# A Celebration of Dave Ankney: Scientist, Educator, Wildlife Advocate, Hunter, Mentor and Friend

**Saturday, October 26, 2002**

**TABLE OF CONTENTS**

# Dave Ankney's Contributions to Waterfowl Research, Conservation and Management at Long Point

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As you will all hear today, and as most of you know from past experience, Dave Ankney has a keen interest and has made substantial contributions to many facets of wildlife research and conservation, at the national and international level. However, at least I'd like to believe, Dave's first love with respect to wildlife is waterfowl (swans excluded), and his first love with respect to wildlife habitat (and his own) is Long Point. Because of this, Dave has immersed himself in the conservation, management and study of waterfowl and wetlands at Long Point for the past three decades. As many of you have travelled some distance to get here, and some have quite possibly never experienced the joy that is Long Point, I will start by giving you a brief synopsis of the area, particularly with respect to its importance for waterfowl. With this, I hope you will appreciate why such a giant of the waterfowl world chose to dedicate so much of his personal and professional life to Long Point, and why he has opted to live out his good old days in such a place.

Long Point is a 35 km sand spit that is situated on the north shore of Lake Erie. This spit has protected the inner bay and outer bay from the wave action of Lake Erie. This, combined with a number of other forces, has facilitated the formation of 27,000 hectares of wetland habitat. The inner bay, being, on average, 2 meters deep, provides another 72 square kilometers of primarily diving duck habitat. This impressive wetland complex is one of the most important staging areas for migratory waterfowl in eastern North America.

Long Point's importance can be attributed to several key factors:

- it is uniquely situated between Atlantic and Gulf coast wintering areas on the one hand and boreal, prairie and Arctic breeding areas on the other,

- the shallow, productive waters of the Inner Bay and associated wetlands provide ample feeding opportunities for approximately 25 species of waterfowl,

- due to the conservation efforts of local hunt clubs and government and private agencies, it is one of the best conserved and least negatively affected coastal wetlands in the world.

Long Point was designated as a Ramsar site in 1982, based primarily on its international importance as a waterfowl staging area. Long Point and its surrounding waters have been designated as a World Biosphere Reserve by UNESCO, and a Globally Important Bird Area by BirdLife International. Ten thousand acres of Long Point's wetlands have also been designated as a National Wildlife Area. Long Point is also (and will continue to be) one of the best places to hunt waterfowl in Ontario.

It was this incredible background of superlatives that attracted Dave to Long Point to conduct his own research, to participate in and help direct several local conservation and research-based organizations and projects, to fish and hunt, and more recently, to retire with Sandy.

Dave's contributions to waterfowl and wetlands at Long Point have been exhaustive. Be it providing advice to hunt clubs on wetland management, sitting on scientific advisory committees, or providing expert advice to new hunters on blind selection, I feel confident in saying, Dave knows more and has contributed more to waterfowl conservation, management and hunting advocacy in the Long Point area than any other individual or agency.

I will limit the rest of my talk to two of Dave's major commitments and accomplishments - that being his contributions to Long Point Waterfowl and to the Long Point Waterfowl Management Unit and Waterfowlers' Association.

Long Point Waterfowl (LPW) was established in 1989 through an agreement with the Canadian Wildlife Service and the Bluff's hunt Club. The Bluff's Club is a group of conservation-minded hunters concerned with the long-term welfare of waterfowl and wetlands at Long Point. Through this agreement, the Bluff's Club established an endowment and agreed to provide annual support to form a research-based organization at Long Point. In return, they were given the right to continue to hunt waterfowl on a property they had leased from the Long Point Company, prior to its being designated as a National Wildlife Area. With this, LPW was formed. LPW is a non-profit, non-government organization dedicated to the study and conservation of waterfowl and wetlands at Long Point, as well as throughout the lower Great Lakes. LPW is administered by Bird Studies Canada and continues to be supported primarily by Bluff's Club members. However, LPW also receives generous support from the Waterfowl Research Foundation, Ducks Unlimited Canada, as well as from concerned individuals and interested corporations.

By making our research results available to the public and scientific community, we are a strong voice for conservation and we make a substantial contribution to the science of waterfowl and wetland ecology. LPW is also committed to providing hands-on opportunities for young wildlife technicians, biologists and scientists, as well as to increasing public awareness of the importance of maintaining healthy wetlands and sustainable waterfowl populations.

LPW is managed and directed by a Board of Directors and a Scientific Advisory Committee. Dave was initially asked to be on the Scientific Advisory Committee as a representative of the academic community. Dave's passion for waterfowl and Long Point became increasingly evident as he pushed LPW to adopt the Delta Model; that being an organization that conducts leading-edge research through the provision of guidance and financial assistance to top quality graduate students. Through his commitment, as well as his vast knowledge of waterfowl and the ecology of coastal wetlands, Dave was instrumental in laying the foundations of LPW, and he continues to be a driving force today. For instance, Dave has provided expert assistance and guidance with LPW's work on waterfowl foraging ecology, aquatic macrophyte distribution and abundance, Tundra Swan staging ecology and competition with other waterfowl, Tundra Swan satellite tracking, scaup nutrient reserve dynamics and contaminant burdens, as well as Phragmites distribution and wildlife use of this invasive plant. Dave was also asked to sit on the LPW Board and he now also provides a key link between the Scientific Advisory Committee and Board of

Directors. Quite fortunately for us, Dave has agreed to continue his commitment to LPW during his retirement.

The "Long Point Waterfowlers' Association," originally called the "Long Point Hunters' Association," was formed in February 1987, but was not officially constituted until March 5, 1998. Dave Ankney was a founding (and still active) member of a group that today includes over 300 active supporters. The objective of the Association was and is "To maintain the opportunity for quality public waterfowl hunting at Long Point." Dave was and continues to be the LPWA consultant on all matters related to waterfowl biology, science and management. For instance, he is often consulted on matters pertaining to blind and bait pond placement, as well as wetland management.

By the early nineteen nineties it became obvious that the Long Point Waterfowl Management Unit could be lost if it continued to rely on the Provincial Government for funding. While user fees more than covered expenses, they were going into General Revenue and costs were coming out of the Ministry of Natural Resources operating budget. The Long Point Waterfowlers' Association needed to capture the user fees and recycle them back into the Waterfowl Unit without government intervention. Only problem was that the LPWA was not in a position to handle payroll and other day-to-day management issues. To quote Arnold Freigtag Dave Ankney came to the rescue once again. As president of the OFAH, Dave immediately realized that the OFAH could serve this function. Dave, representing the OFAH, and Richard Manley, representing the MNR, got their heads together aknd the result was a Partnership Agreement signed by John Marchington for the MNR and Rick Morgan for the OFAH on September 5th, 1995. User fees are now sent to the OFAH and for a small fee their staff handle all the necessary paperwork. The Long Point Waterfowl Management Unit is now self-financing and in a much better position to maintain the integrity of this very unique public hunting area.

As you can see, this is simply another example of how Dave Ankney, as a hunter, wildlife professional and OFAH Director and President, has worked for the betterment of wildlife and hunting.

Finally, I'd like to thank Dave for his substantial contribution to my development as a waterfowl biologist and scientist. Nobody has had a stronger influence on my thought processes or career development than Dave - I am forever indebted to him for that.

Dave, thanks to your hard work, and that of many that you have mentored, "the good old days are now". I hope that you and Sandy enjoy them to their fullest.

# Dave Ankney's Contributions to Ontario's Wild Turkeys and Wild Turkey Hunters

***Dave Reid***

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## Introduction

The original draft agenda had me speaking to Dave's significant contributions to wildlife conservation locally in Long Point Country. And I could have spoken to a long list of initiatives which Dave has played a crucial role, including but not limited to: his lead in convincing the Canadian Wildlife Service (CWS) to allow a regulated deer hunt on Long Point National Wildlife Area in 1989 to protect numerous sensitive species that were threatened by deer over-browsing; his influence on the Ministry of Natural Resources (MNR) and CWS to limit hunting to within 400 meters of shore on Long Point Bay (and Lake St. Clair) so as to reduce disturbance to diving ducks, and; his membership on the technical advisory committee for the Waterfowl/Wetlands Initiative for Long Point Country which has identified ways (many of which have been implemented) to conserve waterfowl and wetlands that have landscape impact and community support. But after a little thought I decided to concentrate on the one conservation achievement Dave's made that has to be at the top of his list of achievements over his very accomplished career … that is, the successful restoration of wild turkeys to Ontario.

Back in the 1940's, C.H.D. Clarke, Chief of Fish & Game for the Ontario Department of Lands & Forests and respected wildlife author and scientist, said this about wild turkeys in Ontario "Bring it back? It would be just as reasonable to reintroduce the bison to the prairie wheat belt." Well, thanks in large part to Dave Ankney, Dr. Clarke has been proven wrong. And if I were a prairie farmer, I'd be keeping an eye on this guy, or they might wake up some day to a herd of wild bison exasperating their farming woes.

My presentation will include a brief status of the wild turkey in Ontario, followed by a long list of examples of how Dave Ankney has played the pivotal role in this most successful wildlife management story.

## Status in Ontario

Wild turkeys were extirpated from Ontario by 1900 or shortly thereafter due to habitat destruction and unregulated hunting. Experience in the US since the early 1950's has proven that wild turkeys could be restored by releasing truly wild birds into suitable habitat. Our understanding of what constitutes suitable habitat continues to evolve from the days when wildlife biologists felt that tens of thousands of acres of contiguous forest with a preponderance of oak species were required for turkeys to survive. We now know that wild turkeys can thrive in agriculturally dominated landscapes with less than 10% forest cover. And these populations

can support hunting recreation given improved conservation laws. Restoration efforts began in Ontario in 1983, and by 1987, a total of 274 birds from Missouri, Michigan, Iowa, New York, New Jersey and Vermont were released at six different sites including Norfolk County. Today, the population in Ontario is estimated at approximately 48,000 birds and they occupy an area greater than their historic range. Spring turkey hunting began in 1987 and has grown annually in leaps and bounds. A record 4,692 turkeys were harvested in Ontario last spring. License sales have been increasing by about 20% per year and totaled 15,273 in spring 2001. License sales from 2002 are not yet available but are expected to approach 20,000. The economic impact of turkey hunting in Ontario in 1999, when just 9,025 turkey hunting licenses were sold, was estimated to contribute $2.3 million to gross provincial income. I'm sure that impact is closer to $5 million this year.

**In the Beginning**

Dr. Paul Prevett, Southwest Regional Ecologist with MNR and since retired, actually wrote the feasibility report on restoring wild turkeys to Ontario back in 1983. Chairing the 1986 Wild Turkey Management Workshop in Peterborough he said Dave Ankney "… was one of the prime movers. It certainly took a few people to grease the skid within government, and Dave was the guy that gave the jabs in the ribs to the government types." At that workshop, Dave gave the concluding remarks and I'd like to quote his explanation of the beginning of wild turkey restoration "It was only three years ago, just about exactly today that I got a call from Jim Collins (local MNR Fish & Wildlife Supervisor). Jim said "I have got a helicopter on charter for "X" number of hours for doing some waterfowl brood surveys down at Long Point, and I am not going to need all the time. I think I will have about an hour left. Would you and Paul Prevett like to go up in the chopper and fly around the (MNR) Simcoe District, and see what you think about it in terms of turkey habitat?" Now you heard yesterday that Jim's a mean S.O.B.. Well, he is a smart S.O.B. too, because he obviously picked on the two major experts of turkey habitat in Ontario. I have hunted turkeys in Virginia for four years and I lived in Iowa for two years, so obviously I was qualified. Paul Prevett had just got back from living for 10 years on James Bay. Anyway, we did get up in the chopper, we flew the Big Creek valley primarily, we saw lots of trees, lots of oak trees, lots of corn, so obviously to us it must be good turkey habitat. Then two months later, as you know, Paul had a feasibility report prepared and in the hands of his supervisor. Of course the rest is all history."

A steering committee was struck in the fall of 1983 (Rick Morgan and Lance Males of Ontario Federation of Anglers and Hunters (OFAH), Don Simkin and John Harcus of MNR and, Ron Tasker and Ross Bateman of the Ontario Federation of Naturalists (FON)) to oversee the restoration effort and set three prime objectives: 1) restore part of Ontario's natural heritage; 2) provide for hunting and viewing recreation and, 3) derive economic benefits from this recreation.

The inaugural release of wild turkeys to Ontario occurred on March 3, 1984 with the release of eleven hens from the Ozark Mountains of southern Missouri at the Backus Woods of Norfolk County. Eventually a total of 22 hens and 5 toms were released that month, and most of the

hens and all of the toms were fitted with radio collars and monitored by "Turkey Joe" Weaver. Turkey Joe was a grad student of none other than Dave Ankney and he eventually published his M.Sc. thesis in 1989 called "On the ecology of Wild Turkeys reintroduced to southern Ontario".

This was a good start but more money was going to be needed to achieve the three objectives and complete restoration of wild turkeys to all suitable habitats within Ontario. The OFAH responded with the establishment of a dedicated pot of money called the "Wild Turkey Trust Fund". Locally the Long Point Wild Turkey Committee quickly formed and committed to raising funds for the Trust Fund. Chaired by Jim Cronkwright, the committee membership included Dave Ankney, Bruce Bowyer, Jim Collins, Terry Dunlop, Joel Hopkins, Tom Parker, Bob Sidway and Jack Taylor. Thanks to this committee's efforts, with the sponsorship of the OFAH and support of the National Wild Turkey Federation (NWTF), the Long Point Wild Turkey Fund Raising Dinner was held on March 29, 1985 at the Belgian Club in Delhi and netted over $50,000. At the time this was one of the most successful fund raising dinners the NWTF, who had been in the business of raising such funds since 1973, had been involved in. The Delhi dinner also established the groundwork for fund raising by the OFAH through what is known today as their Conservation Dinner program.

Not only is Dave Ankney a great team player who pitches in to get the job done, he also puts his money where his mouth is. For example, this original acrylic painting entitled "Wild Turkeys Return To Norfolk" was donated to the Delhi dinner by Candice McManiman (Paul Prevett's wife) and purchased by Dave & Sandy for $675. Dave & Sandi, then turned around and donated the painting to the OFAH head office for display to the general public visiting their office in Peterborough. It was one of four pieces of art engraved in granite on top of the wild turkey stone cairn mentioned later in this presentation.

**The Early Years**

By 1986, the turkey program was growing across Ontario and this put onus on the MNR's field staff to become involved, answer public enquiries and manage this newly established resource. Problem was, we were all novices in this Province when it came to knowledge about the wild turkey. Thus the Ontario Wild Turkey Management Workshop was organized and held August 27 - 28, 1986 at the Holiday Inn in Peterborough. I've already mentioned Dave's role with concluding remarks … and two main observations he had at that time were prescient and had great influence on the wild turkey program. These were: 1) the tremendous potential for continued involvement of sportsmen and others, not just hunters, in the wild turkey restoration project, and 2) the need for MNR to develop a plan that will identify priority areas for trap and transfer of birds … and a time table for doing that.

Trust me when I tell you there has been tremendous involvement of many people in the wild turkey program. The first Wild Turkey Management Plan for Ontario had been drafted by September of 1985 … but having a plan and acting on it are two different things. With the establishment of the first spring hunting season in Eastern Ontario in 1987, all three objectives set by the steering committee had been met and their mandate was fulfilled. We needed to get on with the business of managing the wild turkey resource and implementing that plan. A Wild

Turkey Working Group (WTWG) was established about that time to make management recommendations to the Wildlife Branch of MNR. It was chaired by John Harcus and initially had three MNR field biologists from the three main areas where turkeys had been established (myself from the Southwest, John Dobell from Central Ontario and Terry Humberstone from eastern Ontario). Terry soon moved to Northern Ontario and was replaced with Karen Bellamy.

In 1990, the WTWG met to develop a set of criteria and guidelines for ranking release sites so that we could stock our best quality habitat first. I can't recall if Dave Ankney was considered an official (or ex-officio) member of the WTWG at that time but as one of two leading experts on wild turkey habitat in Ontario (see above) we obviously needed his involvement. Further, I happened to know he had been "boning up" on turkey management ("Mathew and the Midnight Turkeys") so Dave was invited to help develop the criteria. Dave's input helped develop a set of broad criteria (Table 1) by 1992 and these guided the MNR to release a total of 3,574 birds to 228 new release sites across the Province as of today. All class 1 and 2 Wildlife Management Units (WMU) have been stocked as have most class 3 WMU. In fact, it is anticipated that restoration at all provincial priority sites will be completed in the next two years, leaving only local releases and possibly some northern releases for future years.

**Table 1. ASSESSMENT OF WILD TURKEY RELEASE AREAS IN ONTARIO**

| Factor | Category | Score |
|---|---|---|
| % of total area in forest cover | > 75% | 10 |
| | 41 to 75% | 20 |
| | 26 to 40% | 30 |
| | 16 to 25% | 20 |
| | 10 to 15% | 10 |
| | < 10% | 0 |
| Mean # of days with > 5 cm of snow on the ground | < 60 | 30 |
| | 60 to 90 | 20 |
| | 90 to 120 | 10 |
| | > 120 | 0 |
| Ecoregion | southern agricultural | 15 |
| | central agricultural | 10 |
| | eastern midlands | 5 |
| | other | 0 |

**Classification of WMUs**

| Class | Total Score and Additional Conditions |
|---|---|
| 1 | 51 to 75 |
| 2 | 26 to 50 |

| | |
|---|---|
| 3 | 0 to 25 **OR** < 10% forest cover **OR** > 120 days with more than 5 cm of snow on the ground |

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**The Later Years**

The WTWG continues to meet annually, making recommendations to MNR's Wildlife Branch and serving as a resource for field staff to draw upon. It is no coincidence that we meet at different locations during the turkey-hunting season. Dave has shown his hospitality on a number of occasions including the May 1998 meeting at his Long Point cottage and the May 2000 meeting at the Unionville Sportsmen Club he belongs to in Missouri. Membership on the WTWG has grown to include ex-officio representatives from the OFAH (Beth MacKay replaced by Cam Brownson in 2001) and the NWTF (Joel Pedersen). Bruce Pollard took over as chair upon the retirement of John Harcus in 2000. I believe the success of Ontario's wild turkey program is in part due to the successful workings of the WTWG and its unique membership make up. Dave Ankney has played a huge role in that make up and its success.

Dave Ankney has long been a champion for the little guy interested in wise use of our natural resources. Two examples include his support for an experimental release of wild turkeys near Sudbury and his support for the restoration of wild turkeys to Pelee Island. The Sudbury release proceeded after the WTWG visited the Noelville area in January of 1999 and recommended it proceed on an experimental basis to test the northern limits of the turkey's range. Thirty-five birds were released there later that winter and were supplemented with an additional 13 birds in February 2000. Despite near record severe winter weather in 2000/2001, research that suggested poor survival and less than enthusiastic interest and support by the local MNR office, local turkey enthusiasts (the "little guys") are optimistic. They have been monitoring the birds through gobbling surveys and documented sightings and have been able to confirm a population of at least 50 birds that appear self-sustaining at this time. Based on these encouraging results, other northern releases well outside the historic range are being considered. Similarly, turkey enthusiasts on Pelee Island successfully called upon Dave Ankney and the OFAH for help in convincing MNR to restore wild turkeys there. Twenty-five birds were released on Pelee last February, numerous brood sightings have been reported and the community has established a significant fine for anyone shooting a wild turkey illegally (e.g. during the annual pheasant hunts).

The production and 2001 release of the video "Reintroducing the Wild Turkey, the story of the wild turkey in Ontario" is one of the most recent contributions to Ontario's wild turkeys and wild turkey hunters involving Dave Ankney. He participated on the editorial committee for this video and was interviewed on it. It was produced by Pinegrove Productions and is available for $15 from OFAH, NWTF and/or MNR.

Last year Dave participated on a committee chaired by Russ Piper to have a stone cairn designed, built and commemorated at Backus Woods, the site of the first wild turkey release in Ontario. Dave served as the Master of Ceremony at the actual dedication, which took place on January 19,

2002. Many of you may have noted the signs along highway 59 just north of here directing the public to this historic site.

**In Conclusion**

"All progress has resulted from those who took unpopular positions." I don't know who's quote this is but many would agree that it could apply to Dave Ankney … he has had a hand in a whole lot of progress in the conservation of our natural resources, and at times his positions were not popular with the powers to be. Yet Dave is not always controversial. To quote MNR biologist Pud Hunter … "He has an unselfish approach with anything to do with wildlife. He has great insights and has played a valuable consultative role".

Dave has been known to give a beginner a helping hand, for example guiding Beth MacKay to her first wild turkey. Or allowing me to hunt his special spot in Missouri where I got lucky and shot my first wild turkey back in April of 1986.

Dave may have helped prove C.H.D. Clarke wrong on the feasibility of restoring wild turkeys to Ontario. But there is one quote I know he would agree with Clarke on and that is " The wild turkey is a game bird without equal in a world once well provided with game birds. He is beautiful as the autumn woods where he is hunted, fast on the wing and afoot, wary to a wonderful degree. On the table he has an aroma and flavour that has made turkey the symbol of good cheer since the days of the New England Puritans and their first Thanksgiving. And what a size! A day's bag of lesser game birds could be used as stuffing for one ordinary turkey and some of them would certainly gain flavour in the process".

On behalf of Ontario's wild turkey enthusiasts, thank you Dave for your past involvement and influence. We hope you continue this on the WTWG. I'm very proud to have worked along side you and to be able to call you my friend. I wish you and Sandi all the best in your retirement from academia. That's the truth and that's the tails end!

# Contributions to Avian Energetics, Nutrient Dynamics and Avian Life History Evolution

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It is no exaggeration to state that Dave Ankney has had a large influence on both ornithology and conservation. Dave's contributions either through his own research or that of his students and other colleagues has left a mark on many facets of ornithological science not only in Canada, or North America, but also in other hemispheres. I think that it is safe to say that that everyone in the room here today, and countless others, already know that. Many of you may also know that Dave's largest influence in ornithology was likely in the field of nutritional ecology – specifically in how waterfowl deal with energy or nutrient bottlenecks throughout their annual cycle by storing and using nutrient reserves. We know that, and you all know that, but for our own edification, lets put some empirical support behind this notion. As we do that, you will all notice that there are some undeniably Anknian properties that clearly emerge.

First of all, let us start with Dave's record of scientific publications refereed by his peers. The number of publications in peer-reviewed scientific journals is usually the metric considered in evaluating an academic's productivity, contribution, and influence. If you check his *curriculum vita*, you will note that Dave lists about 153 publications of this type. Just between you and me, this is where we find an example that illustrates how he can tend to put a spin on things. Have any of you ever had a "debate" with him about any topic that he happens to have strong feelings about? If you have, you will probably agree that Dave is never wrong in support of causes that he holds a particular passion about. (Maybe I should back off from 'never', and qualify that with 'almost never' – he won't admit it, but the one person to whom he had to concede a debate is me – but I don't want to go into that today). Aside from these few exceptions, Dave is never wrong because he is quite gifted in using carefully measured doses of hyperbole and smoke to the point where the smoke can turn into a screen, and often Dave or the person with whom he is debating actually end up thinking that Dave is "right". So, Dave's powers of persuasion can be so effective that even he ends up believing what he is saying.

But I digress somewhat. Let us get back to Dave's CV, and actually check for which papers he is even listed as an author; we find that there are only (ONLY!!) 121 publications to his credit. Only 121! (as of last year, anyways) which he had some hand in authoring. The rest listed in his CV were either the products of his students' work, or in the field of Human Biology – and we don't mean the birds and the bees; his foray into Human Biology had something to do with human intelligence – something that I don't feel qualified commenting on probably because I was not endowed with healthy doses of it. We'll leave that face of Dave Ankney's academic career to Dr. Rushton. Anyways, Dave, shame on you for padding your resume in such a fashion!

Now that we have properly cut things down to size, lets have a closer examination of this paltry pile of 121 publications. Although the sample size is much reduced now, we might still be able to uncover some pattern in Dave's ornithological interests from Figure 1.

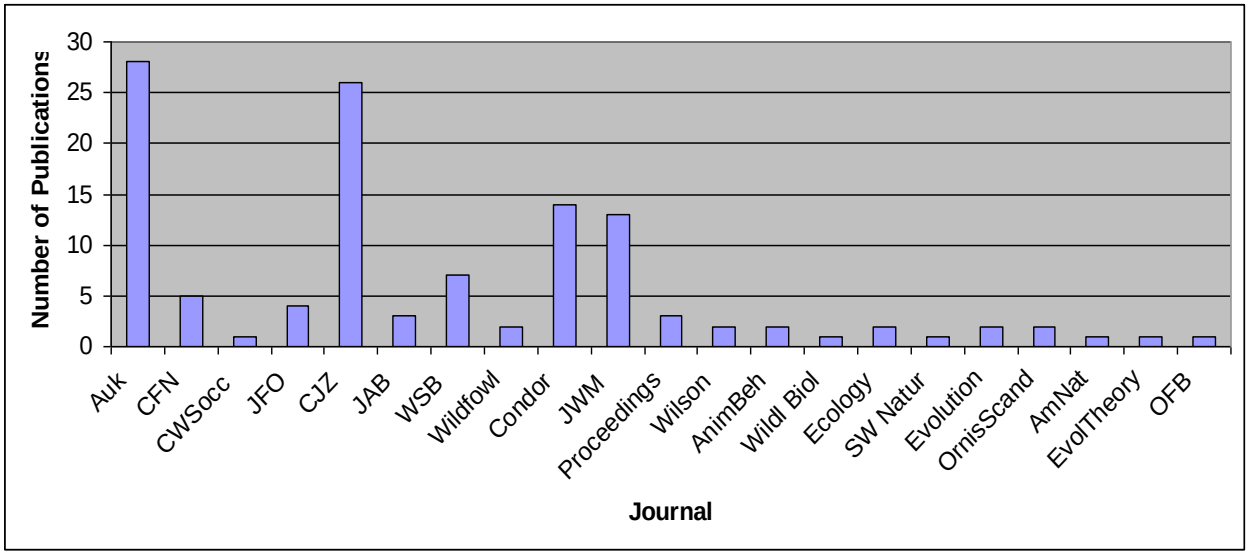


Figure 1. The Ankney Weak Preference for ornithological journals.

It turns out that these 121 publications are dispersed over at least 21 different journals indicating, I strongly suggest, a lack of focus on Dave's part. If only he would forget the Human Biology stuff, and spend less time with conservation work, he might have ended up with a somewhat more impressive publication list in what some people think really matters – and that is, bird stuff! Granted, much of these 121 publications are in such "bird" journals like Auk, Condor, Wilson Bulletin, etc., but Dave's lack of focus again becomes evident when you see that he has also published in such wide-ranging and (some would say, prestigious) journals like Ecology or Evolution. My advice to you, Dave, is that the human biology stuff is like a lightning rod – no matter what you find, you are going to tick somebody off – just ask Phil Rushton, who is probably one of the most experienced lightning rods around, to be sure! That just won't happen with bird stuff; people simply don't get worked up about bird stuff – I can't think of one feather being ruffled by, say, the Black Duck-Mallard debate, right Dave and Tom? (Incidentally, Tom, I happen to know that a man with 3 first names, i.e., Darrel G. Dennis, put Ankney up to all this Black Duck-Mallard stuff). On another matter, I BET (dollars to dognuts) that Dave would sign an affidavit that he and the late Dennis Raveling never entered into any heated discussions about whether use of neckbands on geese have any effect on their survival. I couldn't possibly lose another bet, could I?

Let's get back to our estimation of Dave's contributions. I did some exploratory data analysis, and there are some strong undercurrents and patterns in that data suggesting that Dave Ankney is without focus. I mean this constructively so that others, including myself, could avoid the pitfalls of his career. I intend to provide some empirical underpinnings for my arguments, and after I do, I think that most of us will agree with this argument! Statisticians like Bob C. Bailey

(as opposed to the Irish waterfowl biologist, Bob O'Bailey) might use the term "lack of precision". However, the story about our estimation of Dave does not end there with lack of precision, or lack of focus. As most of us know, there are two properties that one must deal with in statistical estimation: the first is precision, which we have already covered. But what about bias? How does bias come into our estimation of Dave Ankney? Is Dave biased? To get a better understanding, let's examine Figure 2.

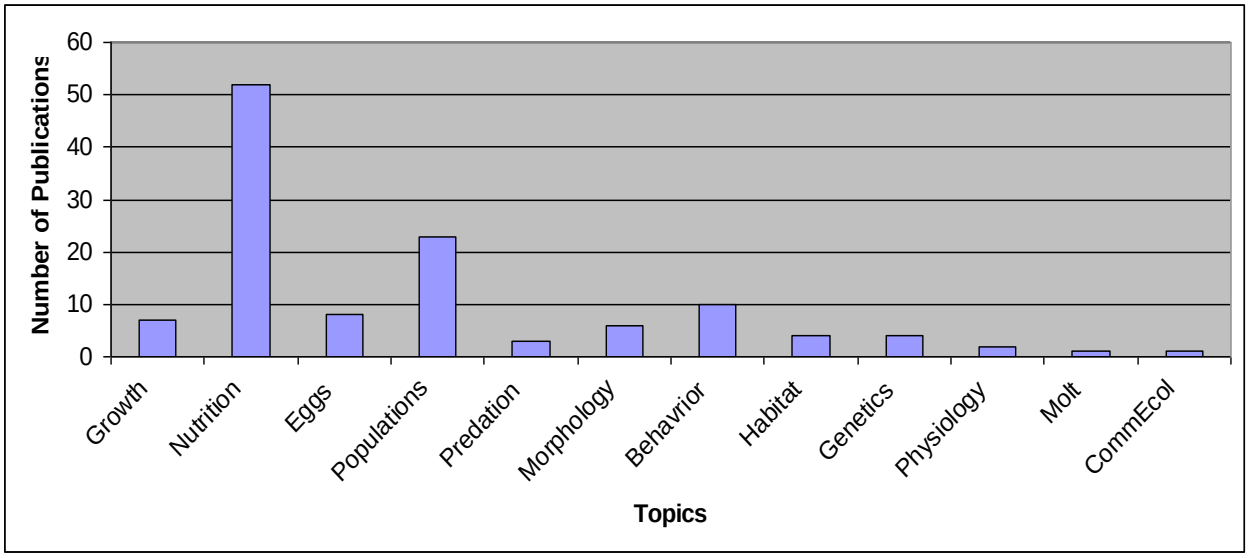


Figure 2. The Ankney Bias in nutritional studies that probably require "bump and grind" sampling schemes

What immediately jumps off the screen is that although he remains unfocussed in the ornithological topics that he has an interest in - from stuff like growth, predation, habitat selection and community ecology (whatever that is, Tom, sheesh!) - he seems to be extremely biased about nutrition in birds. The strong non-random preference for nutrient-based studies justifiably changes what one might call an interest of Dave's to something that he seems to be passionate about. One wonders why Dave Ankney is not more rounded out as a researcher and ornithologist. Why that passion for nutritional studies? Does it have something to do with the fact that to quantify changes in nutritional state, one needs to kill the study animal? Not only kill the animal, but do so with a Beretta auto shotgun - preferably, but not necessarily- and, if possible, over decoys in a marsh with a black dog watching your every move? Some of you would call this hunting, but I suggest that the proper term is "quasi-hunting" because he was not doing it in his spare time scratching some primordial itch. Dave was doing this as part of scientific endeavour. We understand that, depending on the question, it is no longer necessary to "sacrifice, euthanize", or otherwise use double-speak for "kill" the animal to measure it's nutritional state; however, in the glory days - in the absence of TOBEC and other new-fangled gizmos - you didn't have much choice. You had to go hunting for sample size, and - get this - you were paid for it! This was, and still is, Big Science. Imagine that, Dave - fuel up the smoke pole with some candy and go collecting samples in between making sweet susie music on a walnut trumpet. And, justifiably, all this was high-powered science.

Our inferences about Dave's bias for doing research that involves quasi-hunting is based on the assumption that there further should be a bias in the taxonomic groups of interest to him. Quasi-hunting under the guise… wait a minute, not under the guise, but rather as an integral part of the scientific method…, so, let me start over: Quasi-hunting as part of doing science is made easier in some cases if the focus is on game birds. By this we mean birds for which there is normally some sort of an open season, such as ducks, geese, snipe, coots, blackbirds, etc. etc. Let us test the assumption that there is a demonstrable predilection for certain taxonomic groups in Dave's ornithological research (Figure 3).

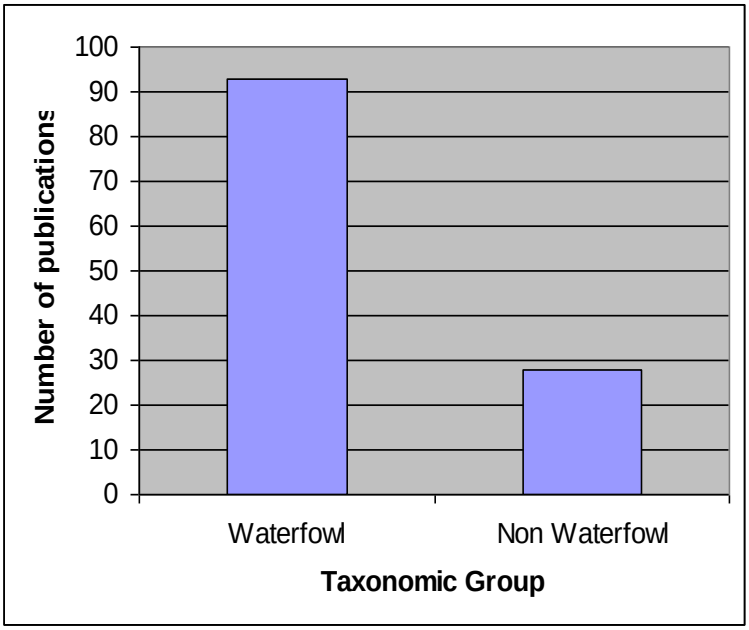


Figure 3. The Ankney Bias for waterfowl as study species.

When one groups Dave's 121 publications by predominant taxonomic group compared to all other taxonomic groups, one finds a very non-random pattern. He clearly seems to involve himself with waterfowl studies at the expense of all other orders of birds. Now…, I have documented evidence that Dave has spent an entire summer shooting non-game birds from swifts to grosbeaks to veeries. HOWEVER, I know that he did not enjoy doing that and, if I am correct about the timing of this expedition to western Canada, he was simply trying to fill up Dave Scott's museum up in the attic of the B&G building at UWO with sorely-needed dickie birds, strictly as a mercenary who needed money to get him through grad school. Either that, or he was under consideration for a faculty position at Western at the time, and was just sucking up. But – never mind! Such collecting trips for dickie birds and sundry other non-waterfowl orders of birds were aberrations, I submit. Instead, I still strongly maintain my thesis that Quasi-hunting has played a very strong role in defining who Dave Ankney is, and what kind of research he does.

Well, to close out this session, I want to finish up talking about Dave's contributions to ornithology. That, after all, is why all of you are here – I came here for the food! I could spend the next few minutes shining the spotlight on his better-known contributions and achievements by citing things like his 1978 classic paper with Charlie MacInnes, which was so classic that it became a Citation Classic – and that was in 1992 - it is still being classically cited in the literature. (Incidentally, Dave, everyone knows that Dave Scott really wrote that paper). Instead, I want to take the last few minutes highlighting his generally unrecognized contributions – this, under the category "Sampling techniques for waterfowl". Most of this research was/is conducted during his annual fall trips to Saskatchewan. The focus of the research is on the most effective sampling procedures for nutrient reserve studies. Well, here we go again: I start out trying to confer meritorious praise and accolades in recognition of Dave Ankney's contributions and accomplishments, but now we find out that Dave really is a slouch when it comes to publishing all the research he has done. My question to you, Dave, is "when you gonna publish that techniques paper about the effectiveness of "Heavy Shot" vs. Lead vs. Steel vs. Tungsten Matrix vs. Screws vs. Nails vs. Broken Glass for coaxing geese out of the ozone onto *terra firma*?" Your sample sizes for the spheres manufactured from different kinds of alloy must exceed 1000 by now? What are you waiting for? You really have no business retiring until you write up that *magnum opus*!

# Dave Ankney's Contributions to Behavioral Ecology

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As will be apparent from this collection of essays, Dave Ankney's research contributions have been substantial. Documenting how that research has influenced different fields of endeavor is for the most part quite straightforward. In fact, using computer-based searches of the literature, one can readily quantify how often a paper has been cited, by whom, and where, without ever having to venture to the library. My objective here is to document Dave's contribution to Behavioral Ecology. Unfortunately for me, however, a straightforward compilation of the numbers of papers published and their subsequent frequency of citation would not do justice to Dave's contribution to this field. While it is true that a perusal of Dave's *Curriculum vita* reveals papers on topics such as mate choice (Ankney 1977) and dominance relationships (Gregoire and Ankney 1990), I think it is also fair to say that one's overall impression would be that Behavioral Ecology has not been Dave's main field of endeavor. Nonetheless, I will argue that Dave has contributed to Behavioral Ecology, but in a subtle manner that is not apparent just from examining his CV. By using the tools of Behavioral Ecology in his research, Dave has both enhanced his own research and has provided an excellent example of how Behavioral Ecology can be used to understand and address real-world problems.

To understand Dave Ankney "as Behavioral Ecologist" it is appropriate to examine the intellectual atmosphere in which Dave began his research career. Scientists such as Dave, who were interested in animal ecology and behavior and who did their graduate work in the 1970s, witnessed and were influenced by a true paradigm shift. Through the first half of the twentieth century an incorrect interpretation of Darwin's (1859) theory of evolution by natural selection had become the mainstream perspective. The central premise of this group-selection view of evolution was that behavioral traits in animals evolve because they promote the survival of the species. There is a fundamental flaw in the logic of this view, however. Individual animals that sacrifice their own fitness for the good of the group will generally be less likely to pass on those behavioral traits to future generations compared to individuals with traits that promote their own fitness. The kind of traits that will evolve are those that make animals effective at surviving and reproducing, and thus effective at passing on those traits. Self interest rather than group interest will prevail. The prevalence of the group selection perspective, at least within evolutionary biology, ended with Williams (1966) book *Adaptation and Natural Selection.* Williams effectively debunked the group-selection perspective, setting in motion a scientific revolution. Other seminal works that followed, including Wilson's (1975) *Sociobiology* and Dawkins' (1976) *The Selfish Gene*, helped broaden and deepen the shift in perspective.

Selectionist theory, as Alcock (2001) has termed the individual-selection perspective, was clearly an approach that Dave recognized early on as a powerful way to understand the natural world. I still recall upon meeting Dave for the first time in the summer of 1976, being impressed with his ability to bring the logic of selectionist thinking to bear on a given problem. Unsound ideas were quickly dispatched and more logical alternatives offered in their place. Interestingly, despite Dave's obvious facility with selectionist theory, his own research has often addressed ecological problems for which that theory was

not central to the solution. Nonetheless, evidence of selectionist thinking can be found in all of Dave's writing.

Selectionist thinking is central to Behavioral Ecology. A behavioral ecologist asks why individual animals behave the way they do. How does the animal's behavior enhance its fitness? Even though Dave's research has fallen primarily outside the realm of Behavioral Ecology, he has regularly asked this same question about the animals he studies. By incorporating this approach into his research, Dave has brought a broader perspective to bear on the problems being addressed. I will illustrate this point with two examples. I have selected these particular examples because both involve problems in which I was interested, and in one of the cases Dave and I collaborated.

North American waterfowl biologists have been concerned for some time with the decline in abundance of American black ducks (*Anas rubripes*) and the concomitant increase in abundance of mallards (*A. platyrhynchos*) (Johnsgard and Di Silvestro 1976). These patterns have been attributed to the clearing of eastern deciduous forests, which has both converted black duck habitat into mallard habitat, and increased direct contact between the two species (Johnsgard 1967). The increase in contact between black ducks and mallards may have exacerbated the problem in two ways. First, because the two species hybridize, the much larger gene pool of mallards could potentially swamp that of black ducks and second, mallards could competitively exclude black ducks from the best habitat (Ankney et al. 1987). Working with students and colleagues, Dave initiated a research program that investigated this problem from a variety of perspectives, including Behavioral Ecology. Brodsky and Weatherhead (1984) had hypothesized that hybridization was promoted by mallard drakes being particularly successful competing for black duck hens when ducks of both species winter together. Together with Lynn Brodsky and Darrell Dennis, Dave used captive birds to test this hypothesis (Brodsky et al. 1988). They found that male mallards generally dominated male black ducks, and females of both species preferred dominant males. Thus, mating between black duck hens and mallard drakes would result. They also examined how early social experience affected subsequent mate preferences, and found that both species preferred the species with which they had been raised (Brodsky et al. 1989). Captive birds were also used to study other aspects of the dominance relationships of black ducks and mallards and surprisingly, in this instance, failed to find that mallards dominated black ducks (Hoysak and Ankney 1996).

The behavioral studies referred to above did not resolve all the behavioral issues associated with black duck-mallard interactions, but collectively they refined our understanding of those issues. Simultaneously Dave was investigating other aspects of the black duck-mallard problem. Another study using captive ducks failed to support the hypothesis that limited resistance to Leuocytozoan blood parasites by black ducks might slow the spread of mallards (Shutler et al. 1996). Genetic and morphological analyses confirmed the exceedingly close relationship between black ducks and mallards, and indeed gave credence to the suggestion that these two "species" might best be considered different color morphs of the same species (Ankney et al. 1986; Avise et al. 1990; Hanson and Ankney 1994). As well, both harvest and survey data were used to confirm that the decline in black duck abundance was associated with an increase in mallards, and that mallards appeared to exclude black ducks from the best breeding habitat (Ankney et al. 1987, 1989; Merendino et al. 1993; Merendino and Ankney 1994). Together these studies comprise a body of work that is at least as impressive for the diversity of approaches brought to bear on a single problem as it is for the high quality of the individual components.

A second example of Dave's work directly relevant to Behavioral Ecology involved the behavior of ducks responding to hunters' decoys. In the book *The Outlaw Gunner* (Walsh 1971) about illegal market hunting of ducks on Chesapeake Bay in the late 1800s, a curious observation is reported. Many hunters knew that ducks shot at night were nearly always fatter than ducks shot during the day, although why that was so was unknown. Although this observation was not the basis for Dave's interest in how ducks respond to hunter's decoys, Dave's work did help provide an explanation for this old observation. In a master's thesis done under Dave's supervision, Geoff Bain found that canvasbacks (*Aythya valisineria*) and redheads (*A. americana*) shot over decoys tended to be in poorer condition than birds in the general population (Bain 1980). At the same time, work I was doing assessing the value of decoy traps for controlling blackbird populations indicated that younger birds and birds in poorer condition were disproportionately vulnerable to trapping (Weatherhead and Greenwood 1981). The explanation for both these results appeared to involve the same two factors. First, birds that flock together often use each other to find food. Second, when birds finds what appears to be a flock of birds feeding but there is something odd about the situation (e.g., a decoy trap or a spread of artificial decoys), hungrier birds are more likely to overcome their fear of the unknown and thus be trapped or shot. Therefore, the reason that ducks shot at night on Chesapeake Bay were fatter than those shot during the day was probably because only the latter group was shot over decoys. Upon discussing the parallels between the results of our respective studies, Dave and I recognized that this phenomenon should be widespread, and also should be of particular concern to waterfowl managers. Waterfowl management relies heavily on the return of bands from hunter-shot birds. The banded sample is usually obtained by attracting ducks into decoy traps, and the majority of band returns come from ducks shot over decoys. Thus, both the banded sample and the band returns are likely to be biased toward ducks in poor condition, rendering suspect any use of data derived from these samples.

A commentary that Dave and I published outlining these arguments (Weatherhead and Ankney 1984) generated an interesting sequence of responses. First, informally we were told that waterfowl managers had long been aware of condition biases and accounted for them in their analyses, although no evidence in support of either contention was ever forthcoming. Second, a critique of our commentary was published (Burnham and Nichols 1985), but none of the arguments was compelling (Weatherhead and Ankney 1985). Third, when critics of the condition bias hypothesis claimed some of the associated ideas as their own (Hepp et al. 1986), we knew the hypothesis had legs. Subsequent efforts to test the hypothesis, including work from Dave's lab (Dufour et al. 1993a), have consistently confirmed that hunters do disproportionately kill ducks in poor condition. Fewer attempts have been made to determine whether samples of ducks trapped for banding are also biased by condition. The limited evidence available thus far does not support this prediction (Reinecke and Shaiffer 1988; Dufour et al. 1993b), but more testing is needed. Overall, however, it appears that thinking about why birds behave the way they do can help explain some old mysteries and reveal previously unrecognized and potentially serious contemporary problems.

I am uncertain whether the preceding examples adequately illustrate how Dave's perspectives in Behavioral Ecology influenced how he thought about biological problems. I am more certain that testimonials from those who have discussed questions about animal behavior with Dave (particularly his students) would attest to the perspicacity of his selectionist thinking. There is one important aspect of Dave's behavioral research, however, that is very tangible. Academics today are increasingly called upon to justify their research in terms of its relevance to society. If one's interest in Behavioral Ecology tends toward the esoteric this societal demand can lead to tortuous reasoning and arm waving of heroic

proportion. Dave has suffered no such difficulties justifying his work. Dave has a passionate interest in waterfowl, a group of organisms of great economic value. Furthermore, as much as Dave is interested in the biology of waterfowl, he is equally interested in promoting management of waterfowl that was firmly based in a scientific understanding of their biology. Thus, Dave's entire body of waterfowl research is an outstanding example of basic ecological research (behavioral or otherwise) that is relevant to society. As Dave's work continues to influence how waterfowl are managed, that legacy should only increase in stature.

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# C. Davison Ankney's Contributions To Population Ecology and Management

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A trademark of Dave Ankney's career has been debate. (Some might say that Dave would be willing to argue with anyone, any time, anywhere, about anything - we are confident that Dave would disagree with that.) As a hunter, a wildlife advocate, an educator, and a scientist, Dave has made substantial contributions to our understanding of population ecology and management, particularly in relation to North American waterfowl populations. In almost every instance, these contributions have taken the form of debate. Whether initiating a discussion, challenging the views of others, or defending his own views, Dave has often been on the leading edge of research directed at some of our most critical questions involving waterfowl populations. Herein we describe some of those issues, the impact that Dave has had in shaping the debates, and his continuing contributions toward understanding waterfowl populations and their management.

## The Condition Bias Hypothesis

Much of Dave's early career, including his graduate work, focused on studies of avian energetics and the evolution of life history traits in waterfowl. From the beginning, it was clear that he was not one to shy away from controversy, nor was he afraid to take on some of the most firmly entrenched hypotheses of the day. One of his early forays into waterfowl management issues, through a commentary published in 1984, dealt with potential biases associated with duck banding, a cornerstone of waterfowl monitoring programs in North America. Together with Pat Weatherhead, Ankney suggested that ducks captured by bait trapping might not be representative of populations if individuals in the poorest condition were most likely to be captured, a possibility they viewed as highly likely. They argued that such a bias could lead to incorrect inferences regarding survival rates, and further, that such data could be misleading in terms of evaluating the effects of hunting on waterfowl populations (i.e., the issue of additive vs. compensatory effects of hunting on waterfowl populations, an ongoing debate which we will mention again later). Their commentary on this subject was immediately challenged by Ken Burnham and Jim Nichols, two heavyweights in the analysis of banding data. The debate had begun, and Dave and his students (including Geoff Bain, Kevin Dufour, Mike Hill, and Keith Patton) have continued to delve into this and related questions in the nearly two decades since then. It is now widely recognized that body condition affects survival probability in birds, and considerable work has been done to explore the possibility of trap bias in waterfowl banding. Regardless of the outcome of ongoing research into this topic, Dave has played a pivotal role in bringing the issue to light, and discussing its ramifications. This was an early example of the role that Dave would come to assume, that of a leading thinker regarding waterfowl population ecology and management.

**The Decline of Black Ducks in Eastern North America**

Few issues have been more passionately debated than the reasons behind the decline of the black duck in much of eastern North America. Loss of habitat, overharvest, and hybridization and competition with mallards have all been suggested as the root cause of the decline. The decline was most prominent in western portions of the range, where mallards had made explosive gains, particularly in parts of Ontario. Though black ducks had actually increased in some areas of eastern Canada, and were far from endangered, the Humane Society of the United States made an effort in the early 1980s to close black duck hunting in the eastern U.S. through a lawsuit, arguing that hunting was perhaps the only significant mortality factor that could be controlled by managers. Here was an issue that appealed to Dave on at least two fronts; both as a passionate defender of responsible hunting, and as a scientist.

In the mid- to late 1980s, Dave Ankney and his colleagues were again front and center in the debate over reasons for the decline of the black duck in much of its former range. A comprehensive research program led by Ankney, his students, and colleagues focused on several fronts, including genetic relationships between black ducks and mallards, geographic trends in mallards, black ducks, and their hybrids in areas where the two "species" occurred together, behavioral studies of mating preferences, and habitat use by both species. First, Ankney et al. used genetic techniques to show that black ducks and mallards were indeed closely related (first with protein electrophoresis, and later with more sensitive techniques involving mitochondrial DNA), arguing that they ought not to be considered separate species (this, of course, sparked a debate). In a paper entitled "Increasing Mallards, Decreasing American Black Ducks: Coincidence or Cause and Effect?" Ankney et al. (1987) demonstrated that the incidence of mallard-black duck hybrids was positively related to the abundance of both species in a given area, and argued that neither habitat loss nor overhunting could explain the decline of black ducks and the concomitant increase in mallards. Rather, black ducks were disappearing from parts of their range because of introgressive hybridization and/or competition with mallards. This paper, too, sparked controversy, but Ankney and colleagues vigorously defended their views. Later work by Lynn Brodsky, under Dave's supervision, examined social interactions and mating preferences between black ducks and mallards, indicating that in some circumstances male mallards were dominant to black ducks. Further research by Todd Merendino into habitat quality and use by mallards and black ducks in Ontario indicated that, in areas where the two species co-occurred, mallards tended to occupy the most productive wetlands, while black ducks were relegated to less productive areas. Additional work with Dave Shutler and Darrell Dennis showed that differential susceptibility to blood parasites could not deter the eastward expansion of mallards into the eastern boreal forest stronghold of the black duck. These lines of research were all consistent with the idea that the decline of black ducks was related to the increasing prevalence of mallards in eastern North America, and so far no other hypothesis has satisfactorily explained these trends. (As an aside, Dave maintains an active interest in the potential role of the Black Duck Militia, a subversive group purported to be involved in the systematic eradication of the black duck from west to east across their range. So far, information has been sketchy, and no credible evidence has been found to verify the alleged activities of the group).

Because of his leading role in researching "the black duck question", Ankney was asked by the Wildlife Society to serve as a member of the Ad Hoc Technical Advisory Committee on Black Duck Conservation and Management, a group that summarized and published a review of our understanding of black duck populations in 1989. In 1996, Dave joined with Tom Nudds and Mark Miller at the International Waterfowl Symposium in Memphis, TN to again review the state of our understanding and recommend research approaches designed to simultaneously test the influence of multiple factors on the relationship between black ducks and mallards. We have come a long way from the possible closure of black duck hunting in the early 1980s to our level of understanding today. Black ducks continue to thrive, and be hunted, throughout their range, and narrow-minded approaches to black duck management are less of a threat today because of Dave's input into this debate.

**The Lead Vs. Steel Shot Controversy**

The use of lead shot for waterfowl hunting has led to lead poisoning in waterfowl, caused by the ingestion of spent lead pellets. Concern over this mortality factor was heightened by early reports that placed potential duck losses in the millions each year in North America. Numerous studies were implemented to evaluate the toxicity and ballistic efficiency of various lead shot alternatives, with steel being considered a suitable alternative by many authors. As a result, the use of steel shot was advocated in both Canada and the U.S., and the use of lead shot was completely banned in the U.S. in the mid-1980s. Canada's initial response was less drastic, preferring instead to ban lead shot only in areas where it was known to be a problem (i.e., the "hot spot" approach). Here again was an issue that involved both waterfowl and hunters, and to which Dave would apply a hefty dose of skepticism.

The conversion to non-toxic shot in the U.S. was met with remarkably little opposition, except perhaps by some hunters who were less than thrilled with the killing efficiency of the new loads. In Canada, Dave Ankney lobbied in support of the hot spot approach over a complete ban on lead shot, publishing his views in an article entitled "The Great Lead Shot Boondoggle" in Angler & Hunter magazine. Some people might find this surprising, but as usual, Dave had examined the lead/steel shot issue in critical detail, and stirred the debate over the wisdom of using steel shot across Canada. He was not convinced, first of all, that annual duck losses due to lead poisoning were in the millions. Rather, he suggested that these figures arose from extrapolation of mortality in a few problem areas in the U.S. to the continent as a whole, and were therefore unrealistically high. Further, ducks spent considerably less time exposed to lead in Canada than they did in areas of the southern U.S., where losses to lead poisoning were higher because lead was more prevalent and birds spent several months exposed to it during winter. He also was critical of analyses that suggested steel was a suitable alternative in terms of ballistics, pointing out that most studies had evaluated crippling effects only in terms of number of shots fired, not in terms of numbers of birds that were actually hit (obviously, Dave argued, if a bird was not hit with steel, it could not be crippled). Steel shot is faster and harder, and patterns more tightly than does lead, making it more difficult to hit a flying target, and less likely to deliver a killing blow. Ankney argued that increased crippling of waterfowl caused by the use of inferior steel shot might well exceed losses that had ever occurred due to lead poisoning in Canada. (Interestingly, some anti-hunting organizations have since begun to use crippling losses as an excuse to ban waterfowl hunting). Alas, this was to be a losing battle, as the hot spot approach

was abandoned when Canadian politicians abruptly announced an outright ban on use of lead shot for waterfowling beginning in 1999. However, Dave's public outcry about the deficiencies of steel shot probably aided the development of two better shot substitutes: Bismuth, which was developed by a Canadian, John Brown, and Tungsten Matrix, developed in England but manufactured by Canadians Ken Elliot and Bob Coe. Regardless of anyone's opinion on lead vs. steel shot, Dave had once again demonstrated his willingness to get involved on behalf of waterfowl and waterfowl hunters, and to bring his critical view of the facts to the table. If nothing else, this was a case where he raised awareness of the danger of blind acceptance of government policy without critical review.

**Overabundant Goose Populations**

We are probably all familiar with the explosive growth in some goose populations in North America, a result of their adaptation to agricultural and urban development, and conservative management practices over the past 40+ years. The dramatic increase in giant Canada Geese, thought to be an extinct subspecies as late as 1963, has been widely hailed as a wildlife management success story, as indeed it is. Lesser snow goose, Ross Goose, and Greater Snow Goose populations also have exploded over the past few decades. However, along with increases of some goose populations to unprecedented numbers have come management challenges. Perhaps nowhere else has Dave Ankney's leadership role been more obvious than in his call for fundamental changes to the way we manage goose populations. Ankney first proposed changes to the Migratory Bird Treaty (MBT) of 1916 in an open letter to the Canadian Wildlife Service in February of 1995; an invited essay was later published in the Journal of Wildlife Management in 1996. Here Ankney made a case not only for harvest liberalization, but also changes to the MBT that would give managers more flexibility in attempting to control overabundant species of geese. Moreover, he advocated the important role of hunters in meeting the management challenges posed by overabundant goose populations. Ankney's (again controversial) proposals included removal of the prohibition on hunting during 11 March-31 August, removal of the 107 day limit on waterfowl seasons, legalization of the commercial sale of waterfowl, and legalization of certain hunting techniques, such as electronic calls, unplugged shotguns, live decoys, and baiting. In Dave's own words, these ideas would be viewed as heretical by many, and once again he found himself at the forefront of one of the largest waterfowl management debates of our time.

Dave's initial calls for change provided the stimulus for a workshop concerning management of overabundant geese in the fall of 1995; the meeting was attended by goose researchers and managers from across Canada and the U.S. From there, the issue snowballed, and debate ensued over the level of harvest that would be required to bring mid-continent lesser snow geese to a more manageable level. As Dave had predicted, his ideas were initially greeted with resistance, from some biologists, bureaucrats, and anti-hunting groups alike. Nevertheless, many of his proposals have been adopted, and we now have spring seasons for lesser snow geese in Canada and the U.S. for the first time in ca. 85 years! Spring seasons were opposed by the Humane Society of the United States, who filed suit in both Canada and the U.S.; both suits eventually failed, and Dave worked tirelessly behind the scenes in support of his original proposals. Some restrictions on method of take also have been lifted, and more are under consideration. Realizing that increased harvest by hunters may not be sufficient to stem the increase in numbers of lesser

snow geese, Dave continues to participate in an international working group that is examining the feasibility of direct control methods, should they be deemed necessary in future. In the more than 8 years that have elapsed since Dave first spoke out on this issue, we have seen fundamental changes to our system of goose management in North America. Throughout that time, Dave's input and opinions have been sought by hunters, the media, the scientific community, and waterfowl managers alike; he continues to be involved on many levels.

**Ongoing Debates in Population Ecology and Management**

Waterfowl researchers and managers continue to be faced with challenges in managing and understanding waterfowl populations, and Dave Ankney continues to apply his own brand of critical thinking to many of these questions. He is a keen observer and participant in discussions of factors affecting lesser scaup, though he may be less inclined to believe that lesser scaup have declined so much as they have failed to increase as expected during years of favorable breeding conditions. He continues to participate in research aimed at determining whether contaminants may have played a role in the scaup decline, perhaps exacerbated by the explosion of zebra mussels, on which scaup feed, in the Great Lakes.

Dave has had an ongoing interest in the evolution of waterfowl management regimes, the latest being termed Adaptive Harvest Management. This system of management uses information about spring mallard populations and water conditions on the prairies to determine one of 3 levels of allowable duck harvest each year (i.e., restrictive, moderate, or liberal season). Dave was probably the first to point out, in a provocative address at the 7$^{th}$ International Waterfowl Symposium in Memphis in 1996, that such a system had been proposed over 40 years earlier by Johnny Lynch, a long-time biologist of the U.S. Fish & Wildlife Service whose ideas had been largely misunderstood or ignored (" Why Did The Ducks Come Back in 1994 and 1995: Was Johnny Lynch Right?"). He went on to suggest that the high duck populations we saw in the mid-1990s were entirely predictable, and would have occurred even in the absence of the North American Waterfowl Management Plan and earlier harvest restrictions.

Dave is a strong proponent of at least two ideas that relate to Adaptive Harvest Management: (1) that duck production on the prairies is inextricably linked to water conditions, and (2) that abundant duck populations cannot be stockpiled through restrictive harvest strategies in the absence of suitable habitat conditions on the prairies (because compensatory natural mortality and lack of production would simply replace hunting mortality in the absence of suitable habitat conditions). There has been considerable debate recently over the decision to allow a liberal duck season in the U.S., despite drought conditions (but relatively high mallard populations) that persist over much of the Prairie Pothole Region of North America. On one side, Dave has spoken out in support of the liberal season, arguing that duck populations will bounce back quickly when favorable water conditions return to the prairies. If drought conditions persist, then the current population of prairie mallards cannot be sustained anyway. On the other side of the debate are more conservative-minded folks who argue that harvest must be reduced in order to protect the populations in the face of declining productivity on the prairies. At the very least, the Adaptive Harvest Management program may provide further insight into the relationships between ducks, water, and harvest over the long term. In the meantime, Dave continues to help shape the debate over harvest management by forcefully voicing his opinions.

## In Conclusion

In a recent e-mail exchange regarding Adaptive Harvest Management, Dave Ankney alluded to a quote describing the mark of a genius: "To see what others see, but to think what others do not.". At the time, he was referring to the genius of Johnny Lynch, who long ago described the intimate relationship between duck production and the wet/dry cycle of the prairies, among other things. A similar quote has been used to describe Charles Darwin, author of "On the Origin of Species", and the grandfather of modern evolutionary theory. Darwin saw what many had seen before him (i.e., the tremendous diversity of species), but was the first to advance the theory of natural selection to describe the evolution of this diversity - his theory changed the course of scientific history. In much the same way, Dave has consistently brought a unique perspective to issues involving population ecology and management that have been considered by many, and has vigorously defended his views. History will judge how often he has been right, but right or wrong, Dave will have contributed much to our understanding of wildlife populations and their management. As Dave has said himself, it is not the job of a biologist to prove that he is right, but to seek the truth. Nevertheless, we predict that he will be proved right far more often than he is proved wrong.

Finally, we have said little about one of Dave's most enduring contributions to the fields of population ecology and management: the education of his students. Over the course of his approximately 30-year career, Dave has instructed hundreds, perhaps thousands, of undergraduate students, supervised at least 45 graduate students, and influenced the thinking of countless colleagues. Many of his students are employed as wildlife professors, researchers, and managers in organizations across North America, where they continue to contribute to our understanding of population ecology and management. All of them have learned, probably through debates with Dave, to be well informed, to communicate effectively, and to seek the truth.

# Dave Ankney's Contributions To Human Biology

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When studying waterfowl and other birds it is routine to examine such life history variables as longevity, clutch size, inter-clutch interval, and body size, in order to determine whether they relate to individual, sex, and subspecies differences in testosterone and other hormone levels. Applying similar analyses to our own species, however, is highly controversial, at least among social scientists.

Back in 1989 when I presented my published research on human race differences, which showed that East Asians and Africans consistently fall at opposite ends of a continuum on over sixty anatomical, behavioral, and social variables, with Europeans consistently intermediate, it caused a furor. The fact that I dared to provide an evolutionary explanation caused me to be tagged as a "racist" and a "Nazi".

**Brain size and intelligence**

No doubt the most politically incorrect of the group differences I found were those on brain size and intelligence (see Rushton, 2000). Controversial or not, there are now over two-dozen studies using state-of-the-art Magnetic Resonance Imaging (MRI) techniques that demonstrate a correlation of about 0.40 between brain size and intelligence in humans. This is about as strong a relationship as is found in psychology, and it raises many important questions. The fact that there are significant racial differences in both brain size and intelligence causes some people to become very uneasy. But facts are stubborn things – the average IQ for East Asians is about 106, the White IQ is about 100, and the Black IQ is about 85, with Blacks in Africa having a lower IQ (70) than Blacks in America. Hundreds of studies on millions of people gathered from countries around the world have confirmed this three-way racial pattern.

But is it fair to use Western-developed IQ scores to compare human races? Yes. First, IQ tests predict achievement in school and on the job just as well for Blacks as for Whites and East Asians. Second, these very same race differences show up on tests specifically designed to be "culture-free" as well as on standard IQ tests. Further, race differences in intelligence test scores parallel those in brain size. Overall, Orientals average 17$cm^3$ more brain matter than Whites, and Whites average 85 $cm^3$ more than Blacks. These racial differences in brain size have been established using several independent methods including weighing wet brains at autopsy, measuring the volume of an empty skull, and measuring the outside of the head. The pattern of race differences in brain size remains after adjustments are made for body size. Since each $cm^3$ of brain matter contains millions of brain cells and hundreds of millions of connections, brain size differences probably explain part of the reason races differ in IQ.

In one of my early studies, I used external head size measures gathered from a sample of thousands of U.S. Army personnel. In 1988, the Army carried out an anthropometric survey on a stratified random sample of 8000 military personnel for the purpose of sizing helmets, clothing and workstations. Individual body – and head-size measures were available separately for men and women, officers and enlisted personnel, and those who identified themselves in the Army questionnaires as being White, Black, or Asian/Pacific.

I entered the length, width, and height of the Army head measures into the appropriate formula and calculated each individual's cranial capacity. I then aggregated these data separately by sex and found that for both men and women, as well as for officers and enlisted personnel, East Asians averaged larger cranial capacities than did Whites who averaged larger cranial capacities than did Blacks. (In each race, officers averaged larger cranial capacities than did enlisted personnel). In short, I had replicated previous findings for an especially large data set that allowed excellent controls for body size.

It is at this point in the story that C. Davison Ankney takes center stage. Dave and I had become friends shortly after my work on race had hit the headlines in early 1989. He turned out to be a superb colleague as well as a fearless defender of academic freedom, a topic to which I shall return. He pointed out fascinating intellectual issues raised by my earlier research and willingly shared with me the benefit of his well-recognized expertise in allometry and taxonomy.

**Sex differences in brain size**

Although Dave was impressed with the initial results from the U.S. Army data, he quickly raised an important question. Why had I not analyzed the data to see whether there were sex differences in brain size in addition to the military rank and race difference? My answer was that it was already well established in the literature that there were no sex differences in brain size when body size was controlled and so I had presented the results as the anthropometric survey's statisticians had given them to me – the U.S. Army found that men and women differed in so many ways in body size and proportion that it reported *all* their charts and tables separately by sex, without formally testing for the significance of the differences.

Dave wasn't at all satisfied with that response. "How can you make a 17% difference in absolute brain size 'disappear' by correcting for a 10% difference in body size," he asked? "I'm not suggesting that you jumble the males and females together into one big analysis, but that you report the analysis for sex differences *in addition* to those for race and rank differences. If there is no sex difference, then this will establish that fact and it will make the findings of race and rank differences that much cleaner."

I was reluctant about re-doing the analyses. For one thing, the data were in the hands of the U.S. Army's anthropometric survey team and they would have to be paid for doing the extra analyses, if indeed, they would even agree to do them. Also, it meant carrying out a more complicated 3 x 2 x 2 analysis of variance design and I was concerned about the possibility of muddying the waters if some interaction effects obscured the picture.

Then Dave asked to see one of the papers I kept citing to him as proof that there were no sex difference in brain size after controlling for body size. Thinking it would end our debate I sent him an autopsy study through the campus mail. Carried out by Kenneth Ho and his colleagues at Case Western Reserve University on 1,261 American adults, black and white, men and women, it had been published in the 1980 Archives of Pathology and Laboratory Medicine. That very weekend, however, Dave telephoned me at home to tell me that he'd found a remarkably large sex difference in brain size favoring males -- 100 grams, about the size of a quarter-pounder. He had also realized why it had gone unnoticed for so long – because earlier studies had used improper statistical techniques, and, thus, incorrectly made a large difference "disappear." (The error was using brain-mass to body size ratios in place of the analysis of covariance: see Ankney, 1992, following Packard & Boardman, 1988).

Dave's reanalysis of Ho et al.'s autopsy data showed that at any given body surface area or height, the brains of White men are heavier than those of White women as are the brains of Black men heavier that those of Black women. For example, among 168 cm (5'7") tall Whites (the approximately overall mean height for men and women combined), the brain mass of men averages about 100 grams heavier than that of women (Figure 1), whereas the average difference in brain mass, uncorrected for body size, is 140 g. Thus only about 30% of the sex difference in brain size can be attributed to differences in body size.

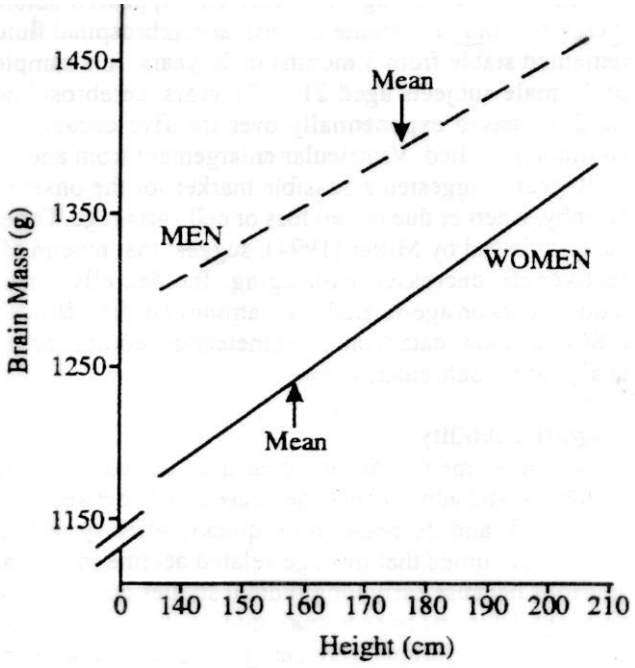


Figure 1. The relation between brain mass and body height in White men and women. Lines drawn from equations in Ho et al. (1980, Table 1): men, brain mass = 920 g (± 113) = 2.70 (±.65) x body height ($r$ = .20, $p$< .01): women brain mass = 748 g (± 104) = 3.10 (±.64 x body height ($r$ = .24, $p$ < .01). From Ankney (1992, p.333)

This was a remarkable finding and one that completely overturned the accepted view on the topic. But the results also presented a paradox. Women have proportionately smaller brains than do men, but apparently have the same average overall IQ. Dave had an answer for this. He hypothesized that the sex difference in brain size must relate to those intellectual abilities at which men excel more than women. Most notably, these include spatial and mathematical abilities, particularly dynamic spatial ability such as rotating an imaginary object, shooting at a moving rather than a stationary target, or catching a fly ball. Such tasks, Dave reasoned, must require more "brain power." He drew an analogy with computers: Whereas increasing word processing power requires some extra capacity, increasing three-dimensional processing, as in graphics, requires a major increase in capacity.

What Dave said made a lot of sense. It dovetailed, for example, with all the findings on sex difference made by Doreen Kimura, my colleague at the psychology department at Western, who was internationally renowned for her work on sex differences in cognitive ability. Kimura (1992) had demonstrated that women average higher in verbal ability, perceptual speed, and motor coordination within personal space, whereas men do so on various spatial tests and on tests of mathematical reasoning. Reviews show that on the "purest" measures, the sex difference approached 1 standard deviation, which is about the magnitude of the difference in brain size (Kimura, 1999; Voyer, Voyer & Bower, 1995).

All this was intellectually very exciting. So I went back to the U.S. Army data and requested the analyses to be done separately by sex as well as by race. Their results completely confirmed Ankney's. After adjusting, via analysis of covariance, for the effects of age, stature, weight, military rank and race, men averaged 1,442 $cm^3$ and women measured 1,332 $cm^3$. This sex difference of 110 $cm^3$ found by analyzing external head measurement is remarkably close to the 100 grams obtained by Ankney from analysis of brain mass (1 $cm^3$ = 1.036 g).

Yet for me, doubts still lingered. I wondered whether the sex difference might disappear, or at least be substantially reduced, if body size were controlled in some other way. That would make the picture as clear as possible for race differences. Since I was already controversial enough for having found race differences in brain size (and now military rank differences too), I was loathe to dive into yet another pot of boiling water by documenting sex difference in brain size as well. However fascinating they might be, sex differences in brain size seemed too dangerous a detour to me.

Dave insisted that his results were solid and that he would go ahead and publish them. My U.S. Army data would thus be the first replication study. Dave assured me there was simply no way we would be able to make so large a difference disappear. But he agreed to do the statistics on the Army data and carried out dozens of additional analyses (the results of about 20 of which are shown in Figure 2). All of them were done to rule out any possible body size effect (see Rushton, 1992: 406-408). As can be seen in the figure, the sex difference was replicated across samples of Asians, Whites, and Blacks in all the analyses. Also (not shown in the figure) it was replicated across officers and enlisted personnel.

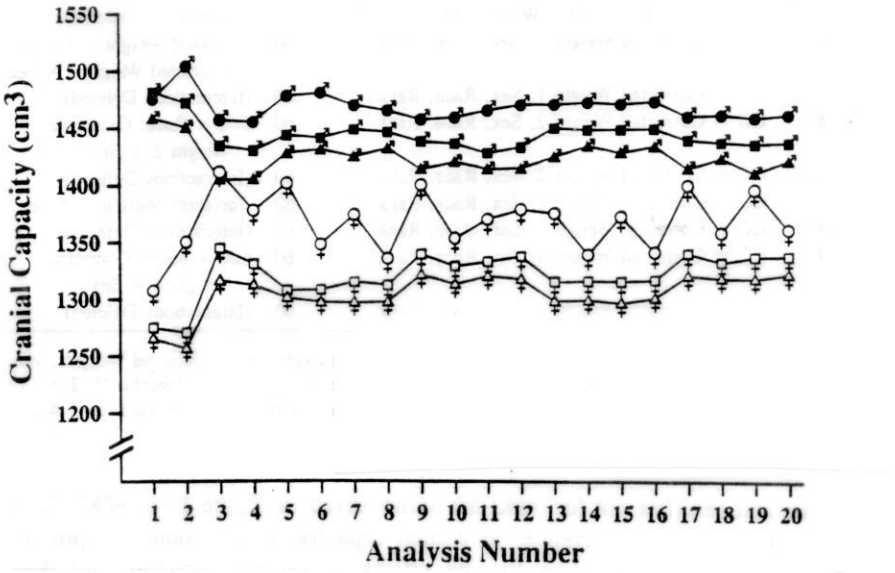


Figure 2. Cranial capacity for a stratified random sample of 6,325 U.S. Army personnel. The data, grouped into six sex-by-race categories, are collapsed across military rank. (Asian men, closed circles; White men, closed squares; Black men, closed triangles; Asian women, open circles; White women, open squares; Black women, open triangles). They show that, across the 19 different analyses controlling for body size, men averaged larger cranial capacities than did women, and Asians averaged larger than did Whites or Blacks. Analysis 1 presents the data unadjusted for body size showing no difference for Asian and European men. Adapted from Rushton (1992a p. 408).

Ankney's findings of the sex difference in his reanalysis of Ho et al.'s autopsy data, and my replication thereof in the U.S. Army data set using external head size measure, were both published in the 1992 issue of the journal *Intelligence.* As I expected, we shortly thereafter found ourselves at the center of a storm of controversy, though much smaller than the race one. The prestigious science magazine, *Nature*, to whom I had initially submitted the U.S. Army data (but who had declined to publish it), now ran a full-page editorial signed by John Maddox, its editor, entitled "How to Publish the Unpalatable?" *Nature*'s editorial described the data on sex, rank, and race differences in brain size quite fairly, but then went on to suggest, with no empirical support for the assertion, that the results were due in some way to selection bias in the U.S. Army, or to uncontrolled body size variables. Dave's controversial results also led to a front page story in the Toronto's *Globe and Mail* and to several months of correspondence in *Nature*, the *New York Review of Books*, and *Discover Magazine*. I don't know if anyone else has noticed, but Dave is really able to rise to the occasion when the good fight has to be fought! In this particular controversy he certainly gave a lot better than he got!

That was not all! Dave now agreed that we publish together. We published six joint papers together over the next 10 years, with still another "*in mento*," as Dave likes to say (Ruston & Ankney, 1993, 1995a, 1995b, 1996, 1997, 2000). We also had four joint conference

presentations, including one given at the 1996 meeting of the American Association for the Advancement of Science. This is in addition to the two papers Dave published alone on sex differences (Ankney, 1992, 1995) and numerous letters-to-the-editors and telephone interviews given to reporters.

The most important of Dave's and my joint papers was the review we undertook of the entire literature on whole brain size and normal cognitive ability, not just of sex and race differences, but also including those on age and social class differences. Published in the 1996 issue of the journal *Psychonomic Bulletin and Review*, it concluded that the direction of the brain-size/cognitive-ability relationship were more accurately described in the nineteenth-century by such visionaries as Paul Broca and Francis Galton than by the many contrarian, egalitarian advocates to the contrary over the last half century. Political correctness had nearly buried some very well established scientific research.

Our 1996 review also amply confirmed the sex difference published by Dave in 1992. For example, we re-examined autopsy data on children and adolescents and found the sex difference held from birth through early months. We also found the sex differences in children aged from four to seven years using external head measurements. From 7 to 17 years, sex differences in cranial capacity are in the range of 60 to 100 $cm^3$, or from 0.60 to 1.00 S.D. By this time, other researchers were also reporting the relationship using MRI (e.g., Andreasen et al., 1993; Harvey et al., 1994). Any doubt that might have remained was dissipated when Pakkenberg and Gundersen reported in the 1997 issue of the *Journal of Comparative Neurology* that the average number of neocortical neurons was 19 billion in female brains and 23 billion in male brains, a 16% difference. Using modern stereological methods to count the number of neurons in the brain, Pakkenberg and Gundersen cited Ankney (1992) as central to their rationale for doing the study.

Ankney's finding of a sex difference in brain size has also led to a resurgence of interest in the question of whether there is an average sex difference in *general* intelligence. British psychologist Richard Lynn (1994, 1999) has been especially concerned to resolve the sex difference in brain size paradox – what he refers to as "the Ankney-Rushton anomaly." (Its not everyone who has an anomaly named after them!). Lyn presented evidence that argues against the consensus view that there is no difference in general intelligence. Reviewing data from Britain, Greece, China, Israel, the Netherlands, Norway, Sweden, Indonesia, and the United States, he showed that men averaged about 4 IQ points more than did women on a number of published intelligence tests. Another colleague at Western, Douglas N. Jackson (1995) reported a similar magnitude advantage to men in general factor of ability extracted from data from the Scholastic Aptitude Test (SAT; N=112,516 individuals). Also Stumpf and Jackson (1994) reported a half-standard deviation advantage to men in reasoning ability extracted from data on 180,000 German medical school applicants.

**The evolutionary selection of brain size**

Dave's and my most recent collaboration (Rushton & Ankney, 2000) involved yet another re-analysis of those U.S. Army data, examining race and sex differences in head shape, along with head size, along with data on fossil hominids. We found that over evolutionary time, as brains

became larger they also became more spherical. An evolutionary sequence fits with and can perhaps explain how and why the races differ both in head size and in head shape (see Figure 3.).

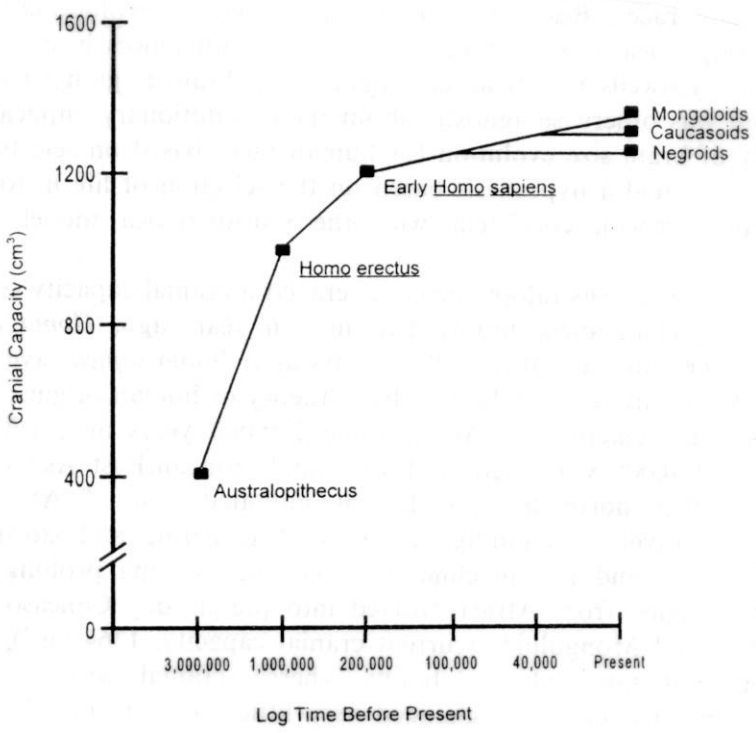


Figure 3. Increasing cranial capacity over evolutionary time.

Three million years ago, Australopithecenes averaged a cranial capacity of less than 500 $cm^3$ (about the size of a chimpanzee brain): two million years ago, *Homo erectus* averaged a capacity of about 1000 $cm^3$; and 0.25 million years ago, *Homo sapiens* averaged a capacity of about 1200 $cm^3$. According to the "African Eve" theory of human origins (Stringer & McKie, 1996), modern humans emerged in Africa some 200,000 years ago, with an African/non-African split about 110,000 years ago, and with a European/East Asian split about 41,000 years ago. The further north the populations migrated, out of Africa, the more they encountered the cognitively demanding problems of gathering and storing food, acquiring shelter, making clothes, and raising children successfully during prolonged winters. As the populations that migrated out of Africa evolved into present-day Caucasoids (current average cranial capacity, 1347 $cm^3$) and Mongoloids (current average cranial capacity, 1364 $cm^3$ ), they did so in the direction of larger more spherical brains, whereas cranial capacity and head shape of populations that remained in Africa changed very little (current average cranial capacity, 1276 $cm^3$).

**Academic freedom issues**

For many people, of course, these findings are highly controversial, or even inflammatory. Perhaps we are wrong in some of our analyses. Errors occur in all empirical sciences. That is why the only way to find out if one is wrong is to have the academic freedom to publish and

discuss these issues. Unfortunately, many forces of political correctness would close down even the right to discuss these matters, let alone research them.

Dave's belief's in the vital importance of unfettered scientific enquiry led him to play a key role in defending academic freedom in Canada. He certainly played an invaluable role in defending *my* academic freedom. I know I shall be forever grateful for the wonderful letters that he wrote on my behalf when I ran afoul of political correctness in 1989 when the media began a witch-hunt, the premier of Ontario called for my dismissal, the Ontario Provincial Police and the Ontario Human Rights Commission investigated me, and some university administrators bayed for my blood. Dave stood like the Rock of Gibraltar: completely reliable and absolutely unbudgeable. I happen to know that his letter in particular carried special weight in my department , with my dean, and with the administration.

When the U.S. -based National Association of Scholars, an academic freedom society, intervened on my behalf, Dave lent his considerable energy and prestige to starting a sister organization in Canada, the Society for Academic Freedom and Scholarship. A founding member, he was elected to its first Board of Directors, and was active in many of its early cases, writing letters on its behalf, proffering expertise, and counseling injured parties. Although this took precious time away from his already busy schedule, Dave has never wavered in his personal commitment to preserving personal liberty and freedom of enquiry for all his colleagues.

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# Dave Ankney's Contributions to Community Ecology

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## Introduction

When one first looks over the program for this symposium, it might be easy to wonder, given Dave Ankney's considerable reputation in the area of bioenergetics and life history evolution, what on earth could be added in the areas of behavioural ecology, population ecology and community ecology. Did Dave "do" those, and especially the last?

Certainly, it can be said that Dave "did" these things in the sense that his research program was as broad in various aspects of avian ecology as it was deep in avian bioenergetics (something that the symposium organizers tried to "capture" by the range of topics). But that's not all there is to it. Even allowing for our tendency, as community ecologists, to see everything as community ecology (or at least more interesting if it could be community ecology!), we assert that Dave Ankney "did" more community ecology than maybe even Dave knows. To appreciate this, it is important to step back and consider three ways in which it is legitimate to consider how Dave "did" community ecology.

First, Dave nurtured community ecology in an academic environment that, generally, promoted free thinking and independence and he accepted students to work on topics that weren't necessarily the focus of his "mainstream" research. Second, he found ways to support those students through his extensive connections and with his considerable experience, such that ideas developed in his lab were able to take root and flourish elsewhere, and without which such things never would have happened (and each of us has a personal tale to weave through the historical fabric of this essay about how we benefited). To see that, will require a little discussion (1) about the field of community ecology, generally; (2) about how it, and waterfowl community ecology specifically, evolved since the beginning of Dave's career when he advised his first student of community ecology on a project about resource partitioning by diving ducks at Long Point; and (3) about where some of the ideas that were nurtured in the Collip Building eventually led. Third, against that backdrop, and with a little further reflection on the list of thesis titles and published papers from Dave's research group, it should become readily apparent why we contend that Dave did more community ecology than maybe even he knows.

## The environment that nurtured waterfowl community ecology

Community ecology, as practised by animal ecologists at least, was still very young in the mid-1970s when Dave took up his faculty position at Western. R. H. MacArthur, working as a graduate student in the lab of G.E. Hutchinson, only 20 years earlier had begun the process that eventually "stole" community ecology from the "plant sociologists", who were still infatuated

with the mere spatial arrangements of plants, and less so about *why* they were that way. MacArthur and others in his generation added exciting, theoretical and quantitative dimensions to the field that led ecologists much later to remark that community ecology was going nowhere until the animal ecologists got interested (e.g., P. Yodzis, pers. comm.). Those additions included concepts like n-dimensional niches, theory like Lotka-Volterra competition equations, and Rosenzweig-MacArthur predator-prey dynamics that became the wellhead from which sprang much of modern, quantitative community ecology.

At the same time, in the field of wildlife biology, perhaps transfixed by Aldo Leopold's 1933 definition of wildlife management as the production of sustained annual crops of wild game, "wild life" continued to be treated less in an ecological context, than in an agricultural one. The fields of ecology and wildlife biology and management (understandably, given the state of affairs in what was called ecology then) hadn't had much in the way of cross-fertilization for many decades.

In the mid-1970s, I (TDN) stumbled on ducks as a neat system to study from a community ecological perspective. I had only started to appreciate the lack of cross-over between the ecological and wildlife literatures when a final, take-home exam question in R.T. M'Closkey's graduate class in community ecology pushed me over the edge and into the radical middle ground: "Pick and justify any group of species to test any hypothesis of interest in community ecology. Present your answer as a fully fleshed out proposal on the 4-page NSERC grant application form provided." Ducks! Those big gaudy birds that comprised several species (that even I could tell apart) that co-occupied the marsh I'd been driving by weekly, where (because of their outward similarity) they should be competing, bobbing around right there in the open. I had noticed them before, but I hadn't really SEEN them, and the more I looked into them, the more interesting they became. But where to find a PhD advisor who was at once interested in ecology and waterfowl biology? Then I saw Dave Ankney present his "egg-size in snow geese" paper. Here was the guy: waterfowl biology and evolutionary ecology all wrapped up together. One stop shopping. And the fact that I had to start from scratch on the matter of duck biology didn't phase him.

Once at Western, Dave's connections and influence proved invaluable to solve a second problem: finding the location and money to study duck community ecology. In those days, the kinds of divisions between game and nongame species interests that are in some circles still evident, unfortunately, ran even deeper. It was tough to use "ducks" and "niche" in the same proposal. For the game-types, it was "too theoretical, and of no practical importance" – one species at a time was the way to study ducks – and nongame-types said: "Ducks!?!? Those are 'management species'! Go ask them." Dave engaged his colleagues D.G. Dennis of the Canadian Wildlife Service (CWS) in London to advise me on a site for fieldwork, and D.L. Trauger (then at Northern Prairie Wildlife Research Station in Jamestown, North Dakota), who gave me a basement full of data from old waterfowl surveys that comprised counts of individuals of many species of ducks on wetlands of variable sizes, shapes and permanency. It hadn't occurred to me that it might be more productive to start in the archives and than to jump straight into the field, and the data turned out to be a gold mine. In turn, Trauger asked his counterpart at CWS in Saskatoon, J.H. Patterson, whether Patterson would pay for the analysis of the data. Patterson eventually agreed, but Dave Ankney had put it all in motion, including cajoling a Dean of Arts

and Science into dipping into his slush fund (Deans had those then; they may now, but won't admit it) to pay for an airline ticket so I could get to Saskatoon to argue the case for the money.

**Duck community ecology then and now**

*1970s-1990s: Resource partitioning and coexistence*

Between the late 1950s and the mid-1970s, community ecologists generally were consumed by the question posed by G. E. Hutchinson: "why are there so many species?". The focus was on species that fed similarly (shared space at a particular trophic level) and the issue was how they could all coexist when Gause's competitive exclusion principle implied they shouldn't – unless they were different *enough* in some way that they didn't, in fact, compete. A great deal of community ecology at the time consisted of research that set about to document the ecological differences among the coexisting species that must clearly be there. It was merely a matter of looking until such a difference was found (which could virtually always be done in one creative manner or another) and then proclaim data consistent with "competition theory". Into this "factory" of work on resource partitioning in the mid-1970s waded J.E. Connell, J.A. Wiens and, later, the "Tallahassee mafia" who argued that "predators and hurricanes could change ecology", by which they meant that competition might not be of overarching importance in determining how many species could coexist. With the old data that Trauger had handed over, I concluded that "hurricanes" could, in fact, change ecology, or at least that competition was weaker among dabbling ducks in temporally variable environments than it was among diving ducks in less variable environments, as Wiens had suggested.

The focus was limited, however, to a single trophic level and the topic was still about the extent to which, and how, resources (food and habitat) were partitioned among species. At Western with Dave, we jointly considered how ecomorphology related to the assembly patterns of waterfowl species on wetlands, and co-authored a paper that foreshadowed a 15-year-long disagreement with Finnish colleagues about the relative importance of different morphological traits to coexistence among ducks. H. Pöysä completed a thesis about resource partitioning in dabbling ducks, the main conclusion of which was that, despite much evidence of differences in habitat and foods, they didn't compete. Further, he suggested that neck length was the morphological trait that determined how species divided habitat (such that longer species fed deeper in the water column) – the exact opposite of what would be expected on the basis of differences in lamellar density, prey size and filtering efficiency (species with finer lamellae, that is, generally the smaller species, feed in deeper water and smaller prey), as we had elaborated. The disagreement was recently reconciled as due at least as much to differences in the kinds of wetlands in which we studied as to differences in methodologies. But, although much had been learned about ecomorphological relationships and coexistence among ducks over its course, the debate, in retrospect, was a sideshow.

The "real" question, of both theoretical and practical importance – Do ducks compete for limited resources? – was likely only to be answered by better controlled experiments that simultaneously took account of the role of predators and whether they might mediate any competition between ducks. After all, the entire field of community ecology was moving in the direction of controlled field experiments and trophic dynamics, even if birds, at the time, were considered to be "tourist

species", i.e., consumers not able to affect trophic structure strongly because of their transient nature in food webs. Waterfowl community ecology had to make a full-blown move to the study of whole foodwebs, an explicit consideration of trophic dynamics, and an escape from the tyranny of the single trophic-level perspective. But how to do it? I really wasn't sure, and it didn't become apparent until Dave Ankney intervened, in a manner of speaking, yet again, by suggesting that Diana move up the highway to Guelph to study for her doctorate.

*1990s-the present: Waterfowl in food webs*

While Tom was contemplating competing ducks, I (DJH) was growing gypsy moths. Endearing though they were, I decided to spend my time studying something that moved a bit faster for my MSc. I visited Dave Ankney, who described a project he and R.C. Bailey were starting that dealt with the interaction between this new food source (zebra mussels) and ducks in the Great Lakes. At the time I knew nothing about ducks (judging by my competence at separating decoys of diving and dabbling ducks at a Long Point party), but I was interested in predator-prey interactions, and jumped at the opportunity to deal with ducks in the context of a broader system. Dave enthusiastically participated, encouraged, and advised me along the way.

During the year after I left Western, I watched eiders feeding on blue mussels (my duck identification skills had improved by then) and wondered about the effect of all this predation on mussels and the rest of the intertidal invertebrates. In Lake Erie, the effect of ducks on zebra mussels had been substantial, but short lived, largely because ducks were only present for a few months each year. In the Bay of Fundy, eiders were present year-round, so lasting effects seemed much more likely. I started searching the literature on intertidal systems and ducks, and found that there was literally no overlap (sound familiar?). Intertidal community ecology was extremely well developed, but vertebrate predators were rarely considered in studies of intertidal food webs. Waterfowl, in particular, got no attention, despite the fact that ducks are frequently members of intertidal and near-shore marine communities. I thought that we had to be missing something.

Dave suggested I talk with Tom, who, as it turned out, had visions that the type of project I was talking about might be done in the prairies, but he was willing to let me tackle it in St. Andrews. What goes 'round comes 'round. After starting at Guelph, I asked Dave to be on my advisory committee; he had experience with eiders eating blue mussels, and I valued his input. He kindly agreed, and his involvement in community ecological research continued. Using techniques developed while I was at Western (Dave's influence again), modified for use in the intertidal zone, I set about to exclude ducks from certain areas, some of which were artificially disturbed to simulate effects of ice or severe wave action in one area, and rockweed (seaweed) harvest in the other. The idea was to figure out what effect ducks had on the system, and determine whether disturbance altered it. Given that ducks were clearly among the top predators in the system, I thought I might find all sorts of indirect effects stemming from their predation, and perhaps even a top-down trophic cascade. What I found was unexpected: eiders significantly altered mussel abundance in mussel beds, but had little effect in rockweed. However, predation also had a huge indirect effect on dogwhelks (predatory snails): whelks congregated under exclosures as ducks reduced mussel abundance elsewhere. Thus, ducks actually competed (in a lopsided way) with whelks in the system and, as a result, any trophic cascade that might have resulted from duck

removal was blocked.

The "duck effect" was so striking, that an editor strongly encouraged me to label them a "keystone predator" – not bad for birds that had been all but ignored in the intertidal literature in particular and the food web literature more generally! Eventually, G. Benoy did carry out the similar experiments in prairie potholes Tom had envisioned, and found similar strong "top-down" effects of duck predation that DID cascade down to the primary producers in those diverse and highly productive systems – exactly where trophic cascades are not, in theory, supposed to be found! And there, ducks competed as strongly with tiger salamanders as I found eiders did with dogwhelks, yet the trophic cascade did not attenuate, as theory predicts. We are poised to learn yet more of the significant role of waterfowl predation in food webs, whether they limit the abundance of their foods, and whether they, in turn, are limited by predators in the trophic level above them....and Dave Ankney set it all in motion.

In this modern context, "community" has become synonymous with "foodweb", and the "duck community", with a "taxocene". And, gratifyingly, it has been acknowledged in "management circles" that the next frontier will necessarily need to take account of the reality of interspecific interactions (i.e., more than one species at a time) to understand and improve the reliability of management actions. Considering, then, how community ecology has evolved to the modern view of "communities" as synonymous with "whole foodwebs", it is relevant to return to the question of whether Dave Ankney "did" community ecology. Certainly, he has had keen insights to the ecology of foodwebs. Most readers will be excused, however, if they overlooked his published work on trophodynamic relationships in the mid-1990s. Here, it is reproduced in its entirety from the Globe and Mail:

**Your Morning Smile**

Mistakenly, a Texan walks into a vegetarian restaurant and orders the daily special. When it arrives he looks aghast at the plate and proclaims: "That's not dinner! That's what dinner eats!"

*Dave Ankney, London, Ontario*

**A retrospective look at community ecology in Dave Ankney's research group**

But, seriously, as we recall the contributions that Dave's research group has made to community ecology, it is plain that they also reflect this same evolution and emphasis from early studies on resource partitioning to explicit considerations of trophic interactions. Before any graduate students had produced anything in the area of community ecology, Dave (1977) himself had reported kleptoparasitism by snowy owls on duck hunters. As already mentioned, D. Smith (1979) was Dave's first student of community ecology; he examined resource partitioning in diving ducks staging at Long Point. At about the same time, J. Eadie (1979) undertook his undergraduate thesis work on interspecific variation in foraging behaviour of dabbling ducks, also at Long Point. These studies were followed shortly by T. Nudds' (1980) work on habitat and food partitioning by dabbling and diving ducks in prairie Canada, D. Abraham's (1982) study of

resource partitioning by Arctic gulls and terns, and I. Goudie's (1984) studies of food and habitat partitioning by seaducks. Some of the first work on topics that would, by the 1990s, be recognized as trophic ecology, and thus legitimately in the realm of the "new" community ecology, emerged at this time: T. Quinney's (1985) work on foraging by tree swallows and P. Kehoe's (1985) undergraduate thesis work about variation in "internal morphology" that promoted diet differences among five species of diving ducks at (you guessed it) Long Point, and later (1988), about diet diversity and its effects on gut structure in mallards, with Dave and R. Alisauskas.

By 1987, Dave was leading a multi-faceted series of studies (spanning at least 4 theses and 11 published papers) into the nature of interspecific relations between two species, mallards and American black ducks, that lasted until 2000 with the completion of the A. Hanson's dissertation (which, although it dealt with black ducks, was itself prompted by the question of measuring the background variation in black duck abundance necessary to try to understand the influence of mallards at large spatial scales). These studies, no less community ecology simply because they dealt with only two species, involved students, postdoctoral fellows and colleagues in and out of academics – D. Dennis, R. Bailey, A. Hanson, L. Brodsky, T. Merendino, J. Avise, D. Hoysak, D. Shutler, M. Miller and T. Nudds.

While this was afoot, B. Lagerquist (1989) documented interspecific differences in bill and tongue morphology of diving ducks, S. Gilliland (1990) studied predation by black-backed gulls on eiders in the Bay of Fundy, and D. Hamilton (1992) was piecing together the effects of predation by diving ducks on zebra mussels in Lake Erie, these latter studies clearly advancing into the realm of trophic community ecology, insofar as each dealt directly with predator-prey interactions. Later, J. Thompson (1996) worked out the interspecific relationships between buffleheads and Barrows goldeneyes with respect to diets and digestive tract morphology, and S. Badzinski (1998) and J. Gleason (in progress) investigated competition between snow geese and Canada geese on Akimiski Island. By 2000, E. Osnas had used null models to significantly improve on the kinds of inferences Nudds was allowed 20 years earlier about the strengths of interspecific interactions, like competition, among diving and dabbling ducks on prairie wetlands, and in 2002, S. Badzinski was discerning the effects of swans on the distribution and abundance of ducks at Long Point. In all, published works in the realm of, or related to, community ecology comprise about 20% of all of Dave's refereed publications, and almost 33% of all of the theses produced from Dave's research group.

**Conclusion**

This essay has provided us an opportunity to write, for the record, that whatever comes of wondering what's going on with more than one duck species at a time, none of it was likely ever to have happened if Dave Ankney wasn't in the habit of encouraging graduate students to do their own thing, and then watching their backs as they did it. We contended at the outset that Dave certainly "did" community ecology, in the manner in which he advised students with a wide range of interests, in which he supported those students, and in the nature of the studies themselves, even as the field has evolved. We trust that our thesis is well-supported by this review: Dave "did" community ecology, he did a lot of it, and he did it well. And we thank him.

# A Scientist in the Socio-Political Arena:

# Dave Ankney's Contributions to Fish and Wildlife Conservation in Ontario and to the Ontario Federation of Anglers and Hunters.

***Rick Morgan[1] and Terry Quinney[2]***

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[2]Ontario Federation of Anglers and Hunters, P.O. Box 2800, Peterborough, ON K9J 8L5

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Rick Morgan:

Our comments will naturally be focused based on Dave Ankney's extensive involvement with the Ontario Federation of Anglers and Hunters (O.F.A.H.), but I am sure you all recognize that Dave has also made contributions to organizations such as the National Wild Turkey Federation (N.W.T.F.), Ducks Unlimited (D.U.), Delta Waterfowl, the Long Point Bird Observatory/Bird Studies Canada and others operating in similar spheres.

Whenever I reflect back on Dave's many years of contributions to and through the O.F.A.H., I think about the unique way he started with the organization...unique at least for a scientist. We have had the assistance and advice of other highly qualified scientists, but usually only after we have identified and approached them to request their assistance. Dave was different. First, he felt that everyone who hunts or fishes and enjoys the outdoors should support an organization working on their behalf, so he and Sandi purchased memberships in the Ontario Federation of Anglers and Hunters. Then, he wrote a letter to me, in my capacity as O.F.A.H. Executive Vice President, explaining that he was a scientist, researcher, professor at the University of Western Ontario, etc., and offered to help our organization in any way he could. It seemed too good to be true, so I thought I'd better check this guy out before responding. That checking around led me to understand that Dave was highly intelligent and dedicated, a hard-core resource user, and generally "a good guy." So I contacted him and took him up on his offer; an offer that would ultimately lead to Dave Ankney, the scientist and hunter and angler; devoting thousands of hours to the O.F.A.H., and making a significant difference through his efforts.

Even before he was fully immersed in his O.F.A.H. duties, Dave was working locally with the Ministry of Natural Resources (M.N.R.) and advocating an Ontario wild turkey reintroduction program. He asked me to push the idea with provincial politicians and M.N.R. senior officials. As a result of those early efforts, and the subsequent efforts of many others, the reintroduction program was approved and the rest is history. We already have over 35,000 wild turkeys in Ontario; we have been trapping and transferring our own Ontario birds; we have hunting seasons

over almost all of the historical turkey ranges; and we have trained over 45,000 new turkey hunters in mandatory education programs. The Ontario wild turkey success story needed not only a lot of effort, but also much money, and Dave served on our very first fundraising dinner committee, a dinner committee that showed the way for all others which followed by netting over $50,000 for this important conservation project.

At about the same time, the O.F.A.H. amended its bylaws to provide for four Directors-at-Large, and Dave was one of the very first elected to this significant position. He then became Chair of the Federation's Wetlands and Migratory Birds Advisory Committee. Small Game was later added to his responsibilities.

Dave made a difference at his very first O.F.A.H. Board of Directors meeting. He got involved. In fact, I specifically recall him expressing concern that nonlead shot legislation would very soon be forced upon us, for political, not scientific reasons. I suggested he develop a position paper on the matter; he did one advocating the "hot spot" approach, and it astutely guided O.F.A.H. action on this item for many years, stalling for several years the inevitable politically-based government policy which ensued. As an O.F.A.H. Director-at-Large, Dave weighed in and contributed to many subjects and areas of concern for our Federation and Canadian conservationists. He ultimately served some time on all of the Board's advisory committees, particularly the O.F.A.H. Big Game Advisory Committee and O.F.A.H. Fisheries Advisory Committee.

Dave's talents and dedication were recognized by his early election to the O.F.A.H. Executive Committee as a Vice President. He then rose to the Presidency, where he served an unusual three consecutive terms. Coincidentally, the years in which Dave served as O.F.A.H. President turned out to be a difficult and controversial period. The province was then governed by a "social democratic" party which had what some described as a "natural resources giveaway program" and an "inequality initiative," but which the government would call its aboriginal agenda. It put most anglers and hunters at odds with the government. The O.F.A.H. insisted that the government priorities must be conservation and equality. This led to conflicts, name calling, and insults. True to his nature and his dedication, Dave did not back away from the fight or the controversy. Several times we traveled to Queen's Park and to aboriginal communities to express our Federation's concerns and to seek common ground.

Working closely with Dave as President was rewarding and enjoyable, and at times a challenge. As you might expect, asking Dave to exercise diplomacy was not an easy task. As we've all learned from time to time, Dave doesn't "suffer fools lightly," and he is not adverse to stating what he really thinks. But, he fulfilled his responsibilities admirably, clearly ranking as one of our most capable and dedicated Presidents ever. And, unlike many, Dave didn't fade away once his terms as President were up. Nine years later he continues to be incredibly active as both a member and a Director-at-Large. Since his Presidency he has won several awards including the O.F.A.H. Professional Conservationist Award, the Wild Turkey Award of Merit, and others.

Among the many very positive things Dave has done for his Federation was recommending the hiring of one of his former doctoral students, Dr. Terry Quinney, as Provincial Coordinator of

Fish and Wildlife Services. Terry will now give you some of his perspective on Dave Ankney's important contributions:

Terry Quinney:

I could write a monograph on Dave's contributions to Fish and Wildlife Conservation in Ontario, and to the O.F.A.H., but instead let me share just a few examples. Dave once told me that his mother wanted him to be a mathematician. "I wanted to hunt animals all of the time, but because I couldn't, I'd study them when I wasn't hunting them," he said. Mathematics' loss is our collective gain.

Over the course of Dave's professional career, the management of Ontario's wildlife resources has benefited greatly from Dave's expertise. Dave has been a long-time technical expert on the province of Ontario's Waterfowl Advisory Committee, the province's Ontario Wild Turkey Working Group and until recently, the Ministry of Natural Resources' Provincial Big Game Management Advisory Committee. Each of these committees plays important roles in the management and regulation of Ontario's wildlife. For example, the Waterfowl Advisory Committee makes recommendations to the Federal government on Migratory Bird Hunting Regulations. Dave has served as an expert witness on conservation and wildlife management in the Divisional Court of Ontario and the Supreme Court of Canada.

Under Dave's tutelage, many of his graduate students have distinguished themselves in their own right in various areas of research. Many of these graduate students have been recipients of the annual O.F.A.H. Wildlife Research Grant, including Kevin Dufour, Todd Merendino, Michael Hill, Erik Osnas, Jeff Gleason, Shannon Badzinski, and Michael Schummer.

In his first term as President of the O.F.A.H. in the summer of 1991, Dave met privately with the then Minister of Natural Resources, Bud Wildman. Dave's meeting with Bud Wildman generated a flurry of letters (or should I say snow storm) between the two. Dave began the jousting by telling Wildman that his N.D.P. governments' new native hunting and fishing policies were racist and discriminatory. "Imagine your outrage, Minister" wrote Dave, "if another Government and another Court said that non-natives could hunt and fish however they chose, but that natives were subject to conservation laws...the only answer that can ever work for long is equal rights and opportunities for all regardless of race." Unexpectedly, Minister Wildman promptly wrote back to Dave: "You pose a solution to these issues," said Wildman, "equal rights and opportunities for all, regardless of race. Equal rights do not always mean the same rights. For example, if all persons are to have an equal right of access to a given building, some may enter by stairs and others may enter by ramp."

Dave's reply to Wildman was written in what I'll term classic "Anknoidian"[a] fashion; blunt *and* precise[b], "Your analogy *re* my point about equal rights and opportunities for all regardless of

[a] *Apologies to Pat Kehoe for my plagiarizing a word he coined.*

[b] *First, Dave stuns his adversaries with a heavy mallet, then he uses a scalpel for dissection.*

race, is false. If all people have equal access to a building, some by ramp and others by stairs, they have equal rights to enter the building. The *Sparrow* decision (of the Supreme Court) and your expansion of it does not give equal access to the 'building' (fish and wildlife). Rather, your policy says that one race can enter the building first, and if there's still some room left, then other races can enter."

In the months following, President Dave and the O.F.A.H. were publicly vilified by a few people and groups as racist, bigoted, red-neck, anti-Indian. For those of us in positions of responsibility in the O.F.A.H. organization, it was a period of controversy and great pressure. That autumn, the fall of 1991, I made my annual mecca to the marshes of Long Point for a diver hunt with Dave. My recollection is that we weren't able to obtain an outer blind in the (Waterfowl) unit, so we hunted puddle ducks instead. We were about to quit and pick up decoys; Dave hesitantly began to tell me something, and he seemed to be in anguish. I had never seen Dave so upset. Well, my mind began racing and involuntarily played tricks on me as I tried to predict the nature of the catastrophe he was about to explain. I jumped to the conclusion that our illustrious President was about to confess a terrible crime of passion that would land him in jail to the complete and utter disgrace of the O.F.A.H. What Dave told me was that he had decided to proceed with the publication of his recently accepted manuscript titled, "Sex Differences in Relative Brain Size..." but only after he had exhaustively searched his soul and conscience, because of the potential implications for the O.F.A.H. if "sexist" was added to the list of defamatory adjectives the mudslingers were hurling at the time. Dave's love of the O.F.A.H. and his commitment to the conservation movement is so great that he was fearful that misrepresentations of his discovery would harm the reputation and work of the O.F.A.H. He cared so much about the O.F.A.H. that he had considered asking the journal editor to withdraw or postpone its publication. Dave said to me that despite the potential character assassination which might befall him (and by association the O.F.A.H.) that he simply *had* to publish the paper, and my memory is that Dave spoke Shakespeare's immortal words (from Polonius' speech to his son in Hamlet): "This above all: to thine own self be true."

Dave has a voracious and insatiable appetite for new knowledge (as well as the flesh [served rare!] of wild game). I believe that's why Dave told me he *had* to publish his article, and I believe he *is* true to himself by relentlessly pursuing and discovering knowledge. And speaking of relentless pursuits, have you ever met a man that enjoys a good argument better than Dave Ankney, because I have not. But fundamentally, argument can be an effective method to test one's knowledge and reasoning, and obtain new knowledge.

As a member of the O.F.A.H. spring bear hunt court case team, Dave has volunteered *thousands* of hours over the last 3-½ years, and he played an important role in assisting the O.F.A.H. to articulate the animal rights legal precedent at issue. Here's a brief excerpt from one of Dave's affidavits before the Courts: The Ontario government's sole stated interest in canceling the spring bear hunt was to ensure that not one bear cub could become an orphan as a result of hunting in the spring open season despite the undisputable biological facts that orphaning occurs in many, if not all, animal species and populations at that time of year, whether they are hunted or not, and the mistaken shooting of sows in the spring open season (already illegal) had no negative impact on bear populations. By ensuring that not one bear cub could become an orphan as a result of hunting in the spring open season, the government has thereby elevated the right of

this one hypothetical bear cub not to be orphaned over that of all Ontarians to hunt bears in the spring, or for any Ontario citizen to derive an income from the spring bear hunt. We emphasize one bear cub because the government's stated goal was not to reduce or minimize orphaning, but to guarantee that no bear cub would be orphaned due to hunting in the spring. It is unprecedented in the history of hunting regulations in Ontario for animal rights to be even a minor criterion for enacting such regulations. Until now, conservation and human safety have been the only criteria used to regulate hunting.

In his testimony, Dave has informed the Court that virtually all human activities necessarily result in directly or indirectly causing pain and suffering (including orphaning) to animals. He pointed out the most obvious activity is driving automobiles, much of which is done simply for pleasure or convenience. It has been estimated that in North America, one million vertebrate animals per day are struck by automobiles. Thus, there are millions of such occurrences every year in Ontario. Many animal/vehicle collisions result in crippled animals that die slow, painful deaths. Many also result in the orphaning of young animals, including bear cubs. The government has not passed any regulation making it illegal to hit an animal with a vehicle, nor has it banned driving because some drivers *by mistake*, hit animals and thereby cause pain and suffering and orphaning of nursing young. Further, the government has not banned driving for pleasure or convenience, but it has banned hunting bears in the spring for food, although both can result in orphaning of young animals. The government has arbitrarily decided to guarantee a bear cub's right not to be orphaned by a bear hunter hunting for food, but not by someone driving a vehicle simply for pleasure or convenience.

Dave has also pointed out to the Court that many bears, including nursing females, are being legally killed in the spring because they are deemed to be a nuisance or threat to property. It is perfectly legal to shoot a nursing female bear that knocks over your garbage can, your barbeque, or even just crosses your backyard, if that bear is perceived to be a threat to property, *even* if that perception is mistaken. In other words, it is legal to shoot a nuisance sow, and thereby orphan bear cubs, because you *think* it *might* be a threat to your property, but not to shoot a non-nursing bear in the spring for food. Such a distinction, Dave succinctly states, is both arbitrary and illogical.

There are many reasons why Dave Ankney has made major contributions to the O.F.A.H. and conservation in general. His keen intellect and the distinguished body of scientific knowledge he has developed come immediately to mind, but so does the fact that Dave has never forgotten "to smell the roses." Dave *lives* to hunt and fish. In addition to his love of learning and knowledge; in addition to his love of Sandi; in addition to his love of his many hunting dogs like Spook and Drake and Bo and Chen and Bay and Crik; in addition to his love for conservation and the O.F.A.H.; in addition to all of these–Dave loves to hunt and fish (and he loves to eat what he hunts and fishes).

Most thinking people harbour a strong desire (kept secret by most) to make a difference and leave the world a little better than before. Dave, I can say with certainty that conservationists across North America would agree that you have made a difference.

In closing, we jointly want to say that Dave Ankney has been an inspiration to others. He is clearly analytical, clearly an advocate, clearly a person who has enriched the world of natural resources both directly and indirectly. His legacy will continue for many years and we will all be better for it.

On behalf of all of his fellow volunteers in the worlds of conservation, hunting and fishing advocacy, we extend our appreciation, our gratitude, our respect, and our best wishes to Dave for a bright and healthy future. We say thanks Dave, and also thanks to your Sandi for being so supportive over the years and allowing Dave the true freedom and energy to do what he has done so well. It has been a pleasure, Dave, to be your colleagues. It will be an even greater pleasure to remain your friends.

And Dave, as you enter this new era of your life, all of us with the Ontario Federation of Anglers and Hunters want you and Sandi to find "Happy Hunting Grounds" every day you're in the field or on the water.

Graduate Students of Dave Ankney (in alphabetic order)

DM Abraham
RT Alisauskas
TW Arnold
SS Badzinski
GAC Bain
BA Beasley
LA Boon
CH Bonta
RW Cole
JJ Dixon
KW Dufour
S Edmunds
MRL Forbes
SG Gilliland
RI Goudie
J Gleason
DJ Hamilton
AR Hanson
MRJ Hill
DJ Hoysak
JO Leafloor
JP Lightbody
DG Krementz
TD Nudds
MT Merendino
EE Osnas
KA Patton
GA Paquette
TE Quinney
DW Smith
PD Tebble
JE Thompson
STA Timmermans
JE Weaver
RCP Wypkema
AD Young
R Zimmerling
PEF Gregoire