Following is the transcription of a discussion held Tuesday, January 15, 1985, in the Woody Room of Van Pelt Library under the auspices of the Office of the Vice Provost for Research. The original tape has been preserved in case of questions arising from editing to convert spoken word to print as described on page II.

Dialogue on Animal Research

Introduction and Groundrules

Dr. Cooperman: Thank you all for coming here. I think this promises to be a very interesting and informative dialogue. What I thought I'd do in beginning is tell you why we're here, review the events that led to our being here, then just make an announcement or two having to do with procedures. Then we'll get right into the agenda which you all have received a copy of.

The immediate reason we're here, really, is a letter from members of the Law School to the Provost and President requesting a dialogue about experiments done in the Medical School on the study of head trauma. I'm sure we're all aware of the facts, but I thought it would be useful to go very briefly into the background that was behind that letter.

Over the Memorial Day weekend there was a break-in into Dr. Gennarelli's laboratory by a group calling itself the Animal Liberation Front, at which time the laboratory was vandalized and sixty hours or so of tapes recording the experiments done in that laboratory over about a three-year period were stolen. These tapes were provided by some mechanism to an organization known as PETA, People for the Ethical Treatment of Animals. PETA then, over some period, prepared an edited version which they entitled "Unnecessary Fuss," at least allegedly drawn from these tapes, which they have distributed widely throughout the United States and also in Europe. A copy of this edited tape was shown to members of the Law School. These individuals were quite disturbed by what they saw and in the letter to President Hackney and Provost Ehrlich, they said that seeing the tape disturbed them, raised in their minds moral and legal issues, and also raised in their minds the question of whether experiments done at the University were in conformance with scientific norms. They, because of being disturbed, then called for this dialogue.

The Administration felt that, as this was a request from colleagues at the University, we, the University, from a collegial point of view, had an obligation to respond to this group in ways that we don't necessarily feel we have toward other groups. We also felt that, given the attention that this experiment, or series of experiments, has aroused, the concerns raised were not limited to members of the Law School. As a result, we requested Faculty Senate to see if there were other people from other faculties who would like to participate in the dialogue. We also thought that, in responding, we would like to have a discussion of the type that we can have in a group of this size, that is to say, a meaningful discussion with a small group. On the other hand, we felt it important, given the interest the case has aroused, to make the information available to the University community at large. The format that we've established, and your presence here, reflect those two considerations.

In just a moment I'm going to introduce people here, and if you'd raise your hand when I say your name everybody then will know who everyone is. Let me begin with Drs. Gennarelli and Langfitt, as the principals involved in the experiments. Then Professors Francione and Watson, from the Law School, who are two of the original signers of the letter, then four members from the Senate who were chosen as a representative group of the faculty: Gary Cohen from the Department of Microbiology in the Dental School; Abraham Edel from the Department of Philosophy; unfortunately Renee Fox is unable to be here because of the inclement weather, which is really quite unfortunate—Dr. Fox is from the Department of Sociology—and finally, Paul Fussell from the Department of English.

My own feeling was that there were two other individuals on the faculty who should be present in a more or less ex-officio capacity. One is Aron Fisher, who was the Chair of the Medical School Animal Care Committee at the time that Dr. Gennarelli's proposal was reviewed within the Medical School. The other is Helen Davies from the Microbiology Department in the School of Medicine who chairs the University Council Committee on Research. Finally we have four representatives of the University of Pennsylvania press: Ann Bailey from *The Penn Paper;* Karen Gaines from *Almanac;* Jeff Goldberg from *The D.P.;* and Marshall Ledger from *The Gazette*.

I would like to just add a personal note about this meeting before we begin. I see this dialogue as a component of the University response to the general question about the use of animals for experimental purposes in research. I think the major issue is the inherent conflict, as I see it, between what many of us in the University community feel to be the necessity for medical and scientific progress, and the kind of experimentation that is needed for such progress, versus the ethical problem of inflicting pain on sentient beings—for although they are animals they are sentient beings. I think that within the University as a whole, and within American society, there is a consensus on the necessity for such experimentation, so the question becomes, What norms should govern the use of animals in laboratory experiments? I think a big portion of this dialogue and, in fact, the continuing discussion of this issue within the University will center on what those norms should be, how the University should meet those norms, and how University experimenters should meet those norms. In this discussion I expect us to place a lot of attention on whether these norms are, in fact, met within the Head Trauma Center in particular.

That's really all I have to say by way of introduction. I do want to review for you the situation about disseminating the proceedings of this dialogue. I sent to you, in a letter dated December 10, a notification as to how the transcripts of the tapes would be handled. At that time I told you that Karen Gaines would be taping this conversation, so I won't go through that because I consider our stated procedures still operative. Several people have requested that they be allowed, in addition to having the transcripts, to actually having copies of the tape itself. I've agreed to that and so Karen Gaines will be copying this tape very shortly after this discussion has concluded. These copies will be available to anyone who wants one with certain conditions attached. The thrust of those conditions is basically that the tapes be for personal use. I will have copies in my office of both the release form and the tapes available, I think by early this afternoon. And so, I think anyone who wants to have a copy of the tape can have one very soon. That's really all I have to say. Are there other questions at this point? If not, I'd like to turn to the second part of the agenda, which is the presentation of the experiments.

Professor Francione: I have a question here.

Dr. Cooperman: Sure, Gary.

Professor Francione: I have here a copy of this release form. Is it necessary that you sign this release form before you get a copy of the tape?

Dr. Cooperman: That's my intent.

Professor Francione: Participants will not get a copy of the tape if they do not agree not to give it to the "broadcast media" or "to use it in any way to damage the reputation of the University of Pennsylvania"? We have to agree to this?

Dr. Cooperman: That's right.

Professor Francione: Do you see any problem with restricting how your faculty members disseminate information?

Dr. Cooperman: No. There are no restrictions with regard to the transcripts. The transcripts themselves are going to be provided, really, without condition. Since we hadn't reached agreement among the participants about the tapes and this issue came up rather late, I thought the way to distribute them was to put that restriction on it. I mean I can't believe that anybody present here wants to damage the reputation of the University by using the tapes, and so I didn't see that as a problem. Is it a problem?

Professor Francione: It depends on how you define "damage the reputation of the University."

Dr. Cooperman: I guess it does.

Professor Francione: Some people have different thresholds for evaluating.

Dr. Cooperman: Well, it's really intent. I'm not sure what the legal content of the release form is. It really is basically to give people comfort that no one is trying to use the tapes in a way

that has a purpose damaging the reputation of the University or of the participants in this discussion, and that's really the reason for saying that.

Professor Watson: I would rather not receive a tape under these conditions, but I would like to be assured that the transcripts would be available soon as well.

[Discussion of typing and distribution schedule, deleted for space. In what follows, speakers have eliminated false starts and repetitious phrasing, adjusted syntax for clarity, and in a few cases—with the knowledge of other participants concerned—improved word choices for accuracy of meaning intended. Elision marks (. . .) indicate interruptions and resumptions, and clarifications by addition are in brackets.—KCG]

Dr. Cooperman: O.K. If we can now proceed to the second item of the agenda. As Dr. Gennarelli goes through this description of research, if there are technical terms that come up with which you aren't familiar, I think it would be perfectly appropriate to raise your hand to be recognized for an explanation. Dr. Gennarelli . . .

Description of the Research

Dr. Gennarelli: In preparing these remarks it became clear that I could go on for hours describing, in various levels of detail, the research and its background and its productivity. I thought, since this is intended to be a dialogue among us, that the bettter approach would be to give a rather thumbnail sketch of what this is all about so that we can leave ample time for questions and discussion. If the group would instead prefer a longer diatribe from me, I'd be happy to give that, but I thought I'd try to confine it to a very basic view.

I think, obviously, when trying to analyze research, one of the first questions that comes to mind is "Is the question being studied of any significance or importance?" and I hope that many of you have had the chance to read the *Almanac* article that I wrote regarding this. I think it amply documents that brain injuries are, in fact, a major problem—not only to doctors such as Dr. Langfitt and myself who treat brain-injured patients—but is a substantial public health problem in the United States. The numbers are listed in the *Almanac* article, gained from several sources, but the bottom line is that there are in the range of two million head and brain injuries per year, of which roughly fifty thousand of them are fatal and, of the survivors, a substantial number never return to full productivity as useful members of society. So this is a substantial problem and, we feel, worthy of investigation. The question then, is how to investigate such a problem. The problem has been around for millenia and it hasn't gone away with the kinds of investigations that have been present, that have been undertaken to the present.

Historically, the patient, of course, has been the first line of investigation, with clinical studies of various levels of complexity, studying both individual patients in greater or lesser degrees of detail, and then studying groups of patients in an epidemiological manner. Those studies, historically, have provided a great amount of information. They've been able to characterize the types of injuries that occur. Suffice it to say, that there are many, many different kinds of brain injuries; depending on how you classify them, there are anywhere from ten to thirty or forty separate different kinds of brain injuries. Some are clearly more important than others. Some are clearly less important than others in terms of the degree of difficulty of treatment, in terms of the mortality (the deaths that they cause), in terms of the disabling lives they leave the survivors and in terms of the frequency.

We ourselves have undertaken a study as part of our head injury center activities to determine what are the bad injuries and what are the not-so-bad injuries. We'll come back to how those influence the animal research in a moment. So we feel that head injury is an important problem.

There have been a lot of clinical investigations that have shed a lot of light on the problem, but the fact remains that the mortality rate for serious brain injuries remains in the forty to fifty percent range. This has been generally true over the last fifty years and, despite advances in medications, advances in emergency medical services, advances in surgical techniques, intensive care unit treatment, all of these things have not appreciably diminished the mortality rate from major brain injuries. So there's a lot of room to improve the roughly fifty percent mortality rate that exists in serious brain injuries. That gets us to how do we investigate brain injuries other than in an epidemiological sense, that is studying populations of patients or studying single patients intensely. There are simply data that cannot be obtained, that are felt important to be obtained, from human beings. And this relates to a number of matters, not the least of which, is that patients, from a scientific point of view, become injured in very messy fashions. That is, they have circumstances that compound their injuries, multiple injuries, and the specific circumstances that cause their injuries are never to be known. All one may know is that the patient was in a car crash, he may know some greater detail about the circumstances of the car crash, but the critical variables are never known in a clinical situation. And those variables relate to whether the head strikes something, how fast the head moves when it strikes something, the direction that it moves, and some of the other biomechanical variables that are the things that cause the brain injuries.

Now, to divert for a minute about what brain injuries are, I think it's important for the group to understand, because it does relate directly to the research, what brain injuries are all about. There are generally two kinds of brain injuries: focal brain injuries, and diffuse brain injuries. Focal brain injuries generally result from something striking the head or from the head striking something, that is, an impact with an object, and that impact causes localized deformations, localized forces to exist on the skull and the brain, and causes damage that is generally in one area. It may be a very large area, a very small area, but the damage is relatively localized. That is a fundamentally different kind of brain injury than the second category—diffuse brain injuries. Diffuse brain injuries result from the manner in which the head moves during an accident, that is, the direction that it moves, how fast it moves, and whether it's accelerated or decelerated during an injury, and that group of injuries (the diffuse brain injuries) causes fundamentally different kinds of brain damage than the focal injuries. It causes damage that, until these experiments, was exceedingly poorly understood, to the point that some thirty or forty different names were given to these injuries that are clearly part of a spectrum of injuries that go from injuries that are more or less purely recoverable to injuries with a mortality rate in the sixty percent range. The diffuse brain injuries, in terms of their cause, have nothing to do with an object striking the head. They have to do with the way the head moves after it is struck or, in the case of deceleration, how the head moves as it's slowing down. These are the category of injuries that we have been most interested in—the diffuse brain injuries. The reason for that is because they are the least understood: Their therapy is entirely nonspecific. If one considers what we do as doctors to take care of patients with diffuse brain injuries: It can be summed up to say, "We have no specific treatment for this large group of injuries. We provide supportive care to the patients and that's about it." This is despite all that has gone on in clinical medicine in trying to care for these patients historically over centuries. So the thrust of our research has been aimed at trying to understand the diffuse brain injuries.

It's common practice that when these patients come into the hospital we explain to their relatives that their relative has suffered brain damage. We teach that to our residents. Until very recently, and as a product of these experiments, we really didn't know what we were talking about, in specific, when we said "brain damage." Now we have a very precise idea of what this brain damage is, and are engaging upon a better understanding of it with hopes of providing specific therapy for this kind of injury. This injury, by the way, comprises between 40 and 50 percent of all serious brain injuries, so it's not an uncommon injury. This type of injury is one of the two largest causes of death and disability in humans.

So it became clear that, for a number of reasons, we needed to develop model systems that would help us study this particular kind of injury. These were originally developed—let me backtrack a moment to say that pioneering work in experimental head injury has been done for a long period of time—but a signal study was done in the 1940s by Denny Brown and Richie Russell who demonstrated that acceleration of the head could produce cerebral concussion. Those studies in the 1940s were excellent studies and were the predicate upon which many subsequent studies in experimental head injury were done. They were done, I should mention for this group, under deep general anesthesia and, partly as a product of that, many misconceptions arose from their work. As a product of their work, many governmental agencies, directly or indirectly, or many investigators began to study head injury in experimental situations. Many species were

used, many preparations were used, almost all of which involved some type of impact to the head where some type of striking device was thrust against the head of various species and the physiological results and sometimes the pathology was observed. These led to a general understanding of what kinds of events were necessary to produce brain injury. As time went on, those initial studies from the forties and fifties became the groundwork for which most of the protective standards aimed at preventing injuries were designed. These are principally, transportation agencies in the governments of many countries who have designed standards that say, for example, a car environment should not allow the head to do such and such—should not allow it to exceed a certain acceleration level in a certain period of time, for example. Despite the implementation of those standards, as you all well know, car crashes, for example, still cause many, many head injuries. Something in the range of 25,000 head injury deaths a year are caused in car crashes. So rather clearly, the preliminary work that was done in the forties and fifties had a lot of room for expansion just on the basis of the preventative aspects, not to mention the fact that people's treatments were still being provided nonspecifically.

We first began experimental head injury in the early the 1970s with a paradigm that is roughly similar to what we're using now. This involved a small-brained monkey, a squirrel monkey, that has a brain of roughly 20 or 25 grams in size. The experiments were intended, as our current experiments are, to separate what we feel are the critical injury-producing variables, namely, impact force from head movement or acceleration. The experiments were carefully designed by others as well as ourselves to, as best as possible in an experimental situation, deliver a non-impact acceleration force to the heads of animals. To do so is no mean feat considering the levels of acceleration that are necessary to produce brain injury. Therefore, a large developmental effort was originally undertaken and the paradigm that was used is similar to what we use now. The basic gist of it being that an accelerating device develops a force that is transmitted through a metallic helmet, a metal helmet, to the head of an animal. The head of the animal and the metal helmet must be tightly coupled together so that what is precisely delivered is precisely received by the head. What this also accomplishes is a distribution of the injury force evenly and widely over the head so that there's no focal loading or impact of a ball pene-hammer-like effect, but rather, the effect of the force is as if you would grab the head and move it very rapidly in whichever direction and amount you choose to do.

Those initial experiments were performed at the NIH, under NIH review, in house, and when I came to the University of Pennsylvania in 1976, the experimental head injury laboratory was just beginning to engage in these same general types of experiments. From roughly 1976 through 1980, a different helmet situation was recommended by our mechanical engineering colleagues, and this was a thermoplastic helmet. Thermoplastic material was warmed in a pan of warm water. When it becomes very pliable, then the material was molded to the animal's head so that a form-fitting helmet was created. Then that helmet was attached to the accelerating device in the same manner as the metallic helmet.

What we found in those initial experiments —with what is now called the PENN-1 device, as opposed to the subsequent PENN-2 device—what we found in that group of experiments were several things. Number one, we found that at low levels of acceleration physiological changes occurred, often very dramatic in nature, that were not associated with loss of behaviorial consciousness or coma. This was an important observation because in the 1940s, those physiological observations were used as an index of cerebral concussion or coma. The blood pressure would change, the heart rate would go down to perhaps one-third of its control values. These were the kind of things previously used as indices of coma, and in the lightly anesthetized animal—as opposed to the deeply anesthetized animal—it was very clear that these events occurred before coma occurred.

This, of course, brings into question a lot of the other previous work and a lot of the safety standards that were predicated on that previous work. At higher levels of acceleration to the head, cerebral concussion could be produced, reproducively. That was not particularly a striking finding, since other investigators had been able to produce that using different animal models, or in different experimental paridigms. What we were after, was to create the severe form of the diffuse brain injury, the one that has a mortality rate of fifty to sixty percent and that is so commonly seen by us in the clinic. We felt that this injury was simply a more severe variety of injury than the cerebral concussion, and that all we had to do was to continue to increase the levels of acceleration to the brain, and we would go from the short coma of cerebral concussion to the prolonged coma of the major diffuse brain injury. This, however, did not turn out to be the case, and what we found was that instead of producing prolonged coma of the diffuse brain injury type, what we produced were animals that were dying within, or would die without support, several minutes of an injury from an acute subdural hematoma. An acute subdural hematoma is a collection of blood between the surface of the brain and the inner surface of the skull. And this was a bit of a distraction from one point of view, in that it was not our intended endpoint. We were intending to produce the diffuse brain injury, but instead produced the acute subdural hematoma. It turns out that that model to produce acute subdural hematoma was very reproducible and it also turns out that the subdural hematoma is the most frequent cause of death in humans. So we had inadvertently found a reliable experimental model for studying the most important cause of death in humans. We did, however, still want to study diffuse brain injury.

About this time, the late part of 1980, our laboratory was moved from the Hospital building to its current location in the Anatomy/Chemistry building. This was simply a necessity because Hospital reconstruction was taking over the space. Just a word about the laboratory. The laboratory has an absolute requirement of being in a ground-level location because the accelerating device requires a six-by-six-by-six foot cube of reinforced concrete to attach the accelerating device to, so that it obviously is unsafe to put it anywhere other than on a ground level. The only space available to us from the Medical School was the current location which is in the basement, or the subbasement of the lowest level of the Anatomy/Chemistry building.

At this time, or very shortly before that, Dr. Thibault, whom I had previously worked with at the NIH, was attracted to the University of Pennsylvania and came on to the faculty. We went back to our original paradigm of controlling the head, that is, no longer using the thermoplastic helmet, but going back to a situation where we used a metallic helmet and we fitted the animal into that with a plaster-of-paris-like material of dental-stone.

During the early part, the early few months of the new laboratory, many procedural changes were going on and, because of these, we were able to produce our intended goal of the diffuse, the severe form of the diffuse brain injury in the spring of 1981. This became a very exciting time in the laboratory because this was an absolutely unique finding. No one had ever produced—and to this day still has not—produced prolonged coma in any kind of an animal preparation in the absence of large mass lesions within the head. So no one had been able to duplicate the common clinical entity of the severe form of diffuse brain injury until the spring of 1981. We were obviously very excited about that and, in addition to the production of this, it also dispelled many, many questions about experimental head injury. It had been, for instance, argued that this injury could not be produced in experimental animals either because it was unique to human beings, or because it was so rare in human beings that the events that cause it couldn't be duplicated, or simply because the brain size of non-humans is too small to produce such an injury. So the mere production of this important type of clinical brain injury was extremely important.

The thrust of the laboratory since that period has been to better understand the pathology and the physiology, as well as the biomechanics, of this kind of injury. This has led us to a somewhat revolutionary, I guess the word might be, understanding of this particular kind of injury. We now feel quite strongly, the evidence is quite strong now, that these injuries are caused by primary damage that acceleration induces on the axons of the brain, the nerve fibers of the brain. And hence, we've now named this injury "Diffuse Axonal Injury," or DAI for short, and this term has now become very widespread internationally amongst the experts in head injury and is widely used, especially by pathologists, and more and more so by neurosurgeons. It provides a very powerful explanation for the brain damage that occurs in these patients in whom we didn't know exactly what we were talking about previously when we said "Your mother

has 'brain damage.' " Now we know, rather precisely, what we mean by that. We mean that many, many nerve fibers, or axons in the brain, have been damanged at the time of injury, and the very intriguing thing that we've appreciated recently, is the precise location of that nerve damage. It's at what are called the nodes of Ranvier which, if you envision the nerve fiber as a chain of linked sausages or hotdogs together, the nodes of Ranvier are the uninsulated parts of the nerve that are very small, and that seems to be the precise locus of the influence of trauma on the brain under these circumstances. I should say that coincident with these primate experiments, we have developed a rather extensive program of investigation of trauma of the nervous system. This is reviewed, to some degree, in the Department of Surgery's annual research report and, included in the research report, if you are interested, are references to some recent publications from the laboratory. I would just point out to say that the animal use, the animal experimentation is only one portion of all our investigative efforts involving brain injury. The other kinds of things are listed there, and we can go into them in more detail if you desire.

I think I'll stop at this point and open it for questions unless there's something that you feel we should cover specifically now.

Discussion Period

Dr. Fussell: After these experiments in acceleration are applied through this head device, how is the animal behavior tested?

Dr. Gennarelli: Well, there are various methods, depending on exactly the purpose of the experiment. There are some experiments, for example, designed by one of our investigators, Eliot Stellar, who is interested in behavioral and memory deficits after brain injury. That group of animals is pretested for a period of usually two, sometimes three, months before the injury so that his new learning is quantitated, and then those same tasks are applied after the injury to find the difference. So that's one behavioral aspect. The general behavioral aspects are appreciated by those of us in the laboratory from our clinical experience. That is, neurological examinations are performed, observations of the animals are made and recorded on a scale. We've developed a primate injury scale that parallels the kind of scale by which we judge the severity of human injuries (it's called the Glasgow Coma Scale). Using that, we give numerical data in semi-quantitative fashion to be able to describe the severity of the brain injury in one animal versus another.

Professor Francione: Are we following the schedule now?

Dr. Cooperman: Yes, I think we're into part three. If we are going to follow the agenda, and I think we should, that we should begin with the questions on the general focus on the research. This list is not meant to be all-inclusive, but the kind of questions that were raised in communications by this group to me prior to this meeting have to do with the control of variables, the use of animal models, the definition of head injury as opposed to treatment and the like, and so I think if we could do part A, we could move right down this schedule.

Professor Francione: Can I first make a statement about the dialogue? Is that acceptable to you?

Dr. Cooperman: Sure. You can say anything you want.

Professor Francione: Those of us who signed the letter [requesting the dialogue] signed it in response to seeing a tape of approximately twenty-four minutes called "Unnecessary Fuss." And we were upset by that film. We wrote a letter to the Provost and to the President indicating that we were upset about it and that we wanted to talk about the issues raised by this tape. We received a response saying that we would have this discussion about the issues raised by the tape. I stated to you in a letter and on the telephone that I thought that it was crucial that we see the excerpted tape. Your objection was that the tape may contain excerpts taken out of context, and may present some sort of unfair characterization of the work done at the head injury lab. Drs. Gennarelli and Langfitt are here. Dr. Fisher is here. I don't understand why . . .

Dr. Cooperman: Gary, allow me to interrupt. We have a limited time. I think it would be more productive—I mean I consider that issue settled. I understand that you disagree with my judgment, but I don't think we want to have a long discussion about the rationale for or against showing the tape. If we do, we will simply take away from your opportunity to ask questions that I thought you wanted to ask about the contents of the tape. If you want to go on, I can't prevent you, but it seems to me that that's going to take time away from what I thought we were here for, which was to discuss the questions raised in your minds about what was on the tape.

Professor Francione: Fine.

Dr. Fisher: May I ask Gary a question that he may not be able to answer but, as he says, he is a member of PETA, maybe he does know. Why has PETA not given us a copy of the tapes so that we could see the whole . . .

Professor Francione: I, along with twenty-nine thousand, nine-hundred and ninety-nine other people belong to People for the Ethical Treatment of Animals. I also belong to other organizations and I . . .

Dr. Fisher: I'm not asking about your membership. I'm asking if you have any insight into why PETA will not give a copy of the complete sixty hours of tapes.

Professor Francione: I don't have any insight about that . . .

Dr. Fisher: Sounds malicious.

Dr. Cooperman: Well, we could have a discussion about the politics of the tapes, and . . .

Professor Francione: We're sitting around talking about the issues raised by the tapes but we haven't seen them.

Dr. Cooperman: I understand that, but people have seen them, and I think you've examined them and I think you're prepared to ask questions about them and, as I say, if we want to spend an hour discussing the rationale of having the tapes here or not, we can do that, but I think that would be unproductive. I think we're here to discuss what questions were raised, and I'd like to get to the crux of the matter.

Professor Watson: I think we'd have gotten to the crux of the matter if we had seen the tapes.

Dr. Cooperman: Well, I understand that. And I'm quite willing to let you have the last word on that . . .

Professor Watson: Fine.

Dr. Cooperman: . . . but could we now go to the questions that are raised in your minds about the tapes?

Professor Francione: Yes, but I just wanted to make a couple of other observations. The second observation I wanted to make is I think it is unfortunate that Dr. John McArdle from the Humane Society of the United States is being forced to sit in the hall because I was not allowed to bring a science advisor into the room. I thought that was a very reasonable request, especially when I stressed to you that he would not participate in the dialogue at all.

Dr. Cooperman: I think the record of this conversation can have that.

Professor Francione: Fine. The third thing I wanted to say is that this committee was, in part, convened by Dr. Jacob Abel . . .

Dr. Cooperman: Yes.

Professor Francione: . . . and I object to the fact that Dr. Jacob Abel did not make public—he certainly didn't tell me—that he has co-authored published research with Dr. Gennarelli having to do with head injury in primates. I'm not questioning the integrity of anybody who was selected by Dr. Abel. I'm saying that I'm surprised that the Administration, which I believe knew that Dr. Abel had published this research with Dr. Gennarelli, would tolerate such a clear conflict of interest. I'm sure that the reaction would have been very different if you appointed me to choose the committee.

Dr. Cooperman: I will respond to that, but very briefly. It was my judgment that Jacob Abel could make those selections without a conflict of interest. And it was his judgment as well. So, can we now return to the issue at hand, which are the questions raised in your mind by the tapes?

Dr. Cohen: Yes, I have two questions for Dr. Gennarelli.

Dr. Cooperman: I believe Professor Francione had the floor to ask these questions, and is there any reason why we can't proceed in that way?

Dr. Cohen: No. Not at all.

Dr. Cooperman: Fine.

Dr. Cohen: I thought you had finished.

Professor Francione: Tom, you talked before about the statistics of head injury. You talked about the numbers of head injuries that occur every year. In those statistics that you talked to us about this morning and in those

published in the *Almanac*, how many of those injuries are consequential, survivable or not survivable under any circumstances, preventable by simple precautions, self-inflicted? Are we making any sort of breakdown there?

Dr. Gennarelli: That's a mighty complex question. You've got six or eight different questions in there. Can you break them up one by one?

Professor Francione: Yes. How many of those injuries aren't survivable under any circumstances?

Dr. Gennarelli: It's estimated that, of those patients who die, that there is probably an irretrievable mortality of somewhere in the range of 15 to 20 percent depending on the kind of injury, the age of the victim, and so forth. What I'm saying is that the magnitude of the injury itself is of such severity that survival under the most optimal circumstances is not compatible with our current medical practices.

Professor Francione: So those figures that we have, both in the *Almanac* and the ones we've discussed this morning, don't break out injuries that really you couldn't do anything for anyway, no matter how many primates you tested?

Dr. Langfitt: Could I respond to that, Barry?

Dr. Cooperman: I think it's open to anyone, but sure.

Dr. Langfitt: First of all, if we take the figure of a million and one-half million individuals who are at risk for head injury in this nation, the larger majority of those have minor to moderate head injuries. They don't have the severe head injuries with the very high mortality rate. I don't know what the precise percentage would be, but taking that number, those individuals who could not survive, under any circumstances that we understand today, would be a very, very, small percentage.

A point that Tom did not mention, with respect to the story of diffuse axonal injury, is the evidence that it is important not only in severe head injury, but also in the field of minor head injury. Just this past month, one of our colleagues published with Tom a paper in the *Journal of Neurosurgery*, in which they described what happens in primates who have a minor head injury; again, these were rhesus monkeys accelerated in just the fashion that he described. These animals were unconscious for no more than a few minutes, a sort of simple concussion, or a little bit beyond concussion. The animals then awakened and were perfectly all right, but when they were sacrificed in about two weeks, there was evidence of diffuse axonal injury in the brains of those animals that had been unconscious for no more than two minutes. This is important because it has only been in the past few years that we have recognized the consequences of minor head injury.

Quite frankly, neurosurgeons of the past used to sort of brush aside the complaints that people had after having been knocked unconscious for brief periods of time, oftentimes thinking these were functional, or emotional types of things, or goldbricking, whatever term one wishes to use. Largely through the efforts of the group at the University of Virginia in a prospective study of some 500 patients with minor head injuries, it was demonstrated that these patients were suffering significant signs and symptoms suggestive of organic brain damage after no more than just a few minutes of unconsciousness. In fact, a third of these patients were not back at work three months after a minor head injury. Neuropsychological evaluation of some of these patients was done using very sophisticated tests, which indicated that many of the patients had evidence of organic brain damage. We now think that we can draw a relationship between that clinical story of minor head injury producing devastating consequences in these patients and the evidence in the animal studies that even with minor head injury there is evidence of diffuse axonal injury or organic brain damage.

Professor Francione: Can anything be done for axonal injury?

Dr. Gennarelli: We think so. And this is one of the very, very exciting postulates . . .

Professor Francione: Axonal means nerve damage, correct?

Dr. Gennarelli: Right . . .

Professor Francione: . . . as opposed to, say, vascular damage?

Dr. Gennarelli: That's right. Originally, we felt that one thing happened in axonal damage, that is, that the nerve fiber—which in some cases is a couple of feet long—was transsected at the moment of the head acceleration. It turns out that our investigations have shown that that's probably not the case, and the importance, of course, is germane to your question, because we felt that there would be very little to do from the therapeutic point of view if the nerve fiber was actually cut in two at the instant of injury. If that were the case, then one would have to rely on the regeneration mechanisms which are being intensely studied by other neuroscientists, but are not known to be particularly effective over long distances in the adult mammalian brain. However, our evidence suggests that the nerve fibers, the axons, are not transsected at the moment of injury. They're clearly damaged at the moment of injury and, subsequent to the injury, a chain of events is set into motion that by 12 or 24 hours after injury begins to cause damage—axons that are damaged but not transsected *do* become transsected. This is what we call secondary axotomy. That is, axons that aren't broken become broken by some process that begins at the time of the injury but does not become fulfilled for some to 12 to 24 hours. So our efforts now, and this is very current, are to try to understand that process by which a damaged axon goes from being structurally intact, that is, damaged but still together, to the point of being disrupted. And there's the therapeutic challenge, in that if we can understand that mechanism and develop strategies to prevent it then there's a possibility now of a *specific* treatment for brain damage—traumatic brain damage. And, as I mentioned, we now do not have a specific treatment for brain damage.

Professor Francione: These experiments have been going on, at least with respect to your participation, since 1975? Is that correct?

Dr. Gennarelli: I came to the University in July of 1976.

Professor Francione: Were the experiments going on before then, here?

Dr. Gennarelli: In a very preliminary fashion. I believe for only a month or two.

Professor Francione: Can you sort of estimate the number of animals that have been sacrificed throughout the history of the head injury clinical research lab at least going back as far as 1976?

Dr. Gennarelli: To date the number is just over 200, I think.

Professor Francione: The protocol that is effective, or purported to be effective, from 1982 to 1987, talks about the use of 250 babboons.

Dr. Davies: 1987 did you say?

Professor Francione: Yes.

Dr. Gennarelli: You mean the currently running grant?

Professor Francione: What I'm talking about now, Grant Application NS08803-13, principle investigator Thomas Langfitt. This is August 1, 1982, through July 31, 1987. It talks about the use of 250 babboons, but to date there have only been two hundred animals used in the . . .

Dr. Gennarelli: Yes. On the average, that being 24 a year.

Professor Francione: I was reading recently an article that appeared in a Scottish newspaper on December 1, 1983, involving an interview with a man named Dr. Adams, who is listed on the protocol, Tom. Dr. Adams made the statement that, to date, the results are "academic" and that the team—and I believe he was referring to the team comprised of the University of Glasgow and the University of Pennsylvania working on this—has not provided any sort of preventive measures. Now is this business with trying to figure out how to prevent the transsecting of axons going to be the first treatment-related breakthrough that the laboratory will make?

Dr. Gennarelli: First off, let me make a comment about using quotations from newspaper articles as biblical gospel. There have been, I can't tell you how many, misquotations. That particular citation is a misquotation of Dr. Adams.

Professor Francione: He didn't say that?

Dr. Genneralli: You may ask him if he said that.

Professor Watson: Well, you're saying he didn't.

Dr. Genneralli: My understanding from him is that that was a question to which he responded and the response was misquoted in context but, that aside, let me answer your question. There exists no specific therapy for brain injuries in clinical use today despite trials of innumerable therapies. One by one they've been shown by at least some group to be ineffective in one manner or another. The only specific therapy that exists at this moment is the removal of blood clots or what we call mass lesions, that is, damaged areas that are taking up excessive space in the brain. All

other treatments are currently aimed at either reducing swelling of the brain, reducing the water content of the brain, the blood content of the brain, or other measures to treat what we call epiphenomena of the injuries themselves. There is no specific treatment for the injury itself. So we have not yet, as of this date, produced a treatment for brain damage. We're confident that we're on the right road to be able to do that, and we're confident that the data shows us that it's possible to have a specific treatment.

These are all very new ideas, and whether they will become realities in the future, I can't tell you. But I think we're further along than a lot of other people in the area with regard to this. There's no one else in the world who has available to them this model of diffuse axonal injury.

Dr. Langfitt: Tom, if I might make just a couple of comments in response to the question: It is not really possible to develop effective diagnostic and therapeutic modalities for a disorder until the basic mechanisms responsible for that disorder are understood—ranging, for example, from deficiency of insulin as a cause of diabetes to causes and treatment of cancer. One needs fundamental breakthroughs. The more fundamental the information basic to the understanding of the the cellular mechanism, and the abnormalities that lead to the disorder, the greater is the likelihood that one can develop an effective therapy. Cancer is a very good example: In the past four or five years, the discoveries of molecular biology of genetics as they relate to cancer have absolutely staggering implications, many of which results from animal research. When the cause of cancer, or of different kinds of cancers, is determined, we're not going to stop at that point because we don't have the therapy to treat them. Rather, once we find the cause, we will try to develop the therapies. And it could be that in given kinds of cancers it's going to be a very long time, maybe a decade or longer, before we find the therapy to treat the disease. We don't stop once we've found the cause; we go forward to try to find the therapy. Now, as Tom described, we think we understand the fundamental basis of brain injury that form a diffuse axonal injury. It may be a long time before we derive the therapy that's going to be effective in these patients, particularly for that twelve- to twenty-four-hour window that exists between the time of the injury and the time of the secondary axotomy. However, if we succeed in so doing, then we're going to have an enormous impact from this research.

Professor Francione: A greater impact than say, taking all of the money that has been used over the years, and trying to develop safeguards and precautions, trying to outlaw blows to the head in such frivolous ways as boxing?

Dr. Fisher: I don't see any point to this one-way discussion about the merits of scientific research, and it seems to me a waste of time for most of us here. Instead of discussing the philosophy of research, we should be discussing the use of animals in research.

Professor Francione: Well I have specific questions. If you want to hear them I'd be happy to ask them.

Dr. Fisher: I would rather do that. Otherwise, the dialogue is not relevant to what we're after.

Dr. Cooperman: Gary, you had a question.

Dr. Cohen: Well, I don't think I'll interject it at this moment. I think the decision as to the direction of the discussion is more important.

Dr. Fussell: If I might ask Dr. Gennarelli one question, which I think might move us off this into something that is more precise. This is item "C" under Roman "3," the matter of anesthesia. I think if these animals were anesthetized, I think the objections of the PETA people would disappear. I think, therefore, it's important that we ask the question, why are they not anesthetized, to what degree are they anesthetized, and so on. So could you shed light on that?

Dr. Gennarelli: Sure. I'm not sure that I would agree with you that PETA would disappear on the basis of this one issue. It's obviously the most crucial issue, I think. I guess the best way to approach it is by what the particular needs are for anesthesia in the particular experiment that we're doing. Those needs are several-fold and they sort of run longitudinally with the experiment.

The first need is to be able to get the animal safely handled so that he is not a threat to personnel and not a threat to himself while being taken from the animal care facility to the laboratory.

The second need for anesthesia is the need to prevent pain during the time of the minor surgery that's used for insertion of the various monitoring lines. And, I should say at this point, that those procedures are absolutely identical to the kinds of things that we do in the intensive care unit or at the bedside. That is, we're not talking about major brain surgery or major chest or abdominal surgery here. We're talking about minor surgical procedures such as we commonly use out of the operating room in patients.

The third need for anesthesia, and perhaps one that will be the most debated, is the actual traumatic insult itself. Maybe if I just outline these first, then we can go back to specific areas.

The fourth is the need for anesthesia while removing the monitoring lines, the intraveneous lines, the line to measure intracranial pressure. And the last need for anesthesia is in the general recovery phase, and again is one that can be some point of debate, after the brain injury has been achieved.

Now to subserve all of those various needs, and you can appreciate that they're a complex group of needs from being able to handle the animal safely to absence of pain and so forth. It's fairly clear that a lot of thought must have to go into this, in order to develop the agents that we feel are most appropriate. There are several considerations that we needed to consider when choosing the anesthetics, amongst which were the animal's inability to feel pain and, the ability to be able to examine the animal neurologically to determine if we've produced a coma and, if we have, to be able to assess the depth of that coma, the severity of that coma, and any other neurological abnormalities that may be present. One of the critical variables in this whole laboratory effort is the correlation of those clinical events with the input phenomena, the biomechanics, the shape of the acceleration pulse, its height and width and rate of onset, all of these variables correlated with the physiological observations correlated with the structural observations from the pathology of these injuries and in some instances, with biochemical alterations in the brain. So, we chose the particular anesthetic paradigm that we did with those considerations in mind, and chose two agents to be used conjointly: a drug called phencyclidine, trade name Sernalyn, which is a drug given by injection, and an inhalation drug, nitrous oxide . . .

Professor Francione: Where was it injected? How was it injected into the animal?

Dr. Gennarelli: With a needle.

Professor Francione: Yes. I understand that, but where? Intravenously or into the body cavity?

Dr. Gennarelli: No. It was given, usually, intramuscularly—sometimes intraperitonially, but usually intramuscularly. The way that's done is that the animal is in a cage that has a movable back.

Professor Francione: A squeeze cage?

Dr. Gennarelli: A squeeze cage type of affair. If the rest of you are not familiar with that, that's one of the common ways that the animal's brought forward to the front of the cage and given the injection. One of the most important points about this particular drug is that the drug provides an adequate level of analgesia, that is, absence of pain sensation, without producing depression of respiratory function and cardiac function which is one of its unique properties, in distinction to many of the other commonly used drugs such as narcotics or barbituates whose functions depress respiratory activity and cardiac activity in almost the same linear fashion as they depress the ability to appreciate pain. This is of particular advantage, to use a drug like Sernalyn, because the laboratory is not adjacent to the animal care facility, so the animal has to be taken from where he lives to the laboratory. We feel that a drug like Sernalyn can do that very safely because we don't have to worry about whether the animal is going to stop breathing or whether his heart's going to slow down appreciably because this drug does not have the property of depressing the heart and the blood pressure.

Professor Francione: How far in advance, Tom, is this Sernalyn administered before the injury is actually inflicted?

Dr. Gennarelli: Well, the answer is that it's variable from several minutes before to an hour before . . .

Professor Francione: Several minutes before the actual injury?

Dr. Gennarelli: It's possible that that's in the range—depends on how many doses of Sernalyn are used in any one particular animal.

Professor Francione: The protocol says that the animals are given a single parenteral dose of phencyclidine, one milligram per kilo, on the morning of the experiment—that's at page 192 of the protocol. The protocol later

says that it takes between two or three hours to intubate the animal, to get all the monitoring devices placed, and to pot the animal's head into the injury device using the dental-stone. Is the protocol incorrect?

Dr. Gennarelli: It's incomplete, I think. I think maybe Dr. Langfitt could speak to this point of protocols in grants. Dr. Fisher and I had a little go-around about this and there seems to be some concern—as this is an example of—of how detailed one's protocol should be in a grant. The purpose for the protocol in a grant may be substantially different from the purpose of the protocol, let's say to the Animal Care Committee. Perhaps Dr. Langfitt could address that global point; he has more experience with grants.

Professor Francione: Before he does that, can I perhaps flesh out my questions so Dr. Langfitt could do it all at one time?

Dr. Cooperman: Sure. And it would be good to get the reason that you feel you want to press this point.

Professor Francione: The discussion in *Almanac* states that the animals were anesthetized at the time that the injury was inflicted. And, in looking over the protocol and in the published papers, it appears as though the following regimen was used: On the morning of the experiment, approximately two to three hours before the injury was inflicted, one milligram per kilogram of phencyclidine was administered to the animal. The animal spontaneously ventilated seventy to eighty percent nitrous oxide until one hour before the injury, and I'm reading from the protocol, ". . . through the endotracheal tube, the animal spontaneously ventilates seventy to eighty percent nitrous oxide anesthesia until one hour before acceleration, after which it breathes room air, infiltration anesthesia is utilized at all surgical sites." If you're using infiltration anesthesia, there must be some need for it if the animal was anesthetized, and that if the phencyclidine and nitrous oxide combination was working all the way through up until the time of the injury, there would be no need for infiltration anesthesia.

But in any event, the protocol also says that within the hour before the injury, the animals —at least some of the animals, it's unclear— are taught new memories. It's unclear to me how you can be teaching new memories to animals who are under the influence of a dissociative. The excerpted tape itself shows the animals moving around on the table. There are very purposeful activities going on on that table. At one point a baboon twists the same way five different times. I suspect one can say that those could be characterized as random movements, but what is the possibility that that sort of activity could be random? The animals are grasping at the restraints.

Dr. Gennarelli: If we could respond to one point at a time, I think it would be easier. You're getting a lot of questions mixed up and it becomes hard for us to answer them in such a global fashion.

Professor Francione: Were the animals anesthetized at the time of the injury?

Dr. Langfitt: Let me try to add a bit to what Tom said in describing the characteristics of dissociative anesthesia. We don't use Sernalyn or phencyclidine but we do use dissociative anesthesia in our neurosurgical procedures in the operating room. Just as in the experiments, the whole objective in clinical anesthesiology today for neurosurgery is to try to keep the patients as light as possible. We've learned over the years that the deeper the anesthesia, the greater the complications. Under dissociative anesthesia, the patients appear or often look like they are awake. For example, patients will open their eyes and look around while on the operating table when one is doing a craniotomy to remove a brain tumor or operate on the spine, and they'll move a bit from time to time. It really takes the judgment of an experienced anesthesiologist to know what is just right. Any time an animal or a patient has movement, one has to make a judgment as to whether the animal or patient is experiencing pain. With experience derived mainly from the operating room and transferred into the laboratory, the investigators think they can do that quite well. The reason for having the infiltration anesthesia, however, is to do everything possible to relieve pain. Under some circumstances it can be somewhat difficult to know. As I understand it, from the experience of the laboratory, where I have spent some time but not nearly as much as my colleagues, the single dose of phencyclidine generally suffices.

Professor Francione: Three hours before the injury?

Dr. Langfitt. Yes. Tom and I were reading a volume on phencyclidine in which two distinguished anesthesiologists regarded it to be one of the best general anesthetics ever developed . . .

Professor Francione: That's funny, because the materials that I've been reading say Sernalyn is . . .

Dr. Langfitt: . . . May I finish? As a matter of fact, a single dose is ordinarily all that is needed because it generally lasts for two to four hours, and the acceleration occurs somewhere around two hours after the initial dose. But, as Tom has indicated, when a second dose is needed it is given; in the vast, number of cases it is not needed.

Professor Francione: So then the protocol would be inaccurate, or incomplete.

Dr. Langfitt: Yes; the other point is that one is limited in the amount of space in which to present this material. Proposals are very long, so ordinarily one puts down what the general operating methodology is. Exceptions to all these things are not included, since one could go on with pages of exceptions.

Professor Francione: How could you teach a baboon a memory within an hour of the injury if the animal is on phencyclidine with nitrous oxide? I'm just curious as to how that's done as a practical matter. The protocol says that animals are taught memories within an hour before the injury. Now if you've administered phencyclidine, which is a dissociative — I believe that it is used by some illegally as a recreational drug called PCP or Angel Dust —and if the animal is under the influence of phencyclidine and nitrous oxide in the hour before the experiment, how do you teach behaviors to the animal?

Dr. Gennarelli: I think you're getting yourself a little confused. The studies on memory —let me answer your question so that your confusion can be dispelled. The studies on memory are performed by Dr. Stellar and his colleagues. The animals are trained, as I mentioned, for some period of time, usually two to three months beforehand. They are not trained under anesthesia. They are trained by Dr. Stellar or one of his associates working with the animal on a one-to-one basis at the cage, using the techniques that he has developed to train and ingrain memory and learning behavior into the animals. The morning . . .

Professor Francione: But you said it takes three hours.

Dr. Gennarelli: Would you please allow me to finish the answer?

Professor Francione: You are doing most of the talking.

Dr. Gennarelli: Well, you asked a question . . .

Dr. Gennarelli: The morning of the experiment on which Dr. Stellar wants to ingrain a new memory, that is done prior to the animal's leaving his cage, before he is anesthetized, because that's the way the old memories were ingrained. Some of these are called pattern reversal changes, that is, there's a choice of two objects and the animal has learned that one is always correct and he gets a food reward for it. On the morning of the experiment the investigator goes to the cage, and now what previously was the correct object is now made to be the incorrect object, and the animal has to learn that prior to going down, prior to getting his anesthesia and going down to the laboratory.

Professor Francione: But you say in the protocol that it takes between two and three hours to get the animal prepared for the injury and on page 201 of the protocol, it says within the hour before head injury the baboon is taught several new and therefore, recent, memories. What is the purpose of stopping the nitrous one hour before the injury earlier, as you say, earlier in the protocol? What is the purpose for that, and how do I understand the statement on page 201?

Dr. Gennarelli: Well, let me just make a general comment on protocols. Protocols are guidelines. They are not etched in stone, and they are very mobile. That is, the protocol that you have before you has probably changed, not in large degree, but in specific detail, as we've felt that better ways of . . .

Dr. Cooperman: I'm going to take a privilege of convener in this to talk about protocols because I think we're getting off on an unproductive route. The purpose of a proposal is to tell people something of what you're going to do. It's not at all to tell people in precise detail exactly how each experiment is going to be conducted, because the expectation is that you're going to find things out as you do the experiments which will impinge upon how you do the [next] experiments. I think it's unproductive for you to persist in a line of questioning based on a protocol which may have many inconsistencies when compared with the actual procedure. What bothers me about this is that I thought, and still believe,

that what you saw disturbed you, and I really think it makes sense for you to use the opportunity presented by this dialogue, to tell us what you found disturbing that actually happened rather than focusing on a written protocol, in order to see whether there is a fault in the experiment, which is one possibility, or whether there isn't a fault and it's just something that's disturbing but which has a very clear rationale.

Professor Francione: But, Barry, one of the things which is very disturbing about the tape is that the animals appear to be unanesthetized so I did read the protocol and published research.

Dr. Cooperman: I understand why you did it. I'm not saying that a reasonable person would not act as you have. That's not my point. My point is that I think it's a mistake to believe that what is written in an NIH protocol will be slavishly followed by the investigator. That is not the general practice within the scientific community. The purpose of the proposal is to outline the thrust of the research; the details change all the time. And so, if you have questions about what you saw . . .

Professor Watson: Were we that mistaken in what we saw, that the animals appeared to be unanesthetized?

Dr. Cooperman: Let's talk about the state of the animals before the injury. I think that's what bothered you and that's what we should be talking about.

Professor Francione: I have here the research paper written by Dr. Genneralli called "Diffuse Axonal Injury and Traumatic Coma in Primates." I have now stopped looking at the protocol. The research paper reports that "behavioral, psychological, and neurological observations were performed one hour after nitrous was stopped." Now, is one also not to believe what one reads in the . . .

Dr. Cooperman: No. No. I think the paper is a better . . .

Professor Francione: Could you please explain that to me then?

Dr. Gennarelli: Those behavioral observations were not the trained behavioral observations that you've alluded to before. Those were observations of the animal's behavior, that I mentioned before, that we record in a semi-systematic fashion. Behavioral observations being, "Is the animal awake? Is the animal comatose? Is the animal apparently normal? Is he apparently not normal?" Those are neurological behavioral observations.

Dr. Cooperman: Could you give us an indication of what that would constitute so that we're clear about what you're talking about? What do you actually observe?

Dr. Gennarelli: Well, the general state of the monkey and what he does either spontaneously or by inducement or stimulation is what's observed. That's sort of a fundamental clinical observation that one makes and is one of the . . .

Dr. Cooperman: Is it rate of blinking, is it reflexes, is it that kind of stuff?

Dr. Gennarelli: No, it's a general gestalt that you get when you walk up to a patient and say "Is that patient comatose, or is that a patient who is doing differential calculus? Does he have the capability of doing differential calculus with what I see and what I can observe from that person or that animal?" Now, what I think you're really asking is "Does the anesthesia last sufficiently long to be present in sufficient quantities at the time of the injury." Is that what you're really asking?

Professor Francione: I have questions that concern whether the animal was anesthetized properly before the injury, at the time of the injury, . . .

Dr. Gennarelli: Why don't you ask them one at a time?

Professor Francione: . . . after injury, and at euthenasia when the brains were frozen.

Dr. Gennarelli: Why don't you ask them one at a time so that . . .

Professor Francione: Well I've asked—perhaps this indicates the need for a science adviser—I'm trying desperately to ask you the question concerning whether the nitrous was stopped one hour before the injury, because if the nitrous was stopped one hour before the injury and there had only been a single administration of phencyclidine, at the dosage of one milligram per kilogram on the morning of the experiment two to three hours before the injury was sustained, it seems to me that an argument could be made, and it seems to me from what I've read that the argument would be a strong one, that the animals were not anesthetized. In fact, part of the purpose of the experiment was to perform a neurological examination immediately after the injury with which any trace anesthesia would have interfered.

Dr. Gennarelli: O.K. Now you've got a question that we can answer.

Professor Francione: But I had that before.

Dr. Gennarelli: You have to distinguish between the absence of pain sensation, which is what we were after, and an animal being comatose as a product of a drug. Those are two areas that many times are related areas, but are not necessarily always related. That's what Dr. Langfitt mentioned is what a dissociative anesthesia compound does, what it does. That is, it allows the animal to be, or the human to be, pain-free, that is, incapable of feeling pain sensations, but still does not render the patient or the animal into a coma, which is our common conception of what anesthesia is. The common lay conception is you get a shot or you get a gas and you fall asleep and you don't feel anything. That's deep anesthesia. Disassociative anesthesia has analgesia, which is the absence of pain sensation with the presence of varying amounts of cognitive or motion function. It's a difficult concept, I agree. That's the kind of anesthetic that Sernalyn is. One can monitor the effective effect of Sernalyn in several ways. One can do it behaviorally, that is, with experience with a particular species, one can get an appreciation of whether they're feeling pain or not. That's something that is a clinical judgment, and in our clinical judgment the animals were not feeling pain. They were clearly moving. That was one of the intentions because we wanted an agent that would allow us to perform a neurological examination but still enable the animals to be pain-free.

Professor Francione: So the animals were pain-free at the time of the injury?

Dr. Gennarelli: Yes. Now whether that took one dose of Sernalyn or two doses or four doses was entirely dependent on the animal because there's a lot of variability, as with any drug, in terms of its effectiveness, the duration of its action and so forth . . .

Professor Francione: In this paper . . .

Dr. Gennarelli: Now let me finish the response. I mentioned that one of the ways that one can tell if an animal is having pain is by these observations. There are other ways. One could draw drug levels and relate them to normative data. We did not do that. One of the other things to do is to make observations of the kinds of physiological responses that are characteristic of pain. Those are fairly well detailed in all literature, they are very constant. When a human or an animal feels pain, its heart rate goes up, its blood pressure goes up, its respiratory activity generally gets a little erratic and usually goes up. We've monitored those things in every experiment. We've monitored blood pressure, heart rate, respiratory rate and brain wave activity. And in no instance immediately before the experiment have we noticed alterations of those physiological parameters of the type that would be consistent with pain.

Professor Watson: On the film which has not been shown, one of the researchers says that an animal which is about to have his head bashed, "It's off anesthesia." But you say this didn't occur.

Dr. Gennarelli: You didn't ask—I didn't say that didn't occur . . .

Professor Watson: I thought you said that you never noticed that the animals were suffering pain?

Dr. Gennarelli: That comment specifically refers to the disconnection of the nitrous oxide from the animal prior to the injury. That's what the local term "off-anesthesia" meant. And that's one trouble one gets into by extracting things out of context. That's specifically what that meant, that the animal—when the laboratory workers speak of "off-anesthesia" they are talking about the inhalation agent, the nitrous oxide.

Dr. Cooperman: Gary?

Dr. Cohen: I'd like to get your answer to one of the questions that you raised. Was there any time or is there any reason for you to sacrifice an animal and study nervous tissue in the absence of anesthesia—in the absence of anesthesia within the brain?

Dr. Gennarelli: No.

Dr. Cohen: O.K. because that was your question at the moment as stated.

Professor Francione: Although I now realize that I'm not supposed to rely on the protocol, but the protocol talks about how, in some cases, the animals have their brains frozen by

having their skulls exposed and then liquid nitrogen applied to the exposed skull. The protocol does not mention, anywhere, the use of anesthesia in that procedure.

Dr. Gennarelli: A reasonable person would assume that that's the case. I think that's what Barry alluded to with regard to the NIH protocols. I think that's the—what is done is those animals are re-anesthetized before any sacrifice procedure if they're in any way capable of feeling pain; they are anesthetized.

Professor Francione: Well let me ask you this, Tom. In this paper on diffuse axonal injuries, you say that coma, or unconsciousness, following the injury can last as few as fifteen minutes, is that correct? I'm reading it now. It says "cerebral concussion coma of less than fifteen minutes' duration"?

Dr. Gennarelli: Yes.

Professor Francione: It also says "unconsciousness—coma—was defined as the absence of any eye-opening (spontaneous or in response to noxious stimuli) in the absence of occular injury or occularmotor palsy." If the animals are anesthetized and they are anesthetized before injury, at injury, and after injury, what is the purpose of applying noxious stimuli to an animal that is properly anesthetized with phencyclidine?

Dr. Gennarelli: Well, you can't have it both ways. You can't have us test whether they're feeling pain, and not test whether they're feeling pain at the same time. The purpose . . .

Professor Francione: What are the noxious stimuli that you use?

Dr. Gennarelli The purpose of applying a noxious stimulus, which is a pinch, is to see if they respond to pain.

Dr. Langfitt: This is exactly the same thing that we do in the clinic every day. In other words, a patient comes in with a severe head injury. The Glasgow Coma Scale has three items: verbal response, eye opening, and response to painful stimulation. The neurosurgeon or neurologist . . .

Dr. Gennarelli: Sure.

Dr. Langfitt: [continuing] . . . has always tried to determine whether or not a patient is comatose and also to quantify the level of consciousness by the response of that patient to a pinch. It's the same pinch that we use in the animals.

Dr. Cohen: Well how would you know if the loss is due to the injury or due to the Sernalyn?

Dr. Gennarelli: We may not know, on a single basis, but whether coma or not has been produced by the injury for us, with experience, is relatively easy to determine. That is, the animal is unconscious. There's no question about it. The eyes are closed. They don't open to painful stimulation. The eyes are open under Sernalyn anesthesia. I mean, it's perhaps harder to describe than it is to show you or to appreciate it, but, look at an animal and look at his eyes, examine him, and in your judgment, he's either unconscious or he's not unconscious.

Dr. Cooperman: In the interest of trying to complete this agenda, if there's . . .

Professor Francione: I have hundreds of questions I'd like to . . .

Dr. Cooperman: I'd like to move on. I'd like to move to another area if we can, because I don't know how much more we're going to get out of this one.

Professor Francione: I have many questions on this area, additional questions on this area, as I do on the other areas. I do want to finish up with one thing. Dr. Langfitt mentioned before that he has consulted sources that inform him that Sernalyn is a completely proper and wonderful anesthetic agent. I would like to know, and I sincerely mean this, what sources he's talking about because the ones I've looked at state the following: Sernalyn is a dissociative agent useful "rather for its tranquilizing properties than for its usefulness as anesthetic." (*Veterinary Pharmacology and Therapeutics*' Fourth Edition.) Soma's textbook says that although immobilizing or sub-anesthetic doses are particulary valuable in subhuman primates and other animals which are difficult to handle, the use of phencyclidine as a total anesthetic, "should be questioned because of the tremors and poor muscle relaxation." I note that the nitrous oxide was removed an hour so its—an hour before the injury—so the phencyclidine was the operative agent and, so far as we know from the protocol it's only a single dose. And, finally—and I have other sources as well, I don't want to bore you with that, but, in a recent letter from Dr. Wall, who's director of the cerebral functions group at University College, London, Department of Anatomy, and Editor of the British journal, *Pain*, he stated—in a letter which I have— that "these animals were not anesthetized at the time of the head injury." And he bases that on the published work of Dr. Gennarelli. I have that letter. If anyone wants to see it, he is welcome to see it. I also have other letters from other physicians and scientists who have looked both at the protocol, which I guess they put more credence in than this group does, and in the published papers that Dr. Gennarelli has produced. I would like to go on to the . . .

Dr. Gennarelli: Well, you have to give me a chance to respond. I don't know the information upon which Dr. Wall makes his comments. If he has read either the protocol or the published works with the same sort of legalistic viewpoint that you have, it's not surprising to me that he can make some conclusions like—it's actually still surprising . . .

Professor Francione: He's not a lawyer.

Dr. Gennarelli: I understand. I think you obviously have let a lot of things pass by that I mentioned. So I guess that means that I have to stress them. There are several points in our procedure where anesthesia is critical and certain points where it is not. What we feel is the most important points of the anesthesia is the initial part of the animal handling, for the animal's safety and the humans' safety who are working with the animal, and the second point is for the minor surgical procedures that are performed to insert the monitoring devices. In my mind those are two critical points where anesthesia has to be at its deepest level. The actual injury itself is not painful, and a strong argument can be made, and has been made in the past, to do that portion of the experiment with no anesthesia, purposely. That's what we did at the NIH, in-house at the NIH, in those early experiments. Those experiments were reviewed. The laboratory was visited by one of the directors, and those experiments were approved. The laboratory is much more sophisticated now than it was 15 years ago. That argument could still be made, with extreme validity, should we chose to make it. The fact is, that we've provided an anesthesia, albeit a light anesthesia, but sufficient anesthesia at the time of the injury to preclude the animals from appreciating pain. The fact of the injury is, that it occurs in a hundredth of a second. The coma is instantaneous. At the time of the coma, the patient, the animals, are incapable of feeling pain because of the coma. And simply, the duration of the insult which causes the injury is too short for a pain impulse to be registered by the brain.

Professor Francione: What sort of pain do they feel when they recover from the coma which may be as few as 15 minutes after the injury?.

Dr. Gennarelli: Can we handle one point at a time, please?

Professor Francione: Sure. There are just so many of them.

Dr. Gennarelli: There are other issues involving the anesthesia that I pointed out to Dr. Fussell's question. If you want to address the long term effects, it's fairly characteristic of patients in the recovery phases of brain injury not to complain of pain, to require virtually—virtually no—pain medication. This is almost an absolutely regular part of our clinical experience in taking care of patients as neurosurgeons. Patients recovering from severe brain injury do not complain of pain.

Professor Francione: Tom, how long are these animals permitted to survive after they come out of the coma?

Dr. Gennarelli: That depends on which of the many faculty investigators is performing the experiment at that time, and what his goals are for that experiment. So there's quite a range from instantaneous, virtually instantaneous, termination, that is, within a few minutes, to animals that we have kept purposefully for long-term pathological studies, who we, with great effort, I must admit, with great effort cared for in the severely brain damaged circumstance. One in the case of three weeks, and one in the case of, I believe, seven weeks. That range is from a few minutes, to in two cases, more than a month.

Professor Francione: According to documents purporting to be notes made by you and your researchers, baboon B-47 was injured on five different occasions: July 28, 1983; September 20, 1983; February 3, 1984; twice on April 4, 1984. B-47 was killed approximately one month later on May 2, 1984. I am now reading [my notes] from a document that purports to be a lab note. "B-47 was a trained animal whose injury was a little too severe from him to be testable. Woke up first day, post-injury, unable to sit up, more or less remained in this state over the next month." The protocol itself states that the animals would be kept alive from ten minutes to two months. This goes back as far as 1975, Monkey-A,

INSG #E245/75, Monkey B, INSG #E24775. These animals were injured and tested behaviorally over much longer periods of time than seven weeks.

Dr. Gennarelli: The details of those we can go into. Animals A and B preceded my arrival at the University and were studied early in 1976, I think, or 1975. I don't know the details of those animals, and can't speak for the investigators who performed those experiments. The other animal was under the direct control of one of the other investigators in our group and perhaps it would be more appropriate for him to answer that question, although I'm not sure exactly what your question is. What is the duration of survival you felt was inconsistent with something?

Professor Francione: In the *Almanac* you say that most of the animals are terminated, and that the animals are permitted to survive only for a few hours or days. And then I find out about B-47. I also read the protocol again. I know I'm not supposed to put much credence in that, but it says that 80 of the 250 animals are going to be used for behavioral testing. That's more than just a few. It's certainly more than the Society for Neurosciences reported—fewer than 20 . . .

Dr. Gennarelli: Yes. I'd be happy to discuss the current practices in the laboratory if that's what . . .

Professor Francione: This is 1984 though we're talking about, Tom.

Dr. Gennarelli: Yes. I understand. As Dr. Cooperman has mentioned, protocols are fluid. The experiments will go in different directions. If we say we're going to do one animal that's chronic, it may change, depending on the interest of the investigators in terms of what may be most productive. What I can tell you with regard to chronic animals is that in the last four years there have been 35 chronic animals with the mortality in those animals of seven, that is a twenty-percent mortality rate, and we feel that's pretty darn good considering the virtual 40 to 50 percent mortality in . . .

Dr. Cooperman: The question I think . . .

Dr. Gennarelli: . . . similar injuries in humans.

Dr. Cooperman: I just have to intervene, because I don't know how long everybody can stay and I would like to push the discussion along. I think the point is "Is there a philosophical objection to multiple injury?" That would be one of the points. That is, is there anything in your protocol which would make multiple injury on a single animal beyond the pale of your experiments or do you feel strongly that that's perfectly o.k., an o.k. thing to do?

Professor Francione: Can I take a break for one second?

Dr. Cooperman: Sure. We started a little later than scheduled and so we'll probably go to about ten after, but that's about as much as we can do.

[Professor Francione goes to outer room where Dr. McArdle is.]

Dr. Gennarelli: With regard to the multiply-injured animals, let me make it clear that these are the exceptions in the laboratory, and they're virtually only—there are only multiple injuries under two circumstances. The uncommon circumstance is that there is some failure of the injury apparatus. For some reason it doesn't deliver properly the dose of the trauma and then an animal is not injured at all or barely injured at the time of the insult. That animal is then re-anesthetized and if the problem can be fixed with the injury apparatus, the animal is given a second injury on the same date with the intention of going on with the procedure as it was. The second circumstance of multiple injuries are exclusively the animals with minor injury that are being subjected to the detailed behavioral and learning studies. In no instance have we ever given a second injury to those whose first injury was already severe.

Dr. Cooperman: O.K. In the interest of time I'm going to use the privilege of the convener to go to Section 3D of the agenda which deals with the attitude and comments of the experimenters. I think certainly the attitudes and comments of people handling the animals as, from things I've heard, were deeply disturbing to many people. Along with the issue of anesthetics the other issue that some people who have seen the edited tape have really focused on has been what they have said to be a callous, uncaring, attitude on the part of the investigators as revealed in the tapes which I've not seen, but the transcripts of which I've seen. So I'd like to get to that portion of this discussion if we could, and then we'll go to part four of the agenda very quickly thereafter. quickly thereafter.

Dr. Cohen: Who is present at these experiments? Are senior investigators present?

Dr. Gennarelli: It varies during the time course. The people who are absolutely full-time in the laboratory are a technician, sometimes more than one technician, and a neurosurgical fellow, sometimes more than one neurosurgical fellow. Those fellows are either fully trained neurosurgeons who come to us for research exposure, or they are residents of our own, and generally are in the third, fourth, or sometimes, fifth year of their neurosurgical training. They've already had a considerable amount of training before that. In addition to that, there are occasionally students involved in a particular project in the laboratory. One of the senior investigators is, except for exceptional circumstances, in the laboratory for, usually not for the entirety of the procedure, but for portions of the procedure.

[Professor Francione returns during exchange above.]

Dr. Cooperman: Were there any other points that the people wish to raise on the issue of the attitude of the investigators?

Professor Francione: Yes. I would like to know . . .

Dr. Gennarelli: What, about the attitude of the investigators, is objected, to?

Professor Francione: The excerpted tape has a number of very disturbing scenes. There is one scene where the animal is strapped to the operating table and one of the researchers goes over and plays with his limbs, and the voice narrating—not the overdubbed People for the Ethical Treatment of Animals narration voice supplied by People for the Ethical Treatment of Animals, but what purports to be the researcher's voice—says, "Well, the animal is down, ready for the injury," then the camera pans to an animal strapped in what appears to be a highchair, sort of making peculiar-looking movements, and the voice says "there is B - - -" whatever the number is—let's just say B10. "There's B10. B10 is cheerleading in the corner. As you can see, B10 is still alive. B10 is hoping his counterpart has a good injury." There's a scene where a young woman is holding a baboon that has sort of massive cranial sutures, and they are saying, "Oh here is the baboon on the street, and Sir, what do you think about all of this?" And the baboon falls backward and looks at the young woman, and the experimenter says "He's saying, you're going to rescue me from this aren't you?" And there are just a number of scenes like that, that I don't really think you can characterize as analogous to "gallows humor." I spoke with some physicians, recently, about what "gallows humor" means, and I don't think that you can really link that up here. It's correct, Barry, I think to say that people have been very distressed who have seen it. They have just been awed by the callousness and the lack of regard that those people have for the animals that they acknowledge are suffering—that they explicitly acknowledge are suffering.

Dr. Langfitt: I would have to say that we clearly don't condone behavior like that, but I think it's a mistake to totally disregard the whole issue of so-called "gallows humor" in relationship to these experiments. It's very tense, during the course of these experiments for a wide variety of reasons, not the least of which is that no one enjoys this kind of thing. No one enjoys delivering these injuries to the animals. We're doing it for a very specific purpose. It is quite analogous to what happens in the operating room, particularly under very tense situations. Anybody who ever saw the series M*A*S*H* understands what we mean when we talk about "gallows humor" occurring in the operating room.

Professor Francione: Is it directed at the patient?

Dr. Langfitt: I would state again, that no one condones this kind of behavior. Also, experience suggests that the vast majority of such remarks that might have been made were made by one person who was in the laboratory for a very brief period of time.

Dr. Cooperman: Aron?

Professor Francione: Could I . . .

Dr. Fisher: I can see how this would be disconcerting. It certainly would be to me. But I believe that the bottom line is whether the joking influenced their behavior. I see it as analogous to the difference between prejudice and discrimination, for example. Although we may not like what they said, do you think it influenced the care and the treatment of the animal?

Professor Francione: I most certainly do.

Dr. Fisher: Why do you say that?

Professor Francione: I've not watched that many episodes of M*A*S*H, and I don't want to debate with Tom Langfitt about what happens in episodes of M*A*S*H, but it seems to me, that when we are talking about "gallows humor" generally, what we are talking about is jokes made in the course of performing some sort of, or involving, some sort of stressful situation, performing some sort of operation, or whatnot. I don't recall there being in M*A*S*H comments made directly about the patient, derogatory or mocking comments, made about the patient. But even assuming that this were the case, it would seem to me that to the extent that that's morally justifiable, and I'm not saying it is, I'm saying to the extent it is morally justifiable, some of that justification has to derive from the fact that one is attempting to help that person on the table about whom one is making mocking or joking comments, assuming that that's what "gallows humor" means. However, if you look at the excerpted tape, and I invite you to look at it any time it is convenient for you, I think you will see that this sort of humor on the tape is not telling off-color or obscene jokes in the course of doing this research, it is humor-mocking-scorn directed at the animals.

Dr. Fisher: I won't say it's not, and I don't say that I personally would mock animals. On the other hand, I feel that telling a joke or mocking an animal is not necessarily a punishable offense. I would be more interested in whether they did not give the best possible care to the animals because of their attitude. Knowing what I know about the program and how successful they've been in keeping the animals alive, it seems that maybe they did give the best possible care even though they told animal jokes. I mean we all tell elephant jokes. Maybe we shouldn't do it, and they shouldn't have done it in their situation. But more important to me is the result of the joking and whether it really influenced their actions. I'd like to see the whole tape before making that judgment . . .

Professor Francione: There is one scene where surgery is going on, these mocking comments are being made, and one researcher say to the other "Stop it, it hurts him, for Christ's sake." And they're making jokes about it. Now it certainly seems to me that their attitude most certainly influenced their actions.

Dr. Fisher: It sounds possible that they were trying to be protective of the animal.

Professor Francione: They didn't administer any anesthesia that I could see.

Dr. Gennarelli: I assume they gave it. With all the comments that have been made regarding whether or not these attitudes should be prevalent in the lab, I would submit to you that this is one of the most successful of PETA's propagandas and maneuvers with this tape . . .

Professor Francione: I object to that characterization.

Dr. Gennarelli: . . . I don't care if you do or not, but I'll tell you why . . .

Professor Francione: I know you don't.

Dr. Gennarelli: . . . The reason . . .

Dr. Cooperman: Could we have decorum, please?

Dr. Gennarelli: . . . [The reason] being that, number one, these whatever you were going to call them, attitudes or comments, are attributable by the tape to the entire laboratory, and all of its personnel are being characterized as having made these remarks whereas, in fact, the period of time in the PETA tape extractions comprises a three month period of time out of some nine year history of the laboratory. That's less than three percent of the time and, as Dr. Langfitt mentioned, are attributable in large part to one particular individual . . .

Professor Francione: How do you know that if you haven't seen the tape? I'm just curious. If you haven't seen the tape?

Dr. Gennarelli: . . . I've been provided with excerpts from the tape as well . . .

Professor Francione: But they don't identify who the people are. How would you know what voice belongs to whom from an excerpt? I'm just curious.

Dr. Gennarelli: . . . I'm not going to belabor the point of how I know who is on the tape. The tape has been described to me, I haven't physically seen the tape, it's been described to me in exquisite detail. Those comments are basically from one person during a three-month period of time. During that same three-month period of time, several of the chronic animals were being investigated at the time and that same person to whom these comments are attributed spent virtually every night in that laboratory feeding, caring for the animals that were chronic survivors. They could not eat or drink for themselves. They could not take care of any bodily functions. And the person to whom these comments are attributed did that for a seven-week period in that seven-week survivor. Feeding the animal three or four times a day, with a tube going into his stomach, making sure he didn't aspirate it into his lungs, giving him physical therapy and an incredible amount of care—that's not mentioned in all of these activities, of whether the laboratory personnel are being appropriate. My view is that their verbal commentary is a personal attribute, is not characteristic of the laboratory, and I can document that to you by tapes that weren't stolen from the laboratory. If you would like to characterize the laboratory by comments such as these as being characteristic of the laboratory or the current practice of the laboratory, then you are wrong.

Dr. Cooperman: I'm going to stop this part of the session now. I think we can agree to finish at about 11:15, given the fact that we started a little later than scheduled, and I'd like to turn now to the last full item on the agenda which is . . .

Professor Francione: Can I ask one more question, Barry? You told me that if I had questions to ask that I could ask them. You said this to me the other day when we discussed the format of the dialogue.

Dr. Cooperman. Yes, but not without regard to time, Gary.

Professor Francione: Just one more, please?

Dr. Cooperman: One more question.

Professor Francione: Dr. Genarelli, in any of the published works have you ever revealed how the dental-stone helmets are removed? I'm focusing specifically on the fact that the excerpted tape shows researchers using considerable force to knock off the dental-stone helmet with a hammer and chisel. There is some concern that, given that you are trying to distinguish between contact injuries and acceleration injuries, or trying to distinguish between vascular damage and axonal damage, there is some concern about knocking off a helmet, the dental-stone helmet, with a hammer and chisel. In one case, the baboon's ear appears to come off on the dental-stone, and that's in the excerpted tape. I'm curious as to whether that's ever revealed because I can't find it anywhere in the protocol, and I can't find it in the published works I've looked at, although I would admit to you that I have not read everything that you've ever written. I have read a considerable number of your pieces. I'm curious as to whether the use of the hammer and the chisel is revealed anywhere.

Dr. Gennarelli: No, and it wouldn't ever be revealed because it's not germane to the results of the experiment. It's not common practice in the laboratory, and it's certainly not current practice in the laboratory.

Professor Francione: When did it cease?

Dr. Gennarelli: It ceased when we developed better methods of removing the helmet, and only existed under — I wouldn't say exceptional, but unusual circumstances. That is, there were a certain number of animals whose skin simply stuck to the inner surface of the helmet. That's one problem. And in even fewer animals, the dental-stone stuck to the inner surface of the metallic helmet. What you see as hammering is loosening the junction between the helmet and the dental stone, not between the dental stone and the head. Do you understand the difference?

Professor Francione: Oh, yes sir, I certainly do.

Dr. Langfitt: If I could just make a comment. After having spent ten years of trying to develop this methodology, with the enormous amount of effort and the expense and the personal reputation involved, it is beyond reason to me that anyone would think that we would deliver a very precise injury to the animal's head and then bash the animal's head with a hammer in order to try to get the animal's head out of the dental-stone. That is absolutely beyond comprehension to me.

Professor Francione: I agree. It's certainly surprising, but the excerpted tape speaks for itself.

Dr. Gennarelli: It doesn't speak for itself, Mr. Francione. You've interpreted it in a fashion that you choose to interpret it. The facts are, and we don't have time to go into the technical details, that loosening the dental-stone between the helmet and the dental-stone has no effect on the head. There is no pathological evidence of damage.

There's no physiological evidence of damage. And there's even no theoretical reason that it should cause injury, because of the type of acceleration that that kind of hammer blow, away from the head, would cause to the brain.

Professor Francione: I respectfully disagree.

Dr. Cooperman: O.K. Fine. I'd like now to turn to the last item on our agenda. Our discussion of this item will be abbreviated, which is unfortunate, since in many ways it is one of the more interesting parts and that is, to try and put the experiments that we've been exhaustively discussing into some type of perspective and context. I know that Dr. Edel has been patiently listening and has thought about these issues quite a bit. I would be delighted, sir, if you would begin this part of the discussion.

Some Ethical Questions

Dr. Edel: Very briefly. I'm not sure how relevant this is to our discussion because we've been dealing with only one premise here; that's the premise of no pain and, therefore, everything has come within that. Now, all these . . . [the questions listed on the agenda] are broader problems, but they are important because problems of that sort will specify the kinds of reasons that are given for an ethical judgment, and when you have different reasons, then the scope of the ethical judgment is affected thereby. If you say the reason is no infliction of pain, then you can handle it entirely in terms of: "We don't inflict pain," and then kill the animals and that's all there is to that. But if the reason is that animals have rights, then you've got a quite different picture, and the question of the right to induce death is what's involved. Perhaps all I can say about the several questions is simply to amplify a little bit by reading from the original letter that I wrote you. These are questions that try to put in order various kinds of reasons, and then raise the question of what kinds of consistency you have to expect: what will be proof of what, and how far is it possible to agree on practical treatment from different bases and for different reasons? The answer to that last question, of course, is that it *is* possible. One can have many different reasons for doing the same thing, but the reasons are very important because, quite formally, they'll tell you how far you extend when you go beyond the range.

The question "Who is to count?" is, of course, a very central question since every morality does contain a kind of picture of its congregation, so to speak. And we all know how long it has taken, in the history of mankind, for even the whole of *mankind* to be included within the moral community. The old ways of doing it were, at times, to exclude a stranger who's fair game for anything. And even now, one cannot tell whether the whole of mankind has really been included *practically*, although it has, in our usual verbal responses. Who is to count, then, as to be a member of the moral community? All human beings? All animals? All forms of life? Only the higher animals? Only human beings?

The second question, which wasn't included in the agenda, is "Is the ground for inclusion the independent fact of being a specific form of life (for example, as in the claim that animals, as such, have rights) or the dependent fact of being associated with humans?" For example, pets are not to be experimental animals. Is the ground for not hurting animals the fact that they are capable of pain, or that hurting them intensifies propensity for cruelty among humans? The second, of course, is a much narrower ground, which used to be often invoked. There has been some transition to the first one, as was evident in the discussion today.

"How far is the human use of animals for experiments qualified morally by projected human purposes?" For example, learning about cancers as distinct from testing the safety of cosmetics? One could see the reasonable argument about why you wouldn't want to sacrifice animals for testing of cosmetics. Similarly, with respect to killing of animals for food, or for safeguarding health (exterminating rats) as distinct from hunting for sport—in the history of the ethics there has been plenty of debate about hunting for sport. "How far in a controversy over principles is it sound to use consistency as a criterion for credibility? For example, does one advocating animal rights lose credibility if he or she is not a pacifist or vegetarian?" That is the epistemological problem of trying to decide whether to take seriously or not an argument that's presented. I might say that the most sincere people I've ever met in this regard, in my childhood in Western Canada, were the Doukhobors. When they came to work on the question of preserving potatoes from potato bugs, they simply refused to spray any kind of paris green, which was used at the time, or anything like that; they would just spend hours picking the bugs up and carrying them away. Whatever you could say about them, they were thoroughly consistent—vegetarians and living in peace.

Well, now, I think that's enough to suggest about the comparison of the reasons and ethical judgments. You can't simply reject giving reasons and say "I intuit that this is so, this is wrong," at least in communication with others; you have to give your reasons. And so, in raising the questions, I thought we might find many different *reasons* being employed in our discussion today, and it would be important practically to distinguish which would operate at what particular points, and how far they stretched, and whether broader reasons or narrower ones were being offered.

But, as a matter of fact, this didn't happen in our discussion, which stayed with one simple reason—a minimum of pain, or no pain. Can I just make one more remark? There's the kind of question Professor Francione raised: Should this money be spent, could this money not better be spent, instead of on these experiments, on greater safety devices against crashes, things of that sort? That, of course, is a general question of tremendous interest that concerns the whole of medicine in treating human beings, not simply the question of the research done here. Are we spending too much that will benefit only a few people? Shouldn't we do more for general nutrition instead, and the like? But that's another story.

Dr. Cooperman: Unfortunately, I don't think we're going to be able to discuss these questions at least at this session, although some of the comments you've made are quite provocative. As the University goes forward in consideration of this issue, which I think we have the responsibility to do in many forms, I am sure the points you address will be more fully discussed in other meetings. There is an opportunity for a last word from anyone or any persons who wish to make it.

Dr. Fisher: To your knowledge, Tom, were there instances that went beyond the ethical standards expected by the Animal Care Committee at the Medical School?

Dr. Gennarelli: Not to my knowledge, no.

Dr. Fisher: How extensive is your knowledge of what goes on in the head injury labs? Do you know everything?

Dr. Gennarelli: No, I don't know everything. I direct the laboratory, but there are other faculty investigators involved with it. I can't speak for all of them.

Dr. Fisher: If there had been a serious breach of what you consider humane standards, what would have happened?

Dr. Gennarelli: Well, I'm sure I would have found out about it one way or another if I wasn't directly in the laboratory at one time. If it were something that were, for one reason or another, well, if it's something that could be remedied by speaking to the people working in the laboratory, such as clarification of the depth of anesthesia at certain points in the procedure, now that could be done fairly easily. If it were something that I thought were less clear, then I would discuss it first with Dr. Shalev, the veterinarian, and he would then probably get in touch with the Animal Care Committee if he and I couldn't resolve the problem.

Professor Francione: Aron, would the minutes of the Animal Care Committee relevant to the head injury laboratory be available to other colleagues in this University to look at?

Dr. Fisher: I can't answer that as I'm no longer chairman of the Animal Care Committee. I guess that those minutes are not public information. I don't know what the policy would be with respect to colleagues. I suppose that the present Animal Care Committee would have to take that up. I don't know what the standard University policy is.

Dr. Cooperman: I'm going to, on that note, terminate this discussion in fairness to people who have agreed to come and who are very busy . . .

Professor Francione: Could we have another meeting? I have many more questions about this research.

Dr. Cooperman: . . . Oh, I know you do, and the question of another meeting is on the table as far as I can see.

I want to thank you all for participating, and I hope we've made at least a first step toward understanding some of these issues. Thank you very much.