



More Birds in the Bush – Mid programme update

The following questions were asked during our live two-day event but due to time restrictions, we were unable to answer these in the sessions.

John Innes

How does plasticity of species complicate especially for food resource or nest/roost sites complicate this? Do we care in which bush these birds reside?

I presume you mean plasticity of the birds we are trying to help? Do you mean: If they would do fine in cities or pine forests, would we be happy with that? We would at one level because it would preserve the taxon (e.g. kiwi in pines), but the birds would be taken off their mutualisms if they had any (e.g. pollination and disperser roles). And there are ecosystem-level goals where we would prefer that ecological systems were operating with their original native players I think. Your question is an interesting current area of study, about 'conservation evolution' - whether birds can 'evolve' to not be so fussy about everything. See Stephen Ulrich's paper in NZ Jnl Ecol 39(1) 2015.

So since we can't eliminate mice, are they the ultimate remaining problem?

We can eliminate mice; have done so from numerous islands e.g. Rangitoto (2300 ha). I think that preventing reinvasion is very hard since they are so small. They are certainly difficult and have remained in large pest-fenced sanctuaries despite people's best efforts. I would reserve 'ultimate' for 'depending on exactly what you are trying to protect'.

Have Landcare mapped what areas are 'safe' vs not safe? Does 'safe' refer only to zero pests or are there thresholds we can work to create 'safe' areas...and eventually safe corridors to link the highest quality habitats.

No, we haven't mapped safe. Yes I usually mean it in a pest context, and yes, 'safe' varies by taxon. Kokako may be safe in the Hunuas if pest control is effective there but a kakapo or tieke would not be. I spent my career trying to get people to derive thresholds for safety and have largely failed. The outcome has mainly been that people just grabbed the 5% RTC and TTI that came from kokako work. Very few people have worked to derive thresholds residual pest levels for other taxa. The safety of corridors needs work. For a bird like kokako that is vulnerable as an egg or chick but fairly safe as an adult, then only nesting sites may need predator control. This need not include corridors. But birds vulnerable when roosting MIGHT need pest control in corridors.

Follow up on my weasel comment, would you agree that weasels are one of the most 'underrated' (future problem) pests?

Um, I don't know much about weasels. I have never heard of weasels being primarily responsible for the decline of any species, have you? I guess it might be a lizard perhaps? Are weasels easy or hard to control? I really don't know. They are certainly a priority for research, especially if stoat removal increases weasels.

Adrian Monks

If a fruit hangs on a tree across more than one sampling session, is it counted again? i.e. is the long fruiting season due to fruit keeping better up in the fridge?

Yes, it is counted again. We are interested in the net effect of all the factors on the period of food supply for ship rats, including 'the fridge', lengths flowering/fruiting seasons, diversity of fruiting plants etc. Certainly some of these factors may counteract one another as we get colder and higher, as you suggest, which makes the investigation an interesting one; we are looking forward to looking at the data!

Susan Walker

Should that mean that some 1080 ops should stop if they are trying to help small rat-vulnerable birds? What tools are left?

I think this is the very question the broad scale model will need to answer. Certainly long-interval 1080 is not the right regime for small rat-vulnerable birds (or snails, or bats) in warm forests; nor is long-term stoat trapping ... nor stock control as your work in Waikato fenced fragments showed. But I think it will be interesting and worthwhile to see whether better outcomes can be achieved by better timing, placement and combinations of existing tools if we think hard about it.

I don't think you had stoats in your model yet? Is movement alone enough to explain the measured outcomes?

In answer to the first part of the question, no, not yet! It should be possible to incorporate predictions of stoat tracking rates from a stoat model into the ship rat models and that will be interesting... but we are still collating a spatial database of historic stoat trapping effort (it is a big job!) to make a robust stoat tracking model possible. I am not sure I understand the second part of the question fully, sorry!

There may be nervousness about genetic editing possums as a control/extermination technique (because it might get back to Australia where they belong) but is there any issue with this technique for dealing with rats?

I think it's fair to say that there is considerable nervousness about GE generally. I think the Bioheritage science challenge and/or Predator Free NZ have done some thinking about this - let us know if you can't find anything on line. Specific tools and toxins for dealing with rats are not our subject area.

I have suggested we look at developing non-toxic rate baits using immuno-contraceptive proteins as the binding agent but haven't had much uptake of this idea. Would this be an option in your opinion?

I think this is a question for tools and toxins experts! However, it is clear ecologically that we need tools or strategies that enable us to suppress ship rats longer; we get such short periods of respite for what we do now at large scales. There may be ways to do better immediately (e.g. the approach of creating a kind of mainland island with natural barriers, getting smarter at placement and timing of existing toxins etc) but it seems clear some new tools need to be part of the mix.

Josh Kemp

Do you think these results from aerial baiting might also translate to ground baiting operations? I.e. might we see similar patterns of poor baiting if we continued operations in high growth periods or with the same baits used constantly

Yes I think so. I know two examples where bait stations couldn't control growing rat populations in the presence of lots of beech seed. Rotoiti 2006 (1080 pellets in Philproofs stations), and Dart-Caples 2006 (Brodifacoum in yellow submarines). Thanks for the question.

What is your max prefeed interval?

89 days, at Taranaki Mounga in 2016.

Active growth face an obvious impediment for reinvaded islands being re-eradicated. Is there a difference in ability to manage between high (growing) population density and population growth rate? A shame if we have to wait for pop to peak before we feel we can intervene.

Yes I agree with your sentiment but I don't necessarily think you should wait for them to peak before controlling them in a newly invaded island context. I think the problem will be much less with the anticoagulants one would use on an island, because the rats don't associate feeling crook with the bait they ate days ago. Maybe it would help to have bait stations with non-toxic bait constantly available, so that any reinvaders are already familiar with it by the time you respond to them with poison bait? (Provided you can keep fresh palatable bait, so they don't get put off it!).

Are the three main forest types well represented in the tracking tunnel and aerial application data, any reason to think this would play out differently in any one forest type? When would you time an operation in a "consistently ratty" warm podocarp forest - winter?

The data set is about 15% warm podocarp forests (e.g. Taranaki, Waipoua, Coromandel). I think generally winter or spring would be best for these forests (see Gillies 2003 paper from Whanganui) but it may not matter too much if rats are always bumping around the carrying capacity mark anyway, so a big glut of food might be quite rare? Probably the main one I would watch out for is rimu mast, which could make winter-spring not such a good idea.

Do you believe that different 1080 'flavours' could have a significant impact on eradication success?

Yes I suppose they probably could. I think if I were attempting eradication with 1080 I would seriously consider using a non-cinnamon bait.

Don't you need a fairly high rats/tracking level first to get the stoats killed so often 1080 operations wait until rats are creeping up?

No I think that's a bit of a myth that's built up, unfortunately. I think having too many rodents is worse, especially if they're mice. If a 1080 drop leaves lots of un-poisoned rats and mice then stoat kills are probably compromised. Put it this way, you're better off with few rodents and a high rodent kill, than lots of rodents with average kills. The myth built up because of uncertainty as to whether stoat kills were good when rodents are scarce, not because we had data showing it. As a few more ops have been done recently at low rodent abundance, with good stoat kills recorded, we are keen to move away from the idea of waiting for rodents to build up. Thanks, good question!

James Griffiths

Good point about tracking tunnels not being sensitive enough to pick-up subtleties in the data: no tracks does not mean no rats. What monitoring method would you like to use?

Trail cameras.

Graeme Elliot

The two operations in the Whakapohai block were perfectly timed for kaka breeding seasons (spring 2013 and spring 2015). Why do you not recommend trying to do this at sites where kaka are top priority?

Some kaka bred in years when we predicted they wouldn't breed, i.e., when there was little beech seed or rimu fruit. That means if all operations are timed for when we think kaka might breed we will miss protecting kaka during some of their breeding seasons. It turns out that if we treat (in South Westland anyway) once every 3 years we get about the same result as treating when we predict they will breed. That's because with 3 yearly treatment we will treat during some predicted breeding years, we will miss some predicted breeding years, but we also treat in some years when we predict kaka won't breed but they do. In my analysis treating once every 3 years gave about the same result as treating when we predict they will breed. It's easier to organise treatment once every 3 years than it is to be responsive to beech and rimu seeding, so if kaka are the top priority you can take the easy approach (once every 3 years) or the hard approach (responsive to seed) - either is good.

Did you fit transmitters and radio track fledglings/juvs, if so did their survival differ?

No we didn't attach transmitters to fledglings - sorry no answer.

Have there been any studies on repeated 1080 and Powelliphanta snails (I'm wondering about the "rat bounce" effect)?

There have been no studies looking specifically at this, but the results of repeated snail surveys in areas treated with 1080 suggest that for some snails 1080 is making things worse (as the rat bounce effect predicts).

Anne Schlesselmann

Did you look at juv survival between years 1 and 2?

No. Unfortunately we weren't able to band enough juveniles and our re-sighting ability was very low due to the tall canopy. Obviously this is a critical part of recruitment, so it would be great with potential advances in technology (tiny tags and arrays of base stations potentially) to look at juvenile survival in the future.

How important do you think fruit and seeds are in these bird's diets across the seasons? Also - Were you able to measure double-clutching?

Based on diet studies of both species, miromiro and titipounamu, more 95% of the diet is comprised by invertebrates and particularly during the breeding season invertebrates would be even more important due to the higher protein content. I suspect that fruit and seed would be a very minor component during the other seasons, but we haven't carried out foraging observations across the seasons on Mt Pirongia. Due to logistical constraints of not having enough fine weather windows to fully band a population, we didn't have a fully marked population and therefore could not measure double-clutching with 100% certainty. To our surprise, territories were relatively small and we found nests within 10 m of each other of different pairs of the same species, so it wasn't even possible to determine double-clutching based on territories with certainty.

Doug Armstrong

Do you think the toutouwai would have fared better if they had come from a site with the full suite of mammal predators?

Maybe... We have tentative evidence that experience of predators at the source site affects post-release survival and that was included in the prior predictions. I think we had no evidence of effects on longer term survival or reintroduction.

You found a low impact of predation on survival. There seems to be increasing evidence of impacts on survival as well as fecundity in Beech forests. Do you think this is a real difference or possibly a result of having data from only a single year?

Maybe some confusion here. We have lots of evidence of effects of rats on toutouwai survival, e.g. as well as the data shown for the 10 sites the talk, in the fragments at Benneydale annual female survival probability increased from ca. 50% when unmanaged to ca. 90% when rats mostly removed. But reducing from 19% to 5% tracking is not currently predicted to have a huge effect because 19% is already a fairly low rate.