T-1628-78

IN THE FEDERAL COURT OF CANADA

(TRIAL DIVISION)

BETWEEN:

THE HAMLET OF BAKER LAKE, et al

Plaintiffs

-and-

THE MINISTER OF INDIAN AFFAIRS AND NORTHERN DEVELOPMENT, et al

Defendants

* * *

Held before The Honourable Mr. Justice P. M. Mahoney, at 330 University Avenue, 8th Floor, Toronto, Ontario, June 4th - 8th, 1979, inclusive.

* * *

APPEARANCES:

A.E. Golden, Esq., D. Estrin, Esq.,	for the Plaintiffs.
L.P. Chambers, Esq., D.T. Sgayias, Esq.,	for the Minister of Indian Affairs and Northern Development.
Ms. Price,	for Essex Minerals Company Ltd.
W.C. Graham, Q.C., Esq., R.W. Cosman, Esq.,	for Pan Ocean Oil Limited, Cominco Ltd., and Western Mines Ltd.
T.G. Heintzman, Esq., Ms. Marvyn K. Koenigsberg,	for Urangesellschaft Canada Ltd., and Noranda Exploration Co. Ltd.

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I N D E X

I N D E X O F E X H I B I T S

---Upon resuming at 9:30 a.m., June 6th, 1979

---FRANK MILLER, recalled

BY MR. CHAMBERS:

Q. Mr. Miller, I am showing you a copy of your certified statement. Having regard to paragraph two, sir, you refer to field research on the Kaminuriak population (herd) of barren-ground caribou, and as a result of that research you authored or co-authored quite a number of publications which are listed in Appendix 1. Can you just describe to the court the range of the topics covered by these publications, sir?

A. There is quite a wide range of caribou ecology or biology covered off in these publications. Several papers deal with technical aspects of determining age and sex of the animals based mainly on examination of dental material. Several papers deal with different aspects of the socialization of the population such as group structures, post-calving aggregations, the purposes of such aggregations, and so forth.

Q. In paragraph three of your statement you say that you are familiar with the published scientific literature concerning the Kaminuriak Herd of barren-ground caribou and concerning barren-ground caribou in general. Does this refer to literature other than your own?

A. Yes, it does; the published literature that relates to the Kaminuriak population and to other populations of barren-ground caribou.

Q. Having regard to your field research about which you already testified yesterday, can you give the court more detail of what this field research consisted of, sir?

A. For a three-year period, from March of 1966 through July, 1968, four biologists, one including myself, engaged in the collection of animals on a periodic basis, four times a year. We collected animals in March, April, again in June, then in the November-December period. Excuse me, in the September period, then in November-December.

Those four periods relate to different phases of the annual cycle of the caribou. The spring collection allows us to evaluate how the animals wintered, what physical condition the animals are in at late winter and what their reproductive rate looks like. The June collection during the period of calving allows us to evaluate reproductive success of the animals and, again, physical condition, especially of the parturient or gravid cows. Then, when we look at the animals in September, we are able to evaluate how well they recovered during the summer period: how they put on their fat

reserves and in what condition they are in when they are entering into the winter period. Then, in late November -- November-December, after the rut, and the rut takes place usually in late October/early November -- we are particularly interested in looking at the males at that time because they expend great amounts of energy during the rut. In the pursuit of females they often lose most of their fat reserves and may lose twenty or twenty-five percent or more of their body weight in the course of a month. They are rather vigorous courters. So we get an idea how well they may do in the coming winter and what they are up against in terms of reserves or lack of reserves. Also, we can evaluate the condition of the females and get a good idea of how they are going into the winter: if they are in good shape or poor shape.

Q. Having regard to paragraph four, you say:

> "The Kaminuriak Herd is one of the two caribou herds which frequent the area near Baker Lake, NWT, and are harvested by the residents of that community as well as by residents of Eskimo Point, Chesterfield Inlet, Rankin Inlet, Whalecove, NWT, and northern Manitoba."

Can you tell the court generally, first, about the migration pattern or patterns of the Kaminuriak Herd? Can you tell the court how they migrate and what times of the year, etc? Perhaps you could have regard to the map, Exhibit I-8, and demonstrate it.

THE COURT: This map gives me a magnificent view of Greenland and the Arctic Islands, and that is about all.

Could we move it up so we could get the relevant portions we are dealing with?

MR. CHAMBERS: Yes, My Lord.

THE WITNESS: I'm afraid, My Lord, we are still lacking a good deal of Manitoba.

It really is necessary to describe the overall range first before I deal with the movements.

In the mid 1950s, when the population was estimated at about 150,000 animals, the overall range was estimated to be approximately 180,000 square miles, and that range ran, on the south, from the Manitoba/Ontario border, just below the God's Lake country, westward, to the Manitoba/Saskatchewan border; then, northward, past Reindeer Lake in Manitoba. At that point it angled over -- the boundary angled over into

northeastern Saskatchewan and crossed the Saskatchewan district of Mackenzie in the Northwest Territories border in the area of Snowbird Lake or Selwyn Lake, and then ran a more or less northeasterly course to below Yathkyed, then follows the course of Kazan River to about the Baker Lake area-Chesterfield Inlet.

The winter range was boreal and taiga, of Manitoba, and essentially on the tundra of the District of Keewatin and often returning, on mid-summer migration, into the taiga of Manitoba.

In the 1960s -- the late 1960s, when the population was estimated to have declined to about 63,000 animals, the range then shrunk, accordingly, to about 110,000 square miles. The southern boundary had moved northward to a course that ran westerly along the Churchill River -- just south of Churchill River -- through south Indian Lake to Reindeer Lake and then the boundaries previously described.

Subsequently, in the late 1970s -- 1977 estimate of 44,000 animals were left in the Kaminuriak population. The range essentially shrunk again northward to approximately the District of Keewatin NWT boundary and Manitoba boundary. The caribou essentially left the boreal forests of Manitoba and were wintering

mostly or entirely on the tundra, District of Keewatin.

It should be noted that the population over that time period declined a little better than 70%, and the estimated home range shrank by an equal amount -- 70%. So you have a good indirect evidence relationship for the reduction of the herd and the reduction of the range size.

There was a period in our study where the animals were wintering in northern Manitoba and occasionally in the very corner of northeastern Saskatchewan.

The animals -- in the March-April period, as the day lengthened -- would begin to stage up for their spring migration calving ground summer areas. As the caribou began this staging period in early spring, it was quite an unstable period and the movements tend to be quite variable and there is a great deal of vacillation in movements. It is not uncommon to see several thousand animals streaming northward and at the same time several thousand animals streaming southward right along side each other, just like two lanes of traffic on a highway.

Finally, they seem to get themselves sorted out and the pregnant females move to the vanguard and strike out on a course that will

take them to the calving ground.

There is a great deal of fidelity shown for the calving area and, as mentioned before, it is the focal point of the herd and is usually used to identify the herd or the population.

As the animals came out of Manitoba, they usually did so on three fronts. There was a western movement of animals. They came out through the Henik Lake area and the Yathkyed Lake area on to the calving area. Then there would be a movement out around the east side of Nueltin through the Baralzon country up through Edehon, across the Tha-anne and Thlewiaz Rivers and into the Maguse system. Then they would swim north past Kaminak, Carr and headed into the Kaminuriak area. Usually they continued further then the Kaminuriak -- they swing into the areas just to the north and east, around Banks and MacQuord Lake.

During our study at Brown Lake, some twenty-five miles or so from Chesterfield Inlet, is the northern boundary of the calving ground. Gibson Lake, on the east, Kaminuriak and Parker, on the west.

The third segment of the population -- what we refer to as the coastal segment -- often spends most of the winter or all

of the winter on tundra area along Eskimo Point and Rankin Inlet -- in some years. That segment of the population often moves to the calving ground two or three weeks in advance of the two segments that are wintering in the boreal forests.

Even though they arrive early, they do not go beyond the calving ground. They usually enter into what is a clockwise -- counter-clockwise movement, rather, on the calving ground until the other cows arrive, and then they disperse on the calving ground. That is the one time when caribou appear to space themselves out rather evenly. There is some form of spacial regulation that takes place on the calving ground. But it is not a clear-cut picture. Sometimes you still find groups -- especially of non-breeding animals -- on the calving ground.

Calving usually takes place in the Kaminuriak population during the second week of June. One calf is produced annually. After the ten-day range in calving -- well, you have a five-day peak within this ten-day limit when most of your calves are born. It is a very synchronized calving period.

After the calving period is over, the animals form large post-calving aggregations on what we call post-calving staging

areas. In the case of the Kaminuriak, this is always to the west -- slightly to the west of the calving ground proper. They tend to group and disperse, group and disperse for a couple of weeks, foraging across the countryside. Then, when the finology is advanced so that there is vegetation everywhere, virtually, and the ice is going off the large lakes and the small systems are open water, they start on so-called mid-summer migration.

The mid-summer migration range takes the animals south. The cows, the new-born calves and the juveniles that accompany them to the calving ground start south.

I should back up at this point and say that when the cows come on the calving ground, they both do not come on at the same time. They linger behind and move up slowly so that they actually arrive on the calving ground or just south of the calving area during post-calving period.

In some years, when going is quite rough, you have deep-snow, wet-snow and slush conditions, many of the young animals drop out of the movement and fall back in with the bulls and move up with the bulls. This is governed by hardships they encounter on the way to the calving-ground -- how many young animals will be in the

company of the pregnant cows when they arrive there.

Back in July, the animals meet and merge into large post-calving aggregations. Sex and age composition of those aggregations are quite variable. Some may be top heavy with cows and new-born calves, others are predominantly bull and juvenile groups.

Their movement south, again, is variable from year to year. In some years they penetrate into the taiga of Manitoba, going as far south as Mejanilini Lake, Little Duck Lake, Caribou Lake and so forth, in the northern section of Manitoba.

The cows then return to the coastal areas of Eskimo and Rankin -- further north in some years -- during August.

The bulls usually linger behind in the better vegetated country where the raparian willows are higher around margins of the lake, and they spend several weeks getting fat down there on willow leaves and sedges.

Then there is the pre-rutting period that comes on in September. The bulls move up into the Henik's Lake-Tatinnai Lake area along what could be more or less referred to as the treeline and enter into pre-rutting encounters, where they set up sparring matches and determine

their dominance hierarchy. These bulls all sort themselves out before the rut comes so that they know who is dominant to whom, and they do not have this problem to deal with when the females are receptive.

The females and the young animals then swing inland from the coast, and they move through the general rutting area. The bulls pick up the rutting movements of females. They tend to flank the movements. They come on in movements that form like a horseshoe that form long flanks in the ends of the movements. Some people think that they actually sort of herd the movement along, and that this stimulates or accelerates the females coming into heat.

Anyways, when the females come into heat, the dominant males move in and service the females.

Then, by November the movements are starting southward towards winter ranges. And the rate of movement during November can be quite variable from year to year, depending on the weather conditions that prevail. Supposedly the early snowstorms can trigger a faster movement in some years than others, so their time of arriving in or on the winter range can vary considerably from year to year -- several weeks difference.

Usually by mid-December they are well on to their winter range and settling in.

Again, during the sixties, when we were studying these animals, there was a separation at that point in time when the females and the young of the year and most of the juveniles would remain on more northerly ranges within the forest, and the bulls, and especially the older, prime bulls would penetrate to the south and into the Cochrane River area, mainly, in Manitoba.

During the late seventies, when the population was quite reduced from the sixties, the movements were restricted -- at least in two years -- to mainly the tundra.

There are some animals that are coming into Manitoba last year. My information is that ten-to twelve thousand penetrated the western portions of Manitoba in the Lac Brochet area. But this restriction to the tundra wintering area is, as far as we know, a new phenomenon for this particular population -- the Kaminuriak population.

Q. Mr. Miller, you sat in on the trial from its conception at Baker Lake, did you?

A. Yes, I did.

Q. And you heard all the testimony, particularly that given by the Inuit Plaintiffs?

A. Yes, I did.

Q. You heard the testimony to the effect, by the hunters, that caribou do not cross the Kazan River to the west much any more?

A. Yes.

Q. What is your observation with respect to that?

A. This Kazan River is a particular problem because, as I mentioned, it is the boundary, we believe, of the Kaminuriak and Beverly populations, so it is a peripheral area. And as your population shrinks, you would expect animals not to use peripheral areas of their ranges in the same manner as they did in the past.

There is no doubt that at one time -- in before our work in the mid-sixties -- that caribou did cross the Kazan River and maybe on occasion still do in relatively small numbers. We do not have information on movements in recent years along the Kazan.

I guess this map is worthless for trying to illustrate it. But there has been quite a contention over the years about just what animals are doing what in the area of the Kazan River north of Kazan Falls -- the fifty or sixty

miles of river from the falls to the mouth of the river on the south shore of Baker Lake. There are several possibilities there. Beverly animals, in the past, when their numbers were greater, could have made a movement across the Kazan to the east, and then went around Baker Lake in a circular motion back to the west; or Kaminuriak animals could have made a movement across the Kazan to the west, going around Baker Lake to the north and back across Chesterfield Inlet into the more regular range that they occupy in the central area south of Baker Lake. Probably both did occur when their numbers were high.

However, all the animals that have been shot -- all the tagged animals that have been shot north of Baker Lake and reported to us have all been animals tagged in the Beverly population. We have no evidence of Kaminuriak animals occupying that northern portion -- north of Baker Lake -- from our tagging returns.

Q. If you look at paragraph five, of your statement, in which you give estimates of the size of the Kaminuriak Herd over a period from 1948 to 1977; and they show a decline.

The estimates are as follows:

1948 (Banfield)	120,000
1955 (Lawrey)	149,000

1968 (Parker) 63,173

1974 (NWT Wildlife Service) 49,749

1976 (NWT Wildlife Service) 43,925

1977 (NWT Wildlife Service) 44,095

Would you tell the court, first, what the bracketed names indicate?

A. Well, in the case of Banfield, Lawrey, Parker, these are the authors of the publications or reports -- and in those three cases, I believe, actually carried out the aerial survey.

The NWT Wildlife Service, they identify no particular personnel with the survey.

Q. And these are published figures, are they, on scientific literature concerning the subject?

A. Yes, they are. The NWT figures are probably internal reports only. I am not aware of their release to published information as yet.

Q. How are these figures -- these population estimates -- arrived at? Can you tell me? Do you count every animal? Do you stalk? How do you estimate them?

A. The estimates of population are arrived at by aerial survey. The usual techniques involves flying transits at

given intervals.

If you wanted, for example, to do twenty-five percent coverage, and you had a mile wide transit -- that would be half-a-mile either side of your aircraft -- you would fly at four mile intervals, and then you count the caribou you see on either side of the aircraft as to whether they are inside the transit -- referred to as animals on transit -- and animals beyond the transit are animals off transit. You use only the animals in the transit to come up with a quantitative estimate of the population size. The animals off transit are there for your own information to allow you to get some appreciation of what was in the area.

Q. And how reliable are these estimates regarded by scientists such as yourself in the community at large?

A. Well, they are not bang-on. They have plus and minus confidence intervals that could very considerably -- because these statistics that are applied are influenced by the distribution of the animals along the transit lengths -- would have plus or minus values of say fifteen or twenty-five percent.

Q. If you would turn your attention, sir, to paragraph six, page two of your statement, you give the causes of death of

the Kaminuriak Herd caribou. You give --

1. Harvesting by hunters.
2. Predation by wolves.
3. Other causes, e g. accidents, disease, starvation.

Would you tell the court, first, what the life cycle of a caribou is and the life expectancy?

A. Well, as I mentioned, these caribou in the Kaminuriak population are born early June. If we look, first, at a so-called calf crop, that is the total number of calves born a particular year, if you have twenty thousand, for example, females on the calving ground, our statistics tell us that about seventy percent of those would produce calves. This would give you a calf crop of fourteen thousand animals.

At first that sounds like quite a few animals but these calves are born into a hard world, to say the least, and they die in great numbers within the first few days and weeks of life from various causes such as adverse weather conditions. Cold temperatures associated with precipitation results in high losses and sometimes total loss of calf crop due to pneumonia and other respiratory problems. On some calving ground, such as the Kaminuriak, you have the problem of wolf predation. Wolves can take a good number of

the calves out the first few days or weeks of life. Then there is an array of accidents -- drowning, broken legs and so forth that are associated with newborn calves.

So that by the end of summer-early fall it is not uncommon to have lost fifty to sixty percent of your calf crop. You are down to seven thousand calves, say, in round figures.

You then enter winter period where it has been demonstrated that wolf predation is higher on the calves than other sex and age classes in the population. The calves enter the winter in relatively poor condition, even in the best of years. Because they are growing so rapidly during the first months of life, they don't accumulate the fat reserves the older animals put on, so they have to depend more on being able to obtain forage at all times throughout the winter. The adults can go off forage for a while or be reduced in their forage intake and still do quite well if they have good fat reserves.

So it is not uncommon to lose an additional forty percent or so of your calves over the winter. That leaves you with about four thousand calves by spring, when the recruitment counts are done, and that would be

in line with current recruitment counts for the Kaminuriak population, where you have approximately a ten percent recruitment annually on the average.

Q. In paragraph eight you mentioned this recruitment particularly during the period 1966 to 1977, and you estimated it to be ten percent.

Could you explain to the court, first, what do you mean by recruitment?

A. Excuse me. I'm not really familiar with the procedures here, but you cut me off a little short.

Q. I'm sorry. I didn't mean that. I'm sorry.

A. We were just getting the calves going.

Q. I see. I'm sorry about that.

A. The information from the Kaminuriak population that we have obtained over a three year period -- 1966 to 68 -- allowed us to determine the sex and age composition of the population -- make estimates of it and to work out population dynamics of the Kaminuriak animals.

These so-called population dynamics are done much like insurance actuaries look at the probability of your living or dying,

and how they should charge you for premiums and still make money before you do die. We look at the probability of these animals and generate life tables and look at the probability of these animals living for a given length of time, and the work from the Kaminuriak indicated that at birth, a female caribou has the life expectancy of three to four years; that a male caribou has a life expectancy of only about two to three years. If they live to be three or four years old, their life expectancy then, for females, is approximately another five years. If the male lives to be three years old, his life expectancy then is only judged to be about two years.

When you start thinking about how long a cow has -- a female caribou -- has to stay alive before she produces her first calf, you can see you are entering a situation that is quite sensitive. A cow, for most parts, has to live three years before she is a producer. So, on the average, she only has a chance of making it to breeding age, and if she does make it to the breeding age, then, directly on the average, she has another five years. So, her first calf quite often -- there is good evidence to suggest -- would be a loss. It is quite common in all ungulates, even range cattle, that

the first calf, the female is not quite psychologically adjusted for the birth event and quite often abandons the newborn young.

So, you might be looking at fours years before you have a good prime-producing cow, and she produces calves for the next four or five years. When you look at the rate of loss of calves in the calf crop, on the average, she probably has to produce calves for three or four years, at least, before one of those calves, in turn, will live long enough to replace itself.

So, it is a very slow-going process under the dynamics that we have identified for this particular population.

The males are capable, probably, of breeding from even as yearlings or as two or three year olds, but they do not. There are quite strong social considerations that at that age the cow won't tolerate him and the bulls won't tolerate him, so he is not going to accomplish much. He has to live until he is three or four years of age, and then he has to be in good physical condition at those ages to even have a slight chance. More than likely he has to live beyond five years to enter the truly prime state of his life before he will contribute his genetic material to the population.

So, you can see that there is possibility of not having many prime bulls entering the breeding segment if something beyond the so-called natural mortality level is removing bulls at a high rate from the population.

I should mention that longevity of the species -- estimates of twelve to twenty years by different authors. But in our samples from the Kaminuriak only three percent, I believe it was, of the females live beyond ten years of life. The oldest male in the sample was twelve years old. The oldest female was seventeen. Just one at that age.

Q. As I said earlier, in paragraph eight you speak about an annual average recruitment of calves to the Kaminuriak Herd, and you estimate it to be ten percent. Would you tell the court, first, what you mean by recruitment? Is it the number of calves born to the herd, or what is it?

A. Yes, as I mentioned, the recruitment is measured in the spring of the year. Technically, it is the number of yearlings supposedly living one year and entering the population as an increment -- referred to often as a yearling recruitment.

The surveys done to determine the recruitment are usually carried out when the

animals are actually only ten months old because their distributions and locations lend themselves to segregation at that time. But, simply the number of last year's calves that have lived approximately one year entering the population.

Q. I see. And you estimate this annual recruitment to be about ten percent. Can you tell me how this figure was arrived at?

A. Yes. It is not that we estimated that it should be. It should be higher. That is what we estimated it to be.

Q. I see. This is an actual estimate?

A. Yes. Well, I am not sure what you mean by "actual".

Q. Can you describe to the court how you went about arriving at this estimate?

A. Well, as I said --

THE COURT: I think he did. We have been through this, haven't we?

THE WITNESS: Yes. You take the number of calves and divide it by the number of animals in the population and --

THE COURT: Seventy percent of cows give birth to a calf, then sixty percent of those do not survive the first week or so, and another forty percent of what is left don't make it through the next winter. That's where we

end up with ten percent, as I understand it.

THE WITNESS: Yes, it is just the relation of the number of yearlings to the total animals in the population.

BY MR. CHAMBERS:

Q. Thank you. In paragraph nine of your statement, where you say:

> "During the period 1966 to 1977, the average annual mortality of the Kaminuriak caribou from causes other than harvesting is estimated to be 7.2%. That means that the number of caribou dying of causes other than hunting each year is on the average 7.2% of the existing number of the animals in the herd. Given this average mortality from natural causes, any average rate of harvest of greater than 2.8% will result in a decline in the size of the herd."

Can you tell the court, first, how the 7.2% mortality from causes other than hunting is arrived at?

A. Yes. I should back up a little here and tell you that these figures are based on the work of not just myself but it involves eight biologists who are currently concerned with the welfare of the Kaminuriak and

Beverly populations of barren-ground caribou.

Two of these biologists are from the Northwest Territories, two from Manitoba, one from Saskatchewan, one from the Department of Indian Affairs and two from Canadian Wildlife Service.

We examine all the data available on the Kaminuriak population and generated a computer program that showed us the average declines and overall declines of populations based on the estimates, and then it is a mathematical process of working backwards in determining what percentage of the loss or increase would go into the various categories. That is why the 7.2 is lumped as natural mortality. We do not attempt -- because we do not have the information to -- to separate the individual natural mortalities and, therefore, we just take the difference between the overall decline -- the reported hunter kill and the adjusted hunter kill will come up subsequently -- to get our natural mortality figure.

Q. I see. Then you say:

> "Given this average mortality from natural causes, any average rate of harvest greater than 2.8% will result in a decline in the size of the herd."

Is this 2.8% figure merely

a subtraction of the ten percent?

A. Yes, that is correct. Just ten minus 7.2.

Q. If I can direct you now, sir, to paragraph ten of your statement, you say:

> "During the period 1968 to 1977, the average number of caribou reported killed by hunters was 3,031. That constituted an estimated average annual harvest ranging from 4.8 to 6.9% of the total number of animals in the herd. During the latter part of that period, between 1974 and 1977, the average number of caribou reported killed by hunters was 3,720 animals or 8.4% of the total number of animals in the herd in 1977."

First of all, can you tell us where this figure of 3,031 animals killed and also the 3,720 figure of animals killed comes from?

A. Those figures are from the compilation of the Northwest Territories General Hunter Licence returns.

I probably should mention here that, although we are concerned with Baker Lake from the caribou standpoint, we have to be concerned about the entire range of the caribou

and all of the settlements that are harvesting from that herd. And these figures reflect the reported kill from all those settlements.

Q. So these figures do not only come from Baker Lake but also from other areas -- other communities that hunt this herd?

A. All of the settlements: Chesterfield Inlet, Rankin Inlet, Whalecove, Eskimo Point and the settlement of Lac Brochet, Brochet and Tadouly in Manitoba.

Q. And the percentage figures in paragraph ten of your statement of 4.8 and 8.9% and 8.4%, are they merely calculations of these reported kills to the estimated number of animals?

A. Yes, it would be what the percentage reported kill represents in relation to the number of animals that were estimated to exist at that time.

Q. Do I take it then, for example, the increase from 4.8 to 6.9 percent is based on the declining actual numbers of the herd?

A. Yes, that is correct.

Q. In paragraph eleven of your statement, sir, you say:

> "In my view, this reported kill does not accurately reflect the number of animals actually killed. This is due

to incomplete reporting, eg. not all hunters making reports, not accounting for wounded animals and other animals left on the land. At least twenty percent of the actual kill is unreported. The reported kill should be increased by twenty-five percent in order to reflect the actual kill. Thus, during the period 1968 to 1977, the average number of caribou actually killed was 3,789, constituting an average annual rate of harvest ranging from 6 to 8.6% of the total number of animals in the herd. Similarly, between 1974 to 1977, the average number of caribou actually killed was about 4,650 or 10.5% of the total number of animals in the herd in 1977."

What is the basis for your statement that the kills are incomplete? Is it your personal knowledge? Do you rely on literature for this? What?

A. I feel safe to say it is the general opinion of virtually all the people involved in the wildlife profession that are working with caribou not only in Canada but virtually around the world that the report of kills by native hunters are lacking. As indicated, you don't

get a total accounting and there is no way of estimating or no mechanism for really reporting the number of animals that are crippled and left on the land or animals that maybe cached and left on the land and subsequently not reported.

Our estimates here are very conservative, to say the least. There are many places in the scientific literature where you can find estimates exceeding fifty percent. In fact, several made in Canada say that fifty percent -- anywhere from twenty-five to fifty percent should be added on to the reported kill to approximate the true kill.

Q. Mr. Miller, you have personal experience with regard to wounded animals that you mention in this paragraph?

A. No, do you mean --

Q. That may have resulted in the deaths. In other words, wounded by hunters and may have resulted in death subsequently.

A. No direct evidence. We have been in the field when the Inuit were hunting on the Maguse River system, in the fall of the year, in 1967. They are either very poor shots or they crippled some animals. That is just opinion, I guess. When you hear fifty to one hundred shots or more in different areas and they only have one or two caribou at best, those would

be the conclusions that would be drawn by any other hunter, I believe.

Q. What are the chances of survival for wounded animals?

A. It would depend on the wound, but more than likely any animal that is affected in a manner that complicates his locomotion -- slows him down -- or just makes his movements look peculiar, becomes prime target for wolves. The wolves are always in their company, so I would think any animal limping usually becomes prey for a wolf in short order.

I should back up -- or continue here, I guess. We did look at animals, in the fall of 1970. We had a report through the NWT that the Inuit at Eskimo Point were concerned about caribou that appeared to have some sickness and were limping around on the tundra around Eskimo Point. I took a veterinarian pathologist, and we went out and attempted to locate these possibly diseased animals.

We were able to locate five of them. One had just been pulled down by a wolf -- a large male -- as we arrived there. We chased the wolf off his catch. All five animals we examined were suffering from what you could call lead poisoning, I guess. Small calibre bullet holes in the legs. So I guess, there is some crippling.

Q. And you also mentioned in this paragraph eleven animals that are left on the land. Do you mean killed animals?

A. Yes, again, if one went to the literature, one could find many documentations of animals cached and not subsequently retrieved. I myself have seen cached animals along the east shore of the Kazan River in July of the year, when the meat had already gone bad. I forget the exact number. I would think it is three or four caches involved; approximately thirty animals. One cache I do remember had eight animals in it, and only the hind quarters gone from the one. The meat had gone putrid.

It is common knowledge among the biologists that meat is often cached in late winter and, subsequently, as the hunters go out for -- probably go out to retrieve the meat -- they encounter live caribou between them and the cache, so it is always better to have fresh meat. So they kill caribou.

If they keep that process up long enough, the ice goes out in the spring and they cannot retrieve the caches.

Q. Having regard to paragraph eleven and the twenty-five percent increase that you said should be attributed to it, do I take it, then, that the figure of 4,650 in the second-last line

from the bottom of the paragraph is a grossed-up figure?

A. Yes, it is just the adjusted reported kill by twenty-five percent.

Q. And the 10.5%, sir, is a percentage of what?

A. It would be a percentage of the animals estimated to be in the population at that time -- what the figures 4,650 represents related to the total animals in the population at that time.

Q. You say when it is so grossed-up, an actual kill of 4,650 animals in 1977 constitute 10.5% of the total number of animals in the herd?

A. Yes.

Q. In paragraph twelve of your statement, sir, you say:

> "Throughout the period 1968 to 1977, the annual rate of harvest has exceeded 2.8% and has indeed approached the annual rate of recruitment. I am of the view that over-harvesting by native hunters has caused the decline in the size of the Kaminuriak Herd."

Can you elaborate on this a bit more? Can you arrive at a conclusion as to what

the total true mortality, as nearly as it may be estimated, in 1977 was in percentage figures -- from all sources?

A. Well, My Lord, I realize this is a sensitive area and many people will look at it in a socio-political manner. But our considerations here are strictly biological. We are dealing with numbers. We are only interested or primarily interested in the welfare of the population. That is our charge. That is our responsibility as biologists in the federal government, to look after the welfare of the renewable resources of this country. So, I am just trying to say this as a preamble so it is not anything we have to say about hunting by the Inuit or Dene or any other native group or white. It is not taken in a socio-political manner rather than pure biological sense.

The generated model that occurs from the computer allows us to examine only averages, annual averages. We cannot account for the actual pattern that developed on an annual basis -- the variations in that pattern.

The overall program tells us that on the average the population was declining by 3.9% a year. Our surveys are numerous enough so that the trend is well developed, and for the decline of the population it is very evident

and we have no reason, from a biological quantitative standpoint to doubt our figures.

It is a simple number game. If recruitment averaged ten percent, the natural mortality is 7.2; that leaves you 2.8% available for other use -- to Inuit hunters, to all hunters. If they were not hunting, the population would have theoretically increased by 2.8% a year on the average over that five year period. However, when even the reported kills far exceeds 2.8% and the adjusted kill exceeds it by that much more, so that there is only one conclusion to draw from it: the hunting is -- it is not a sense of over-hunting in terms of needs, it is the sense of over-hunting on the base stock available to them. It is a losing proposition. The population has to continue to decline, and our projected models, which are subject to considerable adjustment, and are based on the assumptions that factors will remain the same -- the rate of kill will not change that much because there is no reason to believe it will, the mortality factors will remain more or less constant -- the population will be nearly extinguished entering total destruction within the next few years and by ten to twelve - fifteen years from now, if our figures are anywhere near right, the population is essentially gone.

Q. Mr. Miller, what is the Caribou Technical Committee?

A. Well, the Technical Committee for Caribou preservation was established some time in the mid fifties, when the first so-called caribou crisis was announced. The work done in the fifties indicates that the problem existed then that hunting was removing too many animals from the various populations, that the breeding stock -- the total standing stock in the populations no longer existed and they had declined to a point where they could not support the level of hunting that they were experiencing. So this committee was formed.

I am not all that clear exactly just how it was put together but it involved all the biologists that were at that time concerned about the barren-ground caribou in Canada -- federal, provincial, territorial biologists and administrators and managers.

There was an Administrative Committee counterpart of it. The Technical Committee was for the biologists to discuss and report their findings, to make recommendations to -- I believe it was called the Administrative Committee. I am not sure of the exact title. And it would, in turn, carry these recommendations, if they saw fit, to their superiors. This is how

most of the programs over the years were initiated through these technical meetings.

Q. This Caribou Technical Committee exists today, sir?

A. Yes. It was temporarily stopped a few years ago, and then re-organized with a slightly different membership -- predominantly territorial and provincial people -- and then CWS became a member again. It currently exists much in the format it existed before.

Q. And has this committee recently considered this problem of the decline of the Kaminuriak Herd?

A. Yes, as I mentioned, the eight biologists involved in this work that I am essentially reporting on here -- five of them, I believe, are members of the committee. They represent the committee. The committee used to have a broad membership. Everybody that was working on caribou was more or less automatically a member. Now, they have sort of select representation from different agencies, so other biologists working on caribou are just considered participants now. But all of the information compiled recently has been examined by the eight biologists mentioned, and with general consensus.

Q. Are you one of them?

A. Yes.

establish this decline. Or, are you aware of any such facts?"

And it goes on to the next page. On page 19 we get an answer at line 9. It says:

"A. My understanding of the situation is that for the Kaminuriak Herd, biologists believe that the decline is due to the natural killing of the caribou by wolves and the hunting pressure that the herd is under.

Q. And the biologists you refer to, are these biologists within the employ of the Government of Canada?

A. (Mr. Hornal): Many of them work for the Government of the Northwest Territories. I'm not too sure of the legal definition of ... I would say that they were not under the employee of the Government of Canada.

Q. Well, there has been, at least, in 1978, a contractual arrangement as between these two Governments so that employees of the Government of the Northwest Territories on behalf of the Government of Canada would study the situation. Is that correct?

A. (Mr. Hornal): That is correct."

And I am not sure what the next sentence is: whether it is a question or an answer. It appears in the transcript to be a question. I am not sure if it is or not, in any event, the next question reads:

"Q. Well are there studies or memorandum related to setting out these opinions?

A. (Mr. Hornal): Yes.

Q. Well, I would like to have those studies or memorandums produced.

MR. SGAYIAS: Just for the Record so that I have my undertaking clear: what opinions are you speaking of?

MR. ESTRIN: I am asking for, aside from the surveys that we have already set out, any studied by scientists of the Kaminuriak and I also have some questions in relation to the Beverly Herd now, of which the Government of Canada has knowledge and which supports the conclusions that Mr. Hornal has stated.

MR. SGAYIAS: Yes. We will undertake to do that insofar as that information, to inquire whether that information is available within the Department of Indian Affairs and Northern Development. And if

it is available, we will produce it.

MR. ESTRIN: All right. I think my position is clear that I am asking you to go beyond that in terms of the Government of the Northwest Territories.

MR. SGAYIAS: Yes. Insofar as it goes beyond information within the knowledge of the named Defendants I will take that under advisement and advise you whether we are willing to produce that material."

Subsequently, there were -- Mr. Sgayias drew up what he said were the undertakings that he gave us and undertaking number four was, and he summarized it this way:

"Produce any studies which support conclusions that the decline of the Kaminuriak Herd due to killing by wolves and hunting pressure.

And his answer is see attachments 4A and 4B. Attachment 4A is a study by Parker published in 1972. Attachment 4B is a study by Miller and Broughton, published in 1974. And he lent us copies of those. That was the totality of his response.

My Lord, there were some discussions when we had a motion before you, prior to trial, about a concern I had about studies that could be made available that were in possession

of departments of this government other than the named Defendants. I do not have, My Lord, a copy of the Order in front of me but I do believe that at the very least you urged the government to make available to us any such studies that at least, I think, they intended to rely at this trial in regard to this matter.

I note that the documents which my friend intends to show this witness are dated May 1979, so,obviously, one of the responses is that they were not available at that time.

Well, My Lord, I think I would like to refer to the rules because I do not know that it is necessary to be that technical but, because just on the basis of what has gone on heretofore, if these documents were available in May and my friend intended to use them at trial, surely he should have given them to us in a proper way at that time; but he did not.

My copy of the rules seems to have escaped me for the moment. I think Mr. Golden is going to get them. I am referring, first of all, to Rule 447(2) there is nothing in this Defendant's list of documents. Rule 461 specifically says that if newly discovered documents are going to come to the attention within the meaning of that rule, there ought to be -- as Mr. Sgayias urged on us the other day --

a new list of documents and an opportunity given for a discovery.

I note that in reference to Rule 461 that there is a case that appears to be annotated there that would be relevant. I am referring to National Capital Commission versus LaPointe. It says that where documents that ought to be produced by a party at trial has been in the possession of the party prior to the time an expert's Affidavit was filed or before, it should have been in the list of documents. If the document was recently acquired, the party should seek leave to add the document to its list under Rule 461, otherwise the court would not allow the document to be introduced at trial as it would be detrimental to the other party.

The other case is Moore, of course, makes clear that if there is a new document, we would be entitled to further discovery on additional documents.

I don't think I am entitled to object to my friend asking this witness to state conclusions arrived at by this committee, if he was a member of that committee. But, I do believe it is not at all proper for any documents related to this committee, whatever it may be, to be put in as exhibits. So my objection is to my friend in any way attempting

to have this witness read from these documents something into the record. He can ask the witness. I think I would be content with that. But I certainly would object to the documents per se being made exhibits.

MR. CHAMBERS: My Lord, we were not aware of these two documents until the day before yesterday. They were shown to me by another member of the committee, who will be our next witness, Dr. Calef. I was certainly not aware, nor was Mr. Sgayias aware, at the time of the list of documents was made and, of course, Rule 477 (2) says, "A list of the documents of which he has knowledge at that time that might be used in evidence."

Certainly at the time of discovery we did not know of these documents. Subsequently, when it came to fulfilling the undertakings given, we were not aware of these documents. As I said, we only became aware of them the day before yesterday. In any event, My Lord, all this evidence -- well, part of this evidence and certainly that relating to the cause or probable cause of a decline of the Kaminuriak Herd that is being offered to this witness is strictly in rebuttal to the evidence proffered by the Plaintiffs that the cause and decline -- first of all, there is no

cause of and there is no decline of the Kaminuriak Herd, as far as I understood. They were just chased away. Secondly, that the cause of such decline was mining activity. So, therefore, this evidence, first of all, My Lord, there is a genuine decline, and we have not come to the second part yet, the likely displacement of the herd by mining activities. This is offered strictly in rebuttal.

So, therefore, in such a situation, when a party is faced with a rebuttal of evidence that comes up at trial, it is certainly entitled to muster any such evidence -- documentary or otherwise -- as he may be able to muster. We have made every effort to produce these documents to my learned friends -- I got it early yesterday morning -- at the earliest time at which I could.

Thirdly, I intend to show these two documents to the witness and ask him whether the conclusions stated therein are also his conclusions and that of the other members of the committee. So, it is surely a legitimate question put to the witness: whether he agrees with these conclusions and also are they part of his own conclusions.

For these reasons, I respectfully submit that I should be able to

show these documents to the witness and ask him questions about them.

Thank you.

THE COURT: My Estrin?

MR. ESTRIN: Very briefly, My Lord. There is one other factual matter that I think perhaps I and perhaps the court has been misled on. I note that in regard to each of these documents the so-called Caribou Technical Committee there is an F. McFarland who is listed as with the Department of Indian and Northern Affairs.

THE COURT: Yes.

MR. ESTRIN: Insofar as there is any suggestion that this is not a document that was not within the power of that Department, we should have had it.

THE COURT: Yes, but, Mr. Estrin, it came into existence in May. This is the 6th of June.

MR. ESTRIN: Right. I appreciate that. I don't know when in May.

THE COURT: Neither do I.

MR. ESTRIN: In any event, I can hardly think that it would be plausible --

THE COURT: Would you deal with the point that this is tendered in rebuttal?

MR. ESTRIN: Yes, I was just going to deal with that.

I can hardly think that that could be a tenable argument in that back in December, at the very latest, this was raised during the beginning of the discovery. It was obvious that we considered this to be an essential concern that was going to be raised and, having asked about it, having received an undertaking about it, I do not know how it behooves my friend to say that he could not have anticipated that this is going to be an issue in the case. It was reasonably anticipatible just by having regard to the matters being discussed at the discovery.

I believe, My Lord, that the whole thrust of Mr. Miller's Affidavit is directed at this point, and he is not essentially called as rebuttal witness on this point.

With regard to my friend saying, "Well, I only intend to ask him about the conclusions," Mr. Miller is a member of the committee, and I think he has already answered the question. My friend asked what are the conclusions of that Caribou Technical Committee. The witness just said, a moment ago, "They are the same as mine." These are the committee's conclusions. I think he has already got the answer. And I believe the introduction of the documents per se would be highly prejudicial.

THE COURT: I cannot, for the

life of me, see how this document, if it did not come into existence until some time in May, could have been produced to you at the time production of documents was made. I accept Mr. Chambers' assurance that it only came to his attention the day before yesterday and apparently was made known to you yesterday. I see nothing reprehensible in his conduct in making it available as soon as it came to his knowledge.

Frankly, I think this is rebuttal evidence we are getting now. On that point there can be no legitimate objection to the document, if it is otherwise proper to put it in, going in.

Go ahead, Mr. Chambers.

MR. CHAMBERS: Thank you, My Lord.

BY MR. CHAMBERS:

Q. Mr. Miller, I am showing you two documents --

MR. ESTRIN: May I see which one?

BY MR. CHAMBERS:

Q. Two documents that are entitled, first -- the first one is entitled "The Decline of The Kaminuriak Herd -- May, 1979." The second document is entitled "Management Options -- Kaminuriak Caribou Herd -- May, 1979." And at

the end of these two documents there are certain names listed: page four of the Management Options document and on page three of the Decline of the Kaminuriak Caribou Herd document. In both I see your name "F. Miller" listed, is that correct?

A. Yes, that is correct.

Q. And there are all these names, of which your name is part, headed by "Caribou Technical Committee," correct?

A. Yes.

Q. All these names listed on this page I just mentioned are headed by the words "Caribou Technical Committee," are they not?

A. Yes, they are.

Q. Are the members listed therein, including yourself, members of that committee?

A. I am not a member. I am a participant. Our service is represented by Dr. Thomas.

Q. Having regard to the contents of these two documents, did you participate in their formulation?

A. Yes, I did.

Q. And do you agree with the contents stated therein?

A. I do, if they say what we agree to in the reading.

MR. GOLDEN: Perhaps you better read them and find out.

MR. CHAMBERS: Yes, would you please read them.

THE COURT: Perhaps we can take our usual ten minute recess at this point.

---SHORT RECESS

---Upon resuming after recess

BY MR. CHAMBES:

Q. Mr. Miller, have you had a chance during recess to read these two documents I have shown you?

A. Yes, I have.

Q. Do you agree with the conclusions stated therein?

A. Yes, I do. There are a couple of numbers that vary -- one or two -- from the numbers I have stated, like 3718 vs 3720. But --

MR. CHAMBERS: I see. My Lord, may I tender these two documents as exhibits?

EXHIBIT NO. D-10: Document: The Decline of The Kaminuriak Caribou Herd - May, 1979.

EXHIBIT NO. D-11: Document: Management Options -- Kaminuriak Caribou Herd -- May, 1979.

BY MR. CHAMBERS:

Q. To come back, briefly, to the causes of the decline of the Kaminuriak Herd, one of which is, of course, natural mortality and there are two other causes you have stated and we have received in your evidence already; namely, accidents and hunting. This Exhibit D-10 and also D-11, am I right, has great refinements -- greater breakdowns in the causes responsible for the decline of the caribou herd?

A. No, not realy. They may list diseases, accidents, predation. But wolf predation and all of the other so-called natural causes are all lumped under natural mortality and are part of that 7.2%.

Q. I see. You also said that the Kaminuriak Herd was declining in numbers. These exhibits -- D-10 and D-11 -- state under varying conditions what the rate of decline is likely to be in the future, is that correct?

A. Yes, that is correct.

Q. Particularly I draw to you attention Exhibit D-11, page two, under the heading "Management Options".

A. Is that the document entitled The Decline of the Kaminuriak Herd?

Q. No, "Management Options,"

sir.

A. Yes.

Q. You will see on page two, starting on the bottom, and page three, some management options listed. The first is:

1) No human harvest + wolf control

Herd will reach 100,000 in approximately 13 years.

2) Harvest of 1,000 bull caribou + wolf control

Herd will reach 100,000 in approximately 15 years.

3) Harvest of 1,000 caribou, any sex + wolf control

Herd will reach 100,000 in approximately 17 years.

4) No human harvest + no wolf control

Herd will reach 100,000 in approximately 38 years.

5) Harvest of 1,000 caribou, equal sex ratio + no wolf control

Herd will maintain itself at its present level of 37,000.

6) Harvest of 4,650 caribou, equal sex ratio + wolf control

Herd will decline to extinction within 15 years.

7) Harvest of 4,650 caribou, equal sex ratio + no wolf control

Herd will decline to extinction within 9 years.

8) Harvest of 3,000 bull caribou + no wolf control

Herd doubles and bulls are all killed within 10 years.

THE COURT: That would seem to present some biological problems. Wouldn't it? Option 8. Unless some miraculous process is developed.

THE WITNESS: It is a mathematical consideration, Your Lordship. There is a period of growth, and then, when you lose your males, no growth. I am not sure of the exact doubling.

THE COURT: There would be some very busy bulls in the last couple of years.

THE WITNESS: That would probably help them on their way.

BY MR. CHAMBERS:

Q. In any event, this number 7, "Harvest of 4,650 caribou, equal sex ratio + no wolf control, herd will decline to extinction within 9 years."

Is this the condition that's prevails at the moment?

A. Basically, yes. It would be considered the condition that we have currently or based on the 1977 information.

Q. So, if nothing is done,

at all, there will be no Kaminuriak Herd in 9 years; is that right?

A. That is on the assumption that the parameters remain the same. The herd is going out. There is no doubt in their minds about that. But whether it goes out in 9 or 15 years is subject to the variations in the natural scheme of things, so to speak.

Q. Sir, I would like to ask you a few questions now concerning the topic that has occupied a lot of time in this court already; mainly, the question of caribou behaviour. All right. Can you tell the court, first, how caribou react to aircraft?

A. The reaction to aircraft would be quite variable, to say the least. You can run the gambit from extreme responses such as galloping or trotting off or to no visual or observable overt response to the overflight. This is usually related to the distance the aircraft is above and diagonally from the animals that are being overflown.

There are many other variables that come into consideration: the size of the group, sex and age composition of the group, the number of calves present in the group, the position of the sun relative to the aircraft flight, the direction of wind relative to aircraft flight,

the previous experience of the animals, the type of terrain they are on when overflown and probably many that we have not accounted for yet that do have some influence on the animals' response. But the best relationship or strongest relationship is usually the inverse relationship of distance of aircraft from the animals.

Q. You mentioned distance from the ground. Would it also depend on the type of aircraft used?

A. Naturally you would not expect animals to respond the same to a 737 at 500 feet as to a Cessna 185. There is a visual image to take into consideration as well as the sound associated with the stimulus and possibly even smells. You would have to test the animals to all these different types of aircraft if you wanted to quantitatively determine the variance in response by aircraft type.

Q. You heard the evidence to the effect that some helicopters fly at 150 feet at about 50 miles an hour. Assume further that this overflight this helicopter takes over the herd of caribou, how would the caribou likely respond?

A. Again, there would be great variation in response by certain individuals. You would expect females with young to respond more,

on the average, than adult males. Juveniles tend to have even more mixed reaction.

In most cases, at a hundred and fifty feet there should be an initial response. It would not be surprising to see adult males not responding overtly on occasion. The responses would come again, run the gambit, from trotting, to galloping, to standing alert -- taking up alert positions indicating that they are aware of a foreign stimulus in their environment but not actually responding by displacement -- or just remaining foraging or bedded.

Q. Say, after the overflight has ceased what would be the likely reaction of the animals that had been galloping as a result of this stimulus?

A. Assuming the aircraft was on a course that took them away from the animals after overflown and that the overflight, within a radius of a mile or so of the animals, would just involve a few seconds of exposure, I would assume that -- from my own observations-- that within seconds or minutes after that the animals would return to their maintenance activities: returning to foraging or possibly even bed down. If you have no reinforcement of the stimulus, there should be no continuation of the response.

Q. Is there any evidence that

you know of, Mr. Miller, of any ill effects that such animals -- overflown animals under the conditions that I have cited to you -- may have suffered: either death, or disease or whatever?

A. Personal experience or in literature? Everything?

Q. Both?

A. Everything. For caribou or for any other ungulate, literature has potential stresses for the impact of different forms of harassment on the animals. And, as you have already heard, potentially there is the danger there to run the gambit from just having an impact on their energy intake through different forms of displacement to reproductive problems or death. These are all possible, but all subject to really extreme conditions. When you look at the literature that is associated with those types of problems you will find that the harassment almost always, if not always, involves both prolonged restraint or prolonged pursuit. It is not the type of condition that you would get from a single overflight of an aircraft without any reinforcement.

Q. Is there any evidence, Mr. Miller, of displacement of caribou from their migration routes as a result of helicopter or aircraft overflights?

A. From their migration routes?

Q. Yes.

A. I can't think of any evidence that would suggest or indicate that caribou were actually caused to alter their migration in the sense that if they are going from A to B that they would not end up at B. It is reasonable to assume that disturbance from an aircraft would cause a deflection in the actual movement across the terrain they are traversing. It may cause them to deviate feet, yards, or in some cases, maybe a mile to one side or another. But, these animals have strong affinities for where they are going when in migration. They have thousands of years behind them in traditions. They are not about to be turned.

If they were turned by a single overflight of an aircraft, they would be turned by virtually everything that occurs naturally in their environment.

Q. Could you exemplify?

A. Well -- there is reason to believe that unless the stimulus, the so-called harassing stimulus, is reinforced or of great duration that it will have no more or even less impact on the animals in general than the presence of a gull or a jaeger or the snapping of a bush as it

comes free from the frozen ground, the cracking of ice on a lake, the grounding of ice in river stream. All of these things present sounds and sights to the caribou that, if presented suddenly can be classified as harassing stimuli.

The animal has, as we say, in its genetic material -- in its so-called genetic plasticity -- he would not have existed as a species if a single-event-type harassment left a lasting impact on him. These animals have been competing from day one in their existence to survive in an environment where stress is a natural part of their environment. They are constantly tested and harassed by wolves. They are harassed, if you want to call it harassment, by encounters with Arctic fox, Arctic hare. I have seen a nesting jaeger make caribou walk two hundred yards out of their way to cross a stream because the bird obviously wanted to keep them off a nest site. I have seen caribou run miles across the tundra when gulls have hovered over them. None of these things are going to lead to the caribou's death or cause him to abandon his range -- unless, it might lead to his death if he is already 90% dead from malnutrition and all he needs is an extra push. But these animals are normally not living that close to the line.

These animals have evolved in a tundra and boreal forest situation. They are animals that are quite capable of surviving there if they do not have outside pressures that are of extreme nature. I mean there is a point when you can exert too much of anything on him that will lead to complications and to serious problems. at a population level and certainly at an individual level.

Q. We heard evidence of grid patterns being flown by helicopters at a hundred and fifty feet, fifty miles an hour, for a quarter or half a mile apart or so. Assume this grid-pattern-flying were done over a caribou herd. Would that amount to a reinforcement of stimulus that might have detrimental effect?

A. It is hard for me to preceive how this would occur more than very infrequently, if at all.

You have to look at the size of the range that these animals are occupying, and then the relative size of the area that is being surveyed by aircraft. When you look at the distribution of the animals, say, during the post-calving summering period when this type of exploration is apparently taking place, you have a situation where throughout the session in the court people use the word "herd"

very freely. I often get the impression that many people believe when they hear that a herd did not cross here or that a herd did not go there, they think that is the whole population -- one ball just going down there, forty thousand strong and making the crossing. That is not true at all. These animals occur in various size post-calving aggregations that number from maybe 50, 100, a few hundred animals to several thousand animals per group.

I believe, in 1968, when Parker did his last post-calving work, he had 32 different post-calving groups within the Kaminuriak population. The largest one was just a bit over five thousand animals. So if you look at thousands of square miles of range available to these animals, plot their probable distribution and relate to the activity that has taken place in the past -- and I am not talking about what may come in the future but let us take the past -- there is no reason to believe that the encounter should have been great.

If there is an encounter, there is going to be a reaction. At a hundred and fifty feet, as you said, that action should be shortlived unless that action is reinforced.

Q. Bearing in mind this grid-pattern-flying and supposing you were attached

to an exploration -- or people that do these exploration things -- as a resident biologist or wildlife expert or whatever, what advice would you give the company in order to avoid any possible ill-effects of such overflights?

A. Basically, I would advise them not to overfly the animals if they have a choice. I do not really know that much about the timing of their schedules, but I cannot help but believe that if they had someone that was familiar with the caribou, with the behaviour of the animals, that they could not simply determine by high flight of some small aircraft -- you are looking at, say, a Cessna flying at a thousand to two thousand feet -- can determine the general distribution of animals without any problems.

My understanding is that these people have more than one area to work. It is not at though they have to do this one small square and if animals are on it they have to go to this area regardless if they are there or not.

I may be wrong in this, but I assume that they have different areas that they could fly. If they were flying ten miles or twenty miles away from the animals, it should have no impact whatsoever on the animals. So it

seems as though it would be relatively simple for them to determine the presence or absence of animals in a particular block they survey and then move to a different area.

How that fits in to an operational scheme for the companies, I don't know.

Q. Now, sir, were you, as part of your field work, engaged in tagging animals?

A. Yes, I was.

Q. Where?

A. In northern Manitoba, at a place called Little Duck Lake, and on the Thelon River in the NWT.

Q. Would you describe to the court the process of tagging: what takes place?

A. Well, tagging takes place on these water crossings where we use usually a twenty-two foot freighter canoe with a twenty horsepower motor. You simply wait until your animals are in the water, moving across -- mid-stream, say -- and you over take them by canoe. You use a shepherd's crook and simply slip the crook around the animal's neck and just hold him in against the side of the canoe. One man holds the animal against the side of the canoe, the second man places the ear tag in the ear and

quite often the motor man sexes the animal to verify the sex of the animal. Then the animal is released. It only takes a matter of seconds. You can continue this process until the animals are close to the other shore and then you pull off them and they go ashore.

Q. What is the purpose of tagging?

A. Well, mainly to obtain information on the distribution of the animals at different seasonsof the year especially so that we can get a handle on the discreteness of certain populations -- or, if they are in fact populations and how discrete they are. It is a long-term process. The real benefits are derived from the program being carried out over a ten-twenty year period. We seldom have the luxury of continuing. In fact, we have never had that luxury for a continuous time.

THE COURT: Obviously the word "discrete" has some technical meaning when you use it.

THE WITNESS: It is not with two e's.

"Discrete" in the sense that they are separate. In this case, the Kaminuriak of the Beverly population are referred to as herds. We made the distinctions of calling

them populations because we believe that they were separate from each other to agree that minimized genetic flow from one population to the other. That is usually described as less than five percent exchange over a long period of years. The distinction "population" if someone says, "Kaminuriak population" he is speaking of a group of animals he believes are discrete or separate from the group adjacent to it.

Where "herd" is simply -- it is a rather loose term used to identify a group of animals that have some affinity for a particular geographical area at some particular time in the year. That could be a group of ten animals or a group of twenty thousand.

Q. How are these tags retrieved?

A. Mainly from the hunters. Almost solely from the hunters.

We have to rely on the hunters returning the tags, and that does not always work that well because they quite often retain the tags for souvenirs or they are even used in poker games sometimes as a form of currency because their is a reward associated with the return of these tags. We usually pay a dollar for each tag returned.

Q. To return to the operation -- the tagging operation itself, you pull these animals to the canoe to tag them and then release them. Is this stressful to the animals?

A. Well, yes, there is no doubt there is a degree of stress associated with the tagging. But, again, in most cases you have to assume that it is minimal because there is no prolonged restraint and no prolonged pursuit.

Probably I should take this opportunity to expand a bit on some of the discussion that has taken place about all the dire consequences that prong-horn antelope experienced in southern Alberta and that used, seemingly to me, as an example of the possible stress associated with tagging caribou on water crossings.

The work referred to in southern Alberta was done by two people -- Barrett and Chambers, reported on in 1977, I believe. They, over a three year period, captured 479 prong-horn using both fixed-wing aircraft and helicopter in association. They herded these animals across the prairies for four to fifteen kilometers. They then ran them into corrals where they restrained them for

either several hours or several days in pens that were only an acre in size.

I think anybody can appreciate the additional stress that is placed on the wild free-ranging animal when he is suddenly stuck in a room this size and held. There is no form of escape. He has evolved under a system of escape. His only freedom and his only security is knowing that he has the freedom to run in any direction that he wants to go.

So, if you restrain an animal like that, especially if you restrain him in an enclosure that has wooden walls or burlap sacks so that he cannot see out, that has one advantage: he won't fight the fence if he cannot see through it, but he is still restrained and suffering all the problems of that restraint.

They then released these animals at different times from different places and they subsequently found animals that died within two to eight days after release. They attributed the death to capture-myopathy -- or, as in the business, the CM syndrome.

The so-called CM syndrome is usually associated -- well, always associated with animals that have been involved in prolonged chases and prolonged restraints --

and/or prolonged restraints.

I believe that over fifty percent of the animals they had died. They died during a period when they pushed them when the temperatures were very warm. If anybody knows -- although these prong-horn live in a relatively warm situation in the summertime, they cannot stand being pursued at a fast pace over long distances when it is hot. That will add considerably to any stress.

Subsequently, these two gentlemen wrote a chapter for a book. One is a veterinarian and the other a biologist. In their chapter they dealt in great detail with the CM syndrome in wild ungulates and their final conclusions were, I believe -- or one of their final conclusions is that the capture of animals such as caribou on water crossings represents one of the least stressful forms of capture that exists.

Q. Is there any evidence, sir, of animals dying as a result of tagging operations on water crossings?

A. Days afterward? Hours afterward?

Q. Yes.

A. I know of no evidence of death of caribou that could be related to

capture.

Q. You recall Dr. Geist as saying -- although I do not think he mentioned this in connection with caribou -- that tagging operations may result in up to fifteen percent of deaths?

A. I said capture, but I meant capture on water crossings. Okay?

I'm sorry. Would you repeat that?

Q. You heard Dr. Geist say that in the literature discussed he had a rate as high as fifteen percent mortalities directly attributable to tagging operations -- as a result of stress, I should say, although so far as I recall he did not say that this was true in connection with barren-ground caribou. What is your answer to this, if you have any?

A. He said this was related to water crossings?

Q. No. He said tagging operations on the animals. I believe he had in mind African animals, resulting in mortalities due to stress as a result of this tagging?

A. Tagging covers such a great spectrum. What I was just talking about in relation to prong-horns is essentially a tagging operation. It involves a capture operation.

They lost -- I don't think there was fifteen percent. There was something like ten percent. Ten to twelve percent.

The African ungulates have -- I know that a common practice in Africa for the capture of animals like giraffes is to pursue them over great distances in trucks at top speed. They run these animals with men hanging out the back with large loops they slip over their necks and eventually they get enough loops on the animal to bring him down.

I do not know of any high mortality associated with that type of exercise and that, undoubtedly, is very stressful. There may well be some capture programs that have taken place in Africa or elsewhere that have a mortality rate that approaches or is fifteen percent. But -- mortality related to this capture-myopathy. But, I would believe that if you investigated the particular instance you would find that it involved a severe amount of stress associated, again, with prolonged pursuit and restraint -- and/or restraint.

Q. After the animal has been tagged and released the animal is, undoubtedly, happy to get away. Am I right?

A. Yes.

Q. And it goes out the

river or water body. What is the behaviour of the animal as it jumps out of the water?

A. Well, they usually do not hesitate when they leave the water. Of course, they leave the water on what might be described as a brisk trot. That is truly the way they leave the water virtually all the time, even if they are not being disturbed.

You have to remember that water crossings in the mind, most likely, of the caribou is associated with predators. The wolves and the grizzly bears in the west commonly hunt at water crossings and caribou, you know, seem to have evolved some appreciation for this fact. They leave the water in a quite alerted manner even when not disturbed.

I would say that most of the time when they leave the water after being tagged is no more vigorous than if they had not been tagged.

Q. Has there been any evidence about animals that were tagged and then refused to associate with fellow members of the herd that had not been tagged -- or vice-versa?

A. No, I don't know of any evidence of that nature. I do to the contrary. In my own observations and any observations that are reported in the literature are that these

animals, when tagged, whether they are ear-tagged or streamered or have radio collars on them, rejoin their group and fit in sociably as they did before. They would, in minutes of being tagged, be usually back with untagged animals.

Q. Can you relate to the court a personal experience, if you had any, where this happened where animals displayed no -- or appeared not to display any outward ill-effects of tagging operations?

A. Well, observations in that area would be quite limited. On one occasion we tagged several -- actually just two on this occasion -- two bull caribou -- on a river system in Manitoba that goes into the Little Duck Lake area, and we then released the animals after tagging. We put collars on these animals -- visual collars for identification from aircraft.

The animals left the river on the far bank. It happened to be the end of the day for us, so we returned to our tent, which was down stream a ways, only within twenty feet of the water on the other side. We were inside the small tent, cooking up our dinner and had us a good day. Not much in the way of insect activity, so we had the flaps open. So, lo and behold here come these two

collared caribou. They swam across the river again after we had left them and were coming down the shore and feeding. They grazed up to within ten feet of the front of the tent, acted as though they finally detected our presence. We remained sitting still and not talking. And they then walked on, crossed over a hundred foot ridge and walked down within twenty feet of the single Otter aircraft that was anchored there on the floats and entered the water within ten or twenty feet of the aircraft and swam across that body of water to the far shore.

From all visual appearances, the overt behaviour of these animals -- they were grazing in a normal manner, taking their time, showed a degree of alertness to our presence but there was not anything to suggest that these animals were suffering any trauma from their past experiences.

Q. Did you have occasion to observe caribou on other occasions as they may have walked close to some objects -- unnatural objects? You heard the evidence, Mr. Miller, of oil drums at various crossings in the area under litigation. Kazan River comes to mind. There was evidence to the effect that caribou allegedly shun oil drums or abandoned camps in which there may be equipment. What is your reaction

to this?

A. Well, having an appreciation for the number of oil drums on the tundra, it is hard to believe they do shun them on a constant basis. They would be totally confused, I think, if that is the case.

There is a camp -- or there was a Hudson's Bay post at a lake called Padlei Lake in the Keewatin that was abandoned -- I do not know when it was abandoned, but has been abandoned since I have been there. It is quite an interesting place to go. It is on the north side of this lake, on a relatively steep hill -- probably two or three hundred feet above water. And there are caribou trails cut into the ground, a foot deep or so. They go right between the post house and the two out buildings that are a little closer to the water.

I had the good fortune of being there in September of 1967, I believe it was, just as a movement came through. We estimated twelve hundred animals walking right around the buildings, virtually within touching distance of the buildings. They came up, and there is an assortment of old aviation fuel drums and oil containers there that remain there. Some are full or partially full. Others empty.

Some indirect evidence is

that when you have a drum sitting on the tundra it reflects the heat of the sun and increases the temperature of the soil immediately around the perimeter of the drum and stimulates the vegetation growth. So, if you have any sedges or anything associated with the drum, they grow more proliferous than elsewhere. And it is quite a common sight to see large gatherings or droppings of caribou feces where they have obviously come and taken advantage of this luxuriant growth -- relatively luxuriant growth -- that occurs in a micro situation such as that.

They are, by nature, described as curious animals. They tend to investigate parcolls, tent camps, caches left on the land. Quite often they are walked on, stood on. The indirect evidence is that caribou droppings on top of several parcoll boxes -- which in the summertime are maybe ten feet off the ground, when the snow had blown up against them and made a hard ridge and the caribou would walk up on top. It makes sense that he is going to take advantage of this visual observation post. He can walk up onto a box that is ten feet off the ground and can see for miles across flat tundra. He gains a tremendous advantage from being able to look

over his terrain.

It is very normal behaviour for a caribou to climb up on rock outcrops. They are very goat-like. They have rather large, well developed hooves which allow them to be very sure-footed. It is not uncommon at all to see him up on boulder fields that most ungulates would not possibly be able to traverse. A deer would break his leg in short order if he tried to enter a situation such as a boulder field that is common on the calving grounds of caribou.

Q. Mr. Miller, you also heard yesterday, I believe, of prospecting stakes that are put in. They are about three feet in height. And we were also told about ribbons being fastened to them -- colored ribbons. One exhibit that has been produced shows a dark pink color. The evidence was that these are about three to four inches long and tied to these posts. The evidence was that they are very short-lasting and usually fall to the ground before the season is out.

Assuming a herd of caribou or a group of caribou would come across or approach such stakes, and assume that this ribbon of about three or four inches long would flutter in the wind, how would caribou likely react to this?

A. Again, the response to such a situation would be governed by all the factors that I have mentioned, but particularly the current history of the animals involved. You would expect a group of animals that had just been chased by a wolf to be more leery than a group that had not experienced a wolf for days or weeks.

My own opinion is that it is just impossible for me to conceive how these stakes would have a lasting impact on the animals. I am not saying that an animal might be walking along and all of a sudden he sees a slight twitter in his eye and he responds to it. I imagine he would take up an alarm stance and he would then determine whether the stake was moving or not.

These animals have evolved with the wolves. The wolf is the only thing -- the only yardstick, the only measure they have to relate other problems to, or potential problems, or perceive as a problem. They live with this wolf. When a wolf enters the area where caribou are, they do not just run from the area in wild abandonment. They know that wolf is not a problem until he is a certain distance. They can assumingly determine whether the wolf is hunting or not. A wolf has different postures. He doesn't try to hide himself; he holds himself higher and so

forth when not hunting. When he is hunting he takes on a stalking attitude. This is why it is true when you say that if you attempt to stalk a caribou you will scare him more than if you just walk standing up in plain view, often.

So you get this situation where the stake does not move, then you have to look at smell and probably sound. There might be a slight sound associated with it or there may be a slight smell. That may cause an animal to walk a hundred yards out of his way or a hundred feet out of his way or it may not cause him to do anything. In many cases he may go over and eat the ribbon. There are some examples that show caribou have eaten ribbons off seismic stakes.

You get all this variation. The color of ribbon has no bearing. Caribou essentially do not perceive color. They live in a world of shades of grey, like a black-and-white picture from a camera. Everything is perceived as a shade of grey -- absence or presence of light. The orange or the red would be dark. It would be toward the black side of the shades of grey. So there is no reason to believe that the animal will respond to color.

I have only seen stakes in one area myself, and those stakes were just east

and possibly a bit south of the Kazan Falls, and in 1970 the area was staked. I don't know if the whole area was stated but, the area was staked to the south, along the east side of the river. I know that there was a large movement of caribou at that time that came around the north end of Parker Lake and struck a course -- a southwesterly course -- for the Kazan, coming out several miles upstream from the falls and subsequently moving into the Thirty Mile-Forde Lake area, and staging there for several weeks.

These animals -- I don't know if they passed through the staked area or not, but they had to pass relatively close to it. If I can believe the story told to me by the two men who were staking, the caribou walked within thirty feet of their tent and they estimated there were several thousand caribou. I believe we subsequently estimated that approximately a third of the herd was occupying the Thirty Mile-Forde Lake area at that time.

MR. CHAMBERS: Those are all my questions. Your witness.

THE COURT: Mr. Graham?

MR. GRAHAM: No questions, thank you, My Lord.

MS. KOENIGSBERG: No questions, My Lord.

THE COURT: Mr. Estrin?

CROSS-EXAMINATION

BY MR. ESTRIN:

Q. Mr. Miller, you are a man of great experience in terms of caribou. I think we are going to have lots of talk about.

We have already heard in your examination in chief about some of your experiences, and I have a note that -- would it be fair to say that your most recent fieldwork dealt with the study of the Peary caribou and musk-ox in the higher Arctic?

A. Yes, it would.

Q. Mr. Chambers, when he was examining you, I believe, asked whether Peary caribou that you were studying had behavioural characteristics similar to that of the barren-ground caribou and I believe you told him that they were essentially the same?

A. Yes.

Q. And what about musk-oxen; are they a form of ungulate?

A. Yes, but they are not part of the deer family. They are quite removed from behaviour and physiological structure and taxonomy -- what have you, from the caribou?

Q. I take it you did a lot of work on the deer family before you did any

work on the caribou?

A. Yes.

Q. And you used your experience with the deer in order to enable you to make some conclusions to begin your work or do your work on the caribou. It assisted you, I take it?

A. I don't know if that is a fair statement: that I used that work. That work permitted me to be aware of a wide range of literature and to have some appreciation for the complexity of the situation that I would be dealing with, but I did not actually use information gained on the study of other animals as applicable to caribou.

Q. Were you essentially starting off cold when you did your study on caribou?

A. Well, I don't see how you could say I was cold. I had considerable experience.

Q. With deer?

A. Yes, but there is the relationship -- there are basic relationships, as you have heard, between all the ungulates. It is a matter of degree. You would not expect a white-tailed deer that live in a temperate environment, that is a colonial, pioneering type of

species to be directly comparable to a caribou in an Arctic situation.

Q. I think we can all understand that, but, nevertheless, it was a good starting point for you on your work on caribou?

A. Definitely a benefit having done something before I came to caribou, yes.

Q. I would like to ask you about the work you did most recently on Peary caribou. What years was that conducted? You can refer to your curriculum vitae, if that will assist.

A. That would help. I know the work on Peary caribou began in 1972.

Q. Would it be fair to say you did your basic fieldwork on the responses of Peary caribou and the musk-oxen to helicopter harassment in the years 76 and 77?

A. Yes.

Q. And I understand that you would have spent a fair amount of your time in late 77 and 78 writing up the results of your study?

A. Yes, also engaged in other fields.

Q. Tell us, you did this

study in conjunction with Anne Gunn?

A. That's correct.

Q. How would you term this study? Would it be the most comprehensive Canadian survey of responses by caribou and musk-oxen to helicopter harassment?

A. If I could forego my usual modesty, I would say yes.

Q. And it would accordingly be one of the most extensive in the literature?

A. Would you repeat that, please?

Q. Aside from it being the most comprehensive Canadian survey, I think it would be also fair to say, again if you could forego your modesty, one of the most extensive in the literature?

A. I'm not sure what you mean by "most extensive in the literature". Do you mean the work itself or the discussion of literature related?

Q. No, the work you did -- the fieldwork you did in terms of establishing a relationship between helicopters and caribou and their response. Would your study represent one of the most extensive undertaken as now reported in the literature?

A. Yes, it would be the most extensive that I am aware of.

Q. Mr. Miller, I would like to show you a copy of a paper entitled "Responses of Peary Caribou and Muskoxen to Helicopter Harassment"by Frank L. Miller and Anne Gunn -- Occasional Paper Number 40, Canadian Wildlife Service. Is that your paper?

A. Yes, sir. Hot off the press.

Q. Do you have a copy of it here with you?

A. No, I don't. Actually I do have a copy, but not here.

Q. It is in Toronto?

A. Yes.

Q. I see your counsel has one. If he could assist -- because I am going to be referring the witness to it -- if I may borrow your counsel's copy and give it to you.

The fieldwork was essentially done, as you said, in 76 and 77 and was just published in 79. It was just received by an office that I work with in the last few weeks?

A. Yes.

Q. Would the delay in publishing have something to do with the usual internal reviews this goes through?

A. Yes, the original report was 588 pages long. It required considerable

time to process by the different layers of referees and reviewers and so forth that we are subjected to in the system.

Q. And the result of your initial drafts were boiled down, edited, altered and this is the final version?

A. Part -- part of the final version. We still have other papers -- potential publications on other sources -- scientific journals.

Q. In terms of the title of the paper "Responses", and essentially this is what this paper addresses, is this the final version of the publication that will deal with that particular area?

A. No, it is the final version of many of the aspects associated with what governs responses, but we also have papers that are concerned more with just multi-harassment activities and things such as group defence formation of musk-oxen and so on.

Q. I see. I would just like to ask you a bit about how you went about this study. Referring to page 8, it indicates you used a Bell turbo-helicopter?

A. Yes, sir.

Q. In both 1976 and 1977?

A. Yes, the Bell 206B turbo-

helicopter.

Q. And that the surveys were made in the summer months, essentially? You can tell me what months?

A. Yes, June, July and August in total. The first season was relatively short as compared to the second season which ran through June, July and August -- most of August.

Q. Am I correct that all your flights for purposes of observing reaction of caribou to the helicopter were flown at less than 400 meters?

A. Yes, that is correct.

Q. And that in 1976 most of them were below 200 meters above ground level?

A. Yes, that is correct.

Q. And in 1977 they were mostly above 200 meters above ground?

A. Yes.

Q. And I note from your study that you obtained 3,939 individual maximum response samples of Peary caribou during 671 harassment overflights.

Does that sound like the figure?

A. Yes, but I better define maximum response sample.

Maximum response sample, as we used it, was the maximum response from an individual during the period of the overflight -- which was judged to be when the aircraft was still -- when the aircraft was first audible to the observer and, subsequently when the aircraft passed beyond the animals being disturbed to when it was last audible to the observer.

And by "maximum response" we do not mean per se the maximum response as determined as a gallop. We mean any of the categories which we established. We established the categories of gallop, trot, walk, alarm position, fixed in place, so-called maintenance activities of foraging and bedding. So it is possible that when you flew over a group of animals, the maximum response would be foraging or bedding. It is not attributed to a displacement type response necessarily at the maximum level which would be the gallop. Gallop would be the maximum response you would expect the animals to perform.

Q. I wanted to put it to you in your own words, Mr. Miller. At page 8 you say:

> "The maximum response of an animal during an overflight was taken as a measure of harassment. In total,

3939 individual maximum response samples (IRS) of Peary caribou were obtained during 671 harassment overflights ..."

Then you go on to talk of musk-oxen.

"... 64.0% of the Peary caribou samples ... responded overtly to the helicopter overflights."

Is that right?

A. That is correct.

Q. I understand that in addition to overflying these animals, you also landed near groups of caribou?

A. Yes, we made 116 landings near 736 caribou samples.

Q. And I note that nearly 30% of the caribou in this category we have just mentioned responded at the extreme level to harassment?

A. Twenty-six percent.

Q. Well, again, I don't want to quibble about a couple of percentages. But percentages are part of your case. I am just reading again from page 8:

"In total, 28.7% ... of the Peary caribou IRS ..."

That is the Individual Response Samples.

"... responded at the extreme level to

the harassment."

That is nearly 29%?

A. Okay. We'll correct it.

Q. And as I understand it, you also carried out other forms of activity about passes simulating cargo slinging. Could you tell us about that?

A. The simulation of cargo slinging involved finding a group of animals in a favourable situation where we could position ground observers, and when we found such a group, we landed the helicopter out of sight of the animals on some adjacent high terrain and the observers would then move forward to a position from which they could view the animals -- two ground observers in each case.

One observer stayed in the aircraft, and the aircraft lifted off. Depending on the situation, whether we thought the animals would stay there for any length of time or it looked like we had to do the job and get it over with before they left, we would either immediately start the overflight or we would wait one to several hours to start the overflight.

We attempted to try to collect as much information on their behaviour during the pre- and post-harassment stages as

possible.

The aircraft, once the flights were started we made six passes over the animals in straight-line flights in a manner -- relatively slow speed, maintaining constant altitude -- that would simulate cargo slinging. It sets up sort of a vibration in the machine, causes a slightly different sound from the machine: the same sound you would get basically when hauling a sling of several hundred pounds of cargo between sites. We would make these flights, usually, five miles either side of the animals. The aircraft would touch down as though it was depositing a sling, lift off and repeat the flight in the opposite direction.

The duration that involved in those type overflights varied because of wind speed and so forth.

Q. You mentioned low speeds. I gather from your report that these were less than 100 kilometers per hour?

A. Yes.

Q. Before we go too much further, I would like to refer you to your summary: the results of your study, found at page 84 and continuing on page 85.

MR. ESTRIN: I apologize for not having a copy for you, My Lord, because

I do intend to ask the witness to read from portions of this.

I only have the one copy, and it got marked up before I realized that it might be useful to put it before the court. So, unless we can make a copy of certain portions at the lunch break, I am going to ask to read certain portions.

BY MR. ESTRIN:

Q. Mr. Miller, I wonder if you could just start reading your summary at the top of the right-hand column on page 84 and continue. We have eleven conclusions there. I would like you to read them.

A. At "Our objectives..."?

Q. Yes.

A. "Our objectives were to determine the response of Peary caribou and muskoxen to helicopter-induced harassment. We simulated likely activities of helicopters involved in inspection flights, cargo slinging and deployment of work parties and personnel involved in amateur photography. We flew about 289 h in a Bell-206B helicopter in July-August, 1976, and June-August, 1977, over northeastern Prince of Wales Island and Russell Island,

NWT. In 1976, we used a three-man team for mainly airborne observations during flights mostly <200 m agl. In 1977, we used four, two-man teams as ground observers during mainly high level flights (>200 m agl)."

Q. Just stopping there "agl" stands for above ground level?

A. Yes, meters above ground level.

"We took the maximum response of one individual during one flight and categorized it as at the extreme level if the animal galloped or trotted (caribou) or galloped, cantered or moved together to take up a group defense formation (muskoxen). If the animals walked or became alerted, but stayed in place, we categorized the response as at the moderate level. If the animals did not aparently respond but remained foraging or bedded, we recorded the response as at the maintenance level. We analyzed the IRS in relation to measured variables by observed/expected indices from Chisquare tests of independence and three stepwise multiple regressions for

each species."

Q. Can you just stop right there. Can you explain what you just said in the last sentence very briefly?

A. It might take us a couple of days.

Well, very basically, an observed/expected indice is just a mathematical look at what you observe in relation to the theory of independence for hypothesis of independence which you would have to go to high-class statistical books to get the derivation of, and you come up with values that you can then look up in a so-called Chisquare table and see whether they are significant or not significant in a mathematical sense or statistical sense.

The regressions are just computer programs that are run to determine which factors are contributing the most influence to what you observe -- the responses, such as what does the number of calves present contribute to the overall level of responses, the group size or sex composition, the sun, the wind and so forth.

Q. And IRS, when you use that abbreviation, stands for?

A. Individual Response Sample. It is just a technical consideration there because you do not always know that you have had

a different animal under observation. You refer to them as samples rather than individuals. This gets you around the problem of knowing whether you have eleven hundred animals or ten hundred and ninety-nine.

Q. Could you go on reading with the words, "Our results were ..."?

A. "Our results were as follows.

(1) Of the 3939 IRS obtained for Peary caribou during 671 pass-type harassment overflights, 35.1% were extreme level responses, 28.9% were moderate level responses and 36.0% were maintenance activities.

(2) Of the 4011 IRS obtained for muskoxen during 315 pass-type harassment overflights, 28.6% were extreme level response, 15.0% were moderate level responses and 56.4% were maintenance level activities.

(3) Both Peary caribou and muskoxen showed a decline in response levels within a set of passes (simulated cargo slinging) which may be a form of habituation but such declines did not persist between different sets of passes flown days apart."

Q. Stopping right there: were you doing your flying "days apart" over basically the same herd of animals?

A. With the musk-oxen we were more or less positive of our group structure, and we knew what individuals we were working on throughout the field.

Caribou are a lot more dynamic at that time of the year. The group structure is fluctuating on a daily, if not hourly, basis. The type of terrain that they occupy, their relative mobility and so forth -- some groups you know you have the same animals and others you do not know. That is why it is sample.

Q. Would it be accurate to say that you were not doing -- when you make this observation, such declines did not persist between different sets of passes flown days apart. I take it you are still flying over the same geographic area?

A. Relatively speaking -- northeast of Prince of Wales.

Q. No, what I mean is, to make that observation.

A. Yes. Okay. Well, again, in the case of musk-oxen we were positive; caribou, we did have certain groups that we

re-worked. We re-worked them over a period of time.

Q. Yes, that is what I am asking about. Would you go on with number four.

A. "(4) The most extreme and intense level of response would have been galloping and/or tight defense formation during approach and departure of helicopter. We only observed this strength and intensity of response in 0.3% of the Peary caribou and in none of the muskox responses.

(5) Peary caribou and muskox response exhibited an inverse relationship with the altitude of the helicopter overflight: the higher the helicopter the smaller the proportion of animals which responded at the extreme level.

(6) Peary caribou cows and calves were the most and bulls the least responsive of the sex/age classes. The apparent responsiveness of caribou juveniles and yearlings was usually a reflection of their investigative behaviour.

(7) Muskox bulls as solitaries or in single sex groups tended to be more

responsive than individuals in mixed sex groups. Muskox bulls in mixed sex groups tended, however, to be least responsive.

(8) Peary caribou were more responsive if calves were present and cow-calf pairs were the most responsive group type. Peary caribou were also more responsive in larger groups.

(9) The responsiveness of muskoxen in mixed sex groups appeared to be more a function of the make-ups of individuals within the group rather than group size or the number of calves present.

(10) In 1977, we made 116 and 69 landings within 201-1000 m of Peary caribou and muskoxen, respectively, and the inverse relationship between distance from the harassing agent and extreme responses by animals pertained.

(11) Ground activities by people after the landings seemingly influenced the subsequent responses more than did the presence of the helicopter."

Q. All right. Stop there. Now, you went on, after having set up those eleven results in summary form -- you made recommendations

for an altitude ceiling for all aircraft types; is that correct?

A. I don't recall exactly how it is worded -- "All aircraft types".

Q. Perhaps let me ask another question and I will give you a chance to read what you said. You made a recommendation as a result of your findings for an altitude ceiling for all aircraft types during certain periods of the year, and with regard to minimum distance for landing of aircraft. Would it be fair to say that those recommendations are set out in the lines immediately following number eleven?

A. Yes.

Q. And those are that you recommended --

> "... altitude ceiling for all aircraft types of 300 m agl from November to April and 600 m agl from May to October ..."

Which is, you say

> "... the period of calving, post-calving and rutting - three critical periods in the annual life cycle of both species."

Is that what it says here?

A. Yes.

Q. Is that your recommendation?

A. Yes.

Q. And also you "... recommend that aircraft land no less than 1000 m away from animals and much further if possible ..."

A. Yes, I did.

Q. Now, I would like to refer you back to page 18 of your report in regard to that recommendation. Having regard to those recommendations, I take it that your study showed that even at 300 to 400 meters above ground level you still had nearly 30% of your animals making observable responses to the aircraft. I am referring to the second right-hand column on page 18, first paragraph.

A. Where did you get the 30%?

Q. It is 29.3%. The sentence is:

"Peary caribou responded more at the extreme level, 74.1%, when the overflights were at <50 m agl, followed by 55.6% at 51 - 100 m agl, 44.1% at 101 - 200 m agl, 15.2% at 201 - 300 m agl and 29.3% at 301 - 400 m agl."

Is that correct?

A. Yes, that is correct.

Q. When it says in that

sentence they were responding, you say that 29.3% were responding in an observable way.

A. Yes.

Q. On the difference between observable impacts and non-observable ones, I would like to refer you back to page 85.

I take it your study here was directed solely at observable results, as opposed to information on energy expenditures or long-term effects?

A. Yes, that is correct. That is what it says.

Q. You were concerned only on the immediate behavioural responses to helicopter harassment; is that true?

A. Well, that's all we could measure. Yes.

Q. And you go on to say:

"Our study, the first designed specifically to measure behavioural response to helicopter harassment, has to be regarded as only the beginning of the research necessary to gain an adequate understanding of the subject."

A. Yes.

Q. And you also go on to say:

"This condition is especially true if the Government of Canada is to honour

its obligations to maintain the traditional rights of native peoples and to wisely manage renewable resources for all Canadians in a manner that will minimize the impact of northern development."

A. Yes, that is true. And I would like to emphasize "for all Canadians in Canada". The renewable resources belong to all Canadians in Canada, if they belong to anybody.

Q. I see. You do not feel that the Inuit or the native people who live in these areas in which the caribou come have any special relationship to these animals?

A. Only in the sense that they have the privilege to use the resource. When they abuse the resource, then it is for the rest of the people of Canada to do something about it.

A renewable resource -- you know, once it is gone, it's gone. You cannot stock reindeer there. Some politicians might try that, but reindeer are not caribou.

And when you talk about tradition, they no longer -- in my own opinion -- practice a traditional way of life: they practice a chosen way of life. There is big distinction there. When you give up the spear and the kayak for power boats and telescopic rifles, skidoos and

aircraft, you cannot refer to that in the true sense of any meaning of tradition, unless the tradition started in 1950 -- or, in effect, 1960 would be more correct.

Q. In any event, coming back to the topic of harassment --

THE COURT: You asked for that, Mr. Estrin.

MR. ESTRIN: I would like to go on and ask the witness about it. I had no doubts about how the witness felt.

THE WITNESS: Well, My Lord, since --

THE COURT: I question the relevance of the opinion. Frankly, this is a long trial anyway, and there is no sense in addressing it too far.

MR. ESTRIN: I had no doubt about how the witness felt.

BY MR. ESTRIN:

Q. I take it that your colleagues on this Caribou Technical Committee would share your view?

A. I don't know. I can't speak for them on matters such as that.

I think they all feel that their first responsibility is to the resource; that they are charged with the responsibility of

protecting the resource. That protection includes the wise utilization of the resource. When the utilization becomes foolish, improper, then there should be -- you know -- some action taken to prevent that type of abuse.

Q. Now, continuing -- I am sure we will get back to this some how.

THE COURT: Preferably in argument. Okay?

MR. ESTRIN: Yes. I didn't intend to extend the cross-examination on that point.

THE COURT: Okay.

BY MR. ESTRIN:

Q. We have just examined your recommendation about minimum or maximum altitudes above ground level for aircraft?

A. Yes.

Q. The last sentence on page 85 says:

> "We will require physiological studies of harassment and long-term studies to examine the adequacy of our recommendations."

Is that what it says?

A. Yes, it does.

Q. And that would apply to the recommendation, among others, about minimum

flight altitude?

A. In the sense that if you believe that an animal that has evolved by flight as its only form of escape would surely stand in place and elevate its heart-rate two or three times rather than simply walking, trotting or galloping out of the way.

To digress a bit about heart-rate, you get the same response in elevated heart-rates when it starts to rain, the leaves rattle, or -- if you are raising penned animals -- when they can hear the person preparing the baby bottles for them to be fed. The heart-rate goes up higher than if they are exposed to, say, some stranger walking by. You are presenting a foreign stimulus, so you have to have a pretty hard look at the physiological side of it -- what it means, truly, and the interpretation of the stress that is placed on the animal.

Q. In your study you do set out what the range of physiological responses might be and what they might lead to. You say, do you not, at the bottom of page 77:

> "An animal's behaviour is often as much a manifestation of its internal as of its external environment and its behaviour will be reflected in its physiological state. It is, thus,

inescapable that behavioural responses to harassment will be accompanied by physiological responses. Physiological responses range from changes in heart or respiration rates to acute pathological conditions such as overstraining disease."

Is that what you said then?

A. Yes, I did.

Q. I am showing you Exhibit P-89. It is entitled "Aviation Notice, Conservation - Caribou Calving Areas." Would you mind just reading that page to yourself, and I would like to ask you a couple of questions about it?

A. Yes.

Q. This notice, first of all, requests that pilots "avoid flight below 1500 feet agl over ..." certain areas, between May 25th and June 30th.

First of all, I take it your recommendations would -- first of all, based on your observable responses, have a greater distance, a greater height -- would that be true -- than 1500 feet, 600 meters?

A. Well, there would be some likelihood of responses at above 1500 feet, but these would be a minimum percentage of the overall.

Q. Your recommendation was 600 meters?

A. Yes.

Q. And your recommendation of 600 meters should apply over a much longer period of time than May 25th to June 20, is that correct?

A. Yes.

Q. Then it goes on:

"Low flying aircraft are a known cause of stress to caribou especially during calving periods. Pregnant adult females, just prior to calving, are prone to abort the unborn calf if they are subjected to stress from airplane harassment."

Would you agree with that?

A. No. I know of no documentation in the literature of caribou aborting calves just prior to calving.

I would think where it is from is from Russian literature on reindeer, where Ziginof makes reference to abortion, without any documentation, which could result from prolonged chasing of near-term caribou over hard crusted snow. But I know of no actual documentation of abortion in caribou in North America just prior to calving from

aircraft experiences.

Q. When you make recommendations, are they always based on that kind of absolute, hard field observation?

A. Well, those recommendations, as you know, were made basically on the quantitative information that we obtained in that particular study. Every time you repeat the study, you will get slightly different quantitative information. And that could influence whether you subsequently say five hundred meters is as good as the 600 meters or the reverse.

Q. Let's go on to the next sentence:

"In attempting to escape harassment after calving, adult female caribou will abandon their young for unpredictable lengths of time."

Would you agree, or disagree?

A. I would disagree with that, basically. There are probably exceptions. Again, it would be the amount of stress, the type of aircraft. If the aircraft -- if the guy wanted to take pictures and is hanging over the top of the animals with a chopper, that is true.

But, My Lord, one of the most interesting aspects of caribou ecology is the so-called maternal filial bond between mother and

her newborn young. This bond develops within minutes and hours of birth and becomes a very strong sort of all-determining relationship between the mother and her young.

You have a situation within where hours after birth the calf is what we refer to as a follower type. It will be up and actively following its mother across the tundra. She will shortly join so-called nursing bands or nursing groups of other females with their newborn young and they will merge into large post-calving aggregations which will result, most of the time, in several hundreds or thousands of animals travelling across country in rates in excess of five miles an hour.

If the cow is prone to desertion at the slightest overflight or attack of a wolf or whatever, she abandons its calf totally and had no mechanism -- behavioural mechanisms -- for retrieving the calf, you know, you wouldn't have caribou as they exist today. It would be impossible for the young to survive long enough to ever become breeders themselves.

I don't know where this information comes from. It is a generalization. You could make a case for it. Like I say, if you hovered over the animals with the aircraft for five or ten or fifteen minutes and pursued them

as they ran in one direction and the other. But, there is no reason to believe that that would be the response to one overflight.

And, if the overflight was in excess of a thousand feet, even, I would stand and say that there is no probability -- virtually no probability. The only time that something like that might happen would be if the female was a prima parous cow -- that is a cow giving birth for the first time -- and she was not psychological adjusted. This represents a very minute -- well, not minute, but a very small portion of females that could be producing young at any one time on the calving ground.

It is a highly unlikely situation.

Q. Well, you have in your publication, table 85. Would you look at that with me. This is entitled "Schema of some potential effects of harassment and their interrelationships for Peary caribou and muskoxen".

A. That is correct.

MR. ESTRIN: Perhaps, My Lord, we will have an opportunity to photocopy at least that chart. It begins a bit difficult to describe it graphically -- verbally, rather.

BY MR. ESTRIN:

Q. Over in the left-hand

column we have something happening, I take it, which generates an "Immediate Behavioural Response".

A. That is correct.

Q. We follow that through, and it can cause a number of things. Let's go up to the top. And follow that arrow through. And you have the word "alert". The animal can assume an alert position or otherwise exhibit alertness?

A. Yes.

Q. That arrow goes further, and the next indication is "Alterations in daily activity pattern". Is that true?

A. True.

Q. That arrow continues to the statement "Reduced use/intake forage", is that right?

A. Yes.

Q. From that arrow we go down to the words "Energy loss".

A. Right.

Q. And we follow that through and the next words are "Weight loss".

A. True.

Q. And from there you can go in either of two directions. You can go from "Weight loss" to "Failure to breed" or "High calf mortality".

A. Yes.

Q. Which in turn leads to a "Population decline"?

A. Yes.

Q. Or you can go from the "Weight loss" to "Increased susceptibility to predation/disease"?

A. Yes.

Q. I would like to go back again to the beginning of the reactions. "Immediate Behaviour Response", -- another reaction could be "Flight"?

A. Yes.

Q. You follow that arrow through and the first word we find "Acute pathological conditions"?

A. Yes.

Q. And from there you can go in either of two directions. From that can results "Pulmonary emphysema, Shock, Capture Myopathy, Prolapsed Rectum, Abortion, Hyperthermia, Stress"?

A. Yes.

Q. And if you follow those through, those lead to "Increased susceptibility to predation/disease"?

A. Yes.

Q. And from there, that can lead to death. Or, following from the "Acute pathological conditions" lead immediately to "Death"?

A. Yes.

Q. Again, following this flight reaction through on another arrow, the first thing that we come to is "Cow-calf separations". Is that true?

A. Yes.

Q. And that takes us again to "Increased susceptibility to predation/disease"?

A. True.

Q. And that again goes back to a "Population decline"?

A. Yes.

Q. Another result of flight on the part of these animals can be "Injury"?

A. Yes.

Q. Another reaction can be "Changes in behavioural patterns"?

A. Yes.

Q. Under that heading, if I am reading this correctly, we have "Short-term" and "Long-term"?

A. Yes.

Q. "Short-term" changes in behaviour patterns result in "Aggressive behaviour" which can, in turn, lead to "Injury"?

A. Yes.

Q. Or, to "Maternal behaviour"?

I'm not sure what that means. What does that mean?

A. Vagrancies in the reaction of the cow to her young because of the stress -- the level of stress, that is.

Q. I take it that can lead to some negative results?

A. Yes.

Q. And then you have "Long-term" with regard to these behavioural responses under four categories. Some impact on "Distribution"?

A. Yes.

Q. Impact on "Cow-calf behaviour patterns"?

A. Yes.

Q. Impact on "Activity patterns"?

A. Yes.

Q. And another one, "Habituation"?

A. Yes.

THE COURT: This would be the time to quit, if you should move into another area.

MR. ESTRIN: Thank you, My Lord. I believe I have one more question that might follow from this.

THE COURT: Go ahead.

MR. ESTRIN: I have one or two very brief ones.

THE WITNESS: My Lord, do I get a chance to explain this table?

THE COURT: If you don't take it now, you will in re-examination. Don't worry, your lawyer will take care of it.

BY MR. ESTRIN:

Q. Referring to page ten of your report --

THE COURT: Whatever Mr. Estrin prefers, I suppose. If Mr. Estrin would prefer you to explain it now so he has more cross-examination on it.

MR. ESTRIN: I believe it would be helpful for me to go where I wanted to go next, and I'm sure you will get an opportunity to say what you want to say.

BY MR. ESTRIN:

Q. In reference to these kinds of reactions, I want to refer you to page ten, the left-hand column. Again, you only noted observable responses or lack of response in behavioural terms. Then you go on to say -- and this is the last sentence in the third paragraph:

> "Any harassing stimulus causes a change in an animal's environment and the animal will respond in an attempt to adapt to the change. Therefore, the apparent lack of response during some

harassment is, in fact, a reflection of our inability to detect a response."

A. Yes, that is correct.

MR. ESTRIN: I think, My Lord, we can leave it right there.

THE COURT: Mr. Miller, you are in the process of cross-examination, so you appreciate that you ought not discuss your evidence over the lunch hour with anyone, even your solicitor.

We will recess now until 2:00.

---Luncheon Adjournment

---Upon Resuming after Luncheon Adjournment

BY MR. ESTRIN:

Q. I am showing you a photocopy of Table 85. Would you look at this and tell us if that is indeed your Table 85?

A. Yes, it appears to be a photocopy.

MR. ESTRIN: My Lord, I would ask that this photocopy be made the next exhibit.

EXHIBIT NO. P-91: Photocopy of "Table 85", from report entitled 'Responses of Peary caribou and muskoxen to helicopter harassment".

BY MR. ESTRIN:

Q. Mr. Miller, in relation to this chart, I see the reference to it in the text is, as I understand it, at page 77, in the left-hand column, it begins with:

"(b) Other potential effects of harassment (Geist 1975:4-9) would only become apparent months or even years after harassment (Table 85)."

Is that where the reference in the text comes to this table?

A. Yes, it is at least one of them. There may be other references.

Q. If you come across other references to Table 85 in the text, I would appreciate your telling us about them. That is the only one I came across immediately.

Are you saying that this table then sets out potential effects of harassment as written about by Dr. Geist in his submission to the Berger Commission?

A. Dr. Geist, yes. It is essentially an accumulation of the potential harassments in the relationships to ungulates in general that was put forward by Dr. Geist -- I believe at the Berger Commission. And in other literature you can find the same type of information, at least in part.

Q. And you have published this in your own report under the table that says: "Schema of some potential effects of harassment and their interrelationships for Peary caribou ...". I take it you believe that what Dr. Geist is saying has relevance to caribou.

A. Only in the sense that it reflects a potential. I mean you cannot rule out one hundred percent that any of these things would not occur given enough exposure to detrimental stimuli.

Q. But you thought it was real enough and relevant enough to publish his interrelationships as part of your own study?

A. I don't believe you can say that these are Dr. Geist's. These are sort of a compilation of all the people involved in the study of ungulates under different conditions. Most of the assumptions, by far the majority, are theoretical. They have never, to my knowledge, been documented in a quantitative manner. Like I told you before, to my knowledge Ziginof was the only one who made reference to abortion in caribou, and he presents no quantitative data. This is simply a presentation of the worst possible conditions that could occur and they are extreme -- and I have to emphasize really extreme forms of harassment. I mean he would have to try to go out and beat the

animal over the head for a long period of time to get that type of reaction. There you are still dealing with an individual.

How you could cause reactions at those levels to a population, short of moving maybe the Canadian army in on the calving ground to go through manoeuvres for three or four months, is beyond me.

Q. Mr. Miller, it tells me a bit because you published this chart and indeed in the summary discussion portion of your study -- the final paper on which the particular aspect of "Responses of Caribou to Helicopter Harassment," and yourstudy was to consider the likely types of activity of helicopter activities that could be anticipated during the construction and maintenance of a pipeline, isn't that right? It wasn't talking about the Canadian army?

A. I believe, sir, that this article was written in a scientific publication for other scientists who have the necessary background to make the evaluations and exceptions of the text that is put to them. This is not a publication for a layman. A person that this publication would be directed to would be another biologist who knows, without being told, more than likely, where this material comes from -- different sources of literature that

it was extracted from -- and how it is used by other biologists.

It is the most extreme case that could occur. That's all I can say. It has not occurred to date. It has not even begun to be approached to date. I am not saying it never will happen in the future because I don't know what the army has planned.

Q. Mr. Miller, I take it you will let us know, while you are in the stand, or through your counsel afterwards, if you can show us where in the study it suggests that this table is meant to represent only the most extreme, worst case effects that you have just suggested, such as --

A. Well, in the title, sir, where it says "Potential".

Q. All right. Mr. Miller, you are now saying your study was meant for a specialized audience. Is it fair to say that you meant the results to be taken seriously by those who did read it?

A. The results are strictly our conclusions of -- excuse me. Say again. The results or conclusions?

Q. Well, your conclusions.

A. Our conclusions are based solely on our results. In this particular

study, when you extend these conclusions, then they are generalities, at best, and there are -- I don't know. All I can repeat is that you are looking at the ultimate in harassment. The final word-- when the animal dies. Enough of them die, the population declines. A situation that I know that nobody can document in ungulates -- in caribou in North America, Alaska or Canada. None for any of these.

The only facet of this table that, to my knowledge, has occurred to date is the one item under short-term which leads to only aggressive behaviour in maternal care which is related mostly to musk-oxen. It is not itself necessarily harmful to injury-type situations. It involves just a few animals.

Q. Mr. Miller, do you remember a paper you wrote, and published in 1970: "Calf mortality on the calving ground of Kaminuriak caribou".

A. Yes, sir.

Q. That paper was published by the Canadian Wildlife Service. It is Report Series Number 26?

A. I believe so.

Q. You wrote then -- which might help us with the subject matter we are discussing -- you do not have a copy of

it with you, do you?

A. I do in my briefcase.

Q. Perhaps you could get it, if you don't mind. We will be coming back to it. Do you recall that before I referred you to Table 85 I referred you to Exhibit P-89, which is the "Aviation Notice"?

A. Yes.

Q. I referred you particularly to the sentences in the second paragraph that talk about:

> "Low flying aircraft are the known cause of stress to the caribou especially during calving periods. Pregnant adult females, just prior to calving, are prone to abort the unborn calf if they are subjected to stress from airplane harassment."

And you basically felt that you could not agree with that. Is that what you said?

A. I cannot agree with it having happened. I can agree that it could happen under certain conditions.

Q. Oh, I see.

A. I said these are all potentials.

Q. I see. Because I

understood you to say because you had never seen it happen, it couldn't happen?

A. No, sir, I have never said that -- or in this session. I stated all I reported on is what has taken place to date. I have not said anything about beyond what the potentials are for the future. I make no point about it not being a possibility. It is a possibility, as I indicated in Table 85.

Q. Could you turn with me to page 4 of your report "Calf mortality on the calving ground of Kaminuriak caribou"?

In the Abstract I refer you to the right-hand column and there is a sentence about half-way down the page. It says:

> "Most calving took place between June 4 and 10. If harassment by aircraft takes place during the peak of calving, a considerable loss of new-born calves could occur."

Is that what that sentence says?

A. Yes.

Q. Would you look with me, please, at page 12, in the middle column. The paragraph begins:

> "Abandonment of new-born calves by their maternal cows was the second most

common cause of mortality."

We are talking about the Kaminuriak Herd?

A. Yes, sir.

Q. It says:

"This seemingly abnormal behaviour of the maternal cows could be explained by the following suppositions."

I want you to refer to the third supposition. Could you just read that paragraph?

A. "The three older calves that were classified as abandoned could have become separated from their maternal cows when the group was disturbed by hunting wolves or low-flying aircraft, especially our helicopter. Helicopters create a much higher level of disturbance than fixed-wing aircraft. Ground observations during the study showed that caribou were alerted to an approaching helicopter at three to five times the distances at which they responded to an approaching fixed-wing aircraft. Cows who were unable to nurse their calves because of udder inflamations would be least likely to seek out their calves after being

separated in this way."

Q. And would you turn with me to page 17, under the heading "Aerial photography". I wonder if you would just read the first paragraph.

A. "We began the aerial photography of caribou on the calving ground in early June. The low level, 100 to 165 m (300 to 500 ft) flying that was necessary for good compositional photography of mixed groups was a serious form of harassment to the caribou. As our primary objective was to determine the causes of mortality to new-born calves we decided not to introduce possible additional mortality by aircraft harassment. Therefore, we carried out most of the photography for determining composition of caribou groups at heights above 200 m (600 ft). Thus, the photography was mostly unsatisfactory for compositional counts by current techniques and knowledge of resultant imagery."

Q. And one more paragraph I would like you to refer to is on page 19 under the heading "Conclusions and recommendations". Would you agree with me that the following occurs --

at the bottom of the first column. It says:

"The weather during the 1970 calving period was favourable for calf survival with the exception of the one heavy rainfall of 2.18 cm (0.86 inch) on June 16 followed by freezing temperatures at night. However, in some years unfavourable weather conditions would indirectly account for most of the deaths of new-born calves, by causing pneumonia or other respiratory diseases."

And it goes on for one more paragraph. Would you read the next paragraph?

A. "If harassment by aircraft does result in injuries in new-born caribou the incidence will probably increase with the increasing aircraft exploration on the calving grounds and post-calving areas. If harassment by aircraft takes place during the peak of calving a considerable loss of new-born calves could occur."

Q. Now I would like to turn to another topic that you addressed in your study. This is the 1979 publication of the Baker Lake Study. As a result of that study, I take it, you have expressed some concerns about the -- let me back up. You have already given us your

recommendations as to what should be minimum flight separation distances between aircraft and caribou?

A. Yes, from that particular study. Yes.

Q. Additionally in this study it says, as I understand it, it has expressed some concern about the likelihood of enforcing these minimum height rules?

A. Yes, I believe so.

Q. Would you refer to page 80. Could you perhaps read that for us?

A. "We must have the pilot's co-operation (willingly, if possible) to minimize any unnecessary flying in the vicinities of wildlife for the purposes of photography, or a "better look" at, or an impulse to run the animals encountered. The likelihood of enforcing such heigh restrictions for aircraft is questionable at best and then only if development companies choose to comply with the rules. Also, weather conditions in the Arctic often force single engine aircraft below 200 m agl to maintain visual contact with the ground, the need for and the wisdom of which cannot be argued against by anyone who has flown under such conditions --

especially if they have ever 'lost an engine'!"

Q. And you also expressed concern about the realities of the pressures of those who are charged with working in the north. Is that true? Would you read the next paragraph?

A. "The federal, provincial and territorial agencies charged with the responsibility for conservation of wildlife and their habitats must vigorously press for the constraints necessary to protect their charges in a time of hasty development of unrenewable resources. The 'corporate citizen' may preach moral sensibility with some degree of sincerity, but such principles are not revered by subcontractors faced with deadline penalities, narrow profit margins and a transient labour force that often shows a conqueror's indifference to the land that they are manipulating and the native species of that land."

Q. And I would refer you to the last paragraph on page 82, under your "Conclusions", and the last sentence which reads:

"The short-term costs to individuals ..."

Caribou, I take it, or musk-oxen,

"... and the long-term impact on populations are not known. Therefore, the matter must be treated with concern if we are to maintain harvestable ungulate populations for the native peoples of Arctic Canada."

Would you say that it is fair to conclude that that statement would apply and be applicable to barren-ground caribou?

A. If you put it in its proper perspective: which priority in which is the most decimating fact influencing the population, yes. It is a consideration for the future.

Q. Well, would you not agree that the matter of low-level helicopter overflights, based on your own observations and your work on caribou -- would you not agree that as a result of your conclusions stated herein and stated in this report that that kind of activity must be treated with concern in the area of the Baker Lake population?

A. Yes, concern. But, again, it has to be put in its proper perspective.

Q. Do you know Mr. T.C. Dauphine, Jr.?

A. Yes, I do.

Q. I take it he is the biologist that works with the Canadian Wildlife

Service?

A. Yes, still does.

Q. And he wrote an article in the same series that the last one we referred to was in by the name of "Biology of the Kaminuriak population of barren-ground caribou, Part 4"?

A. Yes.

Q. Published by Environment Canada -- Wildlife Service? Is it Mr. or Dr. Dauphine? Do you know?

A. Mister.

Q. I am referring to page 61 of his report, the last paragraph under the heading "Conclusions and over-view". I am going to read this to you and ask you whether you agree or not.

> "As a final point, the results presented in this report suggest the key importance of summer forage to the reproduction, growth, and winter survival of the Kaminuriak population. Caribou are adapted to marginal subsistence during winter, but depend on full nutritional recovery during summer to prepare for demands of body maintenance and reproduction the following autumn and winter."

Would you agree with that?

A. Basically, yes.

Q. Going on:

"Any disturbance to caribou on their summer range which decreases feeding time or increases energy demands is detrimental, especially preceding or following a winter of deep snow. Human activities that may disturb caribou on summer range should be carefully regulated."

Would you agree?

A. Yes, basically.

Q. Mr. Miller, I would like to refer you to certain portions of your Affidavit. Do you have a copy?

A. Yes.

Q. In paragraph four you say:

"The Kaminuriak herd is one of the two caribou herds which frequent the area near Baker Lake ... and are harvested by the residents of that community as well as by residents ..."

Of some others.

Can you tell us, please, what percentage of the Kaminuriak Herd the Baker Lake Inuit population killed in each of the last

ten years?

A. Not from memory, no. There are various tables produced by different groups that give those figures as reported.

Q. Did you look at those figures before you made this statement?

A. Yes, I did.

Q. And you do not have any recollection of what they are?

A. Well, I know what the average is: 3,031 reported over that period.

Q. Let's be clear. I am asking you, do you know, either in percentages or in numbers, what was the harvest by Baker Lake Inuit in each of the last ten years or any of the last ten years of the Kaminuriak Herd?

A. I know that the tables that were produced gave those percentages. They are broken down by caribou population of a herd. They say what percentage is from the Kaminuriak or what percentage is from the Beverly on an annual basis.

THE COURT: I think there is a bit of confusion here, Mr. Miller.

As I understand your evidence, taking the 3,031 average in paragraph 10, that is the total number that is killed not only by Baker Lake Inuit but by Eskimo Point, Chesterfield Inlet,

Rankin Inlet, Whalecove, and perhaps something in northern Manitoba.

I think what Mr. Estrin wants is if you can remember what percentage of that one hundred percent was attributable to the Baker Lake Inuit?

A. No, I can't remember the percentage, as I said, but it is given in one of those tables.

Q. Would those tables you are referring to be from the NWT game officers reports?

A. Well, I am not a hundred percent sure if it is the game officer's reports. It is by NWT Wildlife Service personnel, yes.

Q. You are not sure whether --

A. I don't know if the game officers did it, biologists, or administrators.

Q. Can you tell us exactly where you got your information from?

A. From the table.

Q. It was a published table?

A. No. I don't know. I don't believe it was published. I guess it was what you could refer to as an in-service report. It is part of the material we examined to come up with the material that we presented.

Q. Would it be fair to say

that the game officers' reports from the Baker Lake area are published?

A. Published? I don't believe so, no. I don't think so.

Q. You are not aware of it?

A. I am almost positive there is no publication. I cannot conceive of any journal that would handle such dubious material.

Q. You are saying that the game officers' statistics are dubious material?

A. I don't think that they are -- as I stated, obviously low.

Q. Oh, I appreciate what you are saying. You would add some percentage on to the figures by way of kill. But, tell me, Mr. Miller, do you have any other data to work with in terms of the number of killed animals, to begin with?

A. No, if we had other data we would have used it.

Q. So, that we would have to go to that data --

A. Those data.

Q. To try to establish where your three thousand and-some-odd figure comes from. All right. I would like to show you some of those game officers' reports because I would like to ask

you whether you agree with me as to what they show as to the percentage of kill by the Baker Lake Inuit. Let me ask you something before I show you this report, Mr. Miller. There have been some reports lately about herds of caribou wintering north of Baker Lake. Some people call them the Melville Herd, some call them the Wager Bay Herd, some call them the Lorillard Herd. What is your view as to whether or not any of those three herds are part of the Kaminuriak Herd or not?

A. I would say it is highly unlikely that they are part of the Kaminuriak Herd. I would say that as far as Wager Bay or so-called Lorillard Herd -- very, very remote, ninety-nine and nine-tenths percent likely that they are not part of the Kaminuriak.

Kaminuriak, just on the information that is available, we can account for virtually all the animals that theoretically should exist at this time.

Q. First of all, Mr. Miller, I would like to show you a photocopy of the Annual Report, 1972 - 73, Government of the Northwest Territories, Game Management Baker Lake by Ronald Hawkins, Game Management Officer. This report has a table of contents and goes on to page 1 -- 2. Then that report -- attached to it is a letter, as an appendix, and by letter dated

April 26th, 1974, Mr. Hawkins writes to Mr. Tom Armstrong, Regional Game Management Officer, Government of the Northwest Territories. The heading there is "Revised Summary of GHL Returns for 1972 - 73", is that what it says?

A. Yes.

Q. And I want to refer you specifically to the third paragraph. Perhaps you could just read that?

A. "The total caribou kill based on monthly estimates was 1,401 which represents 63% of the reported kill (not 60%) the estimated distribution of caribou kills is ..."

Q. It says "for 1972 - 73"?

A. "Melville (i.e. north of Baker and east) 11%, Beverly 74%, Kaminuriak 15%."

Q. Do you have any basis for disagreeing with the percentage breakdown of the kill as reported here?

A. Yes, but I am not sure that the officer in question really knew the distribution of the animals that well to come up with the breakdown like he did on a monthly basis.

Q. Have you seen any other statistics in that regard?

A. No, those figures, I

believe, are the figures that are used in the tables that I am referring to for those particular years. If you go a few years later, then you are talking about a hundred percent or so.

Q. I want to show you the statistics for later years that are avaiable to me. I would you to now look at the Annual Report - 1973 - 74 by Mr. Hawkins, Baker Lake, NWT, Area Game Management Officer. There is a Table of Contents, page 1, 2, 3, 4, 5, 6 -- 7 page, and can we just read together this paragraph beginning at the top of this page under the heading "Barren-ground caribou". I will read it, and you tell me if I have got it right. It says:

> "The people of Baker Lake are highly caribou oriented, which is already well known. Following the main way of hunting activity through the year provides an interesting pattern on the distribution and numbers and movements of caribou. The Beverly Herd provided 65.4% of the total kills (1,564) and all of this was taken between July and November. Very large numbers of animals were taken during September between Schultz and Aberdeen Lakes, providing many good winter caches. The Kaminuriak Herd was harvested only lightly

(234 animals) mostly during July to October and provided 9.6% of the total kill. The Melville Herd, which is the name I use to refer to all animals north and east of Baker Lake, wintered over this area between White Hills and Tehek Lake, east all the way past Quoich River to Lunan Lake. 594 of these animals were harvested mostly during December to May 1974, constituting 25% of the total caribou killed for Baker Lake in 1973-74."

Is that what it says?

A. Yes.

Q. Do you have any other figures that would dispute that data?

A. Not available right here. I don't know if they are in agreement with what was in the table that we used or not. I assume so, since the tables were provided by the NWT.

Q. Now, I cannot seem to find anything specifically for the year 1975-76 in terms of game officer's reports. Would you have anything that would assist me?

A. For Baker Lake?

Q. Yes.

A. No, I don't think so. Like I say, I don't have the tables. So if it is

in the table, it is in the table. In that table, possibly. There may be -- I vaguely remember that, I think, they did not have a game officer in Baker for one year. That may be the year.

Q. Perhaps, if you come across some data for 1975 - 76, Mr. Miller, you will let us know about it.

I am showing you what is actually a carbon copy on the stationary of the Government of the Northwest Territories entitled "Caribou Harvest - Baker Lake - 1976 - 77". This just simply lists two areas of hunting. Is that correct? An area south of Baker Lake for a total of 343 (7) and an area north of Baker Lake for a total of 1,416?

A. Yes.

Q. Is that correct?

A. Yes.

Q. Then I believe I have a complete copy of the Annual Report for 1977-78. I am showing you a photocopy of that -- of the complete copy of the Annual Report -- July, 1977 to June 30th, 1978, Wildlife Service, Government of the Northwest Territories, Baker Lake, NWT, prepared by Doug Stewart, Wildlife Officer, Baker Lake, NWT. And there is an appendix A. It says, "Caribou harvests, Baker Lake area, 1977 - 78 set out". The first category is the Kaminuriak area.

The total kill is listed at 615?

A. Yes.

Q. And would you read footnote number four, which is beside that figure?

A. "Unusually late spring and fall allowed hunters to travel to calving area by snowmobile in June, which increased hunting pressure on Kaminuriak."

Q. Now let's look at north of Baker Lake. What is the total there?

A. Five hundred and ninety-four.

Q. And under the headings "Schultz-Aberdeen area", what is the total there?

A. Three hundred and ninety-nine.

Q. Again, Mr. Miller, have you seen any statistics which differ from this document or the 1976-77 totals?

A. As I said before, unless I have the other table in front of me, I could not really answer.

Q. Would you agree with me, on the basis of the report that I have shown you, Mr. Miller, that in none of those four specific years have Baker Lake Inuit, according to the game officers of the NWT, hunted anything close to even say fifty percent -- has their kill been anything

like fifty percent taken from the Kaminuriak Herd?

A. No, not according to those statistics that you showed me.

Q. And, indeed, when we go back to 1972-73, we saw that it was fifteen percent was the actual tabulation for the Kaminuriak and in 1973-74, 9.6%.

MR. ESTRIN: And perhaps in argument we can say what the percentage is for the other two years.

This is not evidence, My Lord. I have worked it out.

THE COURT: While you are on this point, you did, if my notes are correct, give us the 1972-73 figures, you gave us the 1973-74 figures, then you made a point of saying 1975-76 figures were not available. And I was curious about 1974-75. Which category do they fall into?

MR. ESTRIN: That was an omission on my part, perhaps I didn't have that either and I didn't note it at the time.

What I have for that, My Lord, is the Annual Report for 1976, and perhaps I can show that to the witness. There is no breakdown per se, but it is a narrative statement.

THE COURT: I thought perhaps you overlooked something.

MR. ESTRIN: I did overlook that year, My Lord, but I don't have any statistics.

BY MR. ESTRIN:

Q. Would you agree with me that we are looking at the Annual Report for 1976, the NWT, for Baker Lake area, prepared by the Fish and Wildlife Officer?

A. Yes.

Q. And if we look at page 1 and we look at page 2 -- I am just going to read this, and you tell me whether I have read it correctly. It says:

"Two caribou herds, Kaminuriak and Beverly, have traditionally been important to the people of Baker Lake. Previous to the winter of 1975-76, the Beverly Herd received most of the hunting pressure. Hawkins, 1974, indicates that over 65% of the total annual harvest was from the Beverly Herd.

In the winter of 1976 and 1977 the harvest pressure was greatest on the Kaminuriak Herd due to unusual winter movement of the herd."

Is that what it says?

A. Yes.

MR. ESTRIN: That is as helpful

as I can be, given the reports available to me.

BY MR. ESTRIN:

Q. Mr. Miller, may we go back to your Affidavit, or certificate, if we may, paragraph six.

You say:

"The causes of death of the Kaminuriak Herd caribou have been and are:

1. harvesting by hunters
2. predation by wolves
3. other causes, eg. accidents, disease, starvation."

A. Yes.

Q. Are you, in saying that, denying that there is no way whatsoever that causes of death of the Kaminuriak Herd have been caused by low-level aircraft, including helicopter movements, exploration on the calving ground and post-calving ground?

A. I don't really understand your question. I think you misused a word there.

Q. Let me try again. I am asking you whether you would say that there is no deaths -- no causes of death involved in the low-level aircraft exploration activity that has gone on over the Kaminuriak calving- and post-calving grounds over the past ten years?

A. No, I would not say no

deaths. I would say very insignificant. Undoubtedly, the odd animal may have stumbled and killed himself while running across the tundra, as they do when chased by wolves sometimes.

Q. You were very concerned about harassment by aircraft when you wrote back in 1970?

A. I still am, sir. And if you will notice, all those statements are conditional.

Q. Are you saying that you just have not studied the relationship between them at this time?

A. No, sir, that is not what I am saying. I am still concerned about the problem. I think there is a point where, if allowed to go on -- if it exceeds a certain point, then it is a problem. But, from when it started until now, it has not been a serious problem.

Q. On what basis do you say that?

A. Well, on the availability of the literature, on my own experience, the experience of all my co-workers. If we can't say it, I don't know who can.

Q. You told us, though, that there have been no studies of the cumulative

impact of all this overflight; is that not true?

A. Cumulative impact is a consideration only when it is reinforced with other stimuli.

Q. Well, now, Mr. Miller --

A. An animal scared today, six months later is not going to remember that he was scared for thirty seconds today, and then add on to it another thirty second flight.

Q. Mr. Miller, didn't your own recent study show that caribou tended to, as you say, get more used to repeated aircraft passes but, then, if you repeated that a few days later, they seem to be as scared again as they were initially?

A. It is possible that they could have responded at the same level during the second set of passes or they may have responded at a lower level. It went both ways. It went up, and it went down.

Q. That was not what your conclusion was, Mr. Miller?

A. My conclusion was that there was no habituation, which is a totally different proposition.

Q. You, in your 1979 article, called for studies of the cumulative impact?

A. Of the cumulative impact?

Q. Cumulative effect of the aircraft overflights year-after-year?

A. I'm not saying that there is not need for more study. There surely is. As we stated, ours was only preliminary work.

Q. So you wouldn't -- without those studies -- eliminate low-flying aircraft, year-after-year, basically in the same area, as a cause of death, without those further studies?

A. At the level it has taken place in the past, I feel safe in saying it is highly unlikely it would have any significant impact on the animals of the Kaminuriak population. It may have influenced the few individuals but in no way could it have influenced enough individuals to be reflected in the observed decline of the population.

Q. Mr. Miller, I am a little puzzled why you were so concerned to make sure that in regard to the Peary caribou the aircraft stayed at least 600 meters, or close to 2000 feet, above those herds --

A. Because --

Q. For about half a year?

A. Because, as you noted, the study included musk-oxen. Musk-oxen happen to be

a much more sensitive animal to aircraft disturbance. You cannot isolate the flight of aircraft. The pilot cannot look down and say, "Those are caribou. We can fly lower over them," or "Those are musk-oxen. We must climb up."

The recommendations are for habitat on the High Arctic Islands where Peary caribou and musk-oxen occur in a close association.

Q. Well, Mr. Miller, you have the opportunity of making different recommendations for different animals.

A. How could you, sir, when Peary caribou may be within a half mile of musk-oxen. You can't isolate the two. You have to give the recommendations that are applied to the most sensitive animal under consideration, and that happened to be the musk-oxen.

Q. How do you say that, Mr. Miller, when you don't even tell us at the time of this study where the calving-ground, post-calving and rutting areas are?

A. Of what?

Q. Of the -- I guess it was these clumps of animals.

A. Well, as far as we know, musk-oxen have no calving ground or rutting areas. Peary caribou, if they have calving ground, they

probably have what you could refer to as calving areas, but they are not necessarily -- this has not been documented for sure -- the same in spacial restrictions as the calving ground of barren-ground caribou.

Q. Let me ask you, in relation to your statement in your Affidavit that there were only those certain causes of death and you omitted to mention any reference to aircraft overflights, whether you now want to withdraw any of your statements made in the 1970 article about harassment and problems of that and aircraft exploration can have on the calving ground and post-calving areas in relation to loss of newborn-calves?

A. No, sir, I believe those are all conditional statements and at some point in time, if the right amount of stimuli were -- the animals were exposed to the right amount of stimuli, you could have a serious situation.

Q. Have you made any study of the information available, I am told, in the public records in Yellowknife, as to the amount of low-level flying done by mining exploration companies in the Baker Lake area in the last ten years?

A. No, sir.

Q. I take it your only source

of information as to the specific of low-level flying is what you heard testified to at this trial?

A. No, sir. We have been exposed, over the last few years, to the map put out by IDS and the general conversations that have taken place at various meetings and -- you know -- it is a generalized briefing. But I think it allows me to make my judgment, yes, sir.

Q. But I am asking you specifically: you have not reviewed the information that has been produced in the government record and supplied by the company as to where they have been carrying on their activities, what kind of aircraft were being used at what altitudes, what speeds, etc?

A. No, I have not, as I said, personally reviewed those records.

Q. One other matter related to this that perhaps we should reflect on. I would like to draw your attention to your 1979 article, page 79. This is in your summary discussion. You have already mentioned, I take it, in your evidence in chief that cows bearing young for the first time are often poor mothers and apparently not psychologically well adjusted to the birth event?

A. Yes.

Q. And you go on to say here:

"Therefore, their behaviour is more apt to vary when harassed and they might either stay with or abandon their newborn calves more on an individual basis."

A. Yes, I believe I said that already.

Q. I don't know that we heard the last part. Thank you.

Now, I would like to refer you to paragraph 7 of your certificate. You therein expressed the:

"... opinion, on the information now available, over-harvesting of caribou by native hunters is the cause for the decline in the ... Kaminuriak Herd. In the absence of hunting, the size of the herd should have increased rather than decreased during the period 1968 to 1977."

A. Yes.

Q. You have studied this herd in some considerable detail over the last nine years; haven't you?

A. Yes, relatively speaking.

Q. And you have published some

of the leading articles on this herd?

A. Yes.

Q. And one of the things you have studied is population dynamics?

A. Yes, sir.

Q. And is it not true that there is an interrelationship of several factors with regard to population dynamics; for example, that you want to consider certain things. What things would you consider when you are looking at population dynamics?

A. Well, you are essentially interested in the rates of birth and death in the population, so you are looking at, as I said before, the number of calves present in the population, then the recruitment; in subsequent years, the yearling recruitment. You take the sex-age composition of the population. You are interested in how many adult females you have; how many adult males per adult females; the number of juvenile animals -- the yearlings and two-year olds and three-year olds that are coming up, by sex, into the breeding segment of the population and -- do you want me to go on?

Q. Would it be fair for me to summarize the factors that go into the population question as follows: recruitment, which is the result of rates of pregnancies and calf mortality;

harvest -- for the fourth one -- and what you have termed cohort composition. Five major factors. I believe those are some that you have written about?

A. Yes. Well, they are all part of it, yes.

Q. And the question of calf mortality. You have not specifically mentioned that in paragraph seven with regard to the decline in size of the Kaminuriak Herd. Wouldn't calf mortality have something very important to do with the size of the herd?

A. Yes, that is a very important parameter. That is the only source of new animals. So, if you have good survival in the calf crop, then the population can take that much more pressure. But the major cause of problem that can be assigned to calf mortality would be predation by wolves.

Q. That is just what I was going to ask you about. That was the observation back in 1970, reading from your 1970 paper, "Conclusions and recommendations". You say right at the beginning of that:

> "The principal cause of observed calf mortality during the calving and post-calving periods in 1970 was predation by wolves."

A. Yes.

Q. At page four, in the Abstract, I think it was put another way: "Pressures of wolf predation on the young of the Kaminuriak population may be a principal factor limiting the population's total growth. The importance of wolf predation on the young of caribou has not been fully realized. Predation by wolves is the most readily manageable cause of caribou deaths."

A. Yes.

Q. Still true?

A. Yes.

Q. We have heard evidence read into the record from the examination of Mr. Hornal, for the government of Canada, that there was a wolf bounty in the Northwest Territories at one point, and that it was removed. Do you have any information about that?

A. Not very many specifics. There was a wolf bounty, and it came off, I believe, sometime in the early seventies.

Q. On whose recommendation?

A. I really don't know. It would be a NWT matter.

Wolf -- I should mention that

the bounty system has been proven over the years to be, if anything, a form of rural relief but not a method by which a predator is controlled. Good bounty hunters are the best animal husbanders that you will ever meet. They assure that they will have a harvestyear after year. That is no way to solve the problem of wolf predation on caribou -- to put a bounty on them.

Q. But it wouldn't help to take it off?

A. Well, sir, wolves are fetching $400 to $500 a pelt now. If that is not an incentive to hunt wolves, a $25.00 bounty surely isn't either.

Q. What about the year it was taken off? How much were they fetching then?

A. I don't know. I would think over a hundred -- two hundred dollars.

Q. What year was that?

A. Early seventies -- 72 - 73. They have been running quite high in the seventies.

Q. If wolf predation is the most manageable aspect of the population growth, what have you recommended be done about wolves?

A. I recommended that we remove wolf from the calving ground.

Q. How would you do that?

A. Well, there are several

ways of doing it. None of them are sociably acceptable and most of them, for that reason, are not politically acceptable.

Q. But, Mr. Miller, you are not concerned about political considerations?

A. No, I am not personally; but my bosses are.

The most effective way that has been proven to date is by poison. If you are working on a relatively restricted area like a calving ground and you can catch the wolves before you have many cows producing calves, shooting from a helicopter probably would be an efficient technique. But --

Q. Did you say shooting from helicopters?

A. Yes, before the cows are on the calving ground. The wolves tend to arrive slightly ahead -- at least some of them do. If you catch the wolves when they are not in association with caribou, then you could use that as a management tool.

I would think the only practical way that I know of to get a high percentage of the wolves removed would be through poisoned baits.

Q. Do you know Professor Bergerud?

A. Yes.

Q. He is a biologist who studied the population control of caribou -- or who studied caribou --

A. Yes, he has.

Q. I am referring you to a paper entitled "The Natural Population Control of Caribou" by him. He cites some of your work at page 18.

"The cause of the low calf survival of the Kaminuriak was investigated by F. Miller and Broughton (1974) and Parker (1972 - 73) and D. Miller (1975). Of 53 dead calves found by Miller and Broughton, 34% had been killed by wolves, 23% had been abandoned by their does, and 11% were stillborn."

Is that part of your work?

A. Yes, sir.

Q. It says:

"They felt each wolf on the calving ground would kill at least one calf per day in June and July. They surmised that predation of the young may be a principal factor limiting the population growth.

D. Miller (1975) reported that the

proportion of calves killed by wolves in the winter was double the proportion found in the population."

He refers also to Clark (1971). Would that sound reasonable?

A. Yes, sir.

Q. He says:

"Parker (1972) attributed the 40% loss of calves over winter to wolf predation and concluded:

In view of the calculated ratio of one wolf for 114 caribou for the Kaminuriak population in the spring, it seems quite possible that wolves may control the population."

Would that be a conclusion that you would be prepared to agree with?

A. No, I don't believe so; not that they can control the population at that level, necessarily, but they definitely have an impact on the population.

Q. You say in 1978 that the principal clause of the loss of newborn calves was wolf predation?

A. Yes.

Q. I didn't see any inconsistency with what you said?

A. Between controlling the

population -- the recruitment is just one aspect. You have to consider the mortality of the adult segment of the population. If the mortality in the adult segment is excessive, it does not really matter in the sense that you can have a high calf recruitment and it is still going to end up in a negative situation.

Q. Are you saying that the major -- that there has been a great percentage more of the adult population being killed now than there was when you wrote your article?

A. Proportionately speaking, due to hunting, yes. There is a greater -- it is a greater percentage annually taken by hunters from the existing population than back in the 1960s, more than double, probably.

Q. I asked you, I think, about -- what did you say, "percentage" or "proportion"?

A. Percentage or proportion.

Q. You would say that is the same?

A. In a sense. It is a larger proportion or larger percentage of the existing number of animals being taken in annually by hunters now than in the 1960s.

Q. In paragraph seven of your Affidavit, your certificate, you say:

"In the absence of hunting, the size

of the herd should have increased rather than decreased during the period 1968 to 1977."

A. Yes.

Q. Wouldn't it be fairer to say that with the absence of wolf predation the size of the herd should have increased rather than decreased?

A. No, sir.

Q. Not at all?

A. Not at all, because wolf predation is incorporated within 7.2%.

Q. We are going to come to the 7.2%. So that wolf predation -- it does not matter how many wolves?

A. Wolf predation would result or should result in a higher recruitment, which will allow the population to grow faster if you can cut back on some of the other mortality in the adult segment.

Q. Well, I am not sure I understood you. But as long as Your Lordship did, I think that's fine.

Let's talk about your 7.2%. You say:

"During the period 1966 to 1977, the average annual mortality of the Kaminuriak caribou from causes other

than harvesting is estimated to be 7.2%."

Has anybody else stated a different statistic?

A. Well, maybe. I don't know. But I know that there are at least eight people who now agree on this statistic, and all biologists concerned with the Kaminuriak population.

Q. We are talking now about the average annual mortality rate from causes other than harvesting i.e. natural mortality. Is that what we are talking about?

A. Everything except hunting by Inuit.

Q. Whose figure is 7.2%?

A. I don't know whom we might give the credit to as an individual. It was a figure that was generated as a result of a meeting of the eight biologists on a computer program. I guess the computer programer could claim the figure as his, but we all shared it.

Q. But you don't know how the computer was programmed?

A. Yes, I believe one of those documents presented gives all of the conditions of the model.

Q. Was one of those people

who agreed to this figure Mr. Calef?

A. Yes.

Q. And I would like to suggest to you that he has written an article with Mr. Heard. Do you know Mr. Heard?

A. Yes, I do.

Q. And he puts the rate at 8.5%. Does that surprise you, since he is also a member of this committee?

A. No, not at all, because after he benefited from the wisdom of the other six biologists.

Q. Oh, I see. A little manipulation of the figures?

A. No manipulation whatsoever; just a matter of taking a better examination of the data that was available.

Q. Don't computers just spit these things out to the last place?

A. Well, you can have a range in any of these things. If you want to reduce the figures, you could put in your assumptions. But these assumptions are the best biological assumptions available at this time.

Q. At this time.

A. Well, it doesn't look like the herd has much time to wait for us to

improve our data collection.

Q. So I take it before you arrive at what are supposedly agreed statistics, you have meetings and discuss these figures and massage the data, and that kind of thing?

A. Well, "massaging" these data has a rather derogatory connotation.

Q. Well, you discuss it.

A. Yes, we discuss it in great detail.

Q. You know those documents that counsel for the government put to you this morning "Management Options -- Kaminuriak Caribou Herd -- May, 1979" and "The Decline of the Kaminuriak Caribou Herd -- May, 1979". Those were discussed in great detail before they were put out in this form?

A. Yes, you will find Dr. Calef's signature on them.

Q. And these discussions went on when this court case was pending?

A. No. Well, no, I have been involved in the court case all along. It couldn't have been.

Q. No, what I mean is these discussions took place just before the trial opened in Baker Lake?

A. I'm not sure, but I believe

that document was just finalized last week.

Q. Your name appears on it?

A. Yes, sir; in type.

Q. Pardon? In type?

A. I signed a draft version which appears to be reproduced there.

Q. When did you sign the draft version?

A. I'm not sure. I would think around the last of April or beginning of May. I'm not sure.

Q. And you knew that the court case was coming up in May at that time?

A. No, I don't think so.

Q. Well, now, Mr. Miller, your certificate is dated May 1st?

A. Okay. Well then the meeting -- I don't know. I still don't know if the meeting would have been before or after. I do not believe that I knew the court case was coming up at the time we were holding the meeting on this material. Possibly I did, but I don't recall that I did.

Q. And you knew as of May 1st, that you would be testifying in this action?

A. As of May 1st, yes.

Q. And indeed I suggest you had to know prior to May 1st?

A. Yes, that makes sense. I presented the figures from the session, so that session took place in April then.

Q. And some of the people that are also listed on this Caribou Technical Committee, Mr. MacFarland, he was in court, wasn't he?

A. In court?

Q. Not in the witness stand, but here one day?

A. Yes.

Q. And Mr. Thomas, he is here?

A. Yes, he is.

Q. Mr. Calef, he is here?

A. Yes, he is. But that work we produced there was not for Baker Lake. It was for the entire range of the Kaminuriak population -- all of the settlement, including the Dene of Northern Manitoba, was our concern.

Q. Yes, I appreciate that. There was another paper that came out of these meetings in April, wasn't there, entitled, "Wolf Management on the Kaminuriak Caribou Range"?

A. There should have been,

yes.

Q. One of the papers that was distributed yesterday -- and I will see if I can get a clean copy. I am showing you a paper, "Wolf Management on the Kaminuriak Caribou Range, May, 1979." It is three pages, signed by the same Caribou Technical Committee. Is that one of your papers?

A. Yes, it must be.

Q. I show you a copy of this for a moment to refresh your memory about it. Tell us generally the objective of that particular piece of paper.

A. The objective is, "to increase the caribou recruitment (the addition of caribou over one year of age) rate from 10% to 15%.

Q. And you discussed a methodology. There were three of them. You discussed two of them: poison and aerial shooting. Number three is aerial shooting, is that correct?

A. Yes.

Q. Shooting wolves from helicopters is considered as the most effective method, is that correct?

A. Not by me, no.

Q. But by the committee?

A. Yes, in general.

Their thinking in the social-political atmosphere.

Q. How is that going to be achieved? Through the use of fixed-wing and helicopter?

A. Well, it would have to be one or the other. I don't know. That would be the responsibility of the NWT Wildlife Service.

Q. It says here:

"Fixed-wing aircraft will be used to locate the wolves associated with caribou herds and the shooting and carcass collection will be accomplished from the helicopter."

Is that what it says?

A. If that is the method they decided, yes. The Canadian Wildlife Service is not a management agency. We are basically a research organization. We provide advice to these management agencies. They determine how they will actually carry out the various management options.

Q. Were there any social and political concerns involved in the preparation of these other two documents, D-10 and D-11?

A. I don't believe so, no. It is - social-political, as you may gather. I mean here it is common knowledge there is a large body of people in North America and the

world in general that would object to poisoning or shooting wolves from aircraft, since they would not have the appreciation of the wildlife management considerations and why it was being undertaken.

Q. Let's look at Exhibit D-11 for a moment. It is entitled, "Management Options, Kaminuriak Caribou Herd". On the second page is a discussion about management options. It is talking about wolf control and limiting human harvest. That is essentially the two methods?

A. Yes.

Q. I do not see anything in here discussing the possible impact of mining exploration activity and whether that ought to be considered as a method for the purposes of achieving an improved herd?

A. No.

Q. That is not mentioned in here, is it?

A. In the current light of the existing situation these were so paramount compared to any considerations that could be given to mining exploration at this time that they are not considered except for whatever regulations that the Department of Indian Affairs puts forth.

Q. But, Mr. Miller, you

yourself have now said that those kinds of regulations, based upon your study of Peary caribou, are implicitly inadequate. Your recommendations are for 600 meters, minimum, for about half a year, and you said and even that does not consider a lot of other things that ought to be known.

A. Well, it does not account for everything that ought to be known. But 1,500 feet, 500 meters, versus 600 meters in an aircraft -- most light aircraft do not have radar altimeters or the pilot does not constantly keep his map in front of him to read it, so when you look at the practicality to enforce the difference between 500 and 600, I do not see how you can say that the 500 is that much worse than the 600. If we get the 500, that is a big step forward in the right direction.

Q. Well, there are problems in regard to the navigation in the north at achieving certain altitudes, are there not?

A. Yes.

Q. One of those is that your altimeter has to be set at a rather arbitrary setting, not related to the actual ground?

A. Well, if you are using radar altimeter you just worry about how high you are.

Q. Let me ask you, in terms of the surveys of population that you have been referring to, were those done with radar altimeters?

A. Not ours. Some of the others could have been, yes.

Q. Do you know of any specifically?

A. Yes, I know a lot of surveys have been done with radar altimeters.

Q. Of the Kaminuriak Herd?

A. Yes, I believe so.

Q. Which one?

A. Renewable resources surveys, I believe, were all done with global navigation and radar altimeters.

Q. When was that, do you know?,

A. Not really. I think it's the 1974, 75, 76 period. I may be off a year there.

Q. That is the Polar Gas Study?

A. Yes, Renewable Resources Consulting Limited, I believe, was the title of

the outfit that did the work.

Q. But all your studies were done with non-radar type altimeters?

A. Yes, we can't afford the fancy aircraft.

Q. You will recall before getting to the topic of altimeters that we were discussing what were the important factors influencing the size of the herd, and I would like to ask you if you are aware of this statement by Mr. Heard and Mr. Calef:

> "Clearly the Kaminuriak herd will continue to decline unless there is a decrease in the mortality rate or an increase in recruitment. It is unlikely that the birth rate could be increased, since reproductive rates of barren-ground caribou are relative constant."

Do you agree with that?

A. Well, they are constant within a fairly wide percentage range, actually.

Q. I don't understand that.

A. I mean, "constant" -- you can find examples in the literature of reproductives rates that probably vary from 75 to 95% in round figures. There is a constant

probably about the middle of that which would more or less be considered a constant.

Q. It goes on:

"Recruitment can only increase if calf mortality is reduced. This could probably be achieved most easily by predator control."

A. Yes.

Q. Do you agree with that?

A. Yes.

Q. It goes on to say:

"Wolves are undoubtedly the most important predator of caribou and in caribou ranges where wolf numbers have declined, recruitment has always increased. Adult mortality is also likely to decline if wolf numbers were reduced."

Would you agree with that?

A. If wolf predation is the most stringent mortality factor, yes. If wolf predation is not and some other factor is more stringent, then the adult segment will not necessarily increase, as in the example of the Kaminuriak population.

Q. Well, you don't know that, though?

A. I think I do, sir.

Q. You yourself wrote in 1970 that if you dealt with the wolves you would have the whole thing under control?

A. You keep mentioning recruitment. Recruitment are the calves. You are talking only about those individuals coming into the population. That has no consideration with the adult segment of the population.

Q. Would it not be fair to say that aircraft flying at 150 feet over herds of little animals might have something to do with whether or not they grew up to be big animals that you are concerned about?

A. Only, as I have expressed before, if the period of exposure is long enough and associated with reinforcement stimuli, then, yes. But not to date, I don't believe.

MR. ESTRIN: My Lord, if it is convenient, may I at this time take a recess?

THE COURT: Yes. I hate to give you a chance to recharge your batteries, though, to go on forever.

Anyway, ten minute recess.

---Short recess

---Upon resuming after recess

THE COURT: Mr. Estrin, I

hope you are not entirely refreshed, but carry on.

MR. ESTRIN: I'm worried, My Lord. I don't think I will be a great deal longer.

THE COURT: It doesn't matter.

BY MR. ESTRIN:

Q. Now, Mr. Miller, one of the things we discussed right before break was reproduction rates. I think you said that -- I referred you to a statement made by Mr. Calef and Mr. Heard about being a constant. I think your evidence was that it could vary over a range.

A. Yes.

Q. Could you give us what that range might be? First of all, tell us, what are we talking about?

A. I assume what they are talking about is so-called fertility rate in female caribou, and that this rate varies with age but usually when biologists look at the over-all rate they just lump the rate for the population and the young females are included in with the old females. And I would think that if you have a rate that exceeds 80%, then you could assume that you had a normal situation in that population.

If you had a rate below 80 - 85%, then you have some kind of problem in the reproductive rate.

Q. And can this reproductive rate, in your experience, vary, say, as much as between 60 and 90%?

A. Well, I guess it's possible. I don't recall -- 90. It could go as high as 90, for sure. The 60 might be an extreme figure for some populations from Alaska or Canada that has been reported. I don't know. I can't recall if it goes that low.

Q. What figure would have been plugged into this computer for the Kaminuriak?

A. You would have to look at the set of assumptions to get that figure.

Q. But isn't that rather important?

A. Very important.

Q. Who would have plugged it into the computer?

A. The programmer.

Q. Where would he have got that from?

A. From us or from the group, collectively. There would have been

agreement on that figure.

Q. What was that agreement?

A. You are asking for the figure?

Q. Yes.

A. I told you, I would have to look at the set of assumptions for the exact value.

Q. Couldn't a change in that figure substantially influence the outcome of what the computer was saying as to the decline or rise in the Kaminuriak Herd?

A. The percentage of fertility rate?

Q. It would be a major factor?

A. Well, it's a factor. I don't know if it is the major factor. It is an important factor. It would obviously alter -- increase or reduce the number of animals being born.

Q. When was this computer model established?

A. The programs were run to produce the curves that are in the back of these documents -- the two documents we had earlier -- I believe must have been run within the last couple of three weeks. I'm not positive

on that.

Q. You signed your name to that?

A. I signed my name to a draft. That is a typed name on there. I signed my name to a draft, as far as I can tell, containing the same information that these documents do.

Q. That draft would have been part of this. You would have seen some kind of curve projection?

A. Yes.

Q. And the curves in the projection as to population would be importantly influenced by the reproductive rate?

A. Well, they would be influenced, yes.

Q. You don't know what figure was plugged in?

A. I don't recall it, no.

Q. Wouldn't you want to know whether it is a valid one?

A. I don't believe that any of my friends would change it after I agreed to it.

Q. Aren't you one of the people with the most expertise on this study --

what the reproductive rate is?

A. No.

Q. Who is?

A. Not by any stretch.

Q. Who is?

A. Well, for the Kaminuriak population you would have to accept Dauphine's findings as the best to date. Perhaps now I have to qualify it. The NWT may present a different argument. They may say that their work would be better.

Q. Mr. Dauphine is not part of your committee?

A. He is now administrator.

Q. He is administrator?

A. Yes.

Q. Where does his figure come from? Is it published?

A. It is in that report you have, yes.

Q. Are you saying the figure that would be plugged into the computer was Mr. Dauphine's?

A. No, I'm not saying that. I am telling you that I can't recall what the exact figure was. I don't know if any adjustment was made on Dauphine's figure. I am saying I can't recall.

Q. You can't recall a discussion about it?

A. No, sir; all I can say is the figure that was agreed upon was agreed upon by all present.

Q. You can't recall that discussion specifically?

A. No, sir.

Q. One of the factors, you agreed, importantly related to population dynamics was, of course, calf mortality and that could be influenced by two major factors, in turn: wolf predation and harassment.

We have already discussed wolf harassment.

A. Well, I just cannot agree that -- that the harassment is any kind of consideration in the model that we presented.

Q. I see. Are you saying that the computer model that you presented that went into, say, these exhibits -- D-10 and D-11 -- didn't take into account harassment as a factor at all?

A. No, it did not, because I don't believe that anyone present -- I certainly did not believe nor do I think anyone else present believed that harassment was or has been a consideration to date.

Q. That is despite your own articles -- 1970?

A. As I told you, all those statements are conditional. All things that could happen.

Q. Do you want to withdraw your statement made in 1970?

A. No, sir; I stand by them all.

Q. I am thinking particularly about the one about aerial photography on page 17, which I have already read from, that low-level flying was necessary for it, which was a serious form of harassment. That sounds like a pretty definite statement to me.

A. I believe it could be a serious form of harassment, as I stated there — some effect. I was studying mortality of new born calves, so the last thing I wanted to do was to introduce any possible problems. I don't know whether it would have been a problem or not. But, I surely could prevent it by demanding that the photography be flown higher.

Q. Even though you say it was so speculative but yet it turned out that that was most unsatisfactory -- for flying higher was most unsatisfactory or mostly unsatisfactory for your compositional count?

A. Yes, we had to disregard many of the photographs and work with a much smaller sample.

Q. Tell me, all these people that are doing all these surveys of population, what altitude did they fly at?

A. You mean?

Q. Of the counts.

A. Counting.

Q. Transect surveys.

A. I would think -- I am not sure of the other people involved -- that most counts are done at approximately 500 feet above ground level.

Q. Have you done any at 300?

A. Survey counts? Not myself, no.

Q. You know of any?

A. It's possible some have been done at 300. I don't know of them. I can't say if they were done. I would feel safe to say that they were not done by the Canadian Wildlife Service.

Q. If surveys were flown at 300 to 500 feet over the calving ground post-calving ground to count caribou, would you agree that these could constitute harassment in your

terms?

A. A degree of harassment which would be very slight because, as I stated several times before, they would be very brief, there would be no reinforcing stimuli; therefore, there is no reason to believe there would be any long-term effect derived from the overflight.

Q. Tell us how spaced apart would these flights be? As I understand it they fly grids?

A. Four miles. Well, it depends on what you mean by grids. They are transects. As I say before, if you wanted 25% coverage, they would be four miles apart. It is rather hard to conceive of an aircraft four miles away causing any problem whatsoever.

Q. No matter what altitude it's flying or what is below?

A. No matter the altitude. If you are four miles removed from the animals, it is the diagonal distance you are concerned with, not the altitude.

Q. Are you saying that is the narrowest distance between transects that are flown for counting caribou -- particularly Kaminuriak?

A. Canadian Wildlife surveys, yes.

Q. There have been lots of others? NWT?

A. NWT surveys, yes. Right now I am not familiar enough with their techniques to be able to say exactly. I would say their intervals are as wide as that or even wider. But they may have altered them for some reason. I don't know.

Q. Are there not other forms of activities by caribou biologists who are trying to study these animals that in the view of Inuit constituted harassment? I am thinking of the tagging programs. You were asked about that?

A. Yes, sir. They --

Q. Go ahead.

A. They believe it constituted harassment.

Q. Indeed, you were involved in tagging programs, you have testified. And there were tagging programs in the Schultz-Aberdeen area in the early seventies that you were involved in?

A. Yes, sir. Sixties.

Q. Yes. You have to use two men to actually do this, don't you?

A. To catch the animal. One man catches the animal and holds him, the other man puts the tag in.

Q. And Mr. Ungungai told me that he was involved in one of these and it was his duty -- you know Mr. Ungungai?

A. Yes, very well.

Q. To use the metal hook and get the caribou over to the boat, and then he had to hold the neck while somebody else put on the tag. That is the usual procedure?

A. If it is a male, you usually hold the antlers. If it's a female, more than likely you would grasp it gently under the chin and hold her against the canoe.

Q. And he told me that on one of his last experiences doing this that such force had to be used with the particular animal that its neck was broken?

A. Probably because the Inuit driving the canoe was going too fast. But --

Q. Oh, it is always an Inuit driving the canoe --

A. Yes, always.

Q. Too fast.

A. -- Every tagging operation I have been on with the NWT.

Q. It is not ordinarily a strenuous exercise. Ordinarily the caribou really like to be grabbed around the neck?

A. No, I never said that. I told you that there was a degree of stress associated with it. I also pointed out the authorities that Dr. Geist referred to stated that it was the least stressful form of capture technique available.

Q. You used some other techniques, haven't you, I understand, for trying to determine where caribou go. Were you involved in a study recently where you dropped bags of green florescent paint on caribou from about fifty feet above them?

A. No, that is a rather simplistic -- are you talking about Peary caribou?

Q. Yes.

A. Well, we were a bit more sophisticated than dropping bags of green paint on them.

Q. Oh. Tell me about that.

A. Well --

Q. It is my understanding that there are some caribou with florescent green paint roaming around out there.

A. No, the paint only lasts until the hair molts off in the summer season, approximately. We dye-sprayed the animals in April, and by July they were molting and no colour remained, so we couldn't determine them from

the others.

In the spring, I believe, of 73 we tested aerial spray techniques out of a base at Mole Bay on Prince Patrick Island. The technique was used on sheep and several other types of ungulates, I believe. It involves a tank inside the aircraft that is mounted inside the aircraft behind the front seat, much on the idea of a crop-dusting operation, actually.

The tank has a capacity of approximately 50 gallons. The dye is mixed in solution of warm water and 10% alcohol to allow the dye to penetrate the hair -- cut the lanolin, so to speak.

A group of animals were located, and the pilot would make a low pass over the group and attempt to release his load as he ran ahead of the animals and the spray would come back and colour the animals. Appropriately, the animals on Prince Patrick were coloured green and animals on Eglington Island were coloured red.

We marked, I believe it was, approximately 200 animals on each island in 1974. In April of the year, I believe, and then we returned to do our summer surveys -- and returned a bit early in June -- to attempt to relocate dye-marked animals. Which is typical of the Arctic,

it happened to be a bad spring and the weather was unfavourable for flying, so we didn't get that much flying done. But we were able to relocate dye-marked animals. Animals marked on Prince Patrickhad travelled to the eastern side of Melville, a distance of several hundred kilometers -- straight line distance. And many animals marked on Eglington travelled to Prince Patrick. And that is some of the type of information we were after in our work that allows us to see the seasonal distribution of these islands and to document the fact that there was indeed large-scale inter-island movement of Peary caribou. A fact that had not previously been documented.

Q. Just to finish that off, very briefly, my understanding is this was done by low-flying aircraft -- flying quite low -- and, indeed, quite slowly, a stall-speed of maybe 50 miles per hour?

A. Yes, about that I would think. One pass. No reinforcement stimuli.

Q. You mentioned in your report collecting, say, a thousand caribou?

A. Yes, sir.

Q. Does that mean killing them?

A. Yes, it does. Shooting.

Q. You did that over the

period of two years?

A. Three years.

Q. And do you know -- I gather you do know what percentage were male and female?

A. Well, not off the top of my head. But the majority would have been female, I would think. I don't recall. I would think it is something like a 60/40 split.

Q. Is that what you got from a random sampling?

A. As random as it could be under the technical problems associated with the technique, yes.

Q. And --

A. And I should mention that it agreed very well with Parker's information at the time, who was doing the actual counts of the population.

Q. And in fact that relates to another area that might affect the population, and that is this fact that you have written about the cohort composition factor?

A. Cohort composition -- well, you will have to explain to me what you mean.

Q. Let me explain to you what I mean. The male/female composition of each

yearly cohort as it moves through its life span, for example, all the animals born in 1970 are cohort, as all are born in 1971 another cohort?

A. Yes, that is correct.

Q. And you have written about that?

A. Yes.

Q. To do that study you killed 943 Kaminuriak caribou and 56 Beverly caribou?

A. Those are the same animals lumped under the thousand, yes.

Q. And you say, I believe, in that study that once it becomes established that there are enough breeding males and rising breeders in the population to service the breeding females, the most important consideration becomes one of age distribution?

A. Yes, I believe I said that.

Q. That factor was not mentioned in your certificate as influencing the population?

A. I mentioned sex and age composition. I believe so.

Q. I think you described it generally as something you addressed, but it is not mentioned in your certificate as one of the

matters that has influenced the decline of the herd?

A. I'm not sure just what you mean by that. I don't believe there is any information to suggest that the sex and age composition of the population is terribly altered or skewed in any direction, that it is quite normal as caribou populations go.

Q. Can you tell me what studies you have made recently -- since this study was done about when? In the early seventies? The detention survey?

A. No. Those would be 66 through 68, and calving ground in 70.

Q. What studies have been made since that time as to the actual composition of the male/female ratio in the Kaminuriak?

A. That information would have been obtained by the NWT Wildlife Service.

Q. And that information would have been obtained from kill statistics?

A. No.

Q. I see. From how?

A. From the aerial surveys we have been talking about.

Q. From the aerial surveys?

A. Yes, as Parker did in 66 or 68.

Q. And what do those surveys show -- the recent surveys? Any different from yours; do you know?

A. I can't -- I couldn't give it to you verbatim. I don't know.

Q. Has there been a change or not?

A. In what?

Q. In the male/female ratio, for example?

A. I don't know. I would have to see the figures. I mean I have to see the figures to give you an exact ratio. It is not a significant change, no.

Q. You are assuming?

A. Well, I am more than assuming. I know it is not significant, based on what I can recall from what went into the computer program.

Q. Do you know what went into the computer program at this time on that factor?

A. Not the exact value, no. I mean you have it all in that document. Just look at that document.

Q. I don't have the document, Mr. Miller. If you have it, I would be pleased to look at it.

A. I'm sorry about that.

Q. That is one of those internal, unpublished ones?

A. It is in the court evidence.

Q. Are you saying this three page document is that one?

A. I thought it was one of them.

Q. You are referring to this "Management Options" page two, the middle of that page, perhaps, where it says,

"The Management Options illustrated in figure 1 are based upon a mathematical model (Mike Kingsley). Very similar results were obtained using a computer simulation model (John Smith)."

A. Yes.

Q. And have those models -- are those models here?

A. Pardon?

Q. Are those computer simulation models here?

A. Well, the curves are the product.

Q. Yes, I think we are all aware of that.

A. Oh.

Q. I have been asking you about what you plugged in to get those curves?

A. Well, what it says there.

THE COURT: Mr. Estrin, the witness indicated that he didn't think the male/female age make-up was abnormal in the herd.

Does his credibility turn on a close examination of his recall of the figures that went into a computer?

MR. ESTRIN: No, not necessarily.

THE COURT: No.

MR. ESTRIN: Not necessarily. I think there is something as to whether or not he was aware of what went into the computer.

THE COURT: Yes.

THE WITNESS: Well, I wouldn't put my name on the draft if I had not been aware of what was going in.

BY MR. ESTRIN:

Q. Do you recall a specific discussion about that factor?

A. About what factor?

Q. About the relationship between breeding males and rising breeders on the one hand and breeding females?

A. All I remember, in a general

sense, is that there was no reason to believe, from data that were available or are available, that there was any problem in the sex ratio in the breeding segment of the population, that it was quite a good ratio and you would expect good production at the existing ratio.

Q. Well, aren't you trying to suggest that the Inuit like to hunt male caribou over female?

A. No. No.

Q. Oh, you're not?

A. When did I suggest that?

Q. I am sorry. If I misunderstood you, I'm sorry. I thought that is the understanding I got.

A. No, just the opposite. I would think they prefer to hunt females for a larger part of the year than males.

Q. So there is not going to be a problem about -- you are not concerned then about Inuit hunting in terms of this disturbing this relationship between males and females in the population?

A. It's always possible, by chance, if no other reason, that you could disturb the relationship.

Q. From what you have seen, that has not happened?

A. Not to date, no.

Q. I would like to refer you to in your Affidavit to what you have to say about population figures. You say in your Affidavit:

> "The estimates of the size of the Kaminuriak Herd over the period 1948 to 1977 has shown a decline in the size of the herd."

And you set out the sources. How reliable would you say those figures are that they could be assigned a plus or minus 15 to 25% confidence interval.

Does that mean they could be higher or lower by 15 or 20%?

A. We know from the distribution of the animals and the size of their home range that the estimates -- the early estimates were minimal estimates, at best. There is no serious consideration on our part to take away from that value.

Q. What I am asking you specifically -- let's talk about the figures for 1974, 1975 and 1976. Are you saying that they could vary 15 to 20% either way?

A. I am saying it is a mathematical possibility, yes, that they could vary that much. But, in a sense, we have 74 and then 76

and 77 estimates that are so close that that argues strongly for the trend and the magnitude of the trend being real and, as you know, the range has shrunk considerably again in the seventies.

Q. Well, you told us that.

A. Well, your Inuit told you that too.

Q. About the southern portion. The Inuit tell us about the northern portion has shrunk, and you do not reflect that.

A. We told you there are virtually no caribou in the southern portion except in this last year when there was a report of some ten thousand that moved into --

Q. Let's stay with the figures, if we can, for the moment. You are saying that because of these three surveys that you referred to -- 74, 76 and 77 are approximately in the same order of magnitude that that shows the trend and regardless of any errors in calculation, you are confident about them?

A. Yes, we are confident about the direction of the population declining.

Q. I note that they are all done by the NWT Wildlife Service?

A. Yes.

Q. The last three years you have referred to?

A. Yes.

Q. Wouldn't it be fair to say that certain assumptions were made in the counting procedure in one year that were reflected in the other two years, that you could have the same kind of assumptions and, accordingly, if there was an error creeping in one year that it would creep in the other years?

A. You will have to back up a little on all those assumptions.

Q. All right. Let me ask you this. These surveys that are made. Do you know of any survey where every caribou in the whole population of a particular herd was reported to be counted? Or do you try to only count a certain segment of the population, and then do some estimations and calculations?

A. As I said to you, sir, surveys are only a percentage coverage of the total area. You could not begin to count all the caribou unless you were defying all the so-called rules of distribution. You are only surveying a small percentage of the total area, so it is, as stated, an estimate. Those are estimates, not total counts.

Q. Can you tell me whether these estimates -- when these were made, were they made from surveys of the calving-ground, the

post-calving ground or spring migration?

A. Well, you would have to check with the NWT, but I believe estimates are based on calving-ground surveys.

Q. Let me ask you this: when you do a survey and you try to come up with a population estimate based on a survey of the calving-ground population, is it not fair that one of the things that goes into that is an assumption that, say, 80% of what the count constitutes is the breeding females or maybe some others somewhere else?

A. That would vary with the year, depending on the percentage of young animals on the calving ground.

Q. So where does this assumption of 20%, or whatever the figure is plugged in -- who determines that?

A. What 20% is that?

Q. Well, my understanding is, for example, if you do a survey in a particular year, you purport to count so many caribou on the calving ground, that you assume that what you have counted is say 80% or so, or some percentage of the animals that are actually available.

A. Well, you will have to account for your missing adult male segment, yes, and some non-breeders.

Q. So you have assumed something on top of what you actually count in order to come up with the total population estimates, is that fair?

A. Yes, that's correct. But those assumptions are based on surveys done at other times of the year to more or less verify the assumptions.

Q. Is it not true, Mr. Miller, that for reasons that caribou are counted on the calving grounds is because it has been a more reliable method of trying to catch all, basically -- to catch the population virtually together at one time?

A. Just the females.

Q. Yes. But that is one primary reason for doing a calving-ground count?

A. Yes, to determine the number of females in the population and how many are producing young.

Q. Then, if you want to do a more accurate study of the total population, you do a study of the post-calving ground?

A. No, that would not be a good time to look at the male segment because they are mixed in at varying degrees in the different post-calving aggregations.

THE COURT: Mr. Estrin, I am really mystified as to where this is all leading me. I rather thought that everybody was in agreement that the caribou were getting more scarce and that they were fewer and the real issue in this case is why; not the accuracy of the technique of the counting or arriving at the conclusion that they are getting scarce.

MR. ESTRIN: Well, My Lord, I think I should say that we take the position that these figures are unreliable.

THE COURT: Yes.

MR. ESTRIN: And there is some kind of allegation or assertion being made by the government and perhaps by other Defendants that insofar as there is a decline, it is the fault of Inuit and native people.

THE COURT: Well, the hunters.

MR. ESTRIN: Yes, hunters. All right.

THE COURT: I presume mostly Inuit. Not all, but mostly.

MR. ESTRIN: But I think that since, My Lord has said that this whole issue is, among other things, one of the things to be considered in this case then, accordingly, we feel it incumbent upon us to try to demonstrate that there is no decline.

The essential complaint of the Inuit at Baker Lake is that the herds that they used to hunt are no longer available to them in the places where they used to hunt; we are not alleging there has been a decline in the population per se and we certainly do not want to be blamed for an alleged decline in the population by our over-zealous hunting activity. So that is the area I am going.

THE COURT: Well, you have certainly been over, and over, and over the methodology by which these counts are made.

MR. ESTRIN: Well --

THE COURT: And you do not seem to be getting substantially different answers as you retrace your steps.

MR. ESTRIN: Well, My Lord, actually I hadn't really got very far at all with respect to the area of the methodology of the counting. I think I dealt with methodology of other aspects of harassment and what goes into the population dynamics.

But we are now into a separate area, if I may make myself clear, and I am sorry if I haven't been. I am referring specifically to that portion of Mr. Miller's Affidavit that relates to population and the alleged decline.

And that, I think, is a separate type of area than what factors caused that decline. He says that there has been a decline --

THE COURT: Okay. You made the point. Carry on.

BY MR. ESTRIN:

Q. Now, you know Mr. Calef?

A. Yes, I do.

Q. Who is he?

A. A research biologist with the NWT Wildlife Service.

Q. And he is one of the people on your Caribou Committee?

A. Yes, he is.

Q. And I think that his writings on caribou and particularly population of the caribou in the Northwest Territories would be fairly reliable reading material for people like yourself?

A. Published material?

Q. Things that he has chosen to put in writing and he has given public symposiums to yourself and other people with your qualifications?

A. No, I would not agree. I would not accept certain writings that are

imbedded in internal reports.

Q. Were you present, for example, at the symposium on "Parameters of Caribou Population Ecology in Alaska," in Fairbanks, in 1977?

A. Yes, I was.

Q. Is it not the case that Mr. Calef presented a paper at that symposium on the population status of caribou in the Northwest Territories?

A. Yes, he did. That is a published document now.

Q. So you would feel that he was quite qualified to make the statements he did in that regard?

A. Possibly. I haven't examined each statement that he made in detail.

Q. Let's look at what he had to say. He delivered this at the symposium held November 17 and 18, 1977. I am just going to read from the bottom of page 8:

> "The Kaminuriak herd is the most intensively studied in the Northwest Territories and censuses have been carried out in six of the last seven years. The censuses have yielded quite variable and inconsistent estimates of the population size (see

section on problems in population estimation)."

He relates that the last two estimates -- 44,000, Hawkins and Calef, in preparation and 45,000, Heard, in preparation, would have certain confidence ratio or interval. In the case of the 44,000 by Hawkins and Calef -- that that could be varied by as much as plus or minus 34%.

A. Well, that's possible. If he said it, there is no reason to disbelieve it. He must have done the statistics on it.

Q. You would accept it?

A. Well, I would have to, unless I reworked the statistics associated with the survey and --

Q. And he said with regard to this 45,000 survey -- the 45,000 Heard, in preparation, survey, could vary by as much as plus or minus 23%. You have no reason to quarrel with that?

A. No.

Q. Let me ask you this: if we take a range of 34% and let's say add that on for a moment to 44,000, we could have an additional how many animals, approximately?

THE COURT: Fifteen thousand.

MR. ESTRIN: Thank you.

THE COURT: More or less.

BY MR. ESTRIN:

Q. And, Mr. Miller, if we added 15,000 on to 44,000 we would have a population of the Kaminuriak Herd based on that survey of approximately 60,000 is that correct?

A. If there was any justification for doing it, yes.

Q. And if we had a sixty thousand population figure, that would coincide very nicely with Mr. Parker's estimate in 1968 of 63,000; isn't that true?

A. Everything that we know of the population argues against such an adjustment.

Q. Everything you know about the population that is based on these recent surveys?

A. Based on determination of ranges, based on information from the Inuit of the animals not occupying areas that they used to occupy, range shrinking, the Indians in northern Manitoba having no caribou on their range for several years -- the range has shrunk. Range shrinks because animals are reduced.

Q. Mr. Miller, all of that is equally consistent with animals changing their range, isn't it?

A. Where? What population did they move to?

Q. Well, you tell me.

A. They didn't move: I'm telling you: they died.

Q. Isn't it true you have discovered some new populations north of Baker Lake?

A. No. No. No.

Q. No? Isn't that the claim --

A. There have always been caribou there.

Q. "Always"? Is that in the statements of the people who have studied those herds in the last few years?

A. I don't know. But I know you can check the NWT files back to the fifties and you will find the Wager Bay population existed, coastal caribou from Alaska, across, existed -- in much greater numbers than they do today.

Q. Let me just read you what Mr. Calef says following what I have just read to you. He says:

> "The population has probably declined since the late 1960s when it was estimated to number 63,000 ..."

He is not very certain as of

that date. Are you more certain than he is?

A. Yes, I am.

Q. I see. You have done more work on that, have you?

A. Yes; on the population in general.

Q. I take it you disagree?

A. Well, I disagree with the qualification "probably". I think there is no doubt that the population has declined considerably since 1968.

Q. Let me ask you if you disagree with some other comments made by Mr. Calef in his paper under the heading "Problems in Estimating Caribou Population".

He says:

> "Unfortunately, surveys carried out to estimate caribou populations in the Northwest Territories have not been either accurate or precise enough to allow the detection of anything but very large changes in population."

Would you agree or disagree?

A. Basically true. You would have to define "large". But you won't detect a thousand animal change.

Q. He is talking about very large. What would you say is a very large change?

A. Five thousand.

Q. I see. And he goes on to say:

"The unreliability of these surveys stems from the following factors:

1. Sampling procedures have not allowed for varying densities on the calving ground.
2. Unmeasured bias has resulted from observers estimating rather than counting large groups of caribou.
3. Unmeasured bias has result from observers missing animals.
4. Corrections for missing age and sex classes applied to calving-ground estimates were often done without accurate classification of animals on the calving-ground or in the whole population.

MS. KOENIGSBERG: Excuse me, My Lord, I wonder if Mr. Estrin could let us know where he is reading from.

MR. ESTRIN: Page 11 and 12.

MR. GRAHAM: Of what?

MR. ESTRIN: A paper entitled "The Population Status of the Caribou in the Northwest Territories," Mr. George W. Calef, November, 1977.

MS. KOENIGSBERG: It is a different paper from the one you have been reading from.

MR. ESTRIN: No, it is the paper I've been reading from Mrs. Koenigsberg. I don't have a different one.

THE COURT: Is it a paper that you have provided to the other people?

MR. ESTRIN: No, I have not provided this paper. It was just provided to me.

THE COURT: All right.

BY MR. ESTRIN:

Q. Mr. Calef has written about these four sources of unreliability of the surveys, and I would ask you whether you would agree whether those are four factors leading to the unreliability of caribou-counting surveys?

A. They are all possibilities. I believe that if you check with Mr. Calef you will find that those were all surveys done in the early seventies and deleted from consideration that is presented in my Affidavit.

Q. So they do not apply to the three surveys in 1974-76?

A. I don't believe so. To my knowledge they were early 1970 surveys, basically, that suffered mainly, in my opinion, from inexperienced wildlife officers. It is no

reflection on them. They just happened to be new in their job and were ordered at the last minute to go out and do surveys, without proper training or preparation.

Q. You would be surprised, I take it, if Mr. Calef said, with regard to the 1974 survey of the Kaminuriak, that it suffered from the same problem: that is the number of caribou was estimated rather than counted accurately.

"It appears to suffer from the same problem, although perhaps not to the same degree."

A. Well, I couldn't question it until I have examined the data related to the survey. I would have to look at the raw data before I could pass judgment on it.

Q. You have not done that?

A. Not 74 -- well, not any of the NWT surveys. I assume that since they are biologists -- Dr. Calef and Mr. Heard -- have taken over the survey work, it is now being done properly as best it can be done.

Q. I thought you said that you know more about the surveys than Dr. Calef?

A. No, I did not. I said I thought I knew more about the Kaminuriak

population than Mr. Calef.

Q. Not more about the estimates. You don't know more about accuracy of estimates?

A. I am thoroughly familiar with all the principles of survey techniques and all of the shortcomings and possible errors inherent in the techniques, and you could present these surveys to caribou biologists around the world and I'm sure that we would get them back and -- this paper was accepted by a learned group, so we would have to assume that they believe that there are errors inherent in the techniques, and we would make due consideration for those errors.

Q. If there is error, Mr. Miller, in the surveys, what would your view be as to whether or not they would result in underestimates or overestimates?

A. Well, the basic assumption that I would make is they can't take away the animals you see, so you have a starting point. If you see twenty thousand, you know you have twenty thousand. Then you have to be looking at your total store of knowledge about the population that you are dealing with, such as significant reduction of range, difference in movements of the animals, wintering and so forth.

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That will lead you to your conclusion, or should, about whether it is more likely to be over or under.

If population was as high as it was in 68, we would still have caribou in Manitoba and throughout the entire Kaminuriak range that existed at that time.

Q. Well, I understand that. He said that earlier. What I am asking you is, as you said, you know what you have seen, but if the number you counted that you have actually seen does not correspond with the number that you think you have, then you have, would you not, an underestimate; and you would want to add something to the number you had seen to come up to complete your estimate?

A. Would you repeat that, please?

Q. If you see a certain number of caribou in your survey, and the number you have come up with does not correspond to what you think you should have based on the previous year and other things that you might want to take into account, that would tend to be an underestimate?

A. The estimate is based on a sound mathematical procedure, supported by statistical analysis. It is not just a guess off

the top of the head.

Q. I am not suggesting that, but I am suggesting --

A. It is a quantitative measure of what is estimated to be there. It is based on the number of animals seen on the transect. I mean you wouldn't calculate an estimate lower than the animals seen.

Q. You don't supply corrections for missing age and sex classes, for example?

A. On a calving ground, yes. Those corrections are determined by surveys at other times of the year to get what percentage should be applied to the missing segments -- sex and age segments.

Q. When you apply past survey results to get your corrections, is it not possible that they can be applied inconsistently? That is what Dr. Calef suggests?

A. Applied inconsistently?

Q. Yes.

A. You mean in the same survey?

Q. When you are applying past survey results -- I'll just read to you what he says:

"In many surveys, no classifications

were carried out at all on the population at the time of the survey. In these cases past survey results were used for corrections, and these were applied inconsistently."

A. I don't know about that. That was done by the NWT. I would think it refers mainly to those surveys done in the early seventies.

Q. Well, he says -- for example, the Machanko 1974 sighting, Dauphine, 1971. It assumed that adult females comprised 57% of the spring population, while Calef and Hellmer and Calf and Boxer cited Parker -- he stated that breeding females comprised 43% of spring population.

A. What difference does that make in the total estimate?

Q. What difference would it make?

A. That's what I am -- I don't know. That is what you would have to look at to see how important that distinction is.

Q. Well, Dr. Calef, in 1977, at the symposium at which you were present says:

"Clearly, the inaccuracies and biases described above must be ... "

And I have not referred to them all --

"... must be remedied before our caribou censuses are reliable enough for management purposes."

A. Well, that's what I think he attempted to do in 1976 and 77.

Q. So everything before that is suspect?

A. No, I don't believe so. I think the 78 survey is quite reliable.

Q. I said before that.

A. Sixty-eight. Excuse me.

Q. It is quite reliable, is it?

A. Within the confidence that arose that we discussed previously.

MR. ESTRIN: If I may have a moment, My Lord.

THE COURT: Certainly.

BY MR. ESTRIN:

Q. Mr. Miller, what can you tell us about the population of the Beverly Herd in the last ten or twelve years?

A. Not very much. I believe that the last survey suggests that they are in a stage of possibly in a state of decline, but they do not have enough information to develop the trend. I don't think they have. I heard from

a Saskatchewan biologist concerned with the Beverly population that he thought, again, hunting by natives using aircraft to fly out and locate their game and land and kill the caribou and fly them back will become, if not already, a problem for the Beverly population.

Q. I see. I take it you are not aware of the figures given on discovery by the government which said that the population in 1974 of the Beverly Herd was 124,000 and Dr. Banfield, in his Affidavit, that we are going to hear about, in 1979 says that the population is 125,000?

A. No, I can't recall the exact figures. I don't attempt to keep those types of values in my head. They are in reports and literature. I can look them up when I need them.

Q. There was one statistic quoted to us about these caribou that I just wanted to ask you a little bit further about. I think you said that various authors estimate the life-span -- and I think you may have been talking about bulls -- after a certain period of lifetime, they could end up living twelve to twenty years?

A. No, that is longevity for the genus rangifer caribou. That is based on an observation of several different authors. I

believe the range is between 12 and 20 years.

Q. I see. Has that been broken down to be more particular with regard to, say, the Kaminuriak and the Beverly Herd?

A. Yes, life tables I have produced -- on the average the males tend to live a shorter life-span than females.

Q. What I am getting at is has anybody else come up with figures for the life-span of the Kaminuriak Herd that differs to your figures, or would you be the only person that has studied that matter?

A. I think I am the only one. If there is somebody else, I can't recall.

Q. I am not aware of any. I just wanted to know that.

You mentioned the Maguse River, 1967, something about shooting -- a lot of ammunition being used?

A. Maguse River, yes.

Q. That is around Eskimo Point?

A. It is inland from Eskimo. It is the Maguse River System through Maguse Lake and in towards Padlei.

Q. You were concerned about crippled animals. Is it not the case that low-flying planes can constitute such harassment that they

can end up stampeding the animals so that they cripple themselves? It is quite possible, isn't it?

A. Not quite possible, no. Not at the rate of aircraft overflights that have taken place in the past, but it is possible, yes, and would be more possible if the rate of aircraft was significantly increased.

Q. You have not made any studies about that, though, have you, about low-flying aircraft in the Baker Lake area and what relationship, if any, they have to stampeding caribou and what happens to them?

A. Not full-fledged studies, no. Just empirical observations.

Q. I think you said there is no indication in the literature that aircraft disturbance would cause a change in destination?

A. Well, I don't think I said it like that. I said I don't believe there is any information in the literature to prove that aircraft disturbance altered a migration so that the animals were going from A to B and did not arrive at B.

Q. You have also written how caribou will react differently at different times of the year to the same kind of stimuli?

A. Yes.

Q. And that their reaction -- you told us already, I think, in your writings and before today -- that calving caribou are more -- react more in a more reactive way in reaction to low-flying aircraft than the same caribou at another time of year; is that true?

A. Yes, but those are short-lived reactions unless reinforced.

Q. I think you said it was hard for you to perceive how grid patterns could bother the caribou, given the size and the range that they use. In other words, it was such a large area and such a small bunch of caribou that in relative terms you could not perceive how grid pattern activity could bother them. Is that substantially correct?

A. In the past -- at past rates of survey because of the clumping phenomenon of the post-calving aggregation of relatively small ground compared to the total range they would be occupying or during other periods when they would be dispersed. You are still, to my mind, looking at a mathematical consideration: what is the probability of these two entities occurring in the same bloc at the same time when you have hundreds of thousands of blocs.

Q. Well, I think you also told us, if I heard you correctly, that recent

surveys or studies have shown that the Kaminuriak Herd is now broken up into -- I think the figure mentioned 32 groups?

A. No, no. I said in 1968 the post-calving aggregations that Parker counted numbered 32. That those could change.

Q. From year to year?

A. They could change from day to day or week to week -- hour to hour.

Q. If he observed that, it suggests, as far as I can understand it, that the herd is not in one clump at that point?

A. No. There is no evidence to suggest that the Kaminuriak Herd has ever been standing on one piece of ground -- the whole herd.

Q. Accordingly, if that is the case, if they do -- from time-to-time and from day-to-day associate and dis-associate to form different groups, does that not lead to the likelihood that there will be more opportunity for encounters with these various low-flying planes?

A. I don't know. You would have to take a hard look at it -- a statistical program, to see what the probabilities looked like.

Q. There is something you said that interested me. You said -- we were talking about fuel drums and some other kind of boxes, or something. You said the caribou would climb up

and have a look and they could see around for miles on that point?

A. I assume that is the reason for them doing it, yes.

Q. I take it you disagree with Barnabas Peryouar and other Inuits who say that caribou have very poor eyesight?

A. Very poor eyesight? I think if you check with Peryouar that he will tell you that is when objects are standing still. They are very quick to detect any movement.

Q. Well, we heard from him.

One more matter. I take it you are saying, with regard to these red flags, that caribou are essentially colourblind?

A. Yes.

Q. On what basis are you saying that? Have you done any studies?

A. No, I have not done any studies. But you can go to literature and determine the perception of colour by the number of rods and cones in the eyes of ungulates versus birds and so forth and get their range of colour perception.

Q. And you are saying that all studies on ungulates show that they cannot perceive colour? Is that what you are saying?

A. If they perceived colour,

it would be a foolish exercise for the southern deer hunter to run around in the woods in flaming red outfits and yellow outfits to hunt deer.

Q. That is another area in which you disagree with the Inuits as to the behaviour of caribou because they are quite concerned about wearing red -- well, red being out on these areas?

A. They may be concerned. I can believe that. But I believe that you can call in all the authorities you like on composition of the eye of an ungulate --

THE COURT: It is not a fact that is in evidence up to this point, Mr. Estrin.

MR. ESTRIN: I'm sorry.

THE COURT: The concern of the Inuit about people running around in red.

MR. ESTRIN: Yes.

THE COURT: That is a new one.

THE WITNESS: I would think it is likely that they would be concerned, since they are not in the habit of wearing red themselves, so anything new to their situation would be perceived as a problem.

MR. ESTRIN: My Lord, what I perhaps should have stated is that as I recall

the evidence, one of the witnesses, or more, said, "Well, it would be rather foolish if the Inuit would go out hunting with red colours or red clothing on".

THE COURT: I don't recall any discussion of that at all. However, the transcript will indicate it. It began on the 14th of May, so I don't pretend to remember everything that was said.

MR. GRAHAM: My friend is confused, perhaps, with an incident on discovery where one of the witnesses offered to take me out in a red coat or something like that.

THE COURT: I see. All right.

MR. ESTRIN: Well, I hope I did not misstate it.

BY MR. ESTRIN:

Q. Dr. Banfield, in his Affidavit, has stated that caribou notice moving or fluttering objects and pay scant attention to stationary objects.

A. That's what I said, essentially, yes.

Q. You would agree with that?

A. Yes.

MR. ESTRIN: Thank you, Mr. Miller.

THE COURT: Mr. Chambers, I don't know how long your re-examination is going to be. If it is within the bounds, I think it would be nice to finish this witness and I am certainly prepared to sit extra time in order to do it.

MR. CHAMBERS: I shall not be very long, My Lord.

THE COURT: All right.

RE-EXAMINATION

BY MR. CHAMBERS:

Q. Mr. Miller, you were referred to a study -- this was the 1970 study, which I believe that particularly you were referred to page 12. It was read to you, or you read into the record, the third numbered paragraph?

A. Yes.

Q. And the third numbered paragraph reads as follows:

"The three older calves that were classified as abandoned could have become separated from their maternal cows when the group was disturbed by hunting wolves or low-flying aircraft, especially our helicopter."

Would you read to the court the paragraph immediately preceding the paragraph numbered one.

A. "Abandonment"?

Q. Yes.

A. "Abandonment of new-born calves by their maternal cows was the second most common cause of mortality. This seemingly abnormal behaviour of the maternal cows could be explained by the following suppositions."

Q. And then you list the suppositions, numbered one, two, and three. Suppositions one and two, could you just generally indicate to the court what they refer to?

A. Now I would really have to re-read them.

Q. Would you, please?

A. "1. It was the first time

that they calved. They were probably 2- or 3-year-olds that were not truly aware of, or adjusted to, the state of parturition, therefore, they left the calves and did not nurse them. Young cows calving for the first time, especially after the peak of calving when the caribou are in large groups, may retain strong social ties with the group and fail to establish bonds with their newly dropped calves. During the last 2 weeks of June we observed that many of the small peripheral groups were composed of maternal cows and new-born calves that were only hours or a few days old. The groups were usually several miles from large post-calving groups. The cows in the small groups had apparently left the large groups to drop their calves and in so doing established a strong mother-young bond which is essential for successful rearing of the calves. Any cow, especially one calving for the first time, that stayed in the large moving groups and dropped her calf, would, however, probably retain an affinity to the group and desert her calf on the migrational path."

Q. Would you read the second supposition?

A. "2. The cows may have had some physiological or pathological problems that resulted in irritation during the birth processes and desertion of the calves when they were dropped. If they cleaned the calves and stayed with them, they may have later suffered from some condition such as mastitis which caused them to desert their young. The second supposition would be extremely hard to defend and prove. In our limited collection one cow with metritis, mastitis and so on stayed with her calf even though it was unable to nurse. Another cow with a distended udder, suggestive of mastitis did not desert her calf."

Q. This third supposition has been read into the record. I have read the first sentence of it already.

These three suppositions, are they just that; or are they based on any factual data?

A. Well, the first one has some basis in fact because you can find examples in the literature of problems of young cows

giving birth for the first time. You can find it in the domestic livestock literature, as well.

Q. What about the second supposition, sir?

A. That has very strong basis in fact. Physiological and pathological disorders associated with the birth process and with subsequent nursing, such as mastitis -- that is when the cow can't let down her milk or won't let down her milk. She is very sensitive. The calf is attempting to nurse and she just kicks it away. That is not what you would call a common event, but it does occur. It falls into the sources of mortality for caribou calves.

Q. How about the third supposition; namely,

> "The three older calves that were classified as abandoned could have become separated from their maternal cows when the group was disturbed by hunting wolves or low-flying aircraft, especially our helicopter."

Is this pure supposition on your part or was this actually observed as having happened?

A. I currently cannot recall any literature that would support desertion from an aircraft. It remains a possibility under

certain conditions. And, also, from wolf disturbance, you would expect, as evidenced by finding separated calves sometimes on the tundra, that wolf harassment of the group may lead -- if it is prolonged, persistant -- may lead to separation of calves from their cows.

Q. On page 17 your attention was directed to aerial photography at low-levels. And you said in the opening paragraph:

> "We began the aerial photography of caribou on the calving ground in early June. The low level, 100- to 165-m (300- to 500-ft) flying that was necessary for good compositional photography of mixed groups was a serious form of harassment to the caribou. As our primary objective was to determine the causes of mortality to newborn calves we decided not to introduce possible additional mortality by aircraft harassment. Therefore, we carried out most of the photography for determining composition of caribou groups at heights above 200 m (600 ft)."

Now, did you determine or find any evidence, I should say, or is there any evidence otherwise in the literature, that low-level photography of the nature described necessarily

results in calf mortality?

A. I cannot recall any that would actually document calf mortality having been caused by aircraft harassment.

Q. So was this just another supposition you made to eliminate --

MR. ESTRIN: Well, My Lord, it sounds like my friend --

THE COURT: It is a leading question.

MR. ESTRIN: That he is cross-examining his own witness.

MR. CHAMBERS: Fine.

BY MR. CHAMBERS:

Q. Tell the court why you alluded to that in this connection -- why you decided not to include this low-level photography?

A. Well, as I mentioned earlier, I was there for studying natural mortality of caribou calves, and I did not want to inject any possible unnatural, if you like, or additional form of mortality. I didn't know what the reality of it occurring was, but it was for me to make a judgment of how careful I would be in carrying out my work.

Q. You were referred to a document, and I do not believe it was put in evidence, but it concerned the Technical Caribou

Committe, in which there was discussed various programs for wolf control?

A. Yes.

Q. And this also came in existence at the same time that Exhibits D-10 and D-11, the Management Options paper and the Kaminuriak Caribou Herd paper came into existence, is that correct?

A. I believe so.

Q. Can you tell me whether there were any decisions reached by this committee about which form of these possibilities or possible methods of wolf control would be adopted?

A. I can't recall if any decision, as such, was reached.

As I mentioned earlier, our function -- the Canadian Wildlife Service -- is an advisory one. We have no say whatsoever in the actual management practices carried out, so it would really be up to territorial and provincial people to decide which technique they would opt for, and they would consider cost of the technique, the efficiency of the technique and so forth.

Q. Was there any indication given at that meeting or meetings as to which option would be opted for?

A. I believe it was suggested that they would go for the shooting because they

felt that the Inuit would object to the poisoning because of the current high price of wolf pelts and, of course, we know that friends of animals and so forth would definitely object to the poisoning program of wolves in Canada.

Q. But, if I understood you correctly, no actual decision has been made as of this time, is that right?

A. The decision would have to come at a much higher level than ours. We are just, as a group, presenting our findings, our belief on how to manage the population.

Q. You also said that you personally -- if I understood you correctly -- shot some caribou -- a considerable number, as a matter of fact, in the course of your research?

A. Yes, the research team on the project involved, at different times of the year, if we were working in northern Manitoba, several Chipewyan or Cree Indians; and if we were working in the NWT out of Eskimo Point, or Baker or Rankin, it would involve Inuits from those communities along with the C.W.S. biologists and technicians.

Q. Were these animals shot by you or under your supervision for research purposes?

A. Yes, they were shot

so that we could obtain all the physical condition information, the reproductive tracts for reproductive information, information on pathology and physiological problems, growth parameters and so forth. And all of the meat was delivered to the respective settlement that was nearest to the hunting areas -- and hides. All the usable parts.

MR. CHAMBERS: Those are all my questions. Thank you, sir.

THE COURT: Thank you very much, Mr. Miller.

We will recess now until 9:30 in the morning.

---COURT ADJOURNED TO June 7, 1979, at 9:30 a.m.