Pat and Quinn Whiting-OKeefe
Carey Road, Port Charles
Coromandel, NZ
Email: pwok@alumni.caltech.edu
Phone 07 866 6633
Fax 07 866 6603

Aerial Monofluoroacetate in New Zealand's Forests

An appraisal of the scientific evidence

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Introduction

Annually, the New Zealand Department of Conservation (DoC) and the New Zealand Animal Health Board (AHB) drop from the air food laced with enough of an "extremely hazardous" (17) poison (sodium monofluoroacetate, also called compound 1080) into New Zealand's unique forest ecosystems to kill every person in New Zealand 8 times over. DoC rationalizes its policy by saying that it is necessary to control feral "pests". They claim that it benefits native species and forests and does not do significant harm. AHB believes that it is necessary to control bovine tuberculosis (TB). DoC and AHB have jointly applied (1) to ERMA to continue and extend authorization for this practice. The purpose of this document is to examine the scientific evidence supporting the contention that aerial monofluoroacetate (aerial 1080) is benign and beneficial to our forest ecosystems and the contention that it is essential to the control of bovine TB. Since these objectives and the evidence needed to support them are quite different, they will be dealt with separately.

Issues not addressed

One-shot use of aerial 1080 on true islands. Aerial 1080 has been used on true islands to eradicate feral mammals. The important feature of this is that it usually requires only one poisoning, or at most two. This is environmentally profoundly different from repeated applications every 2 or 3 years of 1080 into non-island native forests.

Other uses of 1080. Monofluoroacetate itself is not the issue that we have investigated. It is rather the aerial application of food laced with 1080 into our forest ecosystems that is the subject of this paper. We suspect that any other broad spectrum poison would have similar effects, e.g., cyanide. The use of such poisons in traps that limit access to all but targeted species may be necessary and even desirable, but in any case is not the subject of this investigation.

Risks to humans. We have not attempted to assess scientifically the risk of aerial 1080 to humans. Compound 1080 is a highly toxic chemical that will certainly kill humans if they are exposed to even minute amounts, but this is true of many substances. It is fairly clear from the literature the aerial 1080 in the concentrations in which it is usually applied does not constitute a major risk to humans from water contamination, providing it is used and applied as it is supposed to be. It would require eating the poison bait directly, eating a poisoned animal or an accident in a water catchment to achieve that level of toxicity. So the acute risk to humans comes down to that from accidents, errors and malice[*].

Over the last two or three decades, there have been numerous reports of accidents and near accidents, of accidental animal poisoning and the like. As the use of 1080 becomes more widespread, its handling would be expected to become increasingly "routine", which means it is probably just a matter of time until something really serious happens. The particularly grave possibility is that of a child walking into a recently poisoned forest and eating some bait. Because DoC frequently drops aerial 1080 into forests that are near human habitation and that are commonly accessed by humans, this risk would appear to be substantial; indeed at least one child was almost killed (2)[†].

However, what is **not** known is the effect of sublethal and chronic poisoning. Since humans cannot be experimented upon, there are two potential avenues of approach regarding the risk to humans. First are animal experiments. The more similar the experimental animal is to humans, the more compelling. In this case, it may not need to be that close since the

[*] As a weapon, 1080 would certainly qualify as one of mass destruction. A few kilograms put into at city's water supply in the right place could result in the death of hundreds or thousands of persons.

[†] Personally, as a physician, I would not wish to be the one who signed off on this practice.

mechanism of poisoning by 1080 is common to virtually all air breathing organisms. There are very few studies in which chronic and sublethal effects have been examined and they tend to be limited in scope and short term. What research has been done indicates that 1080 in sublethal doses can cause infertility, hormonal dysfunction, and mutations in several vertebrate species (8). The second approach is to examine theoretical arguments based on the modes of the poison's action, the organs most affected and biological mechanisms of cellular disruption. Peter Scanlon's submission to ERMA (3) is the best review covering these issues of which we are aware.

Risks to domestic animals. We do not assess the risk to domestic animals in this document. It may be substantial, but we have not looked into the issue.

Limitations of this paper

We have not attempted to be exhaustive in our coverage of the research literature. We have selected papers which seemed to have the best methodology, that were frequently referenced by other authors or the DoC/AHB submission, or that contain important results. We are confident that we have not missed major studies on the central issues, but we have not reviewed every scientific paper on the subject that has anything to with aerial 1080, nor do we think it would be useful to do so.

There were one or two apparently minor papers copies of which we were unable to obtain. There was one large retrospective management report (85) a copy of which we have not yet received from DoC despite a request and verbal assurance that it would be sent.

We would have liked to reanalyze the data from several critical studies, and have requested copies of the data. In the case of the Spurr invertebrate study (63), reanalysis may have been particularly helpful in reconciling the author's results with a previous study. However, we are informed by the Director of Landcare Research that Dr. Spurr was unable to locate the data and he would not be able "to search" for it until 30 January when this report is due.

The material covered in this document is vast in scope and it has been necessary to do our research in a relatively short time frame. Thus, it is possible, even likely, that we have made some errors of detail. For this we apologize if it turns out to be true. However, we are convinced that the bulk of the evidence is as we have represented it, and thus that the conclusions are substantially as we have stated.

Science, politics and the nature of this document

At the outset it was our intention to confine ourselves to the scientific evidence supporting the use of aerial 1080. However, it quickly became apparent that, although the scientific evidence is far from adequate to justify the extraordinary national policy of indiscriminately spreading poisoned food throughout whole forest ecosystems, the scientific evidence is not the whole story.

We will show that the manner in which DoC has been interpreting the scientific evidence is as much a problem as is the evidence itself[*]. There is a pattern of misrepresentation, omission, and distortion in DoC's writings and pronouncements so obvious and so flagrant that the scientific evidence could not be explained without documenting this aspect as well[†]. It is further clear that DoC-sponsored scientists are under considerable pressure from DoC

[*] Relative to DoC, we have found much less evidence of misinterpretation on the part of AHB.

[†] The nearly universal presence of misrepresentation by omission is exemplified by a quotation of the purpose for the application from ERMA's web site: "… the reassessment of 1080 and substance containing 1080 (a vertebrate toxin) …". In fact, compound 1080 is toxic to all organisms that burn carbohydrates to produce energy, i.e., all animals, not just vertebrates.

management to consistently support the agency's statements regarding the real effects of 1080. Consequently, we have in several places documented and explained the political context in which scientific investigation s have taken place.

However, we have attempted to make it clear from context when we are presenting a scienti fic, technical judgment as opposed to explaining the political, bureaucratic, and human context.

In writing this document, w e have been candid, sometimes brutally so. We have not attempted to euphemize in order to protect the sensibilities DoC/Landcare Research researchers or to spare DoC management from embarrassment. We feel that when something as important as New Zealand's rich native forests and national reputation as an environmentally conscientious nation are at stake, it is too crucial a juncture f or equivocation or pusillanimity. Rather, we believe that it will require the full force of plain and direct language to effect a change. Nonetheless, it is not our intention to offend gratuitously and we apologize to the extent that we may appear to have done so.

Monofluoroacetate facts

Monofluoroacetate (1080) was originally developed and marketed as an insecticide (70).

It functions primarily by interfering with the citrate step in the Krebs cycle (27). The Krebs cycle is the major and an essential mechanism by which all air breathing creature s utilize food to produce energy. This means that it is toxic to all animals, essentially everything living except perhaps plants and some micro -organisms.

Of course some species are more susceptible on a weight basis (**Table 1**). Remarkably, given that New Zealand uses 80 to 90% of the world's production in our forests (4,5), the susceptibility of most of New Zealand's native species have not been studied, as DoC unashamedly admits (6).

Table 1 Relative Toxicity of Monofluoroacetate

Species	LD50 (mg/kg body weight to kill 50% of a population	Relative Tolerance (LD50s for Species/LD50 for possums)
Dog	0.06	0.08
Pademelon	0.13	0.16
Bennett's Wallaby <0.2	0.20	0.25
Cat	0.40	0.50
Rabbit	0.40	0.50
Cattle, sheep, deer 0.2-0.6	0.40	0.50
Red-browed firetail	0.60	0.75
Possum	0.80	1.00
2 AU bird species 0.6-0.99	0.80	1.00
Rat	1.00	1.25
Wombat	1.50	1.88
Man	2.00	2.50
Finches	2.70	3.38
House sparrow	3.00	3.75
Chukar	3.50	4.38
Golden Eagle	3.50	4.38
Sulphur-crested cockatoo	3.50	4.38
Eastern quoll	3.70	4.63
Parrots 8 species	4.00	5.00
Tasmanian devil	4.20	5.25
California quail	4.60	5.75
27 AU bird species 1.0-9.9	5.50	6.88
AU Insectivorous birds 3.4-18	7.30	9.13
Mallard	9.10	11.38
Birds 3-19	11.00	13.75
Mouse	13.00	16.25
Great Horned Owl	20.00	25.00
11 AU bird species 20.0-49.9	35.00	43.75

Monofluoroacetate and cyanide

Monofluoroacetate is very similar to sodium and potassium cyanide in its profile as a poison. Both are universally lethal to animals. Both have no antidote. Both are rapid acting, though cyanide is more so. Time to death after monofluoroacetate poisoning is quite consistent among species (Atzert, 1971, 7). Both have low environmental persistence when wet, though monofluoroacetate is more persistent than cyanide. The exact degree of persistence of monofluoroacetate is a matter of dispute in New Zealand. It depends dramatically on circumstances and varies widely but an average is about 50% loss in 24 days in baits (55). Weaver (8) concludes that there is evidence that, since degradation rates vary dramatically with temperature, in some circumstances it may persist for a very long time. This has not been adequately investigated.

Secondary poisoning is possible, and perhaps even frequent, with monofluoroacetate, but essentially impossible with cyanide. Cyanide is cheaper than monofluoroacetate.

The risk to humans is substantial according to the WHO, which classifies both as "1A extremely hazardous" (9). In discussing their relative merits, DoC and AHB in their ERMA submission listed cyanide as having the disadvantage of "risk to humans if ingested", but surprisingly did not do so for monofluoroacetate despite the fact that as little as 30 mg can be fatal to humans (10). In a definitive review done independent of DoC, Eisler noted (55):

Compound 1080 is highly poisonous to all tested mammals and to humans. There is no known antidote to 1080, and it has been impossible to resuscitate any animal or human during the final stages of 1080 poisoning

? **Table 2. Monofluoroacetate versus Cyanide (27)**

Factor	Comparison
Range of action	Both poison all animals
Human risk	Similar
Antidote	Cyanide advantage
Cost	Similar
Environmental persistence	Cyanide less
Secondary poisoning risk	Cyanide: non-existent
Speed of action	Cyanide faster (10 minutes vs. 1 - 24 hours), which may lead to higher probability of bait aversion*

Given Table 2, one might wonder what is the big attraction to monofluoroacetate over cyanide. Although we have no direct evi dence, the answer seems to be in the politics. Because monofluoroacetate is relatively unknown, especially outside of New Zealand, it is politically acceptable to indiscriminately drop food laced with monofluoroacetate into forests, whereas doing the same with cyanide would generate both a national and international outcry that would bring the multimillion dollar practice of dropping tonnes of a universal poison into our forest ecosystems to an immediate halt. In view of this, it is instructive to note ho w DoC and AHB represent monofluoroacetate versus cyanide (1, 10). They make several insignificant distinctions: they describe both as having “low” environmental persistence, but then fail (as noted above) to mention the human risk for monofluoroacetate.

A brief tutorial on experimental design and statistical inference

DoC and AHB, mostly through Landcare Research, are essentially the only sources of scientific investigation on the question of the effect of aerial 1080 on ecosystems. This is because no other country in the world is doing anything remotely comparable. This means that one cannot challenge the validity of DoC -sponsored research with independent studies done domestically or abroad. There are none. Thus, we must evaluate the quality of DoC research. To do this it is necessary to use accepted standards for experimental design and statistical inference as a benchmark against which to judge the quality of DoC’s investigative work. This section reviews these principles.

Our intention is to provide a basic knowledge of the principles of experimental design and statistical inference for people who are not well versed in is such arcane matter, so that they can read and understand the inform ation presented in this document that presupposes an understanding and appreciation of those principles. We provide here a few references for the

* While this has been asserted by DoC and AHB as a major advantage of monofluoroaceta te over cyanide, we can find no published study that would give scientific credibility to that claim, particularly as regards 1080 administered aerially at infrequent intervals.

principles described below. These span a wide range of detail and sophistication (11, 12, 13), but there are literally thousands of books and textbooks on the subject.

Controls

Virtually all scientific hypotheses have embedded in them implied or explicit controls. If one says, "Our forests got worse." The immediate question arises of relative to what have they worsened: relative to Hawaii's forests, relative to what they would be if we did not saturate them with 1080 laced food, relative to what they would have been if the possum had never been introduced, relative to what they would have been had Europeans not been introduced, etc.? The statement, "Our forests got worse" is entirely meaningless without the relevant comparison. When formalized in experiments, the comparison entity (or entities) becomes the "control", giving us an anchor from which to judge observed change.

In many respects controls are the key to good scientific research in complex systems. The quality of the control (s) predetermines the quality of the scientific investigation , and to a substantial degree, the quality of the control group determines the validity of the results and the strength of the conclusion.

Controls can be categorized into a hierarchy, which we have named for subsequent discussion as numbered levels.

Control Level 0: No control group at all

This is the category into which fits DoC's statements on the overall effect of aerial 1080 on our forests, namely uncontrolled observation (often by biased individuals). For example, in DoC's premier brochure advocating aerial 1080 (14) we find this statement regarding "mainland islands":

> *"Using 1080 in these forests has been successful in helping restore birdsong that was diminished before 1080 was first used."*

Ignoring the fact that "mainland islands" are more comparable to real islands than the forests usually poisoned by DoC*, this assertion is based on nothing more than opinion, i.e., uncontrolled "observation". It is not based on science. It is an anecdote and as such is more likely to represent the prejudice of the writer than truth.

Control Level 1: Historical controls

In this case, the experimental group is compared to a previous state of the system under investigation. Many DoC studies fall into this class. Such controls have two major problems. First, historical circumstance is often not comparable to the current ones and, second, it is impossible to determine the cause of any observed difference (or lack of difference) between the control and experimental observations. In addition, historical controls are often accompanied by retrospective observations, which are notoriously unreliable. The literature is filled with examples of historically controlled research that turn out to be false when examined with simultaneous controls .

Control Level 2: Simultaneous controls

A few of DoC's studies have simultaneous controls. There are perhaps two dozen that bear directly on the question of the effect of aerial 1080 on our forests. The problem with

* … because repeated applications of 1080 are not usually necessary on islands .

simultaneous, but not randomized, controls is that one never knows whether the controls are really comparable to the experimental group. The chances that the controls are inherently different from the experimental group can be reduced by two techniques, and definitively eliminated by one*.

Control Level 3: Simultaneous, matched controls

One can carefully examine what are thought to be relevant factors to assure comparability and attempt to prove this comparability statistically. The problem here is that, one can never be sure that s/he has gotten all the relevant factors or that the factors examined are the correct ones. DoC-sponsored studies almost never do this kind of comparability checking. In fact, often (as will be seen) they simply ignore clear evidence of incomparability.

Control Level 4: Simultaneous, matched controls with diversity and multiplicity

One can have multiple and varied control and experimental areas that truly represent the range of conditions to which the study will be applied. None of the research that DoC cites to support its use of aerial 1080 reaches this level of control quality, and indeed, any study that did would have been most likely to go on to Level 5, randomized controls.

Control Level 5: Simultaneous, randomized controls

This is the highest standard of control quality. Really it should be Level 10 since none of the others approaches its ability to insure reliability of results. The concept of randomization in research design was developed by R A Fisher† in the 1920's to support agricultural and genetics research. Randomized design is now the gold standard for experimental research in complex systems, for example, in clinical medical research and in biological systems. Though little known to the general public, it is among the most important discoveries of all time. The reasons for its power are subtle and deep, and beyond the scope of this brief discussion. It will suffice here to describe its effect in experimental inference. It removes the influence of most forms of bias, it validates the assumptions underlying the statistical tests, and it is the only way to prove causation in multivariate systems with substantial variation among analyzed parts. In the particular case being addressed in this submission, the relevant causative relationship is that aerial 1080 causes benefit or harm to our forest ecosystems. In short, randomization is the only reliable path to the unvarnished truth.

We can find no DoC-sponsored study in which the selection of control and experimental units was randomized--none, let alone one that bears on the issue of the effect of poisoning our forests with aerial 1080. The existence of one such study addressing the relevant questions would trump all the other research, opinion, tradition and propaganda put together. Despite decades of dropping tonnes of 1080 into our forests and despite hundreds of millions of dollars having been spent, that one essential study has not been done.

Blinded observation

Blinded observation in study design is the use of observers and assessors of experimental results who are unaware of the control status of the observations they are making. It is vital

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* Strictly speaking randomization does not eliminate the possibility that control baseline characteristics account for an observed difference. Rather randomization allows a researcher accurately to calculate the probability of that possibility, and thus consciously to decide how big chance of coming to a false conclusion he is willing to accept.

† Sir Ronald Aylmer Fisher, (17 February 1890 – 29 July 1962) was a British statistician, evolutionary biologist, and geneticist. He was described by Anders Hald as "a genius who almost single-handedly created the foundations for modern statistical science" and Richard Dawkins described him as "the greatest of Darwin's successors", high, but highly disserved, praise.

when the variables being observed are subject to judgment, which is frequently true in biological field studies such as the ones we have reviewed in this paper. It prevents observer bias from influencing the outcome of a study. Most observer bias is not conscious or malicious. It is simply a function of being human. For example, people examining aerial photos of a forest to determine the degree of deforestation from possums is very subjective. None of DoC's studies that we have reviewed have blinded observers.

P-values

A P-value is the probability that a particular statistical result could have happened by chance. The lower the P-value the less likely that an observed difference (between treated and control area) was due to chance. By convention, scientific results are generally not considered to be "statistically significantly different" unless the P-value is less than 0.05 which mean there is a 5% percent chance that the observed difference was just an accident.

It is important to understand when reading scientific papers that the term "significant" usually means "statistically significant", and it bears no relationship to the concept of scientific significance. Thus, a difference might by statistically significantly different but not scientifically important, or it might be scientifically important, but not statistically different.

For example, a 1% drop in robin population numbers from aerial 1080 might be statistically significant (and thus real and reproducible) if the number of observations was great enough, but few would argue that it was ecologically or scientifically important. On the other, hand a 50% drop would certainly be ecologically important, but if the P -value were too large (>0.05) then it should be ignored, except, of course, as a guide to future research. This is not just mathematical sophistry. The consequence of disregarding these principles is that one will end up drawing a lot of false conclusions (and in the case at hand, might end up doing vast damage to our forest ecosystems).

As will be seen below, P-values were not calculated for many of the results on which DoC bases its claims of benignity and benefit of aerial 1080. In at least one case, they were calculated selectively, which allowed the DoC-sponsored researchers to claim a benefit to robin populations that did not exist and that was not reproduced later in the same study.

Confidence intervals and statistical power

Confidence intervals (CI), when appropriate, give some of the same information as formal power calculations (see below) and are much easier to understand. Most confidence intervals are calculated for a 95% confidence or a 67% confidence*. Roughly, a 95% confidence interval tells one the range over which 95% of results would occur if the same experiment were done repeatedly.

Perhaps an example will help. Let us suppose that we did two experiments: one in which 4 of 10 robins died of aerial 1080 and a second in which 40 of 100 robins died. In both cases 40% died. This is the way DoC typically reports its results. However, common sense tells one that these are very different results. One would have much more "confidence" in 40/100 than 4/10. Confidence intervals quantify that intuitive confidence and express it in a standard form so that it is easily understood. The 95% CI for 4/10 is (19% to 74%), for 40/100 is (31% to 50%), and for 400/1000 is (37% to 43%). If 4/10 is the result, we know with 95% confidence that the true value for robin deaths is between 19% and 74%, which usually would not be close enough to make a decision about aerial 1080 in our forests, whereas 31% to 50% probably would be enough, and 37% to 43% would be overkill and a waste of scarce research resources. The point is that confidence intervals tell us how accurately a particular result is known and thus

* A 67% confidence interval is conventionally called the "Standard Error". (Technical note: the "67" number was not chosen arbitrarily as was the 95 number. It is a natural mathematical consequence of normal distributions.)

how much "confidence" we should put in them. Without them statistics are uninterpretable and useless, or worse, misleading.

"Statistical power" is a more difficult concept, but it is vital when one is attempting to show that there is not an "important" difference between experimental and control results (e.g., robin populations after aerial 1080). Confidence intervals provide something of the same information as statistical power once the study is complete, but statistical power calculations done before a study is started allow one to design the study to have a predetermined probability of detecting a certain difference between treated population and controls. It allows the researchers to set their chance of drawing a falsely negative conclusion (that there is no important difference between control and treated populations). In what follows in this paper, we will see example after example of DoC-sponsored researchers concluding that there was no difference between 1080 poisoned native species and those not poisoned when they had merely failed to detect a difference because the statistical power of their research was insufficient. It is not an exaggeration to say that this statistical error is the basis of most of DoC's claim that poisoning with 1080 is benign to native species.

For at least 30 years, since the age of computers, power, P-value and confidence interval calculations have been trivial to do. There is no excuse for not including them in published reports. Put bluntly, any researcher that publishes summary statistics without P-values and either power calculations or confidence intervals is either deliberately deceptive or incompetent. There are no other choices. The manner in which DoC researchers have used P-values and power calculations (and more often not used them) will be seen below.

To some this may seem daunting and difficult to understand. However these people need not despair of being able to judge for themselves the quality of quantitative research. Just follow this rule: if a percentage or average is not accompanied by a P-value or confidence interval, it is worthless, or almost so, and should be disregarded. Think of the 40% example above.

The role of random sampling

Strictly speaking one can only generalize results to populations that are randomly sampled. However, true random sampling is rarely done*. Instead scientists rely on including in their study populations, multiple and varied representatives from the population to which the results will be generalized. The importance of this depends on how varied the subjects are known to be. Most scientists would agree that a breast cancer victim in New Zealand is quite similar to those say in the United States. Thus, results of breast cancer research done in the United States are assumed to "generalize" to New Zealand women. However, that is certainly not the case for forest ecosystems. So if we wish to generalize results to all of New Zealand's forests†, we MUST study a representative (if not random) sample.

All of the controlled studies regarding the effects of aerial 1080 on New Zealand forests involve a very few sites, usually less than three, and always relatively close to each other.

* Technical note: Random sampling refers to taking a sample randomly from the population to which one intends to generalize his results. True random sampling is not often done. Randomization of control and experimental groups is different and is almost always done in good research when it is possible. As noted elsewhere randomization into control and experimental groups accomplishes most of the benefit of random sampling from a population, but means that the observer is left to judge whether the set of study subjects faithfully represent the population to which one wishes to apply the result. For example, if DoC wished truly to discover the effect of aerial 1080 on our forests, it would first randomly sample plots from our forests and then randomize those plots to determine which were to be "treated" with aerial 1080 and which were to be "treated" with nothing or ground control or whatever. However, the first step might not be possible because not all forests were equally available. A reasonable substitute would be to select a "representative" set of plots and then randomize them as to "treatments".

† … which we do since DoC is actively "treating" them with the same "therapy", or at least intending to do so.

Thus, the generalizability of all the claims would be suspect even if the studies were otherwise well done, which they are not.

The need for multiple studies, multiple investigators, and multiple locations

The essential element that distinguishes an experiment from other kinds of organized observation is reproducibility. Before any assertion based on experiment can be considered a scientific fact, it must be reproduced by others who are geographically, socially, academically, and financially independent of each other.

Virtually none of the DoC-sponsored research on aerial 1080 has been reproduced, and none of it is independent of DoC influence and therefore DoC's bureaucratic agenda.

The absolute need for researcher independence: the human factor

Another reality driving the need for diversity and independence in research might be called the human factor. Scientific research is a struggle engaged in by people who are often, if not usually, passionately committed to their efforts. Their reputation, professional status and financial well-being frequently depend on being correct and getting positive results. Anyone who is honest with himself and has been there can tell you of the pressure and the tendencies that are consequent. One does not lie or actively misrepresent. He does not need to. It is easy enough to convince oneself of the "good reasons" why this result was flawed and should not be published, or why the statistical tests should be done this way or that way. The situation is worse when experimental conditions are difficult to control, as is usually the case in clinical medicine and environmental research. The net result is that many, perhaps as many as 60%, of positive results turn out not to be reproducible. Rigorous and prospective study design and strict adherence to protocols help, but the only real antidote to this very human problem is to insist that results are independently reproduced by others.

The absolute need for researcher financial independence

There is another kind of independence that is needed: financial independence. Any experienced scientist will testify that even for the simplest experiment there are a thousand ways to influence the way the results appear when finally published: choice of controls, how exceptions are dealt with, choice of statistical tests, the choice of what tests to report, where in the paper a fact is placed, conclusions, etc. The list is almost endless. Again randomization, blinding, formal protocols, and multiple researchers can largely obviate the inadvertent influence that bias and self-interest will introduce. Recognizing this, the Federal Drug Administration that authorizes all drugs and medical devices in the United States requires that pharmaceutical companies pay for multiple studies, usually randomized and double blind at multiple sites.

As has already been repeatedly pointed out, DoC-sponsored researchers are not financially independent of DoC. This flaw alone in the execution of the aerial 1080 research should shed real doubt on its validity, especially when coupled with all the evidence of bias in the published reports themselves. In addition, none of the DoC and AHB research has been done with study designs that tend to immunize against influence and bias, and consequently virtually all have the taint that financially sponsored research inevitably engenders.

Does it matter?

Many people reading this document will be asking the question, "Does it really matter? Is it not good enough to do Level 1 or 2 research; after all, one will get it right most of the time." As the head of the Northern Coromandel Biosecurity Subcommittee, Douglas Wright commented to me in a startlingly unselfconscious communication defending the lack of good science supporting aerial 1080: "management trials" (read : Level 0 or 1, controls) are what has been and should be used.

Scientists have a saying, "If you wish to know a researcher's prejudices, read the results of his last uncontrolled study". One does not get half the truth with a half-good research design. One gets a result that will reflect the bias of the researcher, which may or may not be anywhere near the truth.

It is not possible to prove this assertion, but it is possible to illustrate it with a particular case : human clinical research. In the last 60 years, clinical research has gradually evolved from what may be called organized anecdote * (which did little more than perpetuate rumors) into a experimental standard for clinical truth that can be summarized as randomized, double -blind controls with full statistical disclosure. This transformation has revolutionized clinical healthcare throughout the world because it means that clinical knowledge is no longer dependent on anecdote, opinion, or individual experience. Clinical knowledge can no longer be held captive to the prejudice of well-meaning advocates or of self-serving profiteers. Vanity and political power have taken back seats. Individual whim, academic position and self-aggrandizement no longer dictate clinical truth.

We have selected, from among hundreds of potential examples, the following two to illustrate, first, the dangers of Level 1 controls, and second the benefits of Level 5.

The case of polycythemia: Level 1 controls can kill

In 1969, on the basis of large clinical studies that compared the past with current practice, it was "known"† to the medical community that the correct treatment of an uncommon red blood cell cancer (called polycythemia rubera vera) should be either chlorambucil (a chemotherapeutic agent) or radioactive phosphorous -32. The studies, which involved thousands of patients, were done by respected academic physician researchers, but the controls were historical and some of the data had been collected in retrospect. The medical literature contained unquestioned evidence of the benefits of both chlorambucil and phosphorus -32, so the only question seemed to be which, chlorambucil or phosphorous-32, was better. Thus, a randomized, double-blind, multi-centre clinical trial was designed to settle the question. This is the only type of study that can definitively answer such a question. Almost as an after -thought, the study designers included a placebo group (i.e., one that received no treatment other than phlebotomy, the removal of blood from a vein of the patient). By 1976, the results were in. Patients in the "placebo" group lived longer, much to the astonishment of previously fervent believers in chlorambucil and phosphorous-32. The reason for this result was that the chlorambucil- and phosphorous-32-treated patients had an unanticipated consequence: a high rate of leukemia (another kind of blood cancer) , and medical care in general had improved . Also, medical care in general had improved which meant that the historical control groups were not comparable and they looked worse than the treated patients.

The medical literature is replete with examples, such as this one, of conclusions drawn on the basis of seat-of-the-pants observations and poorly designed studies that were subsequently shown to be false when careful studies were eventually done.

Childhood leukemia: a disease beaten in little randomized steps

In 1970, the overall cure rate for childhood leukemia (acute lymphoblastic leukemia) was about 5%. That means that 19 of 20 children with this disease were dead within 5 years. By 1995, the cure rate had risen to 85% overall and to more than 95% in some subgroups of children. How was this miracle accomplished? Was there a spectacular break -through in

* Very much like the DoC's claims for the benefits and benignity of aerial 1080.

† Much like it is now "known" to DoC and the Forest and Birds organization that dropping universal poison "treatments", as DoC often calls them, into our forest ecosystems benefits them.

treatment? Did someone win a Nobel Prize? Was there an overlooked Jonas Salk? The answers to all these questions are no. This therapeutic miracle was accomplished through thirty years of careful randomized double -blind clinical trials, each building on the reliable knowledge produced by its predecessors. There was no break-through, rather just hundreds of dedicated doctors incrementally using a methodology to establish truth that was so reliable that subsequent studies could use the results as a basis from which to launch the next incremental bit of progress on the road from 5% to 85% survival.

Summary and implications

The principles of good research design are well established, and remarkably, they are quite simple. We will show in subsequent sections of this document that they have not been followed by DoC in the research supporting their assertions on the use of aerial 1080. Only one or two of the two dozen or so directly applicable studies have reached as high as Level 3 controls, many lack statistical tests, and only a few contain power calculations to support negative results.

Worse, DoC's answer to the bottom line question of the net ecosystem benefit or harm is supported only by Level 0 assertions. If healthcare used this standard of evidence, we would likely still be bleeding patients as a cure for pneumonia. We would certainly be killing polycythemia patients with chlorambucil, and thousands of children who are alive today would have died of leukaemia.

Why has DoC not adopted this universal antidote to prejudice and bias that is the only way to prove causal relationships in complex systems*? Some will say it is because of cost. It is true. Randomized controls and observer blinding is a bit more expensive than the sorts of research that DoC has sponsored, but not a lot more, and the scientific soundness of the results is vastly increased. Some will say it is because high quality research is difficult to do. It is, and this may be a part of it. Some will say it is attributable to ignorance and lack of scientific sophistication. There are certainly individuals in DoC and AHB (like Environment Waikato's Dr. Wright, quoted above) who will say that Level 2 un-reproduced research done by financially captive researchers without independent review is good enough. Our answer to those people is simple, you are absolutely correct: un-randomized trials done by financially dependent researchers are good enough … unless you also want the truth.

Finally, some will even say it is because the truth about 1080 is not in the interests of the DoC bureaucracy because it could threaten their approximately $50 million dollar per year pest control budget. We can neither prove nor disprove this latter accusation, but it is difficult to account for DoC's behavior otherwise given the detailed review presented in the rest of this paper that shows the wholly unconvincing quality of the research supporting the use of aerial 1080 in our forests. (See the section entitled: ***The Department of Conservation: guardian of the environment or typical bureaucracy?*** beginning on page 57 for a discussion of the bureaucratic imperative.)

The question of net ecosystem benefit (or harm)

Burden of proof

For at least the past four decades and with increasing frequency, our Department of Conservation (DoC) and its predecessor, New Zealand Forest Service, have been routinely dropping from the air into our forest ecosystems food treated with tonnes of a poison capable of killing every oxygen-consuming creature in existence, a poison with characteristics similar

* For example, causal relationship frequently claimed by DoC that aerial 1080 results in improved forest health.

to cyanide and a poison that the United Nations World Health Organization (WHO) classifies as "extremely hazardous, 1A" (9).

A priori, it would seem that anyone aware of the interconnectedness of ecosystems and of the tendency of animals to eat any food containing concentrated carbohydrates and protein would be very concerned about a practice that dropped such food indiscriminately into a semi-tropical forest ecosystem. DoC claims that the aerial 1080 only affects two targeted "pest" species, and leaves hundreds of other species (both native and feral) unharmed. On the face of it this assertion would seem to be absurd. All known principles of ecology would say this assertion cannot possibly be true. The negative expectations are wide ranging. They include disturbed population balances, disruption of the normal food chain, secondary poisoning, predators switching prey, changes due to effects on invertebrates and microorganisms, sub-lethal effects on reproductive capability, primary, secondary and tertiary unintended consequences, etc. Indeed, Innes et al (15) commented:

> *Pests, and control methods such as toxin use, can have ecosystem-level effects by influence on properties emergent from the interaction of the biota and the physical environment. These ecosystem level properties include litter decomposition rates, relative size of different nutrient pools, and net primary productivity.*

The possibilities are so many and varied that the probability of something serious and negative is virtually certain.

It is an axiom of ecology that changes in ecosystems can have dramatic, unpredictable and far reaching side effects. For example, when the wolf, previously exterminated from Wyoming and Montana in the USA, was reintroduced a decade ago, the effects were so wide ranging and unanticipated that they surprised even veteran ecologists. River bank plants that were thought to have disappeared came back, resulting in increased habitat for certain water fowl. Bear numbers increased, apparently due to better springtime food supplies from wolf kills. Coyote numbers and sizes decreased. A massive programme of winter elk feeding was no longer necessary. The list goes on and on. The point is that ecosystems are interconnected, subtle, nuanced, and very complex. No change is entirely isolated and each element of the ecosystems affects every other.

Thus, DoC is swimming upstream against the basic principles of ecology and biology (not to speak of common sense) when they assert that somehow only their designated villains are killed and affected and the other hundreds of species are just fine, in fact, better off. For example, it is inconceivable that an animal like the brown rat (*Rattus norvegicus)* that can out-breed almost any other vertebrate* will not recover faster than slow breeding native birds. It is possible that the absence of possums which will opportunistically prey on rats might not have a differential effect on the fast breeding rat populations, but unlikely. It is possible that stoats (*Mustela erminea*) will not turn to bird nests, when their rat food supply is cut off by 1080 poisoning, but unlikely (15). However, these are speculations.

The reality is that <u>the burden of proof that aerial 1080 is an elixir for our forests and native birds is on the advocates of the policy</u>. In the following sections we will investigate how good the evidence is supporting that advocacy.

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* With a gestation period of 23 days and litters up to 10, it has been calculated that a single pair of brown rats can have up to 1,000,000 offspring in one year if population is not restrained by food, space or predation.

Unprecedented practice

One might ask: is the re a precedent? Are there countries or places that are doing something similar to dropping tonnes of food laced with a universal poison wholesale into ecosystems? The answer is that there are none. We are unable to find any country other than New Zealand that is carrying out an activity even remotely similar[*].

New Zealand uses between 2,500 and 4,000 kg/year of monofluoroacetate (16,17)[†], some 80-90% of the world supply (4,5). Most countries, if they allow it at all, use monofluoroacetate very cautiously and sparingly. For example, the Canadian Wildlife Service allows the province of British Colombia to use only 2 kg per year for an area over three times that of New Zealand, and then only in ground traps (18). Its representative expressed surprise and concern when I told him of the policy in New Zealand . Australia has used aerial 1080 baiting on a very limited basis in extremely remote areas to control feral dogs, rabbits, pigs and foxes, but no other place in the world, not a single one, is cavalier enough to routinely drop monofluoroacetate bait, or any other broad spectrum poison, into a semi -tropical forest, often within a few kilometers of populated areas. Australia's total annual use of monofluoroacetate is about 200 kg. Thus, New Zealand's density of use is about 400 times that of Australia, which, for practical purposes, is the only other country using monofluoroacetate. Thus, we in New Zealand stand entirely alone in our use of aerial 1080, or any broad spectrum poison by air .

New Zealand is unique

DoC claims that New Zealand is in a unique ecological position and that is why we are singular in the world in our use of aerial poisoning[‡]. But this is simply not true. Many Pacific Islands have unique, predominantly avian fauna. Many have extremely rugged terrain. Most have native species that are threatened by feral mammals. The state of Hawaii in the USA, for example, has a very similar situation on the island of Hawaii. Hawaii has many unique species of native birds. There are only two indigenous species of native mammals, a seal and a bat. Native birds are threatened by non -native mammals, particularly rats, the mongoose and feral cats. Much of Hawaii (the Big Island) is covered with impenetrable forest, of which there is about one million hectares, compared to the roughly 11 million hectares of native forest in New Zealand. One must presume that Hawaiians are just as concerned about their native species as are we about ours. Yet the State of Hawaii would not consider mass aerial poisoning with monofluoroacetate any more than they would with cyanide or any other poison for that matter .

To get a sense of how another country with an ecosystem management problem simil ar to New Zealand's would react to DoC's policy of widespread use of aerial 1080, we contacted the Forest and Wildlife Department on the Island of Hawaii. I spoke by telephone to the branch manager on 17 Jan 2007[§]. In that conversation, I began by descri bing the DoC/AHB practice of dropping food laced with 1080 into New Zealand forests. Here are some of his reactions:

> *I have read about that. I don't understand how you get away with it … you are pretty cavalier using a poison like that … you are sure to get a lot of secondary*

[*] … unless one accepts the United States' use of dioxin during its Vietn am War as a valid precedent, which most of us would not.

[†] The exact number is unimportant except that the customs department reports more than DoC and AHB admit (16).

[‡] Actually, DoC does not usually admit that it is doing so mething that no one else does or would consider doing. See the current ERMA application for example. However, when pressed this is their rationalization.

[§] Telephone conversation on 17 Jan 2007 with Miles Nakahara, Forest & Wildlife branch manager on the Island of Hawaii.

> *poisoning … they would never allow that here … they took 1080 off the market here; we can't use it at all.*

He knew of only one case in which a poison was dropped from the air in Hawaii. It was an experiment in which an anti-coagulant was dropped in a 1000 acre test area to determine the effectiveness of aerial administration at controlling mongooses. The carefully monitored study demonstrated such secondary poisoning and unintended damage that it was never repeated. He stated that now they use traps. Finally, he asked if the objective was to eradicate the possums. When I told him the objective was control and explained that forests would have to be re-poisoned every 2 or 3 years, he was dumbfounded, <u>"That means you will be destroying the forest. You will lose the very thing you are trying to save."</u>

Another example that relates directly to the aerial 1080 poisoning programme is given in *Viewing invasive species removal in a whole-ecosystem context* by Zavaleta et al (118) from which we quote here:

> *When exotic predators and prey co-occur, <u>eradication of only the exotic prey can also cause problems by forcing the predator to switch to native prey</u>. In New Zealand, introduced rats R. rattus and possums Trichosurus vulpecular are an important part of the diet of the stoat Mustela ermina, an exotic mustelid(19). Efforts to remove all three species by poisoning the prey species had an unexpected result: the stoat populations were not eliminated by either the prey eradication or the poison application and, in the absence of abundant exotic prey, the stoats switched their diets to native birds and bird eggs.*[*]

We wish to make it clear that we do not consider these quotations as scientific evidence of the harm of aerial 1080 in our forests. What it is evidence of is that the DoC/AHB practice of mass poisoning our forests is seen as highly suspect by at least one neutral observer whose business it is to manage an ecosystem very similar to our own.

Thus, in addition to swimming upstream against the basic principles of ecology and environmental management, New Zealand is definitely on the leading, or trailing, edge depending on your point of view, in its use of aerial 1080.

Ecosystem level studies showing net effect

Putting aside for the moment the effect on specific populations of native species, which we will address in subsequent sections, one would expect that there would be good solid scientific evidence of net ecosystem benefit from the use of aerial 1080. There is none. What we mean precisely by this stark statement is:

> **There is not one controlled study (e.g. Level 1 or better) addressing the ecosystem-level benefit, harm, or the unintended side effects of the practice of routinely dropping large amounts of food laced with a broad spectrum poison into our forest ecosystem … NOT ONE.**

A fortiori, there is no high quality research, such as a randomized, blinded and controlled study (Control Level 5).

The only study even addressing the question of ecosystem-level effects is a theoretical paper on a methodology to examine the question by Innes and Barker (20). In it they make a number of revealing comments, such as,

[*] The underlining is ours.

> *The ecological consequences of toxin use for pest mammal control are complex. Toxins kill many targets directly but non-target individuals may also be lethally or sub-lethally poisoned. Secondary or even tertiary poisoning of individuals of other species may occur. (21)*

And that,

> *We suggest that large-scale use of toxins continues in New Zealand despite these large knowledge gaps. (21)*

Perhaps the most important opinion expressed in this paper is that aerial drops should be "regarded as experiments" (21), which they most definitely are not.

The problem with Control Level 0 "research": who to believe?

To illustrate the problem with Level 0 "research" (i.e., personal testimony and opinion) we give the following example, which appeared in a recent newspaper story (22). The following quotations were found:

> *"It's so quiet. You normally hear the birds but there is nothing. There is very little birdlife at all. It's silent in the Mamakus at the moment. You can smell the rotting carcasses before you get anywhere near them." Robin Fredricksen, Rotorua, trapper commenting on Mamaku Forest after a recent 1080 aerial drop.*
>
> *"They say Kiwi, which are endangered, once flourished here but these old timers say since 1080 they have all vanished. We shouldn't be drenching the area with poison. You can't tell me it doesn't do any damage. It's so quiet out in the bush here. You used to hear the birds chirping away." Mamaku dairy farmer.*
>
> *"It indiscriminately kills everything in its path including little pigs, good stags and native birds. In parts of the Kaingaroa Forest there is no birdlife at all where you once heard magpie, skylark and other birds. It's cleared it out." Alec McIver, Rotorua Deerstalkers*

On the other side:

> *"There has been a lot of research on this. They have refined it and I've seen incredible results from its use. Twenty years ago I would not have approved of it but since then I have watched and read the research papers and I'm quite impressed". Chris Ecroyd, Rotorua Forest and Bird president.*

To be certain that Mr. Ecroyd was not referring to scientific research that we had missed, we contacted him asking for the most important research papers by which he has been "impressed". He responded with three citations: one from 1995, one that was outright propaganda from DoC, and one that was a technical review from the Animal Health Board. All contain only references to the research papers that are examined in detail in the section, *Species level evidence* (page 21) of this document.

In another newspaper article DoC weighed in with:

> *The Pureora Forest "has greater diversity of bird species and population densities than almost any other area in New Zealand", John Gaukrodger, DoC, Hauraki Area Manager (23)*

Or from a major 1080 advocacy document prepared for DoC and A HB (6),

> *The direct and secondary targeting of these predators [rodents, stoats, and ferrets] by 1080 operations benefits forest regeneration as well as assisting bird recovery.*

Many will say that we should simply accept the op inions of our government officials and their hired scientific experts. The problem with th is is two-fold. First, if there is one lesson in the history or science, it is that expert opinions are often wrong*, and even more so the opinions of vested authorities. In fact, it is anathema to the most fundamental principle of science, it s method of determining truth, the scientific method, to suggest that opinions from authorities are a substitute for experimental evidence , if such evidence is obtainable. (Sometimes such evidence is not obtainable, but that is not true here.)†.

Second, our New Zealand "experts" are virtually all beholding to the advocating agencies, DoC and AHB. Essentially everyone (in the world, not just New Zealand) who do es research on the effects of aerial 1080 is either a DoC employee or is dependent on the goodwill of DoC for research contracts. <u>They are not independent and consequently should not be assumed to be unbiased.</u>

In the end, we are dependent on testimonials on both sides for ecosystem level (net effect) information. One side (DoC and AHB) has public money (lots of it) and therefore most of the "expert" testimonials. They say the forests are filled with birdsong and the ecosystems are greatly benefited by aerial 1080. The other side says the forests are silent. Both attribute the cause to aerial 1080. Some testimonials come from outdoorsmen and trappers who have spent their lives in the forests. Some come from farmers who have worked the land for decades. Most of the claimants from both sides benefit if the practice of aerial 1080 use goes their way. Some have careers dependent on the advocating and benefiting agencies (e.g. DoC). However, all of the testimonials have one thing in common; they are just opinion.

Clearly, the way out of this dilemma is an experiment or experiments of sufficient quality to settle the question‡.

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* This was systematically pointed out by Thomas Kuhn in his classic 1962 monograph, *The Structure of Scientific Revolutions*. Another much more entertaining source is Bill Bryson's, *A Short History of Nearly Everything*, which is advertised as a popular history of science, but is in fact a history of experts and authorities being wrong.

† … providing such evidence is obtainable. Sometimes it isn't, but that is not true in the case of New Zealand's use of aerial 1080.

‡ Exactly as was done in the 1980's at a cost of several millions of dollars to settle the question of whether vitamin C cured or ameliorated the common cold. Three randomized double blind clinical trials settled the question once and for all —at least for doctors and scientists.

An example study design

So what is to be done when opinions differ? This is not like the global warming issue that is impossible to resolve with experiment. The answer here is quite accessible and quite simple: do the study. The definitive study would look something like this:

- At least 10 matched triad areas of sufficient size and separating distance to insure relative ecological isolation (for the duration of the study).
- The areas would be randomly assigned to one of three arms: 1080 treatment, placebo treatment (i.e., with identical baits not laced with 1080), or ground-based trap treatment*.
- All parties would be blind (to the extent possible) as to whether an area is getting placebo bait or not. Subjective assessments would be double blind.
- Pre/post design with cross-over after 4 or 5 years (two or three poisoning cycles)
- Execution and analysis by disinterested parties, i.e., entirely independent of DoC and AHB influence.
- Data collected in parallel by specialist teams on at least 10 species of birds, 5 species of "pests", representative species of invertebrates and representative species of plants.
- A detailed cost analysis of the alternative interventions should be included.

It would not be difficult. The scientific world would stand up and cheer. New Zealand would be the world leader in ecological research , instead of the leader in the dubious practice of mass poisoning forest ecosystems. There would be hundreds of discoveries as secondary fall out and dozens of publications in internationally respected journals.

It would be expensive, costing perhaps $20 million or more, and the study would take at least 6 years, probably more. The cost cited is large, but it is a small fraction of what is being spent every year on a practice that may be doing significant and even irreversible damage to our forest ecosystems, our unique native species, and our reputation as a sane, environmentally conscientious country. Much of the cost of such a study is being spent anyway in the aerial 1080 "operations" now being undertaken. Twenty millions is also a fraction of the amount that has been spent on aerial 1080 research much of which is nearly useless, falling far short of answering the critical questions regarding the use of aerial 1080 (as will be demonstrated in subsequent sections).

We are not suggesting that every scientific question needs to be settled with the highest quality of study design. It would be foolish for exploratory research. It is impossible in what might be called "observational" research. There are issues of such minor importance that the additional cost would not be worth the additional cost, though that is often surprisingly small. Randomization is not always possible, such as when human subjects are to be studied over a long period. For example, a study of the beneficial effects on health of drinking alcohol would be almost impossible to effectively randomize. However, for the case at hand, namely a multimillion dollar practice that could be seriously damaging native forest ecosystems, properly designed studies are possible, essential, and affordable relative to the potential benefits and current costs.

Of course another alternative would be for ERMA simply to ban aerial monofluoroacetate, which would immediately bring New Zealand's ecosystem management policies into conformance with those of the rest of the world .

* Remarkably no study has been done addressing the effect of aerial 1080 at either the species or ecosystem level that has included these three obvious alternatives.

Summary: Ecosystem level evidence

Repeatedly dropping food laced with monofluoroacetate indiscriminately into a forest environment is an ecosystem level intervention of an unquestionably extraordinary nature that *a priori* would be expected to have wide ranging effects on both flora and fauna. Thus, ecosystem level scientific evidence is required to prove the benignity and benefit of such a practice. The stakes and costs are hig h, and yet, despite years of massive aerial 1080 "operations", not one Control Level 1 or better study has been done at the ecosystem level . Thus, the question of net harm or good of aerial 1080 is unanswered in a scientifically credible way.

The species level evidence

Bird populations and mortality

Contrary to suggestions by DoC (1,24,25), many bird species are very sensitive to monofluoroacetate poisoning [*] in the concentrations typically administered by DoC and AHB aerial poisonings. Indeed a paper (26) published over 27 years ago, well before the DoC era, stated the already-known situation regarding bird sen sitivity to 1080:

> *"Thus, most of the small insectivorous birds probably require only a tiny fragment of a bait (less than 0.1 g; perhaps one mouthful) to receive a lethal dose of 1080. The available evidence (i.e., a consideration of the diets, the species of birds killed, and the amount of bait probably required for a lethal dose) indicates that most of our land bird species should be regarded as being at risk of being killed by feeding directly on poisoned baits or secondarily on poisoned prey."*

So the question at hand is: What is the evidence that monofluoroacetate when mixed into food and dropped indiscriminately into the forest does kill native birds, and, if it does, to what extent? *A priori* the assumption would be that it does kill native species since it is a universal poison and the poisoned food is also either food for birds or for their prey. If we for the moment ignore the vast, immensely complicated plexus of New Zealand's ecosystems, the issue reduces to a sort of contest between the toxici ty/breeding rate for the birds and toxicity/breeding rate for the so-called "pests". Which wins is an empirical question, so it is essential to look at the direct scientific evidence. Theoretical arguments regarding the sensitivity of birds to 1080 are of no use. We must determine empirically whether bird populations are affected either positively or negatively.

Dropping food laced with monofluoroacetate into forests is known to kill native birds. Numerous studies have documented bird deaths from monof luoroacetate immediately after aerial drops of the poison or by direct exposure to the poison. Table 3 cites a few examples

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[*] Although the per kilogram of body weight sensitivity o f birds is generally less than that of mammals, in proportion to gram of food metabolized, the field metabolic rates of birds are generally higher than those of mammals. This means that bird consumption of food is generally greater that mammals of comparable body mass, which in turn increases the susceptibility of birds beyond what one would expect suppose from **Table 1 Relative Toxicity of Monofluoroacetate**. However, regardless of this, what matters for the purposes of this dis cussion is not the theoretical susceptibility of birds, but rather their operational susceptibility of birds as aerial 1080 poisoning is done in New Zealand, since no other country engages in such a practice.

(27,28,29,30,31,32,33,35)*. The species include robins, tomtits, moreporks (ruru), blackbirds, kiwis, weka, pigeons, brown creeper and others. In the 1970's and 1980's baits were changed with some apparent reduction, although this was not established with good studies (33). Furthermore, such reports will likely under-report actual numbers because many will die in the nest or roost and thus never be seen (55).

? **Table 3 A Brief Summary of Documented Bird Deaths after aerial 1080 "operations".**

Reference	# Dead Reported	# Natives
Nugent (31)	20	3
Peterson et al (34)	hundreds	unknown
Peterson et al (33)	34 blackbirds 14 chaffinches magpie song thrush goldfinch greenfinch house sparrow hedge sparrow skylark redpoll	15 tomtits
Powlesland et al (28)		12 robins 5 tomtits 1 morepork
Spurr (35)		Australasian harrier pukeko rifleman brown creeper whitehead yellowhead grey warbler silvereye weka kaka kea morepork New Zealand pipit fantail tomtit robin

Before the 1990's, DoC-sponsored researchers were quite candid about the effects of aerial 1080 on birds. For example, Spurr (35) concluded that ground-feeding rare and un-dispersed bird species and slow reproducers are less likely to tolerate the depredations of poisoning by 1080 than species without these characteristics :

> *"Species with poor reproductive potential and poor dispersal have a high risk of non-recovery, e.g., the three species of kiwi, the takahe, kakapo, laughing owl, bush wren, rock wren, fernbird, yellowhead, stitchbird, saddleback, kokako, and*

* In looking through this evidence one cannot miss noticing a remarkable shift in tone in government-sponsored research publications on 1080.publication. From 1970 to the early 1990's, the admissions of bird deaths and toxicity were both common and quite frank. After that period, which perhaps not coincidentally corresponds to the time when DoC's budget was dramatically increased by $50 million dollars to "control possums", DoC-sponsored publications became defensive in tone and began avoiding discussions of bird deaths, touting instead their improved nesting success.

> *New Zealand thrush. Species with either poor reproduction or poor dispersal are medium risk species, e.g., the New Zealand falcon, weka, New Zealand pigeon, kaka, kea, the three species of parakeets, the morepork, rifleman, brown creeper, whitehead, and robin. Species with good reproductive and good dispersal capacities are low risk species, e.g., the Australasian harrier, pukeko, kingfisher, welcome swallow, New Zealand pipit, grey warbler, fantail, tit, silvereye, bellbird, and tui."*

He then went on to comment that in those cases " special precautions should be used, including possibly shifting to bait stations rather than aerial application ". To our knowledge no such "special precautions" have been undertaken and DoC is currently planning to substantially increase its aerial 1080 "operations". Thus, while it is possible (but by no mean proven) that some bird species populations may indeed be aided over the long term by such poisoning operations, there are a number of species that are at significant risk of not recovering , and of course, repeated aerial poison operations would be expected to compound this effect.

Indeed it has been clear from numerous reports from almost the beginning of the use of aerial 1080 that native birds were being killed by it. Furthermore, such reports will likely under - report actual numbers because many will die in the nest or roost and thus never be seen (55).

To counter this criticism, DoC claims (1) that poisoned populations of birds recover over the following season to "greet a world" with many fewer predators than previously and thus to benefit from this exposure by much improved breeding success. Indeed if one reads DoC's literature, monofluoroacetate is a nearly universal magic elixir for all that is wrong with our forests (1):

> *"The risk assessment demonstrates that there are significant benefits to the environment, market economy and community from the continued use of 1080. Under the current stringent controls on the use of 1080 there are no significant adverse effects on the environment or human health. Without the continued use of 1080 for Tb control and conservation purposes, the most likely alternative control option would be an increased use of cyanide baits and traps. This would lead to a reversal of hard-won gains in Tb control and less protection for native ecosystems and threatened native species."*

And,

> *"Extensive research and post-operational monitoring has shown that current 1080 aerial operations do not pose a threat to populations of native vertebrate (especially birds) or invertebrate species, in either terrestrial or freshwater habitats."*

In spite of DoC's claims regarding bird death s cited above, there is remarkable little scientifically sound research addressing the question of the effects of aerial 1080 on our native birds, and what does exist is far from demonstrating a beneficial effect on native bird populations. After over 25 years of aerial 1080 "operations", DoC even admits "there is little LD50 data on New Zealand insectivorous birds" (25). Furthermore, as we will show, the vast majority of species including native species have never been adequately studied at all. This is difficult to understand given that our birds are universally considered to be our most important contribution to the world's biota. Most , if not all, of the existing studies are of limited value because of poor experimental design, invalid statistical analysis and inference, obvious bias in many cases, and the lack of independence of the researchers from DoC, an agency that is an advocate of and major beneficiary of aerial 1080 poisoning.

We have examined the best of the scientific controlled research. Here are the results.

The Miller et al study.

This study (36) monitored the bird populations by counting birds sighted and their calls for one year following an aerial 1080 drop on Rangitoto Island that was intended to poison the possums and wallabies. This study is typical of several other studies that we have cited below, but discussed in less detail (see section: *Other research cited in the AHB/DoC submission regarding native birds* on page 29).

Claimed results: No bird populations appeared to be impacted in the immediate aftermath of the 1080 drop. Over the following year, four species appeared to experience an increase in population.

Flaws:

- Study lacked simultaneous controls (i.e., this is a Control Level 1 study). This obviated credible use of most of the data because of seasonal variation.
- Insufficient pre-poisoning and post-poisoning observation periods: one two-day pre-poisoning period on which the subsequent results hinged.
- No power calculations or confidence intervals on negative results.
- Harrier is claimed to show a vast increase in its population despite zero being observed in the pre-poison period, and having substantial standard error in the post-observation period. (Similarly for the Tui.) It is unexplained why the Harrier should have increased at all.
- Lack of even intermediate-term follow up.
- Important ground feeders, the most likely to ingest 1080 (directly or indirectly from aerially deposited baits), were not studied.
- Reported that statistically significant increases in some populations occurred within one month of poisoning, but failed to explain how this could have been consequent to the 1080 poisoning operation. For example, Silvereyes went from 0.93 to 2.33 in one month and one year later were found to remain at essentially that level 3.00 +/- 0.35.
- Multiple comparison errors: P-values were not corrected for the multiple comparisons despite standard methods being available to do so. Instead a higher than usual alpha level was used (0.001), but this may have obviated detecting important changes in some populations such as the apparent decrease in populations of blackbirds and thrush which were not distinguished.
- Concentration density of poison not reported.
- Inapplicability of study to mainland aerial 1080 because the goal was extermination on an island where reapplication was not necessary.

Conclusion: without adequate controls (either simultaneous or extended historical), a reliable conclusion cannot be drawn from this study. The authors claim that, "The poisoning does not appear to have had any negative effect on the bird populations of Rangitoto". Strictly speaking this does not overstate their results, but the implication that there was no negative effect is not supportable from their study, and the final statement in the abstract that "the removal of browsing mammals may in future prove to have beneficial effects" does not follow in any way from the results presented in this paper.

The Pierce et al study: decreased numbers of tomtits

This 1990 study (32) involved several bird species. The bait was cereal at 4 gm/ha , a relatively low number. The discussion and abstract sections are mostly free from apparent bias. The authors rationally and scientifically discuss the various factors that could account for the observed differences*. For kiwis, rurus (moreporks) and kokakos, numbers are simply too small to draw conclusions. Tuis and silvereyes showed increased numbers (as measured by call counts)†. <u>Tomtit and blackbird numbers decreased.</u> For fantails, grey warblers, kukupas, rosella, and mynas the study did not detect differences. However, since power calculations and confidence intervals were not reported, as is the norm in DoC-sponsored studies, no conclusion can be drawn from this. To the authors' credit, they did not falsely claim that the failure to detect a difference between control areas and poisoned areas for some species implied there was no difference.

Powlesland et al: The egregious case of robins and tomtits.

In 1998, 1999, and 2000, Powlesland et al (28, 29, 30) reported on a three year study evaluating the effects of aerial 1080 on robins, tomtits and moreporks. These papers are unusual, if not unique, in that they extended over three years. They are of exceptional importance because they are the ones cited by DoC and others (37) as establishing aerial 1080 as benign to and good for the forest birds. Indeed, together with the Pierce study (32), they are the only controlled (Level 2, not randomized of course), prospective studies on the effect of aerial 1080 on native birds.

In essence, this is it. The science behind the avalanche of paper and claims from DoC and AHB asserting the benefit and benignity of aerial 1080 to native birds rests only these papers. Thus, these papers should be of considerable interest to the deliberations of ERMA, which is supposed to make its judgment on basis of scientific evidence.

When we started to examine the first of these papers we noticed that tests of statistical significance (P-values) were not given to support the major conclusions of the study, even though P-values were reported to support incidental points in the text. So we re-analyzed the data from the published papers and report these results in Table 4. This was possible since the major conclusions are based on proportions which were reported and can be reanalyzed without the raw data detail. Thus, Table 4 shows the results of the three studies on the two major outcome variables, survival and population status, after the forest was poisoned.

In addition, we analyzed the survival data for robins over the entire three years using multivariate logit linear model (SAS Institute, Version 8.2, Proc Logistic). The logit model "predicted" the dichotomous dependent variable, survival, as a function of the independent variables: poisoned (or not) and 1080 distribution density. Both independent variables are highly significant predictors of survival ($p < 0.001$, and $p < 0.002$ respectively).

* Sadly, this cannot be said for most of the subsequent studies in post-1993 "big money" period for DoC.

† That tui numbers by some unknown mechanism are observed to increase after aerial monofluoroacetate poisoning is a common, if unscientific, claim that DoC has repeatedly seized upon in their "silent forest" propaganda.

? **Table 4. Summary of Survival and Population Results of the Three Tomtit and Robin Studies**

		Survival					***Population Status***				
		Poisoned		***Control***		***P Value (1)***	***Poisoned***		***Control***		***P Value***
Year (Bait)	***Species***	***# Dead***	***# Alive***	***# Dead***	***# Alive***		***# Pre***	***# Post***	***# Pre***	***# Post***	
1996 (Carrot at 12 gm/ha)	**Tomtits** (banded)	2	0	0	0	1					
	Tomtits (total)	5	0	0	0	1					
	Robins (banded)	12	10	0	24	<.0001	28	36	32	33	0.60
	Robins (total)	12	16	0	32	<.0001					
1997 (Carrot at 8 gm/ha)	**Tomtits**	11	3	0	9	<.001					
	Robins	3	28	1	41	0.30	35	48	49	57	0.66
1998 (Cereal at 4 gm/ha)	**Tomtits**	0	14	0	16	1					
	Robins	0	17	2	40	1					

1. The P-values reported in this table are calculated using Fisher's Exact t est as implemented by the SAS Institute, Baltimore, USA, Version 8.2.

2. In most cases these P-values were not reported by the paper's authors.

In substantial degree b ecause of small numbers, few conclusions can be gathered from these studies. However, the following can be stated :

- Two variables account for the difference in robin survival: whether their habitat was poisoned or not and the dose of poisoning, although dose of poisoning cannot be distinguished from type of bait since the two were not var ied independently. This is a hard scientific, statistically valid conclusion, i.e., ignoring its obvious design limitations, this study shows that aerial 1080 administration was associated with robin deaths.

- At doses of 8 gm of 1080 per hectare or more, b oth robins and tomtits died in high percentages when food laced with 1080 was dropped into their habitat, the Pureora forest park ($p < 0.0001$ and $p < 0.001$).

- Post-study robin population numbers between poisoned and not-poisoned areas were not different, i.e., there was no population benefit.

- **Nesting success** for robins was better in the immediate period following aerial 1080 administration in the 1997 drop of 8 gm of 1080 per hectare. However, in the 1996 poisoning, a P-value was not calculated and in the 1998 drop, the nesting results were not reported at all.

- One of six tagged rurus (moreporks) died of secondary poisoning.

This is simple enough: aerial 1080 kills robins and tomtits, there was no effect on populations detected despite some indication of improved nesting success in the immediate post-poisoning period. However, the studies that constitute the flagship for DoC's claims regarding birds are hardly conclusive even for these two species in this one location. There were several design anomalies, if not flaws, in these studies. For example, the control area was used as the treatment area in the second year.

However, consider the disparity between the reality of these studies and the claims made the authors, for instance this claim in the abstract of the 1998 paper (28):

> *The high nesting success in the treatment area resulted in the number of robins present just before the start of the next nesting season (August 1997) being 36, a 28.6% increase in the number present prior to the poison operation.*

In reality, there was no difference between the treated and untreated plots in population of robins ($P = 0.60$, Fisher Exact Test). In contrast, the death rate was much higher among robins, being 55% versus 0% ($P < 0.0001$). Despite the high profile of these much cited (38) "results", <u>neither P-value was reported by the authors</u>. This cannot be attributed to incompetence or negligence since P-values were reported in various other inconsequential places in this paper. Each reader must draw his own conclusion from this incident, but we do not see an explanation other than deliberate misrepresentation of the study results. If such misrepresentation was done in plain view, one wonders what else was done that is not evident in the published report.

And as if this level of misrepresentation in the original papers were not enough, DoC's submission document to ERMA (1, Page 303), this comment appears:

> *However, where 1080 operations occur prior to the robin breeding season, nesting success has improved significantly – more than compensating for any losses (Powlesland et al. 1999b).*

And (1, Page 13),

> *At Pureora, in the central North Island, robin fledging success has been shown to be far higher where aerial 1080 use reduced pest populations, by allowing robins to lay and hatch multiple clutches of eggs in a season. In the non-treatment areas, fewer robins survived to maturity and more adults were killed on the nest.*

Here DoC has not pointed out that the Powlesland (28) study failed to confirm the results of the nesting success and all three studies do not support the implied result for populations, indeed DoC is implying just the opposite. Nesting success is an intermediate process variable, which by itself proves nothing about population success. What counts is population success as DoC has repeatedly pointed out when confronted with the deaths, even mass deaths of individuals (39).

The following quotation shows how DoC characterizes the tomtit results in their ERMA submission (1, Page 303):

> *… cereal bait operations have little, if any immediate impact on tomtit populations.*

This is the most serious kind of chicanery and misrepresentation by omission. DoC's own studies cited here (Powlesland, Pierce, Westbrooke) have shown a profound impact of carrot bait on tomtit survival and no population study with cereal baits producing credible data has been published.

Again in the Powlesland study (29), the authors conclude (the underlining is ours) :

> *In areas at Pureora where no mammal predator control had been carried out, the robin sex ratio was 1.5-2.0:1.0 (male:female) and their nesting success was low … Thus, the long-term viability of such populations depends on no further mortality events impacting on the adults unless they coincide with increased nesting success and recruitment.*

This inflated conclusion regarding long-term viability is simply not justified by the results of the study. In fact it is belied by their own results which strongly suggest that populations did not benefit short term. Moreover the population effects were not examined long term. So, the authors appear to have deliberately misrepresented the implications of their results. In addition they failed to mention the nesting success results from the 1998 poisoning which is a glaring omission given the importance that they place on the 1996 study results.

Several additional observations and concerns should be noted about these studies. Populations of these birds have not been studied over a sufficient period of time. Sufficient numbers of tomtits were not monitored to draw conclusions, except in one case and then the conclusion was devastating. Baits have been varied in type and in concentration of sowing by 300%. None of these studies have been repeated.

Two other papers bear directly on the tomtit issue ; Westbrooke et al (40) in 2003 and Westbrooke et al (41) in 2005*. The 2005 study is an extension of that reported in 2003. The study had one simultaneous control area , examined population density from 2 -6 weeks post poisoning and used cereal baits. It showed a maximum negative effect of cereal baits on tomtit population density of -36% at the 95% confidence level. The second study, showed a similar effect on tomtit populations. The authors noted in the abstract that the study "indicates that cereal bait operations … have little, if any, immediate impact on tomtit populations". This is only true if one considers a potentially -36% effect "little" . Of course the study addresses neither the intermediate term effects nor the effect of repeated biennial or triennial poisonings.

What the authors fail even to mention is that the study also showed a profound statistically significant negative effect (between 20% and 50%) of carrot baits, even at the much lower dose, on tomtit populations : 2.4-4.0 gm/ha sowing concentration (see Figure 1, page 145).

Carrot baits are still in wide use by DoC†. They are requesting permission in their ERMA submission to use up to 30 gm/ha (42), 10 times the amount used in these studies, and we know that for the previous decades concentrations of 10-14 gm/ha were common. Finally, it should be remembered that the above studies only involve two species out of hundreds.

* The authors describe this report as a "case study, not a formal experiment".

† Two of DoC's last 20 aerial 1080 "operations" used carrot baits, as reported by Barbara Browne, General Manager Operations, DoC.

Despite these negative results on tomtits and robins , DoC continues to proclaim the benefits of aerial 1080 to native forests and birds (43).

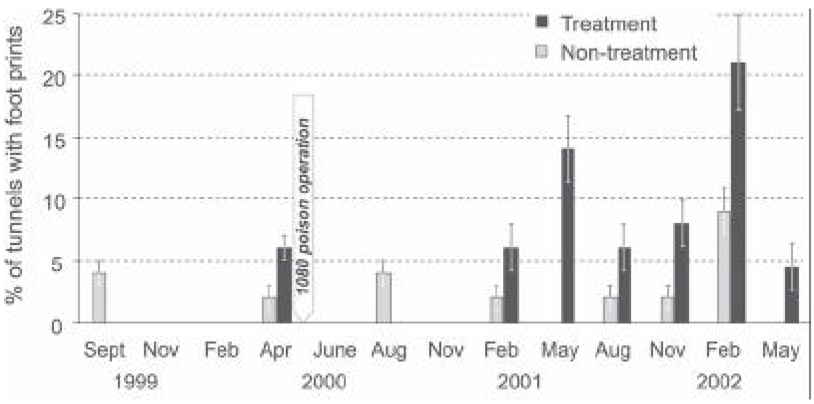
Study of nesting success and population impact of aerial 1080 on kaka and kereru

A study frequently referenced looking for an upside to balance the clear downside of aerial 1080 on New Zealand's native fauna is that of Powlesland et al published in 2003 (48). This study examined kaka and kereru in Wirinaki Forest Park over breeding seasons between October 1998 and June 2002 to determine what impact aerial 1080 operations had on, among other things, nesting and population success. It was a controlled study with a single poisoned and a single non-poisoned areas* (or "treated", as the authors call it displaying with apparent lack of self-consciousness their brazen bias).

When they reported on the nesting success and fledgling survival for the radio-tagged birds, **incredibly, the authors did not distinguish the data from the poisoned and unpoisoned areas.** Instead they only showed the combined results from both the treatment and non - treatment areas. Hence, their reported research results could not demonstrate either ne sting or population success resulting from aerial 1080 operations. Moreover, their study could not have detected a statistically significant difference in nesting success since neither of the bird species bred in the season following the poisoning. The k aka had only limited breeding in the second season and by the third breeding season, the populations of both possums and rats had recovered in the treated area. Hence, **the study demonstrates absolutely <u>nothing</u> about the impact of aerial 1080 on the nesting success or populations of kaka and kereru.**

On the other hand, there were some interesting observations derivable from the study's data that shed considerable doubt on the rational used by DoC to justify the $80 million per year pest control efforts. One observation was that rat population numbers recovered relative to the non-treatment area within 14 months of the poisoning. This is, of course, expected given the remarkable reproductive capacity of rats, but it flies directly in the face of DoC's cla ims that populations of birds will benefit from triennial poisoning of the forest with aerial 1080.

Another observation was that mustelid numbers actually seemed to increase in the treatment area. Why this happened is uncertain, but it has been observed (118) that poisoning of possums and rats can lead to the stoat switching prey to native bird and bird eggs with the consequential decreased competition from rats and possums. Regardless, more mustelids would not seem to bode well for native birds.



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* Reminder: As always, proper design would be to use several plots to be randomly selected for treatment or non-treatment, and to analyze using plots as the unit of analysis or a linear model with "treatment" one of the independent variables. The reason for this is that without multiple treated and control areas one can never be confident than an observed difference is not due to inherent differences in the areas. In short, the only thing a study like this had any possibility of showing was that the treatment and control plots are in some way different, which we already knew from the author's descriptions of the area.

As usual with DoC-sponsored research, it is instructive to look at the difference between actual research results and the authors' claims for them. Thus, we have the last sentence in the paper's Abstract:

> *Effective control of introduced mammalian predators … should benefit these bird populations.*

Notice that the authors did not claim that control did benefit bird population, but rather that it should. How the authors would justify this statement given the actual results of this study is almost beyond comprehension. DoC and others have interpreted these results to mean that aerial 1080 is absolutely necessary for kaka and kereru survival.

Having completely flopped on showing population benefit themselves, Powesland et al cited the Innes 2005 study(44) the results of which are analyzed below. However, the cited Innes study did not address the issue of Kaka and so the could not help with the Powesland paper's failure for that species.

"Pest" control and bird populations (Innes et al, 2004)(44)

When one examines research papers that Powesland (48) referenced as demonstrating population improvement under intense pest control, again the claims are vacuous. For example, a study by Innes et al (44) of several bird species looked for such population improvement under intense pest control. This study implemented a variety of control toxins and strategies including an initial poisoning with aerial 1080 in October of 1997. It is so poorly designed, analyzed and interpreted that it deserves detailed attention as it illustrates most of the reasons why DoC's research often is not worthy of the term "science".

First, the study design is such that valid conclusions are impossible. There was only one control and one treatment area, which means that any observed population differences between control and poisoned areas might have been simply due to inherent differences between the areas studied. There is ample evidence that this might have been the case since the populations varied substantially between the Motatau (poisoned) and Okaroro (non-poisoned) areas. For the twelve species examined, the ratios between Motatau and Okaroro for their mean 5-minute bird counts varied from 19.88 for the Rosella to 0.41 for Myna. This suggests that the treatment and control areas were grossly dissimilar. Of course the cure for this kind of problem is random selection of multiple areas to be assigned to control or "treatment", but not a single randomized aerial 1080 study has even been done.

The author's acknowledge this problem to some degree with a comment well hidden in the Methods section of their paper.

> *The lack of replication in this study means that any pest control effects are confounded with area differences, so inference from statistical analysis is weak.*[*]

This is an understatement. A correct appraisal would read that "inference from statistical analysis is impossible". In short, the results of this study should have been ignored. However, that did not prevent the author's from drawing grand conclusions as will be seen below.

[*] The underlining is ours.

Second, the population results were incorrectly analyzed. The population results are given in author's "Table 2", our Figure 1. Although their analysis is incompletely described, the authors apparently used a general linear model to analyze the data with an area/year interaction term as an independent variable. This choice is simply not appropriate for two reasons. The interaction term does not tell one whether there was an independent effect of "area" (i.e., poisoned or not), which is the issue in which we are interested, that is, whether the "area" variable had an independent effect especially in relation to the baseline popula tion values.

In addition, the "year" variable as such could not give useful information. Whether the "year" variable was used as an ordinal or discrete variable (which is not stated), it would not answer the question of how the populations changed from the baseline (pre-treatment) period to the post-treatment period in Motatau and in Okaroro. A correct model would have included "area" and pre-poison/post-poison as independent variables, the question being whether "area" correlated with affected populati on numbers from baseline to poisoned periods. The erroneousness of this analysis method is illustrated by the case of the Chaffinch, for which it comes to a self-apparently false conclusion that the Chaffinch population significantly increased in the trea tment area. Thus, it is evident looking at the summary data in "Table 2" that relative to the baseline in 1996/1997 (the pre -treatment period) the population increased more in the control (Okaroro) area than in the treatment area (Motatau), and yet the an alysis of the authors concluded just the opposite. Using the data given in "Table 2", we calculate the relative growth of the Chaffinch population to have been about 33% more in the unpoisoned area, Okaroro*.

? Figure 1: "Table 2" from Innes et al 2005

Table 2. Differences in changes in mean November 5-minute bird counts during 1996–2001 at Motatau (pests controlled from October 1997 onwards) *cf.* Okaroro (pests not controlled). *P* values < 0.05 indicate a significant interaction between date and area (treatment *v.* non-treatment). No counts were undertaken in Okaroro until 1997. We regarded counts of sixteen other species as too sparse to warrant their presentation here. Scientific names of all bird species are in the Appendix.

Species	*P* value		Mean no. of birds per station					
			1996	1997	1998	1999	2000	2001
Significant increase at Motatau *cf.* Okaroro								
Chaffinch	0.01	Motatau	0.48	0.41	0.66	0.56	0.43	0.39
		Okaroro	NA	0.21	0.28	0.53	0.44	0.46
Eastern rosella	< 0.01	Motatau	0.96	0.63	0.93	0.91	1.01	0.85
		Okaroro	NA	0.04	0.40	0.62	0.29	0.55
Kukupa	0.01	Motatau	0.21	0.22	0.56	0.78	0.66	0.70
		Okaroro	NA	0.14	0.32	0.34	0.28	0.36
Myna	0.01	Motatau	0.02	0.22	0.35	0.20	0.68	0.46
		Okaroro	NA	0.29	0.30	0.25	0.30	0.09
Tui	< 0.01	Motatau	0.89	1.26	1.45	1.00	1.35	1.54
		Okaroro	NA	0.57	0.94	0.53	0.61	0.79
Significant increase at Okaroro *cf.* Motatau								
Grey warbler	0.01	Motatau	1.68	1.78	1.29	1.28	1.37	1.44
		Okaroro	NA	0.93	1.53	1.49	1.44	1.73
Pheasant	0.04	Motatau	0.01	0.04	0.09	0.22	0.18	0.09
		Okaroro	NA	0.04	0.20	0.45	0.10	0.25
Silvereye	0.01	Motatau	0.57	0.41	0.93	1.57	1.12	0.78
		Okaroro	NA	0.89	1.46	1.49	1.41	1.68
Not significantly different between blocks over time								
Fantail	0.13	Motatau	0.36	0.56	0.48	0.52	0.43	0.41
		Okaroro	NA	0.43	0.72	0.70	0.61	0.93
Kingfisher	0.11	Motatau	0.39	0.56	0.36	0.59	0.39	0.37
		Okaroro	NA	0.82	0.53	0.59	0.46	0.59
Shining cuckoo	0.29	Motatau	0.09	0.00	0.15	0.13	0.44	0.09
		Okaroro	NA	0.07	0.14	0.22	0.05	0.07
Tomtit	0.08	Motatau	0.90	0.85	0.44	0.55	1.00	0.81
		Okaroro	NA	0.14	0.17	0.28	0.41	0.23

In Table 5, we have summarised the author's conclusions from "Table 2" and our best estimate of what the data actually say. To do this end we calculated a more meaningful statistic, the

* We are unable to supply confidence intervals since we do not have the raw data. We have requested it, but it has not yet been forthcoming. It really does not matter much though since the most one could prove with this study design is that there is some difference between control and treated area, which we already knew because of the authors' description of them.

"*Relative Population Growth*"[*] that measures the relative (between treatment and control) populations change as one moves from the pre -treatment to the post-treatment period. Unfortunately, without the raw data we cannot calculate confidence intervals on the Relative Population Growth statistic.

Notice that if one accepts the authors' analysis in "Table 2", predominately non -natives benefited with populations of Chaffinch, Eastern rosella and Myna increasing in addition to the native the Kukapa (also known as the kereru or New Zealand pigeon). Also notice that, according to the authors, two native birds, the **Grey warbler and Silvereye, had population decreases in the poisoned areas,** a result entirely ignored by the authors in the Abstract or Discussion sections of the paper. On the other hand, according to our *Relative Population Growth* statistic shown in Table 5 most native bird populations underwent relative decline, the shining cuckoo being the only one to show apparent important population increase.

? **Table 5 Comparison of the "Table 2" conclusions and those derived from a more rational statistic.**

Bird Species	The Authors' "Table 2" Assessment	Relative Population Growth Statistic **
Chaffinch *	Increase	-33%
Eastern_rosella *	Increase	-91%
Kukupa	Increase	21%
Myna *	Increase	402%
Tui	Increase	-8%
Grey_warbler	Decrease	-55%
Pheasant *	Decrease	-36%
Silvereye	Decrease	-1%
Fantail	No Difference	-50%
Kingfisher	No Difference	13%
Shining_cuckoo	No Difference	78%
Tomtit	No Difference	-50%

Table notes:

* Feral species.

** The Relative Population Growth Statistic is calculated as follows. First calculate the average population numbers in the baseline (before treatment 1996-1997) and in the after poisoning period (1998-2001) for each of the Motatau (poisoned) and Okaroro (control) areas. Then calculate the ratio between Motatau and Okaroro for each of the periods. Then calculate the percent difference between the Motatau and Okaroro ratios. This gives a valid measure of the relative population growth between baseline and treatment periods. A positive value indicates that the population increased more in proportion in the Motatau (treatment) area than it did in the untreated area. It is not clear whether any of these apparent differences were statistically significant.

Thus, this study is deeply flawed structurally and the analysis as done by the authors is misleading at best, and more likely just plain incorrect. Nonetheless, it is instructive to examine how the results were interpreted by the authors and subsequently by others. The authors' conclusion from the paper's Abstract section reads as follows.

[*] The **Relative Population Growth** is defined as follows. First calculate the average population numbers in the baseline (before treatment 1996 -1997) and in the after poisoning period (1998 -2001) for each of the Motatau (poisoned) and Okaroro (control) areas. Then calculate the ratio between Motatau and Okaroro for each of the periods. Then calculate the percent difference between the Motatau and Okaroro ratios. This produces a valid measure of the relative popul ation growth between baseline and treatment periods.

> *After pest control, counts of kukupa and some other bird species increased at Motatau compared with counts in a nearby non-treatment block, suggesting numbers of adult kukupa can be increased in small forest areas by intensive pest control.*

There is no comment about the significantly decrease d (according to author's analysis) populations of Grey warblers and Silvereyes. There is no recognition that "Table 2" indicates that non-native birds seem to be the principle beneficiaries of pest control. These omissions are simply outrageous. Ignori ng the fact that their analysis was deeply flawed, **it is difficult not characterize the omission these results from any comment in the text of the article as deliberate misrepresentation.**

And here is how Powesland et al, cited above, interpreted the result s from the Innes paper.

> *Given the significant increase in kereru populations at several mainland sites following intensive and sustained control of introduced mammalian predators … [Motatau Forest (Innes et al., 2003)…], a similar response could be expected at Whirinaki with an appropriate predator control regime.*

Finally, when the claims of these papers reach the DoC publicity department and the members of DoC's obsequious echo, the Forest and Bird Society, the connection to the reality of the science becomes unrecognizable. For example, DoC has publicly claimed that k ukupas owe their continued existence directly to 1080.

In summary, the Innes et al study reaches a new low in scientific technical quality and biased interpretation. **Not only did the basic study design preclude valid conclusions, but the authors incorrectly analyzed their results and even then cherry picked the answers ignoring their own evidence of damage to at least two native species. Worse, the Powesland and Innes studies together show how one bad study references and misquotes another even worse study so that in the end they become one big self-reinforcing rumour that has no basis in scientific evidence whatsoever.**

Other research cited in the AHB/DoC submission regarding native birds

The submission cites the results of observations during routine aerial 1080 cereal poisonings at various times and places of falcons, fernbirds, harriers, kaka, kakariki, kereru, kiwi, koka ko, mohua, moreporks, weka, and whio (45, 46, 47, 48, 49, 50, 51, 52, 53, 54). Only one (Powesland et al (48) described in detail previously) of the studies had a control. It involved 20 radio tagged kakas with no deaths (95% CI, 0 -17%)*.

The other papers all involved uncontrolled observations.

- Four citations (involving kakariki, kereru, mohua, and blue duck) monitored bird calls or did visual counts. The y all observed "no difference". However, none of these reports included power calculations or confidence intervals.
- Two citations on New Zealand falcons and harrier hawk s reported "sighting s" and "occupation of sites". No effect of aerial 1080 "was noted", whatever that means .

* All of the confidence intervals reported in this section were calculated by us using the binomial distribution..

- Of 75 radio tagged kiwis none died.
- Two of 302 kokakos died 0.7% (95% CI 0.2 - 2.4%).
- Four of 23 fernbirds disappeared 17.4% (95% CI 7.5 - 38.5%).
- One of 40 wekas died after aerial 1080 poisoning 2.5% (95% CI 0.6% - 13.2%).
- Zero of 12 moreporks died after aerial 1080 poisoning 0% (95% CI 0 -26%).

The carrot bait results were similar to the cereal bait studies in quality and results. They did however report substantial ingestion of non-poisoned bait by wekas (10 of 87) and the death of one of 6 moreporks 16.7% (95% CI 4.1-64%)

Thus, ignoring the obvious methodological shortcomings of these reported studies*, they suggest substantial mortality among kokakos, wekas, moreporks and especially fernbirds. The kakas, the only species studied with level 2 controls and a reliable outcome variable experienced zero deaths of 20. For the other species the results are at best inconclusive.

Contrary to DoC's conclusion, to us these poorly done studies suggest a very disturbing mortality among ground feeding native birds from aerial 1080 operations.

Summary and conclusion

In general, even taken without the misrepresentations and erroneous extrapolations, the design and execution of bird studies sponsored by DoC are generally not scientifically passable. There are a number of problems that make them far less than an ideal basis on which to rest a national policy of indiscriminant poisoning of our native forests and its fauna:

- Most are not published in peer-reviewed journals.
- All are funded by DoC, the advocating and financially benefiting government agency.
- None are free from the influence of DoC management.
- None are published in international journals, which are more likely to be free from the highly inbred nature of research on this subject in New Zealand.
- None have been reproduced in diverse circumstances. Indeed, none have been reproduced at all.
- None have randomized controls.
- None have blinded observers, which is wholly inexcusable given the fact that the researchers are dependent on DoC for their jobs
- Most have low numbers of observations, especially relative to their broad conclusions.
- All are confined to two or three geographical areas.
- P-values are often missing from reports, sometimes selectively.
- Power calculations are only rarely presented to support negative conclusions.
- The duration of the studies is short, in fact extremely so given the fact that DoC apparently intends to re-poison every three years *ad infinitum.*

* …including the short term nature of these studies, absence of controls, absence of power calculations, and the insensitive outcome variables

- Even in the absence of randomized controls, there is almost no effort to establish comparability of controls.
- Dozens, if not hundreds, of species of native birds are entirely unstudied.

In spite of these shortcomings, there is substantial evidence of significant mortality among native species (especially tomtits, robins, fernbirds, wekas and moreporks). In many other species, the study quality, scale and data are insufficient to draw mortality conclusions. Furthermore, many species of native birds have not been studied.

In moments of candor, DoC representatives and its hired researchers often claim that the losses from aerial 1080 are "acceptable" because of the net benefit to native birds from aerial 1080 "treatment". However, we have been unable to find scientifically credible evidence of true benefit to so much as a single bird species. Certainly none of the studies cited above provide such evidence. In its absence, many people will judge that any deaths among native birds are unacceptable. We disagree. Our view is that what matters is overall population success of all native species and of the ecosystem as a whole. Here DoC's research bucket is truly empty. The studies simply have not been done.

In spite of the lack of direct evidence showing benefit to native birds from aerial 1080, another assertion frequently heard from DoC is that we are removing the predators and therefore the native birds must benefit[*]. This absolutely does not follow. It is a *non sequitur* that is typical of DoC's simplistic univariate view of ecosystem dynamics. It is quite possible (even likely) that native bird numbers are limited by something other than predation by feral mammals. Their numbers may be limited by food supply, territory, or more likely competition with the numerous species of highly successful feral birds. In addition, eliminating rats, even in the short term, can increase predation on birds by mustelids (118)[†].

The point is the same one made elsewhere in this document. An ecosystem is an immensely complex plexus of interactions. The effects of perturbations cannot be predicted. There are so many dynamic interactions and cybernetic processes that it is impossible to know what the effect of any single change will be. Reliable empirical data are required but they do not exist even for the short term, and much less so for the repeated poisoning that DoC does.

In summary, what is known about the effect of aerial 1080 on native birds can be reduced to a few statements:

1. The research is of poor quality and almost always inconclusive.
2. Native birds of some species are killed in substantial numbers by aerial 1080 poisoning of the forests.
3. There is no credible published evidence of population benefit for any native species of bird[‡].

[*] See for example the Green document (6) on pages 32, 35, and 39, or in the reassessment application (1) page 52.

[†] On page 32 in the Green document (6), he actually states "In fact, when aerial operations are timed to decimate rat and stoat populations as well, then birds benefit, especially endangered species." This is pure myth. There is no evidence to suggest this effect of aerial 1080 on stoat populations, much less the complex interactions between short term rat population decline (known to happen with aerial 1080), native bird population and stoat numbers.

[‡] The often cited nesting success data are not applicable here. Nesting success is a process variable, not a bottom line outcome variable. In addition, the one study that showed improved nesting success for robins failed even to report nesting success in the final year, from which we can conclude either that it was not studied or the results were not reported.

Thus, if the decision of ERMA is to allow DoC to continue its current practices both with regard to operations and research, then that decision will be made on the basis of something other than scientific evidence regarding the effect on native birds.

Invertebrates

There is a large body of research documenting the toxicity of monofluoroacetate to invertebrates (55,56,57,58,59,60). One of the early warnings of the potential toxicity to New Zealand's native forest invertebrates was raised in a review paper by Notman in 1989 (61) in which he commented:

> *"In the light of the evidence of the effect of 1080 on invertebrates, and the complex role that invertebrates play in the ecosystem, the unrestricted use of 1080 is likely to be disruptive to the environment, and where endangered invertebrate species are know to be present, 1080 should be used judiciously, if at all."*

The case regarding invertebrates rests largely on two papers the conclusions and political machinations around which are of considerable importance for ERMA's work (particularly as regards DoC's part of the submission).

Meads

The first field study looking at the effect of aerial 1080 on invertebrates was done in 1994 (62)*. In the main part of this study, multiple control plots and treated plots were used. Thus, it had Level 2 controls. The design and statistics are quite well done and apparently accurately reported. The study showed a strong statistically significant difference between control and treated areas. The net invertebrate population as measured by pit-traps dropped dramatically and significantly in the treated area while the control area underwent an expected seasonal increase. Poisoned areas had 36% and 26% of control area numbers of invertebrates at 2 and 4 weeks (respectively) after aerial 1080 poisoning. Many species were involved, including insects, beetles, bees, ants, butterflies, moths, springtails, flies and spiders. Table 6 from the paper shows the main results:

* The extraordinary history of how this paper failed to be published at first is given elsewhere in this document.

? **Table 6: Total mean number of invertebrates (individuals), and associated standard errors, as estimated from generalized linear model. (Meads 1994).**

	1080 Poison		Control		
Weeks Relative to Treatment	**Mean Count**	**(StdErr)**	**Mean Count**	**(StdErr)**	**P-Value for Difference**
-2	54.7	(11.5)	39.7	(9.8)	ns
0	75.0	(13.5)	67.0	(12.7)	ns
2	45.7	(10.5)	126.4	(17.5)	P < 0.001
4	57.8	(11.8)	229.7	(23.6)	P < 0. 001
6	18.5	(6.7)	25.4	(7.8)	ns
8	25.4	(7.8)	30.4	(8.6)	ns

At weeks 6 and 8 post poisoning, numbers dropped dramatically in the control area. Although unproven, the researchers reasoned that a recent heavy rain had washed monofluoroacetate into the control area which was only 100 metres from the treated area. They also point out that two weeks after the baits were dropped over the study site, 73% were still present following a possum kill exceeding 90%.

In addition, a one year later comparison with two similar forests that were not treated with monofluoroacetate showed dramatically fewer invertebrates in the study ("treated") areas. This study had some difficulties in that some of the poison was washed into the control area and that helicopter avoidance of the control area in the initial 1080 drop resulted in spotty coverage of the poisoned area requiring remedial ground bait distribution to simulate the aerial coverage.[*]

Nonetheless, the results showed a devastating decline in invertebrates that may have persisted for at least a year and if confirmed would have almost certainly mandated the end of aerial 1080 in New Zealand.

DoC's reply to the Meads paper: Spurr 1994

Apparently in response to the Meads study, the Department of Conservation commissioned a similar study to be done by Spurr. In 1994, the results were published by DoC, but not in a peer-reviewed journal (63,64[†]). Traps, baits and general conditions were similar to the Meads study. The author concludes in the paper's abstract (the underlining is ours):

[*] This was done very carefully and scientifically. The author first surveyed the poisoned area to determine average density of administration of 1080 baits that had actually been achieved in other parts of the "treated" area. Then he duplicated that density in the study area. In fact, this may have achieved a more reliable result that could be expected from the vagaries of helicopter administration.

[†] We have found the results of this study published in two places. Once in a Royal Society Proceedings in 1994 and again internally by DoC in 1996. The versions are slightly different, but not in substance. The Proceedings version has graphs in which the wide confidence intervals can clearly be seen.

> *There were <u>no significant differences</u> in the immediately pre- to post-poison (June to July) population trends or in the seasonal fluctuations from March to September (autumn to spring) of amphipods, ants, chafer beetles, leiodid beetles, weevils, other beetles, collembolans, millipedes, mites, slugs and snails, spiders, and cave wets in the poison area compared to the non-poison area.*

Thus, the Spurr study failed to detect the high level of invertebrate mortality demonstrated by Meads. The quality of the Spurr study is difficult to assess because many details of the study are not in the published paper. The results section of the paper consists of a single table showing pre- and post poisoning P-values for various orders of invertebrates. The paper reflects a number of methodological errors and reporting omissions:

- Numbers of observations are not reported.
- The methods of statistical analyses are not well described in this paper, and consequently are very difficult to interpret.
- In the 1994 version of the paper, graphs are shown which display a non-standard form of bars that are apparently similar to confidence intervals, but it is unclear exactly what they mean.*
- Controls areas were substantially different from the treated area, e.g. they had different soil types and were some distance from the treated area, thus raising questions about their comparability to the poisoned area.

Study power

However, the real problem with the study, especially in relation to its conclusions, is its lack of statistical power, which would prevent it from detecting anything less than catastrophic mass die off.

The author notes in the Methods section of the paper that the population changes would have to have been "at least twice <u>[i.e. dropped to less than half]</u> those of the non-poisoned area for the analysis to detect a significant difference (alpha = 0.05, beta = 0.20)". In other words, the study had little chance of detecting population drops of less than 50%. Half of a population is a catastrophe. Thus DoC commissioned a study that would have little chance of detecting as much as, for example, a 30 or 40% death of invertebrates in the treated forest area.

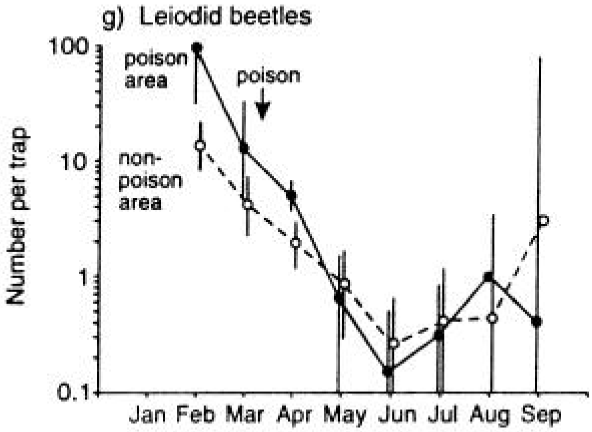
Worse, it is difficult to know how to interpret Spurr's statement of a population change "at least twice those of the non-poisoned area" because the statement is not specific to individual species, the variation among which is huge. If this assertion represents a best case among species as its wording suggests, the statistical power for many species may be worse still. Furthermore, it is never stated and we do not understand how the above power statement was determined given that the primary method of analysis from which the negative conclusions were drawn was ANOVA.

Graphs indicating wide variances and low statistical power

Figure 1, a typical example which is included for a single invertebrate type below, shows what appear to be huge standard errors or confidence intervals and what appear to be large relative changes between poisoned and non-poisoned species. We don't understand these nor do we understand why the researcher used anything other than standard errors or 95% confidence

* The author states only "Vertical lines represent least significant differences". Usually, Least Significant Difference refers to a statistical test that deals with multiple comparisons in ANOVA, it is often called Fisher's LSD, but how it relates to the bars in Spurr's figure is unclear, at least to us.

intervals. Nonetheless, from the author's description we gather that the vertical bars are intended to communicate the same information as confidence intervals. If so the data variation is very wide, in some cases exceeding a full order of magnitude. The graphs are logarithmic, tending to decrease the appearance of variation. Furthermore, if the author had reported relative change from the time of poisoning, it appears that there would have been a statistically significant decrease in Leiodid beetles.



Other issues

The author did not aggregate the invertebrate numbers (as Meads did), which would have increased statistical power, and increas ed probability of detecting differences like those detected by Meads.

Generally ANOVA, the method used by the author to look at "trends" in the difference, is used to compare means within categories. It is not a good way to look at time-based trends, especially ones that are not necessarily monotonic as it is ev ident is the case with the invertebrate data.

Spurr Study Summary

The statistical power of this study was very low, and cannot justify the broad negative conclusion that its author and subsequently DoC have ascribed to it. The method used to analyze the data virtually guaranteed the failure to "detect differences".

Wishing to give the author an opportunity to explain the disparity between his paper's conclusions and its evident lack of statistical power, we contacted him. He responded by email that, "Th e statistical power of the study design was not known beforehand because such studies had not been done previously", which is difficult to understand given that he was certainly aware of the existence of the Meads research. Spurr had cited Meads paper in his own, both Spurr and Meads worked for Landcare Research, Spurr's supervisor at the time, Charles Eason, was one of the internal reviewers of the Meads paper, and Meads had taught Spurr the pit trapping technique that Spurr used.

Furthermore, recognizing the importance of this paper, we have attempted to obtain from Dr. Spurr and Landcare Research a copy of the raw data under the Official Information Act so that we could reanalyze it. After 8 days, we were informed on 17 January by the Director of Landcare Research, Warren Parker, that Dr. Spurr has been unable to locate either electronic or paper copies of the raw data, that we could pay Landcare Research to search for it, and that Dr. Spurr would be unavailable to do that search until after 30 January, too late to support our submission to ERMA. We will continue to attempt to obtain the raw data, but the reanalyzed results are not available for this submission. [We have subsequently been informed that Dr. Spurr is unable to locate a copy, either hard o r electronic, of the raw data.]

DoC's submission to ERMA repeatedly cites the Spurr study to support their contention that aerial 1080 is benign to invertebrates, whereas the Meads study is not cited anywhere in DoC's literature that we can find (65,66).

The politics of 1080 and how the Meads' paper came not to be officially published

The two invertebrate studies discussed above are important because the conclusion regarding the effect of aerial 1080 on a vital part of the forest ecosystem hinges substantially on them. Their results and conclusions are seemingly contradictory and they have not been duplicated by DoC, let alone by an independent party. The circumstances under which Meads' study came to be turned down for publication are of considerable interest since the paper contained such important results. Most of the story that follows is a matter of public record.

The Meads study was done in the latter half of 1991. It was about that time that DoC had received a 3-year government grant of $50 millions that was earmarked for possum control and that they lost if it were not spent. The Meads paper was completed and submitted for internal review at Landcare Research by seven reviewers, all of whom approved the paper. As discussed above, the results showing a profound negative effect of aerial 1080 on invertebrates quickly became known internally at Landcare Research and at DoC. Since the study was sponsored by DoC, it was then submitted to DoC as a finished report. They rejected it; the reasons given were that the study was "flawed" because of manual seeding of the poisoned test area (that we discussed above and that we feel may have improved the study's precision).

None of Meads previous or subsequent research publications has been rejected. This includes over 100 papers, at least 50 of which appeared in peer-reviewed journals. Meads was told by DoC that the paper could not be published, even internally. This was a very unusual restriction, especially for a paper the results of which were of such importance. DoC then, almost immediately, commissioned the Spurr study that was to use a similar approach to that of Meads, except that the control plots were widely separated and had soil types very different from the poisoned plots. Despite years of successful invertebrate research experience and an excellent reputation in the scientific community, Meads was not selected to do the follow up study. Instead, Meads was told to instruct Spurr on the pit trap technique so that Spurr could do the study. After two years, Meads left Landcare Research when he was threatened with transfer out of his group to another geographic location.

The controversy has gone on since. Meads went on to a successful scientific career outside of the government, but continued to criticize the lack of research into the use of aerial 1080 in our forests. Also, he became the subject of DoC's criticism in the press. Richard Sadler, who was Meads' immediate supervisor in 1991 and who was one of the seven originally approving peer reviewers, subsequently became a director at DoC in which role he publicly reneged on his approval of the Meads paper. Typical of the public interchange is this article reproduced from 4 September 1995 Rural News in which Meads defends his research, while stating the case eloquently (and moderately) against indiscriminately dropping large amounts of a broad spectrum poison onto forest ecosystems.

In the end, DoC was largely successful in suppressing the Meads paper (despite the fact that Meads defiantly self-published it in 1994). The current DoC/AHB submission to ERMA for renewal of the 1080 license says (67):

> *… there is no evidence that invertebrate populations are significantly impacted by aerial 1080 pellet applications or that invertebrates are a significant factor in secondary poisoning of other animals.**

Scientist rejects DOC criticism

THE Department of Conservation states (*Rural News*, August 21) it did not publish my scientific paper because of concerns about the manuscript, for which they sought an independent review by another Crown Research Institute. They fail to mention that I was not given the opportunity to take account of their reviewer's comments, as is normal practice in the scientific community.

Prior to the department receiving my paper for publication it had undergone separate reviews by five of my peers, and each time drafts of the manuscript and content were improved in accordance with the reviewer's recommendations.

The department states that my study was compromised by among other things "a failure to account for 1080 drift into the control area." I did fail to account beforehand for a 1080drift from the helicopter over the protected control area. However, the pre, during and post analyses of the results have shown that large numbers of native insect species were killed and these findings have proven to be highly significant in scientific terms.

Further, it should be noted that this is the first acknowledgement from the department that there is a "1080 drift" associated with aerial 1080 operations over New Zealand.

I note also that the department's reply is aimed directly at me and my scientific paper, and in no way challenges the content of the *Rural News* article.

The department defends its priority of protecting native plants and animals, and also the widespread use of 1080, but fails to recognise, when dropping 1080 from the air, that they kill plant and animal communities. Forest litter is plant material that nourishes the forest, and insects and other invertebrates are animals which play a vital role in the health of the forest as well as being food for native birds and bats.

The department's statement that "there is a great deal of accepted scientific evidence to suggest 1080 is one of the best and safest possum control methods available" can only be drawn from research conducted in NZ, as no other country in the world has a need to control possums. There is no scientific evidence that I can locate that suggests aerially-dropped 1080 to be safe.

The use of 1080 has been banned in the UK and US until they have evidence of its safety.

Cost/benefit analysis may show aerial 1080 programmes to be the most cost effective form of possum control in the short-term, but the longer term effect on native plants, animals and their eco-systems will show that the department should have taken a more cautionary approach in the way they spread the poison.

Mike Meads,
Ecological Research Scientist

Conclusion

The scientific truth regarding the effect of aerial 1080 on invertebrate populations cannot be known for certain from the Meads and Spurr papers. Both papers lack randomized controls, blinded and unbiased observers, and contain other design and implementation defects.

* We are appalled by the apparent audacity and mendacity of these claims by the Department of Conservation to ERMA whose job it is to protect our environment.

Despite the apparent difference in conclusions, the data of the two studies are not necessarily irreconcilable. Because of the way the data were analyzed and because of the inherent insensitivity of the Spurr research, Spurr had a low probability of detecting the differences seen by Meads (as Spurr admits in his paper). It is quite possible that Spurr's data, analyzed with more sophisticated and appropriate tools, would show results similar to those of Meads.

However, certain things are clear. DoC suppressed the publication of a paper which would have been devastating to its bureaucratic ends. The research to establish the truth regarding the effect of aerial 1080 on a broad range of native invertebrates simply has not been done despite the existence of strong evidence that it does enormous harm and almost a decade and a half of ongoing large scale aerial 1080 operations.

It is this bottom line fact that is perhaps most disturbing. Instead of doing the unequivocal research that is its legal mandate to protect our native species requires, DoC has attempted to suppress serious and strong indications that it is damaging the very ecosystem that it is paid to protect. It is our view that the allegations of this one section of this document are quite sufficient for ERMA to shutdown DoC's aerial 1080 "operations", and to justify an immediate inquiry into DoC's management practices regarding the promotion of and the use of aerial 1080 in our forest ecosystems[*].

Other invertebrate studies.

In the Submissions section 4.1D on adverse effects on invertebrates, numerous studies documenting observed sub-lethal effects on specific invertebrates are cited. While we have not reviewed these papers, the reported observations certainly do support the contention that invertebrates are damaged by 1080.

We comment here on several invertebrate studies often cited by DoC. Some of these studies have been published in peer-reviewed journals, though not ones of international stature. They are included to highlight design, methodological, analysis and interpretation problems characteristic of much of the research on 1080 impacts.

Sherley

A study often cited by DoC regarding invertebrates is Sherley (68) that looked at the number of invertebrates that fed on poisoned versus non-poisoned baits. Although more invertebrates were found on non-poisoned baits, they could not distinguish between bait aversion and mortality as the cause. Furthermore, even if there was some relative bait aversion, it would by no means prove that it was sufficient to importantly affect the mortality rate. Moreover, the significance of these observations surely does not reach to the claim of the authors that: "*These results are relevant to assessments of the risk to non-target species from primary and secondary poisoning while controlling pest mammals by aerially broadcasting baits laced with 1080.*" This study simply does not constitute evidence regarding the effect of aerial 1080 on invertebrates, one way or the other.

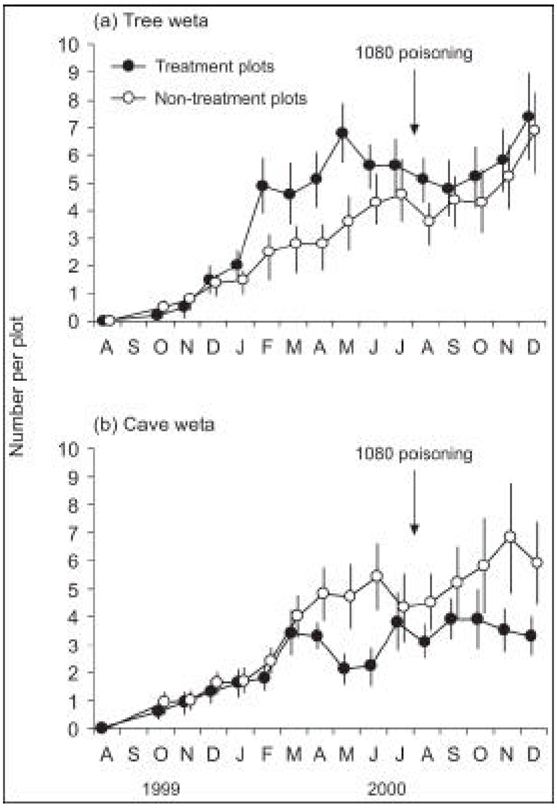
Spurr and Berben

Spurr and Berben (69) studied the effect of hand-laid ground baits on wetas. The baits were washed away by rain 5 weeks after application and not replaced. Not surprisingly, there was

[*] One might ask what ordinary citizens, lacking scientific expertise, are to do under such circumstances when a large well-funded government department with tens of millions of dollars and thousands of employees at its disposal ceases to do their legally mandated duty. Citizens do what they are doing. They appeal on any terms they can, which means that much of what is written by these non-scientist citizens will be emotional and will appear irrational. But as is evident, it is all done in the hope that the responsible scientific oversight agency, ERMA, will do its duty and hold that government department to a higher.

no effect detected on tree wetas as wetas are known to be among the le ast sensitive invertebrates (70). Again, the power of these negative results is not addressed in the paper. The researchers used three outcome variables (*numbers seen on baits*, *numbers in artificial tree refuges*, and *number of live marked wetas found*); all three are of dubious value for obvious reasons.

However, Figure 3 from the paper showed what appears to be a declining trend in number of refuge cave weta after poisoning with aerial 1080 . The months of November and December show what appears to be diverging curves with non -overlapping standard error bars (an indication of significant difference). For clarity we have highlighted this in yellow. A neutral observer would undoubtedly have noted this divergence and tested for a significant difference between poisoned and non-poisoned plots in the later months, since it appears from the graphs to be exactly that.



Despite this, the abstract included the following apparently unsupportable quotation:

> *The results indicate that 1080-poisoning for vertebrate pest control is unlikely to have any negative impact on populations of weta or the other invertebrates monitored.*

It should also be pointed out that this experiment could not duplicate a real 1080 drop. There could be no chaff produced by the bait application. The distribution was not over the same kind of varied terrain including leaf litter. Therefore various modes of secondary poiso ning were not possible. Regardless, this study was essentially a single species experiment , which does not conflict with the Meads results, and properly analyzed may actually confirm them .

Powlesland 2005

Another study (71) focused on tree-dwelling invertebrates by counting numbers in artificial refuges. This study which considered only tree-dwelling invertebrates had insufficient numbers (i.e., statistical power) to have a reasonable chance of detecting a difference between poisoned and un-poisoned plots. The following figure from this paper illustrates the problem perfectly.

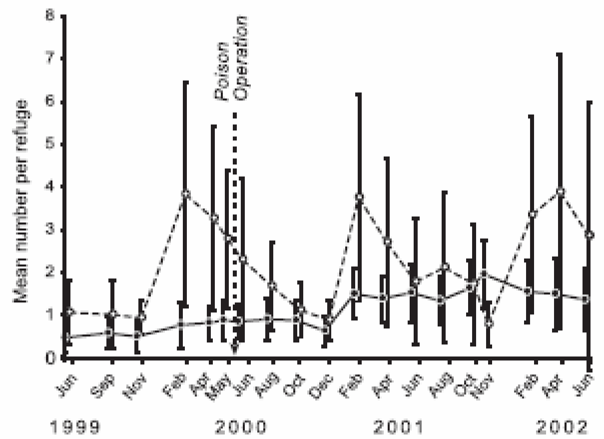


Figure 3. Mean number per refuge (±95% C.I.) of cockroaches in artificial refuges in 1080-poison treated (37 refuges, •) and non-treated (39 refuges, o) study areas, Whirinaki Forest Park, June 1999–June 2002.

Here one can see 95% confidence intervals spanning 600% percent differences (from 1 to 6). Confidence intervals this wide indicate that the failure to find a difference is totally meaningless and should be ignored. Looking at this graph, the authors' conclusion that "1080 operations are unlikely to have a detrimental effect on invertebrates" is not justifiable.

Acknowledging the excessive variation in the data, the authors comment: "*There was an erratic relationship between the numbers of invertebrates found in refuges in the two areas during the pre-poison period so occupancy of refuges in the non-poisoned area could not be compared usefully with occupancy in the poisoned area following the poison operation.*" However, instead of admitting that no conclusion was possible under these circumstances, they then went on to conclude erroneously that 1080 had no detrimental effect on invertebrates*.

An unfinished study to investigate "benefits" to invertebrates

A recently reported study (72) on invertebrates highlights a most unscientific bias on the part of scientists in favour of DoC's "pest" control programmes in the first sentence of its abstract. It states their objective (emphasis is ours): "This paper presents the first results from a 5-year study investigating the ***benefits*** to terrestrial invertebrates of reducing the abundance of mammalian predators in forested sites in Tongariro National Park, New Zealand." Note the use of the word "benefits" in place of the scientifically neutral term "effects". Scientific methodology demands that any experiment/study be open to observations that may contradict those hoped for and that the structure of the study allows for a range of possible outcomes. Investigation of only benefits clearly implies that not only are harms or damages to terrestrial invertebrates not to be investigated but they need not even be reported if encountered. This is counter to the very core of science.

* What was done "erroneously" was to incorrectly conclude that there was no difference, when their experiment would not support that conclusion. Whether 1080 has a detrimental effect is simply not determined by this experiment. It is useless.

We cannot comment further on this study since during this first phase the investigators have only assessed the invertebrate population baseline and determined their trap configuration. No rodent control had yet taken place. Interestingly, it is not stated in this report how the intensive rodent control operation is to be conducted nor is it stated what poison is to be used. This would seem to be a critical methodological omission for any scientific study.

Mammals: bats

In a study on the short-tailed bat (*Mystacina tuberculata*) mortality Lloyd and McQueen (73) estimated that bats exposed for 14 days after an aerial 1080 drop would suffer 14.4% mortality. However, the rate could have been as much as 28.4%, depending on assumptions about bat behaviour. Even though lacking untreated controls, this study is well designed and executed (bats being famously difficult to study). In our view, the authors should be congratulated for the objectivity of their reporting. However, as the authors point out, this study is short term, done in winter, and at a single site. All of these would tend to underestimate the mortality. In addition, this study tells us nothing about population effects over the intermediate or long term due to repeated aerial 1080 "operations". What this study does indicate is that a rare and unique New Zealand mammal is likely being killed in substantial numbers, but we do not know the effect of aerial 1080 on populations especially through the long term.

Sub-lethal effects on birds and the ecosystem

A further issue is that of longer term consequences to bird species of sub-toxic exposure to monofluoroacetate. Several possible scenarios may need to be examined given the multitude of ways that monofluoroacetate can affect the body.

One study that has examined such potential consequences is "Chronic toxicity of 1080 and its implications for conservation management: A New Zealand case study" by Weaver (8)*. It suggests potential population-damaging outcomes of sub-lethal ingestion of the drug by non-target animals including infertility via endocrine disruption, hence producing what the author termed chronic toxicity. The author states that if such a disruption of the hormone system does take place, then even 10% of an average lethal dose for a species may be capable of inflicting infertility on exposed animals, a concentration actually measured in wetas, for example, after exposure to a simulated aerial 1080 drop (74). Of course, this observation would not be relevant in the presence of conclusive evidence that weta populations are not effected, but as indicated above, no such evidence exists, which means such observations become profoundly relevant.

Weaver notes that the United States Environmental Protection Agency (1988) found sub-lethal doses of monofluoroacetate adversely affected the cardiovascular system and reproductive system of rats. Other laboratory experiments have shown negative effects in mink, ferrets, skinks and others.

Ataria et al (75) studied the effect of monofluoroacetate on mallard ducks to determine how sub-lethal doses of the poison influence their physiology. Mallard ducks have an LD50 of about 9 mg/kg. The study used duck controls that received no poison and ducks that were dosed with 8 mg/kg monofluoroacetate. This corresponds to the ducks consuming about 3 to 6 baits (4 g) containing 0.15% monofluoroacetate. Then both poisoned ducks and controls were killed and autopsied at a range of intervals following dosing: 0, 2, 4, 6, 12 and 24 hours. The monofluoroacetate concentrations in the poisoned ducks' blood and heart tissues were found to rise to a peak at about 2 hours but then decreased to a low but still detectable level at 24 h. On

* It is worth commenting that this is the only paper that we were able to find that appeared in an international journal. The author reports to us that the New Zealand Journal of Ecology delayed so long in making a decision on it that he withdrew it and sent it abroad. This is also the only article since 1990 that even indirectly criticizes DoC aerial 1080 policy.

the other hand serum citrate levels rose quickly in the poisoned ducks due to monofluoroacetate poisoning with its interruption of the Kreb's cycle and did not drop significantly even at 24 h. The controls experienced no increase.

Ducks that died within two hours of dosing with monofluoroacetate were found to have necrotic lesions in their skeletal muscle. This may mean that the high energy demands on bird muscle tissue make it more susceptible than mammals to energy metabolism disruption that occurs when the Kreb's Cycle's energy metabolism is interrupted. Haemorrhages were found at 24 hours in the epicardium of all monofluoroacetate-poisoned ducks. Enzyme measurements suggest that muscle damage was local. One of the authors separately found that the serum citrate concentrations rose significantly with monofluoroacetate doses as low as 2 mg/kg (25% of that used in this study). Thus there are detectable changes affecting energy metabolism and persisting at much lower levels of dosing. This and other studies cited in the report indicate that damage may occur to organs at very low monofluoroacetate dose exposures.

The authors conclude from the changes they detected in birds that medium and long term monitoring of populations of non-target species exposed to monofluoroacetate are needed to detect whether there are long term adverse effects. They also emphasize the need to minimize exposure of bird populations to monofluoroacetate[*].

Conclusion: We have not in this section attempted to exhaustively review even the limited literature on the complex and difficult subject of sub-lethal effect, nor is it necessary to do so. The evidence cited is sufficient to show that monofluoroacetate in sub-lethal doses can have profound long and intermediate term negative effects on a variety of organ systems in a variety of animals. Monofluoroacetate poisons the Kreb's cycle that is fundamental to the cellular biology of every oxygen-consuming creature (i.e. all animals). So in the absence of good evidence proving otherwise, a rational person might reasonably conclude that aerial 1080 dropped repeatedly into our forest ecosystems is causing such negative effects in hundreds of non-target species that ingest it (either primarily or secondarily). How could it possibly be true as DoC asserts that aerial 1080 kills one or two species of "pests" and benefits all the thousands of indigenous species? It borders on the absurd.

In short, this state of affairs regarding potential chronic toxicity is utterly deplorable. DoC has not seen fit to investigate the extent to which these may be affecting native species via repeated exposure even though its stated intention is to "treat" our forests with 1080 poisoning every two or three years into the indefinite future. At present, we can only speculate on the long term and chronic effects of these sublethal doses of 1080 on our native species, AND incidentally, ourselves. Lacking evidence to the contrary, to assume that there is no collateral damage and significant chronic effects is irresponsible in the extreme. DoC's lack of concern and hubris matches that of the DDT story and the U.S. dropping of dioxin (agent orange) on Vietnam.

As noted previously, sound research would trump arguments based only on the theory and knowledge of biological mechanisms, but we have no such evidence. The vast majority of native species have never been studied at all. As shown above, the results for the few species that have been studied are short term, equivocal or suggest actual harm and were limited in focus. No research has been conducted that would determine the level of sub-lethal effects being experienced in our native species from exposure to 1080 poisoning, but unless our native species are miraculously different from the species that have been studied for sublethal effects, such detrimental effects are a virtual certainty.

[*] DoC is doing neither of these things.

Evidence that possums are importantly deleterious to native forests

It is important to acknowledge at the outset of this discussion that we do not question that possums can and do affect trees. They are primarily herbivorous and the plants they eat will be affected, as any herbivore will affect the plants on which it feeds. Furthermore, possums have no natural predators in New Zealand, which means that their numbers are essentially only naturally limited by territory and food supply. Thus, there is no doubt that individual plants may be killed. The question that we must ask is the same one that DoC insists on asking when they reluctantly admit that aerial 1080 kills native birds. The question is this. What is the effect on populations? We agree with the population standard in both the effect of 1080 on birds and the effect of possums on plants, and have mostly confined our investigations of benefits and harm to native species to that question.

Thus, the issue that we address in this section is on the quality of scientific evidence relating to the bottom line questions[*]:

- To what degree do possums negatively impact populations of native floral species, especially trees?
- Does aerial 1080 prevent or ameliorate that damage and if so to what extent?

There is a vast literature on the subject of possum damage. Most of it is not directly relevant to these two questions, and consequently we will not discuss it here. In addition, we have mostly confined ourselves to studies that at least involve systematic prospective data collection and Level 1 (or better) controls. We have examined the studies cited by DoC/AHB in their 469 page submission to ERMA, i.e., studies that are alleged to establish the horrors that possums are visiting on our forests and that justify dropping from the air tonnes of a universal poison into them. We have also included two studies not cited by DoC of at least equal quality that do not support DoC's contention. Finally, we have concentrated on the more recent studies, which are more likely to have control plots and include appropriate statistical tests. In short we have attempted to find the most recent, scientifically sound studies in the literature, including those not cited and largely ignored by DoC.

As with other issues addressed in this paper, the quality of the scientific evidence is far from ideal, and the number of studies truly worthy of detailed critical review is few. However, unlike for the question of the effect of aerial 1080 on native fauna, most of the studies cited in this section appeared in peer reviewed journals, although none in a quality international journal.

Four species studied: aerial 1080 associated with worsening of the forest canopy

A 1995 study by Smale et al (76) assessed the impact that reducing possum populations has on the canopy cover. The chosen forests were two catchments consisting of largely unmodified vegetation – mostly rata, kamahi, totara and fuchsia. These were said (without proof) to be suffering from canopy dieback due to possums. Three blocks of forest were studied. Canopy measurements were taken in 1988 prior to intensive possum control with 1080 in two areas (Otira, Deception). Then again in 1993, five years into the possum control program, all blocks were re-measured. Possum populations were estimated to have been reduced by 70%.

[*] The vital, but separate, question of the safest and most cost effective way of controlling possums will be addressed below.

There was a control plot that may not have been very comparable because it consisted of modified forest in poorer condition than that in the trial blocks. By DoC standards, this is a good study, having a simultaneous (Level 2) control.

As the authors note, Table 2 (below reproduced) appears to confirm that the untreated canopy was in somewhat worse condition than the experimental area at the start in 1988 though no statistical test was reported testing that question .

TABLE 2 CHANGES IN CROWN CONDITION (MEAN % FOLIAR CROWN COVER) FOR CANOPY STEMS OF FOUR MAJOR POSSUM-PREFERRED SPECIES IN EACH CATCHMENT BETWEEN 1988 AND 1993.

	OTIRA 1988		OTIRA 1993	DECEPTION 1988		DECEPTION 1993	TARAMAKAU 1988		TARAMAKAU 1993	AREA X TIME INTERACTION
Hall's totara (n = 295)	56	**	46	64	**	53	44		45	**
Southern rata (n = 371)	77	**	70	76	**	67	63	**	58	
Kamahi (n = 683)	81		73	83	**	67	77	*	74	*
Fuchsia (n = 90)	76		69	76	**	63	(69		66)	
n		454			556			429		

*Notes: Within each catchment, significant changes in crown condition for each species are indicated (blank = not significant; * = P<0.05; ** = P<0.01; brackets indicate insufficient sample size). Significant area x time interactions indicate different rates of change between catchments.*

Table 2 also shows that the canopy actually got worse for some species in all three areas. However, it appears that the canopy decreased much more in the possum controlled area of Otira and Deception than it did in the control (not -poisoned) area of Taramakau. The authors did not report a statistical test on the hypothesis of whether the canopy declines in the possum controlled areas were greater than that in the control (unpoisoned) area.

This relatively long duration study was undertaken with the expectation that canopy dieback would be clearly reduced after 5 years by the ongoing possum control program. It was not. To explain this conclusion of the data which was not reconcilable with their prejudices, the authors point out several problems with the study. The control plot was not comparable. Different techniques were used to measure the pre - and post-poisoning canopy cover for which they tried to adjust. Their assessment of possum control leve ls was based on pellet frequency adjusted using the control block possum pellet frequency change.

Of course the study also has many of the usual defects that we have become accustomed to seeing in DoC research: lack of randomization, small number of stud y subjects (i.e., three plots and wrong unit of analysis), incomparable controls, non-blinded assessors, not published in a peer reviewed journal, etc.

Despite the authors' bias, even with the study's flaws, the authors conclude,

> *… there has not yet been any detectable improvement in crown condition for four major possum-preferred tree species. Instead, most show evidence of significant deterioration. If taken literally, these declines indicate no beneficial effect from possum control after 5 years.*

In essence, 5 years of possum control had not lessened canopy dieback . The authors did not mention the fact obvious from Table 2 : that declines were <u>worse</u> in the possum control areas than in the not-poisoned area. Instead they went on to provide another page of potential explanations as to why things had not come out as expected. For example,

> *Because of a short history of monitoring canopy recovery after possum control, it is difficult to extrapolate results from a single study such as this to other areas.*

This is almost certainly true, but one wonders whether that statement would have been in the last paragraph of the Conclusion section of this paper had the results been consonant with the authors' expectations*.

DoC/AHB has not cited this study in their re assessment application, despite the fact that its scientific quality exceeds many that they have cited . We point this out as yet another example of biased omission by DoC in the Reassessment application.

The Payton et al (1988)

A study conducted in 1988 (77) investigated the relationship between canopy dieback and possum browsing on rata and kamahi stands. Th is observational uncontrolled study considered only process, not outcome. Trees in nine stands were studied, measuring the relationship of forest structure to dieback. The progression of foliage loss was assessed. The authors concluded that once the canopy cover was opened, the exposed leaf bunches deteriorated. Stands consisting of young trees recovered by closing the canopy cover, but mature stan ds continued to deteriorate due to wind, fungi, insects and age without further browsing. Thus, for these "possum-favoured" tree species, possum browsing generates foliar damage, but not necessarily dieback. Dieback can result if the canopy cannot close.

Bellingham et al

In 1999 a study report (78) examined five conifer/broad -leaved rainforests that had monitoring records covering a period of between 14 and 25 years and that had received catchment –wide possum control at different times and with differen t frequencies. The researchers selected (sometimes randomly) representative plots to study in five different forests on the North and South Islands. Most had undergone possum control at various times. The outcome variables were changes in the biomass, species, structure of the forests over time, and the history and death of seven canopy species palatable to possums to examine correlations with possum presence and control. In spite of varying maturity of these forests, no important changes were detected in species composition (though power calculations and P -values are absent). Dieback occurred at different times and in limited areas, frequently progressing over decades. Regeneration of former canopy species occurred unpredictably. Although different factors can contribute to forest dieback, they conclude <u>that possum control had "little apparent effect in arresting the declines of some palatable tree species."</u>

One species, Hall's totara, did decline notably and consistently across the time span and catchments, without correlation to the presence of possums since <u>"the basal area and live stem biomass of other tree species palatable to possums remained unchanged during census periods."</u> The presence in the catchments of some other studied species remained fairly stable across the entire time span of the study. The biomass of the forests remained stable and there was little species composition change. Stem density increased over time, reflecting the replacement of some canopy trees with shorter species.

This study fails to demonstrate overwhelming damage to native forests from possums and it fails to demonstrate the alleged value of possum control (by aerial 1080 or other means) to native flora. It does show that New Zealand's forests are highly dynamic ec osystems in which

* In fairness to the authors, it is always a sev ere strain on an investigator's objectivity when results turn out different than from expected. It is very difficult to switch roles from trying to prove something to accepting what the data are saying. On one occasion in my own career (QEWOK), just such a situation resulted in my losing a colleague and close friend of 20 years.

populations densities and compositions can vary dramatically over time unpredictably and without correlation with possum control.

The controls in this study are naturally occurring, which makes this study what is sometimes called a "natural experiment". As such it looked for correlations that have occurred in the course of normal DoC operations, and thus, should be considered Control Level 1. It fails to make good use of statistics, which means that conclusions cannot be considered quantitative. However, it is the most broad-based study that we have seen and its selection methodology is quite good. The paper has only been published by DoC* and not elsewhere. The conclusions of the authors correspond better to the actual implications of their data than is typical of DoC-sponsored research that we have examined. There is little indication that the authors were influenced by pre-held opinions.

Noting this latter point and the importance of the study's conclusions, we checked to see if the DoC/AHB submission document to ERMA had cited it. It did not. In order to determine how the paper has been interpreted by others, we also searched the literature for citations. Only one citation seemed relevant to the subject of this document. It was by Cowan in 2001:

> *While the detrimental impacts of possums on native plants and animals are now well recognized, the complex interactions of factors influencing the nature, extent and consequences of their damage at community and ecosystem levels are only now becoming clear (Bellingham et al. 1999; Payton 2000; Sadleir 2000) (79).*

Mistletoe

Sweetapple et al in 2002 (80) looked at mistletoe re-growth two years after 1080 possum control in a forest that had been inhabited by possums for decades. The study lacked controls, even historical controls. It examined "browse" rates on mistletoe relative to possum trap-catch rate, but failed to calculate the correlation coefficient or its confidence interval. Graphs are displayed without point confidence levels. Nonetheless, the study documents the profound predilection possums apparently have for mistletoe. The study suggests, but does not prove that mistletoe will suffer even when very low numbers of possums are present.

Conclusion: Possums aggressively eat mistletoe, but the case relating possum control and numbers to mistletoe population survival is not established by this study.

Nugent et al (2002)

Nugent et al (2002, 81) compared a control (not-poisoned) area, Okaroro, with a poisoned area, Motatau, using a before-possum-control baseline in 1997 and an after-possum-control evaluation in 1999. It is a Control Level 2 study, for which the authors' principle conclusion is that species recovered rapidly after the "possum control" in the study area. The principle results of the paper are summarized in Figure 1 from the paper:

* A DoC-sponsored study's absence from the peer reviewed literature can mean that it was either deemed not of sufficient quality or that DoC did not wish it published. Our view of the quality of this paper is that it is equal in quality to many DoC-sponsored papers published in reviewed journals.

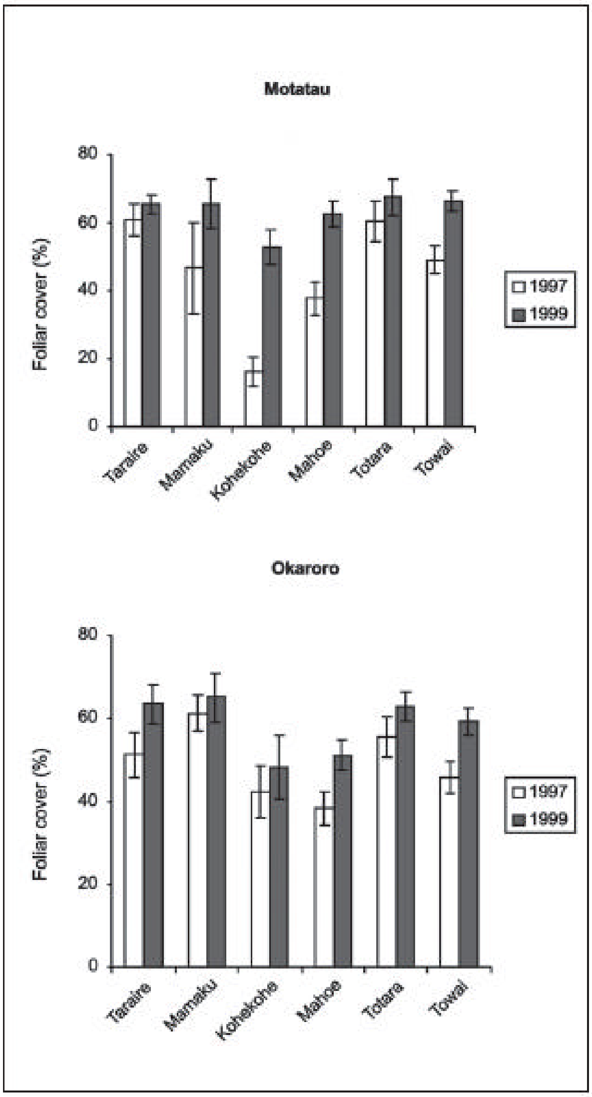


Figure 1. Foliar Cover (mean ± 95% confidence interval) scores for six plant species in 1997 and 1999 at Motatau and Okaroro.

Several observations can be gathered from this figure:

- Control and poisoned areas were clearly not comparable at the outset; see the differences between the 1997 values for different spec ies, especially the one, kohekohe, of which the authors make so much in the abstract and conclusions sections of the paper.
- Both not-poisoned (Okaroro) and poisoned (Motatau) areas tended to show improvement from 1997 to 1999.
- Indeed, the species Mahoe, Taraire, and Towai appear to have improved to a statistically significant degree in the not-poisoned (Okaroro) area.*

* As judged by the non-overlapping 95% confidence intervals. Of course, without the raw data, we are not able to actually calculate P-values, but when 95% confidence intervals do not overlap, a statically difference is highly likely to be confirmed by appropriate statistical tests.

- Even though the species Taraire appears to have improved significantly in the control area (Okaroro)*, it did not do so in the poisoned area (Motatau).
- All species monitored in both areas ended up with very similar foliar cover by 1999 despite possum control only in Motatau.
- This is also true for the species emphasized by the authors in the abstract, kohekohe. The differential improvement in this species appears to have been due in part to the obvious lack of comparability between control and treatment areas at the outset of the study in 1997.
- The results for Towai appear almost identical in possum-controlled and not-controlled areas.

Contrast this with the conclusions of the authors. It is worth quoting the actual words in the paper's abstract:

> ***Abstract:*** *We document the rapid recovery of kohekohe (Dysoxylum spectabile) canopy cover following the control of brushtail possums (Trichosurus vulpecula) in Motatau Forest, Northland, New Zealand.*

and:

> *Mean canopy cover scores for kohekohe in Motatau increased from 16.1 ± 4.5 % in 1997 to 52.6 ± 5.2% in 1999, but increased far less at Okaroro, from 42.3 ± 6.3% to 48.0 ± 7.75%. Changes of a similar nature, but of a much smaller scale, were recorded for four of the five other species monitored.*

In addition, the authors repeatedly confuse the dubious evidence of association demonstrated by this study between possum treatment and canopy improvement with evidence of causation. For example, in the abstract they claim,

> *…further confirming that at least part of the observed increase in canopy cover was a response to the removal of possums.*

As frequently observed elsewhere in this document, in the absence of randomization of control areas one simply cannot draw a causal conclusion like the one above.

Conclusion:

It is possible that possum control may help the canopy foliage of the six species studied, but this study certainly does not prove that. It presents at best weak evidence of the effect of possum control on canopy foliage that can easily be interpreted to suggest just the opposite. What is of interest here is the contrast between the authors' conclusions and assertions about its meaning and what the data actually show. Almost bizarrely, however, in their introduction the authors pen a statement that nearly matches our own assessment of the state of the evidence on possum damage to forests:

* As judged by non-overlapping 95% confidence interval in the one case and not in the other.

> *Despite an immense effort over the last 50 years to protect forest canopies from browsing by brushtail possums (Trichosurus vulpecula), there are surprisingly few published examples of positive and substantial responses in canopy cover after possum control.*

Thus, given the inferable bias of the researchers and the methodological flaws of this study , the only thing one can say for certain from this study is that the researchers believe possum control helps the forest canopy.

Sweetapple et al (2004)

This study (82) compares forest canopy, browse evidence, tree dieback and bird count numbers in three forests that the researchers estimate (without stating how) to represent 10-, 20- and 30-years of possum infestation. Several species of birds and trees were examined. The authors do not report whether any form of pos sum control had been carried out in the areas. They conclude:

> *Canopy condition of common possum-preferred trees was scored progressively lower in areas with increasing length of possum occupation, especially at the site where the possum population had apparently declined from its maximum density. Native forest bird abundance also declined with increasing length of possum occupation.*

Structurally, this study should be classified as a variant of Level l controls. The conclusions entirely depend on the assump tions that

1. the forests were very similar prior to “possum invasion”,
2. the possum invasion happened when it is alleged to have happened, and
3. nothing else could account for the observed differences.

None of these can be verified. Given this lack of cont rols, strictly speaking, this study, and others like it, should only be used as a guide to the design of a proper study with randomized controls. The inclusion of a truly comparable area that had not been invaded by possums would raise the Control Level to 2, which would strengthen the conclusions somewhat, but that was not done.

Perhaps the weakest part of the study is that the degree of damage , as measured by foliage cover, did not correlate at all with possum numbers, the worst damage occurring in the 3 0 year area when possum numbers were dramatically reduced compared to the 20-year area. The authors, always faithful to their bias at the outset even when it is contradicted by their own data, attribute this apparent contradiction to the declining food so urce for the possums. The problem with that conclusion is also in the authors own data, which show that the possum numbers in the 30-year ares (roughly 25-40% of those in the 20-year level) are out of proportion with the changes in t he trees. In fact, t he tree foliage cover numbers do not show uniform decline at all. The foliar cover of fuchia and totara in the 30-year data are down by roughly 60 and 70% of the 20-year data, but that of pate, kamahi, and mahoe are the same and rata and haumararoa appear to be the same for both the 20-year and the 30-year areas*.

* The relevant P-values were not reported and lacking the raw data we could not do them ourselves, so we cannot be certain. However, most of this is likely from the bar graphs and the P -values that are reported.

The bird data are wholly inconclusive, some species appear to be up, some down, some unchanged, and some are up then down. *A fortiori*, the authors' conclusion that "native forest bird abundance" declined is not justified, and borders on outright misrepresentation (see Figure 6 from the paper, reproduced below)*.

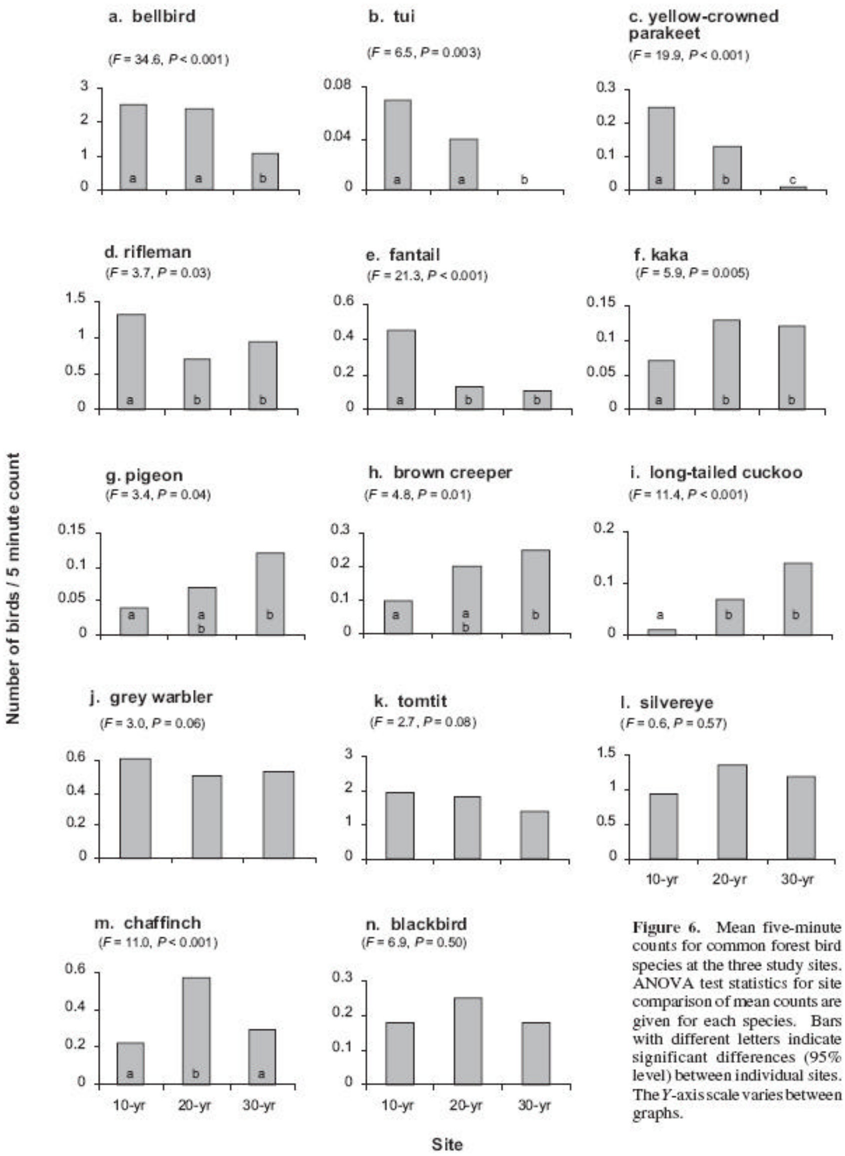


Figure 6. Mean five-minute counts for common forest bird species at the three study sites. ANOVA test statistics for site comparison of mean counts are given for each species. Bars with different letters indicate significant differences (95% level) between individual sites. The *Y*-axis scale varies between graphs.

Fuchsia study (2005)

The most recent study that we have been able to find that addresses the question of the net effect of possums on flora populations confines itself to a single species of plant, a 2005 DoC-funded and executed Control Level 2 study (83) purporting to show the benefit of aerial monofluoroacetate in preventing loss of a single tree species, *Fuchsia excorticate.* Among other things the study claims about a 20% improvement in stem loss comparing

* Incidentally, the analysis of variance is the incorrect statistical test to have used in this case, where the trend over time is the relevant scientific question, not just whether the means were different between groups regardless of their order. This comment also applies to the *foliage cover* outcome variable discussed before.

monofluoroacetate-poisoned areas to a control area. A review of the published manuscript turned up the following eleven methodological defects.

- There was only one untreated area. (Thus valid statistical conclusions are not possible.)
- P-values were not reported for overall change in any of the three outcome variables.
- Control and treated areas were not randomly selected.
- The control plot initially had a very different stem size dis tribution from any of the study areas, suggesting the control and study areas were not comparable.
- There was no ground-baited control.
- The control area results are dominated by a single plot (of 9) that did extremely poorly. If that plot (KP2) were eliminated most of the observed differences would disappear.
- The untreated area had a much higher proportion of small stems in the before period than did the treated areas, further suggesting at the least that the control area was not comparable to the treated a reas and possibly accounting for much of the alleged differences.
- The researchers admit that the "foliar cover" outcome variable can be error-prone and subjective assessments were not done by a consistent group of people, and the assessors were not blind as to plot status. (A subjective outcome variable without assessor blinding is more or less a guarantee for biased results.)
- In fact, <u>none</u> of the outcome variables were measured by assessors who were blind as to plot status.
- The unit of analysis was inc orrect. It should have been areas of coverage, not trees or stems (84).
- The study was funded by DoC, a n unquestioned advocate.

These study flaws guarantee that the results will reflect the biases of the researchers, which are readily apparent from reading the paper itself. Any one of these flaws could account for the observed differences, but taken as a whole one can hardly consider the case of benefit even for the single species, Fuchsia, as proven. Yet this study is among the better that we have been able to find on the subject of the effect of aerial 1080 on forest canopy .

Furthermore, "Stem loss" as an indicator variable is by no means bottom line. It is a process variable that if increased would only show that possums eat plants, which we already kn ow. The "foliar cover" outcome variable is more to the point, but there was little difference between treated and untreated areas in foliar cover, two of the treated areas declining statistically to the same degree of certainty ($p < 0.001$) as the one untre ated area (Figure 3), which hardly implies the benefit claimed by the authors in the paper's abstract, "Mean foliar cover showed a greater decline (42%) in the untreated area than in the treated areas (range 0 to 26%)". <u>As usual this statement is unsupported by of P-values or confidence intervals either in the abstract or the body of the paper.</u>

We are not suggesting that possums do not impact Fuchsia. Indeed Fuchsia may be one of the favourite foods of the brushtail possum. Of course possums eat vegetati on (they are omnivores). If this paper were not tainted by inadequate controls, lack of randomization, lack of assessor blinding, and researcher bias, it would constitute substantial evidence that possum control was associated with a slightly improved fol iar cover in the four areas studied.

The Green Report (2003)

At the bottom of the hierarchy of scientific value is a 135 page report written by Green (85) entitled, apparently with unembarrassed advocacy, *Benefits to Forests of 1080 Operations.* We have requested this document from DoC and were assured that it would but it has failed to arrive in three weeks. The document is apparently a compilation of field “observations”. We mention it here because it is cited by the submission (1) (e.g., on pages 281 and 303), so we describe in brief what we know of it. The report was done retrospectively by a contractor who also authored DoC’s other advocacy document: *The use of 1080 for pest control: A discussion document* (25). He had no primary contact with the original observations which were also retrospective and *ad hoc*. There are no controls whatsoever and, of course, the observers were not blinded. At best this is a management report, and at worst hired propaganda. It is not scientific evidence.

Other studies

We mention two studies cited in the submission that warrant comment.

Cowan et al (**86**) in 1997 published the results of a study of the effect of possum browsing on 24 rata trees between 1970-74 and 1990-94. This is not a population study , there were no controls, only a single area was examined (which may not generalize well to the rest of New Zealand), and the small sample is very small. Because of these and other structural weaknesses, we did not review this study in detail. However, t he authors’ conclusion that possums are the main cause of decline of northern rata simply is not justified by this study (again confusion between possible association and causation) . Furthermore, this study tell s us nothing about whether possum “control”, by aerial 1080 or other means will ameliorate the alleged “decline of the northern rata”.

Payton et al (**87**) in 1997 reported on a possum control study in which aerial 1080 poisoning followed by trapping reduced the possum population in Waipoua Forest to a 7 -9% trap catch level. They noted no short term improvement but rather a cessation of deterioration and a reduction in stem damage and grazed foliage. Again there was no control. Since we were unable to obtain a copy of this study for in -depth review prior to our submission, our comments are based on the abstract only.

“Pests” other than possums

DoC’s apparent goal, though not explicitly stated in the submission, is to obtain ERMA’s permission to control a wide variety of “pests”, not just possums, with aerial 1080. Certainly their literature contains many references to rats and mustelids as being undesirables .

At no time does DoC define what functionally constitutes a “pest” that they intend to “control”. Looking for consistency in DoC behavio ur one might hypothesize that all feral species are to be considered “pests”. This at least would be consistent with DoC’s often proffered “ecological restoration” goal. However, there are dozens, or even hundreds, of feral species that seriously compete with or kill natives while not yet being promoted to the DoC hit -list, e.g., mynas, sparrows, starlings, magpies, pheasants, etc.

Why are we not carrying out a compulsive, expensive, and environmentally risky effo rt to “get rid” of myna birds or pheasants? They undoubtedly compete with and therefore limit the native populations. The ring-necked pheasant occupies a niche almost identical to the pukeko and weka. Thus, we observe that being an exotic species does not necessarily qualify it as a pest according to DoC’s criteria since the vast majority of exotic species are not on either their control or exterminate list. On the other hand some native species are treated by DoC as pest s when it suits their aims. For example on the Coromandel Peninsula pukekos have been killed by DoC in order to protect the brown teals that they have released there.

Rats: one of the most commonly mentioned non-possum targets is the rat. We will just point out here that trying to control rats in a forest by administering 1080 or any other poison every 2 to 5 years is a significant waste of time and money (29). Rats have such capacity to breed that their populations quickly recovery in a much shorter time than any of DoC's poison cycles. In addition, as we have shown elsewhere, rats (though not the same species) have been a part of New Zealand's ecosystems for 800 years, and they are almost certainly an integral and important part of it now.

Finally, must elids (especially stoats) are sometimes mentioned in DoC published documents in the context of aerial 1080 "pest" control (6). Here it is implied that control of stoats will somehow result from aerial drops of 1080 along with the myriad of other benefits they cite. This implication is entirely without justification. In fact, we have found evidence of stoat numbers increasing following aerial 1080 poisonings (48) but also evidence that they can experience secondary poisoning by eating poisoned rats, possums or birds (88). There is also evidence that reducing rat and possum populations by poisoning leads them to switch prey to native birds and bird eggs (118). Thus populations could go up or down or stay the same.

Conclusion

Our conclusion is quite simple. The answers to the two critical questions regarding aerial 1080 possum control are not known. We do not know the degree to which possums negatively impact populations of native floral species, and we do not know if aerial 1080 ameliorates that damage. Furthermore we do not know the quantitative impact or effectiveness of other less risky methods of possum control. Possums undoubtedly "prey" upon native forests, but the net effect of that predation, the degree to which it can and should be reversed is far from clear. As can be seen from the evidence review in this section, even the existing flawed and biased studies present a confused and inconsistent picture.

Therefore, it is impossible to make a rational decision about whether the *a priori* as well as the empirically proven risks of aerial 1080 are justified by the benefits. The implication of this is the absolute need for high quality, multi-site studies with randomized controls, blind assessors, bottom-line outcome variables, and most importantly DoC independence. And yet not one study even remotely approaching this standard has been done.

The Department of Conservation: guardian of the environment or typical bureaucracy?

DoC is a bureaucracy. That it happens to have the word "conservation" in its name does not make it immune from the typical behaviour of bureaucracies. There is a considerable body of literature and substantial agreement on the nature typical bureaucracy behaviour (see for example the classic paper from the 1950's of VP Roberts (89)). First and foremost bureaucracies have a penchant for budget maximization (90) which CP Schmidt described to perfection in a report to the US Army:

> *While agreeing that bureaucrats hold a variety of personal goals, each of these goals is attainable through increasing the agency's discretionary budget. Thus, it is in the bureaucrat's self-interest to work toward budget maximization. It is assumed that by doing so the bureaucrat will be able to attain a variety of subsidiary goals, such as increasing salary, perquisites, reputation, power, patronage, productivity, convenience, and ease of management.*

Unfortunately, virtually all scientific information concerning monoflu oroacetate is controlled and generated (directly or indirectly) by DoC, the bureaucracy that benefits from the growth of what may very well be called New Zealand's "possum control industrial complex".

It is difficult to determine exactly how much DoC's budget has grown because of the aerial 1080 (and other) possum control activities, but it is substantial, estimates range from $30 millions to $50 millions (91). Importantly, much of the pest control spending is discretionary (the kind most prized by burea ucrats).

Add this to the ardor of DoC's advocacy which includes the numerous examples of misrepresentation and distortion that we have documented in this paper, and it becomes clear that DoC simply cannot be assumed to be a neutral broker of conservation strategy[*]. In the case of aerial 1080, it is quite possible that DoC's legislative mandate as conserv ator of the environment has been pushed aside by its baser bureaucratic imperative of maximizing its budget. In this respect, DoC 's pest control activit ies are like those of Eisenhower's military industrial complex, it is essential to keep the enemy out there and never win the war since otherwise the money disappears[†].

Aerial 1080 and the control of bovine tuberculosis (TB)

One of the two primary motivati ons for the use of aerial 1080 in New Zealand is the control of possums which are believed to be a bovine tuberculosis vector and primary host of the disease for cattle. Control of the brushtail possum is deemed necessary to achieve official f reedom from Tb for New Zealand's cattle and deer herds by 2015.

We have not found in their submission where AHB makes its case that possums are a major source of bovine TB in New Zealand. Nonetheless, a brief review of the literature reveals the state of the evidence. Most of this we do not dispute and so will not reference it.

- There is an association between the prevalence of TB in cattle and its prevalence in possums living at pasture margins. The problem is that the conventional measures (testing and slaughter) t o control bovine TB have been undertaken with similar intensity at the same time that possum control measure s have been undertaken. Thus, it is difficult to be certain which is cause and which is effect.

- There is considerable evidence from the United Stat es and other countries that wild animals, such as deer and badger , are vectors for bovine TB.

- The case that there is one or more non-bovine vectors is suggested by the fact that conventional measures (roughly since 1970) have not been as successful in New Zealand as in other places[‡], such as the United States, Australia, Britain, and Western Europe. Of course, this presupposes that AHB efforts have been at least comparable to those of other countries.

- Cattle have been seen nosing and licking apparently dead or dying possums in pastures.

There are holes in the evidence however. For example, cattle were infected with bovine TB in New Zealand long before possums were known to be infected by bovine TB (6). There are

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[*] As one might reasonably argue that the Parliamentary Commission for the Environment has functioned. See below.

[†] Thus, despite hundreds of millions of dollars spent on aerial 1080 and possum control in the last decade and a half, accor ding to DoC, the number of the enemy has not changed. It was 70 million in 1994 and it 70 million now.

[‡] We have not verified this statement, but for the purposes of this paper we accept it.

carefully investigated cases in which infected possums could not be found to account for new outbreaks in previously uninfected herds (95), suggesting that there may be at least one other vector. Also, there are other known candidates that also contract bovine TB, namely deer, mustelids, and even sheep.

What the evidence should be

In the U.S.A., an unexpected outbreak of bovine tuberculosis occurred in Michigan (92). Investigation led to the discovery of the white tailed deer as a wild vector for the disease. Research was then conducted to discover how infected deer spread the disease to cattle (30). It showed that an artificial feeding program was the major factor. This feeding program was then discontinued and the outbreak was contained.

No such research that we could find has ever been done in New Zealand in relation to the assertion that possums are the cause (or even a cause) of sporadic outbreaks of bovine TB in New Zealand. Nor have we found evidence that AHB has ever conclusively shown that possums can infect cattle, even though it would require only a fairly simple experiment to do so. Their evidence is circumstantial association and theory (93) without conclusive causation. So at the least we are fighting a battle with incomplete knowledge of the possum enemy and its means of operation.

Possums are a vector of bovine TB

Regardless of all this, the preponderance of evidence (though not conclusive or complete) indicates that the brushtail possum is an important vector for and reservoir of bovine TB that may at least in part be responsible for the persistence of bovine TB in New Zealand's cattle population despite what should have been adequate measures to control it. Thus, the question becomes what is the best way to control the TB-infected possum population in potential proximity to cattle.

Pasture margins are the place to work

The reason that aerial 1080 must be used by AHB, it is argued, is to reach possum populations in rugged and inaccessible areas where it would otherwise be extremely expensive or impossible to carry out. There is not conclusive evidence in the literature that bovine Tb exists to any extent in these remote locations. If bovine Tb is found in these inaccessible regions, is there any evidence that eliminating them will have a high probability of reducing the incidence and spread of bovine tuberculosis among cattle or deer herds?

One study (94) examined the prevalence and epidemiology of brushtail possums in the vicinity of forest pasture margins in the Hohonu Range in New Zealand. Over one year, 1467 possums were trapped or poisoned and an additional 334 were shot in the forest pasture margins and/or the adjacent forest. Of the animals 141 had macroscopic lesions. The farthest an infected possum was from the pasture boundary was just over four (4) km. The infected animals occurred in groups of 2 to 5. The study concluded that the highest populations of possums ranged in the forest pasture margins and that the cattle that were infected also tended to be located there.

Another study (95) in the Featherston region studied the mechanism for transmission of tuberculosis from infected possum to cattle. Here too the greatest population of possums was at the forest pasture margins with little evidence of migration either into or out of the area. In this instance, tuberculosis infection persisted at a low level among the cattle even after it was no longer detectible in the possum population. This was presumed to be because of intensive poisoning campaigns prior to this study. A Massey University Doctoral Thesis (96) examining the role of vector pest species in the epidemiology of tuberculosis in cattle likewise found that the possums (both infected and tuberculosis clear) tended to cluster at the forest pasture margin where they presumably had good access to desirable feed.

How big a pasture margin buffer?

These and other results indicate strongly that the focus of possum control should be at the forest pasture margins, especially at locations where possums are known to be concentrated, with supplemental coverage of a buffer zone outside these margins. This would target the biggest reservoirs of infected possums and would control migration in to or out of the region. This was confirmed by a 1999 study conducted by Landcare Research for AHB (97) entitled "How deep into the forest should possums and deer be controlled to manage bovine Tb?" They aerially baited 1080 across three different size buffer zones at the pasture/forest margins: 1, 3 and 7 km wide and followed it y early with ground baiting. They found after 4 years that Tb among the possums in the 3 and 7 km buffer zones has remained below 1%. Furthermore, the recovery of the possum populations in those two zones has been modest (25% and 10% respectively of the previous populations). The bovine Tb in cattle adjacent to these buffers declined following the initiation of the study and reached its lowest point at the time of the report. Although more infected cattle remained in the pasture adjacent to the 3 km buffer, the authors questioned whether it would be economical to extend the buffer as far as 7 km for the small detected benefit.

We observe that such pasture margin control should not <u>require</u> aerial application since these margins are not inaccessible*. Aerial drops are particularly inadvisable close to cattle paddocks because of the risk to the cattle herds, working dogs and humans. At the same time, it would seem likely that focusing energy on these locales is likely to have more immediate results and to be more cost effective than blanketing vast reaches of deep forest country with poison pellets that have a high probability of resulting in collateral damage to the ecosystem. Furthermore, we have seen no evidence that these remote possum populations are contributing to the herd infection problem. We acknowledge that AHB does carry out active possum control at the pasture margins but continuing and intensifying this effort could be more likely to bring about the desired control over the cattle bovine Tb problem than the broader blanket approach.

Given the above, DoC's preferred approach of using <u>aerial</u> 1080 drops on DoC land and reserves adjacent to pasture land is ill-conceived. This can be seen in one of the above discussed studies investigating why bovine tuberculosis continues in the Featherston region (95). The report states there to be strong local resistance on the part of large farm owners, life style block owners and pig hunters to DoC and AHB's possum control programmes, and it avers: "While gaining access to private farmland is likely to remain the driving influence of the success of on-farm pest control, and will be improved only by ongoing public education programmes on the need for local possum control, public liaison, and the development of local 'working' groups, there appears to be a very strong case for more frequent use of aerial baiting for possum control on Crown and NZFT lands." (14)†

Summary

Based on the AHB research discussed above, not only would forest pasture margin possum control have the near-term effect of reducing bovine Tb among infected herds (in addition to culling infected cattle), but, by concentrating efforts here, the incidence of bovine Tb among

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* Although AHB's current policy appears to be to only use aerial 1080 in hard to get to areas where it is necessary because of "adverse public reaction", however, they still do it (95,102). Most AHB operations are ground based.

† The attitude this quotation seems to evince is that if farmers won't let us do what we want and deem best, then we will drop aerial 1080 next to their land. This attitude and behaviour on the part of DoC is exactly what has enraged so many people. It is often patronizing and condescending while obviously being environmentally unsound. We believe that farmers and local landowners are not in need of education by the all knowing and always correct Department of Conservation. In our experience those farmers who are treated by DoC with such hubris and condescension often have correctly assessed marginal, poorly considered DoC programmes as not founded in either good science or good sense.

the possum population would likewise be reduced since it is known to be concentrated in these locations. Thus intensive ground baiting along these margins will progressively reduce the possum population and the prevalence of Tb in the resident possums .

Alternatives to Aerial 1080

The argument for D oC's use of aerial 1080 and the argument for AHB's use are profoundly different and must be clearly distinguished. DoC has made neither a convincing case for net forest ecosystem benefit nor provided credible evidence of benignity. Furthermore, they have consistently misrepresented and distorted the evidence. Thus, their license to use aerial 1080 should be immediately and completely withdrawn until such time as properly designed independent studies are completed such as the one outlined on page 20.

On the other hand, AHB is concerned with controlling bovine TB in New Zealand's cattle herds, an unquestionably desirable goal. Furthermore, the connection between bovine TB in possums on the pasture margin and bovine TB is relat ively well established*. Thus, there is a clear necessity to control possums on the pasture margins †. AHB asserts that if bovine TB is to be eradicated in possums they must be eradicated it in the deep bush (98). This has not been proven. It is an educat ed guess. Furthermore AHB's evidence that they are not causing irreparable damage to the deep bush with aerial 1080 is no better than DoC's, which as we have seen is close to non-existent. Given all this , what are the alternatives to continuing to indiscriminately poison our forest ecosystems?

Ground-based trapping

It is important to recognize that the only thing driving the continued use of aerial 1080 for either DoC or AHB is money - the added cost of ground-based trapping in remote and rugged terrain (99)‡. AHB does most of its possum control with professional trappers and dogs in the pasture forest margins (only 22% of 8.2 mega hectares is subjected to aerial 1080 (100)). Fortunately, AHB has carried out the experiment necessary for us to estimate the a dditional cost of ground-based trapping (101). This excellently done research study shows that the additional cost of using ground -based methods is about $17/ha, the difference between aerial 1080 at $20.25 and two other methods at about $37/ha. Although A HB sometimes asserts that some areas are just too remote, this study included some very rugged areas, suggesting that these numbers are at least roughly applicable. However, adding an additional $3/ha for "extra roughness", we get a $20/ha differential, which translates into about an additional $ 36 million/year to get AHB out of the aerial 1080 business (and thereby to stop risking our precious forest ecosystems and our ecotourism industry).

Trap only the margins

As suggested above, another alternative is to only control possums in the pasture forest margins and relative ly accessible forest, and leave the truly rugged areas uncontrolled. This is not as cavalier as it might first sound. (See the section entitled *Pasture margins are the place to work* starting on page 59.) There is little direct evidence that controlling the really rugged deep forest is necessary. Possums tend to cluster in the pasture margin. The juveniles, which are t he principle source of spread among populations, do not travel far, the mean distance being about 5 kilometers (102) and individual possums with bovine TB die quickly, usually within six months (103). According to Landcare Research in 2000 (103),

* Though the strength of this connection is far from perfect, as we have previously noted.

† Evidence indicates to at least about 3 kilometers.

‡ Here again DoC distinguishes itself from AHB in that DoC increasingly carries out aerial 1080 poisoning operations in relatively accessible regions (123,2).

> *The mathematical models predict the disease will die out in possum populations in 5–10 years if possum numbers can be reduced by about 70% and kept at that level, and immigration of infected possums can be prevented. Field observations suggest that eradication may occur sooner if control is more intense. Most large areas where the disease occurs in possums are now surrounded by a buffer zone in an attempt to control the outward spread of the disease. Possums in these zones are controlled by poisoning and/or trapping to reduce the risk of disease establishing in possums in these zones. Any Tb outbreaks outside these areas are closely investigated, and if it is suspected that wild animals are involved, the area around the outbreak is intensively controlled to try to eradicate the disease. Successful possum control operations combined with regular tuberculin testing of livestock quickly reduce the number of tuberculous livestock, but experience has shown that if possum control is not maintained, infection in cattle increases again within about 5-8 years.*

This is the song AHB was singing in 2000. It has certainly gone by the way side. In view of this, one wonders why we should believe their current crop of predictions of the future in the current DoC/AHB submission. At the least, the certitude in their tone should be taken with a grain of salt.

It is likely that to have an optimal chance of reaching AHB's eradication goal *, the possum population in the rugged forest probably must be controlled to some degree. Unfortunately, the experiments that would definitively determine that degree have not been done. But they surely should be before the two agencies are turned loose with an unlimited license to poison the forests however they wish.

Other approaches

Many approaches to eliminating possums are known and have been and/or are in use. These include trapping, hunting, hunting with dogs, using kill traps, using catch traps followed by killing, poisoning using refillable bait stations, poisoning using paste or gel, and aerial drops of poison from helicopters. Much promise is held out for biological control methods, but such approaches are unlikely to be commercially available for at least 10 years, possibly much longer (103).

Possums as a business

Perhaps the most neglected approach to possum control is to support the possum products business and thus encourage the market for possum pelts. There have been and are businesses and industries based on possum products (104,105). Possum fur is a prized animal fur and is used in a variety of ways in clothing and other fur products. Possum skin is also used for gloves and other skin products. Numerous companies both here and abroad manufacture such products. For example, a New Zealand company Snow Peak Limited (106) that has been manufacturing possum-fur blend knitted products for about 25 years and Rente Corporation Limited also manufactures and distributes possum fur products. Possum rugs or throws sell for between $1,000 and $4,000 each (107). There are intermediate processing industries in support of these target markets. These industries could consume a number of pelts. In the early 1980s, about 3.2M possum pelts were exported to contribute $23M to our export market. If the marketing image of possum fur were exploited as a quality fur without the environmental stigma of competing furs, then over ten years tens of million of possum pelts could be exported and/or contribute to the growth of internal infrastructure to exploit this resource, producing upwards of $30M. The 1.3 million possum pelts harvested today have a market value of $18M (108).

* It is not clear that the goal of eradication of bovine TB in possums is attainable with existing technologies.

There are problems with turning possums into an industry. The most common one cited is its unreliability. However, we have found no e vidence that there has been a significant effort to find a way to harness the potential power of private enterprise to assist with this costly and serious national problem. It is difficult to imagine that some combination of incentives and regulation would not be effective. The question is how to make the industry's goal of profits coincide with the national goal of controlling possum numbers. There certainly should be little concern that the industry would be reluctant to hunt possums to extinct ion and put itself out of business, since that is exactly what happening world wide with the fishing industry. Neither DoC nor AHB has pursued these alternatives, but that should not be surprising given that it is contrary to their bureaucratic interests.

Government/citizen collaboration

There is a large reservoir of environmentally conscious citizens and landowners dedicated to protecting the native bush and who are willing to do their share. Many large landowners who have farms or large tracks of forest and/or n ative bush are dedicat ed to reducing the predation of these pests. There are also volunteers who work with conservation community groups in Forest Reserves and on other government land to work trap lines or refill bait stations. There is even the example of the motivated inn in the Tasman District that offered various bounties for different dead pests – the possum earned a "free handle of beer or cider". The possum "bounty" was shut down after 5000 had been "cashed in" (109).

By its own numbers DoC's programme has failed

A New Zealand-wide government-sponsored bounty system was operational between 1951 and 1961 with pelts bringing the equivalent in today's dollars of about $13 each (110). That decade saw the death of 12.4 million possums with bounties being collected on 8 million possums (111). The programme was stopped when it was claimed that the harvest level was not exceeding the reproductive rate of the possums.

By this standard the current government programme appears to be equally guilty. Around 1993, the possum population was estimated to be 70 million (112). Five years later, the possum population was still estimated to be 70 million (113) and that estimate does not appear to have been changed even now. Thus by the 1961 rationalization that stopped th e bounty system, we would discontinue the tens of millions of dollars going to DoC now and look for a different approach to possum control .*

If this is even approximately true, then since 1993, it appears that the taxpayer has expended well in excess of several hundred million s of dollars in direct operational control and research to do no more than *maintain* a healthy population of possum pests and, in the process, to distribute on the order of 30,000 kg of a lethal poison into our ecosystem s in the name of "control" of that species.

Multi-pronged strategy

It seems apparent to us that achiev ing optimal efficiency in possum control requires first taking primary responsibility for strategy away from DoC, an agency that is benefiting by a particular approach, and an agency that has not succeeded despite exposing our ecosystems to considerable risk. It is not our intension to defend any particular approach, but rather to

* Interestingly, there is a curious dislocation between the fact that bovine TB is clearly coming under control and the fact tha t possum number apparently have not, which further supports the contention that bovine TB control should be concentrated where it has been, in the forest pasture margins.

suggest that a multi -pronged approach has never been tried and at least in theory would seem to have merit*. Such a programme might contain the following elements

- Set up a tiered bounty of $1 to $4 for each "clean" possum turned in to the government by hunters. Structure incentives (e.g. via the bounty rate) to motivate hunters to continue working their areas as the possum density drops. Allocate blocks of "high value" conservation land to bounty hunters or trappers on a permit basis and set performance targets.

- To enable the control of bovine Tb, contract with professional hunters and trappers to work blocks of bush at pasture forest margins where possums tend to concentrate and where the wildlife vector for transmitting bovine Tb to cattle is located. Contracts would be let on a competitive bid basis and performance above the target Residual Trap Catch (RTC) would attract bonuses. Contractors would be encouraged to collect the skins or to extract the meat for sale to those markets, thus enabling them to bid their services at a lower cost.

- Encourage other regional councils to undertake programmes such as that adopted by the Taranaki Regional Council, involving landholders participating in possum control (114).

- Set up government reach-out support for submitted possum carcasses (clean & undamaged), facilitating their delivery to suitable markets. Possums that were killed by poison, traps or hunters could be used by the fur industry. Those killed by hunters could potentially be used by the food industry (providing they were not killed in a possum poisoning zone).

- Investment by the government in possum fur export market development, market infrastructure development, and a loan programme for start-up businesses in this arena.

- Encourage farmers and native bush landholders to reduce the possum populations on their blocks by providing bait stations and /or traps suitable to the size of their blocks and providing bait on an on -demand basis, giving them guidance as to the best regimen to follow.

- Assume that the private sector agricultural and forestry industries will continue their culling of possums. Encourage these sectors to participate in contributing dead possums to the market economy.

Conclusion:

The important message from this section is that there are serious alternatives to possum control for the purpose of eradicating bovine TB. Some of these may be less costly and more effective than the current practice of periodically blanketing our forest environment with a universal poison.

Does the structure of research funding guarantee biased results?

In the early 1990's Landcare Research was reorganized. This resulted in researchers having to attract contracts from the funding agencies . When DoC decides to do a particular study, they contract with Landcare Research to execute it. Naturally, this financial control leads to

* As always any new approach should be tested against other contenders with experiments that will objectively establish success or failure, as was done with marvelous success by AHB in determining the costs of various possum control methods (101)

substantial scientific direction and influence over the research team assignments, which in turn has a profound effect on a researcher's future career. If the paying agency asks for a particular investigator, his supervisor and his section benefit. If the paying agency discourages or rejects a particular scientist, the opposite may result.

Since DoC became a strong advocate of aerial 1080, much of the key research investigating the effects of aerial 1080 on the forests and native species has been led by a few individuals whose papers show that they strongly believe in the benignity and benefit of aerial 1080 to our native species and forests (see details and quotations elsewhere in this document, especially the section entitled *The species level evidence*). Thus, the same few authors repeatedly appear, notably Eason, Spurr and Powlesland. On the other hand, authors that are more circumspect about the use of aerial 1080 do not repeatedly appear, for example, Meads [*], Bellingham and Innes. Weaver (8), who has questioned the accuracy of Eason's monofluoroacetate degradation data and raised concerns about chronic toxicity, has a tenured academic position and therefore is mostly beyond DoC's direct reach.

Such influence does not have to be explicit. It exists by virtue of the inherent political and fiduciary control exercised by DoC in its relationship to Landcare Research. Until some more neutral brokers begin to sponsor research on 1080, there will be no way for New Zealand researchers to escape the web of dependency and hence influence of DoC's bureaucratic agenda that is so evident in the DoC -sponsored research since 1990.

Ultimately, the scientific investigators at Landcare Research (where the vast majority of the research is contracted) are not at fault. It is an inherent part of the structure of research funding that, in order to keep their jobs, Landcare Research investigators must keep the DoC bureaucrats happy with what they do and what they say about what they have done. In our view, this is all too apparent in the published literature in the highly political, big money domain of aerial 1080 research. [†]

Does the DoC/AHB submission misrepresent the truth about aerial 1080?

Upon initially examining the DoC/ AHB 1080 reassessment application, we were struck first by its length, and second by the number of statements that were entirely unsupported by evidence, that were factually in error, and /or that misrepresented the evidence in the scientific literature. We had neither the time nor the resources to exhaustively critique this nearly 500 page document. On the other hand we could not leave many of the claims in the document unchallenged.

Thus, as a compromise we have done a statistical study. First, we generated a sequence of random numbers between 1 and 467 using the random number generator provided in the SAS Institute's software Version 8.2. Then we eliminated certain sections of the document such as the glossary, data sheets, blank pages, and Maori issues (because these are outside our expertise). When these pages were encountered we did not examine them, but their number was recorded. All other pages were examined for problems that fell into one or more of five categories. The results are summarized in Table 7 below.

Twenty-three of 40 eligible pages contained one at least one erroneous or fallacious passage. This is **58%** (95% confidence interval: 43-73%). Restated this means that 58% percent of

[*] The case of Meads is discussed in the section: *The politics of 1080 and how the Meads' paper came not to be officially published, page* 36.

[†] It is important to note that not all of the research on aerial 1080 is obviously biased. There are several authors whom we have cited and who appear not to be biased in reporting their results; among these are Bellingham, et al, Innes and Barker, Weaver, Lloyd and McQueen, and Meads.

pages had at least one omission, unsupported conclusion, misrepresentation, logical error, or false claim. Of these, three pages contained multiple reportable instances. Thirteen pages were pre-excluded for the reasons noted above. In one case DoC claims that there is no evidence indicating harm to invertebrates. This is only true if one ignores the Meads paper which DoC attempted (with considerable success) to suppress*.

? Table 7 Summary of DoC/AHB Submission Survey for Errors

Category	Symbol	Definition	#	% Pages with Error(s)
No Error	NE	Pages containing no errors	17	42
Any Error		Pages containing at least one passage with an error of one of the following types.	23	58 (CI 43-73)
				% of Erroneous Passages
Omission and selective reporting	Omi	Passages that implied selective reporting or omissions that were obviously relevant	10	20
Unsupported conclusion	Uns	Passages that made claims that are unsupported by evidence. By "evidence" we here mean scientific evidence of at least Control Level 1 or higher that were cited in the DoC/AHB submission or that we became aware of in the course our investigations for this document	10	20
Misrepresentation	Mis	Passages that contain misrepresentation or distortion as established by the published scientific evidence	18	36
Logical errors	Log	Passages containing logical errors, usually *non sequiturs*	1	2
Factual errors	Err	Passages containing a factual error or false claim	11	22

The complete survey results are given below in Table 8. We urge the readers to read through these in detail. They are instructive in that they provide a good cross section of what is in the whole document, but they are also enlightening in that collectively they expose the degree to which the DoC/AHB claims must be taken with circumscription .

? **Table 8 Errors, Distortions, and Misrepresentations Encountered by Examining Randomly Selected Pages from the DoC/AHB 1080 Reassessment Application**

Quotation/Claim	Page	Comment/Correction	Ref	Type
Applicant details: Department of Conservation (DoC) "Within 1M ha under sustained possum control by DoC, 30 are intensively managed including use of aerial 1080."	73	No mention is made of the fact that DoC is increasingly using aerial 1080 on private land as well, including on areas that are <u>not</u> inaccessible for ground based control and/or are near human habitation . For example private lands were included in 1080 drops on the Whenuakite and Whirinaki Forest Parks in 2006	**115** **116**	Omi
Adverse Effects on Market Economy: Tourist Spending: "International visitors are probably not aware of the use of 1080 or cyanide in New Zealand when they make a choice on destination for their holiday. It is therefore unlikely that the use of 1080 or cyanide will prevent an international visitor from coming to New Zealand or visiting a region." DoC concludes that although the "negative perceptions" are likely, they are due primarily to local visitors, the risk is minimal with a low cost of containment.	201	Use of the words "probably" and "therefore" without evidence. In our view, when DoC's extraordinary practice of dumping a universal poison indiscriminately into our forest becomes known internationally, it is probabl e that New Zealand's clean green image will be importantly damaged, with consequent damage to our tourist industry which represents our largest source of foreign exchange. We confirmed this by calling six American acquaintances and asking them for their reaction and whether it would influence their opinio n of New Zealand. All six were shocked and had negative reactions.		Uns, err
NA	462	Glossary		
NA	177	Defines categories for 'Magnitude of Adverse Effect' Matrix; not suitable for comment		
Context: Pest Control in New Zealand: Introduction "The desir ed outcomes have not focused on the number of pests killed, but on saving threatened species and ecosystems as well as improving the productivity and health of the primary production sectors, notably agriculture."	2	The scientific studies almost always report pests killed. Deaths among native species are often entirely ignored, excused or suppressed. Studies looking at species harm are few, and most of those are of unacceptable quality, or actually suggest harm despite stated conclusions, or are inconclusive. No ecosystem level studies have been published. There is no credible scientific evidence of net ecosystem benefit.	20 61 **117** **118** 8 **119** **120**	Mis, err
Context: Pest Control in New Zealand: Vertebrate Pests: Possums. Central to DoC's aerial use of 1080 is the internationally unique circumstance that most of the mammals in New Zealand's forests (especially rats) are usuall y regarded as pests and their losses following aerial 1080 operations are generally desirable or inconsequential with respect to the ecosystems they inhabit.	2	• This suggests that aerial 1080 kills only mammals which is false (see page 5). • New Zealand is not internationally unique (see page 16). • Rats have been in NZ for 800 years and are by now an integral part of the ecosystem. • No ecosystem level studies have been conducted by DoC.	27 **121** **122** 61 117 118	Mis, err, uns

Quotation/Claim	Page	Comment/Correction	Ref	Type
Context: Pest Control in New Zealand: Vertebrate Pests: Possums. "Destruction of forest canopies has modified many areas; shrublands have replaced tall forests."	7	We are unable to find credible evidence supporting this statement, i.e. no published study at Control Level 1 or above documents the reduction of tall forests to shrublands by possums (see page 47).		Uns mis
NA	69	Blank page		
NA	150	Section 3.4 Default Controls: Annex 1. ACVM Controls: registered 1080 products list		
Section 4.5 Proposed Management (of use of 1080): Reviews controls applicable under various regulatory agencies. Concludes risks are managed under these controls and no additional controls are required.	439	No problems identified on this section. We have not reviewed all risks and controls.		
NA	421	Assessment of RCB significant to Maori – page cites and discusses Maori concerns		
Context: Pest Control in New Zealand: Timing of 1080 Operations "At Pureora, in the central North Island, robin fledging success has been shown to be far higher where aerial 1080 use reduced pest populations, by allowing robins to lay and hatch multiple clutches of eggs in a season. In the non - treatment areas, fewer robins survived to maturity and more adults were killed on the nest."	13	See page 25 for a detailed discussion of what was actually shown in the Pureora studies. In summary, • Nesting success was only improved in one of three studies. • Nesting success did not translate into population success. • The damage to tomtits is not mentioned here. • Large numbers of both tomtits and robins were killed. • The studies were poorly done and reported with considerable distortion.	28 29 30	Mis, omi
NA	122	"References" page		
Effects on the environment: native fish, eels and freshwater invertebrates: "… general benefit from enhanced ecosystem health and biodiversity" and "Improved habitat for native fish, eels and freshwater invertebrates from enhanced eco system services." Assessed as "Likely" of magnitude "Minor" benefit B	289	The claims of "enhanced ecosystem" and "enhanced ecosystem health and biodiversity" are wholly unsubstantiated by scientific data as an effect of 1080. There is not one ecosystem level study to support these kinds of claims. They are completely fabricated. Indeed there is much evidence to suggest that just the opposite is the case. See the section on ecosystem level effects starting on page 14.	2011 7 118 62	Uns err mis
NA	279	Categorizes poison delivery methods considered in the Effects Section.		
International Considerations of 1080: Canada, Israel and South Africa's regulatory status of 1080	452	It is not pointed out that these countries use only minute quantities of the chemical, and none of them drop it indiscriminately into forest ecosystems. Annual usage should be part of the "use patterns" discussion. See pages 5 and 16.		Omi
Adverse effects: Manufacture: Disposal/offsite discharges (normal mfg. process)	292	No problem found with procedures or risk categorization.		

Quotation/Claim	Page	Comment/Correction	Ref	Type
Default Controls: Hazardous property controls for vertebrate poisons: A person in charge of the substance must ensure that signs are erected at every normal point of entry to the place where the substance is to be applied or laid before the substance is applied or laid…The signs must remain until the earlier of when the substance is no longer toxic or when the substance has been retrieved from the place. Signs must be removed… until the expiry of that longer… time.	135	• In the first place our investigations failed to find another country in the world that would even consider dropping 1080 into a forest that is within a few kilometers from a population center or that is heavily used by people. As a representative of the Canadian Wildlife service said to us, "What do they want to do - kill a few people?" • Second, the rules described with all these "musts" are often not followed. To take one recent example, in mid-August 2006 there was an aerial 1080 drop along the 309 road just south of Coromandel town along one of two arteries to Whitianga. A local resident first noted that the signs that were flimsy, located adjacent to a stream and had effect until 1 Jan 2007. She later reported them missing. DoC claimed that signs were often removed by others, but surly that does not mitigate DoC's responsibility or cavalier behaviour. • Incredibly the replacement sign was dated as expiring 31 Jan 2007 because 1080 laced food was still lying around. This likely would not have been discovered had it not been reported missing.	**123** 18	Err
Adverse effects Sodium Cyanide and HCN	261	No problems found with risks and controls.		
Effects on market economy – M-B1 Benefits of reduction of Tb in stock and increase in earning with 1080 based on AHB projections of 75% Tb reduction in cattle and deer by 2015.	183	Agreement with the benefits cited but disagreement with quantitative analysis contributing <u>entire</u> increase to 1080. However basically agree.		
Adverse effects of 1080 pellets in aerial application on terrestrial invertebrates "and there is no evidence that invertebrate populations are significantly impacted by aerial 1080 pellet applications or that invertebrates are a significant factor in secondary poisoning of other animals."	306	This is simply false, and because it is easily shown that DoC is aware of the existence of the contradicting evidence, we feel compelled to label this statement for what it is: a deliberate fabrication, i.e., a lie. See Invertebrates section beginning on page 36.	62 61	Err mis
Potential toxicity from human consumption of meat from wild animals	247	No discrepancies detected		

Quotation/Claim	Page	Comment/Correction	Ref	Type
Outcome of possum control for habitat protection: Vegetation – Future without 1080 – "progressive attrition or forest collapse over many thousands of ha "	54	We can find no scientifically credible evidence to support this claim. While it is clear that possums eat plants, there is no sound evidence of important effects on populations (with the possible exception of mistletoe), and certainly no evidence of forest collapse caused by possums. Indeed even the management studies show an inconsistent message. See th e section on forest effect beginning on page 47.	76 83	Mis err
Pest Control Scenarios: Outcomes of possum control for habitat protection: General species benefits Claims with 1080, maintain or increase security of threatened species including forest birds and invertebrates Claims without 1080, species will be reduced etc.	54	• Both birds and invertebrates include species that evidence suggests may be harmed (possibly seriously) by 1080 to a level that they may not recover. • The one part of one study that showed improved nesting success in one species failed to translate into population success. • The "without 1080" argument assumes there would then be no possum control. There are many effective alternatives to 1080 for possum control one or more of which would certainly be used. The evidence is reviewed in detail in several sections of the documents. See for example, sections beginning on pages 21 and 36.	32 28 29 30 40 41 **124** 33 61 71 62 57	Uns mis
Pest Control Scenarios: Outcomes of possum control for habitat protection: Ecosystem services: Claims future without 1080 would see a potential decline over thousands of ha no longer receiving treatment. Losses may include: "reduced soil and water quality, lowered resilience to flood, drought and storm events, reduced carbon storage capacity".	54	This is a particularly hyperbolic version of similar statements made and dealt with elsewhere. It is unsupported by evidence and furthermore the DoC/AHB submission fails to provide any.	8 61 118 20 62	Uns mis
Pest Control Scenarios: Outcomes of possum control for habitat protection: Landscape and ameni ty values	56	Claims of ability to maintain these values for recreation and tourism are unsupported by any evidence. Indeed monofluoroacetate may equally undermine the tourism industry by its aversion to ecosystems blanketed with a universal poison. See above.		Uns
Effects on Human Health and Safety: Adverse Effects: Public exposure to sodium cyanide paste through uncontained application applied to natural features – very unlikely, extreme effect, level of risk E	264	Appears to be error free.		

Quotation/Claim	Page	Comment/Correction	Ref	Type
Pest Control Scenarios Figure 12. Areas forecast to be occupied by tuberculosis-infected possums in 2015 WITHOUT 1080 use	49	• Makes at least two unsubstantiated claims to support the figure. First, it assumes a spread of infection from possums migrating from deep forest to pasture margins followed by a contamination of herds from these. (We are unable to find good evidence that deep forest possums are infected with bovine Tb. Unless possums run out of their food source, they do not migrate great distances, not more than 1.3 km. Therefore even assuming that young will leave to find a new home range, this scenario is unlikely.) • As is demonstrated in the text of this document, there are alternative strategies to aerial 1080 to control bovine TB. Controlling bovine Tb in possums at the pasture margins can probably be done using traditional techniques – trapping, bait stations, bounties, etc., thus preventing its spread. • Regardless, the projection suggests what will happen without possum control, NOT without 1080.	**125** 94 96 **126**	Uns mis err
Significant Risks, Costs and Benefits: Effects on Social and Community: Benefit of recreational activity enjoyment due to: maintenance of heal thy forest habitat native biodiversity. Adverse effects include lost deer hunting opportunity and "grief caused by pet suffering or mortality".	379	• Again speculation that is entirely unsupported by evidence, either cited or extant. • One of the more insidiou s effects of aerial 1080 is the mass indiscriminant loss of animals that are hunted for recreation and food, including pigs, deer and goat. • Adverse effects should include horror of many people at wholesale poisoning of the environment and its likely consequences and the restricted access to our "conservation estate" for months on end following aerial poison operations.	20 118 **127**	Omi mis

Quotation/Claim	Page	Comment/Correction	Ref	Type
Effects on the Environment. Adverse effects Aerial sowing of 1080 pellets: Robins - claim that nesting success <u>more than compensated</u> for any robin losses from 1080 (Powlesland et al 1999)	303	This is false or at least distorted. • The study cited was one of a series of three. The other two failed to show this much - publicized effect on nesting success even on the one species studied, robins. Furthermore, the report of year three failed to document the nesting results. • The bottom line population studies showed no difference. • Only one species was reported despite tomtits being in the original study. • The studies were DoC-sponsored and under DoC financial control. • The studies had only Level 2 controls. • The studies lacked sufficient numbers. Much more detail is given in our section on these studies (see page 25 and beyond). And, yet this one unreproduced result of a process variable is repeatedly cited by DoC as proving benefit to native bird species. This strikes us as outrageous.	28 30 29	Omi mis
Effects on the Environment. Adverse effects Aerial sowing of 1080 pellets: Tomtits – claim that one disappeared of 29 colour-banded tomtits over 2 aerial 1080 cereal operations (Powlesland et al 2000, Westbrooke et al 2003)..	303	The first study cited covered three poisonings during which tomtit mortality was studied but only one used cereal bait; the other two killed large numbers of tomtits. The other references also examine cereal bait poisonings. No mention is made of the fact that in the first study carrot baits have proven devastating to tomtits, killing up to 100% of the animals tracked. It fails to mention a later study by Westbrooke et al 2005 that confirmed to mtit deaths with much lower concentrations of 1080 in aerial distribution of carrot baits.	28 30 40 41	Omi mis
Effects on Social and Community. Intangible adverse effects to the nation and local communities from pest mgmt. for conservation and bovine Tb outcomes: Concern for welfare of non -target animals exposed to vertebrate pest control methods – Likely, Minor effect, D risk	218	Various members of the community are devastated or directly affected by these "pest" control methods. This includes trapper (loss of way of life in addition to livelihood), hunters (loss of prey), farmers (loss of dogs, cattle and sheep to accidents), and outdoorsmen. All of these people report perceived negative effects from loss of bird sounds to loss of ecodiversity. This impact, if real, is likely to increase over time if indiscriminately dumping monofluoroacetate into our forest ecosystem is permitted to continue.	22 5	Mis
NA	208	Title Page		
NA	428	Maori issues. Not qualified to assess.		

Quotation/Claim	Page	Comment/Correction	Ref	Type
Hazard Classification: Terrestrial Vertebrate Ecotoxicology: Classification of 1080 active: 9.3A	111	No disagreement with classification. One of the LD50s reported differs from the LD50.we found in the literature. (Mallard)		
Context Pest Control scenarios Annex 1. Forecast outcomes of distribution of infected herds by 2015 WITH 1080	57	Accept. Based on information and models not available to reader.		
NA	222	Title Page		
NA	121	Reference Page		
NA	119	Reference Page		
Adverse effects of 1080 pellets: Controls for native frogs and lizards	308	No errors found		
NA	367	References		
Import/delivery/manufacture of 1080 products	160	No errors found; although have seen higher number for total quantity used by New Zealand annually.		
Context Pest Control Scenarios, Areas forecast to be occupied by Tb-infected wildlife WITHOUT further use of 1080	22	This is based on several dubious assumptions: • The bulk of the reduction in bovine TB is attributable to controlling possums with 1080. This has not been shown with good evidence. A) Infected herds in Featherston continue with no infected possums in the vicinity. B) farmer management and purchase practices also contribute to the spread of the disease. C) Deficiencies in testing apparently contributes to inadequate detection in cattle. • It further assumes that 1080 is the only viable control option for possums. This is patently false. Other poisons and other poison techniques (bait stations) would be used and/or traps and/or contract hunters or some biological technique would be implemented or... This is especially true since the main focus of threat from infected possums is at the forest pasture margins. See pages starting on 58.	95 96 33	Mis err log
3. Hazard Classification	95	No problems found		
Benefits to Invertebrates from predation and reduced competition for food: Likely, Major effect, level D risk Claim of Major because some populations of invertebrates may become locally extinct WITHOUT 1080.	288	Totally unsubstantiated. No evidence. This barely disserves a response, but there is at least one good study showing just the opposite (see pages starting on 36).	62	Uns err
E-A34 Adverse effects to Soil from 1080 contamination by 1080 paste residue Rated Extremely likely, minimal effect, Risk D (tolerable)	323	We take exception with the claim "minimal effect" and with risk rating pending more research. At low temperatures, 1080 will adhere to the soil and could take much longer to degrade, thus potentially endangering ground dwelling insects and invertebrates. (However the claim could be made that even if it persisted, the quantity would be localized. That is a judgment call.)	62	Uns

Quotation/Claim	Page	Comment/Correction	Ref	Type
Adverse effect without 1080: Cyanide – transport from Port of Auckland to mfg facility Exposure improbable; minimal magnitude A risk (minor)	344	No problems found.		
Hazard Classification: 5.2.4 Degradation in soil	110	No problems found, except that the research should have tested soil residues following aerial 1080 drops at locations with low ambient soil temperatures (<10C).		
Effects on Human Health & safety: Adverse effects H-A14 Pellet formulations…Aerial Application	236	The risk of terrorist attack via water supply is not mentioned. Although unlikely it could affect thousands. The most probable avenue might be through a disgruntled employee who handles the raw powder. This risk should be added along with mitigation measures.		Omi
Default Controls: specific documentation rqmts.	141	No problem found		
Default Controls: Control codes 17 & 18; Ecotoxic Classes 6, 8, 9 controls	135	No problem found, but consideration should be given to resetting the Ecotoxic EELs to the defaults as no EELs exist for 1080.		
Effects on Environment: Transportation from Manufacturing Site to Application Site E-A12 Pellets	294	No problem found		
Benefits: Benefits to domestic economy from reduction or elimination of bovine Tb in cattle and deer: Reduced likelihood of restrictions on access to export markets for live cattle and deer.	187	No problem found with assessment rating of benefit, except dispute the claim that it would not be possible without 1080.		
Adverse Effects Pellets aerial application: Pellets are sown from the aircraft onto ground E-A15 Native fauna – terrestrial invertebrates Rated as Likely, Minimal effect, D risk (tolerable)	305	Only direct feeding on baits is considered. In fact, numerous pathways are known: directly feeding on bait, eating poisoned insects for insectivorous invertebrates, larvae killed from 1080-contaminated eggs, feeding on poisoned mammals, poisoning by eating roots with adsorbed 1080 or by soil dwelling invertebrates in contaminated soil before it is defluorinated. - "In the light of the evidence of the effect of 1080 on invertebrates, and the complex role that invertebrates play in the ecosystem, the unrestricted use of 1080 is likely to be disruptive to the environment, and where endangered invertebrate species are known to be present, 1080 should be used judiciously, if at all.". This suggests that a more accurate rating would be : Likely, potentially devastating effect, F risk (unacceptable)	61 62	Mis omi err

Quotation/Claim	Page	Comment/Correction	Ref	Type
Risk assessment methodology: 4.1.2.Assigning level of risk or benefit 4.1.3 Identification of benefits and adverse effects	173	For looking at effects on market economy, social & community, human health & safety and environment, the methodology developers appeared to be selected with a "pro-1080" bias. In particular the participants consisted of DoC employees, Landcare Research researchers who are beholding to DoC for contracts, 1080 manufacturers and applicators with no apparent counterbalance. In short, all people with a vested interest in the 1080 industry. We believe it should have been balanced with more independent participants such as toxicologists, Federated Farmers, Reserve Bank of New Zealand, sociologists, economists, Office of Parliamentary Commissioner for Environment. As for research organizations, one should include independent perhaps academic scientists, not just ones that work for DoC or Landcare Research working on 1080 research. Consequently, this entire section, over 40% of the AHB/DoC submission, should be view with grave skepticism.		Mis omi
Hazard Classification 5.2 Subclass 9.2 Soil Ecotoxicity: Summary of Data; Toxicity to soil invertebrates and plants	108	A New Zealand earthworm species and a common garden snail were used for their assessment of toxicity to soil invertebrates. However, LD50s can vary widely among and even within species and so may not reflect the range that may be applicable. Moreover, more than just soil invertebrates may be affected by 1080 in the soil. For example, 1080 may leach from the soil onto eggs and subsequently poison the larvae when they emerge. Thus more than just soil invertebrates should be considered.	61	Omi
Default Controls: Packaging	142	No problems detected		
Context Pest Control Scenarios: Figure 1 Areas forecast to be occupied by Tb-infected wildlife WITH continued 1080 use	21	No problems detected, inadequate information to question model or projections.		

About the authors

Quinn Whiting-OKeefe, MD, Ma*

Dr Whiting-OKeefe graduated from the University of Utah Sigma Cum Lauda and Phi Beta Kappa with dual Bachelor degrees in chemistry and mathematics. He then did PhD work in mathematics at the California Institute of Technology until he interrupted his dissertation work to go to medical school at the University of Utah. He graduated from medical school second in his class in 1974 receiving the degree of Medical Doctor. He did a residency in internal medicine at the University of California, San Francisco (the life science campus), and was Board Certified in Internal Medicine in 1978.

At the time, much of clinical knowledge was based on studies similar in structure to D oC's research on aerial 1080. He recognized that this was an important problem for which his dual backgrounds in mathematics and medicine especially qualified him to address. In 1977 he applied and was accepted as Robert Wood Johnson Clinical Scholar specializing in biostatistics and clinical study design. As an RWJ Clinical Scholar, he published several papers on this and related subjects. Simultaneously, he did a fellowship in Rheumatology and was board certified in 1983.

In 1979, he joined the faculty at the University of California, San Francisco, with a dual appointment in Medicine and Medical Information Science. He rose to the rank of Associate Professor before going to work in the private sector developing clinical information systems. He attained the position of Vice President at three companies, including Senior Vice President of Engineering at Bell Atlantic Healthcare Systems, where he designed and lead the development of OACIS, a $50 million project.

Throughout his career in computer science, he continued to design and analyze clinical research studies, it being his first real academic passion after mathematics. From 1995 to 1998, Dr. Whiting-OKeefe single-handedly developed the Healthcare Outcomes Performance System which uses advance hierarchical statistical linear modeling to predict healthcare outcomes. Since1998 when he retired and moved to New Zealand, Dr. Whiting-OKeefe has continued to design and analyze healthcare experiments and publish papers with his long time colleague and friend, David B Hellmann, Vice Dean and Aliki Perroti Professor of Medicine at Johns Hopkins University School of Medicine.

Dr Whiting-OKeefe proudly became a New Zealand citizen in 2002. He is 63 years old and lives with Patricia, his wife of 35 years and co-author, in Port Charles, Coromandel, New Zealand.

Patricia Whiting-OKeefe, PhD

Dr. Pat Whiting-OKeefe attended the University of Delaware, U.S.A. graduating with a Bachelor of Science in Chemistry with high honors, honors in course and distinction in 1965. She received her Doctor of Philosophy in Chemical Physics from the California Institute of Technology in 1971. Her dissertation concerned quantum mechanical orbital modeling in

* We have provided brief resumes of the authors so that the ERMA reviewers will have some notion of the academic and scientific qualification of the authors. However, we do not believe that the content of this paper should be accepted (or rejected) on the basis of the authority of credentials. We believe that the document should stand on its own merits. With respect to what constitutes good experimental design and statistical inference, we suggest contacting international authorities at, say, the Johns Hopkins School of Public Health, or other internationally respected authorities. With respect to questions regarding the principles of ecology and the management of ecosystems, we would encourage you to contact the ecology departments at Harvard, Stanford and Cambridge Universities.

lithium. She subsequently worked in a variety of scientific, technical, management and consulting positions, including:

- Project Manager, Eaton Kenway (1971 -1974)
- Director, Advanced Computer Systems Department, Stanford Research Institute (1974-1982),
- Managing Scientist, Failure Analysis Associates (1988 -1990),
- Director, Information Resources, SyStemix, Inc (1992 -1998), which develops cellular therapy technologies
- She was Associate Professor, San Francisco State University (1976-1977).

In 1998, she also retired and moved permanently to New Zealand. She became a New Zealand citizen in 2002.

Summary and Conclusion: "First do no harm"

There are two distinct issues and agendas regarding the use of aerial 1080 in our forests. The Department of Conservation (DoC) literally claims that aerial 1080 is necessary to prevent the degringolade of native forest and the mass extinction of native species (128), and further that aerial 1080 has irreplaceable beneficial effects on native species .

The quality of scientific research supporting these claims is not good. Most of it reaches only the lowest levels of control quality. Statistics are often poorly done, absent or selectively reported. The studies are short term and narrow in scope. There is not one randomized, blind experiment. Results are frequently misrepresented and distorted , often with obvious bias. There are numerous errors of inference, omission and commission. Roughly ha lf of the studies are only published internally by DoC or LCR. Most of the others are published in a single journal, the New Zealand Journal of Ecology. There is only one study that appeared in a peer reviewed international journal. Worse , the entire lo t, excepting one or two, was produced by researchers who are pecuniarily dependent on DoC's goodwill.

Collectively, scientific literature regarding the use of aerial 1080 in our forests affords a few facts:

- There is not a single scientifically credible study showing that aerial 1080 pest control is either beneficial to or necessary for New Zealand's native forests or their inhabitants.
- There is strong evidence that aerial 1080 kills substantial numbers of native birds, invertebrates, and our only native m ammal, the bat. The effect of aerial 1080 on populations is not known, and even less so of repeated applications of aerial 1080 over time.
- The net overall effect of aerial 1080 on our forests and forest ecosystem s cannot be determined. Evidence on both s ides of the argument is at the level of rumor and anecdote.

Seven Summary Points:

1. DoC's aerial 1080 research does not address the bottom-line, fundamental question of net ecosystem effect.
2. DoC's aerial 1080 research contains numerous methodological, statis tical and inferential errors.

3. There is substantial evidence that DoC has suppressed critical research that is unfavorable to its bureaucratic agenda.

4. The research is pervaded with bias and misrepresentation.

5. Even if the research were not poorly done and were unbiased, it still does not show what DoC claims it shows, namely that aerial 1080 is benign and beneficial to forest ecosystems.

6. The aerial 1080 research is uniformly tainted by the lack of financial and career independence of the researchers.

7. The DoC/AHB reassessment submission itself is rife with errors of omission, claims unsupported by evidence, misrepresentation, non sequiturs, and factual errors.

The strongest argument that 1080 is helping and not harming our forest ecosystems is the cacophony to that effect persistently emanating from DoC at considerable public expense.

In our minds this is not enough to justify this extraordinary policy, unique in the world, of indiscriminately poisoning our native forests in defiance of the known principles o f ecology and ecosystem management. In medicine, there is a saying that is often attributed to Hippocrates, "First, do no harm", and so it should be here.

The second issue is that advocated by AHB, of using aerial 1080 to control bovine tuberculosis in cattle and domestic deer. There is reasonably good, but not conclusive evidence that tuberculosis-infected possums inhabiting forest pasture margins are at least in part responsible for New Zealand's failure to eradicate bovine tuberculosis. There is a gre at deal that is not known about possum population dynamics in relation to the epidemiology of tuberculosis that could bear importantly on the best way to control possum spread tuberculosis. At present, it is uncertain what degree and nature of possum cont rol is needed in the deep rugged forests where aerial 1080 is most used by AHB. Good research has no t been done to determine this.

It is clear that there are alternatives to aerial 1080 that do not involve its risks to humans or the environment and that h ave not been adequately investigated or seriously tried. This pertains equally to DoC's and AHB's rationalizations for aerial 1080. <u>It is important to understand that the only reason for the continuation of this extraordinary and risky practice is the co st of the alternatives, which by AHB's own figures is about $36 millions per year.</u>

Recomendataions

Our recommendations are as follows,

- DoC's aerial 1080 operations should be stopped until a properly designed and executed study <u>independent of DoC</u> can be completed showing the benignity and benefit of aerial 1080 to our native forests and its inhabitants. Such a study should include arms that test alternatives to aerial 1080.

- Environmental research funding should be reorganized to remove it from the control of agencies and individuals with potential pecuniary interests, such as in this case the DoC bureaucracy. The National Institutes of Health in the United States might serve as a good model.

- An inquiry should be initiated regarding the practice of the aerial application of 1080 and the manner in which DoC has advocated that it should be funded by the New Zealand government. The inquiry should not be under political control of any kind. It should be undertaken by a one -time independent commission at least half constituted by respected international scientists.

- AHB's license to continue aerial 1080 should be limited to experiments specifically designed to determine its absolute necessity and the real cost of alternative approaches.
- We, the citizens of New Zealand, should pay the additional cost (if there really is any) of ground-based possum control until sound research can be completed that credibly establishes that aerial 1080 is beneficial to our forest ecosystems.

Personal comment

We think that it is appropriate to provide to the ERMA reviewers some insight into our personal reasons for undertaking the substantial project of which this paper is a result.

We, the authors, are foreign born, reared and educated. When we retired, we could have moved anywhere in the world. We chose New Zealand. A couple of years ago, we were shocked and indeed viscerally disturbed to learn that the government of our "sane and clean" New Zealand was routinely and indiscriminately dropping food laced with large amounts of a universal poison into its forests[*]. Before that when asked why we chose New Zealand (as we frequently were), we always answered that we chose New Zealand because it is environmentally sane and clean. Having observed for years the irrationality, lack of science, and ecologically simplistic nature of DoC's interventions in the Port Charles ecosystem where we live, we were not surprised when our early investigations failed to turn up good evidence supporting the use of aerial 1080, which is *a priori* so anti-environmental.

We have stopped proffering New Zealand's environmental sanity as our reason for living here, but we also have determined to do what we could to put things right. Initially we tried to convince both acquaintances and DoC representatives to look to the scientific evidence, but the arguments there are complex, presupposing knowledge of and belief in the principles of scientific inference, which most people simply do not have. Furthermore, the DoC propaganda machine regarding aerial 1080 has been in full operation for so long and the aerial 1080 practice now so enculturated that there are few Kiwis capable to viewing the evidence objectively. Our efforts were entirely in vain.

The announcement of ERMA's reassessment of 1080 a couple of months ago afforded a new line of approach[†], and we resolved to do what we could to put this important national issue on a more rational course, one dominated by the scientific evidence rather than bureaucratic whim or self-interest. That resolution has resulted in this paper, which despite its inherently critical nature, we hope will be taken as it is intended, for the betterment of our much appreciated adopted country.

[*] We suspect that many in the international community, especially the scientific and environmental parts of it, will react exactly as we have when they learn the truth about what New Zealand is doing to its forests.

[†] One in which the subtleties of scientific evidence and inference would be an asset rather than a liability.

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