(*D. australis*) that are in the British Museum (Natural History) were examined to see if they could provide evidence of louse infestation, but there was none. The presence of lice could have helped to solve the problem of the origins of this canid if they had been identifiable either to the species commonly found on *Dusicyon* species or to those found on *Canis* species.

Discussion

Domestication may be defined as the exploitation of one group of social animals by another more dominant group which maintains complete mastery over its breeding, organization of territory, and food supply. Only certain groups of mammals can flourish under such drastic alterations to their way of life. On the other hand, all mammals can be tamed if reared close to man from an early age, and I believe that individuals from most species that lived in the same environment as early man were tamed, at one time or another, and were kept in captivity for short periods. Domestication will only follow, however, if the social behavioral patterns of the tamed animals are sufficiently well developed to allow successive generations to breed in captivity, isolated from the wild species. Obviously the most highly social animals are the most easily domesticated and this is why the South American Indians preferred the European dog (descended from the wolf, *Canis lupus*) to their more recalcitrant but tamed dusicyons.

Although Lorenz's original view, that domestication of the jackal and wolf led to two separate strains of dog, has been disputed and is no longer believed even by its author, it was Lorenz who was one of the first to draw attention to the now seemingly obvious connections between the social behavior patterns of animals and man. Twenty years ago discussions on these topics were in disrepute and those who indulged in them were accused of being anthropomorphic. Now it is quite the other way about, so that in discussions on animal behavior it is permissible to include early man as a primate with highly developed hunting behavioral patterns and the ability to coerce other animals to live with him. Like Lorenz in his foreward to *The Wild Canids* (*12*) I used to believe, having also read Jack London's *White Fang*, that, "a tame purebred timber wolf would be the highest ideal of a dog to which man could aspire," but I must agree that this is, indeed, an error. We cannot go backward and in terms of social evolution it is the "humanized dog" that is the pinnacle of domestication. The Pekinese lap dog epitomizes this association no less than the huntsman's tireless hound.

References

1. M. Degerbøl, *Proc. Prehist. Soc.* **27**, 35 (1961).
2. D. M. A. Bate, *The Stone Age of Mount Carmel* (Clarendon Press, Oxford, 1937), vol. 1, part 2, pp. 175–177.
3. B. Lawrence, *Z. Säugetierkd.* **32**, 1 and 44 (1967); P. F. Turnbull and C. A. Reed, *Fieldiana, Anthropol.* **63** (No. 3), 81 (1974).
4. S. Washburn and C. S. Lancaster, in *Man the Hunter*, R. B. Lee and I. De Vore, Eds. (Aldine, Chicago, 1968), pp. 293–303.
5. E. M. Banks, Ed., *Am. Zool.* **7** (No. 2), 221 (1967).
6. J. Clutton-Brock, G. B. Corbet, M. Hills, *Bull. Br. Mus. (Nat. Hist.) Zool.* **29** (No. 3), 119 (1976).
7. A. B. Chiarelli, in *The Wild Canids*, M. W. Fox, Ed. (Van Nostrand Reinhold, New York, 1975), pp. 40–53.
8. C. H. Smith, *Dogs I* in *Naturalists Library*, W. Jardine, Ed. (Lizars, Edinburgh, 1839), vol. 18.
9. C. Darwin, *Journal of Researchers into the Natural History and Geology of the Countries Visited During the Voyage of H.M.S. 'Beagle' Round the World* (John Murray, London, 1860), p. 194.
10. A. Langguth, in *The Wild Canids*, M. W. Fox, Ed. (Van Nostrand Reinhold, New York, 1975), p. 193.
11. G. H. E. Hopkins, *Proc. Zool. Soc. London* **119**, 536 (1949).
12. K. Lorenz, in *The Wild Canids*, M. W. Fox, Ed. (Van Nostrand, Reinhold, New York, 1975), p. x.

NEWS AND COMMENT

Recombinant DNA: NIH Rules Broken in Insulin Gene Project

A breach of National Institutes of Health rules on gene splicing occurred earlier this year in the Department of Biochemistry and Biophysics at the University of California, San Francisco, one of the leading centers for practice of the new technique. No hazard resulted, but the episode underlines some of the difficulties experienced by research laboratories in adapting to the new rules.

The breach was the use of a biological component, or "vector," before it had been certified by the NIH director. The researchers, a team engaged in isolating the rat gene which codes for insulin, say they destroyed the experiment as soon as they realized their mistake.

The experiment was repeated in a certified vector and published in *Science* on 17 June. It received considerable attention because the researchers had achieved, much earlier than expected, the first step toward the goal of isolating the human insulin gene and using it for the manufacture of insulin protein. The UCSF team was in competition with a group at Harvard which was known to be working with a better source material.

UCSF's preeminence in the gene-splicer's art has brought it some mixed blessings. Because of the practical implications of what its researchers are doing, a company called Genentech has established a relationship with Herbert Boyer, one of the pioneers of the technique. Members of the insulin team have set up a nonprofit corporation, the California Institute for Genetic Research. These commercial developments are a tribute to the department's success, but have also created internal stresses. "Capitalism sticking its nose into the lab has tainted interpersonal relations—there are a number of people who feel rather strongly that there should be no commercialization of human insulin," says UCSF microbiologist David Martin.

Another mixed blessing is fame, which has attracted press attention not only to the department's achievements but also to certain internal tensions. A lengthy and circumstantial article in the June issue of the *Smithsonian* called into question the respect accorded to the NIH safety rules by UCSF researchers, and in particular by the younger, postdoctoral workers who perform most of the experiments. Written after a 3-month internship in Boyer's lab by Janet L. Hopson, formerly a reporter for *Science News*, the article observed that "half of the researchers here follow the guidelines fastidiously; others seem to care little. . . . Among the young graduate students and postdoctorates it seemed almost chic not to know the NIH rules," Hopson noted. In a letter to the editor criticizing the article, Boyer stated that "In practice [the NIH rules] are followed seriously."

The stresses of both commercial success and media attention came together this May when the insulin team announced their production of the rat gene.

Other researchers resented not only the intrusive presence of the press but also the fact that they were hearing of their colleagues' success for the first time. The team had worked in unusual secrecy, which many regarded as inappropriate in an academic setting as well as disruptive. "People would stop talking when you came into the room, or change the subject if you tried to make conversation about how the insulin project was going," says UCSF biochemist Brian McCarthy. The insulin team say that no secrecy was intended, and that it was the speed of obtaining results that occasioned surprise.

The secrecy and suspicion surrounding the insulin gene experiment, together with perhaps a touch of resentment, helped to fan rumors within the department alleging that the NIH rules had been broken and even that the experiment published in *Science* might not have been performed as described. The focus of the rumors was the obvious fact that the whole intricate experiment had been completed only 3 weeks after the NIH had certified the vector which the researchers used. Even experienced gene splicers were surprised by the rapidity of execution. "It is conceivably possible to do such an experiment in three weeks if everything works perfectly the first time, but you know as well as I do that science never works as well as you hope," a member of the department remarks.

"Well, what can I say? It did work well. We were all set to go," says a member of the insulin team queried on the speed of the experiment. Yet members of the team concede that an earlier experiment took place, but say it was aborted half way through and before any pertinent information had been gained.

The episode of the earlier experiment illustrates the problems experienced by research laboratories both in acclimatizing to the NIH's gene-splicing rules and in devising ways of enforcing them. "We can't run a policing service," says David Martin, until recently the chairman of the UCSF biosafety committee. "What we do is to try to raise the consciousness of the individuals involved to make them respect the guidelines." Martin spent 2 days investigating the episode and concluded that a breakdown in communications was responsible. Precise details remain obscure because the minutes of the biosafety committee record only Martin's conclusion, and a lengthier account prepared for the minutes by the present chairman, James Cleaver, is far from complete.

The episode revolves about the use of the vectors—virus-like entities known as plasmids—which are used to carry genes of experimental interest into bacterial hosts. Two particularly useful plasmid vectors, known as pBR322 and pMB9, had been developed by Boyer and others. But before either could be used, it had to go through a two-stage administrative process, the first of being approved by the NIH recombinant DNA committee and the second of being certified by the NIH director. Vectors, in other words, are off limits to researchers until certified.

Plasmid pBR322 was tentatively approved by the NIH committee on 15 January, finally approved on 23 June, and certified for use on 7 July. Plasmid pMB9 was certified on 18 April.

According to both Martin and two members of the insulin team, the episode of the earlier experiment was as follows. Early this year, sometime in February, an attempt was made to produce copies of the rat insulin gene with pBR322 as the vector. The gene was linked to pBR322 and the plasmids inserted into NIH-certified bacteria designated EK2 hosts. Some of the bacteria were successfully colonized by the plasmids. To verify that the clones of colonized bacteria contained the rat insulin gene it would have been necessary to carry the experiment to completion by extracting and analyzing the DNA sequence of the genes. This step was not performed because at that moment, on or around 1 March, the team say they learned that the plasmid had not been certified for use. They decided to destroy all their clones. Further attempts were made to clone the gene with an already certified vector, known as pCR1, but without success. When pMB9 was certified, team members say, they had all their materials ready to go, and repeated the experiment from scratch in the new plasmid. Martin says that he inspected the team's records and has "complete confidence" in their statement that the entire experiment was done after the approval of pMB9. "When the manuscript appeared so soon, people said 'How in hell can you do it?' But you can do it—the experiment was very straightforward," Martin said from Great Milton near Oxford, England, where he is at present on sabbatical. "I think they realized they would be questioned and that if there were any shenanigans they would be really jeopardizing a hell of a lot, not only their own careers but the whole advance of science through recombinant DNA technology."

In the categories of the NIH rules, the experiment with pBR322 was assigned to the highest available containment level short of going off campus to a specially secure laboratory. But it is clear that the experiment posed no issue of public health since it was performed in the required type of laboratory—a "P3" facility—and with a vector which has now been certified as safe.

The experiment also took place at a time when the NIH rules were still new and local procedures for implementing them were still in a formative stage. "This was one of the inevitable things that happened as we tried to evolve a new system," comments biosafety committee chairman Cleaver.

Not wholly plain from the Martin-Cleaver accounts of the episode is how the insulin team came to believe it was all right to go ahead with pBR322 prior to certification. Howard Goodman, chief of the laboratory which did the cloning and sequencing part of the experiment, is out of the country, as he has been for most of the past year. One of his postdoctoral colleagues says there was great confusion at the time about the status of the plasmid but the reason for the confusion is "sort of cloudy now."

According to Martin, it was clear that everyone knew that pBR322 had been approved but not certified, but the NIH, Martin says, was advising researchers that certification was imminent and that they should go ahead. According to the minutes of the 20 May meeting of the UCSF biosafety committee, Martin reported that the researchers "had been verbally informed that the certification of an approved EK2 vector was imminent and to proceed with its use."

This version is strenuously denied by William Gartland, director of the NIH Office of Recombinant DNA Activities, and by his only assistant, Daphne Kamely. Both say that they would never have advised use of any vector before certification. Gartland notes that the team "must have got the vectors from Boyer, who certainly knew they were not certified" because Boyer had complained repeatedly to the NIH of the delay in certification. According to Boyer, the insulin team "kept on asking" if the plasmid had been certified and he told them it had not. Boyer states that he never encouraged anyone to go ahead prior to certification. Thus the source of encouragement for the team to go ahead prior to certification remains obscure.

A different account from Martin's is given by William Rutter, a member of the insulin team and chairman of the UCSF Department of Biochemistry and Biophysics. In its memorandum filed with the UCSF biosafety committee, the

Schism Among Psychic-Watchers

An internal dispute within the Committee for the Scientific Investigation of Claims of the Paranormal has prompted the resignation of one of its two cochairmen, Marcello Truzzi, professor of sociology at Eastern Michigan University.

Truzzi has also resigned as editor of the *Zetetic*,* the journal launched by the committee at a press conference held in New York this July.

The focus of the dispute was whether the *Zetetic* should continue to be an academic and analytic journal or whether it should become more popular. Truzzi did not wish to edit a popular magazine and resigned after the executive body of the committee decided by a 7-to-1 vote to broaden the *Zetetic*'s appeal.

But the fate of the *Zetetic* was not the only point of disagreement on the variegated committee. Its other cochairman, State University of New York philosophy professor and editor of the *Humanist* Paul Kurtz, considers that belief in the paranormal constitutes a threat to science, whereas Truzzi sees it as a threat not to science but to conventional religion. Kurtz has taken an activist role in combating paranormal beliefs. A recent issue of the *Humanist* carries the front-cover headline, "The Psychics—Debunked!" Two years ago Kurtz mounted a campaign to persuade newspaper editors to drop their astrology columns.

There is thus a spectrum of opinion on the committee between those who tend to favor a harder-line, debunking treatment of the paranormal and those who tend toward a skeptical but open-minded assessment of paranormal claims. The "debunkers" wish to deploy the full power of the scientific method against paranormal beliefs; the "skeptics" consider that such prejudgment of paranormal claims is as unscientific as some of the claims may be themselves.

These methodological differences emerged after the July press conference, at which committee members urged the media to provide "consistent and balanced presentation of the critical and scientific viewpoint" of paranormal phenomena. At a meeting held after the reporters had departed, Truzzi resigned as both editor and cochairman.

Influence of Magicians

Although the committee describes itself as a "scientific group," magicians are strongly represented on its executive body and natural scientists are not. According to member Ray Hyman, a University of Oregon psychologist who earned his way through college by practicing magic, three other members have strong part-time interests in magic and one is a professional magician. "People with a background in magic know they can fool one another and so they take it for granted that a good psychic can always fool a scientist," Hyman observes. The magicians on the committee "tend to see this as a crusade for people's minds, in which we should fight fire with fire, and not get too subtle or scholarly or we will lose by default. I believe we would be more effective by being more scholarly and building up our credibility," Hyman says.

Cochairman Kurtz confirms that the *Zetetic* is to be changed from a scholarly to a more popular magazine. "We want to have a wider impact," he says. The target audience is "scientists, the educated public and members of the media."

The new editor of the *Zetetic* is Kendrick Frazier, who recently resigned as editor of the Washington-based *Science News* to join his family in Arizona. Frazier, who is happy both with Truzzi's approach to studying paranormal claims and with Kurtz's desire for a more popular magazine, says that he has been promised editorial autonomy. "I hope the *Zetetic* will be a respectable, open publication which can contribute to the public welfare by helping people sort out what is good from what is nonsense," says Frazier.—NICHOLAS WADE

*The *Zetetic* is available from 923 Kensington Avenue, Buffalo, N.Y. 14215. Subscriptions: $10 for individuals, $15 for institutions.

insulin team had said it would use as vectors pCR1 and any other vectors that might in future be approved by the NIH recombinant DNA committee. When the NIH committee approved pBR322 on 15 January, Rutter says—(the committee gave tentative approval on 15 January and full approval on 23 June)—he therefore assumed that pBR322 was sanctioned for use, since he was not then aware of the NIH distinction between approval and certification.

The UCSF biosafety committee did not learn until May that the pBR322 experiment had taken place. Researchers doing recombinant DNA experiments are required to file a description of the experiment for committee approval. But, as Rutter has said, the memorandum filed by the insulin team did not mention pBR322 specifically. Researchers using the P3 laboratory at UCSF are also required to sign a logbook describing their experiment. Yet Martin says that when he inspected the logbook at the outset of his investigation, he found "nothing recorded." Those in charge of the facility "were not being compulsive enough in seeing that people were filling in the logbook"—a situation which has now been corrected, Martin adds.

Cleaver, Martin's successor as biosafety committee chairman, told *Science* that two entries from the insulin team are recorded in the logbook between 1 February, when the logbook was instituted (the P3 lab officially opened on 9 November 1976), and the end of April. An entry on 1 February notes in the column headed "vector" that pCR1 will be used, and the second entry on 23 April gives pMB9 as the vector. The pBR322 experiment, according to Rutter, took place after 1 February. The vector was not mentioned, he says, because the 1 February entry referred to the general experiment already described in the memorandum filed with the biosafety committee, in which the team had said it would use pCR1 and other approved vectors, and the logbook has to be signed only for each experiment, not for each use of the laboratory. "The signing in of the logbook meant for the insulin cloning experiments in general. The vectors were not designated specifically, and that was just human error," says Rutter. Rutter's laboratory provided the insulin gene for the experiment; it was members of Goodman's laboratory—Goodman was away until mid-April—who performed the cloning experiments.

How should a local biosafety committee respond to an incident of this sort? "I would have expected that the biohazards committee to have investi-

gated the whole thing," says Gartland. Martin did conduct an investigation, and he took action both with the NIH and the insulin team. "I felt comfortable we had resolved the question and eliminated the possibility of it happening again," he says. But in fact, written documents of the committee record criticism only of the NIH.

The pBR322 experiment raises no question of hazard but it does raise the possibility that the insulin team might have gained an unfair advantage over other researchers who had abided by the NIH rules. Another team at Harvard is also working on the same problem. Members of the UCSF team say that they gained no information from the pBR322 experiment which was helpful to the later experiment with pMB9. As it happens, the Harvard team was not neck-and-neck with UCSF because it has not even now published any results.

As far as is known the pBR322 experiment is the only occasion on which the NIH rules governing recombinant DNA research have been broken. The researchers say that the breach was the result of innocent error, a statement not refuted by the available evidence. The experiment presented no hazard to public health nor, in the event, was any unfair advantage gained over competitors. As for the UCSF biosafety committee, its response included action to ensure against repetition of the incident, although not a full public account. The committee's discussion of the experiment, as reflected in the minutes of its 20 May meeting, is confined to an attempt—unsupported by available evidence—to ascribe the error to confusion generated by NIH. But both the experiment and the biosafety committee's response to it occurred in circumstances to which researchers were then still adapting, and for which there were few, if any, precedents.—NICHOLAS WADE

Cryptology: Scientists Puzzle Over Threat to Open Research, Publication

A group of university and industry scientists who are planning a symposium on cryptology have found themselves victims of a bizarre threat from an employee of the National Security Agency (NSA), the government's code-building and code-breaking agency, which says that they may be violating federal laws. And, while the scientists have declared they are not intimidated and will proceed with the symposium, the incident has brought out an unanticipated conflict between researchers' rights to academic freedom and the NSA's job of protecting national security.

Historically, primarily the intelligence community has been interested in cryptology, and it has tended to keep the subject under a tight blanket of secrecy. But in recent years, a number of developments have combined to bring the subject out, so to speak, into the open. One development is a growing corporate interest in secure telephone and data communications. A second is the discovery by scientists doing basic research in mathematics, engineering, and computer science that recent results in these fields can be applied to devising what may be virtually unbreakable codes. The result is that a new field of civilian research has sprung up on this traditional military preserve.

The new research involves both ways to break existing codes and ways to make new codes that are, for all practical purposes, unbreakable. The new codes have generated intense interest among the scientists because they are based on a collection of mathematical problems which can only be solved by running computers continuously for years or even for decades. The only known way such codes can be broken is by solving one of these problems. Interest in these "unsolvable" problems has also sparked discussion among the scientists about the vulnerability of existing coding schemes developed by the government—specifically the NSA and the National Bureau of Standards—and approved for commercial use and foreign export (*Science*, 29 July 1977, p. 428).

The cryptology symposium that has come under fire has been arranged under the auspices of the Information Theory group of the Institute of Electrical and Electronics Engineers (IEEE)—the nation's largest engineering professional society—and is scheduled for 10 October in Ithaca, New York. Prominent among those who will be speaking and presenting papers are Martin Hellman of Stanford University, Ronald Rivest of the Massachusetts Institute of Technology (MIT), Aaron Wyner of Bell Laboratories, and other researchers at IBM, Cornell University, and Ohio State University. As an IEEE symposium it will be open to the public, and a number of foreign guests and participants are expected to attend. In addition, there had been plans to send preprints of the talks to the Soviet Union, under a general umbrella agreement the IEEE made a few years ago with the Soviets. (But this agreement has never been implemented.)

The Information Theory group was preparing for the session as any group of scientists would when, in August, they were sent a mysterious, single-spaced, one-and-one-half-page letter which argued that both to publish in the field of cryptology and to export such publications could violate the 1954 Munitions Control Act (now revised as the Arms Export Control Act). This is the law by which the federal government, through the Department of State, regulates the flow of weapons, computers, and other sensitive equipment to foreign countries.

The letter, from J. A. Meyer at a Bethesda, Maryland, address, argued that several of the group's past, present, and future activities could violate the rules—the International Traffic in Arms Regulations (ITAR)—by which the State Department implements the provisions of the act. It cited the forthcoming Ithaca symposium, a past symposium held at Ronneby, Sweden, several publications in which articles by Hellman had appeared, and the plan to send preprints to the Soviet Union.

Meyer warned the IEEE scientists with sentences such as: "I assume the IEEE groups are unfamiliar with the ITAR, which apply to the publication and export of unclassified as well as classified technical data. . ." and "[A]tomic weapons and cryptology are also covered by special secrecy laws."

The letter also ended with an ominous paragraph:

"Superficially, it appears that a small number of authors are providing most of the papers and most of the motivation. They may not be aware of the full burden of government