captive chimpanzees with special emphasis on the ecological aspects. van Hoof, J.A.R.A.M. (Laboratorium voor Vergelijkende Fysiologie, Utrecht) Technical Report No. ARL-TR-67-15, 6571st Aeromedical Research Laboratory, Holloman Air Force Base, New Mexico, 1967.

There are indications that present methods for the

care and management of captive chimpanzees are not fully adequate, particularly from an ecological point of view, resulting in a physiological and, especially, a psychological condition of the animals which is below optimal. In contrast to what is reported from the wild, many captive animals become very aggressive and unpredictable as they get older. The fact that the present reproduction rate in captivity appears much too low to make a continuous importation from the wild unnecessary is also regarded as significant in connection with the conditions of captive chimpanzees. These points and their causes as well as possibilities for improvement are discussed in this report.

Instruments and Techniques

Electrodes for recording cortical electroencephalograms from monkeys. Robinson, F. R., & Gisler, D. B. (Aerospace Med. Res. Lab., Wright-Patterson AFB, Ohio 45433) Laboratory Animal Care, 1967, 17, 316-320.

Three types of cortical electroencephalogram electrodes used in monkeys which received high levels of vibration are described and evaluated. The type C electrode which was implanted external to the inner bony plate of the calvarium was preferred because the electrodes remained firmly in place throughout the experiment, the integrity of the inner calvarial surface was not interrupted, satisfactory records were obtained, and the esthetic properties were favorable since the subcutaneous electrodes were barely discernible.

Periodicals Specializing in Primate Research

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Folia Primatologica, 1967, 6 [3-4], 161-316: Shriver, J. E., & Noback, C. R., Color vision in the tree shrew (Tupaia glis); Castell, R., & Maurus, M., Das sogenannte urinmarkieren

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REMINDER ABOUT DEADLINES FOR THE 2ND INTERNATIONAL CONGRESS OF PRIMATOLOGY

January 1, 1968, is the deadline for abstracts of papers for the 2nd International Congress of Primatology to be held at Yerkes Regional Primate Research Center, Atlanta, Georgia, June 30-July 3, 1968. Papers are accepted on virtually any subject related to primatology and although some preference will be given to papers on Great Apes, papers on other primates will be welcome.

Deadline for advance registration ($20) is March 31, 1968. Registration fee after that date will be $25.

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