Day 3 MORNING: Bringing it All Together - Panel Q & A

Asked To	Asked By	Q & A
All Panelists	Philip DeWitt	**Is there a scenario where you would not recommend using an IPM approach?** **Josh's reply**: Time to not use may be related to social capital (cultural); if you made a change a few years ago that cost a lot to the agency in terms of stakeholder support it may not be the time to make another change. When we have minimum amounts of data, the exercise of setting yourself up to apply IPM at scale is useful b/c makes you have holistic thought process about the timing of data collection, how store data, how deal with change in spatial units, movement into / out of unit, counting & shooting same animals etc. The model may not however provide you with much in the end if the monitoring program just doesn't support it. Important for agencies to think about what you want to accomplish and what's required to communicate with decision-makers, stakeholders etc. **Brett's reply:** For some of our species we're not there yet and are relying on minimum counts. We need IPM and data for deer and bear. We won't be able to do this for all species. **John's reply:** You do need estimates that have variances or you're not taking into account precision of estimates. The quality of the data will influence how good model estimates will be. I'm hesitant to put minimum counts into an IPM context. Also an IPM may not be as useful without long-term datasets or where change methods frequently, as this will make it hard to then model trends.Hopefully people think ahead about their long-term goal and standardization of data. It is a challenge with IPMs for caribou to communicate the results and make managers, the public etc. understand what it's doing and how it works. The Bayesian approach, though more robust, makes it harder to explain. The White - Lubow spreadsheet approach could get people to understand it.
All Panelists	Tracy McKay	**Any thoughts on how "priors" or historical/long term data should feed into an IPM? e.g., how do estimates or minimum counts from previous years play a role or serve as a starting point?** **Josh's reply**: Ideally given MCMC (Markov Chain Monte Carlo) and how we want it to behave the starting point should be forgotten as part of our measure of convergence (if it can't deviate from where we started, that's a problem). We want to start our 3 chains in MCMC for example at different starting points and forget the idea of a starting point. We don't explicitly tell or force the model to start at a given # but we ask what the # was in the first year which will be informed by priors which are hopefully independent from the data we're putting into the model. Those priors could include things like expert opinion or local knowledge for e.g. Long-term data are super important; as a rule of thumb we want 10-years minimum when possible. The abundance or things that locate the time series on the y-axis they're pretty rare; running for longer is often helpful. However you may run into changes about how you did things (e.g., how estimated harvest or collected data) that we may not have told the model; those changes can throw a wrench in the works. We can write separate observation models for different time periods.

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		John's reply: we've looked at various ways to start our caribou models. The Bathurst herd has a very long-term dataset, with the earlier datasets being fairly sparse. We've tried putting less emphasis on earlier datasets and also changing prior sensitivity from uninformed to various informed priors (which has had less effect than expected; maybe b/c all priors were in the same ball-park). I agree that if you're looking at temporal trend and have less than 10 years of data, you hope you have a lot of data within that period. Answer depends a lot upon the case.
All Panelists	Tab Graves / Jamie Belt	We had reasonable results with randomly 'subsampling' telemetry data (~ 20-30 locations/individual) to incorporate in SCR. We wonder if this relates to the specifics of heterogeneous distribution of individuals. Do you have any thoughts?
		Brett's reply: I've tried random effects on an individual, covariates on all 3 parameters etc. It's not working for lions now and maybe SRC is wrong and we should let the telemetry drive results. When data from multiple sources don't agree, it's a bit of a problem. I've seen too with sound recorders and e-bird for e.g. and there's different scales there too. If data agrees, just makes things stronger. Need to think about things critically, not just the math.
		Josh's reply: we often run into a particular dataset that doesn't agree with anything else and sometimes you need to throw it out or can work around it. Something to look for. We will run models with data-in, data-out in multiple iterations.
		John's reply- my experience with SCR is more with bears. The tricky thing with bears is that you can get some bears that move a ton more than most others, and it can throw a wrench into the SCR. We've used an exponential detection function as one way we're tried to allow the tail to extend further but it makes you wonder how the 2 methods are capturing different processes. SCR samples a lot of individuals with very little information whereas telemetry samples few individuals with a lot of information. Trying to marry 2 datasets can be difficult.
Josh	Cassie	What is the minimum amount of data required to use IPMs effectively?
		Josh's reply: It depends on your tolerance for risk. You can turn the IPM in a Bayesian context into a simulation model by providing very informative priors and say a minimum of harvest information and maybe some ratios, but results won't be strictly data-driven. In general we find that abundance is the most important parameter and then we tend to do the power analyses and cost-benefit analyses to help agencies determine what else they need and how often they need to collect that data. All the results lie on some gradient between simulation and purely data driven inference. You need to decide how much you want to pay to not be wrong, how precise you want to be, and how that fits into your decision-making process. There isn't a strict statistical answer. I would like to see harvest data every year and ratios and something about abundance; say abundance b/c most abundance isn't stage structured but if you had stage-structured abundance it would fix a lot of problems.

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		Brett's reply: there might be other reasons you have a threshold for what % of the population you're willing to harvest. For bighorn sheep we have regulatory language that says that's 15% of the mature rams. In the past we've used minimum counts so you could always use the lower end of the confidence interval whatever the precision is. **John's reply**: hard to generalize. When we've attempted to fit IPMS to very sparse data the CL on the predictions with IPM are very large. What I really like about IPMs is that they keep you close to your data. Simulations are a great exploratory tool but they're not as data-driven or as data-limiting as an IPM. The fact that IPMs are data-driven is both a strength and weakness- more a strength; it's a strength b/c it prevents you from moving too far beyond what you know.
Josh Novak	Anne	Can you share some case studies where you've used Hierarchical Models? We do have models where we've done random effects in space, time and both. That allows you to get a consistent estimate for all your DAU / WMUs that also sums to the total for the state. Whereas if you run each unit independently and sum of them, and then run a state-wide model, they may or may not agree. Using random effects on space and time, sometimes it's hard to get enough flexibility to get what the expectation is b/c the random effects always smooth things. The approach can also be useful when historically data has been collected at some smaller scale that scales up to a larger analysis unit (e.g., GMU fits within GMA, or GMU fits within a region). In these situations, you can model the populations of the individual smaller units and then scale them up. For example, you could use data from an aerial survey of a GMU in a given year that doesn't cover the whole population vs. throw out the data. This approach does add a lot of parameters. Third example is from Mark Hurley's dissertation on juvenile survival in mule deer where he fit a bunch of covariates to survival data and put those into an IPM and let it inform in space. This approach may not be flexible enough. We are looking at machine learning techniques for fitting the survival models to give us flexibility. It's useful and may have application to multi-species models. For example, a bad winter for elk may also be a bad winter for deer; the survival values may differ between species but there's information to share there. We don't have to independently say it's a bad winter for each species if we include both species in the same model.
John Boulanger	Philip DeWitt	You pointed to some data types being particularly impactful. Can you speak to how that has informed monitoring priorities? **John's reply:** We have been most concerned about the survival rates of the adults. We increased collar sample sizes to get a better sense of that, which also provides ways to manage harvest. The Bathurst herd is so low that they have a mobile zone where they track females's locations,which forms a protection zone that varies across the landscape. That's one of the things that is certainly informed. The challenge with caribou is that the cost to do any of these is quite high. The IPM has been very useful to better understand collar

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		survival rates. There are inherent issues with collar survival rates in general and especially with caribou b/c determination of fate is challenging and there are only 20-50 collars over large areas so coverage is low.
Brett Furnas	Philip DeWitt	Your study area covers a wide variety of ecosystems that could influence wildlife behaviour, demography, and responses. How have you had to adjust your IPM to account for different systems?
		Brett's reply: We don't have a single regression for the entire State of California but are breaking it up into different regions and having different covariates (e.g., human density).We are looking at papers where one can have a single model with random effects that allows covariates to vary among regions. With the IPM, we've done this with bears; age structure data, the covariates and parameters can vary among the 6 different bear conservation regions. We are also using masks (e.g., valley bottoms) for extrapolating SCR and the composition etc. across the entire state.
All Panelists	Anne	Can you talk more about the challenges of integrating telemetry data into spatial capture recapture models that use genetic or camera detections?
		Brett's reply: I discussed this in my 2018 paper a bit wrt deer. I've tried to do this with larger datasets with deer and mountain lions when scaling up with fecal DNA across the entire State of California. There's a lot of telemetry data for mountain lions across the state and I wanted to put a lot of covariates on the spatial scale parameter b/c I expected lion ranges would vary across the state (as supported by telemetry data). I fit a bivariate normal model on the telemetry to estimate directly a spatial scale like an SCR (spatial capture-recapture) (not going through home range kernels or the like) which fit well. I then used that as a highly informative prior on the SCR and it just biased it up b/c fundamentally the mean spatial scale for the telemetry did not agree with the SCR scat. We had so much modeling data we were able to fit all the same covariates on the spatial scale parameter just from the scat alone. Even if you have only an average of 2 recaptures per individual, SRC is very robust. There's literature that says don't trust the biological implications of what the spatial scale parameter and SCR says, but the density estimates are pretty robust to a lot of things. I would like to integrate the telemetry but at this point it maybe needs a totally fundamentally different sampling process. The temporal scale is different and there's a lot to understand. It would be great to integrate the telemetry data but I don't know how to do it.
		Note- our study design differed from that of most SCR studies that typically have an intense grid where can capture individuals across the grid. Our transects were independent of one another and treated like different sessions b/c we wanted to make inferences across the entire state. We were looking at recaptures within a transect.
		Note- we use "EarthRanger" / "MoveBank" for managing collar data.
		John's reply: I agree that most of the literature suggests that the spatial scale parameter should be taken with a grain of salt and that it can vary with many things but that density is very robust. With bears, I've tried to integrate fecal DNA and telemetry and found challenges. The DNA sampling for bears is across a certain

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		time period and is a cumulative range across. It seems that it's hard to match with telemetry. More research would be useful as fecal DNA and telemetry should be able to inform one another.
		Josh's reply: For New Mexico mountain lions it worked out well and didn't change the estimates much. I'm not sure why but perhaps the lion habitat was homogeneous in NM.
Brett	Marcus Becker	Can you provide more details on how you use the camera data to get doe/fawn ratios?
		Brett's reply: People are reviewing the camera images and counting up the minimum # of unique deer in a 1 hr sequence of photos to give us minimum counts by class. This could be determined by for example the largest group within that 1hr sequence of photos or from buck antler classes etc. We're also using fecal DNA to determine if pellet size can indicate age or adult/fawn.
		Followup from Philip: Are you also looking at how these ratios are changing; loss over time?
		Brett's reply: Yes, we're looking to see how fawn: doe ratio decay from June to Sept, and we want to have the cameras out year long. This will allow us to potentially do fecal DNA collection in Feb. and make it comparable to the camera data.
Brett	Embere Hall	It sounds like deer hunting seasons are set annually in California, correct? If so, how do you disentangle yearly changes in season structure from other aspects that also change annually (e.g. weather) in order to inform adaptive management?
		Brett's reply: My role is not setting harvest quotas (that's CA Fish and Game Commission embedded in a democratic process; we make recommendations only). We need population numbers and are working to update all our management plans for all species that have the same IPM adaptive management framework in them. There's a policy and science side. We need to make sure that these IPMs and management plans that they're linked to are in place first so it's transparent how we use data.