

Rich: yellow highlighted items are either action items (changes to protocol or further research questions) or areas that I need you to check my layman's summary.

Frank's detail as interim leader ends in 2 weeks.

- stick to the science...no policy or values.

brief overview of 9th circuit decision

Obj. 1: review of mark-sight estimator

Obj. 2: demographic review & update using 2002-2012 data

Obj. 3: changes in demographic parameters & potential causes

Obj. 4: review mortality criteria – zoning? change criteria?

suggested Obj. 5: research questions & strategies to answer uncertainty arguments more effectively

9th circuit took issue w/ all 3 of our arguments regarding WBP:

1. individual vital rates and their relationship to lambda: had a hard time understanding that poor WBP years can be related to increased conflicts and decreased litter size the following year w/o having an impact
2. omnivores – yeah, maybe they switch foods in poor WBP but is that putting them in direct competition w/ hunters and getting them killed?
3. comparison w/ NCDE – need to figure out how to make this very valid comparison w/o undermining our unique ecological setting argument for the DPS (the unique ecological setting argument isn't necessary to the DPS analysis). Need to make it very clear this unique ecol. setting does not render comparisons with the NCDE moot.

Obj. 1: review of mark-sight estimator – H&W method

Issue: population estimate is biased low and it's more biased as population density increases

Summary of how we do it now: Unique FCOY + Chao2 = Total # FCOY...

- o Demographics flow chart (1983-2002) Frank showed
 - couple of components have changed and will be need to be updated to set sustainable mort.
- Higgs & White tried to get at F(cubs) and bypass other components historically used
- Eval. of mark-resight estimation of N(fcoy) using aggregated aerial survey data 1997-2011 (should get this ppt. from her for Rich's summary)
 - Obj's:
 - o describe model
 - o implement the model (exact & approx. versions)
 - o compare results to current Chao2 estimates

- discuss sensitivity to the assumptions and future directions

describe model:

- data used = marked FCOY & unmarked FCOY; assumption that collared bear can only be seen once during a single flight (this holds true w/ data used)
 - m = # of marked FCOY in a given year
 - S = total number of sightings of unmarked bears
 - $Y(0), Y(1), Y(2)$ = # of re-sightings of marked bears (I think...need to confirm)
- max. # of marked bears = 9 (2001); lowest # of marked bears = 3 in 2005, 10, and 11
- lots of 0's for re-sightings
- 2 parts of the model
 - sightability model – model for the marked bears
 - model for unmarked sightings – the data we wished we had; what we really see is the S , the total # of sightings of unmarked bears
 - assumption that we have equal prob. of re-sighting marked and unmarked bears relates the Y and the Z (resightings of unmarked bears)
- aggregated data
 - yearly data are sparse, not enough info. to implement mark-resight analysis
 - can assume probabilities of being sighted do not change over time so we can combine data from 1997-2011.
 - $M = 79$ total marked FCOY over all years, $Y = 59, 18, 2$
- Assumptions:
 1. prob. of re-sighting remain constant over time (i.e., can aggregate data)
 2. prob's of being sighted 0, 1, or 2 times are the same for collared and uncollared FCOY
is this true? Most uncollared sightings of FCOY are from moth sites
 3. closed population of unmarked FCOY within each year
 4. max. # of sightings of any one FCOY is 2
- Methods for fitting the model – Gary & Higgs have a paper describing these and she did not go into detail.
- Disclaimer: all results presented are approx. and exploratory
- next slide: 95% Posterior intervals and medians (97-2011) figure; used same sightability model for every individ. year (last bullet from Aggregated Data slide)
- next slide: Posterior distributions of $N(\text{fcoy})$ for 4 years using different S values. Same S value will give same results.
- next slide: Posterior distributions of the prob's – shows prob. of being sighted 0, 1, or 2 times (based on 79 bears over 15 years)
- next: Prob. of 100 or more uncollared FCOY?
 - goes from 0% chance in 1999 to nearly 100% in 2007. This may be hard to swallow.
- next: Compare to Chao2 estimates for 97-2011 (not the model avg'd, just the raw Chao2 est. of $F(\text{COY})$)
- next: Relationship betw. $N(\text{fcoy})$ and S figure. Flatness of Chao2 is due mostly to the nature of the Knight et al. rule set
- next: Compare results to other mark-resight models

- Bowden came out similarly even though it's the most simplistic

Assumptions (a closer look at #'s 1 & 2):

1. prob. of re-sighting remain constant over time (i.e., can aggregate data)
 2. prob's of being sighted 0, 1, or 2 times are the same for collared and uncollared FCOY
 - is this true? Most uncollared sightings of FCOY are from moth sites
- next slide: Moth sightings. The majority of unmarked sightings occur at moth sites while very few of marked bears occur at moth sites.
 - next: Flight vs. ground total sightings
 - next: sensitivity to sightability model
 - what if unmarked bears are more sightable than marked bears?
 - actual data $Y = (59, 18, 2)$
 - Scenario 1: Assume 6 FCOY are sighted more often $Y = 53, 23, 3$
 - Scenario 2: Assume 10 FCOY are sighted more often; $Y = 49, 26, 4$
 - next: Sensitivity to sightability model figure. Both scenarios lower the total estimate of $N(\text{fcoy})$
 - more precision around our prob's of being seen fewer times
 - Scenario 2 makes estimates about 30% lower
 - could you also game the number of sightings? (i.e., assume marked bears are more sightable?) Gary says this doesn't make sense because of rigorous aerial survey protocol.
 - could you grid off the area to determine how many marked bears are in each area to determine "how representative" marked bears are of unmarked bears? Then, adjust the Sightability model to account for these results. Gary says data are too sparse (79 bears over 15 years)
 - Is there a bias introduced by the pilot's previous knowledge and experience? Meaning, does this bias marked bears to be seen more frequently? Ans: Marked bears are only visually sighted only about 10% of the time even when searching for their known radio frequency.
 - Can you just remove the moth sites from the sample? Response: Would make the sample too small. 5 marked bears that were observed on moth sites would have to be thrown out too.
 - there are Flight areas where bears are easier to see than others (timbered vs. open)
 - what about a covariate about how many radio animals are in a quadrant so you could adjust count of unmarked animals based on that ratio. If you've got very few animals marked in an area where you see a lot of unmarked animals, the sightability isn't the same and needs to be adjusted. Gary will think about how to do this but still thinks this type of partitioning will make the sample(s) too small
 - a lot of variability is from normal variation in the # of FCOY each year
 - if we set the moth sites aside, can we have a discussion about whether the remaining data meet Assumption #2? i.e., Have a different method to monitor moth sites independently. Response: sightability is so low over the ecosystem, that you may be basing it on very little data. Think about sightability models for elk and deer...they make it once and use it forever. We've improved because ours is empirical. We know there are annual differences but these kind of "wash out," on average.

- another thing to consider: timing of flights changed on 2002. Prior, initial flights occurred before bears were on moth sites, 2nd flight did detect some bears on moth sites though. Starting in 2002, emphasis was to find as many FCOY as possible so flights were delayed to occur when bears were on moth sites.
- if you split up your data, you're making the assumption that the ratios stay the same over time. Are there differences between the 1st half of the dataset vs. the 2nd half? Response: not significant and maybe not even detectable. Gary ran a "Fishery Zach" test, very powerful, and it did not detect differences...definitely not a trend.
- maybe the assumptions don't matter if we find out the data is so sparse, that it is so sensitive to any stochastic event
- another assumption: observation flight effort is constant over the 2 flights within the year and across years. This could vary based on weather or fires, etc.
- we're also assuming the moth sites are of equal value to bears every year...which we know not to be true.
- could we add another column of "bears seen not on moth sites"
- could we use marked males to expand our data set? No, observation flights are only looking for FCOY. Unmarked, non-FCOYs are not documented. Study in 97-98 attempted to do this: turn receiver on for every bear seen, not just FCOY...eccentric results. Estimator went through the roof due to poor re-sights. Pt. est. of 1200 bears.
- What about doing this again in the future to estimate N (all bears male & female). That would introduce an additional assumption that the marked sample is representative of all bears.
- Or, stick w/ estimate of FCOY but use all sightings of collared bears if you assume they have the same prob. of sighting as marked FCOY. Would it take much more time if they turn on the receiver any time they see a bear (instead of after they see COY).
- Another logistical hang up is that males move a lot further so it would take a lot more time on each bear to figure out who it is. Is there an equip. solution? Maybe.
- 2 issues really: a retrospective analysis vs. a future monitoring system? Response: Not sure if you can untangle those 2.
- Could you take GPS locations of females on dates to see if collared females were there but not seen? REsp: we only have GPS collars on about 13 FCOY. May not be useful with this small of a sample.
- if we transitioned to ARGOS upload collars, we could re-do the 97-98 CMR study...not just w/ FCOY but all marked bears
- why is closure an issue? Response: It's a bigger issue w/ males and larger home ranges. Wouldn't be as much of an issue if you transitioned to ARGOS collars over time.
- during break, Gary ran analysis w/o moth site bears. (red/brown is w/o moths; blue is with moths). Seems to actually smooth the data because it removes the annual variation associated w/ the moth sites. Used the Bowden estimator...it's the simplest. It makes the no distributional assumptions, it's a non-likelihood estimator.

- If there's a year where there aren't many non-moth site sightings, what does that mean? Mark's sense is that they "generally track" In years of few sightings, there are fewer sightings on moth sites too.
- Why aren't marked bears seen on moth sites? Not sure but a few theories: it's harder to trap female bears w/ HR's near moth sites because they're very remote. FCOY were usually (90%) collared before they had COY.
- Any room for a post-hoc correction factor? Resp: Flight 1 and 2 are really about 1 month each.
- Could we use other data we collect already e.g., FCOY/hr/flight and Total # bears/hr/flight to get at any of this? Total bears/hr. has been increasing in recent years. Resp: Will still have a lot of variability.
- Other issues: observer bias...sometimes it's the same person, other times it varies. Some observers are better at seeing animals than others.
- Could bring in all the co-variables to "smooth" the data because smoothing will always be necessary for the purposes of mortality limits.
- Any conclusive statements or decisions to be made?
 - even if we feel comfortable w/ Assumptions, is the level of variability acceptable. We know the CI's around Chao2 estimates are "ludicrously small." This notion should be captured in the summary. Error is not completely propagated through the entire system.
 - Can the numbers here be used as "real" estimates? Is the difference between Higgs&White estimate be used to re-calibrate the Chao2 estimate? If we decide to use this, are there ways to improve it...maybe group collared males w/ unmarked bears but include all marked females, w/ offspring or not. Possible drawback = these flight periods are really about a month long, you don't sample the entire ecosystem in 1 flight...females could move around.
 - bears stay on moth sites for about 6 weeks. If we don't include these, we'll still be underestimating. Chao2 is not only biased low, but it increasingly biased at higher densities. This would not be.
 - If they're w/in 500m of a moth site, they're considered to be part of the moth site. Not a very big area. Could also buffer sites by 1500m. Mark says it would make that much of a difference.
 - 1986 is first time moth sites were "discovered" We have about 52 now and they're all in the SE corner of the ecosystem (between Dubois & N.Fork of the Shoshone). Several sites have merged into "complexes" and they change from year to year. Sometimes, known sites can be covered by snow through the use season.
- quick recap: there is potential for this technique. It can be used as a trend. Can we go backwards and what improvements can be made? Dave's idea of scanning for all marked females may be a good one. Need to really understand what the relationship between the H&W method vs. the Chao2 is if it's going to double the population estimate. Response: We've been saying for years that the Chao2 is biased low. The media hasn't ever reported it as being biased low.
- some perspective: We've had around 90 bears marked for some portion of the year for the last 10-12 years. Between 2002-2011, only 36 bears w/ collars died. There are a lot of bears out there.
- need to do some sensitivity analyses too to see how important the imprecision is? How long to detect a downward trend? Would it have to be a steep decline? Resp: For an S of 30, CI = 67-206.

- to some degree, marking is always a little behind population expansion. Resp: Expanded sampling efforts account for this. We just don't have as much success in these areas of low density. In that sense, our radio-collared sample is a density dependent sampling method.
- Is there an issue as bears move into peripheral habitat and sightability drops?
- Have we accounted for all possible causes of a radio-collared bear not being detected...specifically, pilot error or frequency error? Mark says yes. Same 3 pilots from 1997 to about 2008. Now, it's the same 3 guys + 1 new guy for the last 3 years therefore this dataset is pretty solid.
- should we be looking for an entirely different method? DNA-based effort.
- Or, maybe we could generate a mortality limit based on monitoring female survival and lambda instead of a known-to-be biased low population estimate? There's a CI around lambda too though.
 - If lambda is more precise, maybe it's more appropriate than an actual number.
 - maybe make the mortality limit # a guideline instead of a standard? Acknowledge how dubious this number is and that we can't be held to it anyway...but how would hunting limits be set?
- Is it more reliable than Chao2? Is it more representative of the population? Gary says yes.
- Megan showed a preliminary graph of what the H&W method produces if moth sites are excluded
 - What if you had 2 bears w/ 2 re-sightings instead of only 1 (what we have now). Could come down quite a bit and this is a concern that 1 bear can have that much influence.
 - what about occupancy instead of mark-recapture methods?
- Is it appropriate to make mgmt. recommendations based on a pt. estimate when the CI's are so large? Resp: need to combine multiple models together...there are books written about how to do this. Build a mega-model.

Obj. 2: demographic review & update using 2002-2012 data

- review triggered by shift in weight to quadratic model
- wanted to compare to Monograph data and treat it as an indep. data set to prevent historical data from masking recent trends
- 2002-2011 = data set
- cub survival $S(0) = 0.553$ (.42-.667)
 - derived from monitoring of radio-collared females
- yearling survival $S(1) = 0.538$ (.345-.697)
 - derived from monitoring of radio-collared females
- A = unk. mortalities removed from calculations = censored
- B = unk. mort's assumed dead = AD sample
- subadult survival $S(2-4) = 0.911$ (C) vs. 0.878 (AD)
- adult survival $S(5+) = 0.95$ (C) vs. 0.939 (AD)
- 2002-2011 indep. bear survival $S(2+) =$
 - need to know this number to compare to monograph
- proportion of FCOYs in the population = 0.269
- mean litter size = 2.12 cubs/litter; adjusted for mortality of cubs before 1st observation (56 days) = 2.49 cubs/litter

- fecundity $m(x) = 0.336$ (# of cubs/female bear/year) vs. 0.362 in Monograph
- litter interval
 - o 2.78 years in Monograph
 - o they did not calculate this yet but they will
 - o would expect a decrease in interval since cub and yearling survival decreased and females are available for breeding more frequently
- λ
 - o = 1.015 (C) (.953-1.052)
 - o = 0.999 (AD) (.936-1.037)
- $\lambda(\text{MC})$ includes process and sampling variance
 - o = 1.005 (C)
 - o = 0.988 (AD)
 - o what's the chance the population is actually decreasing? (the same way Mace et al. reported it based on 10,000 MonteCarlo simulations)
 - o AD sample includes a couple of bears that they're pretty sure are not dead but they didn't have time to go get the collars in 2011 before doing the calculations
 - o is the age distribution truly stable? Should you report the asymptotic or the realized?
 - o matrices make the asymptotic assumption, not the Monte Carlo simulations so these values are "true" regardless of whether the stable age distribution assumption is met. Gary & Rich continued discussion about exactly whether they made stable age distribution assumptions and agreed that they did make some. Rich, need your help here??? It might be worth thinking about how to project another lambda to reflect the changes in juvenile survival. Mark said we were already going to ask Rich to do this and this would likely produce an even lower λ estimate since fewer individuals are making it to the adult stage.
 - o IMPORTANT take-home message: these estimates of λ are probably biased high and will likely change, maybe slightly but they will decrease once the lower juvenile and cub survival is taken into account.
- λ inside YNP (1983-2001) vs. (2002-2011) = 1.054 vs. 1.018;
- λ outside YNP but inside RZ (1983-2001) vs. (2002-2011) = 1.121 vs. 1.032
- λ outside RZ (1983-2001) vs. (2002-2011) = 0.887 vs. 0.960
 - o this increase in λ could be because bears are long-lived and the ones that live on the periphery have figured out how to coexist with humans
- what proportion of the population lives in each of these zones?
- how would you parse out the density vs. resource effects...it's the same thing? per capita resources.
 - o the timing of the changes could provide some insight...if the changes were occurring before 2002
 - o What would you expect to be different if resources had been the same but population had continued to increase?
- male survival may have increased because of wolf-killed carcasses being available year-round or because they didn't use WBP as much in the first place

- if certain parameters relate strongly to WBP and those same trends are showing up here, it may indicate it's an impact from WBP declines
- any information on infanticide?
 - o not direct info. but transition prob's haven't changed too much. But that would only reflect changes if entire litters were killed, not if only 1 or 2 of the cubs were killed
- any evidence of weaning yearlings earlier 2002-2011? Yearlings were assumed dead if they weren't with their mother. Response: No, because subadult class has not increased (at least not in capture record).

Higgs point-change analysis

- in 2011, AIC weight shifted. This analysis asked, "when did that shift start"?
- 29 years (1983-2011) – point of inflection at about 2000
- $\lambda = 1.04$ for entire time series
 - o 2008-2011 = 1.012 (check this value)
- change/break point model
 - o assumes there is 1 break point in the time series
 - o break-point between 2004 and 2005
 - o if you remove outlier (2005), breakpoint is 1999
- broken line model
 - o does not converge but puts it at about 2002
 - o if you remove outlier (2005), change occurred about 2003-2004

now, analyses for 1997-2011 only:

- 2008-2011 $\lambda = .93$ (0.8-1.09)
- bottom line...shows a declining population, not just a stabilizing population
- Generally, anytime you take the last few years of a stabilizing population, it will show a decline in the quadratic equation and you cannot infer biological meaning from this.
- there is usefulness in always thinking about how the time series you select will influence your results
- if it was only a byproduct of the rule set, it wouldn't ever lead to a decline, just an asymptote
- are conflicts still increasing even though the pop. is either stabilizing or possibly declining.
- 2007 seems to be the point at which WBP mortality peaked
- while you will always find a break if you look for one, there is some value in looking at when/where the change is greatest

Obj. 3: changes in demographic parameters & potential causes

Overview of analyses:

- % body fat
- cub & yearling survival
- independent bear survival
- transition rates
- human-caused fall mortality counts

- hunting-related mortalities – landscape characteristics
- Covariates
 - WBP = pine cone count from annual transect surveys
 - WBPadj = WBP index adjusted for tree mortality (proportion of trees alive X seed production for that year...a surrogate for kcal avail. on the landscape)
 - WBPyear = # of trees > median count; good or bad (discrete not continuous variable)
 - WBPalive = % of surveyed trees alive; (e.g., 72.6% in 2011)
 - Chao2 = annual Chao2 estimate; surrogate of pop. size
 - MAC2 = model avg. Chao2 estimate
 - PDSI = drought severity index (May-July)
 - SWE = April 1; normalized; surrogate for winter severity and ung. carcass availability
 - LLNz = Lilac leaf-out date (normalized); surrogate for spring onset
 - yr02-11 = period 2002-2011 (i.e., WBP decline period)
 - PoYinRZ = proportion of locations outside YNP but inside RZ
 - PoutRZ = proportion of locations outside RZ
 - Modifiers = -1 = 1-year time lag
- if there were changes in the following parameters, what would we expect them to do under the 2 scenarios of decreased overall food production vs. purely density dependence effects?

Change in parameter	Food production (+/-)	Density dependence (+/-)
% body fat	Decline	Decline
Cub survival	Decline	Decline
Yearling survival	Decline	Decline
Subadult survival	Neutral	Neutral
Adult survival		
Transition rates (No cubs to Cubs)		
Human-caused fall mortality	Increase	Increase
Home range size?		
Transition from no cubs to no cubs	Increase	Could increase too
Disease?		

- body condition varies naturally by time of year, sex, and age. It would be affected by the food produced in both the current year and the previous year. Could you separate sample out by time of year sampled...or assess trend in body condition from July 1 forward. No, this would require multiple samples from 1 bear over the entire year.
 - 3 competing hyp's for % body fat = (1) resource/capita; (2) behavioral/social interactions presumably indep. of food production; (3) likelihood that bears will get into trouble with humans
 - also affected by food quality
 - in MN, they actually see healthier bears in poor food years because they switch to riskier foods like ag. foods. Then, they switch back the next year and will have lower % body fat even though it's a good food year. They've shown a similar effect with black bears in Aspen.
 - estimate is that we lost 28% of WBP in the 1988 fires

- for cub survival, you would expect annual variation in food production to track cub survival vs. a density dependent effect would be constant across all years
- if you look at an individual response variable that reacts immediately to food production, this would not be consistent w/ a density dependence effect...wouldn't expect a low r species like grizzlies to change this fast due to density dependence alone...would be more of a food effect.
- reality is that K isn't fixed for bears. It varies annually.
- If WBP goes extinct, you'll be able to answer these questions in 20 years.
- When the dumps closed, some folks predicted total extinction by 1992. But we don't have any data from these years because research trapping stopped.
- if we can't tease out the effects of WBP vs. density-dependence, is there any way to defend delisting? Not sure yet but brown bears are extremely omnivorous and bears are doing fine in the NCDE w/o WBP.

Feb. 2, 2012

% Body Fat analysis

- 2000-2010 captures, n= 380
- research & mgmt. bears (livestock conflicts only)
- AIC table for May-August data
 - o most imp. to May-August % body fat = proportion of trees that are alive on some transects the year before (but this doesn't indicate the pounds of seeds available to bears on the landscape). This reflects a temporal trend.
 - o kcal of seeds on the landscape would get at the mechanism more effectively
 - o tree loss has been patchy
- WBP alive trend...really leveled out 2009-2011; transect trees are the biggest trees out there (i.e., reproductive, not the tiny trees). This should not nec. be interpreted as an overall trend. It does not include recruitment. The large cone-producing trees are the ones getting hammered by MPB.
- used the top model to predict what % body fat trend would look like.
 - o something about this graph doesn't make biological sense...why would the WBP effect be meaningless outside YNP? Does that indicate that it's not really an important variable? Why does it get so much AIC weight?
 - o anecdotally, trappers have indicated that bears inside YNP in recent years are lighter than what they used to be.
- Would it be useful to plot individual bear's profiles through time? Megan thinks so.
 - o How many of the 380 bears have multiple BIA measurements?
- Is WBP mortality higher in YNP? Not sure.
- AIC table for Sept-Nov. data on % body fat
 - o WBP alive + ln RZ + WBP alive * ln RZ
 - o graphed the top model. If they're outside the RZ, condition goes up as WBP goes down.
 - o graphed the 2nd model: they pretty much track each other inside and outside YNP
- note to Rich: this is my layman's summary of Joe's point...please review for accuracy...We need to be cautious because WBP is the only food variable included and we know changes in body fat are

related to food availability on the landscape so maybe we just didn't include the right food variable.

To think some long-term trend would overcome natural variability in body fat may be a mistake...but that's what the data are suggesting.

- does lower body fat continue to affect bears for years?

Cub & Yearling survival

- nest survival model.
- 1983-2011 (302 encounter histories)
- AIC results: significant negative relation w/ Chao2 estimator from the previous year. WBP_{adj} in the previous year also up there)
- we're seeing differences between cub survival outside YNP inside RZ and elsewhere but the top model shows cub survival has increased over the entire time period but they tested for differences in cub survival between the 2 time periods (1983-2001) vs. (2002-2011) and found no significant diff's. At first glance, this seems inconsistent with the demo. data presented yesterday showing annual cub survival had decreased between these 2 time periods from .650 to .553. Mark says this is most likely related to the transition prob's.
- take-home: cub & yearling survival varies year to year but there's no clear trend over time

Indep. survival – both sexes

- 1992-2011, 1493 encounter histories
- AIC results: proportion outside the RZ and the time period are the top 2 variables
- graph: survival outside the RZ is catching up w/ survival inside the RZ. This is consistent w/ the different spatial λ 's presented yesterday

Transition prob's

- 1983-2011; 172 encounter histories
- base model from Schwartz and White (2008)
- AIC results: WBP_{adj} for transition from no cubs to cubs is top variable;
 - does this mean WBP affects litter production, not just litter size? Yes, that's 1 way to interpret it. They are less likely to have cubs at all. This is diff. than what the Monograph reported.
- Megan showed graphs of relationship between Chao2 and WBP...after 2000, they seem to be negatively associated

Fall mortality counts

- human-caused mortalities (Aug-Nov.) from 1983-2010 of 2+ aged bears
- 94 f and 108 males
- for females, there's a pattern associated w/ WBP production; for males, similar pattern but less pronounced.
- AIC results for females: lots of models pretty close (top model only .24 weight). All the top models are for variables that include variation...WBP_{adj} + MAC2; WBP_{adj} + Chao2
- WBP_{adj} is strongest variable influencing fall mortality of indep. females.
- for males, WBP_{year} + MAC2 (43%); WBP_{year} + year (34%); WBP_{year} + year + year² (10%)

Hunting-related mortalities (more of a spatial analysis)

- 1985-2009

- looked at 81 different locations of bear mort's vs. 1300 random points to assess indep. variables (slope, canopy cover, solar insolation, distance to forest edge, dist. to roads, dist. to trails, dist. to hunting camps (permitted outfitter camps), WBP year (0,1), WQBP decline period (2002-2009)
 - o disclaimer: GNF data for outfitter camps was wrong and is being updated
- had to compare to 1300 random points because very few collared bears died in this way so it's not a survival analysis (like Chuck's)
- in good WBP years, mortalities are more likely to occur further from trails. In poor WBP years, mortalities occur closer to trails.
- in poor WBP years, hunting-related mortalities more likely to occur closer to hunting camps

Any conclusions??

- hard to get anything meaningful if we don't include earlier data. Don't have to include it in estimates of λ but in these exercises where we're trying to tease out WBP effects vs. purely density-depend. effects. This was already done for all the above analyses except % body fat and indep. survival (went back to 1992)
- This stuff is only correlative, not causative. But, if we accept a causal relationship w/ WBP, the most we can say is WBP affects a lot of vital rates...and we already knew this. The question is how are these bears going to adapt to WBP loss in the future and we are not going to be able to do this.

Do we want to move forward w/ the H&W estimator?

- we can reasonably defend the assumptions? Mark H. says yes.
- still need to do a power analyses to detect downward trends before making final decision
 - o Harris et al. 2007 determined the shift from the linear to quadratic would detect a downward trend within 5 years...Joe worries with the much larger CIs of the H&W estimator would be pretty "powerless"
- could move forward with this one and still use the Chao2 until we feel more comfortable
- don't we just need to be certain the population is above 500? Yes, but we continue to exceed mortality limits because the Chao2 is biased low
- Megan says if the population goes down enough, the H&W estimator would be ineffective if we had 0 sightings of marked females? So, Chao2 is better at lower densities and H&W may be better at higher densities.
- explore Dave's idea of expanding the marked bears to include all marked females, not just FCOY.
 - o diff. reproductive statuses may give them different sightability prob's...FCOY may be more likely to stay in cover? But this may not be the case...lots of FCOY on moth sites and you would expect them to avoid these if they really were avoiding being seen.
 - o What about just using marked females w/ offspring (i.e., include females w/ yearlings)
- do we feel comfortable in the sensitivity of λ to assess whether mortality is sustainable (no specific mortality limits)?
- Rebecca's question: Would the H&W estimator still indicate a change in λ in recent years like the Chao2 does?
- maybe use the H&W estimator to set mortality limits and the Chao2 estimator to detect a downward trend?

- is it valid to exclude the moth sites? What about ground observations of moth sites? For sightability model to be valid, it has to follow the same sampling effort. This is logistically challenging since they're in the most remote areas of the ecosystem.
 - Maybe fly all sites on 1 day and add those sightings to other re-sighting data.
 - if you're assuming that once they get to a moth site, they stay there, would you have to fly them all on the same day? It's safer since many are pretty close together. Could maybe lump them together and do over 3 days with 1 plane.
 - how much variability in # of bears between moth sites? Lots.
 - Could we estimate detection prob. of known bears, so that we could build another sightability model for moth sites? could even maybe do this from ground observers.
 - critical question: are these really 2 different "populations"? only 1 that was seen both on and off a moth site using current data. Could use GPS data from bears known to use moth sites to create a buffer around which we exclude those bears if they're seen on the non-moth site flights.
 - could also try to boost # of radio-collared bears to increase sample for H&W estimator
 - still need to figure out a way to smooth the inter-annual variation in the data
 - need to determine the geographic boundary on this too
- hesitant consensus to move forward w/ the H&W estimator & that for 2012, we'll still use the Chao2 until this method is published and peer-reviewed
- what about if we plug in updated transition rates and ratios to get a new population estimate?
 - $N(\hat{h}, 2011)$ using old rates and ratios = 593
 - $N(\hat{h}, 2011)$ using updated rates and ratios = 702 w/ a 6% female mort. limit (this changes from 9% based on new info.)
 - female limit for 2011 = 15 (32 occurred)
 - male limit for 2011 = 36 (24 occurred)
 - increased male mortality flexibility but reduces female flexibility...but increasing male mortality may benefit females by reducing resource competition and/or conspecific predation....maybe.
- why don't the known-fate estimates of male survival (95% = 5% mortality) align w/ observed mortality. There's been more than 5%.
 - related to the fact that known-fate estimate only uses research bears
 - also related to the bias in the Chao2 only allows lower sustainable mortality limits since it biases pop. size low

Boundary discussions

- there are several places where the suitable habitat line doesn't align with the recovery zone
- to address this, they took the annual activity range of a female (6km) and buffered the suitable habitat with that to include the Paradise Valley and other areas that may be sinks to bears from inside suitable habitat. This smooths the suitable habitat line.
- Deferred this issue to the Study Team. Everyone (but me) agreed to drop the paradise valley, the upper green river, and probably the Shoshone

Can we tell the difference between effects of WBP and density dependence? Survey around the room:

- Yes, 1 = Mark H.
- No, cannot tease apart these two effects: 18
- we need more clarity on what we should be measuring and researching to make sure we are ahead of the curve (e.g., % body fat is a good one to measure)
- elk hunter #'s have dropped at the same time increased hunting-related mortalities have increased
- would you expect a population decline or population stability if reaching K...yes, if it's a high r-selected species but not for grizzly bears
- population reconstruction exercise...took all known aged bears and worked backwards to reconstruct the population. Mark H. did this in 2005, 2008, and 2011. All 3 times showed the peak in the known alive population = 2000 or 2001. There are also fewer new bears that have never been previously captured in recent years.
- We could just not understand the mechanisms behind density dependence in gb populations. Could be driven by a greater and greater proportion of the population being males...affecting cub, yearling, and subadult survival and forcing females into lower reproductive rates due to pushing them to lower quality habitat
- if we didn't have any problems w/ WBP and we had the same demo. results and we had a chance to sample Hayden Valley and caught 5 adult males, 3 family groups, 4 adult females (no subadults). What do you think about this age distribution?
 - o it wouldn't happen in a hunted population. Would have younger bears and less males.
- biomass of the existing pines may be more important the % of WBP dead. But the effects of MPB on WBP is so patchy
- what about relaxing the 15% male mortality limit?
- What's the right index to experimentally manipulate the vital rates? Instead of having a mortality threshold, could you just monitor λ ?
 - o maybe but we would need a lot of radio-collared bears and we would need high confidence that our sample is representative
 - o there would definitely be a lag. The question is, are you comfortable with that? You'd need to know what that lag time is.
- for the coming year, we manage under the Chao2 estimate of population size but reduce the indep. female mortality to 6% because that's what the data indicate is sustainable.
- task list with deadlines will be sent out by Frank/Mark