COOPERATION AND CONFLICT IN ENVIRONMENTAL STATISTICS

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Statistics often plays a major role when an environmental problem is being discussed. Typically, managers would like to take actions that are expected to reduce the problem, but the available data are not sufficient to show clearly what those actions should be. In addition, there are various interested parties that have good reasons for favouring particular management actions, or no actions at all. It helps if a statistician is involved in situations like this, providing that they are open minded about the conclusions that can be drawn from the data. The input that they can provide includes suggestions for improved methods for sampling and analysis, and acting as a referee when it comes to assessing alternative conclusions from the available data. In this talk I will discuss the role of a statistical consultant working in the environmental area, based on my experiences over the last 20 years.

INTRODUCTION

I first became interested in applications of statistics in biology in general, and ecology in particular, as a young Assistant Lecturer in the Department of Mathematics at the University of Salford, UK, in the late 1960s. This came about through a chance meeting in the staff room with Mike Parr from the Department of Biology. He was collecting mark-recapture data on dragonflies as part of the PhD that he was working on, and he had a question about using his data to estimate relative survival rates of dragonflies of different types.

I had never heard of mark-recapture data. Indeed, the truth is that I did not know much about anything in statistics apart from some standard test, analysis of variance and regression. I asked Mike to explain how mark-recapture data are collected and what the data were used for. He explained how it is possible to estimate the size of a population when it is sampled, survival rates between two sample times, and the number of new dragonflies joining the population between two sample times. He was using several old methods of analysis, including one developed by Ronald Fisher (Fisher and Ford, 1947), and a very new Jolly-Seber method (Jolly, 1965; Seber, 1965).

Mike had his data sheets on his desk to show me how he recorded his results, and I asked him why people did not just estimate the probability of capture in a sample by the fraction of dragonflies captured out of those that were known to be alive because they were seen both in earlier and later samples. Having estimated this probability, the number of animals alive at the sample time could then be estimated by the total number of dragonflies collected divided by the probability of capture. Mike said that he did not know why people did not do that, and we passed on to discussing his survival rate problem. This was interesting so I said that I would think about it and we could meet to discuss the problem again in a few days.

When we next met, Mike was very excited. He said that the method of estimating population size that I had suggested had never been published, although it was much simpler than the alternatives. He said that we needed to write a paper about it immediately and get it published before anyone else noticed the method. So we wrote a paper (Manly and Parr, 1967), and the Manly-Parr method for estimating population size later became well-known. This is an example of something that I suspect often occurs. If I had been taught about the standard methods to analyse mark-recapture data at university then it would not have occurred to me that there might be some simpler method available that no one had noticed. Sometimes ignorance is useful.

Before meeting Mike Parr I had been interested in the statistics of quality control, in particular in the method of sequential sampling (e.g., taking observations one at a time until it was possible to reach a decision about the mean of a population). However, Mike introduced me to his friends and it soon became abundantly obvious that my future as a statistician would be much brighter if I worked with biological applications instead of quality control. For one thing, there were plenty of interesting problems that were not difficult to solve and really needed solutions. As an example, no one at that time had ever simulated the properties of estimators of mark-recapture data.

For me that was an easy thing to do and it was also easy to get a long paper published with my results in a good ecology journal (Manly, 1970), plus a follow up paper in a statistics journal (Manly, 1971).

I continued my research and consulting in ecology in a similar way for many years, leading to a combination of joint papers with biologists and papers on my own concerned with more technical aspects of the statistics. In general the studies that I was involved with were not controversial. However, that started to change about 20 years ago. In the remainder of this paper I will discuss my experiences as a statistical consultant in areas that vary from being mildly to extremely controversial.

FISHERIES BYCATCH OF MARINE MAMMALS AND BIRDS

I taught at the University of Otago in New Zealand from 1973 to 2000. Through my work with biologists I knew many people in the government Department of Conservation, and one day in the mid 1990s I got a telephone call from the head office in Wellington. I did not know the caller, but he said that the Department needed someone to analyse data on fisheries bycatch of marine mammals and birds. They needed someone that would not be considered biased by the fishing industry or environmental groups, and my name had been suggested. I was asked whether I was interested. I knew nothing much about fisheries or bycatch, but I said that I was interested.

Well, it turned out that it is usual for all types of fisheries to occasionally catch birds or marine mammals and, not surprisingly, this causes concern if the bycatch rate is anything other than minor. In New Zealand the main concern was bycatch of the New Zealand sea lion, which only breeds on islands off the coast of New Zealand and is considered threatened because of the small population (about 12,000 animals) and limited breeding sites. Catches of the sea lion occur during squid fishing close to the breeding sites. For this reason government observers are put on some of the fishing vessels to record any bycatch. The sampled bycatch is then used to estimate the total bycatch from the whole fishing fleet, and if the estimate exceeds a critical number then the fishery is closed.

I went to many meetings that were concerned with the New Zealand sea lion and other bycatch. I discovered that the staff from the Ministry of Fisheries and the Department of Conservation were suspicious of each other and sometimes seemed to disagree about policy. Staff from both of the government departments were suspicious of the fishing industry representatives, and the fishing industry representatives were suspicious of everyone. However, I got on well with the fishing industry statistician, I suppose because we had a similar background.

With the discussions about the New Zealand sea lion statistics was usually important. I remember one particular meeting where everything revolved around this. The Ministry of Fisheries was planning to close the squid fishery based on a confidence interval that indicated that the total allowed bycatch of sea lions might have been exceeded. The data were binomial and it was a small sample. I had already told the Department of Conservation staff that an exact binomial confidence interval did not suggest that the critical number had been exceeded. So I went to the meeting with two people from the Department of Conservation. Before we went in we agreed not to say anything. The Department of Conservation people thought that the Ministry of Fisheries was about to get hammered by the fishing industry. That is precisely what happened. The Ministry of Fisheries representatives did not know about exact binomial confidence intervals. They did not have a statistician involved and they got extremely embarrassed during the meeting. The squid fishery was not closed down. When there is a lot at stake you need to really make sure that your statistical methods are correct.

One interesting thing that I learned from these experiences in New Zealand was that sometimes interested parties do not want to end the general confusion surrounding environmental issues. I first realized this when two colleagues and I proposed a method for determining the fraction of squid fishing vessels to be sampled based on the probability of closing the fishery by mistake and the probability of not closing the fishery when the critical number of sea lions have been caught (Manly *et al*., 1998). It seemed to us that the fishing industry and the government departments could negotiate reasonable levels for these probabilities, taking into account the cost of sampling (which was paid by the fishing industry). It turned out that the fishing industry was not interested in such a plan, I suspect because they preferred the level of sampling to be determined is some ad-hoc way. Then there was the possibility of using the inadequate level of sampling as an argument in a court case to try to stop the closure of the fishery. In other words, it was potentially useful to have government departments that looked a bit incompetent.

Since I left New Zealand in 2000 I have worked on one bycatch project there with a colleague. This was to review all the fisheries in the country and advise on which of these should be considered for observer programmes. I attended one meeting involving all interested parties and it seemed to me that the fishing industry's strategy was to do everything possible to make sure that the observer programmes are not successful. There does not seem to be much cooperation there.

More recently I have been involved with the planning and analysis of data from the Alaska Marine Mammal Observer Program (AMMOP). This is concerned with the salmon driftnet (i.e., a net towed by a vessel) and setnet (i.e., a net attached to the shore) fisheries in the state. In this case the bycatch problem does not seem to be very serious and, with one exception so far, the fishers seem to accept the need for the observer programme so that bycatch can be shown to not be an issue.

Here there is the possibility of using a method along the lines proposed by Manly *et al.* (1998) to set the level of observer coverage for fisheries. In discussions last year with the person in charge of AMMOP we reached the conclusion that the present method of determining observer coverage levels (something like fixing the sample size in order to get suitable coefficients of variation for estimated bycatch numbers) is not very useful. What actually happens is that estimated bycatch numbers are treated as being the true values and then various decisions are made about the future treatment of a fishery based on these values. So what we are going to investigate is the idea of fixing of sample sizes based on the probabilities of making correct decisions about a fishery. This should be much more useful to decision makers, although it may well show that based on past observer coverage levels the probability of wrong decisions has been high.

MONITORING ANTARCTIC WILDLIFE

Partly as a result of my work for the New Zealand Department of Conservation, I was asked if I was interested in being a representative for New Zealand on the Commission for the Conservation of Antarctic Marine Living Resources (CCAMLR). CCAMLR is an international organization involving many countries around the world, but with a limited budget for research. It therefore relies mainly on individual governments supplying scientists to do whatever work is required. I was asked to join the Statistics Subcommittee of another committee on environmental monitoring.

When I attended my first Statistics Subcommittee meeting I was interested to see that although there were about ten people present only the UK and New Zealand had thought that it would be useful to have a statistician on a Statistics Subcommittee. I thought that the composition of the committee said a lot about the extent to which statisticians in general are thought of as being useful, and I wondered what people would think of a Biology Subcommittee with eight statisticians and two biologists.

Needless to say, the work of the subcommittee was all about statistics. It seemed that there may have been no statisticians on the subcommittee in the past because it soon became apparent that some of the statistics being done was wrong. For example, the method for detecting anomalous years had confused the standard error of the mean with the standard deviation of individual observations. I found it rather surprising that an international organization was publishing its annual report with some simple mistakes in the statistics, but it just highlights the need for statisticians to get involved with organizations like this, and realize the extremely important contributions that they can make, even if it is just getting the numbers right.

BOWHEAD WHALES OFF THE COAST OF ALASKA

The example of bowhead whales off the coast of Alaska is interesting in showing the complexity of some environmental issues. Briefly, in the autumn bowhead whales migrate west from the north coast of Canada, through the Chukchi Sea and Bering Strait, to the Bering Sea. The whales are still hunted by native Alaskans, who have complained that the noise of oil drilling operations in the Beafuufort Sea off the north coast of Alaska causes the whales to move further offshore, which makes them harder to hunt.

Presumably as a result of the complaints by the native Alaskans, numerous studies have been funded by the U.S. government's Minerals Management Service to investigate the effect of seismic activity on the whales. These have involved a good deal of statistical analysis. For example, one study used data from all of the aerial surveys of whales carried out from 1996 to 1998 to see if it is possible to detect a tendency for whales to move away from sources of sound (Manly *et al*., 2004).

This study may eventually lead to an analysis of all the available data from aerial surveys, although at present the amount of data involved may make this difficult. Using three years of data meant that there were over 30,000 observations available to fit a logistic model for the presence and absence of whales. Some statistical packages such as *SAS* said that estimation was not possible, while others such as *GenStat* only gave the correct answer if iterations began with a completely null model. It appears, therefore, that the standard statistical packages may not be able to fit a model to ten or more years of data.

Putting this purely statistical problem aside, it is not difficult to imagine that the U.S. government would prefer to continue paying for research on the effects of seismic noise rather than reach any definite conclusions about what those effects are. If the conclusions are that the effects are substantial then it seems very unlikely that oil drilling will stop while oil is still available, plus many government officials are probably secretly pleased that the drilling noise makes it harder to hunt whales. On the other hand, the native Alaskans are likely convinced that the drilling noise has an effect and would be most unhappy to be told that is not the case.

I think it is not that uncommon for a government department and large companies to prefer not to have to make a decision on an environmental issue. A good strategy in that case is to keep saying that more research is needed, even if that research has to be funded. It is always, of course, possible to say that budget constraints mean that the research cannot be done immediately.

WATER EXPORTS IN THE SACRAMENTO-SAN JOAQUIN DELTA, CALIFORNIA

My most recent example of conflict and cooperation with environmental problems concerns the decline in numbers of several species of fish in the Sacramento-San Joaquin Delta of California, and, in particular, the decline of the native delta smelt, which has been declared a threatened species under the U.S. Endangered Species Act. A particular question here is whether the decline in fish numbers is caused to any appreciable extent by the export of water from the Delta for agricultural and town use.

There are an unusual number of stakeholders (interested parties) involved in discussions surrounding the decline in fish numbers and the effect, if any, of water exports. On the one hand there are some private environmental organizations who say on their websites that exports are destroying the delta smelt and other fish populations, while on the other hand some water users believe that the effects of exports are minimal. Representatives from many federal and Californian government departments are involved in the issue, including the U.S. Fish and Wildlife Service, the U.S. Bureau of Reclamation, and the California Department of Water Resources. The opinion of these individuals apparently varies from a conviction that exports are the culprit to a belief that this is unlikely.

I have been involved in various issues related to the Sacramento-San Joaquin Delta since 2000, including the analysis of mark-recapture experiments on salmon smelt to estimate survival rates, population modelling of delta smelt, and reviews of numerous reports. This work has been paid for by one of the largest water boards in the Delta, which basically has the job of providing water to users in their region. I believe that they pay for a statistician because they want to make sure that one is involved with the data analysis.

The decline in delta smelt and other fish seems to have begun in the late 1990s, although it was not until the start of 2005 that it was officially realized that there was a problem and a Pelagic Organism Decline (POD) Committee was set up, comprising representatives of many federal and state government departments and university researchers. This committee involves a number of working groups investigating such things as the effects of pollutants, zooplankton community changes, and the introduction of exotic species. An obvious need was for many statistical analyses, so I was asked to help in that respect. Partly this was because they wanted a professional statistician from outside without any constraints in terms of conclusions that might be reached from analyses.

It was originally thought by the POD Committee that the fish declines began after 2001. However, my analyses pointed to abrupt changes in fish numbers being fairly common in the past, with some tendency for these to occur at about the same time for different species, and the late 1990s being the last of these times. These analyses so far have mainly involved log-linear models, and have required the development of new methods of analysis using bootstrapping to assess the significance of results because the usual assumptions of log-linear modelling do not hold. These methods have attracted attention because shortly after presenting them for the first time at a POD conference in December 2005 I got an e-mail asking me to talk about them at a session at the next Ecological Society of America meeting entitled *Large Scale Studies: Challenges in Experimental Design and Analysis*. As far as I can tell, I am the only statistician speaking in the session, which I think is unfortunate.

My log-linear model analyses have taken into account possible effects of overall water flows and exports, but so far these variables do not account for the major changes in fish numbers. Instead, it appears that water flow and exports only have minor effects on the abundance of fish, and that, if anything, moderate flow levels are best for abundance. This still needs more analysis, but it suggests that when water flows are low exports may have a negative effect, while if water flows are high exports may have a positive effect. Nevertheless, the major changes in abundance are still largely unexplained. This is a disappointment for many people because they would like to be able to point to a simple explanation for the decline in fish numbers in the Delta.

The December 2005 POD conference was to assess progress in understanding the causes for fish declines in the delta. Interestingly, most of the state and federal government scientists did not want to say much because they are uncertain about what is happening and probably think it is best to say nothing. As a result, representatives of water boards and researchers from private research laboratories, were allowed to present papers. The key topic was the effect of exports on delta smelt numbers, and the conclusions reached by different statistical analyses were apparently quite opposite.

When the managers from the state and federal government departments saw this, they demanded to know which conclusions are correct, but nobody knew for sure. So at that point I was asked to come in again as the outside statistician. I was asked to look at all of the analyses, checking the data, all details of the calculations, and the validity of the conclusions. Then I was to report my conclusions. The process started with an afternoon meeting of all of the people who had made analyses, to give them an opportunity to reply to questions about their assumptions and analyses. Then I was to write my report and present it to the group at another meeting. Finally, the report (possibly modified based on the last meeting) would go to the decision makers.

This process is still continuing as I write. It has taken longer than I expected because of a surprisingly large number of errors and inconsistencies in the data that people were using. In fact so far the errors do not change the results much, but they still need to be found. When I agreed to review all of the analyses I said that I thought the final conclusion would be that the effects of exports are still not known. So far that is still the case, although I still have other analyses to examine.

There are several reasons that it is so hard to determine the effects of exports. One is that exports are fairly tightly controlled and no one seems willing to try real experiments with very high and very low export levels at different levels of flow. Another reason is that no one is sure how best to measure export and flow levels to detect export effects, and that, of course, the effects are likely to be non-linear and vary with the amount of water in the system. Finally, when significant relationships are found they are sometimes very strange and no one thinks they make sense. For example, I recently found a regression equation that accounts for 80% of the variance in the estimated survival of juvenile salmon from mark-recapture experiments, which is very highly significant ($p = 0.0002$). The equation has two explanatory variables in it. The first is the average temperature from 0 to 18 days after release, while the second is the difference between the exports 5-9 days after release and exports 0-4 days after release.

Most of the scientists do not know what to make of this equation, which fits much better than anything else. The temperature effect with a negative coefficient is plausible because salmon like cold water, but no one can understand why the a change in exports might effect the survival of salmon. I do not know what to make of the equation, but it seems to me possible that changing export levels quickly may effect the water flows throughout the Delta, leading to some sort of indirect effects on salmon survival.

The Sacramento-San Joaquin Delta project has proved to be an interesting one. My role has been to analyse data, and act as a referee to check the details of others analyses. In these types of role it is vital to stay open-minded and unbiased until definite evidence of effects is found. It is also, in my opinion, most important to be able to communicate well the statistical methods used, why they were used, the assumptions made, and the strength of the inference that can be drawn from the results (if any). Also, complicated ‘black box’ analyses are not likely to be convincing, unless a simple method such as a graph can be used to show that the results seem to make sense.

CONCLUSION

The first point that I would like to make in closing is the advantages that statisticians gain from working with biologists and environmental scientists. Anyone who tries this will find that there are abundant interesting statistical problems to be solved once you understand what studies are really about. I learned this at an early age through my work with Mike Parr and his colleagues at the University of Salford. I stress though, that it is important to be an active participant rather than a passive one. You have to read the same papers that the biologists and environmental scientists read because that is where a lot of interesting statistical problems are hidden.

My second point is that the biologists and environmental scientists seriously need the help of statisticians, even though sometimes they do not realize this. Often they will have taken several postgraduate courses in statistics many years ago, and think they are very competent in the subject. However, they may not be at all familiar with some of the more modern methods of analysis and not even know they exist because generally biologists and environmental scientists do not read statistics journals.

My third point is that if a statistician is involved in controversial areas then it is crucial to stay unbiased, and for people to believe that this is the case. A biased statistician is no use to anyone, and there are usually plenty of biased people already involved in controversial issues. As a guide, if when you present your results none of the stakeholders is completely happy, then you probably got it about right.

My final point is that with controversial issues it often seems to happen that some of those involved do not want the arguments to end. They fear that the truth may not favour their point of view, in which case it is best not to know the truth. Actually, they are aided by the fact that controversial issues tend to be very complicated. Simple experiments to determine the effects of different factors cannot be done for various reasons, and causation needs to be guessed from associations. Therefore, determining ‘the truth’ for sure may not be possible.

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