Appendix J: SNAMP final report peer reviews, public and MOU Partner comments, and UC Science Team responses to peer reviews and comments

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Please note that, in contrast to the rest of the report, page numbers for this Appendix are in the header and right-justified to preserve the original page numbers of some of the peer reviews, comments, and responses that constitute Appendix J.
Introduction

The UC Science Team submitted the draft SNAMP final report for review in spring-summer 2015. The individual team chapters (Appendices A-F) underwent peer review as described below, and the entire report was provided to the public and the MOU Partners to review. In accordance with the UC Science Team’s commitment to transparency, all peer reviews, public and MOU Partner comments, and UC Science Team responses are included in this Appendix.

Peer review process

Each team's draft individual team chapter was reviewed by 2 reviewers, 1 academic reviewer and 1 reviewer from a government agency. The exception was the Water Team’s chapter, which received only 1 peer review because there was not enough time to obtain a second peer review before the project’s contractual end. Peer reviewers were not affiliated with the University of California or with Forest Service Region 5 or Forest Service PSW to avoid potential conflicts of interest. Chapters 1-6 were not peer reviewed.

To ensure the integrity of the peer review process, neither the UC Science Team nor the MOU Partners were involved in the process. The Office of the Associate Vice President of the University of California’s Division of Agriculture and Natural Resources agreed to conduct the peer review process on behalf of SNAMP. Peer reviewers were provided with a brief overview of SNAMP (see Sierra Nevada Adaptive Management Project summary for peer reviewers below) and were notified that their anonymous reviews would be made public as part of SNAMP’s transparent participation process.

Each team wrote a response letter describing whether and how they revised their individual team chapter in response to the peer reviews. Peer reviews and team responses are provided in this Appendix.

Public and MOU Partner comment process

The draft chapters of the SNAMP final report were posted on the SNAMP website as they came available. The Participation Team sent out an email announcement to all the SNAMP
participants on the SNAMP email list when a new chapter was posted on the website. In
addition, the MOU Partners received a direct email notifying them when a chapter was ready for review.

The Participation Team provided a website at which comments about any component of the SNAMP final report could be submitted by anyone so inclined. Comments could be submitted anonymously or with name and affiliation included. The UC Science Team’s goal was to allow at least one month for public and MOU Partners review. In one instance, we were unable to meet this goal. For the second draft versions of chapters 3 and 4 that were revised to include contributions from the Water Team, we asked for comments within a week so that we could adhere to our timeline; we subsequently extended the comment period for the entire report by an additional week.

All comments received by the deadline were passed on to the appropriate team for consideration. Teams revised their chapters in some instances based on comments received, and they addressed public and MOU Partners comments in their team response letter. Comments and response letters are provided in this Appendix.
Sierra Nevada Adaptive Management Project summary for peer reviewers

The Sierra Nevada Adaptive Management Project (SNAMP) is a joint forest management assessment by the University of California (UC), state and federal agencies, and the public. SNAMP came into being in response to uncertainty over forest fuels management in the Sierra Nevada and the controversy resulting from the United States Forest Service’s 2004 Record of Decision that established the current legal boundaries for management prescriptions in the Sierra Nevada national forests. SNAMP was created to assess the efficacy of forest fuels management on fire behavior and the impacts of that management on three natural resources, forest ecosystem health, wildlife, and water, while incorporating participation by all interested stakeholders, including the public.

The 2004 Record of Decision amended the 2001 Sierra Nevada Forest Plan Amendment. This decision led to significant controversies, including direct conflict between the State of California Resources Agency, the Department of the Interior’s Fish and Wildlife Service, and the U. S. Forest Service. To reduce interagency conflict, the California Resources Agency, the Fish and Wildlife Service, the Forest Service, and the Pacific Southwest Research Station signed a Memorandum of Understanding (MOU) in 2005. Stipulated in the MOU was a request for the assistance of the University of California to convene a “neutral, third party” of experts.

Consequently, the agencies and the UC formed the Sierra Nevada Adaptive Management Project to learn how to apply adaptive management as required in the 2004 Record of Decision, with an emphasis on engaging the public in a meaningful way. A key SNAMP objective was to evaluate the impact of Strategically Placed Landscape Treatments (SPLATs), a forest fuel treatment, across four response variables:

- fire and forest ecosystem health,
wildlife, focusing on the Pacific Fisher (*Pekania pennanti*) and the California Spotted Owl (*Strix occidentalis occidentalis*),

- water quality and quantity, and
- public participation.

Each response variable had an associated science team, and the response variable teams were supported by a spatial analysis team. As a whole, the university researchers were called the “UC Science Team”.

Given the challenge of replicating landscape-scale research projects across the expansive mixed conifer forests of the Sierra Nevada, SNAMP took a case-study approach. The feasible alternative was to pick sites that would represent the primary biogeographic gradient – latitude – by selecting one northern and one southern Sierra site. SNAMP considered sites that were broadly representative of northern and southern Sierra Nevada mixed conifer forest; sites that were outliers in any major characteristic were rejected. In 2007, the two SNAMP study sites were selected:

1) the northern site, called the Last Chance Project, was in the American River Ranger District of the Tahoe National Forest. The expanded California spotted owl study area included portions of the Eldorado National Forest. The northern site contained Sierra Nevada mixed conifer forest with considerable numbers of residual old growth trees.

2) the southern site, called the Sugar Pine Project, was in the Bass Lake Ranger District of the Sierra National Forest. The forest was mixed conifer. The southern site provided habitat for the Pacific Fisher, and, as with the spotted owl team study area, the fisher study site encompassed more than just the Sugar Pine Project area.

The fireshed, approximately 4,000 ha, was chosen as the most appropriate spatial scale for reporting SNAMP results and making management recommendations. Each study site comprised a pair of firesheds, one in which a SPLAT treatment was implemented by the Forest Service, the other serving as an untreated control. The spatial scale at which the various response variables were evaluated varied: water on the catchment scale (approximately 100 ha), fire and forest ecosystem health on the fireshed scale, wildlife on larger scales appropriate to the two species under study, and public participation on multiple scales. Once assessments were complete, findings were scaled up or down to the fireshed scale. The integrated SNAMP analyses also employed a uniform time scale: immediate effects (0-5 years post-SPLAT
treatment) and long-term effects (up to 30 years post-implementation, the estimated lifespan of a SPLAT treatment). Impacts were assessed both directly with 7 years of field data and with modelling, especially for longer term effects.

The UC Science Team began pre-treatment data collection at both sites in summer 2007. Pre-treatment data collection continued for more years than had been initially anticipated because there were delays in SPLAT implementation at both sites. The bulk of SPLAT implementation at both sites took place in 2011 and 2012; the Forest Service was solely responsible for designing and implementing the SPLATs. Following SPLAT implementation, the UC Science Team collected 1-2 years’ worth of post-treatment data in 2012 and 2013.

The original SNAMP workplan envisioned a rigorous Before-After Control-Impact (BACI) study design for the evaluated resources. However, significant delays in implementing SPLATs truncated the "After" measurement period. Since an extension of the original study timeline was not financially feasible, the UC Science Team was not always able to use the planned BACI design and instead developed other study designs, including the use of datasets from related studies, to address the project’s primary question of SPLAT impacts on resources. In addition to addressing the primary question, the UC Science Team developed multiple new methods and analytic techniques as well as new insights into the ecology and management of these forest ecosystems.

The primary research goals for each team were as follows:

California Spotted Owl
The primary tasks of the Owl Team were to: 1) assess the impacts of forest management and vegetation change on owl demography over the past 20 years on the Eldorado demography study area, and 2) project the effects of wildfire on the quantity and quality of owl habitat in the Last Chance study area over the next 30 years, with and without SPLATs.

Fire and Forest Ecosystem Health
Understanding SPLAT impact on fire behavior was the primary analytical product. The team also developed the necessary modeling tools to predict longer-term changes in forest composition and structure as well as fire behavior.
Pacific Fisher

Similar to the goals of the Owl Team, the primary tasks of the Fisher Team were to: 1) assess the impacts of forest management and vegetation change on fisher demography and behavior based on multiple datasets, and 2) project the effects of wildfire on the quantity and quality of fisher habitat in the study area over the next 30 years, with and without SPLATs.

Public Participation

The overarching goals were to create a model public participation process that agencies can emulate and to foster an engaged, knowledgeable group of stakeholders to work constructively with the management agency (i.e., the Forest Service). The assessment goal of the Public Participation Team was to estimate how much the various participants learned from each other, from science, and from SNAMP outreach efforts. Further, this team attempted to discover if that learning changed relationships among the involved parties and if constructive channels of communication were built and used.

Spatial

A primary goal was to develop algorithms to extract tree attributes such as height, dbh, height to live crown, location of individual trees, volume, and leaf area index, using lidar data, including metrics from the point cloud and from individual trees. These lidar-derived forest products and maps were used by the Fire and Forest Ecosystem Health, Water, Wildlife, and Public Participation teams. The Spatial Team also produced the pre- and post-treatment vegetation maps that formed the basis for the SNAMP integrated product.

Water

Primary goals were to measure and model the hydrology of paired headwater catchments and streams in the firesheds, to scale this modeling to the fireshed, and to predict and assess impacts of SPLATs on water-cycle attributes over a range of inter-annual climate conditions and across the broader forest landscape.
Additional notes to peer-reviewers

The office of the associate vice-president of the UC Agriculture and Natural Resources Division will conduct the peer-review. As part of the UC Science Team’s commitment to transparency in our SNAMP research, we release peer reviews of workplans and reports to the public. Please be aware that we plan on posting the peer reviews of the final report on the SNAMP website. The peer reviews will be redacted to maintain anonymity.

The UC Science Team sincerely thanks you for reviewing our research.
Retrospective Analysis

The data available for the retrospective analysis is very good. However, the analysis seems very cursory given the importance of the question. Following are some questions and comments concerning how the analysis was conducted.

“We found some evidence that high-severity fire was correlated with a reduced likelihood of territory colonization, but the standard error was unestimable for the parameter coefficient, suggesting that we lacked a sufficient sample size of burned territories to draw definitive conclusions.” This sounds like the authors had a model that was over-parameterized. The beta parameters should be estimable.

Site occupancy – how were sites determined? Only territories that were occupied initially are used? What about territories that were unknown until some point during the study? It seems that estimates of psi have to be biased high, because you start with all territories initially occupied. As a result, extinction is biased high, and colonization biased low. “We estimated a single center for each owl territory as the geometric mean of the most informative owl location(s) from each year that the territory was occupied. We used a nest location if one was located that year, but if we did not find a nest, we used the mean location of all roost trees located that year.” What did you do if a territory was not occupied?

Page 11. “We conducted the modeling in 3 steps to reduce the number of candidate models and thus reduce the likelihood of finding spurious relationships (Table 2). In the first step, we evaluated covariates that represented the amount of potential owl nesting and roosting habitat within territories. In the second step, we used the covariates from the top-ranked model from the first step and included additional covariates for potential owl foraging habitat, amount of private land, and the spatial distribution of forest cover types. In the third step, we used the covariates from the top-ranked model in the second step and included additional covariates that represented different types of forest disturbance. By using this hierarchical approach, we were able to control for existing habitat conditions within each territory when assessing the impacts of forest disturbance. For steps 1 and 2 of our modeling, we used the entire 20-year data set. For step 3, we used the covariates from the most parsimonious models from step 2, but then used reduced data sets for the three temporal scales because we lacked timber harvest data for years prior to 1993. None of the covariates that we used were highly correlated with each other (r >0.60).” AICc handles highly correlated covariates, because if you put 2 covariates with 0.99 correlation in a model, the models with each of these covariates separately will both rank higher than the 2-variable model. Further, the AICc score of the single-variable models will be nearly identical. Thus, it is not necessary to toss correlated covariates.

AIC, not AICc? Sample sizes are decent, but not so large as to assume that AICc is equivalent to AIC. Did you actually use AIC when you should have been using AICc?

No time models for apparent survival or p? How much time variation existed after time-varying covariates were used? Proportion of time variation explained by time-varying covariates? GOF generated using a fully time-specific model, so readers need to know how this full model relates to the covariate models. Both the null and global models AICc values are needed to evaluate the covariate models.
Age in apparent survival – 2 age classes?

I realize that $p$ in the occupancy models is visit-specific, whereas $p$ for the CJS models is year-specific. Yet, it appears considerably more analysis was done on $p$ for the occupancy models, including more covariates than for the CJS models.

Sensitivity analyses were based on a uniform distribution of covariates. What about individual differences in covariate values, and more importantly, time variation in these covariates? Also, if there is a covariance between the covariates, then simulating them independently will bias the sensitivity analysis. Bootstrapping from the actual values might be a better approach.

“If we set the habitat covariates equal to their mean value for all territories, apparent survival was estimated to be 0.73, 0.66, 0.63, and 0.56 for adult males, adult females, subadult males, and subadult females, respectively.” These estimates of apparent survival seem really low. Is this because the habitat variables are so skewed that the mean is not very representative? Would the median of the habitat variables provide a more typical estimate of survival?

Table 3 really needs the base model for each of the steps so that the reader can see how much improvement was made during the step. Further, the reader needs to know the value of the deviance of each model, and the saturated model’s deviance, so that the proportion of the deviance explained by the individual covariates can be assessed through ANODEV. Just presenting the covariate models leaves the reader wondering whether any were better than the dot model.

No mention of goodness-of-fit is made. Was there any attempt to evaluate over-dispersion of the data?

Prospective Analysis

This analysis is pretty much out of my area of expertise. The approach seems reasonable, but I don’t know how experts on these topics might take different approaches.
May 19, 2015


General Comments:
The objective of the Sierra Nevada Adaptive Management Project was to quantitatively assess the effects of forest fuels management practices in the Sierra Nevada Mountains on four scientific areas of inquiry: 1. fire and forest ecosystem health, 2. wildlife, specifically the California spotted owl and the Pacific fisher, 3. water quality and quantity, and 4. public participation. Four science teams were established to investigate each of these four topics. A fifth team, the Spatial Team was responsible for providing each of the science teams with digital remote sensing and mapping data, including airborne laser ranging data, digital vegetation maps, field ground-reference data, and a variety of measures and estimates derived from these airborne laser system (ALS) and optical system observations.

This reviewer’s area of expertise is airborne and space laser remote sensing of forest resources, and this review specifically looks at the approaches taken by the Spatial Team to derive products useful to the 4 Science Teams. This review tries to address the following questions.

1. Did the Spatial Team provide the best available ALS products to the Science Teams?

It is important to note that this reviewer views SNAMP as an “operational” project rather than a research project. The USFS and California state agencies will, based on SNAMP findings, implement specific forest fuels management plans in the Sierra Nevadas. As such, this reviewer approaches the report assuming that the primary responsibility of the Spatial Team is to supply proven ALS measurements and estimates to the Science Teams. It is not the responsibility of the Spatial Team to develop and validate new, experimental, innovative techniques or measures. Did the Spatial Team provide ALS products that have been well documented in the scientific literature?

In short, the answer is yes. ALS systems measure topography (elevation, slope, aspect) and a wide variety of vegetation canopy heights and densities directly, i.e., no models need to be developed to calculate these height and density numbers once a ground surface is defined. These include many, though not all, of the height, percentile, and pulse density metrics listed in Table 1, page 16 and 17. From these measurements, a second suite of useful estimates can be derived, e.g., Lorey’s height, tree volume, tree biomass, canopy base height, canopy volume.

2. The Spatial Team developed their multi-temporal ALS measures and estimates using a multi-stop ALS system. They write repeatedly in their final report that a small-footprint waveform system might have provided better results. Such waveform systems are now commercially available (e.g., www.rieogl.com), but I am not convinced that the extra data load and post-processing requirements provide a significant advantage except in situations where users want to summarize vertical vegetation profiles on relatively small raster cell sizes, e.g., smaller than a 5m x 5m cell. Admittedly, my previous statement is arguable, however Skowronski et al. (2011, Remote Sensing of Environment 115: 703-
714) have reported some success estimating canopy bulk density and canopy fuel weight with an ALS multistop system. I suggest that the authors not characterize a small-footprint waveform system as a possible solution to multistop limitations until direct comparisons can be made. The authors may be correct, but unless you can cite a refereed study where waveform outperforms multistop as regards prediction of, for instance, forest fuels variables, e.g., canopy base height, canopy bulk density, you should not make the claim.

3. Could SNAMP have been managed better so that Spatial Team products better addressed the concerns of the four Science Teams?
   3.1. Yes. The USFS should have implemented their forest fuels treatments on schedule so that the original BACI approach could have been implemented. As written, the seven year SNAMP was designed (1) to study empirically (via field sampling and remote observations) post-treatment effects over a 0-5 year time period and (2) to model post-treatment effects out to 30 years. The UC field teams started pre-treatment data collection in 2007. The fuels treatments were not done until 2011 and 2012. Only 1-2 years of post-treatment fieldwork was done The fact that the fuel treatments were delayed for 3-4 years certainly significantly impacted the 5 year empirical study and most likely impacted the long-term models. This comment is not meant to reflect badly on the Spatial Team since they most likely had little control over treatment implementation, but it does reflect poorly on overall project management.
   3.2. The multitemporal ALS flights should have been timed to be seasonally coincident to improve comparability. To manage costs and logistics, SNAMP wisely took a case study approach, selecting two study areas, one in the north (Last Chance), and one south (Sugar Pine). The Last Chance ALS data collects were done in September 2008 - pre-treatment, and November 2012 and August 2013 (post-treatment). The Sugar Pine collects were done September 2007 (pre-treatment) and November 2012 (post-treatment). Both north and south sites are most likely predominantly coniferous, but if there are significant hardwood components on each area, then leaf-on / leaf-off differences pre- and post- fuels treatment will be convolved, adding noise to any comparisons made and uncertainty to any of the conclusions reached. Project managers should have insisted that multitemporal ALS data collects be done in the same month years apart, preferably leaf-on, preferably mid-summer.

4. Although the Spatial team provide rasterized products to the Science Teams at requested cell sizes, I was surprised to see that you base these raster products on individual tree delineation and measurement. I wonder why you made this choice as opposed to using an area-based approach used by Næsset and many others. The individual tree delineation numbers that I see in the literature vary from about 60 - 80%, meaning that individual trees are properly located and identified for measurement only 60-80% of the time. Understory trees and trees in closed canopy situations are typically undercounted. Somewhere in your report I would suggest that you add a paragraph that explains what you gained by delineating individual trees in the ALS data. I do not say that what you did is wrong, but your individual tree approach brought along with it a host of problems that included individual tree omission and commission errors, increased sensitivity to relatively small GPS misregistration errors in the field and in the lidar data, a more complicated field protocol that included logging locations of all individual stems.
and crowns. What did you gain, and in hindsight, would you do it the same way again or make some changes such as considering an area-based approach with a minimum cell size? In your defense, I understand that at least two of your Science Teams - Wildlife and the fire fuels treatment team - would be interested in changes as regards specific individual trees, and that consideration may have drove your decision to identify and measure individual canopies.

5. Is the report clearly written? Can changes be made which might improve Spatial Team Final Report readability?

In general the report is clearly written, however the specific suggestions below should improve readability. By way of generalities, the authors should clearly draw a distinction between measured variables and estimated variables since the latter incorporate model error. Too often, the variables are muddled together in tables and text. For instance, you measure maximum height or average canopy height or various height or density deciles (or quintiles or quartiles) on a given cell by accumulating first-return and secondary-return laser ranging information as needed. You’d estimate that cell’s biomass or canopy bulk density or Lorey’s Height by developing an equation that relates field estimates of biomass or canopy bulk density or basal-area-weighted tree height to some subset of ALS measurements listed in the previous sentence.

Finally, define acronyms when first used. This report may be read by folks (like me) not familiar with SNAMP.

Specific Comments:

1. Should the title of this report be “Sierra Nevada Adaptive Management Project (SNAMP) - Spatial Team Final Report” (instead of Plan)?

2. pg 5. Define AGB. I realize that AGB = aboveground biomass, but is it total aboveground dry biomass, green biomass, stem only, all aboveground components including leaves/needles? Does tree volume equal stem volume to a certain top limit, or are you actually talking about the volume of space defined by the outer periphery of the tree crown?

3. pg 5. Use of Lidar for biomass estimation: You write the following: “…the availability of, and uncertainly in, equations used to estimate tree volume allometric equations influences the accuracy with which Lidar data can predict biomass volume.” Two things: First, the majority of the uncertainty associated with biomass estimation using lidar data has to do with the fact that, with ALS data, we don’t know the diameter of the tree. With ALS, we estimate biomass (or stem volume, for that matter) based on height and canopy density. The primary driver in ground-based allometry is diameter, not height. The choice of allometric equation certainly makes a difference, but our inability to measure or infer dbh drives the uncertainty in ALS estimates of biomass. Second, what is biomass volume? Do you mean biomass density, i.e., biomass weight per unit area, e.g., 250 t/ha? Or biomass weight within a certain crown volume? I’ve worked in this field for 30+ years and have never heard the term biomass volume. Define.

4. pg 6. Wildlife: I suggest that you qualify your first Wildlife bullet. Two points. First, ALS data can only be used to map potential habitat, not actual habitat. We can’t measure critters, we can only identify/map areas that might make a particular critter happy if it should choose to show up in a given area. Second, we can only map potential
habitat if we can define a particular set of habitat characteristics that can be measured or estimated by the ALS, e.g., particular height, density, overstory/understory, biomass criteria.

5. pg 6, 4th paragraph: Actually, standard lidar products do currently meet the requirements of at least some forest managers, just not, in general, public sector foresters here in the US. This might change soon if USFS managers adopt laser-assisted ground inventory procedures to inventory undersampled areas in Alaska. Scandinavian companies routinely map/inventory forests with ALS, producing stand-level volume maps for sale to private landholders, and their public sectors are actively transferring that technical know-how to selected countries in Africa, SE Asia, and South America under the auspices of U.N. REDD+ and carbon programs. Your point is correct as far as CONUS goes; just be aware that some European countries are way ahead of us when it comes to operationally using ALS data in conjunction with ancillary (e.g., optical) data.

6. pg 9, 2nd to final paragraph: As previously discussed, I’m not sure that I agree with your statement that a waveform lidar can provide a better description of forest structure. And as noted above, small-footprint waveform lidars are available and it’s my understanding that some lidars can be set up as either multistop or waveform, depending on the needs of the mission. In other words, the same laser system can serve as a waveform lidar on one mission and a multistop on the next (they cannot sequentially toggle between these two modes from one pulse to the next).

7. pg 10, bottom: You write that there are no standard ALS metrics that capture forest structure. That’s not really true; you list many of the “standard” variables in Table 1, specifically the height deciles and density deciles. Most of the remaining height and density variables listed in Table 1 are typically very highly correlated with these height and density deciles.

8. pg 11, 4th paragraph: Waveform lidar systems are typically sampled at 1 ns, a sampling interval that corresponds to a vertical distance of 15 cm - true. What is not true is the suggestion that this distance depends on maintaining a typical flying height. It has nothing to do with flying height and everything to do with the speed of light, ~30 cm/ns, regardless of the altitude of the aircraft.

9. pg 12, 2nd paragraph: I think that you meant to say that your Optech GEMINI collected up to 4 discrete returns per pulse. Sometimes you’d receive only a first return, sometimes 2 or 3 returns, and, I suspect only rarely, 4 returns per pulse. Perhaps you could provide a percentage breakdown of 1-, 2-, 3-, and 4-return pulses, though do this only if that information is readily available. Also report the maximum scan angle considered in your analyses, e.g., ±7.5°, ±15°.

10. pg 12: In Section 2.2, report the nominal XYZ accuracy of a given Optech pulse. Also report in 2.3.1 the XYZ accuracy of a given GPS reading.

11. Considering all error sources, can you provide an estimate of location error, ground versus ALS near the bottom of page 12?

12. pg 17, Table 1: Suggest that you identify Lorey’s height as a modeled variable with a superscript, e.g., *.

13. pg 20, 5th line from bottom: change depended to dependent,

4th line from bottom: change expansive to expensive.
14. Section 4, general comment: You discuss the accuracy of many products in this section and report accuracy in Table 2. In order to assess accuracy, you need some sort of ground reference measure, i.e., a validated product that you trust more than the comparable ALS product being evaluated. In Section 4.3, you compare ground-based tree counts to ALS-derived tree counts. This is good, though I believe that you should report the range of percentage of trees under- or over-counted on each plot so that the reader gets a better feel for site variability; a scatterplot would be more informative. In Section 4.1, you conclude that the accuracy of DTM and DSM products increase with sampling density. This makes intuitive sense, but how do you know this? Did you compare the ALS DTM products to field-measured ground elevations? Did you compare, on a per-tree basis, DSM measures derived from various pulse density products to tree height measurements + ground GPS elevations? My point here is that, in each section and in Table 2, tell the reader what “truth” is and what lidar metrics specifically are compared to that ground reference information. When I look at Table 2, I see many $R^2$ values, but that’s not really a measure of accuracy; it’s a measure of percentage of variability explained by a linear model. On a per-tree basis, you can compare field-measured maximum height to the lidar maximum height for the same tree. But tell me how you’re going to measure mean tree height in the field. Where is the mean height of a tree when you are on the ground looking through an angle-finder? You can’t measure mean tree height in the field, though you certainly can with a laser which takes multiple height measurements on a single tree. So you move to a regression approach as you indicate in Table 2, but what are you regressing? Scatterplots would help greatly here for those comparisons denoted by “Indirect: from regression”. An explicit identification of the ground reference data set would be most helpful for those ALS metrics directly compared to a reference data set. And the reader should not be forced to go to the NCALM report or references 4, 6, 24, 13, or some unnamed report yet to be submitted to find out how you assessed accuracy, or your surrogate for accuracy.

This table is the backbone or the skeleton of your report. Spend some time and column inches on it so that the reader knows explicitly what comparisons were made. It’s very important that the reader knows, for instance, that some very critical measures of forest fuels cannot be reliably characterized using ALS measures.

Many of your comments made in Sections 5 and 6 have already been addressed above. As noted previously, I disagree with your statement that standard lidar products do not operationally meet the requirements of forest managers. They can, and in the future, they will. The only items currently stopping their use in the US is cost, need, and the technological intransigence of state and federal forest managers. Airborne lidars can not only tell you where the wood is and approximately how much is there, but for no additional cost will report topographic challenges of interest to forest engineers. Perhaps in 10 years, public sector forest managers will realize this and begin to come up to technical levels attained by Norwegian foresters 10 years ago.
Review of SNAMP Public Participation Team Final Report

Thank you for the opportunity to review the Sierra Nevada Adaptive Management Project (SNAMP) Public Participation Team Final Report. From the “Sierra Nevada Adaptive Management Project summary for peer reviewers,” it is my understanding that the primary goal of the public participation team was to develop a model public participation process to engage stakeholders in the Forest Service planning process. The related assessment goals are to determine: (1) how much participants “learned from each other, from science, and from SNAMP outreach efforts;” (2) whether “learning changed relationships among the involved parties, and (3) if constructive channels of communication were built and used.”

The Public Participation Team’s final report provides a detailed overview of the SNAMP outreach efforts and the Participation Team’s assessment of stakeholder learning and relationships. Summarizing the efforts of a ten year project is commendable. My comments and recommendations for the report follow.

1. First, the report addresses each of the three assessment goals noted above, as well as the five goals outlined in the report’s introduction – transparency, inclusiveness, learning, relationship building, and effectiveness. The executive summary addresses the first two goals noted in the peer review summary but does not clearly address if “constructive channels of communication were built and used. It also does not clearly address the last goal identified in the report - “effectiveness.” It would be helpful to add a sentence or two describing how these goals were met in the executive summary.

2. In my first read-through of the report I found it difficult to follow how it was organized. The first part of the executive summary provides a brief overview of the goals and the findings of the participation team. I recommend clarifying the statement of purpose for SNAMP and the Public Participation Team in the first paragraph of the executive summary. After an initial overview of the purpose and goals, provide a brief summary of the accomplishments and evaluation. I suggest devoting just one paragraph describing how the report is organized, similar to (or copying) what is written in the introduction on page 12, rather than having it
interspersed over several pages. Much of the information on pages 3 to 6 could be incorporated into the introduction or deleted.

3. The introduction does a nice job of providing a brief history of the project while also identifying the goals and activities of the Public Participation Team. You might consider identifying or listing the six other teams involved in the SNAMP to provide context for people who only read this section of the report.

4. For Section II, as a literature review and background, there should be a substantial increase in the use of citations throughout the section; some paragraphs have only one citation yet refer to many conclusions. It is important to also include page numbers for direct quotes in your citations. Also consider creating adding a sub-heading on page 20 before the second paragraph beginning with, “Concerns of the California Resource Agency…” because these paragraphs introduce the specific context for the SNAMP case. Another option would be to re-organize so the background on decision-making processes and constraints to co-management are at the beginning of this section, with no references to SNAMP. This would be followed with a subsection discussing SNAMP and how it provides a new approach within the previous context.

5. Also in Section II, I like the use of Figure F-3 to identify different management approaches over time. It appears the second model in Figure F-3 is not summarized explicitly in the text and this would be helpful for readers to understand how the shift in decision-making was affected by NEPA, litigation, FACA, and other factors mentioned, as well as the challenges which led to the development of the third model.

6. I found Sections III provides a good summary to the public participation process used by SNAMP. I am curious about the annual meeting evaluations reported on page 33 – is what you report the average across all fourteen annual meeting evaluations? It would be helpful to know if there was any significant change in responses over time for the annual meetings, as well as for the integration meetings (page 35), field trips (page 37), and management workshops (page 39). The format used to report the evaluations differs across these meeting types and it would be helpful to report using the same format as was used for the annual
meeting (i.e. include the number of evaluations completed and the rating scale). Also report evaluations of the special projects or mention if there were no evaluations for these projects.

7. In Section III you provide a detailed summary for the in-person meetings but not the distance outreach. It would be helpful to know how the distance learning affected transparency, information exchange, and relationship-building, as was written for the in-person outreach.

8. Section IV does a very nice job of outlining the development of the SNAMP collaboration workbook and workshops. I am curious if there were any significant differences (using a paired samples t-test) in the pre- and post-workshop surveys? It would be helpful to identify significant differences in Table F-5, as well as in the text.

9. Section V summarizes the assessment of the process very thoroughly. My primary concerns relate to the methods and reporting of the email surveys. First, does the population of email contacts maintained by Extension adequately represent all stakeholders? It is noted that the email contacts “represent the community of interest in Sierran forest management that wanted to maintain email contact,” but do the respondents represent the full population of stakeholders? It would be good to know if a non-response bias check indicated whether those who did not respond differed from those who had responded.

Second, it would be helpful to state why Chi-square statistics were used to compare results from 2010 and 2014, rather than a paired-sample t-test. The independent samples t-test, or paired samples t-test where applicable, provides a better assessment of differences or similarities across the two years of surveys.

Third, for the interview and email demographics you provide the range for ages and income levels; it would be helpful to have the averages/means for these demographics. Also consider using the same format for the interview and email demographic descriptions so that it is easier for the reader to compare across populations.

Fourth, rather than mentioning the $p$-value throughout your results and discussion you can state in the methods section that a significance value of $p=0.05$ was used.
10. Section VI provides a nice in-depth overview of learning that occurred with the concept of forest health. One suggestion to improve the readability of this section is to combine the description of the four themes with the description of their evolution over time rather than having the description of the theme as a separate sub-section from the evolution. If you prefer keeping these descriptions separate it would be helpful to have them in the same order for both sub-sections.

11. Section VII summarizes the achievements and challenges of the project. It identifies how participant learning occurred and how participants believed the process had reduced conflict, indicating constructive channels of communication were developed. One suggestion for this section is to provide more detail at the end of the first paragraph on the percentage of respondents who felt “part of the project” and who valued the learning opportunities, etc. Otherwise this section provides a nice summary and Section VIII provides a good compilation of the lessons learned from throughout the report.

12. Lastly, there were several formatting and grammatical issues throughout the report.

- Many of the tables were split across pages making them difficult to read (e.g. Table F-3). I suggest moving tables so they fit on one page; if too large for one page continue the heading across pages and try not to split text that is in one cell). Some tables were also split from their headings which also creates confusion for the reader (e.g. Table F-1).
- Indented sub-headings should be formatted (i.e. italicized or underlined) so they stand out from the text more distinctly.
- Many of the legends used for figures are difficult to read when printed in black and white (e.g. Figure F-17). I recommend switching the legend from vertical layering to horizontal layering (e.g. Figure F-26) so the legend matches the layout in the figure. Or change the color scheme so it is identifiable in black and white.
- Also, check that all abbreviations are defined at first mention in the chapter or appendix and used consistently throughout the report.
- Check the wording and the order of the headings and sub-headings are consistent across different areas of the report. For example, the heading – “SNAMP distance outreach summary” (page 60) differs from the format for the in-person outreach on page 41.
There are several grammatical instances where punctuation is outside quotations rather than inside, e.g. text” instead of text.” Search and replace can quickly fix this.

Thank you for the opportunity to review your report and learn more about the SNAMP project!
Thank you for the opportunity to review the SNAMP Fisher project report. I found this report to be very well written and it contained a remarkable abundance of valuable new information on the biology and ecology of fishers. In concluding my review of the document, I will provide my overarching comments in this letter and I have attached my marginal editorial comments in the attached pdf of the report. Thank you again for the opportunity to review this report.

Major Comments

As mentioned above, this report and the associated publications will provided valuable new additions to the fisher literature and will certainly provide needed information for fisher conservation in California and elsewhere.

The findings in the report demonstrated that the research was effective at addressing the first two objectives of the research project. Meeting the third objective of the project was apparently complicated by the delayed or incomplete implementation of SPLATs in portions of the study area. The complications of SPLAT implementation apparently made it difficult to conduct the before and after comparisons that were originally envisioned for the research project. I had expected to see specific demographic and land use measures for fishers before and after SPLAT implementation, but those data were not presented. I am hopeful that further research will allow for those comparisons to be made. I think they could be important for fisher conservation in California.

While the details of before and after response data were not available, this research project did use a number of useful measures to assess population responses across a large portion of the fisher’s range in the southern Sierra Nevada. Further these measures will be useful for assessing fisher occupancy and population performance in other areas of the fisher’s west coast range.

Other Comments

I think it will be important to make sure you keep the reader aware of any differences that might exist between annual survival rates, mean annual survival rates, and 2-year survival rates (survival over a 2-year period). I was not sure if a 2-year survival rate meant an average of 2 annual survival rates or if it meant the product of 2 annual survival rates or something else.

I was surprised to see how large the home range sizes were for the fishers in your study area. Not having the home range sizes for other CA fisher populations committed to memory, I wondered if they were comparable to those in your study area or if they were considerably smaller. I ask because I wondered if there was some ecological phenomenon...
that exists at the northern extent of the SSN fisher population (your study area) that results in fishers using larger home ranges at this margin of their range. Generally larger home ranges imply lower habitat quality for the species in question.

I also wanted to mention several wording change suggestions that I think would help with clarifying the message to the reader:

- You use the term grid to describe what other researchers refer to as grid cells. I am used to the term grid cells and would recommend using that term and referring to the collection of grid cells as the grid.
- You interchangeably use Camera Years and Camera Survey Years to define the timing of camera survey efforts. I think Camera Survey Years is the best term to use.
- You interchangeably use the words gender and sex. I have recently been warned off using the term gender (a behavior role) because it differs from sex (anatomy) and it is most commonly used when describing humans.
- Also, I noticed that the spacing between sentences was inconsistent.

Lastly, I made a number of smaller comments in the text and in the tables and figures that could be hard to see, so I wanted to make you aware of those so they weren’t overlooked.
SIERRA NEVADA ADAPTIVE MANAGEMENT PLAN (SNAMP)

Appendix D: Fisher Team Final Report

3 May 2015

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i. Executive Summary

Fishers (*Pekania pennanti*) are a medium-sized mammalian carnivore with a pre-European distribution encompassing the boreal forest zone of Canada, the Great Lakes region and northeastern United States, a relatively limited portion of the Rocky Mountains in the United States, and mountainous areas of Washington, Oregon, and California, USA (Powell 1993). Ecologically, fishers are a mature or old forest-obligate species (Zielinski et al. 2005), and in central to eastern Canada and the northeastern United States their numbers were reduced historically by the combination of intensive trapping and loss of forest habitats (Powell and Zielinski 1994). The species is uncommon to rare in the western United States, and is a candidate for listing under the US Endangered Species Act. The California Department of Fish and Wildlife (CDFW) is reviewing the status of fishers in the state, with recommendations concerning listing to the Commission expected in 2014. In advance of listing decisions, conservation planning has been underway since 2013 to develop an approach to maintaining viable populations of fishers in both northwestern California and in the southern Sierra Nevada. Information from the SNAMP Fisher Project (published manuscripts, submitted manuscripts, and unpublished data) described herein has been included in a Southern Sierra Nevada Fisher Conservation Assessment developed by the Conservation Biology Institute, with input from a team of 13 fisher researchers and scientists.

The SNAMP Fisher Project was initiated by the UC Berkeley Fisher Science Team in Fall 2007 in association with multiple other SNAMP research programs designed to provide an independent evaluation of how vegetation management, prescribed by the 2004 Sierra Nevada Forest Plan Amendment, affects fire risk, wildlife, forest health and water. Fuel reduction management includes a mix of activities including mechanical mastication of shrubs and small trees, hand thinning/precommercial thinning, commercial thinning, and controlled burning.
The range and number of fishers in the Sierra Nevada declined by over 50% after the early 1900s (Spencer et al. 2014). A major goal of the SNAMP Fisher Project was to determine whether or not current rates of survival and reproduction will allow fishers to persist in the Sierra Nevada in the context of active forest management to reduce fuels and the risk of catastrophic wildfire. Toward this end, in October 2007 the SNAMP Fisher Team initiated fieldwork by placement of survey cameras (camera traps; O’Connel et al. 2011) in the focal study area referred to as the four “Key Watersheds” (Nelder Creek, Sugar Pine, White Chief, Rainier Creek) where the Before-After-Control-Impact (BACI) design for the larger SNAMP research would be centered. Based on information provided by the Bass Lake Ranger District of the Sierra National Forest, three different fuel reduction projects were planned during the course of the study: Sugar Pine (main focus of SNAMP), Cedar Valley (started in fall/winter 2007-08), and Fish Camp (started in 2010-11). Our approach for assessing how fishers would respond to Strategically Placed Landscape Area Treatments (SPLATs) was designed to be multifaceted including (1) life history responses to fuels reduction (changes in survival, reproduction/fecundity, lifespan), (2) changes in local scale habitat use within individual home ranges, and (3) shifts or changes in habitat use at the home range scale of animal resource use/resource selection. The research required capturing, radiocollaring, and monitoring multiple individual fishers, which were monitored using high intensity aerial radiotelemetry to identify deaths and quickly recover carcasses for necropsy, and repeated camera surveys within and around the Key Watersheds. Specific objectives included:

1. **Determination of all key demographic parameters including age- and sex-specific survival, reproductive rates, and fecundity, and metrics on dispersal and movements**

2. **Identify population limiting factors based on cause-specific mortality due to**
predation, disease, and human-linked factors such as roadkill on local highways

3. Evaluate the effects of SPLATS on occupancy, survival, and fecundity

The SNAMP Fisher Project study area is at the northern edge of the southern distribution of fishers in California, encompassing the area bounded by the Merced River in the north and the San Joaquin River in the south. Administratively, the study area was within the Bass Lake Ranger District in the Sierra National Forest, but early in the study a radio-collared fisher dispersed north into Yosemite National Park, which effectively expanded the research to encompass the southern area of Yosemite National Park. The overall study area encompassed approximately 1300 km² of a topographically complex landscape with elevations ranging from 758 m to 2652 m. The smaller focal study area (Key Watersheds) was located in the approximate center of the study area, and the Key Watersheds entirely encompassed the firesheds designated for the SNAMP BACI research design.

A range of standard methods were used in the study to live-trap, radiocollar and monitor survival status of individual fishers. Monitoring was accomplished almost entirely by fixed-wing aerial radiotelemetry, supported by an “in house” aviation program developed specifically for SNAMP Fisher and administered by the USDA Forest Service. Ground-based radiotelemetry was used to monitor female fishers during denning seasons, and to recover carcasses of deceased fishers. Camera traps were systematically placed in the Key Watersheds and elsewhere in the study near the center points of 1-km² grids. Camera traps in the Key Watersheds were surveyed for fisher activity in each year of the study, whereas those placed elsewhere were not (some of the “external Key Watershed” grids were surveyed in multiple years when other forest management projects had occurred). Three “Management Indicators” were developed and assessed annually to provide stakeholders and managers with information on the status of the fisher population in the study area. The indicators were designed to link to local, home range, and landscape scale responses by fishers to forest management activities occurring in the Bass Lake Ranger District. We also developed five different focused research efforts related to fisher ecology considered relevant for fisher management and conservation. These “Focused Research Topics” related to the use of camera traps for identifying gender of fishers, evaluating fisher activity patterns, and evaluating the distribution of several forest carnivores in relation to roads or other species. One of the focused research topics addressed roadkill mortalities of fishers on a major highway (Highway 41) that bisected the study area.

Surveys with camera traps were completed in 905 unique 1-km² grids throughout the overall study area, including 56 grids within the southern region of Yosemite National Park from a companion
study funded by the California Department of Fish and Wildlife. Fishers were detected in 448 of the unique grids surveyed, which helped to identify that fishers in this part of the southern Sierra Nevada were most common between 4500 and 6500 feet elevation (1372 and 1981 m elevation). Occupancy estimates for multi-year surveyed grids corrected for imperfect detection < 1.0 ranged from 0.62 to 0.80. Detection rates for fishers at camera trap stations were much higher in the fall, winter, and spring seasons compared to summer, likely due to availability of a more abundant and diverse prey base in summer compared to winter especially.

A total 110 individual fishers were captured and radiocollared Project from Dec 2007 to Dec 2013 (62 females, 48 males). Sixty-six (60%) of the 110 individual fishers radiocollared during the study were known to have died, including 32 females and 34 males. On average 10.5 radiocollared fishers died in each population year over the course of the study, and the most common cause of death was predation by felid carnivores (bobcats, *Lynx rufus*, and mountain lions, *Puma concolor*). Four deaths were caused by, or associated with, canine distemper virus in 2009 when a relatively small scale epizootic event occurred in the study area. Other disease deaths included Toxoplasmosis and septicemia. Septicemia-linked deaths were caused by injuries the animals suffered weeks or months before death that led to infection that sometimes contributed to starvation/emaciation. Two radiocollared fisher deaths were roadkills on Highway 41. Four others were directly linked to anticoagulant rodenticides being used in association with illegal marijuana grow sites in the Sierra National Forest, and a fifth mortality is suspected yet currently unconfirmed.

The SNAMP Fisher study generated information on all key vital rates needed to evaluate the population growth rate (*λ*) and for understanding whether the population has the potential to persist. We developed an age-structured matrix model to estimate a series of five deterministic population growth rates (*λ*) for the SNAMP Fisher study population using the observed, “empirical” data on denning rates and litter sizes (fecundity), and survival. Estimates of survival and fecundity were produced for five 2-year groups/pairs of years starting with population years 2008-09 and 2009-10 and ending with 2012-13 and 2013-14. The Leslie-matrix population model was used to integrate data on fisher survival for three age classes, and fecundity for four female age classes: juveniles and subadults (non-reproductive), and young and mature adults (reproductive). Estimates for *λ* for the SNAMP Fisher study area were below 1.0 in two 2-year groups (population decline), equal to 1.0 in one 2-year group (stable), and slightly positive in two 2-year groups (increasing population). Lambda across all years was 0.90, which was suggestive of general population decline, however, the annual and cumulative 95% confidence intervals all overlapped with 1.0.
Prior to the SNAMP Fisher study there was limited information on the distribution and 
abundance of fishers at the north margin of their extant southern range. Many years of survey-based 
research with cameras and track plates conducted by the US Forest Service Region 5 and the Pacific 
Southwest Research Station suggested that the population in the SNAMP Fisher study area was likely 
sparse (low density), and there had been no indication that “surplus animals” were dispersing 
northward into suitable, unoccupied habitat north of the Merced River in Yosemite Valley. We used 
resightings of individual radiocollared fishers in a Robust Design capture-mark-resight framework 
(CMR) to estimate the fisher population size and density in the overall SNAMP Fisher study area. The 
SNAMP Fisher study area corresponds to the “Fisher Habitat and Core Connectivity area 5” being 
used for conservation planning. Fisher population size ranged from 48.2 in 2010 to 61.8 in 2012, 
whereas mean population density ranged between 0.072 fishers/km² in 2010 and 0.093 fishers/km² in 
2012. We considered data from other studies in California and elsewhere that used CMR methods 
similar to ours, and determined that the population density for fishers in the SNAMP study area was 
the lowest reported for the continental United States. Also, and in support of conservation planning, 
we used the mean density from the study to estimate that there were 93 fishers (range 80-107) in the 
Southern Sierra Nevada Habitat Core and Connectivity area 5.

Den cameras used in association with ground-based monitoring provided detailed information 
on the activities of 32 different individual adult female fishers during six spring denning seasons. 
Denning and reproduction in the SNAMP Fisher study area typically began in the last week of March, 
and adult female fishers ceased regular use of den trees in the first week of June. The earliest and 
latest known regular use of den trees was March 22, and June 20, respectively. Seventy-six (85%) 
breeding-age female fishers either exhibited denning behavior (n = 63) or were determined to have 
denned and weaned at least 1 kit. Among the 76 breeding-age females that initiated denning, 64 (84%) 
were identified as weaning kits. Overall, 72% of adult female fishers for which reproductive status 
was known produced at least 1 weaned kit. Eleven (17.5 %) of 63 cases of denning for females that 
were monitored during spring periods failed prior to kits being weaned. Eight den failures were due to 
death of the denning female; 7 denning females were killed by predators and 1 died after exposure to 
rodenticides. We were able to determine litter size for 48 of 59 denning females. A total of 73 kits 
were known produced, with an average litter size of 1.5. After accounting for known mortalities, we 
estimated that 64 of the 73 kits produced were weaned from den trees, whereas seven kits died or 
would have died had they not been rescued.

The availability of suitable den structures is a critical, and potentially limiting, feature of fisher
The mean number of den trees used per female per denning season was 2.4 (range 1 to 5). We identified 125 unique structures used as natal or maternal dens, including 54 black oak trees, 41 incense cedar trees, 19 white fir trees, 10 sugar pine or ponderosa pine trees, and one canyon oak (Quercus chrysolepis). We discovered that repeat use of den trees was not uncommon. Sixteen individual den trees were used more than once; 15 trees were used in two years, and one tree was used in four different den seasons. In most cases of repeat den tree use the same individual reused one or several den trees between successive years, but in two cases a female used a den that had been used by a different female in a previous year. Fifty-six percent of the unique individual trees used for denning in the SNAMP area were live trees (n = 70), whereas 44% (n = 55) were snags. Black oak was the most common tree species used for denning (live or snag; 43%), but a high percent of incense cedar were also used for denning (33%). Among snags used as denning structures, black oak and incense cedar were both commonly used (18% each), whereas white fir and pines (sugar pine or ponderosa pine) were less common as snag-type den trees (4% each). Overall mean DBH of black oak denning structures was 74.4 cm, 115.6 cm for incense cedar, 108.3 cm for white fir, and 111.2 cm for the two large pine species (sugar pine and ponderosa pine; Table 18). Mean heights of live trees were taller for live trees compared to snags of the same species, reflecting that many of the snags used for denning were at advanced stages of decay. The majority of denning structures used in the SNAMP Fisher study area (83%) were in the elevation range 4500 feet (1371 m) to 6000 feet (1829 m), and denning structures were typically embedded in areas of high canopy cover (mean = 72%). Shrub cover and aspect near den trees was variable, and most den trees had multiple large down trees/logs nearby, whereas concealment cover to the base of den trees averaged more than 45%. Also, information on denning habitats near den trees from high resolution Lidar (Zhao et al. 2012) identified that fishers selected den sites with tall trees and steep slopes within a 10-m radius of the den tree, and denning areas were associated with high levels of forest structural complexity and clusters of multiple large trees within 30-50 m.

Dispersal behavior by fishers is of high management interest in California because the southern Sierra Nevada population does not appear to be expanding geographically despite changes in management promoting restoration of suitable fisher habitat. Also, dispersal movements by fishers are potentially inhibited by exposure to multiple restrictive habitat elements (burned forests) and landscape features (steep river canyons). The SNAMP Fisher Project used a combination of data on juvenile and adult home ranges, and maternity assignments based on microsatellite DNA analyses to assess natal dispersal by young fishers based on Euclidean distance between natal areas and subadult or adult home
ranges. We also applied an “expert” cost surface to the landscape, and used Least Cost modeling approaches to predict more realistic dispersal paths/distances based on presence of restrictive habitat or landscape elements considered aversive to fisher movement and life history. The combination of field (juvenile home ranges) and genetic data (maternal assignments) allowed us to assess dispersal for 43 (74%) of 58 juvenile or young subadult fishers in the study population. The average Euclidean distance natal dispersal for female fishers was 5.8 km, compared to 9.8 km for male fishers. The longest Euclidean distance dispersal for a female fisher was 24.5 km, compared to 36.2 km for a male fisher. Although male fishers tended to disperse longer distances than females, the difference was not significant. One male fisher from the Kings River Fisher Project on the High Sierra District of the Sierra National Forest moved across the San Joaquin River canyon and immigrated into the SNAMP Fisher Study area. The Euclidean distance for this dispersal movement was over 36 km, but the more likely Least Cost dispersal path was predicted in the range of 67-69 km. In general, we found very limited evidence for male-biased natal dispersal according to any of the typical metrics reported in the literature for this life history phenomenon. Dispersal distances were not longer for males compared to females based on either Euclidean distances or more realistic Least Cost movement paths, and there was no difference in the proportion of each gender that dispersed, or that remained philopatric. Timing of dispersal was focused during mid-February into July when 80% of dispersal events were initiated and subsequently completed.

Management indicator 1 (occupancy/presence of fisher detections in 1-km² grids within the Key Watersheds), ranged from a low of 53% in 2012-13 to a high of 76% in 2011-12 (mean: 62 ± 9.2%). An index of fisher activity developed for Management Indicator 1 indicated that the estimated detection rate (detections/100 camera survey days) ranged from 10.5 in 2010-11 to 18.6 in 2012-13 (mean: 13.9 ± 2.9). Camera year 2012-13 was atypical in that many grids in the Key Watershed were surveyed during summer when detection rates are significantly lower compared to fall and winter, and it was therefore possible that the low detection rate for 2012-13 was related to timing of surveys.

Management Indicator 2 (number of resident subadult and adult fishers using the Key Watersheds) identified an overall average of 5.0 subadult or adult females and 2.0 subadult or adult males using the Key Watershed focal study area. For both sexes, the number of resident fishers using the focal study area ranged from 6.2 to 7.7, and the variation among years was minor.

Management Indicator 3 (adult female survival in the study population): For this report we expanded the original Management Indicator 3 to estimate survival for adult female fishers for a sequence of 2-year groups of demographic data and included results for juvenile and subadult females,
and estimated population growth rates. Adult female survival ranged from a low of 0.69 in Year group 3 to a high of 0.86 in Year group 4. Relatively low levels of survival and reproduction suggested the population was in decline ($\lambda < 1.0$) between 2008 and 2010, stable between 2010 and 2012 ($\lambda \approx 1.0$), and increasing by 3-4%/year during 2012 to 2014 ($\lambda = 1.04$ and 1.03, respectively).

Occupancy modelling indicated that fishers reduced their use of forest patches exposed to higher levels of restorative fuel reduction; i.e. persistence of occupancy declined with additional acreage treated for fire resiliency. However neither restorative nor extractive (i.e. commercial thinning) fuel reduction was related to either initial probability of occupancy or local extinction. This pattern is likely due to interaction of several factors. First, the overall spatial scale of treatments, both restorative and extractive, is relatively small compared to a fisher’s home range. Second, evidence indicates that fishers simply shift their space use patterns to avoid small treated areas. And third, evidence indicates that the reduction of surface and ladder fuels may change the small mammal community, therefore limiting fisher prey availability.

We found that SPLATs caused an immediate 6% reduction in potential fisher habitat. However they also moderated the impact of fire, resulting in greater available fisher habitat within 30 years. In the absence of simulated fire, the amount of habitat steadily increased over time due to forest succession, and was actually slightly greater on the treated landscape in year 30 than in year 0. The net benefits of SPLATs for the Pacific fisher will depend upon the true, but unknown, probability that high-severity fire effects will occur on a given portion of the landscape. However, future probabilities for specific fire behaviors (e.g., crown-fire initiation) are difficult to estimate, and it is therefore difficult to quantify trade-offs associated with SPLATs in absolute terms (Finney 2005). We further note that the SPLATs which were implemented at Sugar Pine appeared to have relatively modest impacts on forest structure and simulated fire behavior, and that it may be necessary to evaluate additional SPLATs of different intensities over a larger scale to fully assess the effects of SPLATs on fisher habitat.

Fishers have been the focus of systematic monitoring in the southern Sierra Nevada since the mid-1990s. Recent analyses of baited track plate detection histories from 2002 to 2009 found no evidence that the population trajectory for fishers in the area has been significantly positive or negative, based on constant and positive persistent values (Zielinski et al. 2013). In contrast, recent genetics research suggests that the fisher population in the SNAMP Fisher study area was produced by a significant post-1990s population expansion involving dispersal of animals from south of the Kings River (Fisher Core Habitat Area 4) (Tucker et al. 2014). However genetic data are not typically used to
make inferences about population processes over extremely short periods in evolutionary time, and an expansion of such magnitude (approx. 30% range increase) would require a significantly positive population growth rate.

The combination of an overall negative population growth rate and the relatively small estimated number of fishers in Fisher Core Habitat area 5 \((n = 93, \text{range } 80-107)\), warrants concern for the long term viability of the fishers in the region. Any small population will be at high risk to stochastic events such as disease and large perturbations to critical habitats (e.g. forest fires or drought; Noss et al. 2006), and genetic limitation resulting from genetic drift after founder events (Tucker et al. 2014) will hinder population recovery and expansion (Reed et al. 2003). Minimum viable population size has been under debate (Shoemaker et al. 2013, Reed and McCoy 2014), but at <500 individuals (Spencer et al. 2014), the current southern Sierra Nevada fisher population will likely require active management and conservation measures to maintain a positive growth rate across its entire range. The estimated population growth rate in the SNAMP Fisher study area reaffirms the vulnerability of the small, isolated population to external threats (Spencer et al. 2014), especially wildfires that are likely to increase in frequency and intensity with climate change. Moreover, the SNAMP Fisher study spanned a limited period of six years during which multiple novel threats to fisher survival within the study area were identified, and when three large wildfires significantly reduced availability of suitable habitat for fishers immediately to the south and north of the study site. We recommend continuous monitoring of the status of fisher populations in the southern Sierra Nevada region. Development of ways to mitigate for major threats to fisher survival and fisher habitats and population viability analyses are necessary for evaluating the long-term prospects for fishers in the southern Sierra Nevada. Data from the SNAMP Fisher study have provided important new insights on the status of a fisher population at the north margin of their current distribution in the southern Sierra Nevada Range, which will be useful towards developing a comprehensive conservation strategy for fishers in California.
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Introduction

Fishers are a medium-sized mammal carnivore with a pre-European distribution encompassing the boreal forest zone of Canada, the Great Lakes region and northeastern United States, a relatively limited portion of the Rocky Mountains in the United States, and mountainous areas of Washington, Oregon, and California, USA (Powell 1993). Ecologically, fishers are a mature or old forest-obligate species (Zielinski et al. 2005), and in central to eastern Canada and the northeastern United States their numbers were reduced historically by the combination of intensive trapping and loss of forest habitats. Although fishers have recovered portions of their former range in this region aided by reintroductions and more sustainable levels of timber harvest (Lewis et al. 2012, Powell et al. 2003), they remain uncommon to rare in the western states and provinces of the USA and Canada (Lofroth et al. 2010).

The fisher of the US Pacific states, or the West Coast Population Segment, is a candidate for listing under the US Endangered Species Act and is the target of recovery and conservation efforts (Lewis et al. 2012). The US Fish and Wildlife Service (USFWS) is under court order to make a proposal to list (or not) the West Coast population by September 2014 and to make a final decision to list, if proposed, by September 2015 (Center for Biological Diversity 2013). The fisher is also a candidate for listing under the California Endangered Species Act pursuant to a 2012 court order that compelled the California Fish and Game Commission to set aside its 2010 finding that listing was not warranted (Center for Biological Diversity v. California Fish and Game Commission et al. 2012). The California Department of Fish and Wildlife (CDFW) is reviewing the status of fishers in the state pursuant to the court order, with recommendations concerning listing to the Commission expected in 2014 (Spencer et al. 2014).

In the west coast states of the USA fishers currently exist in three remnant populations in southern Oregon, northern California, and the southern Sierra Nevada, California (Zielinski et al. 2005). In California the fisher occupies less than half of its historical range as described by Grinnell in the early 1900s (Grinnell et al. 1937). The decline in range extent and abundance of fishers in California into 2 remnant populations geographically separated by around 400 km had been considered due to widespread timber harvest and fur trapping during the early to mid-1900s (Zielinski et al. 2005), but recent genetic research suggests that the northern California and southern Sierra Nevada populations may have been genetically isolated prior to European settlement (Tucker et al. 2012). Notwithstanding uncertainty regarding the timing or cause of the range retraction, there may be fewer than 500 total fishers in the southern Sierra Nevada population (Spencer et al. 2011), where the species currently occupies approximately 4,400 km² of mid-elevation, mixed-coniferous forest (Spencer et al.
While fishers in the western US are considered at risk of extirpation from disease and other factors (Lofroth et al. 2010; Spencer et al. 2014), the recent reintroduction of fishers at one site in Washington state and another in northern California is promising for maintaining the species.

**Fisher Range Size and Trends**

Grinnell et al. (1937) described the original range of the fisher in California as including the entire western slope of the Sierra Nevada, the southern Cascades, Klamath Mountains, and northern Coast Range, a total area of ~100,000-110,000 km² (Spencer et al. 2014). Lofroth et al. (2010) estimated that the current range of the fisher in California represents <50 percent of the historical range, and fishers are currently absent from most of the northern and central Sierra Nevada, leaving a ~400-km gap separating the two populations in the state (Zielinski et al. 1995) (Fig. 1), one in the northern Coast Range and one in the southern Sierra Nevada (Spencer et al. 2014). Recent analysis suggests that these two population regions may have been genetically isolated prior to European

**Figure 1.** Current estimated distribution of fisher habitat in California, and location of major fisher field studies (Table 1 provides details on the field studies). Current distribution based on minimum convex polygon enclosing recent (>1970) fisher locality points from a comprehensive USFWS fisher locality database; map and methods used to produce the distribution boundaries are described in the Southern Sierra Nevada Fisher Conservation Assessment (Spencer et al. 2014). Question marks illustrate uncertainty on the degree to which eastern and northern portions of historical range were actually occupied.

**Illustration 3:** Camera trap image of a female fisher from SNAMP.

**Map source:** Spencer et al. 2014
settlement of California (Knaus et al. 2011, Tucker et al. 2012). Spencer and Zielinski (in review) used an updated fisher locality database to estimate their current geographic range in California at 55,000-60,000 km², with ~45,000-50,000 km² in northern California and 10,000-12,000 km² in the southern Sierra Nevada. Although the range areas estimated by Spencer and Zielinski (In review) included a mix of suitable and unsuitable habitats, the analysis suggested a 30-50 percent reduction compared to the historical range of the species. Caveats included that there is uncertainty about how wide “the gap” was historically, and how much of the mid elevation forest areas in the northern and central Sierra Nevada were actually occupied (Spencer et al. 2014).

**Table 1.** Major recent and ongoing field studies focused on the distribution, population biology and habitat use/requirements for fisher populations in California.

<table>
<thead>
<tr>
<th>Study name</th>
<th>Location</th>
<th>Period</th>
<th>Brief description of research focus</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sierra Nevada Adaptive Management Project (SNAMP Fisher)</td>
<td>Bass Lake Ranger District, Sierra National Forest</td>
<td>2007-2013</td>
<td>Comprehensive study; population biology, space use, responses to vegetation management</td>
</tr>
<tr>
<td>USFS PSW Kings River Fisher Project (KRFP)</td>
<td>High Sierra Ranger District, Sierra National Forest</td>
<td>2007-2018</td>
<td>Comprehensive study; population biology, space use, responses to vegetation management</td>
</tr>
<tr>
<td>USFS PSW Sugar Pine Fisher Project (SPFP)</td>
<td>Bass Lake Ranger District, Sierra National Forest</td>
<td>2014-2016</td>
<td>Continuation of the SNAMP fisher research effort to document post-treatment population status.</td>
</tr>
<tr>
<td>USFS PSW Fisher Regional Monitoring Program (Regional monitoring)</td>
<td>National Forests in the southern Sierra Nevada: Stanislaus, Sierra, Sequoia, Inyo</td>
<td>2002-present</td>
<td>Landscape-level occupancy monitoring</td>
</tr>
<tr>
<td>Southern Sierra Fisher and Marten Study (SSN fisher and Marten)</td>
<td>Tule River Ranger District, Sequoia National Forest, Tule River Indian Reservation</td>
<td>1994-1996</td>
<td>Comparative study; marten and fisher home range, habitats, diets, and interspecific competition</td>
</tr>
<tr>
<td>California Department of Fish and Wildlife Fisher Translocation (Stirling translocation)</td>
<td>Stirling Management Area of Sierra Pacific Industries, Butte, Tehama counties</td>
<td>2009-present</td>
<td>Monitoring of introduced fishers; population biology, habitat and space use</td>
</tr>
<tr>
<td>Hoopa Valley Fisher Study (Hoopa Study)</td>
<td>Hoopa Valley Indian Reservation, Humboldt County</td>
<td>2004-present</td>
<td>Comprehensive study; population biology, space use, responses to vegetation management</td>
</tr>
</tbody>
</table>
Population Size and Trends

The southern Sierra Nevada fisher population is small (≈500 total individuals and <300 adult fishers; Spencer et al. 2011), but appears to be stable over about the past decade (Zielinski et al. 2013). Following substantial population contractions in the past (Knaus et al., 2012), fishers in this part of California may have expanded in the late 20th century (Tucker et al. 2014). The overall distribution of fisher in the southern Sierra Nevada has been monitored by a mix of track plates and motion detecting cameras since the mid-1990s (Truex et al. 1998, Zielinski et al. 2005, Jordan 2007). Zielinski et al. (2013) analyzed occupancy records from this effort for the period 2002 to 2012, when a systematic survey design was in place, and found no detectable change in occupancy for the entire area or for any of the three subareas examined (Zielinski et al. 2013). The Zielinski et al. (2013) analyses suggest that despite fishers being protected from fur harvest for over 60 years during a time when large scale clearing of forest habitat was diminished (Collins et al. 2010), this population isolate is not showing significant evidence of numeric or spatial recovery. However, genetic patterns and survey data suggest that the population north of the Kings River may have expanded during the 1990s, before the regional monitoring program was established (Tucker et al. 2014).

Insight from prior research in the High Sierra District, Sierra National Forest (KRFP study, ≈60 Km southeast of the Key Watersheds) suggests that fisher population densities range from 0.07 to 0.28 fishers/km² (Jordan et al. 2011, Thompson et al. 2012). Records from research in northern California (Hoopa Study) indicate the potential for fisher densities to change rapidly. In the Hoopa Valley area of Northern California, fisher densities were estimated at 0.52 fishers/km² in 1998, but fell to 0.14 fishers/km² in 2005 (Matthews et al. 2013). Due to the apparent variability in density estimates, developing precise density estimates for different subpopulations and in different habitat types is critical for effective management.

Management and Conservation Planning

Federal and state resource agencies are currently developing strategies to aid in the maintenance of viable populations of fishers in both northwestern California and in the southern Sierra Nevada. As part of a cooperative agreement between the Conservation Biology Institute and USDA Forest Service Region 5, and with input from a team of 13 fisher researchers and scientists, a conservation strategy for fishers in the southern Sierra Nevada has been developed. The “SSN Fisher Conservation Strategy” is based on the findings from a conservation assessment that was previously completed by the same team of scientists. The SSN Fisher Conservation Assessment (Spencer et al.
2014) included a review of all previous published and credibly collected unpublished data on fisher ecology in the southern Sierra Nevada Region. Information from the SNAMP Fisher Project (published manuscripts, submitted/draft manuscripts, and unpublished data) were included in the SSN Fisher Conservation Assessment. In the Conservation Assessment and in association with conservation planning, the SNAMP Fisher overall study area is essentially Fisher Habitat Core Area 5 (Fig. 2).

Insights from SNAMP Fisher appear simultaneously encouraging and discouraging for management and conservation of the species. Causes of mortality were more diverse than was previously known, including evidence for periodic outbreaks of disease, and significantly higher levels of predation than previously documented for any other intensively studied population in North America. Fishers are challenged by the need to cross busy roads passing through foraging and

<table>
<thead>
<tr>
<th>Core, Status</th>
<th>Total area (suitable habitat)</th>
<th>Primary (secondary) jurisdiction</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Occupied</td>
<td>430 km² (50.4%)</td>
<td>Sequoia NF</td>
</tr>
<tr>
<td>2. Occupied</td>
<td>936 km² (62.2%)</td>
<td>Sequoia NF</td>
</tr>
<tr>
<td>3. Occupied</td>
<td>985 km² (56.4%)</td>
<td>Sequoia NP (Sequoia NF)</td>
</tr>
<tr>
<td>4. Occupied</td>
<td>751 km² (55.1%)</td>
<td>Sierra NF</td>
</tr>
<tr>
<td>5. Occupied</td>
<td>1096 km² (57.4%)</td>
<td>Sierra NF (Yosemite NP)</td>
</tr>
<tr>
<td>6. and 7. No fishers</td>
<td>1678 km² (55.8%)</td>
<td>Yosemite NP (Stanislaus NF)</td>
</tr>
</tbody>
</table>

*a* Indicates whether known occupied by a breeding fisher population.

*b* Habitat suitability based on updated modeling for the Southern Sierra Nevada Fisher Conservation Assessment.

Table 2 Adapted from Southern Sierra Nevada Fisher Conservation Assessment (Spencer et al. 2014)

Figure 2. Distribution of occupied and unoccupied fisher habitat core areas in Southern Sierra Nevada Fisher Conservation Assessment area. Habitat cores were mapped as contiguous polygons having a predicted probability of fisher occupancy exceeding 0.41, and large enough to support ≥5 adult female fishers (see Spencer et al. 2014 for details).
denning habitats, they must avoid accidental entrapment in human structures, and coexist with illegal marijuana farmers spreading poisons on the landscape that kill or sicken them and their prey. Encouraging results include that, even with these challenges, fisher survival and demography at our southern Sierra Nevada study sites was comparable to other closely monitored populations in northern California and southern Oregon not considered at imminent risk of extirpation. Management should continue to maintain suitable denning, foraging, and resting habitats, as detailed in current planning documents (North et al. 2009). Moreover, the SNAMP Fisher study spanned a limited period of six years when multiple threats to fisher survival within Fisher Habitat Core Area 5 were identified, and when three large wildfires either significantly reduced availability of suitable habitat for fishers to the south and north of Core Area 5. We believe that continuous monitoring of the status of this population and mitigation of the major threats to persistence, along with population viability analyses, are necessary for evaluating the long-term viability of fishers in the southern Sierra Nevada.

**Forest Management and Fisher Populations**

Fishers were formerly widespread in mixed conifer forests across mountainous areas of northwestern California and in the Sierra Nevada of eastern California. Populations in the Sierra Nevada appear 30-50% reduced (Spencer et al. 2014) and it is possible that the isolated population of fishers in the southern Sierra Nevada will be impacted as the USDA Forest Service implements fuel reduction measures (Strategically Placed Land Allocation Treatments; SPLATS) to mitigate risk of catastrophic wildfire (Scheller et al. 2011). Fuel reduction treatments are becoming the dominant forest management activity in western forests in response to increases in the frequency of intense, stand-replacing forest fires over the past several decades (Mallek et al. 2013, Safford 2013). Advances in fire modeling have greatly improved managers’ ability to plan and evaluate various landscape fuel treatment scenarios intended to reduce fire risks (Collins et al. 2010, Scheller et al. 2011). However, there remains a considerable gap between modeling landscape-scale fuel treatments and implementing them due to concern over the status of rare and uncommon species associated with multi-storied, late-seral stage forests, such as the fisher and spotted owl (*Strix occidentalis occidentalis*) (Naney et al. 2012, Truex and Zielinski 2013). Presence of fishers has strongly influenced managers’ ability to delineate landscape-scale fuel treatments in this fire-prone region (Collins et al. 2010, Weir and Corbould 2010). The amended Sierra Nevada Forest Plan represents the most recent attempt to reconcile the need to reduce fuel loadings in Sierra Nevada mixed-conifer forests and retain characteristics of late-successional forests that are important for these species. The strategy involves a
network of “Strategically Placed Area Fuel Treatments” (SPLATS) that allow up to a 60% reduction in basal area and a 30% reduction in canopy cover in habitats used by fishers and spotted owls. In the long-term, this strategy may increase availability of important habitats for both organisms by reducing wildfire-induced losses (Spencer et al. 2011), but treatments may impact habitat quality for fishers in the near-term (Thompson et al. 2011).

To provide a framework for balancing the habitat needs of fishers with fuel treatments intended to reduce fire risks, SNAMP initiated a coordinated effort to assess the effects of fuel treatments on many environmental features including the fisher, spotted owl, forest health, and water quality as quantity in the central Sierra Nevada. SNAMP began in 2007 and was designed to evaluate the effects and effectiveness of fuel treatments implemented according to the revised Sierra Framework (USFS 2004) under a design of stakeholder participation. SNAMP is a landscape-scale, ecosystem-level experiment in natural resource management and involves a Before-After-Control (BACI) design developed specifically to assess the impacts of SPLATs on the overall forest ecosystem (Popescu et al. 2012).

In September 2007 SNAMP Fisher was launched as 6-7 year study of fishers in the Bass Lake Ranger District, Sierra National Forest to determine population limiting factors, and to evaluate the effects of SPLATS on resource use, survival, and persistence of fishers in the southern Sierra Nevada. We used repeated surveys of small 1 km² blocks of forest habitat with automatic cameras, mark-recapture, and intensive monitoring of individual collared fisher for evaluating how SPLATS contribute to changes in habitat use, dispersal, survival, and reproduction by fisher.

**Research Goals and Primary Objectives**

1. Estimate population parameters including age and sex-specific survival, and fecundity
   a. What are the vital rates (reproduction, survival, population growth rates)?
   b. What is the population size and density in the study area?
   c. What are the patterns of dispersal movements?
2. **Identify population limiting factors in the region encompassed by the study area**
   a. What are the causes of mortality? Are predators, parasites or diseases important?
   b. What are the patterns of home range and habitat use?
3. Evaluate effects of SPLATS on occupancy, survival and fecundity
   a. Characterize resource use by fishers; do SPLATs influence habitat use
   b. What are the patterns of fisher occupancy in relation to forest management?
   c. Do patterns of fisher occupancy change before and after by SPLATS?
Project planning for the study was initiated in early 2007, and in September 2007 a research station near Bass Lake, California was established and staffed with a team of field biologists and research assistants. Field work was initiated in late October 2007. All permits were in place when we initiated live trapping on December 21, 2007 and captured the first two fishers on December 23, 2007. In late December 2007 we initiated the SNAMP Aviation program in cooperation with Forest Service Supervisory Pilot John Litton (R5 Regional Aviation Group, Lancaster, CA). One of the principle goals of the study was to maintain a minimum of 20 actively monitored fishers, a milestone that was achieved on July 23, 2008.

**Figure 3.** Overall Study Area of the SNAMP Fisher Project, including administrative boundaries, including the outer boundary of the “Key Watersheds” focal study area in the approximate center of the map region.
Site Description and Study Area

The SNAMP Fisher Project study area is at the northern end of the southern Sierra Nevada fisher population in California, encompassing the area bounded by the Merced River in the north and the San Joaquin River in the south (Fig. 3). Administratively, the focal study area for the research is the Bass Lake Ranger District in the Sierra National Forest, but early in the study a radio-collared fisher dispersed north into Yosemite National Park, which effectively expanded the research to encompass a part of Yosemite National Park (Fig. 3). Permission was granted by Yosemite National Park to monitor SNAMP radio-collared fishers for survival by fixed-wing aircraft overflights, but the research agreement did not extend to any significant ground-based activities by project staff.

The overall study area was the non-wilderness region of the Bass Lake Ranger District in the Sierra National Forest, near Oakhurst, California, and covered approximately 1300 km$^2$. This area encompasses a mix of public and private land and is topographically complex with elevations ranging from 758 m to 2652 m. Field work was carried out between 1,000 m and 2,400 m in elevation, corresponding to fisher occurrence in the region. Primary tree species in approximate order of abundance for conifers and then hardwoods in the overall study area are incense cedar (*Calocedrus decurrens*), white fir (*Abies concolor*), ponderosa pine (*Pinus ponderosa*), sugar pine (*Pinus lambertiana*), California black oak (*Quercus kelloggii*), canyon live oak (*Quercus chrysolepis*), mountain dogwood (*Cornus nuttallii*), and white alder (*Alnus rhombifolia*). Common shrubs and tree-like shrubs include whiteleaf manzanita (*Arctostaphylos viscida*), greenleaf manzanita (*Arctostaphylos patula*), mountain misery (*Chamaebatia foliolosa*), bush chinquapin (*Chrysolepis sempervirens*), mountain whitethorn (*Ceanothus cordulatus*), and snowberry (*Symphoricarpos mollis*).

Key Watersheds

We already noted that the distribution and overall abundance of fishers in the Sierra Nevada, California declined by 30-50% in association with fur trapping, and timber harvest that removed large expanses of mature and old growth forest habitats in the early to mid-1900s. One of our overarching organizing hypotheses was that fuel reduction management (SPLATS; commercial thinning, understory brush and small tree removal by hand thinning and machine mastication) might exacerbate this contraction by preventing maturation of second-growth forests in the southern Sierra Nevada to where they are less capable of supporting a long term viable fisher population. Therefore within the overall study area, we initiated more intensive monitoring within the Southern SNAMP study area:
four “Key Watersheds” that encompassed three Forest Service projects expected to occur in the study period near the communities of Fish Camp (Fish Camp Project), Sugar Pine (Sugar Pine Project), and Cedar Valley (Cedar Valley Project). The four Key Watersheds are the Sugar Pine, Nelder Creek, Rainier Creek and White Chief Branch watersheds (Fig. 4). A 1 x 1 km grid (1-km²) was overlaid for the Key Watersheds, and used to organize the sampling effort; National Forest land within each 1 km² cell were surveyed annually for fisher occupancy by automatic camera traps (O’Connell et al. 2011). Elevation within the Key Watersheds increases from 900-1100 m in the south/southwest to >2200 m

**Illustration 4 (bottom right):** Photograph of study area from Chiquito Ridge looking east

**Figure 4.** (a) Map views of Key Watersheds focal study area, including the SPLAT polygons originally produced for the Sugar Pine, Cedar Valley, and Fish Camp forest management projects. (b) Key Watersheds overlain with 1km² grid used to organize research effort for camera trap surveys. Yellow circles indicate the three communities located within the watersheds. NOTE: camera traps were placed at or very near the grid center points as plotted.
in the northeast portion of the study area. This elevation gradient corresponds with a mix of hardwoods (*California bay, Umbellularia californica, Canyon live oak, black oak,* and several conifer species at lower elevations (ponderosa pine, incense cedar; California Wildlife Habitat Relationship system MHW, PPN, and MHC habitat types), a mix of multiple conifers (*Jeffrey pine, P. jeffreyi, white fir, incense cedar,* and hardwoods (black oak, white alder, *Alnus* (ponderosa rhombifolia, mountain dogwood, *Cornus nuttallii*) between 1300 m and 1850 m (CWHR Habitat types SMC, MHC, PPN), and grading into red fir (*Abies magnifica*) and lodgepole pine (*P. contorta*) (CWHR Type RFR) above 1900 m. Giant sequoia (*Sequoiadendron giganteum*) are present, but primarily restricted to the Nelder Grove Historic Area within the Nelder Creek watershed. Permanent streams in the Key Watersheds are important for fishers and other wildlife and include Big Creek and Rainier Creek in the Rainier Creek watershed, Lewis Creek in the Sugar Pine watershed, and California Creek and Nelder Creek in the Nelder Creek watershed.

Methods

**Vital Rates and Basic Population Parameters**

**Introduction and Background**

Information on population size and demographic parameters is fundamental for managing wildlife populations, especially when declines in abundance or range size have occurred and the species is the focus of conservation management. The fisher is one such species, and it is at the center of intense conservation efforts as a candidate for listing under the USA Federal, Oregon, and California endangered species acts (USFWS 2004).

The southern Sierra Nevada fisher population is small (<500 individuals), appears to be stable over about the last 15 years (Zielinski et al. 2013a), but may have expanded from an even smaller population during the late 20th century (Tucker et al. 2014). Spencer et al. (2011) used a spatially explicit population model to estimate the potential fisher carrying capacity south of the Merced River and concluded there are probably <300 adult fishers. Fisher density estimates from the Sierra National Forest based on mark-recapture camera station data (Jordan 2007) or scat-detector dog data (Thompson et al. 2012), suggest that the total population (including adults, subadults, and some juveniles) could number up to ~450 total fishers in overall southern Sierra Nevada region of California.

The ecology and habitat use of fishers in the southern Sierra Nevada has been the focus of research since the mid-1990s (Jordan 2007, Mazzoni 2002, Truex et al. 1998). Insight from prior
research in this region suggests population densities may vary from 0.07 to 0.28 fishers/km² (Jordan et al. 2011, Thompson et al. 2012). Information on current density is needed for other areas of the southern Sierra Nevada as well, because as the area of suitable habitat available to fishers in the southern Sierra Nevada is refined by improved modeling (Spencer et al. 2014), density values can be used to develop more accurate estimates of fisher abundance for conservation planning. Although regional occupancy trends (Zielinski et al. 2013a) suggest that the southern Sierra Nevada fisher population is relatively stable, records from elsewhere indicate fisher densities can change rapidly. On the Hoopa Fisher Project study area in northern California, density was estimated at 0.52 fishers/km² in 1998, but fell to 0.14 fishers/km² in 2005 (Higley et al. 2013).

Resource agencies are currently developing strategies to aid in maintaining viable populations of fishers in the southern Sierra Nevada. Data from SNAMP Fisher have aided this process by providing current data on population size and demographic parameters for the species in an area at the northern margin of their current range in the southern Sierra Nevada. Using data on detections of fishers from camera surveys and live trapping to estimate population size and density and estimates of reproductive rates and fecundity from close monitoring of denning behavior, we integrated data on survival and demographic rates into a matrix model to estimate the population growth rate.

Field Methods

Live Trapping.--Although noninvasive methods can be used to generate important data on wildlife populations (Long 2008), estimating vital rates (survival, reproduction, dispersal) almost always requires trapping to radiocollar and then closely monitor the study animal. We followed standard live-trapping procedures previously developed for fishers in California (Jordan 2007, Matthews et al. 2013a), with only a few minor changes. Individual fishers were live-captured in steel mesh traps (Tomahawk Live Trap Company, Tomahawk, WI) modified to include a plywood cubby box to provide the animals with a secure refuge where they were less likely to injure themselves (Wilbert 1992). Trapping to mark animals with radiocollars was focused during the fall and winter seasons between December 2007 and March 2012. Also, with the exception of the first year of the study when we needed to capture fishers to initiate the study, we did not trap during the spring denning period (late March to mid-June) to minimize disturbance to reproduction. Live traps were baited with venison, and checked daily by late morning. Captured animals were restrained in a handling cone, and sedated using a mixture of Ketamine hydrochloride and Diazepam (1 mg Diazepam/200 mg Ketamine) injected intramuscularly. Sedated fishers were weighed, classified by age and gender based on
examination of teeth and genitalia, and measured for standard morphological features. Small samples of ear tissue were collected for microsatellite DNA analysis using a sterile dermal biopsy punch. Several strands of hair were removed from the nape and rump region, also for DNA analysis. Hair samples were stored in a dry paper envelope, whereas tissue samples were stored in 95% ethanol until analysis at the USDA Forest Service Wildlife Genetics Lab (Rocky Mountain Research Station, Missoula MT). Teats on females were measured for base diameter and height using digital calipers (± 1mm), and those data were used to identify females that weaned at least 1 kit when they had not been monitored during the denning period (Matthews et al. 2013b). Each animal was permanently identified by subcutaneous insertion of passive integrated transponder (PIT) tags (Biomark, Boise, ID), and fitted with a radio collar (Holohil Systems Model MI-2M, Ontario, Canada) modified by attaching small bands (0.5-1.0 cm) of infrared reflective tape (3M® Scotchlite™) along the lengths of the antennas. Custom breakaway devices were inserted into radiocollars fitted to juvenile fishers to reduce the risk of injury or strangulation between recaptures (Thompson et al. 2012). Bands of infrared reflective tape and breakaways were modifications used in previous studies. After handling, we returned animals to the cubby box and released them at the point of capture after recovery from anesthesia. Capture and handling procedures followed American Society of Mammalogist guidelines (Sikes and Gannon 2011), and were approved by the Institutional Animal Care and Use Committee of the University of California, Berkeley (protocol R139).

Live-trapping is labor intensive, and the effort was designed to gain advantage from detections of non-collared fishers at camera traps. Live traps were most frequently placed in the same area of camera trap stations after cameras had been removed (to prevent interference with camera surveys). Data from camera detections were used to design linear traplines of 5-10 traps bracketing positive detection stations. Distance of separation between traps was typically ≥500 m, and traplines were usually successful at capturing targeted animals within five nights of trapping. Live-trap success was further enhanced in later years of the study by placing traps in locations where fishers had been captured in the past. Trap success was also enhanced by cleaning and sanitizing traps after captures. In winter, snow falling from tree branches can ice up the treadle mechanism inside live traps. We used lightweight, rectangular canvas tarps (1.12 width, 1.36 length) to protect the inside of the live traps from falling snow, and debris used to camouflage the traps. Traplines were generally removed the day after targeted fishers were captured, and always after 10 nights of trapping when no fishers were captured.
Aerial Telemetry and Radiotelemetry Monitoring.—Tracking radiocollared animals from an aircraft is an alternative to locating them from the ground by homing or triangulation (Thompson et al. 2012). Researchers have been using fixed-wing aircraft to locate wildlife since the early 1970s (Mech 1974). The unique ability of observers in aircraft to rapidly search and locate radiocollared animals over large and inaccessible areas while allowing for nearly line of sight reception between transmitter and receiver makes aerial radio telemetry an attractive research technique in general (Gilmer et al. 1981), and specifically for studying fishers, which often occur in remote mountainous areas where access can be difficult (Weir and Corbould 2008). Partly for these reasons, we used fixed-wing airplanes to monitor and relocate radiocollared fishers for the entirety of the SNAMP Fisher Project. Beginning in December 2007, we worked with USDA Forest Service Supervisory Pilot John Litton to develop an aviation program in support of SNAMP Fisher, which was fully established in August 2008 when a full time pilot was hired and the first of two dedicated aircraft were based at the Mariposa-Yosemite Airport in Mariposa, CA.

The two USDA Forest Service-owned aircraft acquired for supporting the project were a Cessna (Cessna Aircraft Co., Wichita, KS) and a Piper PA-18 Super cub (Piper Aircraft Inc., Vero Beach FL). Two aircraft were considered necessary to maintain continuous monitoring of radiocollared fishers when routine maintenance or engine repair was necessary (John Litton, personal communication).
The optimal search procedure used when locating animals from light aircraft varies depending on the number of animals tracked, and the antenna configuration supported and approved for the airplane being used (Gilmer et al. 1981). Additional details are provided elsewhere (Thompson et al. 2012), but we used two, 2-element H antennas (Telonics Inc., Mesa, AZ) mounted in a sideways configuration on each wing strut, and a single 3-element Yagi antenna (Advanced Telemetry Systems, Isanti, MN) mounted forward-facing on the right wing strut. This antenna configuration was effective in allowing the pilot and biologist to search for radiocollared fishers using the Yagi antenna (detection range 5-20 km), and then switching to the H-antennas to localize to a relatively precise location above the animal using a circling technique (Seddon and Maloney 2004).

Fixed-wing flights (aerial telemetry missions) to locate radiocollared fishers in the study area were scheduled in advance for 4-6 missions/week, depending on weather conditions considered safe for departure and return to the Mariposa-Yosemite Airport. Flights typically occurred in the morning hours, and lasted 2-3 hours. Afternoon telemetry flights were relatively infrequent, and the large majority of aerial radiotelemetry locations were acquired in the AM period of the day. As part of each aerial-telemetry mission, we systematically searched for all active radiocollars deployed on fishers in the study area. Biologists in the airplane recorded (1) active/inactive status for each fisher, (2) time of location, (3) an estimated UTM location for each fisher (typically logged into a handheld GPS unit; Garmin 60 CSx, Olathe, KS), (4) a qualitative ranking for each location (poor, fair, good, excellent), and (5) a record of any radiocollared fishers that were not located. Additional descriptive details were often recorded related to the nature of weather conditions influencing the aircraft at the time of the location (turbulence, “bumpy”, clouds occluding visibility to the ground, etc.), or if the animal had moved an unusual distance or to an atypical area. At the end of each aerial telemetry mission, the

Illustration 7: Forest Service Cessna 185 airplane, and antenna configuration on the right side wing strut.
biologists summarized details on departure and return times and weather and flight conditions during the flight.

Aerial radiotelemetry can be efficient for locating animals that range over large areas in difficult terrain (Gilmer et al. 1981), but the accuracy, or precision of aerial telemetry locations is generally less than for ground-based radiotelemetry (e.g. triangulation; Koen 2005, Gantz et al. 2006). Location error from fixed-wing airplanes varies with flight speed, elevation above ground level, pilot and biologist experience, and signal reflection in rugged topography (Thompson et al. 2012). We assessed error for aerial radiotelemetry locations on the SNAMP Fisher project by calculating the Euclidean distance between GPS locations logged by biologists in the airplane and positions of test collars placed at known locations on the ground. Test collar locations were generally radiocollars that were placed in locations unknown to the biologist in the airplane; biologists were required to regularly estimate positions for test collars during aerial telemetry missions. Other aerial radiotelemetry locations used to quantify accuracy included dropped radiocollars, carcass locations, fishers in live traps, and female fishers in a cavity in a den tree whose locations were also unknown to the biologist in the airplane.

Fisher Reproduction

Background.--Den sites, where female fishers bear and raise their kits, are probably the most limiting habitat element for fisher populations in California. Females typically use more than one den during the denning season (late March to mid-June). Natal dens are where adult female fishers give birth and initially care for young, and they may then move kits to one or more maternal dens from early April to June until they are weaned (Powell et al. 2003, Matthews et al. 2013a). Reproductive dens, both natal and maternal, are nearly always cavities in large trees, live or dead, and are found in forest stands with dense canopy cover and complex multi-layered structure (Zhao et al. 2012). Suitable denning sites are likely a subset of suitable resting sites because the requirements are more stringent: (1) den cavities must be large enough to shelter both mother and kits for weeks rather than days; (2) the female needs to provision her young while they are restricted to the den, so dens must be located close to high-value foraging areas; and (3) denning begins in late March-early April, when temperature are colder and slope position may be more critical in assisting with kit thermoregulation.

Identifying den trees and evidence of reproduction.—Female fishers exhibiting behavior consistent with denning were identified during late March-mid April and then monitored. Denning
behavior was characterized by an abrupt change from a pattern of successive aerial radiotelemetry locations being dispersed within a female’s home range, to a pattern where locations were spatially clustered (3-5 locations within 500 m over a 7-day period; Zhao et al. 2012). When clustering of locations occurred, a biologist navigated to the area with a handheld Global Positioning System device to investigate. Standard ground-based radiotelemetry techniques with a handheld receiver (model R1000; Communication Specialists, Inc. Orange, California) and an H-type antenna were then used to home towards telemetry signals of radiocollared females. Once a collared female was isolated in an area, the biologist circled the fisher until the individual tree or snag was identified (Matthews et al. 2013a). When female fishers were localized to a possible den structure, 2-4 automatic “den cameras” that had been cleaned and de-scented were attached to nearby trees and focused on the bole of the den structure (scent and bait lures were not applied around den trees to avoid attracting other predators). We returned to these structures the day after initial placement of den cameras, and then every 3-5 days to confirm use for denning based on regular occupancy and images indicating up and down movements on the tree or snag. Trees and snags used ≥3 times in succession and with camera-based evidence of up-down movements were considered denning structures (Zhao et al. 2012). We defined “denning opportunities” as the total number of individual, breeding-age female fishers (≥24 months) either directly monitored in mid-March to June (Matthews et al. 2013a), or measured for teat size during July to January to assess weaning status (Matthews et al. 2013b). We considered kits weaned when denning behavior continued until 31 May or later (Matthews et al. 2013a), unless the female was known to have died before June 30.

Activities of known-denning female fishers were chronicled for the duration of each denning season by continuous monitoring with cameras and ground checks of den trees. Female fishers typically transfer kits from natal den trees in which they were born to 1-6 other maternal dens during April to June (Matthews et al. 2013a). Each time we had evidence that a denning fisher moved kits to a new maternal den (images of females transporting kits away from den trees, cessation of occupancy over multiple checks), we searched for the female using ground telemetry and repositioned cameras around the next den structure (Zhao et al. 2012). Den cameras were removed in mid-June when females ceased localizing to den structures.
Information on litter size was determined from images from den cameras, or, less frequently, by climbing den trees and using a video camera (Peep-A-Roo Video Probe System, Sandpiper Technologies, Manteca, CA) to count kits inside den cavities (Matthews et al. 2013a). We minimized disturbance to denning females by (1) restricting visits to den structures to service cameras to once every 3-5 days, (2) using deployments of multiple den cameras for obtaining the majority of kit counts, and (3) by not approaching den trees for climbing until ground-based telemetry indicated the female was well away from the den structure (Zhao et al. 2012).

Maximum reproductive rate was estimated as the sum of the number of adult-age female fishers in the population that localized to den trees during the den season plus the number of adult females with enlarged teats that were not monitored but captured and measured before January, divided by the number of adult-age female fishers in the population during mid-March to late January. Weaning rate was estimated as the number of adult-age females known to have survived and localized to den trees through May plus those with enlarged teats that were captured after the den season, divided by the number of adult-age female fishers in the population during mid-March to late January. We note that measurements of teat size were shown to correctly identify over 90% of current year reproducing females that weaned at least 1 kit, and all but 3 adult females for which teat measurements were used to determine reproductive status were part of the Matthews et al. (2013b) dataset. Annual estimates of fecundity were calculated as the total number of weaned kits/number of females with known kit counts.

Illustration 8: Female fisher F46 moving a 1-month old fisher kit from her natal den tree.
Habitat Characteristics within Fisher Denning Areas.--Denning structures are considered a limiting habitat element for fishers in California and elsewhere (Weir et al. 2012), but site characteristics immediately surrounding the denning structures are also important (Zhao et al. 2012). Current forest vegetation management to reduce hazardous fuel levels, improve the vigor of selected trees (pines and oaks), increase spatial heterogeneity, and provide forest products for society may impinge on denning habitats in a variety of ways (Naney et al. 2012, Powell and Zielinski 1994). These management actions can negatively affect fisher habitat (Weir and Corbould 2008), at least in the short term (Thompson et al. 2011), while others may have little impact on fisher habitat suitability (Spencer et al. 2014). Without detailed “local scale” information on habitat characteristics directly associated with fisher denning structures, it will not be possible to adequately manage Sierra Nevada forests in ways that will maintain viable fisher populations.
Habitat characteristics for denning structures.—We developed a protocol for determining the combination of biotic and abiotic characteristics female fishers are likely selecting/using for denning habitats. The protocol was designed to collect similar types of data as those being recorded by the Forest Health Team on the Core Plots in the Sugar Pine area, while also capturing the same types of data being recorded by the USDA Forest Service PSW Kings River Fisher Project at den trees used by fishers in the High Sierra District, Sierra National Forest. Full details on how different habitat data were assessed are provided as an Appendix. Briefly, we used an 18m radius circular plot centered on the denning structure (Fig. 5) to organize collection of data on (1) canopy cover, (2) litter, duff, and coarse woody debris (associated abundance of fuels), (3) cover and height of herbaceous plants and understory woody shrubs (concealment cover), (4) slope and aspect, and (5) size, number, and height of trees and snags (3 size classes, 4 crown classes). Data on habitat characteristics for den trees were typically collected during late spring or summer, and always when the den trees were no longer in use for denning.

Fisher Survival

Background.--Understanding survival and the details of cause-specific mortality is fundamental for insight into the population biology of any species, and crucial for understanding the limits to population growth and recovery for rare or endangered species of wildlife. Historical loss and fragmentation of important habitats, combined with overexploitation by hunting and trapping are the
most common drivers of endangerment of wildlife (Lande 1993). Although changes in management may sometimes succeed in reversing problems associated with loss of critical habitats, emergence of new threats that impinge on survival or reproduction can counteract improvements that might otherwise reverse population declines. Emerging threats to survival and population persistence may be obvious such as exposure to novel pathogens and increased occurrence of road-kill deaths (Gaskill 2013, Litvaitis and Tash 2008), or less discernible and linked to changes in community structure or composition that produces an imbalance in predator-prey relations (Roemer et al. 2001).

Factors that contribute to limited growth and expansion of fisher populations in the southern Sierra Nevada are likely linked to a combination of resource and population phenomena. Fishers may be challenged by limited access to suitable resting and denning habitats (Purcell et al. 2009) or insufficient numbers of prey (Zielinski and Duncan 2004), whereas survival may be reduced by high rates of predation (Wengert 2013), wildlife-vehicle collisions (Chow 2009), and exposure to anticoagulant rodenticides (Gabriel et al. 2012a, Thompson et al. 2013). Although habitat requirements of fishers and their responses to forest management are increasingly well known (Aubry et al. 2013, Garner 2013, Zielinski et al. 2013), it has only recently been documented that high numbers of otherwise healthy fishers were succumbing to attacks by other forest carnivores (Wengert et al. 2014) and that illegal use of anticoagulant rodenticides and other toxicants associated with illegal marijuana grow sites on national forests and parks in the southern Sierra Nevada was contributing to both direct mortality and reduced survival of fishers in this region (Gabriel et al. 2012a, 2013, Thompson et al. 2013). Because of heightened concern over the stability of the small population of fishers in the southern Sierra Nevada, our primary objective was to evaluate factors contributing to variation in survival among fishers in the region. Young mammals (particularly males) often experience higher mortality early in life associated with dispersal and establishing independent home ranges (Chepko-Sade and Halpin 1987), and general naiveté with predators and other environmental risks (Farias et al. 2005, Murdoch et al. 2010). Fishers typically disperse before they reach maturity at ≈ 24 months (Arthur et al. 1993), when lower survival related to this life history event might be evident. Fishers may experience lower survival during fall and winter due to the combined effects of higher energetic costs associated with deep snow cover (Powell 1979) and prey limitation when several species of rodent prey (Zielinski and Duncan 2004) enter into torpor. Therefore, we hypothesized that (1) survival would be lower for juvenile and yearling fishers compared to adults, (2) males would experience lower survival than females related to higher rates of movement and potential longer dispersal distances, and (3) survival would be lower during fall and winter than in spring and summer.
**Determination of survival rates.**—We monitored the status (alive, dead, or missing) of radiocollared fishers from time of first capture until death, censorship (due to dropped or failed collars on animals that were not quickly recaptured), or the end of the study. Breakaway devices in the radiocollars occasionally resulted in premature detachment, requiring efforts to re-collar animals that were short-term missing (1-2 months). Because of the relatively common incidences when animals were missing for less than one month, we evaluated survival on monthly intervals rather than weekly or biweekly. Overall patterns of survival were determined using the Kaplan-Meier (KM) staggered entry method (Koen et al. 2007, Pollock et al. 1989, Price et al. 2010). KM models were used to produce estimates for annual survival and combined year survival (data pooled by month across all years) by population year. The population year was defined as April 1 to March 31 based on the timing of reproduction for female fishers in California with most offspring produced during March 21 to early April (Matthews et al. 2013a). Annual survival can be moderately to highly variable and may result in a negative population growth trajectory that may not be appropriate for a long-lived species with a generation time of two or more years. We therefore combined monthly data on survival status for individual fishers for five 2-year periods (population years 2 and 3, population years 3 and 4, population years 4 and 5, population years 5 and 6, and population years 6 and 7). Live-trapping to capture young-of-the year juveniles was focused during mid-October to February (a few juvenile fishers were occasionally captured before October or in early March). Kaplan-Meier models to estimate “annual” survival for juveniles were typically initiated in December, thereby producing survival estimates for juveniles for a 3-4 month period from December or January to March. When data for juveniles were pooled across population years, however, the dataset allowed for evaluating juvenile survival for the all years combined model for the 6 month period from October to March. Z-tests were used to compare estimates for combined year survival for all possible age and sex combinations. Significance levels (α) for multiple comparisons were adjusted for Type I error rates using the Bonferroni procedure (McCann et al. 2010).

**Causes of Mortality**

*Background*—Understanding the details of cause-specific mortality provides insight necessary to understand overall survival in relation to factors that are most likely to limit population growth and recovery for rare or endangered species of wildlife. Factors that contribute to limited growth and expansion of fisher populations in the southern Sierra Nevada, California are likely linked to a combination of resource and population-level phenomena. In addition to the challenges described...
previously, it has been hypothesized that the recent change to more open canopy forest conditions with
an understory of small trees and more shrubs is contributing to higher rates of predation by bobcats
(*Lynx rufous*) and coyotes (*Canis latrans*) (Wengert 2013). Although the habitat requirements for
fishers are generally known (Lofroth et al. 2010), details of cause-specific mortality in the southern
Sierra Nevada had not been rigorously evaluated until the SNAMP and KRFP studies were initiated in
2007.

*Monitoring to detect mortality.*— All radio-collars fitted to fishers on the SNAMP study were
equipped with mortality or activity sensors allowing us to detect inactive signals and investigate fisher
mortalities and recover carcasses soon after death in most cases. When a mortality signal was
detected, immediate attempts were made to investigate the area of the signal to recover shed
radiocollars or recover the carcass when an animal was confirmed dead. Carcasses were generally
recovered within 24 hrs of death, aided by the near daily aerial radiotelemetry missions.

Radiocollared fishers have been monitored effectively and relocated by ground-based
radiotelemetry as part of KRFP study centered approx. 60 km southeast of the SNAMP Fisher Study
Area (Garner 2013). However, most prior studies have been unable to identify causes of death for
many deceased study animals because carcasses were not retrieved within 12-48 hours after death
(Truex et al. 1998, Aubry and Raley 2006, Jordan 2007). Decomposition begins immediately after
death, which can prevent identification of underlying disease processes directly linked to death
(Gabriel 2013, Keller et al. 2012). Because of this, our primary rationale for monitoring radiocollared
fishers by fixed-wing aircraft up to 6 days/week was to recover carcasses of animals as soon after
death as possible. The protocol that was in place from the start of the study until approx. June 2012
was for the biologist in the airplane to use the audio system in the airplane to (1) transmit the estimated
location coordinates for any radiocollared fishers detected on inactive pulse to the research office,
whereupon (2) a staff biologist in the vicinity would immediately investigate the location and recover
the carcasses following an approved forensic protocol, (3) transport the carcass to the research office
where they were placed in -20°C freezer for storage until (4) a necropsy could be scheduled at the UC
Davis School of Veterinary Medicine.

Once a radio-collar is activated and deployed, it will typically function (emit a radio signal) for
18-24 months until the battery is expended. In 2011 the SNAMP Fisher project began using
radiocollars from Advanced Telemetry Systems (ATS), but discovered that many of the electronic
“mortality switches” built into the ATS radiocollars became defective within 8-10 months of being
deployed on study animals. Electronic mortality switches are designed to emit pulses at twice the
normal pulse rate when the radiocollar has been stationary for at least 8 hours. When the mortality switches in the ATS brand radiocollars became defective they began to emit intermittent and then consistent false inactive signals. SNAMP Fisher was forced to modify the inactive signal protocol by first plotting locations determined for inactive signals in ArcGIS, whereupon a decision on whether to investigate the location was based on a judgment of the distance of separation between successive aerial radiotelemetry locations (investigation triggered when successive locations were <1000 m apart). The revised protocol was situation specific: the first time an ATS collar was detected emitting an inactive signal, efforts were made to investigate the location immediately. Subsequent inactive signals from that collar were examined carefully prior to on-the-ground investigation. This process may have delayed the recovery of a limited number of carcasses. Efforts were made to correct for the problem of false inactive signals by replacing defective collars with collars of a different manufacture as soon as possible.

When fisher carcasses were discovered we followed a standardized forensic protocol for collecting samples and documenting circumstances at mortality sites using photographs and diagrams of mortality sites (Wengert et al. 2013). When predation was suspected as the cause of death (e.g. obvious punctures, partial consumption), we recorded information on the characteristics of the predation event including patterns of consumption and evidence of caching or burying. Samples included swabs of visible bite wounds, clipped fur from near the bite wounds (clipped to avoid fisher DNA in root bulbs), swabs of the claws and teeth, and any non-fisher hairs left on or near the carcass (Wengert et al. 2013). Carcasses were double-bagged in plastic bags, labeled, and transported back to the field offices where they were frozen in a -20°C freezer until being shipped to University of California, Davis for necropsy.

Pathology.--We submitted all carcasses for necropsy and disease and DNA assessment to cooperating pathologists at the University of California Davis, Veterinary Medical Teaching Hospital, and California Animal Health and Food Safety Laboratory in Davis, CA. When possible, the team of pathologists determined cause of death for each fisher using all available information, including necropsy examination, disease and toxicological results, DNA forensics, evidence recovered or identified as important from the mortality site, and habitat characteristics around the carcass. During necropsy, liver samples were collected and subsequently tested for the presence of anticoagulant rodenticide residues using liquid chromatography-tandem mass spectrometry to screen for presence of anticoagulant rodenticides and high-performance liquid chromatography to quantify positive samples. When predation was determined to be the cause of death, all lesions attributed to predation were
described in detail. To distinguish between ante and post-mortem wounds (i.e. between predation and scavenging), we noted whether the lesions had associated hemorrhage and edema. In 14 cases, too few remains were present to identify hemorrhaging at wound sites, so only molecular analyses were conducted in these cases. Age-class at time of death was estimated as adult (≥24 months), subadult (12-23 months), and juvenile (<12 months) based either on tooth wear or cementum annuli counts.

Molecular Analyses—Forensic samples were processed and analyzed for predator (either felid or canid) DNA according to methods of Wengert et al. (2013). Because multiple polymerase chain reaction (PCR) products were occasionally obtained when the products were visualized on an agarose gel, we gel-excised the appropriately sized fragment (200–300bp for felids and 400 for canids) and extracted DNA using Qiagen Qiaquick Gel Extraction kit according to the manufacturer’s instructions. The PCR products were sequenced, then aligned using RidomTraceEdit (Ridom GmbH, Würzburg, Germany). Sequences were cross-referenced on GenBank using Basic Local Alignment Search Tool (BLAST) to match them to the most closely aligned sequence to identify species of predator DNA.

Population Growth Rates

Background.—All wildlife will respond to changes in habitat, food availability, and recent weather conditions by fluctuations in animal numbers. Fishers are no exception to this general pattern (Jensen et al. 2012). As a rare species, however, variation in population size or abundance of fishers is of conservation concern for maintaining a minimum viable number of animals on the landscape (Spencer et al. 2011, Reed and McCoy 2014).

Information on the growth trajectory for fishers in the SNAMP Fisher study area is unknown, but there are several competing hypotheses regarding population status in the broader region encompassing the SNAMP Fisher study area. Research conducted between 2002 and 2012 suggested no evidence for population increase (Zielinski et al. 2013), which may indicate that despite protection from fur trapping and development of policies designed to better sustain sensitive birds and mammals (North et al. 2009), fishers in the southern Sierra Nevada may not be in numeric or spatial recovery. Agreement with the “no increase” hypothesis, a body of research suggests that fishers in the Sierra Nevada experienced a range retraction of 30-50% over the last 75-100 years (Zielinski et al. 2005, 2013, Spencer et al. 2014). An alternative hypothesis is that fishers were very uncommon in our study area prior to 1990, and the current population resulted from a northward expansion from south of the Kings River (Fisher Habitat Core Area 3; Fig. 2; Tucker et al. 2014), equivalent to an approx. 30% increase in distribution in the overall southern Sierra Nevada region based on analyses of fisher habitat
by Spencer et al. (2014) (Fig. 2). This alternative view is based primarily on genetic evidence of population subdivision (Tucker et al. 2012, 2014), and potentially supported by increasing fisher detections from track plate and camera trap monitoring after the mid-1990s (Zielinski et al. 1995, 2005, 2013). Research on the ecology and habitat use of fishers in the southern Sierra Nevada has been the focus of research since the mid-1990s, and although much insight on fisher ecology has been gained with regards to home range size, population density, and habitat use for resting and other activities (Jordan et al. 2011, Thompson et al. 2012, Purcell et al. 2009), no prior study has reported the growth rate of any fisher population in this area.

**Population growth rates and Leslie-matrix modeling.**— Intensive research as part of the SNAMP Fisher study has generated information on all key vital rates needed to evaluate the population growth rate (\(λ\)) in the area, critical for understanding whether the population has the potential to persist, or if it is in decline. We developed an age-structured matrix model to estimate a deterministic population growth rate (\(λ\)) for the SNAMP Fisher study population using observed data on denning, fecundity, and survival. We defined 2 “adult” age classes (24 months, ≥36 months) for developing and including estimates of fertility in the matrix model for the population. Fertilities (\(F_i\)) were calculated for adult-age female fishers as:

\[
F_i = b(i)P_i \quad \text{Equation 1}
\]

where fecundity, \(b(i)\), was the mean number of female kits weaned per reproductive female (sex ratio at birth assumed = 0.5), and \(P_i\) was the age-specific survival rate (Gotelli 2001). Age-specific survival rates were estimated for radiocollared juvenile (6-11 months), subadult (12-23 months) and adult-age (≥24 months) female fishers in the study area using monthly encounter histories in Kaplan-Meier staggered entry model analyses (KM survival). Survival estimates were produced for combined data on numbers of radiocollared fishers in each age class during the six year study period (All-year), as well as for a series of five 2-year periods. Data on numbers of fishers in each age class for each pair of years were combined for KM survival estimates, starting with population years 2008-09 and 2009-10, and ending with population years 2012-13 and 2013-14.

Data from radiocollared animals from the study area indicated that female fishers commonly die by 6-8 years of age. We therefore included 8 age classes in our Leslie Matrix (\(A\)) formulation, where the numbers of fishers in each age class \(n_i\) to \(n_8\) at time \(t+1 = A X n\) vector at \(t_0\) according to equation 2:
Estimates for fertility were from data on weaning rates, fecundity, and survival for known-age juvenile (F1; 1 month old), subadult (F2; 12 months), young adult (F3; 24 months), and adult fishers (F4,5,6,7,8 ≥36 months).

Numbers of fishers in age classes n2, n3, and n4 for the \( \mathbf{n} \) vector at time \( t_0 \) were based on the number of radiocollared female fishers present at the start of population year 3 on April 1, 2009 (\( n_2 = 5 \), \( n_3= 6 \), \( n_4 = 5 \), \( n_5 = 5 \)), whereas \( n_1 \) was the number of juvenile females in the radiocollared population on Dec 31, 2009 (4 animals). We multiplied the Leslie matrix by the new vector of abundances for \( \mathbf{N}_{t+1} \) for 20 successive years, and summed the number of individuals in each age class each year to obtain a total \( \mathbf{N} \), and the population growth rate (\( \lambda \)) for year \( t+1 \) was calculated as \( \frac{\mathbf{N}_{t+1}}{\mathbf{N}_t} \). After several years a stable age distribution was achieved and \( \lambda \) converged to a constant value, which was the estimate of \( \lambda \) for the set of demographic parameters evaluated. We calculated a lower and upper range for \( \lambda \) based on the 95% lower and 95% upper C.I.s for age-specific survival and age-specific fertility. Finally, due to uncertainty in estimates for several demographic parameters related to methodology [small body size prevents radiotelemetry-based monitoring of survival for juveniles until 6 months of age (Facka et al. 2013); teat measures used to identify weaning for a small subset of adult females were less than 100% accurate in assigning reproductive status (Matthews et al. 2013a)], We evaluated the sensitivity of the matrix model to 20% reductions in rates of survival and fertility for each age class. Fertility is linked to age-specific survival according to Equation 1, and changes to fertility associated with reductions in survival were carried into the model when evaluating sensitivities.
Population Size and Density

Background

Information on population size and demographic parameters are fundamental for managing wildlife populations, especially when declines in abundance or range size have occurred and the species is the focus of conservation management. As previously noted, the overall southern Sierra Nevada fisher population is small (<350 adult fishers; Spencer et al. 2014), but appears to be stable over about the past decade (Zielinski et al. 2013). Research focused on the ecology and habitat use of fishers in the southern Sierra Nevada has been ongoing since the mid-1990s (Jordan 2007, Mazzoni 2002, Truex et al. 1998), but primarily for the area encompassed by the Kings River Fisher Project in the High Sierra District, Sierra National Forest (Fisher Habitat Core area 4; Fig. 2). For that area, Jordan et al. (2011) used a capture-mark-resight/recapture design (CMR) to estimate a density of 0.063-0.109 fishers/km² in 2002-2004, whereas Thompson et al. (2012) used scat detector dogs and genetic detections in a spatially explicit CMR framework to estimate a fisher density of 0.065-0.28 fishers/km². Information on density is needed for other areas of the southern Sierra Nevada as well, because as the area of suitable habitat available to fishers in the southern Sierra Nevada is refined by improved modeling (Fig. 2; Spencer et al. 2014), density values can be used to develop more accurate estimates of fisher abundance for conservation planning. Information from the SNAMP Fisher study has aided the process by providing current data on population size and density for the Fisher Core Habitat area 5 (Table 2). Here, we estimated fisher population size and density in the middle four years of the study using mark-resight techniques (McClintock and White 2009) from camera trap surveys and live trapping.

General Methods for Camera Surveys and Camera Traps

Motion sensing camera traps (Silent Image Professional, Rapidfire PC85; RECONYX Inc., Holmen, WI) were systematically deployed near the center of 1–km² grids in the study area beginning at the start of each of five “fall-winter” camera survey years (October 15-October 14 the following year). Placement of camera traps within 1-km² grid cells was determined based on the presence of habitat elements important for fishers (e.g., presence of mature or large diameter trees, moderate to steep slopes, canopy cover ≥60%, proximity to permanent streams; Purcell et al. 2009, Zielinski et al. 2004). Cameras were focused on bait trees upon which we attached baits and applied scent lures as attractants. Baits were small pieces of venison (140-250 grams) in a dark colored sock (reduced consumption by insects), and 8-10 hard-shell pecans strung onto a wire (initial purpose was to index
squirrel abundance, but were also consumed by fishers). Scent lures were Hawbaker’s Fisher Scent Lure (Fort Loudon, PA) dabbed on the bait sock, Caven’s “Gusto” scent lure (Minnesota Trapline Products, Pennock, MN) applied near the base of the bait trees and on several nearby trees, and ~4 grams of peanut butter smeared on the nut ring (Popescu et al. 2014). Camera survey stations were typically visited (checked) every 8-10 days over 32-40 days to refresh scent lures and bait, and to maintain camera units, but the protocol varied depending on whether the survey station was within the Key Watershed part of the study area, or outside that area. Survey cameras within the Key Watersheds were left in place a minimum of 32 days (four 8-10 day checks), whereas cameras outside this area were deployed for a minimum two 8-10 day checks but removed on check two or three if fishers had been detected. We removed survey cameras after four checks unless the unit had been disturbed (most frequently by black bears, *Ursus americanus*) to where the bait tree was out of view or if the unit had been inoperative due to expended batteries or malfunction for more than five days during a check period. In those cases the survey was extended by one or more 8-10 day periods to assure adequate survey effort (Slauson et al. 2009).

*Camera surveys, live trapping, and radiocollar data.*—Camera surveys were done during all months of each camera survey year, but the time frame of interest for this part of the study was October 15 to March 15, related to assumptions for mark-resight analyses of a closed population scenario. There are 145 1-km² grid cells within or overlapping the Key Watersheds boundary; 128 of them are at least 50% USDA Forest Service ownership, and were surveyed in all four survey years. A total 319 1-km² grid cells external to the Key Watersheds and within the study area boundary (Fig. 4) were surveyed in at least one fall-winter camera survey year, and 221 (69%) of those were surveyed in two or more years.

Full details on live-trapping to radiocollar and mark individual fishers was provided above. However, for the purposes of mark-resight analyses, data on captures and recaptures of known fishers were included in the mark-resight dataset. Also, fishers sometimes shed their radiocollars, or collars separated at the breakaways as designed. Dropped radiocollars were retrieved from the field, and the locations of shed radiocollars were included in the resight dataset.

*Monitoring and home ranges.*—Radio collared fishers were monitored for activity status and relocated 4-6 days/week throughout the year by fixed-wing airplane. Standard methods were used to obtain locations from the airplane as previously detailed, and mean error associated with aerial telemetry locations was estimated at 339 m.
Location records were used to develop home range models for individual fishers using the fixed kernel density method in Home Range Tools for ArcGIS 9.3 (Rodgers et al. 2007). Ninety-five percent fixed kernel home ranges were produced for individual animals for four fall-winter (October 15 to March 16) periods from 2008 to 2012 when ≥ 25 locations were available for an individual fisher. Home range area estimates from fixed kernel utilization distributions are sensitive to the choice of bandwidth as a smoothing parameter (Gitzen and Millspaugh 2003). We used the Ad Hoc method to identify the most appropriate reference bandwidth for smoothing fisher home ranges and minimizing formation of multiple polygons (Kie et al. 2010, Kie 2013).

**Resighting and Mark-resight Analyses.**—Radiocollared fishers detected by cameras were identified by the antennal pattern of bands of infrared reflective tape (Popescu et al. 2014). Detections of known fishers were counted once per camera station per calendar day. We were not able to unambiguously identify all radiocollared fishers detected at camera traps due to occasional loss of bands and breakage of antennas; these detections were scored as collared unknown. Non-collared animals were counted as unmarked seen.

We considered the population as approximating closure during Oct 15 to Mar 16 because (1) most mortalities in the study site occurred between mid-March and September, (2) natal dispersal by juvenile-age fishers in the population was focused during March to August, and (3) fisher reproduction in California begins the third week in March (Matthews et al. 2013a, this study). Data on individual fisher resightings at camera stations or live traps were scored based on presence within 1-km² grid cells. Individual animal detection histories were developed identifying whether fishers were available for resighting based on the presence of survey cameras or live traps within the boundaries of their 95% fixed kernel fall-winter home ranges. Data were also compiled on the numbers of survey cameras and live traps deployed, survey camera nights, and live trap nights for the fall-winter resight period.

The resighting data were analyzed using robust design mark-resight, log-normal Poisson models (McClintock and White 2009). The mark-resight robust design is analogous to the mark-recapture robust design of Kendall et al. (1995) and Kendall et al. (1997), in that it allows for individual covariates in modeling resighting.
probabilities, and an open population between primary sampling occasions. Along with data on marked animals, mark-resight models incorporate sightings of unmarked animals, while the robust design allows for estimating abundance \( (N) \), apparent survival between primary intervals \( (\phi) \), mean \( (\alpha) \) and overall resighting probabilities \( (\lambda) \), random individual heterogeneity \( (\sigma^2) \), and transition probabilities between observable and unobservable states \( (\gamma'' \text{ and } \gamma') \) (McClintock and White 2009). The parameter of interest, abundance \( (N) \), is a derived parameter, as Poisson log-normal models estimate the number of unmarked individuals in the population, \( U \) (McClintock and White 2009).

The Poisson log-normal mark-resight model takes the following form:

\[
[\alpha(.) \sigma(.) U(.) \phi(.) \gamma''(.) \gamma'(.)]
\]

in which \( \phi \) and \( \gamma'' \) (and \( \gamma' \)) were modeled using a \( \sin \) link, while \( \alpha \), \( \sigma \), and \( U \) were modeled using a \( \log \) link.

The model assumptions are: (1) geographic closure, (2) population closure within primary intervals, (3) no loss of marks, (4) no error in identifying marked and unmarked animals, (5) equal resighting probability for both marked and unmarked individuals, and (6) sampling is with replacement within secondary periods (McClintock and White 2009). We used camera survey years as the primary sampling intervals and the number of resights and live trap recaptures for marked fishers within each primary occasion as the resighting histories (Appendix I). Along with capture histories, robust-design Poisson log-normal models require three other quantities: (1) marked superpopulation, the number of marked individuals known to be in the population during primary interval \( j \), (2) number of times marked individuals were sighted, but individual marks could not be identified, and (3) total unmarked individual sightings during primary interval \( j \).

Because camera and live trapping was unbalanced across the study region among years, we added a grouping variable for subregion, with three subregions defined by the spatial segregation of camera trap efforts (Fig. 6) Each fisher was assigned to a particular mark-resight subregion based on the position of its 60% fall-winter home range isopleth. In addition, we included \( \text{area} \) and \( \text{time} \) (primary sampling interval) covariates, \( \text{cams} \) (camera effort for each subregion during each primary sampling interval in hours), and \( \text{live} \) (number of days live trapping was conducted) to account for variation in resighting probabilities, individual covariates \( \text{weight} \) and \( \text{sex} \) to account for individual and gender-based resighting probabilities. In the model parameterization, state transition probabilities remained constant \( [\gamma'(. \text{ and } \gamma''(.)] \), apparent survival was modeled as function of region \( [\phi(\text{area})] \), and different combinations (additive and interactions) of the individual and time and region-based
covariates were allowed.

We considered 19 candidate models and used AICc [Akaike Information Criterion adjusted for small sample size; (Burnham and Anderson 2002)] to rank models. We used model averaging for the top ranked models with a cumulative Akaike weight >0.95 to compute parameters and unconditional variances. The Area grouping parameter allowed for estimating population size and density for each subregion separately. We conducted analyses in program RMark v2.1.7 (Laake 2013) for R 3.0.2 (R_Core_Team 2013), which is an interface for program MARK (White and Burnham 1999). Lastly, the subregion and year-specific abundances were converted to densities by dividing population estimates by the area sampled by cameras and traps for each subregion and year. Areas sampled were estimated from subregion- and year-specific polygons created in ArcGIS 10.2 that encompassed the centroids of all 1-km² grid cells with a survey camera or a live trap with a fisher capture during October 15 to March 16. We then plotted the fall-winter home ranges with the sampling polygons, and, based on visual assessment of spatial intersection of the 95% home range isopleths, applied a 1300 m buffer for each polygon. The width of the buffer for the polygons was the radius of the mean 95% fixed kernel fall-
winter home range for subadult and adult female fishers in the population (mean = 20.8 km² ± SE 0.89, n = 70; Jordan 2007), which encompassed most areas used by radiocollared fishers resident in each subregion and excluded areas below or above the typical elevation range of fisher camera detections in the study area.

**Dispersal Movements**

**Background**

By simply moving from one habitat patch to another, dispersal of individuals has consequences not only for fitness, but also for population dynamics, population genetics, and species distribution at the landscape scale (Chepko-Sade and Halpin 1987, Lambin 1994, Clobert et al. 2001). For these reasons, processes that foment dispersal behavior have been the focus of research interest in relation to inbreeding avoidance, intraspecific competition for mates and resources (Estes-Zumpf and Rachlow 2009, Wolff et al. 1988), and costs and benefits of dispersal, particularly in relation to gender (Pusey 1987).

Natal dispersal, permanent movement from the natal area to the location where individuals reproduce or would have reproduced depending on survival (Howard 1960), is the most common type of dispersal. Gender bias in which one sex, typically males, disperses more frequently or farther than the other, has been documented in many mammals (Greenwood 1980; Pusey 1987, Sweitzer and Berger 1998). Proximate mechanisms triggering natal dispersal and potentially influencing dispersal distance include population density (Gaines and McClanahan 1980), habitat quality (Lidicker 1975), and body condition (Dufty and Belthoff 2001, Nunes and Holekamp 1996). Information on dispersal provides insights on how far, and over what sorts of terrain, individuals may move and therefore how populations may be demographically and genetically interconnected or isolated. Barriers or impediments to dispersal reduce gene flow and may prevent populations from colonizing or recolonizing suitable habitat areas.

Dispersal behavior by fishers is of high management interest in California where the species currently occupies less than half of its historical range as described in the early 1900s (Grinnell et al. 1937). In the southern Sierra Nevada conservation planning area, fishers occupy approx. 4,400 km² of mid-elevation, mixed-coniferous forest between the Merced River in Yosemite National Park in the north to the Greenhorn Mountains in the Sequoia National Forest in the south (Fig. 2, Table 2). The southern Sierra Nevada population does not appear to be expanding geographically (Zielinski et al. 2013), despite changes in management promoting redevelopment of suitable fisher habitat in the Sierra
Nevada (North et al. 2009). Dispersal movements by fishers are potentially inhibited by exposure to multiple restrictive habitat and landscape features (Spencer et al. 2014, Tucker 2013). Moreover, Matthews et al. (2013a) suggested that because of their relatively limited vagility, conservation-directed management to promote fisher recovery in formerly occupied portions of their range in California may require translocations, unless population growth rates significantly exceed 1.0.

We used information on juvenile home ranges, likely maternal home ranges (determined by genetic analyses), and adult home ranges to evaluate patterns in natal dispersal for fishers in the SNAMP Fisher study area. We hypothesized that (1) young male fishers would disperse at a higher rate than females, (2) dispersal distances for males would be longer than for females, and (3) long distance movements would be more frequent for males compared to females. Because of their large home ranges, dispersal movements by fishers may be mediated by restrictive landscape features (Carroway et al. 2011). Therefore, in addition to estimating Euclidean distance between juvenile or maternal home ranges and adult home ranges, we also used a least-cost corridor analyses with an expert opinion-based cost surface to estimate both short and longer distance movement paths associated with natal dispersal.

Assessing dispersal using home range models

Location records were used to develop home range models for individual fishers using the fixed-kernel density method in Home Range Tools for ArcGIS 9.3 (Rodgers et al. 2007; ESRI, Redlands, CA). We developed 95% fixed-kernel home range models for juvenile, subadult, and adult-age fishers when ≥ 25 locations were available for the pre-dispersal or post-dispersal period of interest. Approximate center positions (centroids) were estimated for each home range using the XTools extension in ArcGIS (Data East LLC, Novolsibirsk, Russia). Because both area estimates and shapes of fixed kernel home ranges are sensitive to the choice of bandwidth as a smoothing parameter (Gitzen and Millsapgh 2003), we used the Ad Hoc method to identify the most appropriate reference bandwidth for smoothing fisher home ranges and minimizing formation of multiple polygons (Kie 2013). Finally, in some cases radiocollars were shed by juveniles within a few weeks of initial capture, before ≥25 locations records had been acquired. In these cases we used centroids from 100% Minimum Convex Polygons for natal area centroids (Aubry and Raley 2006).

Dispersal distance by Euclidean geometry

Minimum distances moved between natal or maternal home ranges, and subadult or adult home
ranges were estimated as the Euclidean distance between each pair of centroids. For juvenile fishers without maternity assignments, we used fall and winter location records to identify a centroid for natal areas, but excluded locations that were associated with initiation of dispersal. Fall and winter location records for juvenile fishers were visually screened in ArcGIS to identify calendar dates associated with initiation of the exploratory, or transitional period of the dispersal process (Vangen et al. 2001). Location datasets used to develop home ranges for juvenile fishers (natal area home ranges) were truncated by date to exclude transitional movements. Transitional movements were not apparent for all juvenile fishers, however, and in these cases we used the pool of location records from capture to approx. March 31 for the natal area home range.

Microsatellite genetic analyses for identifying maternity

We used microsatellite genotypes to assign maternity for juvenile and subadult fishers, which allowed for estimating natal dispersal from the centroids of denning season home ranges for their mothers. Whole genomic DNA was extracted from fisher tissues and hair using the QIAGEN Dneasy Tissue Kit (Qiagen, Valencia, CA, USA) according to manufacturer's instructions. We analyzed 111 samples at the following thirteen microsatellite loci: Mal, MP059, MP144, MP175, MP197, MP200, MP247, Ggu101, Ggu216, Lut604, Lut733, Mer022 and Mvis002 (Davis and Strobeck 1998; Flemming et al. 1999, Dallas and Piertney 1998; Jordan et al. 2007). These loci were previously found to be variable in fishers in the Southern Sierra (Jordan et al. 2007; Tucker et al. 2014).

Maternity of kits was evaluated using two approaches; the first was by evaluating allele sharing. Fishers in the southern Sierra Nevada were previously known to be genetically limited (Wisely et al. 2004, Knaus et al. 2011), and prior to step 1 we used insight from field associations (capture positions, home range patterns, denning season data to identify small subsets of 3-6 adult females considered possible mothers for each juvenile/subadult. These subsets of possible mothers were further narrowed to a smaller “candidate set” by excluding those that did not share alleles with the juveniles. We applied the maximum likelihood approach in program CERVUS v3.03 (www.fieldgenetics.com) to evaluate the candidate set of females for maternity assignment. CERVUS is a Windows-based software package for inferring parentage in natural populations wherein laboratory typing error is considered along with data on population allelic frequencies, the number of candidate mothers, and the proportion of potential mothers sampled in Monte Carlo simulations, which produce confidence levels for the candidate set of putative parents (Slate et al. 2000). The confidence measure of CERVUS is based on delta, which is the difference between the likelihood ratio for the most likely
candidate and the second most likely candidate (Marshall et al. 1998). We assigned maternal-offspring pairs based on likelihood ratio LOD scores (natural log of the likelihood ratio) using both strict (99%), and relaxed (95%) confidence. In the last step we considered the maternity assignments with field data to verify, or select the next most likely female from the LOD scores based on the known biological status of putative mothers (reproductive or non-reproductive, age as juvenile, subadult or adult in the season juveniles were born). In several cases, the overall analysis was unable to link juveniles to a known, radiocollared female in the population. Developers of CERVUS previously determined that the analytical procedure correctly assigned parentage for ~92% of known fathers in red deer (Cervus elaphus) (Slate et al. 2000). In our study, we evaluated the performance of CERVUS to correctly identify mothers using five known mother-offspring pairs.

Dispersal distances for juvenile or subadult-age fishers with maternity assignments were estimated as the Euclidean distance between the centroid of the denning season home range of the mother, and the centroid for the subadult or adult home range where the juvenile settled. In some cases the mother had not been monitored during the denning season when a juvenile was produced. In these cases we used the centroid for the female’s “annual” home range. Annual home ranges were calculated when we had at least 75 location records, with a minimum of five locations in at least three of the four seasons of the year. Seasons were spring (Mar 21 to Jun 20), summer (Jun 21 to Sep 20), fall (Sep 21 to Dec 20), and winter (Dec 21 to Mar 20).

Least-cost paths for natal dispersal

Dispersal is most often reported as the Euclidean, or straight-line distance between the natal area and the subadult or adult home range (Matthews et al. 2013). Fishers in the southern Sierra Nevada inhabit mountainous areas within a limited elevation range and with a mix of forested areas with mild topography, and ridges and deep river canyons with extreme topographic relief. In these types of landscape and habitat conditions opportunities for straight-line movement traversing multiple kilometers will be constrained.

Least-cost modeling is an approach for assessing potential animal routes across the landscape based on the assumed cost of movement between locations or termini (Beir et al. 2008). Least-cost models have previously been used to predict dispersal paths for mammals from empirical data (Driezen et al. 2007), and we took a similar approach in this study. Connectivity analysis was performed between centroids of natal and established juvenile home ranges for 24 female and 20 male fishers with Linkage Mapper (McRae and Kavanagh 2011). Linkage Mapper uses a resistance to movement.
A cost surface layer was developed that assigned a resistance score (inverse permeability value) representing the cost to fishers of moving through each land cover type, including potential risk and averse responses to roads and steep topography in river canyons (Fig. 7).

Expert opinion resistance scores were modified from those developed previously for Sierra Nevada fisher least-cost corridor models (Spencer and Rustigian-Romsos 2012) by (1) simplifying the land cover divisions, (2) expanding the overall cost range, and (3) incorporating recent published and unpublished data on fisher ecology summarized in a conservation assessment developed for fishers in the southern Sierra Nevada by a group of 13 research scientists (Spencer et al. 2014). We used the Polynomial Approximation with Exponential Kernel (PAEK) algorithm in ArcMap 9.3.1 (ESRI 2009) to smooth the movement paths for purpose of display. Length of least cost paths between juvenile or maternal home range centers and subadult or adult home range centers were calculated in ArcGIS 10.2. Basic metrics on least cost paths (means, range, standard error of the mean) were produced and summarized for comparison with mean dispersal distances from Euclidean geometry.

Analysis

Mean dispersal distances were compared between female and male fishers using two-sample *t*-tests. We also assessed potential gender differences in dispersal behavior using Pearson $\chi^2$ or log-linear model analyses. We used dispersal distances to classify each fisher as being either philopatric (dispersal distance ≤ 5.4 km) or a disperser (dispersal distance > 5.4 km), where 5.4 km was the diameter of average 95% fixed kernel home range of adult females fishers in the study population (22.93 km², n = 56; Table 31). We also assessed the overall pattern in dispersal behavior by assessing the proportion of each gender that was very philopatric (dispersal distance < 2.7 km; one-half the diameter of the mean 95% fixed-kernel home range for adult female fishers), short distance philopatric (2.7 km ≤ dispersal distance < 5.4 km), a mid-distance disperser (5.4 km ≤ dispersal distance < 10.8 km), or a long distance disperser (dispersal distance greater than 10.8 km; 2x the diameter of the average adult female home range).
Figure 7. Illustration of the Expert opinion cost surface used to develop Least Cost movement path for dispersing fishers. The map encompasses the portion of the SNAMP Fisher Study Area including the Key Watersheds and the area to the southeast including Chilkoot Lake and Mammoth Pool Reservoir. NOTE: Chilkoot Lake is at the northwestern edge of the Chiquito Ridge, a high elevation region including Little Shuteye Peak, Shuteye Peak, and Shuteye Pass; notice the narrowness of restrictive habitat at the Shuteye Pass topographic feature.

Home Range Dynamics Methods

Background

Among terrestrial vertebrates, mammalian carnivores have the largest home ranges for their body size of any organism. The fisher, like other mammalian carnivores, occupies a relatively large
amount of space for its body mass, with average annual home range areas of 38 km² for adult males and 15 km² for adult females across North America (Powell 1994). Studies of the two remnant populations in California have produced home range area estimates generally consistent with North American averages: 22 to 58 km² for adult males and 5 to 15 km² for adult females (Boroski et al. 2002, Zielinski et al. 2004). Zielinski et al. (2004) also reported intraspecific variation in home range size between adult females of the northern coastal and southern Sierra Nevada populations.

Intraspecific variation in home range size has been linked to ecological factors such as population density, prey availability, body mass, and latitude (Buskirk and McDonald 1989, Gompper and Gittleman 1991), and to methodological factors such as sampling interval and duration (Buskirk and McDonald 1989, Swihart and Slade 1985). Our review of the literature suggests that little attention has been paid to potential relationships between home range size and field techniques used to obtain animal locations. Further, the choice of an appropriate bandwidth, or smoothing parameter when creating utilization distributions is a critical step during kernel-based home range estimation in need of standardization (Kie et al. 2013).

We present and discuss home range dynamics for fisher in the Sierra National Forest, while also describing seasonal variation in home range movements for female and male fishers. We hypothesized that aerial telemetry would be more likely than ground telemetry to detect animals outside of their core use areas and during dispersal events and sallies, and would therefore produce larger home range estimates. Our primary objective was to compare our fisher home range sizes with those generated by other studies in the southern Sierra Nevada, where ecological factors would be held relatively constant. Additionally, we wished to characterize variation in space use between genders, among age classes, and across seasons for our study population.

Locations and analyses
Fisher locations from live-trap captures, dropped or shed radiocollars, carcasses, dens and rest trees, camera trap detections, a small number of GPS radiocollars (Telemetry Solutions, Livermore, CA) and position estimates from fixed-wing aerial radiotelemetry (Table 3) were used to delineate home ranges. Accuracy of locations obtained by homing to den tree or rest tree locations by ground telemetry, camera trap detections, capture positions, and carcass and dropped collar positions were generally ± 15 m from a handheld global positioning system device. We addressed and minimized autocorrelation by discarding locations in excess of two per animal per day, or less than 8 hours apart per individual. Location records were used to develop home range models for individual fishers using
the fixed kernel density method in Home Range Tools for ArcGIS 9.3 (Rodgers et al. 2007). Ninety-five percent fixed kernel home ranges were produced for individual animals for four seasons, and for “annual” population years. **Seasons were defined as follows: fall - September 21 to December 20; winter - December 21 to March 20; spring - March 21 to June 20; summer - June 21 to September 20.** Season-specific home range models were produced when ≥25 locations were available for a fisher. **Annual home range models were developed when we had location records in fall, winter, and at least one other season, and sample size was ≥75 for all annual home range models.** Kernels were smoothed using the minimum proportion of reference bandwidth that produced a contiguous home range polygon (Kie 2013). **Areas (km²) of home ranges** were calculated during kernel processing. We tested for differences in home range areas between males and females, stratified by age and season, using two sample t-tests (P<0.05). Potential differences in home range size among seasons was assessed using repeated measures analysis of variance (ANOVA) (P<0.05).

**Table 3.** Types of locations determined for fishers during the study period from December 2007 to December 2013. Data are for research in the Bass Lake District, Sierra National Forest, CA.

<table>
<thead>
<tr>
<th>Location type</th>
<th>No. of locations</th>
<th>UTM accuracy</th>
<th>Description of methods and details</th>
</tr>
</thead>
<tbody>
<tr>
<td>Aerial telemetry</td>
<td>31,367</td>
<td>± 339 m</td>
<td>Standard methods by fixed-wing airplane (Sweitzer 2013)</td>
</tr>
<tr>
<td>Camera trap detections</td>
<td>2,454</td>
<td>± 10 m</td>
<td>Position of camera trap; individuals fishers identified by infrared tape on radiocollar antennas (Popescu et al. 2014)</td>
</tr>
<tr>
<td>GPS radiocollar</td>
<td>633</td>
<td>± 15 m</td>
<td>Used on limited number of animals (N = 8) in 2009 and 2010</td>
</tr>
<tr>
<td>Den or rest tree</td>
<td>526</td>
<td>± 10 m</td>
<td>Homing to trees by ground radiotelemetry during spring denning seasons; did not identify rest trees in other seasons</td>
</tr>
<tr>
<td>Live trap capture</td>
<td>277</td>
<td>± 10 m</td>
<td>Trap positions for known ID captures; most live-trapping was in October to March</td>
</tr>
<tr>
<td>Shed radiocollars, fisher carcass</td>
<td>97</td>
<td>± 10 m</td>
<td>Homing to inactive signals by ground radiotelemetry</td>
</tr>
</tbody>
</table>

*a Accuracy determined as the mean Euclidean distance between aerial telemetry location and fixed position of test collars (N = 501) on the ground. Test locations also included locations to dropped radiocollars, carcass locations, and fishers in live traps (Technicians were "blind" to locations of test collars, or other test locations).

*b Used for limited duration and primarily on male fishers (596 locations for 6 different males; 37 locations for 2 females). SNAMP Fisher ceased using GPS collars due to poor reliability and bias in fix rates; fix rates were high when GPS collars were left in open areas with mild topography, and low when GPS collars were placed at locations with dense overhead canopy and steep topography.
SNAMP Fisher Management Indicators

Background

In 2008 there was great interest in new information developing from SNAMP Fisher that might be important for management and management planning. We therefore developed three Indicators for fisher management that would provide insight on the status of the study population of fishers in the Bass Lake District, Sierra National Forest. These management indicators were chosen based on information that could be summarized annually, and that linked to the likely responses of fishers to management and potential habitat change at the local (sub home range scale), home range, and population level (larger landscape scale relevant to District-level forest management; Table 4).

**Table 4.** Overview of potential negative effects of fuel reduction treatments and other forest management activities on the biology and natural history of fishers, organized according to three scales in the SNAMP Fisher study area, Bass Lake District, Sierra National Forest.

<table>
<thead>
<tr>
<th>Scale of effect, Description</th>
<th>Likely response</th>
<th>Data requirements</th>
<th>Management Indicator</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>LOCAL:</strong> SPLATS may cause habitat patches to become less suitable for current use; foraging, refuge/escape cover</td>
<td>Use of treated areas declines or ceases</td>
<td>Before/After and ongoing use of areas altered by management activities</td>
<td>Use of 1-km² grids within Key Watersheds (or in other areas), estimated annually using camera trap surveys</td>
</tr>
<tr>
<td><strong>HOME RANGE:</strong> SPLATS may reduce availability of key resources such as den sites, rest sites, availability of prey</td>
<td>Individual fishers cease use of treated areas</td>
<td>Monitor individual fishers, acquire locations, develop home range models, track dispersal movements</td>
<td>Estimate of the number of individual fishers using the Key Watershed focal study area during population years</td>
</tr>
<tr>
<td><strong>POPULATION/REGION:</strong> Multiple projects implemented every few years may degrade suitable habitat for fishers; population source areas become sink areas</td>
<td>Survival and reproduction decline; population size and density decline over time</td>
<td>Information on survival and reproduction of individual fishers in the overall study area (min 20 fishers monitored by radiotelemetry at all times)</td>
<td>Survival and reproduction of fishers in the overall study area. Estimate population growth rate, evaluate population viability in the Sierra National Forest</td>
</tr>
</tbody>
</table>
Management Indicator 1: Occupancy/Activity of fishers within Key Watersheds.

Beginning October 2007 we implemented regular camera trap surveys of all 1-km² grids that are partly or entirely encompassed by the boundary of the SNAMP Fisher Key Watersheds (Fig. 4). Several grids that were predominantly private lands (e.g., the Fish Camp area), or that were below the typical elevation range of fishers in the region (< 914 m) were not surveyed unless we had permission of access from the landowner. The annual resurvey of the Key Watersheds was a research priority in all years, and camera trap surveys were initiated at the start of each camera survey year (mid-October), continuing until most grids had been surveyed by late winter or early spring. High elevation areas (northeast portion of Key Watersheds) were generally surveyed first, due to more difficult access during mid and late winter. Deep snow conditions in most winters required use of snowmobiles or an all-terrain-vehicle modified with tracks to access high elevation grid centers. Results of the Key Watersheds camera surveys were summarized according to (1) fisher presence or absence, and (2) level of fisher activity based on the number of days fishers were detected in each grid.

Management Indicator 2: Number of resident female and male fishers using the Key Watersheds area.

Juvenile fishers exhibit exploratory movements, and sometimes dispersed away from their natal areas where we first captured and monitored them. Dispersal by juvenile fishers often extends into summer when they are 13-15 months old and considered subadults. We considered subadult fishers (12 to 23 months old) to be “settled” after natal dispersal in late August/September. Ninety-five percent fixed kernel home range models were developed from location records during September 1 to March 15 for all radiocollared fishers (Sept-March home range). Analyses were completed in ArcGIS 9.3.1 to estimate the proportion of each Sept-March home range for subadult and adult fishers that were included or “intersected” within the boundary of the Key Watersheds. Management Indicator 2 was calculated as the sum of the proportions of individual subadult and adult Sept-March home ranges within the Key Watersheds focal study area. Management Indicator 2 was calculated for female and male fishers for each of six September to March 15 periods beginning September 2008 and ending March 2013.

Management Indicator 3: Survival of adult-age female fishers in the SNAMP Fisher Study Area

Survival, and survival of adult females in particular, is an important demographic parameter necessary for understanding the population growth trajectory for most vertebrate wildlife species (Murdoch et al. 2010). All radiocollared fishers were monitored 4-6 days/week by fixed-wing aerial
telemetry to assess live/dead status. Information on survival status for radiocollared fishers was organized by month of each population year (Apr 1 to March 31), and analyzed using the Kaplan-Meier (KM) staggered entry method (Koen et al. 2007, Pollock et al. 1989, Price et al. 2010). KM models were used to produce estimates of annual survival and combined year survival for the study population. Annual survival can be moderately to highly variable, thereby suggesting a negative population growth trajectory that may not be appropriate for a long-lived species with a generation time of two or more years. We therefore combined monthly data on survival status for individual fishers for five 2-year periods (population years 2 and 3, population years 3 and 4, population years 4 and 5, population years 5 and 6, population years 6 and 7) beginning April 2008 and ending March 2014. We further extended the inference for this management indicator by estimating survival for juvenile and subadult females, and by compiling data on weaning reproductive rates and weaning litter sizes from data collected on fisher reproduction during April 2008 to June 2013. These data were used to estimate fertility rates, which, along with data on juvenile, subadult, and adult female survival, were used to estimate deterministic population growth rates using a four age class Leslie Matrix population model.

**Fisher response to fuel management**

**Occupancy modeling**

For the purpose of occupancy surveys, we deployed cameras in the 128 1-km² grids that were ≥50% public lands and within the 4 focal watersheds. We also deployed cameras in areas with recent histories of extractive or restorative fuel reduction, between 2002 and October 2008, or because forest management projects were planned to occur in the areas before December 2011. Most of these grids were repeat surveyed in 7 different camera years, as part of our initial plan of using a BACI framework for the occupancy analyses (Popescu et al., 2012). However because we were not aware of all planned or prior Forest Projects within the study area when the project was initiated, some of the multi-season grids were added to the group that were repeat surveyed several years after the first camera year (2007-08).

The distribution of fishers in the southern Sierra Nevada, CA is constrained by elevation, and closely associated with mixed-conifer forest habitats with relatively large trees, and high canopy cover (Davis et al., 2007). We therefore developed local, patch-specific biophysical covariates for use in analytical models of occupancy. We calculated the mean elevation \( (elev) \) for each surveyed grid, which was always included in occupancy analyses with its quadratic term \( (elev^2) \). These covariates
were standardized. Habitat covariates included the proportion forest (i.e., total tree) and hardwood cover (denMD) based on land-cover data derived from satellite imagery (CWRH CalVeg; USDA Forest Service 2012). We did not include covariates representing average tree size and slope because of their colinearity with forest cover and elevation.

There were a diversity of forest management activities that occurred on the Sierra NF from 2002 (5 years before the start of our study) until the last camera survey year starting in October 2013 (period 2002 to 2013). Most of the management activities we used for covariates were developed from the USDA Forest Service FACTs database. FACTs (Forest Service Activity Tracking System; http://www.fs.usda.gov/main/r5/landmanagement/gis), is a tracking system including a geospatial database of forest management activities that occur on national forest service lands in California and elsewhere (FACTs User Guide 2013). Polygon layers included in the FACTS database are associated with attributes detailing management activity codes, and dates for when activities were initiated and completed. There are known uncertainties in FACTS with regards spatial precision, area of treatment polygons, and lack of details on whether a treatment activity was completed for an entire polygon (Garner, 2013). We also know that some entries represent perimeters encompassing smaller subunits treated at the same time as well as some areas unaffected by the management activity (Zielinski et al., 2013). Nevertheless, FACTS data constitute the best available and consistent record of the annual management activities that occurred on national forest lands in our study area.

Two recent studies used FACTS information to assess how fishers respond to disturbances from Forest Projects elsewhere in the southern Sierra Nevada (Garner 2013; Zielinski et al., 2013). We considered FACTS activities that were previously used in those studies, but also reviewed full descriptions of each management activity included in the FACTs User Guide (2013) when identifying a subset of 24 that were considered as potentially influencing local scale habitat use by fishers related to how each altered forest habitat structure or if they represented significant ground-disturbing activities (Zielinski et al., 2013; FACTs User Guide 2013). For example, we included forms of harvest (e.g., code 4152 Group Selection Cut) and vegetation management (e.g., code 4220 Commercial Thinning, code 4580 Mastication/Mowing) that would have direct effects on the basis of their disturbance and alteration of forest structure (Zielinski et al., 2013). We excluded activities that did not meet this criterion, and several that rarely occurred, or that silviculturist Dave Smith with the Sierra NF recommended against using (e.g., code 4290 Administrative Changes; code 4314 Pretreatment Exam for Reforestation; code 4530 Prune; code 4511 Tree Release and Weed; code 4552 Area Fertilizing; code 4980 Other Tree Improvement; code 4540 Control of Understory Vegetation).
There were 4 other activities or events that were not systematically tracked by the FACTS system: hazard tree removals (e.g. hazard tree logging), private timber harvests (THPs), and historical or recent wildfires. Hazard tree logging was the removal of medium and large trees (no DBH restriction) within 91 m of forest roads when they were considered likely to fall during storms, or if they were decadent or diseased (SNFP 2004). Information on hazard tree logging in the Bass Lake Ranger District was available for 2009, 2010, and 2011, and we were provided with GIS shapefiles identifying road segments along which hazard tree logging occurred. Private timber harvest occasionally occurred on large parcels of private land within or adjacent to the Sierra NF in Madera County and Mariposa County. Harvesting of timber on private lands in California requires preparation of Timber Harvest Plans (THPs) that are reviewed and approved by state agency Calfire, which was our source for geospatial data on private THP activities in Madera County and Mariposa County (ftp://ftp.fire.ca.gov/forest). Basic records on the estimated spatial extent of wildfires that occurred in the national forest portion of study area were maintained by the Sierra NF, and included polygon shapes and ignition dates of wildfires that occurred from 1911 to 2013. We also acquired geospatial data on natural ignition and management fires for Yosemite NP for 1930 to 2008, which was sufficient for our analyses because there were no camera surveys completed in southern Yosemite NP after May 2009. Attribute information included with the various geospatial data were used to assign activities and wildfires to individual camera survey years. For example, if a management activity was identified as completed before October 15, 2009, the disturbance was assigned to camera year 2008-09.

We used ArcGIS 10.2 (ESRI, Redlands, CA) to estimate the area of each 1-km² surveyed grid with hazard tree logging, private timber harvest, and wildfires, which were merged with the FACTS information for 2002 to 2013. After merging, we reviewed the entries, and removed polygons that were duplicated in several years (e.g. those with the same FACTS code with identical shapes and areas but with different years of completion). We also removed any duplicate wildfire records that were included in both the FACTS data and in the local Sierra NF wildfire database. We then used the detailed descriptions of each FACTS activity type to create 3 composite variables for use as covariates for occupancy analyses. Covariates for extractive fuel reduction (log.5) and restorative fuel reduction (hazfuel.5) included the cumulative areas of these activities in each grid in the 5 years immediately preceding each camera trap survey. For example, the hazfuel.5 covariate for any grids that were surveyed in camera year 2012-13, was calculated as the sum of the areas (m²) of all restorative fuel reduction activities that occurred in those grids during fiscal years 2007-08, 2008-09, 2009-10, 2010-11, and 2011-12, from which we calculated the proportion of the 1-km² grids disturbed by the
Because of the coordinated series of extractive and restorative fuel treatments associated with SPLATs, multiple different treatments could be applied on the same forest stand within a 5 year period (Zielinski et al., 2013). It was therefore possible that the cumulative area of a grid that was treated during a 5-year period could exceed 1-km². In the few cases where this occurred, the proportion of the grid treated was truncated at 1.0 (100%).

The third composite variable that was related to fisher presence in model analyses was for managed burning and wildfires within each 1-km² grid. When we reviewed the FACTS and Sierra NF and Yosemite NP databases, it was apparent that managed burning was uncommon in the study area during 2002 to 2013. Although managed burns were commonly planned in the Sierra NF portion of the study area as part of SPLAT-based fuel reduction, many managed burns were cancelled and not rescheduled because weather conditions were not suitable, or because burning was prohibited by the San Joaquin Valley Air District (D. Martin, personal communication). Also, a late summer managed burn in Yosemite NP in 2009 escaped containment and burned 7,425 ha (Big Meadow Fire), which discouraged other managed burns in the region for several years thereafter. We therefore combined information on managed burning and the longer time-series of wildfires in the study area into a single composite variable representing managed burn+wildfires within 50 years of a survey (burn.1.50).

We used multi-season occupancy models to evaluate the importance of forest management covariates to explain the persistence of fishers at occupied grids and colonization of unoccupied grids (Zielinski et al., 2013). We defined colonization (γ) as the probability that a grid unoccupied in year t would be occupied in year t +1, and modeled it as: logit(γ) = βγ0 + βγ1x1 + βγ2x2 +.... We defined persistence as 1- extinction where extinction (ϕ) was the probability that a grid occupied in year t would be unoccupied in year t +1, and modeled it as: logit(ϕ) = βϕ0 + βϕ1x1 + βϕ2x2 +.... The multi-season models also included a component for occupancy in the initial year a site was surveyed:

logit(ψ_initial) = βψ_initial0 + βψ_initial1x1 + βψ_initial2x2 +....

We created a detection history of whether a fisher was observed by a camera trap within each grid during each consecutive survey period after set-up or re-baiting for up to 5 8-10 day periods during a survey year. This was repeated for up to 6 consecutive years (e.g., 00101 00000 01110 00010 01101 00000) for every grid. If surveys did not occur during any of the 5 periods and 6 seasons at any of the grids these data were treated as missing data. Models were solved by maximum likelihood estimation (MLE) via R statistical software (Version 3.0.1, www.r-project.org) using the unmarked package and the colext function—(Fiske and Chandler 2011). We followed an information-theoretic approach for evaluating the relative importance of different candidate models, and for assessing the relative
importance of individual covariates [sum of AIC weights (AIC wi) for candidate models including each covariate; Burnham and Anderson 2002].

Covariates for potentially explaining detection probability included a categorical, first order Markov process reflecting whether a fisher was detected in the previous survey period in a season (auto.y; Hines et al., 2010; Slauson et al., 2012), the number of functional camera days in a survey period divided by 10 (camdays), denMD, and a categorical variable representing whether the survey was conducted in summer (summer) instead of in fall to spring.

Due to the smaller sample size of sites available for fitting multi-season models (n = 361), we only evaluated the role of forest management covariates (log.5, hazfuels.5, and burn.1.50) in explaining annual transitions in occupancy state (colonization and extinction). For the initial occupancy component of the multi-season models, we restricted potential explanatory covariates to denMD and elev + elev^2. For multi-model evaluations of multi-season models, we first fit models including all 8 combinations of the forest management covariates on the colonization component and an intercept-only extinction component. We considered any covariate with a relative importance value > 0.65 to be predictive and important for colonization. Next, we fit models including all 8 combinations of the forest management covariates on the extinction component multiplied by all combinations of colonization covariates identified as important. We deemed any covariate in the extinction models with a relative importance value > 0.65 as predictive for explaining local extinction. Finally, we computed model-averaged parameter estimates for the colonization and extinction covariates identified as important. Model averaging was based on only those models summing to the top 0.95 of model weights.

Integration
Development of vegetation map

We refer the reader to Appendix C for more complete details because we used the same mapping procedure at Sugar Pine as we did at Last Chance. In summary, we developed a pre-treatment vegetation map using a combination of LiDAR, high-resolution digital color-infrared (CIR) aerial imagery, and an intensive network of field plots. First, we used LiDAR and CIR data to create an initial polygon-based map where the polygons represented areas of homogeneous vegetation in terms of species, vertical structure, basal area, and canopy cover. We collected the LiDAR and CIR data before the SPLAT implementation, and we sampled vegetation at the field plots before and after treatment. We then used the field-plot data to impute detailed attributes (e.g., tree lists and fuels
models) for each polygon. Thus, we derived two different maps (with and without treatment), which we used in fire and forest-growth modeling.

Modeling fire and forest dynamics

We again refer the reader to Appendix C for more complete details of the fire and forest-growth simulations because we followed the same general procedure at Sugar Pine as we did at Last Chance. We used FARSITE (Finney 1998) to simulate a likely wildfire scenario based on the weather conditions during the 2014 French Fire, which burned 13,837 ac (5,602 ha) 12.5 mi (20 km) southeast of the study area. We obtained weather information from the Batterson Remote Automatic Weather Station, limited to the active burning period of the French Fire (August-September 2014), which served as the basis of our fire modeling. Moisture content for live and dead woody fuels and live herbaceous fuels used in the model were equivalent to 97th percentile weather conditions. Our ignition location was established using fire-origin point data supplied by the Bass Lake Ranger District of the Sierra National Forest. Based on the mapped data, we identified an area with the highest ignition frequency, which was located on the west ridge of the Cedar Valley watershed (see Figure 1). The simulation duration was set to allow the fire perimeter to expand through the entire study area.

For all four scenarios (treated/fire, untreated/fire, treated/no fire, untreated/no fire), we then simulated 30 years of forest growth on the study area in 10-year time steps using the Forest Vegetation Simulator (FVS; Dixon 2002) with the Fire and Fuels Extension (FFE; Reinhardt and Crookston 2003). The simulations were performed using the integrated platform ArcFuels (Ager et al. 2006, Vaillant et al. 2011), which runs FVS-FFE to produce the forest structure inputs needed for FARSITE.

Assessing the effects of fire and SPLATs on fisher habitat

We identified canopy cover and large trees as the most important elements of forest structure for fisher habitat because fisher den and resting locations in the southern Sierra Nevada were associated with high canopy cover and large trees (Zielinski et al. 2004, Purcell et al. 2009, Thompson et al. 2011). We defined fisher habitat as forest stands where the canopy cover was ≥60% and the density of large trees (≥24 in [61.0 cm] dbh ) was ≥15.4 trees/ac (38 trees/ha).

We defined the canopy cover threshold for fisher habitat as ≥60% because 95% fixed kernel home ranges for 16 adult female fishers in the Kings River Project area in the Sierra National Forest
averaged 63% (Thompson et al. 2011). Furthermore, fisher resting habitat sites are characterized by high canopy cover that is typically >60% (Purcell et al. 2009, Thompson et al. 2011), and the California Wildlife Habitat Relationships database uses a 60% canopy cover threshold as one of the criteria in its definition of high-quality fisher reproductive habitat (California Department of Fish and Game 2008).

We defined a large tree as ≥24 in (61.0 cm) diameter at breast height (dbh) because resting trees at the lower end of the size distribution (i.e., mean minus the standard error) in two different studies were of a similar size (Zielinski et al. 2004, Purcell et al. 2009). Thus, any tree ≥24 in dbh was potentially suitable as a fisher resting site. Next, we determined the threshold density of large trees (i.e., 24 in dbh) by examining stand-level tree lists surrounding den locations of 28 female fishers in the Kings River study area from 2008-2013 (Rebecca Green, unpublished data). When there were multiple dens per female, we randomly chose a single den for that individual. Data for natal dens were used preferentially; natal dens were where the young were born. We used data at maternal den locations for 7 females for which natal den locations were not available; fisher young were moved to maternal dens when conditions were no longer suitable at the natal dens. We defined the threshold density for large trees as ≥15.4 trees/ac (38 trees/ha) because this was the median density of large trees surrounding the 28 den locations.

Results

Basic Fisher Population

A total of 110 individual fishers were captured in live traps as part of the SNAMP Fisher Project from Dec 2007 to Dec 2013 (62 females, 48 males). In the first 3.5 months of trapping in population year 1 we captured 3.6 noncollared (“new”) fishers/100 trap nights, and 6.8 total fishers/100 traps nights. During population years 2008-09 to 2011-12 a mean of 0.94 previously unknown individual fishers were captured per 100 trap nights, and there were an average 2.43 total captures per 100 trap nights. Data on traps nights were not available for 2012-13 or for March to December 2013, when a total of 9 new fishers and 19 total captures occurred (Table 8).

No fishers died during capture and handling in the study. However, one adult female fisher captured in October 2009 did not fully recover. The female fisher was held in the trap cubby overnight for additional time to recover, but died the next morning while in transit to the Fresno Chaffee Zoo for treatment by a wildlife zoo veterinarian. A necropsy completed for the fisher identified her cause of
death as septicemia from a previously fractured jaw, which led to emaciation and starvation.

An overarching goal of the study was to monitor a minimum of 20 radiocollared fishers at all times, which was considered a requirement for producing reliable estimates of survival and reproduction for the population. The study achieved that milestone in mid July 2008, about six months after live trapping was initiated in late December 2007 (Fig. 10, Table 9). There were brief periods in several years when the radiocollared population declined below 20 individuals (Fig. 10). The annual oscillation in numbers of radiocollared fishers was related to the combination of dropped or shed radiocollars (breakaway units built into radiocollars parted as designed), and mortality which was focused during spring and summer in all years of the study. After the end of our annual pause in live trapping during the spring denning season, the number of radiocollared fishers gradually or rapidly increased when trapping resumed, and as young-of-the-year juvenile fishers were recruiting into the study population in the fall and winter (Fig. 10). With the exception to the first and last year of the study, we were able to monitor survival for at least 40 different fishers in each population year. Notably, in 2011-12 we were monitoring more than 40 individual fishers for several successive months (Fig. 10, Table 9).

### Table 8. Summary data on numbers of trap nights, new fishers captured, and recaptures for the SNAMP Fisher Project from December 2007 to December 2013. Population years start April 1 and end March 31.

<table>
<thead>
<tr>
<th>Population Year</th>
<th>Trap nightsa</th>
<th>New individual sb</th>
<th>Recapture s</th>
<th>Total captures</th>
</tr>
</thead>
<tbody>
<tr>
<td>2007-08</td>
<td>280</td>
<td>10</td>
<td>9</td>
<td>19</td>
</tr>
<tr>
<td>2008-09</td>
<td>2793</td>
<td>34</td>
<td>40</td>
<td>74</td>
</tr>
<tr>
<td>2009-10</td>
<td>2898</td>
<td>20</td>
<td>52</td>
<td>73</td>
</tr>
<tr>
<td>2010-11</td>
<td>2173</td>
<td>15</td>
<td>30</td>
<td>44</td>
</tr>
<tr>
<td>2011-12</td>
<td>1914</td>
<td>22</td>
<td>27</td>
<td>48</td>
</tr>
<tr>
<td>2012-13</td>
<td>No datac</td>
<td>8</td>
<td>11</td>
<td>19</td>
</tr>
<tr>
<td>2013d</td>
<td>No data</td>
<td>1</td>
<td>8</td>
<td>9</td>
</tr>
</tbody>
</table>

a Number of traps set for capture during an overnight period.
b Includes one orphan fisher kit captured in a live trap in 2010-11, and one orphan fisher kit captured in a live trap in 2011-12.
c PSW Forest Service trapping, no data on trap nights.
d Apr 1 to Dec 31; end of SNAMP Fisher.

### Table 9. Number of radiocollared fishers being monitored for the SNAMP Fisher Project at the start and end of six different population years.

<table>
<thead>
<tr>
<th>Population Year</th>
<th>Start N</th>
<th>End N</th>
<th>Individual fishers N^a</th>
</tr>
</thead>
<tbody>
<tr>
<td>2007-08</td>
<td>7</td>
<td>11</td>
<td></td>
</tr>
<tr>
<td>2008-09</td>
<td>6</td>
<td>30</td>
<td>41</td>
</tr>
<tr>
<td>2009-10</td>
<td>30</td>
<td>32</td>
<td>51</td>
</tr>
<tr>
<td>2010-11</td>
<td>32</td>
<td>32</td>
<td>55</td>
</tr>
<tr>
<td>2011-12</td>
<td>32</td>
<td>44</td>
<td>59</td>
</tr>
<tr>
<td>2012-13</td>
<td>44</td>
<td>33</td>
<td>50</td>
</tr>
<tr>
<td>2013b</td>
<td>33</td>
<td>14</td>
<td>33</td>
</tr>
</tbody>
</table>

^a Number of individual fishers radiocollared and monitored for ≥ 1 day
b Apr 1 to Dec 31; end of SNAMP Fisher
Figure 10. Number of radiocollared fishers that were monitored for survival and reproduction (females) during the period of SNAMP Fisher from December 2007 to December 2013.
Basic Camera Survey Results

Camera trap surveys were a major aspect of SNAMP Fisher in all years. In the overall period of the study we surveyed for fisher presence in 905 unique 1-km² grids. The distribution of camera surveys extended from Yosemite Valley in the north, to the slopes above the San Joaquin River canyon to the south and southeast (Fig. 11). Surveys occurred within Yosemite National Park in winter 2009 only, research that was part of a companion study organized by Reginald Barrett and funded by the California Department of Fish Wildlife. We also obtained data from camera trap surveys in 24 grids located north of the Merced River in Yosemite Valley (not displayed) that were completed by cooperating biologists from Yosemite National Park or the Central Sierra Nevada Environmental Research Center (CSERC). Fishers were not detected in any of the 24 grids, reinforcing that the Merced River is the northern edge of the range of fishers in the southern Sierra Nevada.

Figure 11. Distribution of camera trap surveys in the SNAMP Fisher study area from October 2007 to October 2013.
Fisher activity was identified in 448 of the 905 unique grids surveyed (Fig. 12). We used information on the proportion of surveyed grids surveyed with fisher detections to estimate the elevation range for fishers in the overall study area (Fig. 13). Fishers were most common between 4500 and 6500 feet elevation (1372 and 1981 m elevation). Fisher detections were uncommon above 7500 feet (2286 m) elevation, but the pattern suggested fishers occasionally use private lands outside of the Sierra National Forest as low as 3000 feet (914 m) elevation (Fig. 13).

Camera trap effort was focused in the Key Watershed focal study area. The number of 1-km² grids surveyed ranged from 122 in 2007-08 and 133 in 2012-13 (Table 10). Across the larger overall SNAMP Fisher study area we surveyed 204 1-km² grids in 2012-13 and 409 grids in camera year.
Naïve occupancy for all grids surveyed varied from ≈ 0.60 in 2008-09 to ≈ 0.40 in 2009-10 (Table 10). Occupancy for multi-year surveyed grids (corrected for probability of detection <1.0) oscillated from ≈ 0.80 in 2007-08 to 0.62 in 2009-10 and then increased back to ≈ 0.80 in 2011-12 (Fig. 14).

In addition to basic naïve occupancy (presence/absence), we assessed fisher activity based on the number of occasions that fishers visited camera trap stations. Visit occasions were defined as distinct event periods when fishers activated the motion sensors with at least a 15 minute break between successive visits. Review of images suggested this was an appropriate period of time separating distinct visit periods. We scored a total 4727 fisher visits to camera trap stations during the study (range 583 to 951; Table 11). Fisher visits ranged from 11.6 (2010-11) to 20.4 (2012-13) per 100 trap nights (Table 11). However, and in accordance with our finding of lower probability of detection for fishers during summer season compared to fall and winter

Table 10. Number of 1km² grids surveyed with camera traps by camera survey year (Oct 15 to Oct 14).

<table>
<thead>
<tr>
<th>Camera Year</th>
<th>Key Watersheds</th>
<th>Outside Key Watersheds</th>
<th>Entire study area</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Grids</td>
<td>Fisher detected</td>
<td>Naïve occupancy</td>
</tr>
<tr>
<td>2007-08</td>
<td>122</td>
<td>71</td>
<td>0.582</td>
</tr>
<tr>
<td>2008-09</td>
<td>129</td>
<td>75</td>
<td>0.581</td>
</tr>
<tr>
<td>2009-10</td>
<td>127</td>
<td>75</td>
<td>0.591</td>
</tr>
<tr>
<td>2010-11</td>
<td>125</td>
<td>82</td>
<td>0.656</td>
</tr>
<tr>
<td>2011-12</td>
<td>128</td>
<td>98</td>
<td>0.766</td>
</tr>
<tr>
<td>2012-13</td>
<td>133</td>
<td>70</td>
<td>0.526</td>
</tr>
</tbody>
</table>

All years unique grids surveyed: N = 905

a Some grids were surveyed twice during a camera year; those grids were counted once for this summary.
seasons (Popescu et al. 2014), fisher visits/100 camera survey days was very low during summer (3.6), and highest during winter (33.3; Table 12). Higher probability of detection during winter is due to presence of fewer species of squirrels and other prey during winter compared to in summer. For example, California ground squirrels (*Otospermophilus beecheyi*) and long-eared chipmunks (*Tamias quadrimaculatus*) enter into torpor (hibernation) during winter, and data on alligator lizards (*Elgaria multicarinata*) and other summer season prey are not available.

![Figure 14. Estimated fisher occupancy (95% CI) for multi-year surveyed (*n* = 292) during six camera survey years for the SNAMP Fisher study. Occupancy is corrected for imperfect probability of detection.](image)

### Table 11. Summary data on the number of camera days for all camera traps used to survey for fishers (effort), and the number of fisher visits during each camera survey year (~Oct 15 to Oct 14).

<table>
<thead>
<tr>
<th>Camera year</th>
<th>Camera days (all cameras)*a</th>
<th>Camera days (Fisher grids)*b</th>
<th>Fisher visits*c</th>
<th>Visits per 100 camera days*c</th>
</tr>
</thead>
<tbody>
<tr>
<td>2007-08</td>
<td>7914</td>
<td>4328</td>
<td>583</td>
<td>13.5</td>
</tr>
<tr>
<td>2008-09</td>
<td>10605</td>
<td>5550</td>
<td>794</td>
<td>14.3</td>
</tr>
<tr>
<td>2009-10</td>
<td>14955</td>
<td>5990</td>
<td>951</td>
<td>15.9</td>
</tr>
<tr>
<td>2010-11</td>
<td>16457</td>
<td>5614</td>
<td>649</td>
<td>11.6</td>
</tr>
<tr>
<td>2011-12</td>
<td>14059</td>
<td>7149</td>
<td>949</td>
<td>13.3</td>
</tr>
<tr>
<td>2012-13</td>
<td>7584</td>
<td>3926</td>
<td>801</td>
<td>20.4</td>
</tr>
</tbody>
</table>

*a Estimated days that camera traps were functioning and focused on the bait tree

*b Functional camera days for grids with fisher detections

*c Based on images sequences with fishers (fisher detections) that were separated by a minimum of 15 minutes.

*d Fisher visits divided by functional camera days for grids with fisher detections x 100
Table 12. Fisher visit to survey cameras and camera effort by season from October 25, 2007 to October 15, 2013. Seasons were based on the solar cycle: Fall - Sep 21 to Dec 20; Winter - Dec 21 to Mar 20; Spring - Mar 21 to Jun 20; Summer - Jun 21 to Sep 20.

<table>
<thead>
<tr>
<th>Season</th>
<th>Fisher visits</th>
<th>Estimated camera days</th>
<th>Camera days (925 to 2135 m elev)a</th>
<th>Visits per 100 camera days</th>
<th>Visits per 100 camera days (fisher elevation)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Fall</td>
<td>1103</td>
<td>19027</td>
<td>7010</td>
<td>5.8</td>
<td>15.7</td>
</tr>
<tr>
<td>Winter</td>
<td>2543</td>
<td>22113</td>
<td>7629</td>
<td>11.5</td>
<td>33.3</td>
</tr>
<tr>
<td>Spring</td>
<td>733</td>
<td>12069</td>
<td>5032</td>
<td>6.1</td>
<td>14.6</td>
</tr>
<tr>
<td>Summer</td>
<td>348</td>
<td>18365</td>
<td>9597</td>
<td>1.9</td>
<td>3.6</td>
</tr>
</tbody>
</table>

aCamera days within the typical elevation range for fishers in the SNAMP Study area [925 m (3000 ft) to 2135 m (7000 ft); see Fig. 9.

Illustration 17: Winter period and summer period fisher detections at camera trap survey stations
Fisher Denning and Reproduction

Denning period

Den cameras provided detailed information on the activities of 32 adult female fishers during six spring denning seasons. Based on information from the spatial clustering of aerial telemetry locations, ground-based telemetry, and den cameras, denning was initiated in the last week of March in most years (earliest date was March 22), and females typically ceased regular use of den trees in the first week of June (Table 13). The latest known regular use of a den tree was June 20 in spring 2012. It is likely that females continued to use trees as short term den/rest structures during summer when their dependent kits were trailing them, but we did not attempt to systematically identify those types of short duration use structures.

<p>| Table 13. Estimated dates for the initiation and end of denning by female fishers in the Sierra National Forest, California. Data are from March 2008 to June 2013. |</p>
<table>
<thead>
<tr>
<th>Season</th>
<th>N</th>
<th>Mean start of denning&lt;sup&gt;a&lt;/sup&gt;</th>
<th>Estimate</th>
<th>Mean end of denning</th>
<th>N</th>
<th>Estimate</th>
</tr>
</thead>
<tbody>
<tr>
<td>2008</td>
<td>1</td>
<td>27-Mar-08</td>
<td></td>
<td>1</td>
<td>4-Jun-08</td>
<td></td>
</tr>
<tr>
<td>2009</td>
<td>12</td>
<td>27-Mar-09</td>
<td>9</td>
<td>6-Jun-09</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2010</td>
<td>13</td>
<td>25-Mar-10</td>
<td>8</td>
<td>4-Jun-10</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2011</td>
<td>8</td>
<td>1-Apr-11</td>
<td>5</td>
<td>5-Jun-11</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2012</td>
<td>11</td>
<td>31-Mar-12</td>
<td>7</td>
<td>2-Jun-12</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2013</td>
<td>12</td>
<td>27-Mar-13</td>
<td>7</td>
<td>1-Jun-13</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<sup>a</sup>Estimated from spatial clustering of sequential aerial radiotelemetry locations (Zhao et al 2012).

<p>| Table 14. Summary data on denning activities by female fishers determined from monitoring den trees with remote cameras. Data are from the Sierra National Forest, California from April 2008 to June 2012. |</p>
<table>
<thead>
<tr>
<th>Spring</th>
<th>Female fishers</th>
<th>Den trees monitored</th>
<th>Monitor days&lt;sup&gt;a&lt;/sup&gt;</th>
<th>Avg monitor days/female</th>
<th>Total detections&lt;sup&gt;b&lt;/sup&gt;</th>
<th>Daily rates</th>
<th>Up/Down movements&lt;sup&gt;d&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td>2008</td>
<td>1</td>
<td>3</td>
<td>37</td>
<td>37.0</td>
<td>30</td>
<td>0.568</td>
<td>1.1</td>
</tr>
<tr>
<td>2009</td>
<td>13</td>
<td>45</td>
<td>449</td>
<td>34.5</td>
<td>366</td>
<td>0.601</td>
<td>1.2</td>
</tr>
<tr>
<td>2010</td>
<td>14</td>
<td>47</td>
<td>479</td>
<td>34.2</td>
<td>353</td>
<td>0.568</td>
<td>1.1</td>
</tr>
<tr>
<td>2011</td>
<td>9</td>
<td>28</td>
<td>260</td>
<td>28.9</td>
<td>264</td>
<td>0.725</td>
<td>1.1</td>
</tr>
<tr>
<td>2012</td>
<td>11</td>
<td>37</td>
<td>406</td>
<td>36.9</td>
<td>439</td>
<td>0.733</td>
<td>1.3</td>
</tr>
<tr>
<td>2013</td>
<td>12</td>
<td>40</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<sup>a</sup> Excludes periods when females moved kits and the next maternal den tree had not yet been located.

<sup>b</sup> All detections identified as a departure or return to the den tree, as well as events when fishers were at the base of den trees but not identified as departing or returning.

<sup>c</sup> Proportion of days den trees were being monitored for which at least one detection was made by den cameras.

<sup>d</sup> Mean number of return and departures movements for days when fishers were detected by cameras.
In the spring 2008 den season SNAMP Fisher monitored a single female fisher, but in all other years we monitored at least nine individual females (Table 14). The mean number of dens used per female per season was 2.4 (range 1 to 5), and the mean number of cameras used to monitor each den structure was 3.1. On average each denning female was monitored with den cameras 34.3 days/season (range 28.9 to 37; Table 14), excluding days or periods when successive use maternal den trees were yet to be identified. Fifteen female fishers were monitored with den cameras in one den season, 11 were monitored in two seasons, three were monitored in three seasons, two were monitored in four seasons, and one was monitored in five denning seasons.

Denning activity, litter size and weaning rates

Denning status was determined for 89 of 93 total denning opportunities for breeding-age (≥24 months) females in 6 denning seasons from 2008 to 2013 (Table 15). We were unable to adequately monitor 4 breeding-age females for determining denning status when radiocollars were shed (n = 3) or ceased functioning (n = 1) within the first 31 days of a denning season. The average date that females initiated denning behavior was March 28 (range March 22 to April 9). The average date that females ceased localizing to den trees was June 9 (range May 30 to June 22).

Seventy-six (85%) breeding-age female fishers either exhibited denning behavior (n = 63) or were determined to have denned and weaned at least 1 kit based on size of teats (n = 13; Table 15). Among 76 breeding-age females that initiated denning, 64 (84%) were identified as weaning kits. Overall, 72% of 89 known status, adequately monitored denning opportunities for breeding-age females produced at least one weaned kit (Table 15).

Eleven (17.5%) of 63 cases of denning for females that were monitored during spring periods failed prior to kits being weaned (Table 16). Three of the 11 denning failures were females that initiated denning but ceased localizing to natal den trees 17, 35, and 41 days later, potentially related to the death of kits. Eight den failures were due to death of the denning female; seven deaths were by attacks by predators, and one was the result of a denning female either dying of internal bleeding induced by exposure to rodenticides, or from the combination of trauma from being struck by a vehicle on a highway and internal bleeding related to exposure to rodenticides. One of the seven females that died from predator attack was infected with canine distemper virus, which may have contributed to her vulnerability (Keller et al. 2012).
### Table 15. Summary of female fisher (≥2 years old) denning and weaning rates by age class and year on the Bass Lake Ranger District in the Sierra National Forest, California, 2008-2013.

<table>
<thead>
<tr>
<th>Pop Year</th>
<th>Adult Females&lt;sup&gt;a&lt;/sup&gt;</th>
<th>Monitored mid-Mar to May 31&lt;sup&gt;b&lt;/sup&gt;</th>
<th>Teats measured (Jul to Jan)&lt;sup&gt;c&lt;/sup&gt;</th>
<th>Denning&lt;sup&gt;d&lt;/sup&gt;</th>
<th>Proportion denned&lt;sup&gt;e&lt;/sup&gt;</th>
<th>Unknown status&lt;sup&gt;f&lt;/sup&gt;</th>
<th>Failed&lt;sup&gt;g&lt;/sup&gt;</th>
<th>Died while denning</th>
<th>Weaned&lt;sup&gt;h&lt;/sup&gt;</th>
<th>Proportion weaned&lt;sup&gt;i&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age class (Years)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>30</td>
<td>26</td>
<td>1</td>
<td>21</td>
<td>0.78</td>
<td>3</td>
<td>1</td>
<td>2</td>
<td>18</td>
<td>0.67</td>
</tr>
<tr>
<td>3-5</td>
<td>63</td>
<td>48</td>
<td>13</td>
<td>55</td>
<td>0.87</td>
<td>3</td>
<td>5</td>
<td>46</td>
<td>0.75</td>
<td></td>
</tr>
<tr>
<td>≥6</td>
<td>12</td>
<td>10</td>
<td>2</td>
<td>9</td>
<td>0.75</td>
<td></td>
<td>1</td>
<td>8</td>
<td>0.67</td>
<td></td>
</tr>
<tr>
<td>Year</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2008</td>
<td>11</td>
<td>2</td>
<td>9</td>
<td>9</td>
<td>0.82</td>
<td></td>
<td></td>
<td></td>
<td>9</td>
<td>0.82</td>
</tr>
<tr>
<td>2009</td>
<td>17</td>
<td>14</td>
<td>3</td>
<td>15</td>
<td>0.88</td>
<td></td>
<td></td>
<td></td>
<td>13</td>
<td>0.76</td>
</tr>
<tr>
<td>2010</td>
<td>17</td>
<td>15</td>
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<td>14</td>
<td>0.88</td>
<td>1</td>
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<td>11</td>
<td>3</td>
<td>12</td>
<td>0.86</td>
<td>2</td>
<td>1</td>
<td></td>
<td>10</td>
<td>0.71</td>
</tr>
<tr>
<td>2012</td>
<td>17</td>
<td>17</td>
<td>14</td>
<td></td>
<td>0.82</td>
<td></td>
<td>3</td>
<td></td>
<td>11</td>
<td>0.65</td>
</tr>
<tr>
<td>2013</td>
<td>15</td>
<td>15</td>
<td>12</td>
<td>0.86</td>
<td></td>
<td></td>
<td></td>
<td>10</td>
<td>0.79</td>
<td></td>
</tr>
</tbody>
</table>

<sup>a</sup> All females ≥ 24 months of age that were known in the population during the year. Includes females that were captured after the end of the denning season in mid June.

<sup>b</sup> Number of females monitored by radio telemetry during all or part of the period before they died or had a dropped/failed collar after denning status had been determined.

<sup>c</sup> Number of females the were not monitoring during the denning period, but were captured during July to January when teat measurements were taken and used to determined weaning status as described by Matthews et al. (2013).

<sup>d</sup> Number of females that exhibited denning behavior, or that were determined to have weaned at least one kit based on teat measurements.

<sup>e</sup> Number of denning females divided by the number of adult females minus the number of females of unknown status.

<sup>f</sup> Number of females (≥ 2 years old) for which denning was unknown or suspected, but dropped or failed radiocollars prevented determination of denning status.

<sup>g</sup> Number of females (≥2 years old) that exhibited denning behavior and ceased denning behavior prior to weaning.

<sup>h</sup> Number of denning females that were known alive and exhibited denning behavior until after May 31.

<sup>i</sup> Number of females that denning to weaning divided by the number of adult females minus the number of females with unknown status.
Table 16.—Information on female fisher kit production for six spring denning seasons (March 21 to June 31) in the Sierra National Forest, California, October 2008 to June 2013.

<table>
<thead>
<tr>
<th>Age class (Years)</th>
<th>Denning females&lt;sup&gt;a&lt;/sup&gt;</th>
<th>Denning females with kit counts&lt;sup&gt;b&lt;/sup&gt;</th>
<th>Kits&lt;sup&gt;c&lt;/sup&gt;</th>
<th>Litter size&lt;sup&gt;d&lt;/sup&gt;</th>
<th>Denning females deaths&lt;sup&gt;e&lt;/sup&gt;</th>
<th>Known kit deaths&lt;sup&gt;f&lt;/sup&gt;</th>
<th>Denned to Weaning&lt;sup&gt;g&lt;/sup&gt;</th>
<th>Kits weaned&lt;sup&gt;h&lt;/sup&gt;</th>
<th>Kits weaned per litter (fecundity)</th>
</tr>
</thead>
<tbody>
<tr>
<td>2</td>
<td>19</td>
<td>15</td>
<td>21</td>
<td>1.40</td>
<td>1</td>
<td>14</td>
<td>19</td>
<td>1.27</td>
<td></td>
</tr>
<tr>
<td>3-5</td>
<td>32</td>
<td>26</td>
<td>40</td>
<td>1.60</td>
<td>3</td>
<td>7</td>
<td>22</td>
<td>34</td>
<td>1.32</td>
</tr>
<tr>
<td>≥6</td>
<td>8</td>
<td>7</td>
<td>12</td>
<td>1.71</td>
<td>7</td>
<td>7</td>
<td>12</td>
<td>1.71</td>
<td></td>
</tr>
<tr>
<td>Year</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2008</td>
<td>2</td>
<td>1</td>
<td>1</td>
<td></td>
<td>1</td>
<td>1</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2009</td>
<td>12</td>
<td>9</td>
<td>15</td>
<td>1.5</td>
<td>1</td>
<td>10</td>
<td>15</td>
<td>1.5</td>
<td></td>
</tr>
<tr>
<td>2010</td>
<td>13</td>
<td>11</td>
<td>20</td>
<td>1.8</td>
<td>3</td>
<td>7</td>
<td>7</td>
<td>13</td>
<td>1.9</td>
</tr>
<tr>
<td>2011</td>
<td>8</td>
<td>7</td>
<td>11</td>
<td>1.6</td>
<td>1</td>
<td>7</td>
<td>11</td>
<td>1.6</td>
<td></td>
</tr>
<tr>
<td>2012</td>
<td>14</td>
<td>11</td>
<td>16</td>
<td>1.5</td>
<td>3</td>
<td>2</td>
<td>10</td>
<td>14</td>
<td>1.4</td>
</tr>
<tr>
<td>2013</td>
<td>10</td>
<td>8</td>
<td>10</td>
<td>1.3</td>
<td></td>
<td>8</td>
<td>10</td>
<td>1.3</td>
<td></td>
</tr>
</tbody>
</table>

<sup>a</sup> Number of females (≥2 years old) that exhibited denning behavior and were monitored by radiotelemetry, den cameras, or both. Excludes females whose reproductive status was not known and those that initiated denning behavior but ceased denning before May.

<sup>b</sup> Number of denning females for which kit counts were determined by images from den cameras, den cavity video camera, or both.

<sup>c</sup> Total number of kits counted.

<sup>d</sup> Number of denning females with kit counts divided by the number of kits counted.

<sup>e</sup> Number of denning females known to have died during the denning season while provisioning kits in den trees. Numbers of kits in litters were not known for all of the denning females that died.

<sup>f</sup> Kits that were known present in den trees when the mother died, or those that were found dead inside den cavities. This estimate assumes that 5 orphan kits that were removed from den cavities would have perished if they had not been rescued.

<sup>g</sup> Number of monitored denning female fishers exhibiting denning behavior that continued to weaning.
Six of eight deaths of denning females occurred when the locations of den trees were known and were being monitored. In one case den camera images included a bobcat with a dead kit in its mouth, and the partial carcass of the denning female was recovered nearby. In a second case the den structure was a large, unstable snag, and we did not attempt to climb the tree to determine litter size due to safety considerations. In each of the other four cases we climbed the den trees to assess litter size, and recover kits in accordance with California Department of Fish and Wildlife policy. A total five live kits were recovered from two of the den trees (litter size 2, 3), two deceased kits were found in a den cavity of the third tree, and we failed to find kits in the fourth tree. For this fourth tree, the lack of images of the female from den cameras suggested she had moved the litter to a different, unidentified den tree several days prior to her death.

The five orphan kits that were rescued were raised in captivity by a local wildlife rehabilitation organization licensed by the California Department of Fish and Wildlife, and under the care and supervision of a professional zoo veterinarian. One of the orphan kits died in captivity by urinary tract blockage attributed to a parasitic nematode, whereas the other 4 survived captive rearing. Two kits from one litter were released within their mother’s home range, and the two kits from the second litter were released into an area with suitable fisher habitat abutting the south margin of the study site.

We used a combination of images from den cameras \((n = 43)\) and den cavity investigations with a video camera \((n = 4)\) to determine litter size for 48 of 59 denning females that were monitored (Table 16). A total 73 kits were known produced, and average litter size was 1.5 (Table 16). After accounting for known mortalities of denning females, we estimated that 64 of the 73 kits produced were weaned from den trees, whereas seven kits died or would have died had they not been rescued (Table 16).

Denning structures

We identified 125 unique structures used as natal or maternal dens, including 54 black oak, 41 incense cedar, 19 white fir, 10 sugar pine or ponderosa pine, and one canyon oak \((Quercus chrysolepsis)\) (Table 17).

Repeat use of den trees was not uncommon. Sixteen individual den trees were used more than once; 15 trees were used in two years, and one tree was used in four different den seasons. In all but two cases of repeat den tree use the same individual reused one or several den trees between successive years. In two cases a female used a den that had been used by a different female in a previous year. Successive dens of females that used more than 1 den structure were located an average of 413 m apart \((n = 52, \text{ range 75-1398})\).
and the average total distance moved between successive dens was 693 m \((n = 31, \text{ range } 75-1687)\). Also, the distance between the natal den tree and the first maternal den tree averaged 419 m, whereas successive use maternal den trees were in closer proximity \((\text{mean}=287 \text{ m, } t_{69} =1.75, P=0.04)\), potentially because older kits are larger in size and mass and therefore more difficult for the female to carry.

Fifty-six percent of the unique individual trees used for denning in the SNAMP area were live trees \((n = 70)\), whereas 44\% \((n = 55)\) were snags (Table 17). Black oak was the most common live tree used for denning, but a high percent of incense cedar were also selected by female fishers (Table 17). Among snags used as denning structures, black oak and incense cedar were both commonly used, whereas white fir and pines (sugar pine or ponderosa pine) were less common as snag-type den trees (Table 17). Overall, black oaks and incense cedar were the two most common tree species used for denning (Table 17).

<table>
<thead>
<tr>
<th>Tree type, Species</th>
<th>Denning events(^a)</th>
<th>Percent within group</th>
<th>Unique structures(^b)</th>
<th>Repeat use structures(^c)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Live trees</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black oak</td>
<td>34</td>
<td>43</td>
<td>31</td>
<td>3</td>
</tr>
<tr>
<td>Incense cedar</td>
<td>25</td>
<td>32</td>
<td>19</td>
<td>4</td>
</tr>
<tr>
<td>White fir</td>
<td>14</td>
<td>18</td>
<td>14</td>
<td></td>
</tr>
<tr>
<td>Sugar pine</td>
<td>3</td>
<td>4</td>
<td>3</td>
<td></td>
</tr>
<tr>
<td>Ponderosa pine</td>
<td>2</td>
<td>3</td>
<td>2</td>
<td></td>
</tr>
<tr>
<td>Canyon oak</td>
<td>1</td>
<td>1</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td><strong>Snags</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black oak</td>
<td>27</td>
<td>42</td>
<td>23</td>
<td></td>
</tr>
<tr>
<td>Incense cedar</td>
<td>27</td>
<td>42</td>
<td>22</td>
<td>4</td>
</tr>
<tr>
<td>White fir</td>
<td>5</td>
<td>8</td>
<td>5</td>
<td>5</td>
</tr>
<tr>
<td>Pine species(^d)</td>
<td>5</td>
<td>8</td>
<td>5</td>
<td></td>
</tr>
<tr>
<td><strong>Live tree or snag</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black oak</td>
<td>61</td>
<td>43</td>
<td>54</td>
<td>3</td>
</tr>
<tr>
<td>Incense cedar</td>
<td>52</td>
<td>36</td>
<td>41</td>
<td>8</td>
</tr>
<tr>
<td>White fir</td>
<td>19</td>
<td>13</td>
<td>19</td>
<td>5</td>
</tr>
<tr>
<td>Pine species</td>
<td>10</td>
<td>7</td>
<td>10</td>
<td></td>
</tr>
<tr>
<td>Canyon oak</td>
<td>1</td>
<td>1</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td><strong>Total den structures</strong></td>
<td>143</td>
<td>125</td>
<td>16</td>
<td></td>
</tr>
</tbody>
</table>

\(^a\) Count of all known denning events for each species of tree.
\(^b\) Count of individual trees; those used in multiple seasons counted once.
\(^c\) Number of individual trees used \(\geq 2\) times for denning; one live cedar tree was used by the same female in four successive denning seasons, but all other repeat use trees were known used in two den seasons only.
\(^d\) Pine snags could not always be identified as sugar pine or ponderosa.
Habitat characteristics of den structures

Mean diameter at breast height (DBH) of black oak denning structures was smaller than that for other tree species used (Table 18). Mean heights of live trees were taller than snags of the same species (Fig. 15), reflecting that many of the snags used for denning were at advanced stages of decay.

<table>
<thead>
<tr>
<th>Tree species</th>
<th>Live trees</th>
<th></th>
<th>Snags or dead trees</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>n</td>
<td>Mean DBH (cm)</td>
<td>Mean height (m)&lt;sup&gt;a&lt;/sup&gt;</td>
<td>n</td>
</tr>
<tr>
<td>Black oak</td>
<td>30</td>
<td>74.2</td>
<td>21.7</td>
<td>5</td>
</tr>
<tr>
<td>Incense cedar</td>
<td>18</td>
<td>127.2</td>
<td>32.5</td>
<td>22</td>
</tr>
<tr>
<td>White fir</td>
<td>14</td>
<td>110.8</td>
<td>33.9</td>
<td>22</td>
</tr>
<tr>
<td>Pines</td>
<td>5</td>
<td>112.8</td>
<td>37.4</td>
<td>5</td>
</tr>
</tbody>
</table>

<sup>a</sup>Data on mean tree height are for the subset of den trees for which detailed data on habitat measurements were completed (n = 84).

The majority of denning structures used by fishers in the SNAMP Fisher study area were in the elevation range from 4500 (1371 m) to 6000 feet (1829 m; 83%, n = 104; Fig. 15b). Additional information obtained from circular habitat plots assessments included indications of high canopy cover, limited herbaceous cover, and relatively low shrub cover near most den trees (Table 19).
Concealment cover was 64% low ground cover, 46% high ground cover, and 38% and 36% low shrub and high shrub cover, respectively. On average, belt transects within the circular habitat plots around den trees included an average of 6.5 down logs (coarse woody debris, CWD; logs/branches with a minimum large end diameter of 15 cm, ≥1 m total length). Many denning structures were on steep slopes (Table 19) but there was no obvious preference for aspect (Fig. 16).

Table 19. Basic habitat attributes around fisher den trees for the SNAMP Fisher Study area in spring 2008 to spring 2012.

<table>
<thead>
<tr>
<th>Attribute</th>
<th>Mean</th>
<th>Range</th>
</tr>
</thead>
<tbody>
<tr>
<td>Canopy cover</td>
<td>72%</td>
<td>30-94%</td>
</tr>
<tr>
<td>Shrub cover</td>
<td>19%</td>
<td>0-82.5%</td>
</tr>
<tr>
<td>Herbaceous cover</td>
<td>6%</td>
<td>0-29%</td>
</tr>
<tr>
<td>Prevailing slope</td>
<td>37%</td>
<td>3-75%</td>
</tr>
</tbody>
</table>

a Habitat attributes are from circular plots (18 m radius) centered on fisher den trees (n = 82). Habitat data were not available for other confirmed den trees.

Figure 16. Aspect orientation of fisher den structures on the SNAMP Fisher Study. Data are for 79 den trees.

Illustration 10: Black oak used for denning in spring 2011. Image of tree from a distance (left), and view of the forked branching higher up the bole of the denning structure (right).
Activity patterns of denning females

Additional insight on denning activities by adult female fishers was provided by analyses of den camera images. Adult females were detected by den cameras at known active dens an average of 0.64 times/day (range 0.57 to 0.73) (Table 14) and the mean number of detections of up and down movements ranged from 1.1 to 1.3 per day (Table 14), indicating that fishers do not typically leave and return to den trees multiple times a day. In addition to information on male visits to den trees (we obtained image sequences of eight mating, or copulation events at the base of den trees), den cameras identified three occasions when a female fisher briefly returned to a den tree at least one day after she had already moved kits to another tree nearby (Table 20). On eight occasions den cameras detected other female fishers (non-collared or different collared fisher) at den trees of female fishers (Table 20).

Information from the 83 occasions when females were detected moving kits was used to estimate fecundity. A total of 1295 detections were identified as female fishers departing from, or returning to the den tree, whereas there were 99 detections of females at or near the base of den trees that could not be unequivocally classified except as active outside the den cavity (Table 20). We were able to identify 316 image sequences consistent with either continuous den attendance, or continuous time away from the den when denning females were likely foraging. Den attendance bouts were shortest late in the den season and longest in the middle of the den season (Table 21). Forage bouts away from den trees were shortest early in the den season and approximately similar in duration thereafter (Table 21).

<table>
<thead>
<tr>
<th>Year</th>
<th>Departing</th>
<th>Returning</th>
<th>Base tree</th>
<th>Bringing food to tree</th>
<th>Kit move</th>
<th>At tree after kits moved</th>
<th>Other female at tree</th>
</tr>
</thead>
<tbody>
<tr>
<td>2008</td>
<td>15</td>
<td>9</td>
<td>6</td>
<td>1</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2009</td>
<td>118</td>
<td>198</td>
<td>35</td>
<td>12</td>
<td>1</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2010</td>
<td>133</td>
<td>163</td>
<td>37</td>
<td>1</td>
<td>20</td>
<td></td>
<td>2</td>
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<tr>
<td>2011</td>
<td>120</td>
<td>127</td>
<td>10</td>
<td>14</td>
<td>1</td>
<td>6</td>
<td></td>
</tr>
<tr>
<td>2012</td>
<td>178</td>
<td>234</td>
<td>11</td>
<td>8</td>
<td>25</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td>2013*</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>min 11</td>
</tr>
</tbody>
</table>

* Detections at base of tree, or on the tree for which directionality or activity was uncertain

b Detections when the female was carrying objects as they returned and ascended the den tree

c General information only available for 2013.
Sixty-six (60%) of the 110 individual fishers radiocollared during the study were known to have died, including 32 females and 34 males (Table 22). Excluding population year 2007-08, an average of 10.5 radiocollared fishers perished each population year (Fig. 17). The mean number of deaths by gender for population year 2008-09 through population year 2013-14 was 5.3 for females and 5.2 for males.

Fisher survival with population data combined into 2-year periods was generally higher for adult and juvenile fishers than for subadults (Table 23). Ninety-five percent confidence intervals overlapped for females and males in all two-year period with the possible exception of subadults in year group 3. Two-year survival rates among females ranged from a low of 47% to a high of 89% for

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**Table 21.** Information on den attendance and away from den tree foraging excursions, developed from analyses of data from cameras used to monitor fisher den trees during five denning seasons. Data are from the Sierra National Forest, California from April 2008 to June 2012.

<table>
<thead>
<tr>
<th>Season</th>
<th>Cases</th>
<th>Mean (Min, Max)</th>
<th>Cases</th>
<th>Mean (Min, Max)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Early</td>
<td>38</td>
<td>371.0 (4.4, 1072.5)</td>
<td>64</td>
<td>235.1 (34, 746.1)</td>
</tr>
<tr>
<td>Middle</td>
<td>43</td>
<td>535.6 (2.3, 996.2)</td>
<td>53</td>
<td>431.8 (29.3, 811.4)</td>
</tr>
<tr>
<td>Late</td>
<td>68</td>
<td>323.3 (6.0, 1049.4)</td>
<td>50</td>
<td>405.5 (22.8, 807.8)</td>
</tr>
<tr>
<td>Overall</td>
<td>149</td>
<td>396.7 (2.3, 1072.5)</td>
<td>167</td>
<td>348.6 (22.8, 811.4)</td>
</tr>
</tbody>
</table>

*Seasons were Early (March 26 to April 20), Middle (April 21 to May 15), and Late, (May 16 to June 11), identified by dividing the overall den season into three 25 day periods from late March to mid-June.

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**Figure 17.** Number of radiocollared female and male fishers known to have died during the SNAMP Fisher Study.
Table 22. Review of all known deaths of radiocollared fishers in seven population years (Apr 1 to Mar 31), summarized by gender, and cause-specific mortality from necropsy examinations by pathologists at the UC Davis School of Veterinary Medicine (Davis, CA). Necropsy reports were not available for 16 fishers.

<table>
<thead>
<tr>
<th>Year, Gender</th>
<th>Predation</th>
<th>Disease</th>
<th>Starvation-related injury, septicemia</th>
<th>Road kill</th>
<th>Rodenticide toxicosis</th>
<th>Indeterminate, unknown</th>
<th>Pending necropsy</th>
</tr>
</thead>
<tbody>
<tr>
<td>2007-08</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>1</td>
<td>1</td>
<td>1</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Male</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2008-09</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>4</td>
<td></td>
<td>1</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Male</td>
<td>1</td>
<td>1</td>
<td>1</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
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</tr>
<tr>
<td>Male</td>
<td>12</td>
<td>5</td>
<td>1</td>
<td>3</td>
<td>2</td>
<td>2</td>
<td>9</td>
</tr>
</tbody>
</table>

a One female death by predation in 2009-10 may have been related to the animal being weakened/sick from CDV when it encountered a coyote (*Canis latrans*); further discussed by Keller et al. (2012).
b Three disease deaths were associated with canine-distemper virus, one was considered by Toxoplasmosis, and one was due to pleruritus+pneumonia.
c Most deaths in this category were associated with prior injury that contributed to starvation and septicemia.
d Necropsies were completed, but cause of death could not be determined.
e Includes deaths of two male fishers that died January 1, 2014 and March 31, 2014. Although this was after the end of SNAMP Fisher, both of the animals were radiocollared as part of SNAMP.
subadult females (Table 23). Two year survival for juvenile females was always ≥ 74%, whereas among adult females it ranged from a low of 0.69% to a high of 0.86 (Table 23). Fisher survival for all years combined was highest for juvenile females and lowest for subadult males (Table 23). Also, although not significantly different, survival was consistently higher for females compared to males (all age classes; Table 23). In general, survival among females is more important for understanding the status of this fisher population than male survival, particularly because there were a good number of males in the population in all years of the study (Fig. 10).

### Causes of Mortality

Necropsy reports have been completed for 50 of the 66 radiocollared fishers that died during...
the SNAMP Fisher study. Assignment of cause-specific mortality was possible for 47 of the 50
animals with necropsy reports (94%). Three necropsy reports were indeterminate with regards cause
of death for the fisher (Table 22). To date, a known cause of death has been determined for 71% of the
66 mortalities.

Among known-cause mortalities predation was the primary cause of death, accounting for 68%
of 47 known-cause deaths (Fig. 18). Deaths by disease, injury-related starvation or septicemia, and
human-linked factors combined to account for 32% of known-cause mortalities (Fig. 18).

Predation accounted for nearly twice as many known cause deaths for females (43%) than for
males (26%), whereas all of the disease and roadkill deaths were males (Fig. 18). Serological testing of blood samples
collected at captures revealed low levels of exposure to canine distemper virus in the study population (Gabriel 2013).
However, in spring 2009 a relatively small scale epizootic of CDV occurred in the study population, which contributed
to the deaths of four fishers; three by direct infection, and one that was killed by a coyote attack, but was likely in a
weakened state due to presence of CDV infection (Table 22, Fig. 18; Keller et al. 2012).

In Spring 2009 the SNAMP Fisher research team recovered the first fisher known to have died by toxicosis after
exposure to rodenticides. In total, three fishers were known to have died after exposure to rodenticides as of June 2014,
including two males and one female. The discovery of death associated with rodenticides led to two peer-reviewed

Figure 18. Summary of necropsy-determined causes of death for radiocollared female and male fishers (a), and
percent of known-cause mortalities for female and male fishers on the SNAMP Fisher Project from Dec 2007 to
Dec 2013.
papers. One detailed issues with anticoagulant rodenticides on public lands (Gabriel et al. 2012) and a second paper revealed that female fishers with larger numbers of marijuana grow sites within their home ranges experience reduced survival (Thompson et al. 2013).

Illustration 11: Remains of a female fisher killed by a predator (left), and a male fisher that was determined to have died by infectious disease (right).
Population Growth Rates

Empirically developed estimates of key demographic parameters needed to estimate a deterministic growth rate for the population ($\lambda$) were developed during the study (Tables 22, 23). Estimates for $\lambda$ were below 1.0 (population decline) in two 2-year groups, equal to 1.0 in one 2-year group (stable), and slightly positive in two 2-year groups (increasing population) (Table 24). The All Years $\lambda$ was 0.90, which was suggestive of population decline, however, the range for all results overlapped 1.0.

Table 24. Demographic parameters and deterministic population growth rates (range) for five two-year groups$^a$, and for population data for all years of the study combined (All years).

<table>
<thead>
<tr>
<th>Parameter, Age class</th>
<th>Year group 1</th>
<th>Year group 2</th>
<th>Year group 3</th>
<th>Year group 4</th>
<th>Year group 5</th>
<th>All Years</th>
</tr>
</thead>
<tbody>
<tr>
<td>Weaning reproduction</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Young adult</td>
<td>0.67</td>
<td>0.89</td>
<td>1.00</td>
<td>0.83</td>
<td>0.70</td>
<td>0.68</td>
</tr>
<tr>
<td>Adult</td>
<td>0.75</td>
<td>0.67</td>
<td>0.83</td>
<td>0.82</td>
<td>0.87</td>
<td>0.74</td>
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<tr>
<td>Weaning litter size</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Young adult</td>
<td>1.27</td>
<td>1.57</td>
<td>1.50</td>
<td>1.20</td>
<td>1.20</td>
<td>1.19</td>
</tr>
<tr>
<td>Adult</td>
<td>1.31</td>
<td>1.31</td>
<td>1.60</td>
<td>1.55</td>
<td>1.40</td>
<td>1.45</td>
</tr>
<tr>
<td>Weaning fecundity ($b_i$)$^b$</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Young adult</td>
<td>0.42</td>
<td>0.70</td>
<td>0.75</td>
<td>0.50</td>
<td>0.42</td>
<td>0.41</td>
</tr>
<tr>
<td>Adult</td>
<td>0.49</td>
<td>0.44</td>
<td>0.67</td>
<td>0.64</td>
<td>0.61</td>
<td>0.53</td>
</tr>
<tr>
<td>Survival ($P_i$)</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Juvenile</td>
<td>0.76</td>
<td>0.80</td>
<td>0.74</td>
<td>0.80</td>
<td>1.00</td>
<td>0.75</td>
</tr>
<tr>
<td>Subadult</td>
<td>0.47</td>
<td>0.67</td>
<td>0.89</td>
<td>0.73</td>
<td>0.73</td>
<td>0.71</td>
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<tr>
<td>Adult</td>
<td>0.81</td>
<td>0.70</td>
<td>0.69</td>
<td>0.86</td>
<td>0.74</td>
<td>0.73</td>
</tr>
<tr>
<td>Fertility ($b_i$)$P_i$</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Young adult</td>
<td>0.20</td>
<td>0.47</td>
<td>0.67</td>
<td>0.36</td>
<td>0.31</td>
<td>0.29</td>
</tr>
<tr>
<td>Adult</td>
<td>0.40</td>
<td>0.30</td>
<td>0.46</td>
<td>0.55</td>
<td>0.45</td>
<td>0.39</td>
</tr>
<tr>
<td></td>
<td>(0.65-</td>
<td>(0.63-</td>
<td>(0.77-</td>
<td>(0.81-</td>
<td>(0.77-</td>
<td>(0.71-</td>
</tr>
<tr>
<td></td>
<td>1.08)</td>
<td>1.12)</td>
<td>1.22)</td>
<td>1.26)</td>
<td>1.22)</td>
<td>1.12)</td>
</tr>
<tr>
<td>Leslie Matrix $\lambda$</td>
<td>0.87</td>
<td>0.88</td>
<td>1.00</td>
<td>1.04</td>
<td>1.03</td>
<td>0.90</td>
</tr>
</tbody>
</table>

$^a$Two-year groups were 2008-09 and 2009-10 (1), 2009-10 and 2010-11 (2), 2010-11 and 2011-12 (3), 2011-12 and 2012-13 (4), and 2012-13 and 2013-14 (5).

$^b$Fecundity is the number of female offspring produced, calculated as weaning reproduction*weaning litter size*0.5 (assumes equal sex ratio at birth)

$^c$The range for $\lambda$ was based on the 95% confidence intervals for the survival rates for the five two-year groups (Table 23). The range for $\lambda$ for the All years data was based on the 95% CIs for the means for weaning reproductive rate and litter size, and for the 95% CIs for age-specific survival (Table 23).
Population Size and Density

Population size was estimated for the middle four population years of the six year study. In population year 1 (2007-08) we had only a small number of fishers radiocollared during the last few months of that period (Tables 9, Fig. 10) and camera trap images for the entire population year 2012-2013 were not available due to the conclusion of SNAMP Fisher field work. During the central four year period we captured and radiocollared 101 individual fishers (57 females and 44 males) on 258 occasions during 9732 trap-nights between December 2007 and March 2012. Resighting efforts, both by camera and live traps, varied by subregion and, to a lesser extent, year (Table 25). Camera traps accounted for 86% of 1421 total radio-marked fisher detections, with live trap recaptures providing 201 sightings.

![Table 25.—Summary data on camera and live trap activities within 4 fall-winter camera survey years (October 16 to March 15) in the Bass Lake District, Sierra National Forest Study area, October 2008 to March 2012.](image)

Mean overall abundance across all subregions ranged from 48.2 individuals in Year 2 to 61.8 individuals in Year 4. Variation was at least partly related to differences in area surveyed among years (Table 26). Estimates of areas sampled were generally consistent within subregions among years (Table 26). The increase in area surveyed in Subregion 1 in fall-winter 2009-10 was due to a program that extended camera surveys north into the Yosemite South region of Yosemite National Park (Fig. 3) in winter 2010. In fall-winter 2011-12, research effort was expanded in the Grizzly and Jackass subregion when non-collared fishers were detected in areas that had not been surveyed previously. Mean annual population density for the three subregions ranged from 0.072 to 0.097 fishers/km² (Fig. 19).
Subregion 1 had consistently high average densities (0.073-0.125 individuals/km²), with an increasing trend across the last three years of the period (Table 26, Fig. 19). Subregion 3 had initial low density (0.056 ± 0.005 individuals/km²), but gradually increased by the end of the period (0.106 ± 0.005 individuals/km²). Subregion 2 showed no particular trend, and average densities varied across seasons between 0.066 (fall-winter 2009-10) and 0.092 individuals/km² (fall-winter 2010-11). Temporally, mean population density was lowest in fall-winter 2009-10 at 0.075 ± SE 0.006 individuals/km², and increased thereafter to a high of 0.097 ± SE 0.008 in fall-winter 2011-12 (Fig. 19).

### Table 26—Mark-resight estimates of population size for three subregions in 4 Fall-Winter survey years (October 16 to March 15) in the Bass Lake District, Sierra National Forest, October 2008 to March 2012.

<table>
<thead>
<tr>
<th>Subregion, Year</th>
<th>n</th>
<th>95% C.I.</th>
<th>Densitya</th>
<th>Density rangeb</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Subregion 1: Nelder Grove, Sugar Pine, Miami Mtn</strong></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>2008-09</td>
<td>27.9</td>
<td>23.6-32.2</td>
<td>0.125</td>
<td>0.106-0.144</td>
</tr>
<tr>
<td>2009-10</td>
<td>22.3</td>
<td>19.0-25.6</td>
<td>0.073</td>
<td>0.062-0.083</td>
</tr>
<tr>
<td>2010-11</td>
<td>19.1</td>
<td>16.3-22.0</td>
<td>0.089</td>
<td>0.076-0.103</td>
</tr>
<tr>
<td>2011-12</td>
<td>23.2</td>
<td>20.2-26.2</td>
<td>0.103</td>
<td>0.090-0.117</td>
</tr>
<tr>
<td><strong>Subregion 2: Central Camp, Whisky Ridge, Grizzly, Jackass</strong></td>
<td></td>
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<tr>
<td>2008-09</td>
<td>18.8</td>
<td>10.5-21.2</td>
<td>0.070</td>
<td>0.044-0.097</td>
</tr>
<tr>
<td>2009-10</td>
<td>16.3</td>
<td>10.4-21.4</td>
<td>0.066</td>
<td>0.037-0.094</td>
</tr>
<tr>
<td>2010-11</td>
<td>22.5</td>
<td>15.4-24.5</td>
<td>0.092</td>
<td>0.066-0.118</td>
</tr>
<tr>
<td>2011-12</td>
<td>24.6</td>
<td>17.8-26.5</td>
<td>0.080</td>
<td>0.062-0.099</td>
</tr>
<tr>
<td><strong>Subregion 3: Chowchilla Mtn, Rush Creek, Sweetwater</strong></td>
<td></td>
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</tr>
<tr>
<td>2008-09</td>
<td>7.2</td>
<td>5.8-8.6</td>
<td>0.056</td>
<td>0.045-0.067</td>
</tr>
<tr>
<td>2009-10</td>
<td>9.7</td>
<td>8.7-10.6</td>
<td>0.086</td>
<td>0.078-0.095</td>
</tr>
<tr>
<td>2010-11</td>
<td>10.0</td>
<td>8.8-11.3</td>
<td>0.074</td>
<td>0.065-0.083</td>
</tr>
<tr>
<td>2011-12</td>
<td>14.0</td>
<td>12.8-15.3</td>
<td>0.106</td>
<td>0.096-0.115</td>
</tr>
</tbody>
</table>

*Population size (n) divided by the estimated sample area for the subregion in the Fall-Winter camera year, included in Table 1.*

*Calculated based on the lower and upper values of the 95% C.I., divided by the estimate of the sampled area provided in Table 1.*

*Sum of the year- and subregion-specific estimates of population size from the mark-resight analyses.*

**Figure 19.** Mean density (± SE) of fishers in three subregions during 4 fall-winter camera survey years (a), and change in mean density (± SE) of fishers during 4 fall-winter camera survey years (b).
population density was consistently high in the last 2 years of the study across all subregions (0.089 – 0.106 individuals/km²).

Dispersal Behavior and Movements

The combination of field data and genetic data allowed for the possibility of assessing dispersal for 33 female and 25 male fishers that were captured as juveniles \((n = 53)\), or young subadults \((n = 8; \leq 18\) months old) (Table 27). Fifteen of those fishers (25.8%) either died, disappeared, or were caught too late in the year to define a juvenile home range (Table 27). Dispersal was assessed for 43 (74%) of the 58 animals, based on identification of likely natal areas from either field data or genetic-based maternity assignments (Table 27).

Considering data for dispersal using either field or genetic-based natal area determination and based on Euclidean distances, male fishers tended to disperse longer distances than females, but the difference was not significant (Table 28). The longest Euclidean distance dispersal for a female fisher was 24.53 km, compared to 36.17 km for a male fisher, however the large range of dispersal distances for both sexes precludes precise statistical comparison.

Euclidean dispersal movements often originated from within the Key Watershed focal study area, but other Subregions of the study area produced dispersing animals as well (Fig. 20). There was no clear patterning with regards directionality of dispersal, except perhaps the general northwest to southeasterly orientation associated with the Sierra Nevada range (Fig. 20).

One male fisher immigrated into the SNAMP Fisher Study area in the Bass Lake Ranger District from south of Shaver Lake within the High Sierra District. This fisher, KRFP ID M38 (SNAMP ID M47), was originally captured and marked with a PIT tag on the Kings River Fisher Project in December 2010. M38 was recaptured by the KRFP researchers in the KRFP study area in February 2012, when he was released without a radiocollar due to an abrasion from his original radiocollar. M38 was captured 13 months later in March 2013 within the SNAMP study area. Although his Euclidean distance-based dispersal track was estimated at \(\approx 36\) km, it is more likely that his dispersal track was more circuitous, and in the range of 67-69 km (Fig. 21).

Dispersal movements predicted by Least Cost Movement (LCP) analyses over landscape features considered restrictive to fishers produced longer mean dispersal distances than Euclidean paths (Table 29, Fig. 22). Nevertheless, and in accordance with data from Euclidean distances, there was no evidence for a significant gender-bias in LCP predicted dispersal tracks (Table 29).
Table 27. Review of information on juvenile or subadult fishers captured on the SNAMP Fisher study for which dispersal assessments were possible from field data, maternal assignments from genetic analyses, or from either source.

<table>
<thead>
<tr>
<th>Maternal year, Gender</th>
<th>n</th>
<th>Dispersal not assessed</th>
<th>Dispersal assessed</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Died</td>
<td>Missing, disappear</td>
<td>Late capture</td>
</tr>
<tr>
<td>2007</td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>Female</td>
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<td>2008</td>
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<tr>
<td>Female</td>
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<td>All years</td>
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<tr>
<td>Female</td>
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<tr>
<td>Male</td>
<td>25</td>
<td>3</td>
<td></td>
<td>3</td>
</tr>
</tbody>
</table>

- Dispersal was not assessed if the animals died before <18 months old, when they were missing and not recaptured, or if they were captured after mid-January (<10 months old).
- Animals for which home ranges allowed identification of likely natal areas (juvenile home ranges), as well as post dispersal home ranges as subadults or adults.
- Animals for which maternal assignments were made using DNA analyses; natal areas were based on maternal home ranges.
- Animals for which dispersal could be assessed using both field data (juvenile home ranges) and maternal assignments from genetic analyses.
- Number of juveniles/young subadults for which dispersal could be assessed using either field-based home range data, or genetic-based maternal assignments.
### Table 28. Estimates of mean Euclidean distances moved by dispersing fishers ≤18 months old on the SNAMP Fisher study. Dispersal was estimated by (1) distance between centroids for juvenile home ranges and subadult or adult home ranges, (2) distance between centroids for maternal home ranges (based on genetic-based maternity assignments) and adult or last known home ranges, or (3) distance between either juvenile home range centroids (fishers without maternity assignments) or maternal home range centroids and adult or last known home ranges.

<table>
<thead>
<tr>
<th>Dispersal, Gender</th>
<th>n</th>
<th>Mean distance (SE)</th>
<th>Range</th>
<th>t-test contrasts&lt;sup&gt;a&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Juvenile to adult home range (field data)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>20</td>
<td>4.89 (1.36)</td>
<td>0.24-22.26</td>
<td>t&lt;sub&gt;35&lt;/sub&gt; = 1.35, P = 0.19</td>
</tr>
<tr>
<td>Male</td>
<td>17</td>
<td>8.48 (2.39)</td>
<td>0.94-36.17</td>
<td></td>
</tr>
<tr>
<td>2. Maternal to adult home range (genetics)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>20</td>
<td>5.00 (1.21)</td>
<td>0.46-24.53</td>
<td>t&lt;sub&gt;34&lt;/sub&gt; = 1.32, P = 0.20</td>
</tr>
<tr>
<td>Male</td>
<td>16</td>
<td>7.44 (1.41)</td>
<td>1.82-21.20</td>
<td></td>
</tr>
<tr>
<td>3. Juvenile or Maternal to adult home range (combined field and genetics)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>24</td>
<td>5.76 (1.26)</td>
<td>0.52-24.53</td>
<td>t&lt;sub&gt;41&lt;/sub&gt; = 1.67, P = 0.10</td>
</tr>
<tr>
<td>Male</td>
<td>19</td>
<td>9.81 (2.22)</td>
<td>0.94-36.17</td>
<td></td>
</tr>
</tbody>
</table>

<sup>a</sup> Unequal variance t-tests.

### Table 29. Mean Least Cost Movement paths (LCP) developed to evaluate dispersal by fishers ≤18 months old in the SNAMP Fisher Study area. LCP tracks were estimated for (1) dispersal between centroids for juvenile home ranges and subadult or adult home ranges, (2) for dispersal between centroids for maternal home ranges (based on genetic-based maternity assignments) and adult or last known home ranges, and for (3) dispersal between either juvenile home range centroids (fishers without maternity assignments) or maternal home range centroids and adult or last known home ranges.

<table>
<thead>
<tr>
<th>Dispersal, Gender</th>
<th>N</th>
<th>Mean Least Cost path (SE)</th>
<th>Range</th>
<th>t-test contrasts&lt;sup&gt;a&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Juvenile to adult home range</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>20</td>
<td>7.53 (2.39)</td>
<td>0.47-44.09</td>
<td>t&lt;sub&gt;35&lt;/sub&gt; = 0.90, P = 0.38</td>
</tr>
<tr>
<td>Male</td>
<td>17</td>
<td>11.63 (4.11)</td>
<td>1.03-69.82</td>
<td></td>
</tr>
<tr>
<td>2. Maternal to Adult home range</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>20</td>
<td>6.95 (1.62)</td>
<td>0.47-34.06</td>
<td>t&lt;sub&gt;34&lt;/sub&gt; = 1.07, P = 0.29</td>
</tr>
<tr>
<td>Male</td>
<td>16</td>
<td>9.52 (1.77)</td>
<td>1.85-26.15</td>
<td></td>
</tr>
<tr>
<td>3. Juvenile or Maternal to Adult home range</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>24</td>
<td>8.76 (2.11)</td>
<td>0.47-44.09</td>
<td>t&lt;sub&gt;41&lt;/sub&gt; = 1.16, P = 0.25</td>
</tr>
<tr>
<td>Male</td>
<td>19</td>
<td>13.48 (3.71)</td>
<td>1.03-69.82</td>
<td></td>
</tr>
</tbody>
</table>

<sup>a</sup> Unequal variance t-tests.
Figure 20. Plot of Euclidean distance dispersal movements for juvenile and young subadult female and males fishers within the SNAMP Fisher Project study area. NOTE: plot excludes the dispersal track for fisher M47 (KRFP fisher that dispersed north from south of Shaver Lake (High Sierra District) to near “Central Camp” in the Bass Lake District.
Figure 21. Plot of the Euclidean dispersal track for KRFP Fisher M38 from his last live-trap position in February 2013 to his postdispersal home range centroid near Central Camp within the SNAMP Fisher Study area. The plot also includes the estimated Least Cost Movement path for the M38 dispersal event, which we consider more realistic given the very steep, and vertical cliffs typical of the San Joaquin River canyon east of Redinger Lake.
Figure 22. Estimated Least Cost Movement paths for young fishers (≤ 18 months) that were assessed for dispersal in the SNAMP Fisher study area from 2008 to 2013. Least cost movement path were developed as a more realistic way to assess fisher movements given that a number of landscape and habitat features are known avoided or restrictive to fishers as part of their overall natural history.
Young female fishers appeared somewhat more philopatric than male fishers, based on the proportion that moved less than the mean diameter of the annual home range for adult females in the study population (Fig. 23). The pattern was not significantly different however (Pearson $\chi^2 = 1.12$, $P = 0.29$). Also, there was no statistical evidence that male fishers dispersed farther than female fishers when dispersal distances were scored based on two levels of philopatry and two levels of dispersal (Euclidean distance Likelihood ratio $\chi^2 = 3.89$, $P = 0.27$; Fig. 24). The same analysis using LCP distances visualized in Fig. 22 was also nonsignificant (LCP Likelihood ratio $\chi^2 = 1.87$, $P = 0.60$; Fig. 22). However, it was noteworthy from a genetic perspective that 67% of females were philopatric, compared to about 45% of young males (Fig. 24; Table 30).

![Figure 23](image1.png)
**Figure 23.** Proportion of female and male fishers that dispersed less than the diameter of the mean annual home range for adult female fishers (22.99 km; diameter = 5.401 km) in the SNAMP Fisher study area. Fishers that dispersed <5.4 km were considered as exhibiting philopatry, whereas those that moved >5.4 km were considered dispersers.

![Figure 24](image2.png)
**Figure 24.** Plot of four different categories of dispersal distances for male and female fishers. Categories are based on dispersal distances of less than 0.5, 1.0, 1.5 and >2 times the mean diameter of the annual adult female home range in the study area. *Illustration 12 (right):* female fisher on the move.
Information on timing of dispersal is important for understanding whether juveniles captured in fall and winter were resident (born near the area of capture and initial locations), or if they originated elsewhere. Five dispersal events (20.8%) were initiated by juvenile fishers during fall to mid-winter (Table 30). Fourteen (58.3%) were initiated during the late winter to mid-spring time frame, and five started in late spring or summer (Table 30). Thus, nearly 80% of natal dispersal events occurred after February 5 when fishers were 11-13 months old.

### Table 30. Information on periods of the year when juvenile fishers initiated transitional movements as part of natal dispersal, and numbers of young fishers (<18 months old) that were philopatric, or that dispersed more than 1 diameter of the mean adult female home range (22.99 km; diameter = 5.41).

<table>
<thead>
<tr>
<th>Dispersal parameter</th>
<th>Female</th>
<th>Male</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Timing of dispersal initiation</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fall to mid-winter (Oct 15 - Feb 4)</td>
<td>2</td>
<td>3</td>
<td>5</td>
</tr>
<tr>
<td>Late winter to mid-spring (Feb 5 - May 5)</td>
<td>7</td>
<td>7</td>
<td>14</td>
</tr>
<tr>
<td>Late spring or summer (May 6 - Sep 20)</td>
<td>2</td>
<td>3</td>
<td>5</td>
</tr>
<tr>
<td><strong>Dispersal distance</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Short distance philopatric (&lt; 2.7 km)</td>
<td>10</td>
<td>3</td>
<td>13</td>
</tr>
<tr>
<td>Philopatric (2.7 km to 5.4 km)</td>
<td>6</td>
<td>6</td>
<td>12</td>
</tr>
<tr>
<td>Medium distance dispersal (5.4-10.8 km)</td>
<td>5</td>
<td>6</td>
<td>11</td>
</tr>
<tr>
<td>Long distance dispersal (&gt;10.8 km)</td>
<td>3</td>
<td>5</td>
<td>8</td>
</tr>
</tbody>
</table>

* Data on initiation of dispersal were for a smaller subset of juveniles (n = 22) that made transitional movement that were apparent based on aerial telemetry locations and home range models
* <0.5X diameter of mean adult female home range
* 0.5-1X diameter of mean adult female home range
* 1-2X diameter of mean adult female home range
* >2X diameter of mean adult female home range

### Home Range Dynamics

We obtained and processed ≈ 35,365 location records from all sources (Table 3; Fig. 25) from October 2007 to December 2013. The location dataset was screened for errors and duplicates (same day, same animal, <8 hrs apart in time), after which approx. 32,370 of the locations were retained for detailed analyses of movements (home ranges) for 109 different fishers. Most of the location records were from aerial radiotelemetry (88%), which were less accurate than other types of locations in the database (Table 3).

Annual 95% fixed-kernel home range areas differed by gender for all age classes (Fig. 26), with mean values ranging from 20.98 km² for juvenile females to 86.18 km² for adult males (Table 31). Male fishers are larger in body mass and morphological size than females (Powell 1993), and size dimorphism was already evident between genders when juvenile fishers were captured and
measured in October and November (7-8 months old) (Focused Research Topic 1; Table 36). Body size is closely related to home range size in mammals (Swihart et al. 1988), which helps explain the larger size of annual home ranges for all age classes of male fishers in this study (Table 31, Figure 21).

Although fishers have previously been described as exhibiting intrasexual territoriality (Powell et al. 2003), we noted considerable overlap between the annual home ranges of adults of the same sex (Fig. 27). Annual home ranges

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**Figure 25.** Distribution of location records used to evaluate movements and home range size for fishers that were radiocollared and monitored for survival from Dec 2007 to Dec 2013. **Illustration 13:** juvenile female fisher “bycatch” in a Bobcat Trap in 2011.

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**Figure 26.** Mean annual home range size (SE bars) for male and female fishers from the SNAMP Fisher Project. More details, including size of core use areas are provided in Table 10.
overlapped extensively among neighboring females, but overlap declined at the 70 or 60 percent fixed-kernel isopleths. These results suggest that female fishers maintain exclusive intra-sexual territories in their core use areas. Adult males move widely during the breeding season, resulting in widely overlapping use areas during spring (Popescu et al. 2014).

Home range sizes for fishers varied seasonally (Table 32; Fig. 28). Adult female home ranges were smallest during the spring, and reproducing females have smaller home ranges than non-reproducing females during this time when mothers are constrained to the den area and provisioning kits at den structures. Home ranges of denning females were smaller than non-reproductive female home ranges through the summer, before offspring become independent. Size of seasonal home ranges among adult

Figure 27. Annual home ranges for female and male fishers. The plot illustrates space use behavior where (1) the larger home ranges of males (Fig. 12) overlap home ranges of all females in the population, and (2) high overlap in space use among resident females at the 95% home range isopleth.
male fishers was smallest during the summer and largest during the spring, reflecting wide movement associated with mating during March and April (Table 32). In contrast, seasonal home ranges of subadult males (likely non-reproductive) were largest during winter and relatively stable during spring, summer, and fall (Table 32).

Excluding the spring season home range for adult males, home range size was largest for all age and sex classes of fishers during winter, likely due to scarcity of prey.

| Table 31. Mean annual and core use home range sizes (km² ± SE) for radio-tracked fishers at the SNAMP site, December 2007 to March 2013 |
|---|---|---|
| Age/Sex | N | Annual | Core use |
| **Juvenile (<12 months)** | | | |
| Female | 10 | 20.98 ± 3.76 | 6.59 ± 1.18 |
| Male | 4 | 35.68 ± 3.83 | 11.86 ± 1.02 |
| **Subadult (12 to 23 months)** | | | |
| Female | 22 | 25.15 ± 3.20 | 8.59 ± 1.09 |
| Male | 18 | 51.85 ± 4.76 | 18.15 ± 1.66 |
| **Adult (≥24 months)** | | | |
| Female | 56 | 22.93 ± 1.36 | 7.78 ± 0.59 |
| Male | 40 | 86.18 ± 4.87 | 30.23 ± 1.78 |

*a Annual home ranges were estimated for fishers for which locations were available for ≥6 months between Apr 1 and Mar 31; number of location records used for annual home models ranged from 77 to 326.

*b Core use home range estimated using methods described in Seaman and Powell (1990) and Bingham and Noon (1997); ~2/3 of the core use areas were the 60% isopleth, the remainder were the 70% isopleth.

*c Home ranges for juvenile fishers monitored ≥5 months in Oct to Mar period; excludes fishers that exhibited dispersal movement behavior.

| Table 32. Mean home range sizes (95% Fix Kernel; km² ± SE) for fishers during four seasons* of the year. Data for animals radio-collared on the Sierra National Forest, CA from December 2007 to March 2013 |
|---|---|---|---|---|---|
| Age | n | Spring | n | Summer | n | Fall | n | Winter |
| **Juvenile** | | | | | | | | |
| Female | | | | | | | | |
| Male | | | | | | | | |
| **Subadult** | | | | | | | | |
| Female | 17 | 20.78 ± 3.59 | 21 | 17.19 ± 2.73 | 21 | 15.61 ± 1.41 | 22 | 22.87 ± 2.28 |
| Male | 12 | 36.48 ± 4.26 | 13 | 30.49 ± 3.31 | 14 | 33.51 ± 3.15 | 19 | 58.90 ± 8.61 |
| **Adult** | | | | | | | | |
| Female | 59 | 8.18 ± 0.64 | 43 | 14.92 ± 1.02 | 50 | 19.70 ± 1.37 | 50 | 21.77 ± 1.28 |
| Male | 35 | 72.07 ± 6.39 | 34 | 39.49 ± 2.75 | 32 | 49.25 ± 4.04 | 37 | 68.91 ± 4.73 |

*a Seasons were Spring: 21 Mar to 20 Jun; Summer: 21 Jun to 20 Sep; Fall 21: 21 Sep to 20 Dec; Winter: 21 Dec to 20 Mar.

*b Excludes home ranges for fishers that exhibited movements associated with dispersal.

*c Includes home ranges for adult females that denned during the spring period of each year and excludes non-dennning adults.
Figure 28. Plot illustrating size of the mean seasonal home range size (SE bars) for female (a) and male (b) fishers from the SNAMP Fisher Project. NOTE: the scale is different for the two plots, which helps to illustrate similarities in habitat use patterns for the different age and sex groups. More details are provided in Table 32. (Illustration 14: adult female fisher departing a black oak den tree in spring 2011)

SNAMP Fisher Management Indicators

Management indicator 1 (occupancy/presence of fisher detections in 1-km² grids within the Key Watersheds) ranged from a low of 53% in 2012-13 to a high of 76% in 2011-12 (Table 33). The index of fisher activity developed for Management Indicator 1 indicated that the estimated detection rate (detections/100 camera survey days) was highest in 2012-13 and lowest in 2010-11. It was unusual that the detection rate was highest in the same year that naïve occupancy was lowest (Table 33). Camera year 2012-13 was atypical in that many grids in the Key Watershed were surveyed during summer when detection rates are significantly lower (Popescu et al. 2014). It was therefore possible that the low occupancy for 2012-13 compared to most other years was related to timing of surveys.

Spatially, the distribution of fisher active grids changed among years (Fig. 29). Visually, there was the appearance that fisher detections were somewhat reduced in the Cedar Valley Project region of the Key Watersheds (center-south; Figs. 4, 29) immediately after project implementation. There were also changes in fisher detections in the northeast region of the Key Watersheds, which may have been associated with mastication and other activities associated with the Fish Camp Project (Figs. 4, 29).
Visual comparisons of presence/absence are not appropriate for detecting patterns or trend in occupancy (persistence, extinction, recolonization) related to forest management projects, however. Detailed, multi-year occupancy modeling analyses are underway, which include the proportion of each grid treated in each of six years by different forest management activities. Models also include other covariates potentially important for understanding detection histories and habitat use (e.g., season, elevation).

**Table 33.** Management indicator for fisher activity in the Key Watershed focal study area, based on the number of 1 km² grids in which fishers were detected during annual surveys with camera traps. Camera trap surveys were completed in each of six "camera years" (≈ Oct 15 to Oct 14) using our standard protocol.

<table>
<thead>
<tr>
<th>Camera year</th>
<th>Grids surveyed</th>
<th>Grids with fisher detections</th>
<th>Naive occupancy</th>
<th>Fisher detections per 100 survey days&lt;sup&gt;c&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td>2007-08</td>
<td>122</td>
<td>71</td>
<td>0.582</td>
<td>11.4</td>
</tr>
<tr>
<td>2008-09</td>
<td>129</td>
<td>75</td>
<td>0.581</td>
<td>13.2</td>
</tr>
<tr>
<td>2009-10</td>
<td>127</td>
<td>75</td>
<td>0.591</td>
<td>15.2</td>
</tr>
<tr>
<td>2010-11</td>
<td>125</td>
<td>82</td>
<td>0.656</td>
<td>10.5</td>
</tr>
<tr>
<td>2011-12</td>
<td>128</td>
<td>98</td>
<td>0.766</td>
<td>14.2</td>
</tr>
<tr>
<td>2012-13</td>
<td>133</td>
<td>70</td>
<td>0.526</td>
<td>18.6</td>
</tr>
</tbody>
</table>

<sup>a</sup> Camera trap surveys were completed in each of six "camera years" (≈ Oct 15 to Oct 14) using a standard protocol *(see report)*

<sup>b</sup> Number grids with fisher detections divided by the total number of grids surveyed; occupancy rate is not corrected for a survey-specific probability of detection < 1.0.

<sup>c</sup> Estimated as the number of functional camera survey days with fisher detections, but excluded camera days for grids with no fisher detections.
Figure 29. GIS plots illustrating change in patterns of occupancy for Management Indicator 1 for six camera years (Table 33 for details).
As an extension to Management Indicator 1, we also created an overall index of fisher activity for each grid based on mean number days in each camera survey year with fisher detections and the proportion of survey years with fisher detections (Fig. 30). The index illustrates that fisher activity was consistently high in the center and northwest region of the Key Watersheds, and lowest from Cedar Valley southward (Fig. 30).

Management Indicator 2 identified an average of 5.0 subadult or adult females and 2.0 subadult or adult males using the Key Watershed focal study area across all years (Table 34). For both sexes combined, the number of resident fishers using the focal study area ranged from 6.2 to 7.7, and the variation among years was small (Table 34, Fig. 31).

<table>
<thead>
<tr>
<th>Year</th>
<th>Females</th>
<th>Males</th>
<th>Both genders</th>
</tr>
</thead>
<tbody>
<tr>
<td>2007-08b</td>
<td>5.6</td>
<td>2.1</td>
<td>7.7</td>
</tr>
<tr>
<td>2008-09</td>
<td>6.1</td>
<td>1.4</td>
<td>7.5</td>
</tr>
<tr>
<td>2009-10</td>
<td>4.1</td>
<td>2.1</td>
<td>6.2</td>
</tr>
<tr>
<td>2010-11</td>
<td>4.0</td>
<td>2.9</td>
<td>6.9</td>
</tr>
<tr>
<td>2011-12</td>
<td>5.0</td>
<td>1.7</td>
<td>6.7</td>
</tr>
</tbody>
</table>

*Numbers are based on the sum of the proportion of each individual fishers’ 95% fixed kernel home range included within the Key Watershed region.*

*Because of the limited number of fishers radiocollared during the first project year (n = 7, before March 31, 2008) it was not informative to calculate this Management Indicator in that year.*

**Illustration 15:** Male fisher at base of a den tree in spring 2010.

**Figure 30.** Index of fisher activity from repeat camera surveys completed in the Key Watersheds focal study area. The Index was calculated as the mean no. of days with fisher activity for years that the grid was surveyed/1 + proportion of surveyed years with fishers detections in the grid.
Figure 31. Estimated number of subadult and adult female fishers (resident females) with home ranges including portions of the Key Watersheds focal study area. Polylines are individual 95% fixed kernel home ranges based on location records during the Sep 1 to Mar 15 period during each population year (Apr 1 to Mar 31).
The original Management Indicator 3 was recast to estimate survival for adult female fishers for a sequence of 2-year groups of demographic data (data combined for Kaplan-Meier models of survival). For the first 2-year group, we included data for the small number of fishers \( n=7 \) that were captured and radiocollared from mid December 2007 to March 31, 2008. We further summarized data on Juvenile and subadult female survival, and calculated point estimates of weaning reproduction and weaning litter size for each of the five 2-year groups (Table 24). Expanded Management Indicator 3 identified that adult female survival ranged from a low of 0.69 in Year group 3 to a high of 0.86 in Year group 4 (Table 35). Corresponding data on survival for juvenile and subadult females and data on reproduction identified that relatively low levels of survival and reproduction suggested the population was in decline \( (\lambda < 1.0) \) population between 2008 and 2010, stable between 2010 and 2012, and increasing by 3-4%/year during 2012 to 2014 (Table 35). However the fact that 95% CI for \( \lambda \) overlapped 1.0 in all years indicates that these values should be interpreted carefully.

<table>
<thead>
<tr>
<th>Year group, Demographic rate</th>
<th>Juvenile</th>
<th>Subadult</th>
<th>Adult</th>
<th>( \lambda^a )</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. 2007-08, 2008-09, 2009-10</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Survival, ( s(t) )</td>
<td>0.76 (0.53-0.99)</td>
<td>0.47 (0.28-0.67)</td>
<td>0.81 (0.66-0.96)</td>
<td>0.87 (0.65-1.08)</td>
</tr>
<tr>
<td>2. 2009-10, 2010-11</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Survival, ( s(t) )</td>
<td>0.8 (0.59-1.01)</td>
<td>0.67 (0.42-0.92)</td>
<td>0.70 (0.54-0.86)</td>
<td>0.88 (0.63-1.12)</td>
</tr>
<tr>
<td>3. 2010-11, 2011-12</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Survival, ( s(t) )</td>
<td>0.74 (0.54-0.94)</td>
<td>0.89 (0.71-1.07)</td>
<td>0.69 (0.53-0.86)</td>
<td>1.00 (0.77-1.22)</td>
</tr>
<tr>
<td>4. 2011-12, 2012-13</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Survival, ( s(t) )</td>
<td>0.80 (0.55-1.05)</td>
<td>0.73 (0.52-0.94)</td>
<td>0.86 (0.71-1.00)</td>
<td>1.04 (0.81-1.26)</td>
</tr>
<tr>
<td>5. 2012-13, 2013-14</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Survival, ( s(t) )</td>
<td>1.0</td>
<td>0.73 (0.48-0.98)</td>
<td>0.74 (0.56-0.93)</td>
<td>1.03 (0.77-1.22)</td>
</tr>
</tbody>
</table>

\(^a\) Population growth rate was estimated using the demographic parameters developed for each Year group in a Leslie-Matrix model formulation described previously. The range in values for \( \lambda \) was based on the 95% CIs for survival for each age class when producing fertility (\( F_i \)) rates using the equation \( F_i = b_i P_i \), where \( b(i) \) was fecundity, and \( P_i \) was the age-specific survival rate (see Table 23).

\(^b\) Year group 1 includes information for a small number fishers monitored for survival from late December to March 2008. All other year groups include two population years of data.
**Fisher response to fuel management**

Management disturbances and wildfire

Our analyses of FACTS and other extractive and restorative management activities revealed that the estimated area of forest disturbing activities that occurred in the study area was highest for restorative fuel reduction, moderate for logging, and lowest for managed burning and natural or human caused wildfires (Table 36). We estimated that there was an annual average 1.9% (SD 0.70) of the study area treated for restorative fuel reduction each year from 2002-03 to 2012-13, and 20.6% of the study area was disturbed by these activities in all 11 years. We estimated that there was an annual average of 1.1% (SD 0.70) of the study area with extractive fuel reduction each year from 2002-03 to 2012-13, and an estimated 12.1% of the study area was disturbed by logging in all 11 years. We estimated that there was an annual average of 0.25% (SD 0.28) of the study area with managed burning each year from 2002-03 to 2012-13, and an estimated 2.8% of the study area was disturbed by managed burns in all 11 years. Also, the combined area disturbed by all 3 management activities averaged 36.3 km²/year from 2002-03 to 2012-13, which represented an annual disturbance of 3.2%/year from SPLATS in the overall study area. Our fire variables included managed burns+forest fires, and we estimated that the annual average portion of the study area with managed burns+wildfires was 0.56%/year (SD, 0.83) from 2002-03 to 2012-13, and 6.2% of the overall study area was exposed to those disturbances in the 11 years. Also, in the 44 years from 1957 to 2001, we estimated that 130.2 km² (11.6%) of the overall study area was burned by wildfires.

**Multi-season occupancy**

The mean detection probability for fishers per 8-10 day survey period in the 361 multi-season survey grids was 0.31 (95% CI: 0.28, 0.37). Naïve initial occupancy among the multi-season grids was 0.66, whereas our modeled estimate for initial occupancy averaged across survey sites was 0.75 (95% CI: 0.59, 0.87). Mean annual persistence (1-extinction) was 0.87 (95% CI: 0.82, 0.91), whereas the annual colonization rate was 0.34 (95% CI 0.28, 0.42).

Our multi-season occupancy modeling identified a single best model for local colonization that included the intercept only (Table 37). Covariates hazfuels.5, log.5, and burn.1.50 were included in 3 lower ranking colonization models with support, but the relative importance for each individual variable was≤ 0.35. We therefore fit an intercept-only colonization component in our subsequent evaluation of extinction covariates.
Table 36. Estimates of the areas (km²) disturbed by logging activities, fuel reduction treatments, and managed burns in the Bass Lake District, Sierra National Forest, and southwestern Yosemite National Park in 11 camera survey years (Oct 15 to Oct 14) from 2002 to 2013 as well as wildfire activity in 5-year periods from 1957 through 2001.

<table>
<thead>
<tr>
<th>5 yr period or survey year</th>
<th>Restorative fuel reduction Area Study area (%)</th>
<th>Extractive fuel reduction Area Study area (%)</th>
<th>Managed burns + forest fire Area Study area (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1957 to 1961</td>
<td>36.40</td>
<td>7.28</td>
<td></td>
</tr>
<tr>
<td>1962 to 1966</td>
<td>5.30</td>
<td>1.06</td>
<td></td>
</tr>
<tr>
<td>1967 to 1971</td>
<td>6.05</td>
<td>1.21</td>
<td></td>
</tr>
<tr>
<td>1972 to 1976</td>
<td>3.43</td>
<td>0.69</td>
<td></td>
</tr>
<tr>
<td>1977 to 1981</td>
<td>4.65</td>
<td>0.93</td>
<td></td>
</tr>
<tr>
<td>1982 to 1986</td>
<td>11.46</td>
<td>2.29</td>
<td></td>
</tr>
<tr>
<td>1987 to 1991</td>
<td>41.91</td>
<td>8.38</td>
<td></td>
</tr>
<tr>
<td>1992 to 1996</td>
<td>0.99</td>
<td>0.20</td>
<td></td>
</tr>
<tr>
<td>1997 to 2001</td>
<td>20.05</td>
<td>4.01</td>
<td></td>
</tr>
<tr>
<td>2002-03</td>
<td>23.7</td>
<td>2.10</td>
<td>11.6</td>
</tr>
<tr>
<td>2003-04</td>
<td>13.7</td>
<td>1.21</td>
<td>2.9</td>
</tr>
<tr>
<td>2004-05</td>
<td>13</td>
<td>1.15</td>
<td>7</td>
</tr>
<tr>
<td>2005-06</td>
<td>26</td>
<td>2.31</td>
<td>6.8</td>
</tr>
<tr>
<td>2006-07</td>
<td>34.5</td>
<td>3.06</td>
<td>13.1</td>
</tr>
<tr>
<td>2007-08</td>
<td>15.8</td>
<td>1.40</td>
<td>2.1</td>
</tr>
<tr>
<td>2008-09</td>
<td>29.1</td>
<td>2.58</td>
<td>11.4</td>
</tr>
<tr>
<td>2009-10</td>
<td>27.4</td>
<td>2.43</td>
<td>27</td>
</tr>
<tr>
<td>2010-11</td>
<td>12.4</td>
<td>1.10</td>
<td>13.8</td>
</tr>
<tr>
<td>2011-12</td>
<td>12.6</td>
<td>1.12</td>
<td>24.3</td>
</tr>
<tr>
<td>2012-13</td>
<td>23.9</td>
<td>2.12</td>
<td>16.1</td>
</tr>
<tr>
<td>Total area</td>
<td>232.1</td>
<td>136.1</td>
<td>69.87</td>
</tr>
</tbody>
</table>

*a Areas of disturbance were derived from FACTs data, private timber harvest data, and Sierra National Forest and Yosemite National Park databases (Table 1). The overall study area was 1125.6 km² (Fig. 1), which was used to estimate percent disturbance of each type within the study area.

*b Totals for 1957 to 2001, and 2002-03 to 2012-13, respectively.

*c Means for 1957 to 2001 (44 years), and 2002-03 to 2012-13 (11 years), respectively.

Our multi-season models evaluating local extinction identified a single top model including covariate hazfuels.5 only (hazfuel.5 relative importance = 0.98) (Table 37). There were 2 models with support that included the covariates log.5 and burn.1.50, but the individual relative importance metrics for both were low. We found that fisher persistence (1 - extinction) was negatively associated with
hazfuels.5; probability of persistence decreased by 27% as the proportion of the grid treated for cumulative restorative fuel reduction increased from 0 (occupancy = 0.89, 95%CI 0.85, 0.92) to 1.0 (occupancy = 0.65, 95%CI 0.46, 0.81).

**Table 37.** Candidate models for multi-season occupancy evaluations of local patch extinction and colonization for camera trap surveys for fishers in the Bass Lake District, and southwestern Yosemite National Park, California from Oct 2007 to Oct 2014.

<table>
<thead>
<tr>
<th>Model, covariate</th>
<th>AIC</th>
<th>ΔAIC</th>
<th>AICwt</th>
<th>Cumulative AICwt</th>
<th>Covariate importance</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Colonization</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>intercept only</td>
<td>4211.96</td>
<td>0.00</td>
<td>0.34</td>
<td>0.34</td>
<td></td>
</tr>
<tr>
<td>hazfuels.5</td>
<td>4213.15</td>
<td>1.19</td>
<td>0.19</td>
<td>0.53</td>
<td>0.35</td>
</tr>
<tr>
<td>log.5</td>
<td>4213.87</td>
<td>1.91</td>
<td>0.13</td>
<td>0.66</td>
<td>0.27</td>
</tr>
<tr>
<td>burn.1.50</td>
<td>4213.95</td>
<td>2.00</td>
<td>0.13</td>
<td>0.79</td>
<td>0.27</td>
</tr>
<tr>
<td>hazfuels.5 + log.5</td>
<td>4215.14</td>
<td>3.19</td>
<td>0.07</td>
<td>0.86</td>
<td></td>
</tr>
<tr>
<td>hazfuels.5 + burn.1.50</td>
<td>4215.15</td>
<td>3.19</td>
<td>0.07</td>
<td>0.93</td>
<td></td>
</tr>
<tr>
<td>burn.1.50 + log.5</td>
<td>4215.87</td>
<td>3.91</td>
<td>0.05</td>
<td>0.97</td>
<td></td>
</tr>
<tr>
<td>hazfuels.5 + burn.1.50 + log.5</td>
<td>4217.14</td>
<td>5.19</td>
<td>0.03</td>
<td>1.00</td>
<td></td>
</tr>
<tr>
<td><strong>Extinction</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>hazfuels.5</td>
<td>4205.26</td>
<td>0.00</td>
<td>0.50</td>
<td>0.50</td>
<td>0.98</td>
</tr>
<tr>
<td>hazfuels.5 + log5</td>
<td>4207.08</td>
<td>1.82</td>
<td>0.20</td>
<td>0.70</td>
<td></td>
</tr>
<tr>
<td>hazfuels.5 + burn.1.50</td>
<td>4207.11</td>
<td>1.85</td>
<td>0.20</td>
<td>0.90</td>
<td></td>
</tr>
<tr>
<td>hazfuels.5 + burn.1.50 + log.5</td>
<td>4208.96</td>
<td>3.70</td>
<td>0.08</td>
<td>0.98</td>
<td></td>
</tr>
<tr>
<td>intercept only</td>
<td>4212.67</td>
<td>7.42</td>
<td>0.01</td>
<td>0.99</td>
<td></td>
</tr>
<tr>
<td>log.5</td>
<td>4214.16</td>
<td>8.90</td>
<td>0.01</td>
<td>0.99</td>
<td>0.29</td>
</tr>
<tr>
<td>burn.1.50</td>
<td>4214.61</td>
<td>9.36</td>
<td>0.00</td>
<td>1.00</td>
<td>0.28</td>
</tr>
<tr>
<td>burn.1.50 + log.5</td>
<td>4216.05</td>
<td>10.80</td>
<td>0.00</td>
<td>1.00</td>
<td></td>
</tr>
</tbody>
</table>

**Integration**

Fire modeling

Fuels treatments reduced the intensity of the simulated fire, as evidenced by the predicted flame
lengths (Figure 1). On the untreated landscape, 68.6%, 18.4%, 11.2%, and 1.8% of the study area experienced flame lengths of <2, 2-4, 4-8, and >8 m, respectively. In contrast, on the treated landscape, 75.1%, 16.4%, 7.5%, and 1.0% of the study area burned at these flame lengths. Collins et al. (2011) noted that flame lengths >2 m often corresponded to areas with crown fire initiation (i.e., torching). Thus, a greater proportion of the untreated landscape was exposed to potential crown fire (31.4%) than for the untreated landscape (24.9).

![Figure 32. Flame lengths (m) of simulated fires at Sugar Pine on (a) untreated and (b) treated landscapes. We show the location of treatment polygons in (a) for ease of comparison, but treatments were not implemented in (a).](image)

**Assessing the effects of fire and SPLATs on fisher habitat**

We found that SPLATs caused an immediate, slight reduction in potential fisher habitat. The entire area of the four watersheds was 35,103 ac (14,206 ha), and in year 0, there were 16,013 ac (6,480 ha) of potential fisher habitat on the untreated landscape compared to 13,938 ac (5,641 ha) on the treated landscape (Figure 2). In the absence of simulated fire, the amount of habitat steadily increased over time and was actually slightly greater on the treated landscape in years 10 and 30 (Figure 2). When fire was simulated, SPLATs had a slight, positive effect on the amount of potential fisher habitat up to 30 years later. In year 30, there were 14,653 ac (5,930 ha) of potential fisher habitat on the untreated landscape compared to 15,254 ac (6,173 ha) on the treated landscape (Figure
Figure 33. The amount of potential fisher habitat on the 35,103-ac Sugar Pine study area under four scenarios: 1) no SPLATs and no wildfire; 2) SPLATs and no wildfire; 3) no SPLATs and wildfire; and 4) SPLATs and wildfire. Year 0 for the “no treatment” scenarios was 2008, and year 0 for the “treatment” scenarios was 2013 (i.e., after SPLATs were implemented). Simulated fires occurred in year 0 for both the “no treatment” and “treatment” scenarios, and post-fire effects were first assessed in year 10.

Discussion

Reproduction and Basic Demography

Empirical data on reproductive rates and litter sizes are important for understanding the ability of a population to withstand challenges to survival, and to produce realistic estimates of population size in landscape level population models being developed for conservation planning (Lofroth et al. 2010, Spencer et al. 2011). The basic life history of fishers with regards reproduction is generally well known. Fishers have a long gestation period due to the reproductive strategy of delayed implantation.
Once the blastocyst implants in the uterine wall 10-11 months after fertilization of the egg, embryonic development resumes and ≈ 36 days later 1-4 kits are typically born.

Parturition for fishers in northern California typically occurs in mid to late March (Matthews et al. 2013a), and female fishers in the SNAMP Fisher population were no exception based on initiation of denning around March 22-31 (Table 13). Also, duration of denning for fishers in our study (≈ 70-75 days; Table 13), and cessation of localization to den trees in early to mid-June was typical of elsewhere in the western United States (Matthews et al 2013a, Aubry and Raley 2006). The mean denning rate for female fishers in the SNAMP Fisher study was 0.85 (Table 15), which was slightly lower than for fishers in the Hoopa Fisher Study in northern California (0.88; Matthews et al. 2013a), similar to in the Kings River Fisher Project area (0.86; R. Green, unpublished), and higher than in southern Oregon (0.59; Aubry and Raley 2006). The average weaning rate from SNAMP females in our study area (0.74; Table 15) was higher compared to the Hoopa Fisher Study (0.65; Matthews et al. 2013a), and in southern Oregon (0.44; Aubry and Raley 2006). However, as Matthews et al. (2013a) noted, reports of low weaning rates from some studies may be due to issues with age assignment. On our study we closely tracked most animals in the population from when they were juveniles until death, and ages for nine female fishers that were captured early in the study were determined by cementum annuli (Mattson’s Laboratory, Milltown, MT; Poole et al. 1994).

Average litter sizes are larger for female fishers in eastern North America (2-4 kits/litter; Paragi et al. 1994, York 1996) compared to in the western United States where litter sizes are most commonly 1 or 2 kits (Lofroth et al. 2010, Aubry and Raley 2006, Matthews et al. 2013). The mean litter size from SNAMP Fisher (1.5 kits/litter, Table 16) was similar to reports from the Kings River Fisher Project (1.6; R. Green, unpublished), but lower than 1.8 kits/litter reported for the Sequoia National Forest, California (Truex et al 1988), 1.9 kits/litter from the Hoopa Fisher Study in northern California (Matthews et al. 2013a), or 1.8 kits/litter in southern Oregon (Aubry and Raley 2006).

Close monitoring of denning behavior by several current studies including SNAMP Fisher is providing insight on difficulties female fishers encounter while attempting to reproduce. In the course of five denning seasons, we documented eight cases when females died or were killed with dependent kits in den cavities (Tables 15, 16). Death of denning female fishers appears fairly common, based on reports from the Hoopa Fisher Study (n = 5; Matthews et al. 2013a), the USFS Kings River Study (n = 3; C. Thompson, unpublished data), the ongoing California Department of Fish and Wildlife, U.S. Fish and Wildlife, and Sierra Pacific Industries “Stirling Fisher Reintroduction Project” (n = 5; Powell et al. 2013), and the Olympic Fisher Reintroduction Project (n = 2; Lewis et al. 2012). Evidence that a
significant number of females exhibiting denning behavior may die before weaning is important because weaning rates may be biased high unless estimates are based on complete monitoring through the duration of the denning period (Facka et al. 2013).

**Denning Structures and Denning Habitats**

Across their range in North America female fishers give birth to kits in cavities in live trees or snags (Paragi et al. 1996, York 1996, Weir et al. 2012, Zhao et al. 2012, Matthews et al. 2013a). Den cavities and habitats in the immediate vicinity of denning structures provide protection from predators and inclement weather during the early spring to late spring when females are rearing their young (Weir et al. 2012). Most female fishers use more than one denning structure during a den season (range 1-6; Matthews et al. 2013a), and female fishers on the SNAMP Fisher study used an average of 2.4 different denning structures per den season (range 1-5), compared to 3.4 on the Kings River Fisher Study (R. Green, unpublished data), and 3.1 in northwestern California (Matthews et al. 2013). Female fishers may use more than one denning structure in a season for several reasons: to accommodate kit growth by moving to larger cavities, to reduce predation risk, as bobcats and mountain lions may discover a den location due to odors from the accumulation of urine and feces, to move closer to unexploited foraging areas, and to avoid exposure to feces and parasites that may accumulate in den cavities.

The lower mean number of den trees used by female fishers on the SNAMP Fisher study area compared to on the KRFP or the Hoopa Fisher Study may be related to disturbance by researchers. Biologists on both the KRFP and Hoopa Fisher studies climb den structures of most known denning female fishers to obtain kit counts, and they also attempt to extract kits from den cavities to measure body size, collect tissue samples, and to insert PIT tags for later identification (Thompson et al. 2011, Matthews et al. 2013a,b). This process requires presence of multiple biologists at the den tree for periods of 60 to 180 minutes. Although we occasionally ascended den trees in the SNAMP study area to obtain kit counts (n = 9 total den tree climbs during six denning seasons), most kit counts (∼ 90%) were obtained using images from 2-4 motion-sensing cameras placed around denning structures to monitor and chronicle denning activities remotely. Moreover, we noticed that some individual female fishers were sensitive to presence of technicians setting up den cameras (the process requires 30 to 60 min), based on short duration use of their denning structures after the first visit. Our den camera protocol was adjusted to minimize time needed to setup and service den cameras by quickly switching out memory cards and reviewing images away from the denning structure. Also, whenever possible,
we did not approach den trees to service cameras when radiotelemetry identified presence of the
denning female.

On the nearby Kings River Fisher Project where habitats are similar to the SNAMP Fisher
study area, denning structures used by female fishers were most commonly black oaks (54%: 50% live,
4% snags). Overall, 91% of denning structures used on the Kings River Fisher Project were live trees.
When repeat use den trees were counted just once, 43% of the unique denning structures used in the
SNAMP Fisher study area were black oak trees (Table 17; 25% live, 18% snags), and the remaining
unique den trees were primarily incense cedar (33%) or white fir (15%). Only 56% of the denning
structures used by female fishers in the SNAMP study area were live trees.

Weir et al. (2012) noted that trees need to have two very specific features for female fishers to
use them for denning; some form of physical damage to the tree bole to provide access for decay
organisms, and the damage must be of particular dimensions to provide predator-secure access for the
female to the interior of the tree via frost cracks, fire-scars, large anchored branches pulling out from
the tree bole, or woodpecker holes (Lofroth et al. 2010). McDonald (1990) noted that live black oaks
are susceptible to internal decay and probably last longer on the landscape than conifer snags.
However, 26% of 125 unique den structures used by fishers on the SNAMP study site were in conifer
snags (Table 17), suggesting they are not especially uncommon on the landscape in our area. Also, our
observations of incense cedars and white fir suggested these two tree types were susceptible to the
types of damage identified by Weir et al. (2012), particularly with regard to fire scars for cedar trees.
Many of the cedar trees selected for use as den trees had basal fire scars, and the actual den cavities in
both cedar trees and white fir were commonly associated with large branch break points.

Habitat and site characteristics immediately surrounding the denning structures are likely
important for appropriate thermal conditions, availability of prey, and avoidance of predators (escape
cover and concealment cover). Denning structures used in the SNAMP Fisher study area were
generally larger than available trees and snags; mean DBH was relatively large (larger for conifers than
the hardwoods) and mean tree heights were taller for live conifers compared to conifer snags or oaks in
general (Table 18, Fig. 15). Canopy cover was greater than 80% in the vicinity of many den trees
(Table 19). Shrub cover near den trees was variable, as was aspect (Table 19, Fig. 16). Most den trees
had multiple large down trees/logs nearby, and concealment cover to the base of den trees averaged
more than 45%. Although detailed analyses of data from fixed radius habitat plots have not been
completed, habitat characteristics were developed from high resolution Lidar data for many den trees
within the Key Watersheds. As part of collaborative work with the Spatial Team, Zhao et al. (2012)
identified that fishers selected den sites with tall trees and steep slopes within a 10-m radius of the den tree, high forest structural complexity within 20 m, large tree clusters within 30 m, and high canopy cover and larger mature trees within 50 m. Finally, at the larger landscape scale, the mean elevation for denning structures used in the SNAMP Fisher study was 1,591 m (Fig. 15).

**Fisher Survival and Cause-specific Mortality**

The SNAMP Fisher study uncovered a wider diversity of causes of mortality for fishers in the region than anticipated (Table 22). In the first five years of the study many newly deceased fishers were recovered before the pilot and biologist in the airplane landed back at the Mariposa Airport, and almost always within six hours of the first indication that an animal’s radiocollar was pulsing inactive. Although the fixed-wing aerial telemetry effort was expensive, the research identified the first known death for the species caused by active infection with Canine Distemper Virus (CDV). We also recovered the fresh carcass of a fisher in spring 2009 that was subsequently determined to have died from exposure to anticoagulant rodenticides. This discovery prompted testing of archived tissue samples of dead fishers throughout California and in the western United States, leading to two peer-reviewed papers focused on the problem of rodenticides and other poisons used at clandestine marijuana grow sites on California public lands (Gabriel et al. 2012, Thompson et al. 2013). Moreover, an important and very real benefit from the investment in an aviation program in support of SNAMP Fisher has been the discovery that survival and reproduction of fishers in the Sierra National Forest is challenged by multiple factors external to, and not directly linked to current forest management activities.

Well over half of the individual fishers captured and radiocollared during the study had perished as of April 2014. Known sources of cause-specific mortality in the SNAMP Fisher study population include high numbers of attacks by predators (interspecific killing; Wengert et al. 2014), roadkill deaths on Highway 41, infection by canine distemper virus (CDV; Keller et al. 2012) and *Toxoplasma gondii*, injury-induced starvation or septicemia, entrapment in a water tank, and acute toxicosis and hemorrhaging caused by exposure to rodenticides (Gabriel et al. 2013, Thompson et al. 2013). Most deaths from infectious disease, roadkill, and rodenticide exposure were males. On the other hand, proportionally more females than males were killed by predators (Fig. 11), and research with collaborators from UC Davis indicated that males are less susceptible to death by bobcat attack than females (Wengert et al. 2014).
The diverse threats to survival that impinge on population growth in our study population are not unique to the southern Sierra Nevada region. Fishers in the Hoopa Valley in northern California area also died as a result of predation (Wengert et al. 2014), disease (Gabriel 2013), and rodenticides and other toxicants (Gabriel et al. 2013). Also, fishers that were reintroduced in northern California as part of the Stirling Fisher Reintroduction Project site have succumbed to predation, disease, and trauma from collisions with vehicles (Powell et al. 2013).

Survival estimators generally assume that live-trapping and radiocollars do not influence survival of study animals. Based on necropsies and extensive pathological tests completed on carcass remains of 47 dead fishers, no mortalities on the SNAMP Fisher study site were directly attributable to capture-related injury or radiocollars (e.g., strangulation or infection from chafing on the neck). However, one adult female fisher failed to survive the capture process to recovery and release. We acknowledge that the stress of capture and anesthesia contributed to the death of this female, even though detailed pathological examination revealed that she was extremely emaciated and suffering from systemic infection from serious injury (laceration to the rostrum, fractured mandible, partially disarticulated lower jaw) prior to capture (Gabriel 2013).

Analyses of live/dead status of individual fishers from SNAMP Fisher indicated that survival rates for adult females were within the range observed for other areas in the western United States. Overall survival for adult female fishers was 0.74, compared to 0.77 at the KRFP site (Sweitzer et al., In revision), which was higher than the 0.61 rate reported for a smaller sample of radio-collared female fishers on the Sequoia National Forest south of our study region (Truex et al. 1998). Aubry and Raley (2006) reported an adult female survival rate of 0.78 from a study in southwestern Oregon, whereas Higley et al. (2012) estimated adult female survival at 0.77 to 0.79 in northwestern California based on two different analytical methods (Known-fate models and Capture-Mark-Recapture, respectively). Jordan et al. (2011) reported a combined male-female adult survival rate of 0.94 for research completed in the KRFP study area in 2002-2004, however, their survival estimate was based on camera detections rather than radio-collared individuals, and the values reported were considered to have low precision related to tag loss and other factors.

In general, survival of male fishers on the SNAMP Fisher study area was consistently lower among all age and sex classes compared to females (Table 22). All year survival for SNAMP Fisher males ranged from 0.57 to 0.64, which was lower than adult male survival for fishers in southwest Oregon (0.85; Aubry and Raley 2006), in the Sequoia National Forest, California (0.73; Truex et al. 1996) and in northwest California (0.75 to 0.72; J. M. Higley unpublished report).
At the outset of the study we anticipated that survival would be lower for juvenile and subadult fishers compared to adults, as is typical for several species of mesocarnivores (Farias et al. 2005, Murdoch et al. 2010). Although survival among subadults trended lower than for adults, juvenile survival by both male and female fishers was often very similar or trended higher than adult survival (Table 22). We believe this is unlikely and an artifact of our inability to monitor juvenile survival during their first six months of life. Juvenile fishers are small in size and body mass during summer, and in our study and for most prior studies attempting to ascertain survival, juveniles were not fitted with radiocollars until fall or winter when many individuals within the cohort may have already perished (Facka et al. 2013). Even less is known about survival of kits when they are being provisioned inside den cavities (Lofroth et al. 2010). This is important because modeling efforts using empirically derived demographic parameters identify that population size and likelihood of persistence are relatively insensitive to juvenile survival (Buskirk et al. 2012, Spencer et al. 2011), potentially because juvenile survival is biased high.

The all year estimate of female survival on the SNAMP Fisher site was higher for juveniles, similar for subadults, and lower for adults compared to parameter values used by Spencer et al. (2011) to simulate fisher population dynamics under different management scenarios for the southern Sierra Nevada region. Our combined year estimates of survival for juvenile females was 0.75 (value used by Spencer et al. = 0.50), 0.71 for subadult females (value used by Spencer et al. = 0.70), and 0.74 for adult females (value used by Spencer et al. = 0.90). Spencer et al. (2011) reported their model was relatively insensitive to juvenile and subadult survival (and other demographic parameters), but highly sensitive to adult female survival. Our empirically derived estimate for adult female survival (Table 23) was 15% lower than 0.90, which is important because Spencer et al. (2011) noted that a 5% decrease in female survival produced an approximate 18% reduction in the ending population size 40 years after model initiation. Similarly, 10% and 25% reductions in female survival resulted in 37% to 72% reductions in the ending population size. The significantly higher survival rate we estimated for juvenile females might ameliorate the reduced end year population size associated with 15% lower adult female survival. A new modeling effort is underway that will integrate new information on demographic rates from the SNAMP and KRFP study sites (Spencer et al. 2014).

Wildlife populations are exposed to a variety of mortality factors, which vary in importance towards limiting or impinging on population growth. Predation was clearly identified as the most important source of mortality on the SNAMP Fisher research site (Table 21, Fig. 11). Data on percent deviation in survival described by Sweitzer et al. (In revision) indicated that predation was more
important than disease processes and human-linked factors for limiting fisher survival at the site.

Disease in the form of canine distemper, toxoplasmosis, or pleururitis+pneumonia caused death for five radiocollared fishers during the SNAMP Fisher study, and an additional four fishers died of septicemia or starvation due to puncture wounds or other injury (Table 22; Gabriel 2013). The death of four fishers on our study by infection or starvation after suffering wounds or debilitating injury was not unusual or surprising for an animal as active as the fisher. Other long-term studies of radio-collared fishers have reported similar circumstances (Aubry and Raley 2006, Weir and Corbould 2008).

Infectious disease has been a conservation concern for the two isolated populations of fishers in California since exposure to CDV and other pathogens was first documented in northern California in the early 2000s based on serological testing (Brown et al. 2008). Canine distemper is of special concern because widespread, near catastrophic population-wide mortality among multiple species of endangered and uncommon carnivores has been reported (Timm et al. 2009, Williams et al. 1998, Woodroffe 1999). An outbreak, or localized epizootic of CDV that likely originated on the SNAMP site in spring 2009, and then spread south into KRFP during summer 2009 resulted in death of four fishers (Keller et al. 2012, Table 2). This disease-related mortality event confirmed that exposure by fishers to CDV and other agents of disease is of conservation concern for fishers in the western United States in general (Gabriel et al. 2012b), but particularly for the small, isolated population of fishers in the southern Sierra Nevada (Gabriel 2013). One fisher on the SNAMP study site was also confirmed to have died by complications after parasitic infection by Toxoplasma gondii (Gabriel 2013). Although exposure of fishers to Toxoplasma gondii was previously documented for fishers in North America (Larkin et al. 2011), this was the first case where complications from toxoplasmosis resulted in death (Gabriel 2013).

Wildlife-vehicle collisions may be a locally-critical mortality factor. Highway 41 is a very busy road locally referred to as the Wawona Road once it enters Yosemite National Park near the small community of Fish Camp. During the study period six non-collared fishers were also known to have been killed by vehicle strikes on Highway 41. Nine fishers were known to have been killed by vehicles along a 42 km stretch of Highway 41 during January 2008 to March 2013 in Yosemite National Park. Chow (2009) previously reported 4 fisher roadkill deaths between 1992 and 2004 along the same section of Highway 41, identifying this roadway as problematic for fisher survival in the region. Roadkill deaths of fishers have been reported in northern California as well, including two near Trinity Lake, (in either Shasta County or Trinity County—not specified; Truex et al. 1998), eight
along paved highways in Humboldt and Siskiyou County (Gabriel 2013), and one in Butte County (Powell et al. 2012). In total, we are aware of 34 documented cases of fisher mortality by vehicle-strikes in California from 1992 to 2013 (Table 21). Moreover, seven fisher deaths were reported in western Washington state in association with the Olympic Fisher Reintroduction Project (Lewis et al. 2012 unpublished report), and fishers regularly die on highways in British Columbia (R. D. Weir, personal communication), and the northeastern United States (Douglas and Strickland 1987, York 1996).

Our original prediction was that survival would be lowest during winter compared to in spring or summer. Sweitzer et al (In revision) found that this prediction was not supported by the data, and that a disproportionate number of fisher deaths occurred during spring and summer. Increased mortality of fishers in this period is potentially related to exposure to second generation anticoagulant rodenticides, which is typically applied most heavily in the spring growing season. Expanded testing for anticoagulant rodenticides in archived tissues for fishers that died on our study before 2009, and for fishers that died on the Hoopa fisher study in northern California revealed that the majority of the animals had been exposed to anticoagulant rodenticides (>80%) and other toxicants being broadcast dispersed around illegal marijuana grow sites on California public and tribal lands (Gabriel et al. 2012a). Ongoing investigations focused on this issue indicate that use of anticoagulant rodenticides at illegal grow sites is focused during spring and early summer when the marijuana plants are small and vulnerable to herbivory by rodents and insects (Gabriel et al. 2012a, 2013, Thompson et al. 2013). A total of eight fishers (three from SNAMP Fisher, five from the Hoopa fisher study in northern California) have now been documented as dying from exposure to rodenticides or other toxicants associated with marijuana grow sites died during April to June (Gabriel 2013).

Another human-linked source of death for fishers in our study was entrapment or drowning in water tank. At the SNAMP site in spring 2008 we recovered the carcass of a non-collared fisher on the ground next to an open water tank (the cover had been ajar) where maintenance crews servicing the tank deposited the animal. Truex et al. (1998) and Powell et al. (2012) both reported deaths of single radio-collared fishers in abandoned water tanks at research sites in north central California, whereas Folliard (1997) recovered skeletal remains of eight fishers from an abandoned water tank on private timberlands in northwestern California. Finally, L. Davis (personal communication, Sept 7, 2013) reported the death of a radio-collared fisher that maneuvered into a relatively short section of an upright culvert during a study of fishers in the Cariboo-Chilcotin region of British Columbia, Canada (Davis 2008). It appears that death of fishers by entrapment in water tanks and other human structures
may not be uncommon. Folliard’s (1997) 15-year-old recommendation that abandoned water tanks on private and public forests in California be covered, or modified by inserting branches or poles so that fishers and other wildlife can self-rescue should be applied whenever possible.

**Population Size and Density**

Prior to this study there was limited information on the distribution and abundance of fishers at the north margin of their extant range in the southern Sierra Nevada. Despite many years of surveys with cameras and track plates, the lack of evidence of fishers north of Yosemite Valley suggested that the population in the SNAMP Fisher study area was likely sparse (low density). Also, there had been no indication that surplus animals were dispersing northward into suitable, but unoccupied habitat north of the Merced River (Spencer et al. 2011, Spencer et al. 2014). Moreover, reports of multiple roadkill fishers along Highway 41/Wawona Road between the south boundary of the park and the tunnel just north of Yosemite Valley suggested that dispersal and the overall population was being limited by deaths on that highway (Chow 2009).

Federal and state agencies are currently developing strategies to manage for long term viable populations of fishers in the southern Sierra Nevada, and six years of intensive research as part of the SNAMP Fisher study has recently produced the first estimates of abundance for the region. We estimated the size of the fisher population in the overall SNAMP study population at 48 to 62 individuals (Table 4). Narrow confidence intervals for the population estimates were likely due to the combination of a relatively high probability of detection (0.4 to 0.75) for our camera protocol when cameras were within the home ranges of radiocollared fishers (Popescu et al. 2014) (Table 4).

Mean annual population density for the three Subregions of the overall study area ranged from 0.072 to 0.097 fishers/km² (Fig. 2), which was consistent with data from two previous studies of fishers in the High Sierra District of the Sierra National Forest, located 50 km south of our study site. Jordan et al. (2011) used a similar CMR design to estimate a density of 0.063-0.109 fishers/km² for the Kings River study area in 2002-2004. Thompson et al. (2013) used scat detector dogs and genetic detections in a spatially explicit CMR framework modified for variable search intensity to estimate a fisher density of 0.065-0.28 fishers/km² for the Kings River Fisher Project area in fall 2007. Thompson et al. (2013) emphasized that a modal density of 0.104 fishers/km² was the most appropriate point estimate developed from their research. At a research site on the Hoopa Valley Indian Reservation (Hoopa Fisher Study) in northern California, Higley et al. (2013) used CMR methods to...
determine that density of fishers was stable and increasing at 0.12-0.29 fishers/km² over a 9-year period from 2005-2013. In central Massachusetts, USA, Fuller et al. (2001) applied CMR models to camera sightings and determined fisher densities of 0.19-0.25 fishers/km². Considering the subset of studies that used CMR methods, the densities we estimated for the SNAMP Fisher study area are the lowest reported (Table 4).

As previously detailed, conservation planning is underway for fishers in the southern Sierra Nevada, including new modeling to estimate areas of suitable habitat for fishers in occupied “core” regions within the southern Sierra Nevada (Spencer et al. 2014). Our study area is within Habitat Core and Connectivity Area 5, for which the area of suitable habitat was estimated as 1,096 km² (Table 2, Spencer et al. 2014). We calculated the mean density and 95% C.I. for 12 area- and year-specific densities developed by our CMR modeling (Table 4; 0.085 fishers/km², 95% C.I. 0.073-0.097), and estimated that there were 93 (range 80-107) fishers in the Southern Sierra Nevada Habitat Core and Connectivity area 5.

In the context of similar data from other studies, the population of fishers in the Bass Lake Ranger District extending into southern Yosemite National Park is small, genetically limited (Tucker et al. 2014), and exists at a density that is lower than has been reported for any part of California or North America with the exception of boreal forest regions of northern British Columbia, Canada (Weir and Corbould 2006). Moreover, there are important challenges to the long term viability of fishers in the southern Sierra Nevada region as a whole, including periodic epizootics of canine distemper (Keller et al. 2012), exposure to poisons and other toxicants that directly and indirectly reduce survival (Thompson et al. 2013), and large, catastrophic wildfires capable of eliminating thousands of hectares of foraging and denning habitat in short periods of time (days or weeks; Final Update on 2013 Rim Fire: http://inciweb.nwcg.gov/incident/article/3660/21586/).

**Dispersal and Home Range Movements**

Information on dispersal provides important insight on how far individuals of a species may move on their own, which is valuable for understanding the potential that unoccupied but otherwise suitable habitat will be colonized or recolonized by the species without management intervention. For their body size, fishers appear to be relatively poor dispersers and large scale genetic substructure analysis supports this observation (Kyle et al. 2001). Fisher movement ecology varies by age, sex,
season, and habitat characteristics. Juvenile dispersal may vary widely, depending on habitat availability and landscape permeability.

Intensive monitoring of individual fishers by fixed-wing aircraft, in combination with an expansive trapping effort across the entire SNAMP Fisher study area provided insight on dispersal that would have been difficult to acquire otherwise. Microsatellite DNA analyses to identify maternity for many juveniles and some subadults further extended our inference to larger numbers of females \( n = 24 \) and males \( n = 19 \).

We found limited evidence that natal dispersal was male-biased according to any of the typical metrics reported in the literature for this life history phenomenon. Dispersal distances were not longer for males (mean = 8.46 km) compared to females (4.89 km) based on either Euclidean distances or for more realistic Least Cost movement paths (Table 29, Figs. 17). There was no difference in the proportion of each gender that dispersed, or that remained philopatric (Fig. 17, Table 29), and, similar numbers of males \( n = 5 \) and females \( n = 3 \) undertook long distance dispersal movements from their likely natal areas (Fig. 18). Timing of dispersal in the SNAMP Fisher study population was focused during mid-February to July, and the longest distance dispersal event a female fisher in the population undertook was 22.3 km (44.1 by the Least Cost Path), compared to 36.2 km for a male (69.8 by the Least Cost Path)(Tables 28, 29). We did document dispersal by several fishers across landscape features previously identified as restrictive based on population genetics (Tucker et al. 2012, Wisely et al. 2004). Four fishers regularly moved across the Chiquito Ridge (via Shuteye Pass), and two male fishers transitioned across the San Joaquin River canyon.

Our data on dispersal differed from reports from southern Oregon and northwestern California. Aubry and Raley (2006) reported that mean juvenile male dispersal distance was 29 km, while the mean dispersal for females was 6 km. Dispersal distance in the Hoopa area of Northern California averaged 4.0 km (range = 0.8-18.0 km) for 7 females, and was 1.3 km for one male (Matthews et al. 2013a), however the authors noted that their focus on capturing adult females limited their ability to estimate male dispersal.

The maximum known dispersal distance for fishers from the literature was 100 km (York 1996), while the maximum observed movement of a translocated individual in unoccupied habitat was 163 km (Lewis et al. 2012). The relatively limited number of long distance dispersal events noted during the six year SNAMP Fisher study suggests that long distance movements are uncommon and that the effective dispersal distance may be less than maximum dispersal capacity (Tucker et al. 2013).
Population Growth and Threats to Population Persistence

Estimates of $\lambda$ for fishers derived from empirical data specific to the area of inference are rare for California, and absent for the southern Sierra Nevada. The All Year survival and empirically derived demographic rates produced a $\lambda$ of 0.90 (range 0.77-1.22). While this point estimate suggests a negative growth rate, it was encouraging that the range for the all year population growth rate extended above 1.0 (Table 34). Elsewhere in California, Higley et al. (2013) integrated data on apparent survival from CMR models and data on reproduction in a series of random effects models to evaluate $\lambda$ for fishers in the Hoopa Fisher Study. Two models produced $\lambda$ estimates close to or greater than 1 (Both sexes, Females only; see Higley et al. 2013). Swiers (2013) used Robust Design models, software program POPAN, and Pradel models to develop information on demographic rates, population size, and population growth rates for assessing whether removal of adult fishers from a population in northern California/southern Oregon for translocation elsewhere negatively affected population growth. Swiers’ (2013) top ranked Pradel model produced a population growth rate of 1.06 (95% CI = 0.97-1.15), suggesting a stable or slightly increasing population after nine ‘prime breeding adult’ fishers had been live-trapped and removed from the population for translocation.

We identified several sources of mortality in the study population, and indication of a possible overall negative growth rate for the population was in accordance with the fact that 60% of the 110 fishers that were radiocollared died (Table 22). The matrix model we developed was realistic and based on current knowledge of fisher life histories in California, but some demographic parameters were less well known than others. Survival of juvenile fishers during the three month period from mid-June to October is poorly known for our study, and for all other detailed studies of fishers in California (Facka et al. 2013). The estimate for juvenile female survival used in the matrix model was based on the 6-7 month period from October to March, which likely overestimated the number of juveniles recruited into the population. However, a basic sensitivity analysis indicated that the population growth rate was insensitive to variation in fertility for all age classes, and least sensitive to juvenile survival compared to subadult and adult survival.

SNAMP Fisher Management Indicators

Three management indicators we developed in 2008-09 as a mechanism for interim reporting on the status of fishers in the study area appeared useful when considered in relation to data on
population growth rates and population density. Naïve occupancy in the Key Watershed was lowest in Camera years 2007-08, 2008-09, and 2009-10 when population growth rates were negative, but then increased in the later years when the growth rate was stable or positive (Tables 33, 35). The number of resident female fishers using the Key Watersheds did not track changes in population growth rates as closely, but the proportion was lowest in Population years 4 and 5 when the growth rate was negative or at approximate stasis. Adult female survival tracked change in population growth rate closely, declining from 2-year group 1 to 3, and then increasing afterwards (Table 34). Also, population density was in decline from 2007 to 2009, but then increased during Camera years 3 (2010-11) and 4 (2011-12) (Fig. 13), coincident with improved survival among juvenile and adult female survival (Table 34). We recommend that future long-term studies consider developing similar metrics as a monitoring tool, and for interim reporting to interested stakeholders.

**Fisher response to fuel management**

Concerns that initiation of focused management to reduce fuel levels in Sierra Nevada mixed-conifer forests to correct for 90 to 100 years of fire suppression might have negative effects on habitat use by fishers were only partly supported by results from our study. Fisher occupancy was not negatively associated with either extractive or restorative fuel reduction, though disturbances from restorative fuel reduction had a negative effect on local scale persistence. We believe that the lack of a relationship between extractive fuel reduction and occupancy by fishers was most likely due to the combination of related factors. First, the overall extent of logging in our study in the 11 years from 2002 to 2013 was likely much lower than historically, and was likely further diminished by poor market conditions for wood products when a severe recession began in 2008. Second, estimates of annual disturbance from extractive fuel reduction among occupancy survey grids was equivalent to levels known “tolerated” by fishers elsewhere in the Sierra NF (Zielinski et al. 2013). Among the 361 multi-season survey grids, 172 of them encompassed 51.9 km² of disturbance from extractive fuel reduction, representing disturbances of 2.7%/year to grids with disturbance, and 1.3%/year among all grids. Zielinski et al. (2013) investigated tolerance of fishers to forest management in the High Sierra District, Sierra NF, and reported that 14 km² patches of forest habitat with high use by fishers typically had 2.6% of the areas disturbed by forest management annually, whereas 14 km² patches of forest with low use by fishers averaged 3.5% disturbance/year. Thus, the areas of extractive fuel reduction in our study were comparable to the 2.6% disturbance in fisher high use forest patches in the High Sierra District, Sierra NF, and below some threshold of ≥ 3.5% management disturbance/year that would likely cause fishers to forage elsewhere (Zielinski et al., 2013).
Our occupancy modeling supported the hypothesis that fishers would reduce their use of local patches of forest exposed to proportionally higher levels of cumulative restorative fuel reduction. Nevertheless, an important prediction from our multi-season model was that small patches of forest with 100% cumulative 5-year disturbance from mechanical mastication and reduction of understory trees and surface fuels would maintain an occupancy of 0.65. Thus, even at what would be considered a very high level of disturbance, fishers were not predicted to completely cease using those areas. For context, an occupancy rate of 0.65 for fishers elsewhere in the southern Sierra Nevada would be considered high, and a positive observation with regards to long term continuation of occupancy (Zielinski et al., 2013).

Ladder fuels, surface fuels, and thick layers of duff targeted under SPLAT-based management provide important habitat for squirrels and rodents preyed on by fishers, owls, and other forest carnivores (Kelt et al., 2013). Therefore, if forest patches that were extensively treated for restorative fuel reduction harbored less abundant prey, fishers may have shifted to nearby less disturbed forest patches to forage. The possibility that thinning of trees and shrubs, and reduction in understory surface fuels (coarse woody debris) has a negative effect on rodent populations has been considered by several recent studies. Meyer et al. (2007) reported reduced captures of northern flying squirrels in forest stands that were thinned and underburned in the High Sierra District, Sierra NF. Treated stands had reduced canopy cover and relatively shallow litter depth, and Meyer et al. (2007) considered that reduced abundance of flying squirrels may have been due to reduced abundance of truffles (fruiting bodies of hypogeous fungi) when duff was removed or reduced in depth after fuel reduction. Amacher et al. (2008) reported a negative effect of fuel reduction treatments (without follow-on burning) on abundance of deer mice, a positive effect of managed burning for deer mice, but no detectable effects of thinning or burning treatments on long-eared chipmunks, California ground squirrel, or brush mouse (Peromyscus boylei) at a research site in the north-central Sierra Nevada. Amacher et al. (2008) suggested that scattered debris and wood shards from rotary mastication was associated with the negative treatment effect for deer mice, whereas follow-on burning removed residual woody debris and thinned the understory, thereby improving conditions for deer mice. Converse et al. (2006) reported lower density or a trend for lower density for gray-collared chipmunks (Neotamias canipes) and Mexican woodrats (Neotoma mexicana) in thinned+burned forest stands in Arizona, which was linked to reduced coarse woody debris and reduced density of shrubs. In that same study, abundance of deer mice increased after thinning+burning, and there was no treatment-linked change in abundance for golden-mantled ground squirrel (Spermophilus lateralis) (Converse et al., 2006). In restoration-treated
ponderosa pine forests in Arizona, Lobeerger et al. (2011) found that winter season home ranges of tassel-eared squirrels (*Sciurus aberti*) disproportionately encompassed areas that had not been treated, whereas in other seasons their home ranges included a subset of the treated stands that retained relatively high canopy cover. Bull and Blumton (1999) indexed presence of small mammals from track surveys in lodgepole pine (*Pinus contorta*) and mixed-conifer forest stands treated for fuel reduction in northeastern Oregon. We were unable to identify studies that reported responses of Douglas squirrels or dusky footed woodrats (*Neotoma fuscipes*) to fuel reduction treatments, but, based on habitat associations for *Neotoma* (Innes et al., 2007; Kelt et al., 2013), understory thinning and removal of surface fuels and coarse woody debris may be problematic for woodrats (Lehmkuhl et al., 2006), whereas Douglas squirrels are a habitat generalist and less likely to be negatively impacted by fuel reduction (Coppeto et al., 2006, Herbers and Klenner, 2007; Kelt et al., 2013). Kelt et al. (2013) suggested that small mammal assemblages in the Sierra Nevada showed relatively limited responses to canopy thinning under current forest management. Abundance of small mammals in the Sierra Nevada has been linked to variation in production of cones or hard mast by pines and oaks (Coppeto et al., 2006; Wilson et al., 2008), which is important because a general pattern in many studies we reviewed was that interannual variation in abundance of small mammals was evident, and either masked or was much more important than the smaller effects introduced by fuel reduction-induced change to habitats (Converse et al., 2006; Coppeto et al., 2006; Amacher et al., 2008, Wilson et al., 2008; Kelt et al., 2013). We therefore conclude that reduced persistence of local scale habitat use by fishers in grids with larger areas treated for restorative fuel reduction was not likely to have been caused by changes in abundance of rodent prey from the associated disturbance to their habitats.

We consider it likely that the predicted 27% decline in persistence of local scale habitat use when cumulative restorative fuel reduction in a 1-km² grid approached 1.0 (100%) was associated with fishers shifting to forage in adjacent areas with less disturbance. A 27% decline in persistence of occupancy coupled with an annual colonization rate of 34%, suggests that fishers are flexible with regards local scale habitat use, and they might resume use of treated areas after several years of ecological recovery. Modeling analyses by Thompson et al. (2011) applied to a fisher occupied area of the High Sierra District, Sierra NF (Bear Fen) suggested that tree thinning (≤ 89 cm DHB) in mixed-conifer forest did not significantly reduce habitat suitability or “displace” habitat components from reference conditions in home ranges of resident female fishers. Based on these results from a nearby area in the Sierra NF, we believe it likely that fishers in our study area are likely to resume using forest patches treated for restorative fuel reduction within a few years of extensive disturbance. Also, fishers
are known to adjust space use to avoid disturbed areas within their home ranges. Garner (2013) reported that resident fishers included areas treated for extractive+restorative fuel reduction in their overall and core home ranges in proportion to availability on the overall landscape. At the finer scale of individual locations, Garner (2013) found that those same resident fishers avoided using areas within ≈ 200 m of fuel treatments. We interpret this result as consistent with ours; fishers were predicted to continue using 1-km² patches of forest with more extensive cumulative disturbance by fuel treatments, but at a reduced level compared to areas with less disturbance. Finally, our assessment of how fishers responded to forest management was at the scale of 1-km² patches of forest, which was small in relation to resident adult female (≈ 23 km²) and resident adult male home ranges in our study area (86 km²; Sweitzer, In review – SNAMP Report). If a 1-km² patch of habitat within the home range of a resident female fisher was 100% treated for fuel reduction of any type, 95.7% of that animal’s home range could remain available for normal levels of foraging, contingent on SPLATs being dispersed on the landscape and not locally concentrated as appears typical (Modhaddas et al., 2010).

Integration

We found that the SPLATs at Sugar Pine slightly reduced simulated fire behavior and resulted in greater amounts of projected fisher habitat up to 30 years after the fire. In the absence of simulated fire, we found that the SPLATs had an immediate, negative effect on the amount of fisher habitat, but SPLATs did not generally have a negative effect on fisher habitat when we modeled future forest growth for 30 years. In all scenarios, the differences between the treated and untreated landscapes were small.

Our results were in general agreement with prior findings. Thompson et al. (2011) performed an analogous study to ours, in which they modeled fire and forest growth under treatment and no treatment scenarios and assessed fisher habitat suitability in the southern Sierra Nevada. They projected that fuels treatments had slight negative effects on fisher habitat in the absence of fire, but provided significant positive benefits up to 37 years after simulated fire. Truex et al. (2013) suggested that less fisher resting habitat was present immediately after mechanical fuels treatments were implemented in the Sierra Nevada. However, fishers consistently used areas in the southern Sierra Nevada where some timber harvest had occurred, so it may be possible to implement fuels-reduction treatments at an extent and rate that achieves fire-hazard-reduction goals (Zielinski et al., 2013).
As we noted in Appendix C for the California spotted owl, the net benefits of SPLATs for the Pacific fisher will depend upon the true, but unknown, probability that high-severity fire effects will occur on a given portion of the landscape. However, future probabilities for specific fire behaviors (e.g., crown-fire initiation) are difficult to estimate, and it is therefore difficult to quantify trade-offs associated with SPLATs in absolute terms (Finney 2005). We further note that the SPLATs which were implemented at Sugar Pine appeared to have relatively modest impacts on forest structure and simulated fire behavior, and that it may be necessary to evaluate additional SPLATs of different intensities over a larger scale to fully assess the effects of SPLATs on fisher habitat. Nonetheless, we have no reason to believe that Forest Service managers should alter their current policy of avoiding the placement of SPLATs near known fisher denning sites (U.S. Forest Service 2004) because these sites have significant biological importance for this species.

Management Implications of Findings from SNAMP Fisher

Fishers have been the focus of systematic monitoring in the southern Sierra Nevada by track plates, hair snares, and camera traps since the mid-1990s (Truex et al. 1998, Zielinski et al. 2005, Jordan 2007). Analyses of baited track plate detection histories from 2002 to 2009 for the entire southern Sierra Nevada fisher population found no evidence that the population trajectory for fishers in the area has been significantly positive or negative, based on constant and positive persistent values (Zielinski et al. 2013). In contrast, Tucker et al. (2014) suggested that the fisher population in the SNAMP Fisher study area was produced by a significant post-1900s northward population expansion involving dispersal of animals from south of the Kings River (Fisher Core Habitat Area 4; Fig. 2). Tucker et al. (2014) reported evidence of ‘strong genetic clustering’ to the north of Little Shuteye Peak (part of a high elevation ridge that forms the east boundary of Subregion 2 in our study area; Fig. 6), which, along with evidence for other small genetic clusters, was suggestive of multiple founder events associated with contemporary population expansion. Data from track-plate surveys in the Sierra National Forest in the early 1990s rarely detected fishers (Zielinski et al. 1995, 2005), which suggested a very sparse population in the SNAMP Fisher study area (Fisher Core Habitat area 5; Table 2, Fig. 2), compared to the more recent surveys in 2002-2009 (Tucker et al. 2014). Tucker et al. (2014) postulated that very few fishers were present in the SNAMP Fisher study area prior to the 1990s, and that an expansion that occurred only during the last 20-25 years produced the population in this region.
Genetic data are not typically used to make inferences about population processes operating over extremely short periods in evolutionary time. The genetic analyses of Tucker et al. (2014), and the large increase in fisher detections in the region encompassing our entire study area between the early 1990s and 2002-2009 (Zielinski et al. 2013), suggest that a significantly positive population growth rate would be a requirement for understanding the current distribution and abundance of fishers in the SNAMP Fisher study area. During the period from 2007 to 2014, our results suggest that the fisher population in this region has not been experiencing consistently positive or significant population growth (Table 24).

The suggestion of an overall negative population growth rate, the low density, and the relatively small estimated number of fishers in Fisher Core Habitat area 5 ($n = 93$, range 80-107), warrants concern for the long term viability of fishers in the region. Any small population will be at high risk to stochastic events such as disease and large perturbations to critical habitats (e.g. forest fires or drought; Noss et al. 2006), and genetic limitation resulting from genetic drift after founder events (Tucker et al. 2014) will hinder population recovery and expansion (Reed et al. 2003). Minimum viable population size has been under debate (Shoemaker et al. 2013, Reed and McCoy 2014), but at <500 total individuals (Spencer et al. 2004), the current southern Sierra Nevada fisher population will likely require active management and conservation measures to maintain a positive growth rate across its entire range. The observed variation in fisher abundance and rates of population growth in the SNAMP Fisher study area (Table 4) reaffirms the vulnerability of the small, isolated population to external threats (Spencer et al. 2014), especially wildfires that are likely to increase in frequency and intensity with climate change (Bonan 2008, Safford et al. 2012). Moreover, our study spanned a limited period of six years when multiple threats to fisher survival within the study area were identified and during which three large wildfires further isolated the population by significantly reducing the availability of suitable habitat immediately to the south and north of the study site. We recommend continuous monitoring of the status of fisher populations in the southern Sierra Nevada region. It will be necessary to mitigate for major threats to fisher survival while maintaining contiguous expanses of suitable fisher habitats, and detailed analyses using realistic and empirically developed data on population parameters are necessary for evaluating the long-term viability of fishers in the southern Sierra Nevada. Data developed from the SNAMP Fisher study have provided important new insights on the status of a fisher population at the northern margin of their current distribution in the southern Sierra Nevada Range, which will be useful towards developing a comprehensive conservation strategy for fishers in California.
Acknowledgments

The field effort would not have been possible without help from a dedicated team of staff and volunteers including C. J. O’Brien, T. Gorman, R. Wise, W. Mitchell, J. Schneiderman, D. Jackson, B. Niles, J. Ashling, M. Ratchford, R. Wise, S. Vogel, L. Martin, J. Busiek, T. Watson, D. Hardeman, R. Jenson, J. Masarone, T. Thein, A. Vorhees, J. Ruthven, W. Sickard, J. Bridge, C. Jablonicky, S. Bassing, G. Cline, A. Beaudette, Z. Eads, T. Day, and K. Wagner. We thank Forest Service pilots J. Litton, J. Irving, S. Forkel, B. Bulfer, and C. Haney for aviation support during many safe flights. Some flights were more exciting than others, as were a number of landings back in Mariposa that kept us alert. Local support at the SNAMP field station was facilitated by B. Persson, and A. Otto. Necropsies were performed at the UC Davis California Animal Health and Food Safety Laboratory, initially supervised by the late Linda Munson at University of California Davis School of Veterinary Medicine. Partial funding and logistical support with necropsies, pathology, sample processing and testing was provided by the Integral Ecology Research Center, California Department of Fish and Wildlife, U.C. Davis Veterinary Medical Teaching Hospital, and California Animal Health and Food Safety Laboratory. The US Forest Service Region 5 funded the majority of the field and laboratory work associated with this effort. The study is part of the Sierra Nevada Adaptive Management Project, a joint effort between US Forest Service Region 5, the University of California, US Forest Service Pacific Southwest Research Station, US Fish and Wildlife Service, California Department of Water Resources, California Department of Fish and Wildlife, California Department of Forestry and Fire Protection, the University of Wisconsin – Madison, and the University of Minnesota, focused on investigating the effects of landscape fuel treatments on Sierran forested ecosystems. The US Forest Service Region 5 funded the majority of field and lab work associated with this effort. The California Agricultural Experiment Station funded the remainder.
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Peer Review

Introduction

The reviewed report describes a study of the public participation process in a joint forest management project involving a federal agency, the public, and an academic monitoring/science participant engaged in natural resource collaborative adaptive management. The goals of the public participation component of the project were to design a three party model public participation process to be used as a model for designing similar processes; as well as to foster an engaged, knowledgeable group of stakeholders to work constructively with the management agency (the Forest Service). The public participation process was designed based on five core elements: inclusivity, transparency, learning, relationship building, and effectiveness. The authors describe the in-person and virtual public engagement conducted throughout the project. To evaluate the success of the process design in accomplishing the core elements, the authors gathered statistics and profiles of participants at each in-person participation event. They also used affiliation social network mapping and self-organizing maps (SOM) to examine relationships between participants and events. In addition, the authors used a combination of surveys following in-person participation events and qualitative interviews and quantitative email surveys of participants early and late in the project. The goal of the assessment was to estimate how much the various participants learned from each other, from science, and from SNAMP outreach efforts. The authors determined that SNAMP science and outreach were effective at fostering learning and building relationships. They concluded the public participation process was successful overall, particularly in that the third party monitoring/science participant was perceived as independent, unbiased, and responsive to public input. They propose this process serve as a model of other collaborative adaptive management projects.

Merits

This study of collaborative adaptive management represents an important area of research in collaboration in natural resource management. The three party model, in particular, represents a potentially valuable contribution in monitoring for federal agencies. The scope and magnitude of the seven-year public participation process provides valuable information for evaluating the effectiveness of this approach. The review of the public participation process and the assessment results from in-person and virtual participation events generally supported the discussion of the success of these events in achieving the five core elements. The qualitative interviews and quantitative email surveys early and late in the project were well documented and provided good evidence to support the authors’ claims relative to the five core elements.
Critique

This report has general problems with framing and organization and clarity that make it difficult to evaluate how the theory links to the design and data and to evaluate how well the data supports the authors’ claims about the overall success of this public participation process. In addition, while the study is data rich, the findings are not sufficiently analyzed. As a result, the study raises some critical questions that are not addressed.

Framing: One problem with the framing for this study was that there was not a clear link between the theoretical foundation for the study and the design and results of the study. The theory overviewed the historical evolution toward collaboration in natural resource management and addressed the limitations on the degree to which federal agency decision processes could be collaborative. The authors argued the third party model in monitoring allowed stakeholders to have an influence on management decisions that was within the legal constraints of the agency. They also contended that this third party model provided a system of “check and balances” on the science and agency decision-making. Further, they noted the prevailing belief that actively engaging people from multiple backgrounds led to increased support for decisions and better decisions overall. However, these claims were not supported by the study. The authors acknowledged that participants could not track how the science and public input was being used, so there was no way to validate how science or public input influenced agency decisions. Instead, the participants had to trust in the good will of the scientists and the agency to use the information. Many participants at the end of the project still had a “wait and see” attitude toward success.

In addition, key concepts such as adaptive management, collaboration, and public participation were not defined as they were being used in this study. Thus, phrases such as “the left hand side of the adaptive management circle” and “close the loop” were not clear to the reader. Further, it was less clear how the authors were making connections between improving public participation with the goal of fostering an engaged, knowledgeable group of stakeholders and increasing public influence on agency decisions or improving adaptive management.

Organization and Clarity: The general design, execution, and analysis of data in this study were difficult to track. As noted above, the line of argument was not always clear. Further, study results and lessons learned were repeated in multiple sections, and even contradicted at times. For example, discussion of survey results and interviews were intermixed and confusing, with references such as first round and second round that did not easily cross-reference to the chart and graph figures.

Analysis of Findings: The study is reported factually, with insufficient analysis of the findings. For one, some data from the study was presented to support general claims that were disputed by discussions in later sections. For example, the claim that
collaboration focused on shared learning helps foster new and improved relationships was brought into question in the discussion of MOU partners and the funding issues. SNAMP MOU partners expressed frustration over frequent turnover among representatives and lack of commitment, and conflicts over funding created tensions and undermined relationships within the three party model. The authors even cite research that indicates experience in collaboration is not correlated with building trust, but instead is negatively associated with trust.

In addition, the groupings of participants from the surveys and interviews did not support the stated goals of building and assessing the learning between and among the scientists, the agency, and the public in the three party model. At times, there were noticeable differences in learning or changes in opinions among the three parties or even between different public groups, such as the forest products group, that were suggestive, but not addressed in any depth.

Because the analysis was not sufficiently robust, the lessons learned failed to capitalize on key learnings that would be of value in developing a model public participation process. For example, given the inevitable concern with shrinking budgets and limited capacity and resources, the analysis and lessons learned could better indicate which in-person or virtual forums are most critical for accomplishing the process goals and what design factors contributed to the success of these forums. The Management Workshops appear to be the only forum where scientists were able to learn from managers about key management questions and the context for management, which enabled the scientists to better understand how their research could help address these management questions. Given the importance of relevant science to inform decisions in adaptive management, I would expect the lessons learned to remark on the Management Workshops as a critical part of this three party model.

**Discussion**

The study on which this manuscript is based has great value to the field. Once the general problems are addressed, the findings will be more clear and compelling and useful for achieving the purpose of providing others with a public participation model.

*Framing:* Clearly define terms in the introduction to the study and clarify assumed relationship between improved public participation and improved decision-making. Qualify claims about the three party model. State these as assumptions or desired outcomes or provide research or information to support those claims.

*Organization and Clarity:* Improve organization and clarity by organizing the study into goals, design, implementation, findings, and lessons learned, with headings and subheadings for navigation and reference. It would also be useful to summarize the finding for each goal for easy reference.
Analysis of findings: Analyze the data more closely to address inconsistencies. This can be done by addressing the nuanced differences in the responses among the MOU partners, frequent participants, and those who just maintained a familiarity with the process. Also use groupings that better illustrate the relationship between and among the three parties in this model: the scientists, the agency, and the public.

Use the lessons learned to synthesize information and clarify which parts of the process are most critical and for whom for accomplishing collaborative adaptive management. For example, the field trips and management workshops were most highly rated (pg. 48) and were cited as excellent venues for mutual learning.

Also address the concerns voiced with funding, scalability, staff turnover, lack of commitment, and unclear decision-making in the lessons learned. It is important information for anyone considering using this model.

References


Additional Comments for Authors

There is a typo on page 15 in the sentence beginning with “The legacy of this policy has was a problem…”

The word “in” is repeated in the last sentence on page 57.

There is a typo on page 100 in the sentence beginning with “Learning also played an important role…”

The word “in” is missing from the sentence beginning with “The UC Science Team email survey respondents agreed with the Forest Service…”

There are extra words in the last sentence on page 155 which begins with “Use webinars can be used to transfer information…”

Figure F-5 on page 42 is confusing and doesn’t illustrate information well. The numbers are small and there are too many lines and colors.

I thought the use of the self-organizing maps was very interesting, since it illustrated that public discussion stayed focused on content information, but contentious and critical issues (presumably topics such as funding and roles and responsibilities) continued to dominate discussions. I would assume this meant that the more general public discussions remained focused on content, but the meetings among partners who were most involved in the project were dominated by administrative and organizational issues. This data would be interesting to examine more closely.
I am not as clear on how the affiliation network analysis contributed to the study. The lessons learned was that it was a valuable tool to characterize social dynamics, since it illustrated a strong clustering around the northern and southern meeting locations. However, it did not seem to contribute any more information than what was presented in Figure F-8 on the attendance at SNAMP event. Further, I was not clear on the value of characterizing social dynamics to this adaptive management process.

I also do not see the value in the story about people mailing in sox for the fisher project. I understand that it was meant to illustrate the value of the web presence, but I do not think the level of detail provided, including pictures and graphed locations, was warranted or added much value to a generally scholarly report.

I see great value in having this report condensed into key findings and recommendations that could be used as a reference for those designing similar processes. It is too long and inaccessible in its current form to serve the purpose of providing a model for others to use.
June 12, 2015

TO: Bill Frost, Associate Vice President, University of California Division of Agriculture and Natural Resources, University of California-Davis
CC: Joan Taylor Warren
RE: Review comments on the California Spotted Owl Team Report

The California Spotted Owl Team Report is a component of the Sierra Nevada Adaptive Management Project (SNAMP) which addresses uncertainty over forest fuels management in the Sierra Nevada and potential adverse effects of the population dynamics of the California Spotted Owl (CSO). A key SNAMP objective was to evaluate the impact of Strategically Placed Landscape Treatments (SPLATs), a forest fuel treatment, across a number of response variables including CSO demography. The primary tasks of the Owl Team were to: 1) assess the impacts of forest management and vegetation change on owl demography over the past 20 years on the Eldorado demography study area, and 2) project the effects of wildfire on the quantity and quality of owl habitat in the Last Chance study area over the next 30 years, with and without SPLATs.

The California Spotted Owl Team Report is divided into two sections. The first section is based on a recent publication by D.J. Tempel and others published in Ecological Applications (Tempel et al., 2014). This publication, based on data collected from a long-term demographic study of CSOs populations on the Eldorado National Forest, evaluates the effects of forest management practices on CSO populations. I have thoroughly reviewed this publication and found the paper to provide information highly relevant to the possible consequences of forest fuels management on California Spotted Owl (CSO) population dynamics.

I concur with the major conclusions drawn in Tempel et al. (2014) regarding the potential effects of medium-intensity harvest practices on CSO demography. Specifically, these authors focus on possible adverse effects of forest management on both CSO reproduction and survival rates. I believe the data they collected, and the analyses they conducted, to explore the relationship between covariation in habitat and vital rates are compelling. Their finding of a strong association between demographic performance and the key habitat attributes of canopy closure (> 70%) and the density of large trees (>~60 cm dbh) is well-supported by their data and consistent with the majority of published studies on spotted owl habitat ecology (both Northern and California Spotted Owls).

A key concern pointed out by Tempel et al. (2014) is the possible trade-off between fuels management to reduce fire risk and the short-term demographic consequences of these management practices. Given that CSO populations in most areas of the Sierra Nevada are in decline (Blakesley et al. 2010, Conner et al. 2013, Tempel and Gutierrez 2013), whether it is prudent to introduce an additional source of demographic cost to the CSO is debatable. The life history structure of Spotted Owls (delayed age at first reproduction and limited reproductive potential) constrains them from rapid population growth should their populations become both small and geographically distinct (Noon and Biles 1990).
The key unknown in the proposed management practices to reduce fuel loads is the likelihood of future loss of habitat due to an increased fire frequency or severity. Given the impossibility of knowing the future until it arrives, the proposal by Tempel et al. (2014) to focus fuel treatments in dense stands and to emphasize thinning from below to maintain high levels of canopy cover and vertical stand structure seems a very logical recommendation. Climate change projections for much of the Sierra Nevada region suggest an increase in fire frequency and intensity (Geos Institute 2013). These projections suggest that silvicultural practices to reduce fire risk are justified.

In the second section of the California Spotted Owl Team Report, the results of a prospective analysis of the possible effects of fuels treatments on CSO habitat suitability are modeled. The Team simulated forest growth 30 years into the future under four combinations of modeled wildfire and treatment: treated with fire, untreated with fire, treated without fire, and untreated without fire. Effects of treatment scenarios were evaluated with a habitat suitability index model using canopy cover and large-tree measurements as predictor variables. Estimates of population growth rate and equilibrium occupancy were made for four spotted owl territories within the study area for each scenario using the statistical relationships between forest structure and CSO population parameters reported in Tempel et al. (2014).

The 30-year simulations demonstrated that the effects of fuels treatments are contingent on the probability of fire occurrence. Treatments had a positive effect on owl nesting habitat and demographic rates up to 30 years after simulated fire, but a negative effect in the absence of fire. Simulations results showed fuels treatments to reduce territory fitness and occupancy in the short-term, but with the potential to promote higher CSO fitness and occupancy after 30 years in the event of high-severity fire. The report concluded that fuels treatments may provide long-term benefits to spotted owls if fire occurs under extreme weather conditions, but can have long-term negative effects on owls if fire does not occur. Similar to the conclusions of Tempel et al. (2014), the net benefits of fuels treatments on CSO habitat and demography will depend on the future probability of significant fire events.

In general, I found the modeling approach, types of models used (e.g., FARSITE and Forest Vegetation Simulator), data-based model inputs, and inference to be defensible. Actual on-the-ground field measurements, both before and after treatment, are a real strength of this study.

Simulations and model projections are inherently less certain than retrospective analyses based on “hard” data. Despite these limitations, the modeling and analyses reported in this section of the Report are quite useful and robust. The data input to the models was based on extensive field survey data collected both before and after fuels treatments. In addition, field measurements were augmented with remotely sensed data (e.g., LIDAR imagery) to detect post-treatment changes in forest structure.

The conclusions drawn in section two of the Report are consistent with those in section one. Specifically, the net effect of fuels treatments on CSOs depends upon the unknown probability that high-severity fire effects will adversely affect CSO habitat. However, estimating the likelihood of severe fires at the spatial scale of individual CSO territories is almost impossible
given the complexity of fire events and our current understanding of fire dynamics in
topographically diverse landscapes. I agree with conclusions drawn in this section of the Report
that fuels-reduction treatments will likely have short-term demographic consequences to CSOs
but the potential to provide long-term (30-year) benefits the event of severe and extensive fires.

I have a few minor suggestions/caveats on section two of the Report:

- Inference is limited because empirical analyses were available for only four CSO
territories. Given this small sample, and inferences to population-level responses need to
made carefully.
- The logistic model used to estimate the suitability of nesting habitat is based on just two
predictors—canopy cover and large tree density (equation 4 in the report). These are
logical predictors to include in the model but it is unclear how well the model fit the data.
I suggest an ad-hoc measure of model adequacy using some sort of pseudo-R2 metric.
For example, the following metric could be used: \( R^2 = 1 - \ln(L_m)/\ln(L_0) \), where \( L_m \) is
the likelihood of the fitted model, and \( L_0 \) is the likelihood of the intercept-only model.
- A similar comment to the above is relevant to equation 5 as well.
- In equation 6, how was female age assigned to future projections?

Striking the appropriate balancing between full CSO habitat protection versus some level of
active fuels treatment within CSO habitat should be done gradually and accompanied by ongoing
demographic monitoring of both CSO and CSO prey species (specifically, flying squirrels).
Thus, a continuation of the ongoing CSO demographic studies with a specific focus on the
effects of fuels treatments on CSO reproduction and survival should be part of an overall
adaptive management program. Importantly, the adaptive management program should have
specific decision points, expressed in terms of target CSO demographic thresholds, to constrain
levels of fuels treatments if they result in unacceptable risks to CSO viability (Noon 2003,
Nichols et al. 2012).

References:


Gerrard. 2013. Realized population change for long-term monitoring: California spotted owl case

Geos Institute. 2013. Future climate, wildlife, hydrology, and vegetation projections for the
Sierra Nevada, California: A climate change synthesis in support of the vulnerability
assessment/adaptation strategy process.

Nichols, J. D., M. J. Eaton, and J. Martin. 2012. Thresholds for conservation and management:
structured decision making as a conceptual framework. In Application of threshold concepts in


June 2015

Review:

Overall, the spatial team has done a lot of work and definitely have fulfilled the goal of the SNAMP project. The number of peer-reviewed papers (11 published) speaks by itself. However, I wouldn't claim the work as technical breakthroughs. To promote the wide adoption of lidar for forest management, two goals have to be achieved: the high accuracy of the derived veg products compared to the ones from conventional methods, and the capability of applying the lidar-based methods to larger area. Unfortunately, the two goals are rarely met at the same time in most of the current methods. Most methods can get good accuracy at local scale with lidar, but they often require a lot of inputs from the user and fine tuning. When the methods are applied over larger scales, they could break down simply because of the computation demand and the intensive inputs from the users to achieve reasonable accuracy (e.g., how long does it take to apply the point cloud-based method to map trees for the whole study area instead of just those in the field plots? how accurate is it to apply the OBIA method to detect down logs over other areas?). I have no intent to play down the works the spatial team has done, but simply mention a direction in which remote sensing scientists all need to make big breakthroughs.
REVIEWER #8
SNAMP – PACIFIC FISHER TEAM REPORT

Peer Review

Title: Sierra Nevada Adaptive Management Project (SNAMP), Appendix D: Fisher Team Final Report

Authors: Rick Sweitzer, Craig Thompson, Kathryn Purcell and Reginald Barrett

GENERAL COMMENTS FOR THE AUTHORS

I do not know whether this Report will ever stand alone. If so, it needs a succinct introduction to the Sierra Nevada Adaptive Management Project as a whole. As is, the fisher project lacks context at the start and gains context only piecemeal as land management actions are introduced.

In your list of objectives, both in the Executive Summary and in the body of your Report, you emphasize mortality as the population limiting factor. I know that you know that reproduction can limit populations just as well as mortality but you do not mention in your objectives that your population could be limited by reproduction. Given the probable small litter sizes of the fishers in your population, reproduction could very well be a limiting factor, maybe THE crucial limiting factor. You need to mention reproduction in your objectives.

For your Leslie matrix, you used single values for adult reproduction and adult survival. Thus, F4 = F5 = F6 = F7 = F8 and P4 = P5 = P6 = P7. You should use just F4 and P4 in your matrix on page 33. In building your matrix, you have assumed that no fishers live beyond page 8, which is not true. If you replace the 0 in the bottom right corner of the matrix with P4, your matrix will allow survival beyond age 8. Given that you use constant adult reproduction and survival, you can actually collapse the matrix to a 5x5 matrix, assuming you are willing to let model fishers live beyond age 8.

Given that F1 = 0, why not simply put 0 in the matrix? As far as I can tell, F2 = 0, too, for your matrix. Although you state that the matrix was built to estimate the population at 1 month following birth (approximately 1 May), your estimates of litter size appear more accurate for later ages, probably 2-3 months old. And your estimates of kit survival starts at age 6 months. In the end, I am confused as to whether your matrix estimates population size just before or just after
reproduction. Do your fishers die and then reproduce or do they reproduce and then die? Leslie matrices can work either way.

You should state that your estimates of litter size are biased low because cameras do not always show all kits in a litter. Opposing that bias, your estimates of kit survival are biased high, because you have few data on survival of kits to weaning and no data on survival of kits from weaning until trapping in October.

Also related to the biased estimates of reproduction and kit survival, the elasticity analyses that I did for the population model used by Lewis et al. (2012, PloS One) and Powell et al. (2012, Martes 2009 book), showed that the model was most sensitive (via elasticity calculations) to estimates of litter size and kit survival. I do not know why you found your model not to be most sensitive to those variables, contrasting with our results. Sensitivity analyses sometimes yield results that contrast with elasticity analyses (a substantial literature on the differences exists, dated to 10-15 years ago). I encourage you to do an elasticity analysis if you did not.

Writing about the biases for litter size and kit survival reminds me that your estimates of dispersal distance are biased low. This point is worth making clear right from the start. Your bias is undoubtedly smaller than that for research not so flight-based. When trapping fishers for reintroduction in the northern Sierras, we trapped a male 50 km from where he had been marked as a kit on the Hoopa Reservation.

Page 89 – Home range overlap. You must do your analyses using raster values for the entire utilization distributions and not for ranges of contours. Using the contours is a modest improvement over using everything with the 95% contours. Nonetheless, to gain truly good understanding of overlap, you have to use the entireties of the utilization distributions. Really, no study of home ranges can be done well using vector analyses. You need to use raster GIS.

Page 90, Figure 31 and elsewhere – You must justify defining a core as the 60% contour. Why 60% and not 55, or 42, or 73.1415927? Really? Using 60% is arbitrary and not based on the behavior of the fishers, as far as I can tell. If you zeroed in on 60% after doing some undescribed analyses of your fishers’ utilization distributions, then that could be OK but you need to explain the analyses.

Page 114 – Your result that occupancy was not negatively associated with fuel reduction could have been caused by lack of power in your analyses. You should report power so that readers (and you) understand the strength of your result.

I recommend that you be careful about occupancy vs abundance or density. Populations can change in abundance without changing occupancy. You report estimates both. You should recheck how you address occupancy and abundance to make certain that you do not slip back and forth without realizing it, thereby misunderstanding population change or stasis.
I have some specific suggestions for wording saved on a Word file with TrackChanges. Let me know if you want them. If you do a lot of revising, my suggestions might end up being irrelevant.

Does SNAMP stand for Sierra Nevada Adaptive Management Project or for Sierra Nevada Adaptive Management Plan? Which is it? You are not consistent and must be.

I suggest using the word “sex” throughout and deleting all references to “gender”. “Gender” has become a politically correct word to use but it does not have a serious place in biology. Gender refers to social roles that only humans play. In fact, “gender” originally referred to linguistic categories of nouns in many languages. In German the linguistic genders are referred to as “male”, “female” and “neuter” (a girl is neuter). In Danish the linguistic genders are “common” and “neuter”. Thus, gender is languages is tremendously inconsistent with respect to sex. Justice Bader-Ginsburg began the move to use “gender” to mean “sex” for humans because she noted that the word “sex” conjures up ideas in men’s minds that muddled the legal questions that judges and lawyers must handle. I assure you that fishers do not get distracted by the use of “sex” in the ways that human men do and fishers do not have trouble with social roles or trouble with how to refer to gay, lesbian and trans-gender fishers. You do a little switching back and forth, which implies that you use “sex” and “gender” to have different meanings. They do have different meanings but I do not really think that you have used them to mean different things.

Use “sex” for all non-human animals.

Cameras are not traps, they are cameras. They do not capture animals or photographs, they take photographs or, better yet, cameras photograph animals. I recommend removing all references to cameras as traps. In many places you do refer to cameras simply as cameras, den cameras for example. Your survey cameras were cameras also and not traps. Just use the simple language you would use when describing a person using a camera to photograph something. I know that “trap” and “capture” are jargon often used today. Avoid them (don’t fall into their trap). Your readers will thank you.

I strongly urge you not to use acronyms. You are simply stuck with some acronyms, like SNAMP, which is horrid. Had I been in charge, I would have avoided that tongue twister like the plague. You should minimize the acronyms and use only those that are absolutely necessary.

Asking readers to remember acronyms is not a big request, I know, but readers who have just read another paper that required them to remember abbreviations for other things, perhaps some with the same acronyms or abbreviations, can easily forget yours. Recently I reviewed a manuscript that asked readers to remember abbreviations for 3 types of forest, one of which was mixed deciduous forest, abbreviated as MDF. Before reading that manuscript, however, I had been reading a woodworking magazine and all I could think when I read “MDF” was “medium-density
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SNAMP – PACIFIC FISHER TEAM REPORT

fiberboard”. You do not want readers to be confused like that. Spelling out whole names is worth the space used to be clear. If you can think of ways to shorten long names, that is good. For example, “mixed forest” worked for “mixed deciduous forest”. Remember, use abbreviations and acronyms only when they serve to improve your ability to communicate with your readers. Do not use abbreviations and acronyms to save space, to save you from having to write out long names many times, or to make you think that your manuscript is important because it has a bunch of capital letters strewn through it. Do not use acronyms and abbreviations simply out of habit when they are not needed. By and large, acronyms are a sign of authors who have not been thinking. Given that your goal should be to help your readers understand your research, avoiding acronyms is your best approach.

Define all your acronyms the first time you use them, both in the Executive Summary and in the body of your manuscript. You did not define SPLAT (another horrid acronym) when first used. Either that or I simply had difficulty finding a definition when I needed it.

You must include a table of acronyms. I did not have time to read your entire manuscript in one shot. When I picked up the manuscript to re-start reading after a break, I could not remember all the acronyms and had trouble finding their definitions.

All the passive verbs got b-o-r-i-n-g: “was” this and “were” that. Please revise to use active verbs. I know that unimaginative writing with active verbs can come be boring as well, with “We did this” and “We did that.” Even that unimaginative writing is less boring than passive verbs, and I know that you can write imaginatively to make reading more fun.

In your Executive Summary, I am not always certain whether you are writing about individual fishers or fisher years (1 fisher yielding data for 2 years is 2 fisher years). When you had data for individual fishers for >1 year, did you test for independence, did you average data for individuals over years, or did you do something else? For my own data over the past decades, I have seldom found that using animal years showed an animal effect. Any effects of individual animals appear overwhelmed by other sources of variation.

The term “subadult” is possibly the most ill-used term in wildlife biology. What exactly is a “subadult”? Is a “subadult” not an adult? If so, then use “juvenile”. If a “subadult” is an adult, then use simply “adult”. If a “subadult” is an adult that has special characteristics, such as not being an effective breeder, then another term, such as “non-breeding adult”, would be more clear. Reading your manuscript, I had trouble remembering whether a “subadult” was 1-2 years old or 2-3. I recommend not using the term “subadult” but using ages.

The term “subadult” can be particularly confusing for males, since males just turning 2 are probably not effective breeders, while females are effective breeders at that age. Thus, females can be considered adults at age 1 because they breed, yet they do not produce their first offspring till they are age 2. Males are one year behind (maybe 2?).

If you insist on using “subadult”, define it clearly in both your Executive Summary and in the
body of your Report. You should define it in your table of acronyms and abbreviations, too.

You used 2-year periods to estimate vital rates and you used 2-year groupings of camera data. You appear to be good about stating which 2-year groupings are the topic of discussion but I could not remember how they differed. I suggest you help readers somehow keep these groupings straight.

Page 7, bottom paragraph - You imply that the genetic data contradicts the 400 km break having been caused by logging and trapping (and perhaps change of climate at the end of the Little Ice Age). Given the number of canyons that break the Sierras from the Feather R down to the Merced R, given that fishers do not cross the Merced enough now to colonize the north side, and given the genetic subdivisions caused by canyons in the present fisher population in the Southern Sierras, the fisher population in 1800, 1850 or 1900 could well have been continuous from Yosemite to Mt Shasta and still not have had genetic exchange between the 2 extremes for >1000 years. You should acknowledge this very real possibility.

Page 14, just above fisher - This sentence is wrong. The goal was to MAINTAIN \( \geq 20 \) fishers on the air. The number 20 was reached in July 2008 but not the goal of maintaining that number. As written, “milestone” refers to the goal, not to the number of fishers on the air. This sentence needs revising. You have a similar sentence elsewhere in the Report somewhere.

A grid is a set of (usually) perpendicular, intersecting lines. A common grid that we (both you and me) use in our research is the UTM grid. Each square outlined by grid lines is a cell within the grid. Throughout your Report you use the term “grid” to mean “cell”. What you did was lay out a grid of lines that intersected to make 1x1 km cells. Usually your misuse of “grid” is just irritating but sometimes it can be misleading and lead to misunderstanding. Use the Search & Destroy capability of Word to find every use of “grid” and make certain you do not mean “cell”. I bet that you will have to change 99% of them. An easier way to make the change would probably be to change every “grid” to “cell” and then check for the minuscule number of changes that should not have been made.

You have some ambiguous citations needing and “a” or “b”. One page 31, bottom paragraph, to which publication by Zielinski et al. (2013) do you refer? Check your citations.

Page 22, Background paragraph – Avoid opinion. I have seen no data and analyses that show that den sites limit any fisher population. I have read assumptions to that effect but not data and analyses. If you know of data and analyses that support the first sentence in this paragraph, then cite them. Otherwise, consider den shortage to be an hypothesis that needs to be tested but is not KNOWN to be a major problem.

Page 27, paragraph 2 – Zielinski & Duncan (2004) did not report any data on prey numbers and,
therefore, your reference to “insufficient numbers of prey” is an (albeit minor) unethical citing of a reference to provide false support a statement. Check all of your citations to be certain that the references actually support the statements you make.

Page 31, bottom paragraph – Remember that occupancy and abundance are not the same thing. Zielinski et al. presented data on occupancy, not abundance. You do not know from their research whether abundance changed or not.

Page 36, top – To be able to compare home ranges among fishers using a kernel estimator, all utilization distributions developed for all fishers should have the same “h”. To be able to compare utilization distributions across studies, both “h” and the kernel used must be known. For example, the bivariate normal kernel emphasizes peripheries more than does Silverman’s k2 because a bivariate normal distribution has infinite tails. You must state what “h” and what kernel you used.

Page 42, 2nd paragraph – What do you mean by “centroid”? Do you mean the geographic mean of all locations, the point with the highest probability of use, or something else? Be clear.

Page 45, 2nd paragraph – Choosing the kernel is also critical and is critically important for being able to make comparisons across studies. Studies that use different kernels can not be compared quantitatively.

Page 45, bottom – Discarding data in a no-no. This is a point that Ken Pollock makes strongly and regularly. Discarding data also makes no sense, really. You worked hard to collect those data, so why throw some away? You will have better results if you weight locations by the times between them and use all locations (locations farther apart in time than autocorrelation time get full weight). On the flip side, all locations of a given animal are always biologically correlated, otherwise the animal would not have a home range. Animals choose where to go and how fast and when because of background information they have. From this point of view, statistical autocorrelation makes no biological sense and should be ignored.

Page 74 – The reference to Figure 10 is an error. Figure 10 shows that number of males wearing collars and not the number of males in the population.

Page 75, Figure 18 and elsewhere – Numbering the y-axis with numbers to 2 decimal places is false precision.

Page 100, line 2, “By contrast” – Not much of a contrast here.

Page 101, paragraph 2, line 3 – I know of no data that demonstrate that the habitats around dens protect fishers from predators or weather. Den holes can protect fisher kits from predators too.
large to fit into the holes and deep holes can protect kits from predators that might reach into the holes. Den holes do keep some weather out but how much depends on how deep the hole is and how wide the entrance is. Does anyone know why martens do not raid fisher dens and eat young?

Page 106, paragraph 2 – I think it worth mentioning that the single female that did not recover from handling would have died anyway, had you not trapped her.

Page 115, paragraph 2 – Complex structure on the forest floor probably helps fishers catch prey, as it does for martens. So, fuel reduction might affect prey populations, might affect fishers’ hunting abilities, or both.
July 30, 2015

I have reviewed the “Fire and Forest Ecosystem Health Report” component of the Sierra Nevada Adaptive Management Project per the request. Based on my 20 years of experience with fuel management and fire behavior, and my working knowledge of the literature on these topics, I find this report extremely well assembled. The team has an impressive track record in this area of study, made thorough use of the literature (and beyond their own work), employed solid methodologies for this analysis, presented a wide range results, and drew solid conclusions from those results. Beyond this, they give fair treatment to potential weaknesses in the design and in various modeling approaches. Overall, as a reviewer, I found that refreshing.

The report concludes, and I concur, that there are reasonable improvements to fire behavior/effects following the treatments, and that those improvements are limited by the intensity and extent of those treatments—only a relative small percentage of the two areas was treated, and a very small percentage treated with prescribed fire; the Sugar Pine unit had no prescribed burning. Given the importance of prescribed fire in fuel treatments and the effect of residual slash on surface fuels following mechanical treatments, it is no surprise that the improvements to fire risk were modest.

The larger advantage of the treatments as implemented was apparently associated with tree and stand vigor and growth efficiency. This could be of great importance given climate change. Increased vigor will aid in the growth of larger trees, there resistance to disturbance agents (including fire), and the resilience of ecosystems as larger percentages of trees and basal area survive into the future beyond one of more future wildland fire events.

In summary, this synthesis is sound despite being a case study on only two sites, that treatments were delayed (limiting the ability to measure post-treatment responses), and that some plots required relocation. The authors adequately handled these design limits, the need to span scales among the different analyses, and the need to model fuel succession and fire behavior. I found their report solid, well presented, and well defended.

Thank you for the opportunity to review it. I hope my comments will be helpful.
Review comments

Sierra Nevada Adaptive Management Project / Fire and Forest Ecosystem Health Report

Overall comments:

The body of the report presents a thorough professional analysis of fire and forest health in the SNAMP. The concepts guiding the analysis were based on an informed assessment of the scientific literature, although the definition of “forest health” was limited and overlooked a potentially useful approach, ecological restoration. The authors applied appropriate methods of sampling and modeling. The experimental design was well-conceived but factors beyond the control of the authors prevented its full implementation. In any event, the field sampling was quite intensive. As with any modeling exercise, there are a number of uncertainties associated with the predictions. The authors did a good job of describing potential sources of error. The authors may not have been tasked with addressing how the treatment effects may be altered as climate changes, but any modern assessment of multidecadal treatment effects should have such an assessment, in my opinion. Detailed comments (below) may be useful to consider but are not essential.

The Appendix to the report, titled “Appendix A.1, Spatial and temporal components of historical fire regimes in a mixed conifer forests [sic], California”, describes a fire history reconstruction study done in a portion of the SNAMP landscape. The study in the Appendix appears to have been carried out competently in terms of sampling and some of the analysis. However, there are additional analyses that would have been important to include. In addition, the text suffers from some unsupported statements, some exaggeration, and a remarkable number of spelling and grammatical errors. In its current form, the Appendix is not suitable as a professional report.

Detailed comments on the body of the report:

1. General comment on units: in part of the text, both English & metric units are used, in one part metric only (e.g., L. 429), and in the Tables English only. Please be consistent.
2. Abstract p. i: should read “in order to quantify...”, not “quantity”
3. L. 16: of the three references here, only one is relevant to the species in the present study and mentioned on L. 12-13. The Brown and Naficy papers are on ponderosa pine forests in South Dakota and Montana, respectively.
4. L. 25: would be helpful to give a quantitative range of reduced fire risks associated with treating 30% of the area, rather than just saying fire risk “can be decreased”. Presumably fire risk would be “decreased” due to treating 1% of the landscape, or 99%. The point Finney was arguing is that “strategically” located treatments would give a disproportionate benefit in term of reducing the likelihood of severe fire.
5. L. 38-40: awkwardly worded sentence starting “This concern...”
6. L. 45: besides adding sediment (due to soil disturbance associated with treatment?), in what other way would the treatment “lower water quality”?
7. L. 57: should read “forests OF the Sierra Nevada...”
8. L. 61: Westerling's study was over a much larger area, the western US.
9. L. 64: the “fire deficit” described by Marlon et al. appears to be mainly the result of fire suppression, as the authors stated, but it reflects a deficit of surface fires, the theme of the present study, as well as severe fires, which in the context of this study are to be avoided. The Marlon reference blurs the distinction between surface vs. severe fire regimes & therefore is not helpful in the present context (in my opinion).

10. L. 93: would be useful to give a quantitative figure, rather than “noticeable reductions”.

11. L. 106-110: great point about buying time. Note the reference to “restoration activities”, see comments below.

12. L. 115-158: the authors give a reasonable presentation of the literature on “forest health”, a concept with strengths and weaknesses. They make a logical case for the selection of tree mortality and tree growth as useful indicators of tree health and, roughly, “forest health” in this study. However, the review is brief and limited. A surprising omission, in the light of the Forest Service’s emphasis on ecological restoration and the references to restoration in the text itself, is the literature on ecological restoration. A basic element of restoration is the reference condition, which includes as much information as possible about ecosystems prior to recent human-caused degradation: plants, animals, forest structure, ecological processes, influences of non-industrial human societies, etc. See for example the Primer by the Society for Ecological Restoration. Many of these elements are mentioned in the text here and in the Discussion, but not in an integrated manner.


14. L. 183: not clear whether the figure of 1182 mm refers to total precipitation (water equivalent) or to snow depth?

15. L. 208-218: it’s not clear what is the relationship between the cited studies (e.g., Scholl and Taylor 2010) and the fire history study in Appendix A1. Would be helpful to clarify the distance to the previously studied sites, the species, etc. As written, since the cited studies are used to describe the pre-Euro-American fire regime, it is not clear to the reader why new fire history data were collected. (Incidentally, given the long history of Spanish colonization, what do the authors consider to the date of “Euro-American settlement”?).

16. L. 220, Methods section: field sampling was logically designed and a substantial amount of data was collected.

17. L. 239: “were navigated to using…” is awkward.

18. L. 240: should read “Garmin”.

19. L. 264: please give more detail about prescriptions: was there a target forest structure? Residual basal area?

20. L. 282-292: the authors did a good job of assembling the treatment boundary data but this section highlights the ridiculous situation of the lack of detailed spatial information about treatments in the National Forests. The Forest Service doubtless retains all receipts for repair of its vehicles or purchase of a desk, but is incapable of adequately describing the contracts for management treatments on FS lands!

21. L. 299: the Lidar data should be explained to the reader. The report references an Appendix B which was not included for this review.

22. L. 323: why were numbers of seedlings randomly generated? A citation is given but the logic should be explained and justified.

23. L. 326-331: please clarify the role of FVS & FFE. Were any fires simulated in FVS-FFE? Or were these models used solely to grow trees & track fuels, in the absence of fire, with all fire behavior simulations occurring in Farsite or FlamMap?

24. L. 342: describe the use of Lidar to describe topography. My (limited) understanding of Lidar was that different measurement approaches would be appropriate for measuring vegetation vs. measuring the surface underneath the vegetation.

25. L. 344: the use of a “problem” fire is traditional & reasonable, but the reader would expect a discussion of the limitations of the concept.

26. L. 355: was spotting enabled in the Farsite simulation? See later comment about windspeed. In a real fire, higher windspeeds and spotting would be a critical combination.

27. L. 359: should read “used A command-line...”
28. L. 415: true that the design “accounts for” changes, but the BACI design is limited, with no or limited replication.
29. L. 430: is the term “conditional burn probabilities” a standard term? It sounds confusingly similar to the concept of “conditional crown fire” suggested by J. Scott. Please clarify and please also explain how the authors dealt with the situation called “conditional crown fire”, when the canopy bulk density is sufficient to carry crown fire horizontally but the canopy base height is too tall for simulated vertical propagation of the fire.
30. L. 434: where does the 2-m flame length threshold come from?
31. L. 446: it seems quite illogical to calculate the trees that were intentionally cut as “mortality”. Why was this done?
32. L. 495: delete the word “on”.
33. L. 512: LAI appears here as a critical variable, yet it was not measured in the Methods. Where did the LAI values come from? How reliable are LAI estimates?
34. L. 644-647: complicated way to say that trees from dense stands have a large growth response when released by thinning...
35. L. 693: appropriate discussion of the weaknesses of the fire behavior model. See also comment on windspeed below.
36. L. 709: reasonable discussion of CBH modifications.
37. L. 720: good discussion of fuel model issues.
38. L. 738: Lidar needs more explanation.
39. L. 762-764: authors say there is no explanation beyond self-thinning, but what about drought effects during these years?
40. L. 798 & 806: here, very late and only mentioned in passing, are key issues of ecological restoration and global change. These themes merit more discussion in the context of the treatments.
41. Table 2: these windspeeds are extraordinarily low, averaging 6-10 mph. Are these mid-flame or 20-ft windspeeds? Are the problem fires in the Sierra Nevada really associated with such low windspeeds? Please clarify, justify, or change if appropriate.
42. Table 4: the Last Chance site seems quite open at the beginning, BA 133 and TPA 252. Is this really a fuel problem?
43. Table 4: what is Lorey height & why was it used? Please also present actual height values.
44. Table 6 & 7: why such a big difference in mastication effects on fuels, with a doubling at LC but little effect at SP?
45. Table 7: column headers out of line.
46. Table 8 & 9: shouldn’t the treatment impact be “delta mu” rather than “mu”?
47. Figure 7: repeats data given in Table.
48. Figure 9: illustrates the minimal (negligible?) impact of these treatments.
In the Water section, it states: “Primary goals were to measure and model the hydrology of paired headwater catchments and streams in the firesheds, to scale this modeling to the fireshed, and to predict and assess impacts of Strategically Placed Landscape Treatments (SPLATs) on water-cycle attributes over a range of inter-annual climate conditions and across the broader forest landscape.”

This review considers Appendix E-I Water Quantity Observations and Modeling and E-II Water Quality.

Water Quantity Observations

This proposal takes a paired watershed approach to investigate the impacts of forest treatments on the hydrology of headwater catchments (~1km²) and firesheds (>40km²) in the California Sierra Nevada (S-N). There have been a few similar studies, but none of this in-depth nature in the S-N. These catchments and firesheds are well described in terms of the underlying geology, soils and land cover (predominantly mixed conifer forests). Overall this is a fairly sound approach that has good chance of being successful. However, it is not clear how physically applicable the hydro-ecology model is for the area, how the scaling will be done, and there are numerous discrepancies in the description of methods. The discrepancies make it somewhat unclear about the overall methodology; it is possible that some of this was provided in the main text of the document, but I did not receive that. A schematic and better map(s) would improve the illustration of “how the pieces fit together.”

While I outline a number of shortcomings of this proposal, I want to state how important this work is, especially in light of the 1,500 fires currently burning in California. The overall plan is solid enough that it will be successful. I suggest that my comments below be considered when this work moves forward.

Specific Comments

What is unclear is the nesting from the hydrology of the headwater catchments to the firesheds. In the text, figures, and tables, a variety of “areas” are mentioned and it is unclear which are sub-basins are part of which basins. For example, the Big Sandy and Speckerman headwater catchments are stated to both drain into the South Fork of the Merced River, then part of the Sugar Pine research catchment, and then flowing into the Lewis Fork of the Fresno River. While there are streamflow data from different watersheds used in the analysis, these disparities make it difficult to know what flows into what, i.e., how the pieces fit together.

It may have been stated in the main document, but not in this Appendix, what the limitations are of scaling from a headwater catchment scale to the fireshed scale? (Note: fireshed is not defined in Appendix E). Bahro and Barber [2004] define “Firesheds (as) large (thousands of acres) landscapes, delineated based on fire regime, condition class, fire history, fire hazard and risk, and potential wildland fire behavior.” Such a physical “space” does not necessarily match with the headwater catchments. It is proposed to use areas (headwater catchments) of similar characteristics (geology-soils) to the firesheds to scale, but there issues also with land cover forms, canopy density, etc. Perhaps implementing a mosaic approach at a coarser resolution than the 20-m modeling resolution that is proposed to have more of a direct physical applicability/transferability.

It is unclear what time series of meteorological (precipitation, temperature, wind speed/direction, radiation) and hydrological (snow depth, soil moisture) data are collected on site
and used to drive and evaluate the model, respectively, and which data are from a nearby station and used in the analysis. The first paragraph in the methods states that “precipitation from the northern sites was input from the Blue Canyon meteorological station ... 14 mi to the northeast.” Were the upper and lower elevation stations (I assume that are within the study area) used, or was a different station used. Is this for the characterization or the modeling, or both?

The use of lidar to assess before and after SPLAT canopy characteristics is an important advance from any previous study as it more specifically estimates changes, due to a finer resolution of data (likely 1-2m lidar-based vs. 30-m Landsat-based) and the ability to define canopy structure using lidar. However, it is unfortunate that they then decide to use basal area and canopy cover to estimate shrub cover (equations 1 and 2) since these equations are poor fits and it could be possible to use the lidar data to evaluate understorey conditions.

The hydro-ecology model does build on two hydrological models (TOPMODEL and DHSVM) so the hydrological component of the model should be adequate. The model was tested with very good calibration and evaluation statistics; runoff volumes matched well but peaks were often underestimated. The equation used to model long radiation is simple, and perhaps an improved version can provide more snowmelt for peak flow? An alternative is to consider another model that is more suited for snowmelt systems. Mid-winter snowmelt and/or rain-on-snow appears to be important, and will occur more under a changing climate, especially warming and/or less precipitation. Such processed may need to be further evaluated.

**Water Quality**

I will state at the onset that water quality is not my field of expertise, but I understand the processes and have been involved with some monitoring and modeling of water quality. The writing (and references) are slightly different in this section from the last one, so that leads me to believe that there are different experts involved with the water-based work. This is very appropriate. Overall, the sampling and analysis that is proposed is thorough and very reasonable.

Similar to the limited amount of paired studies of water quantity in an environment such as the S-N, there have also been few water quality studies in the S-N or similar environment. Thus this is important work for the research community, as well as for California.

The combination of *in situ* and manual sampling is important; some data are not well collected by automated samplers. The suite of water quality variables (temperature, conductivity, dissolved oxygen, turbidity, major cations and anions, stable isotopes) to be measured is comprehensive, and it is very relevant to measure all of these variables. The measurement of channel bed movement seems somewhat novel and could be quite useful.

Changes in the hydrological systems due to forest treatments, including fire, will alter water quality and it is crucial to understand these impacts. This is especially true as these headwater systems are water sources. They seem to have put together a good, comprehensive plan to monitor and evaluate a variety of water quality variables.
Comments on SNAMP Whole Document

I have participated in SNAMP in the following ways (choose all that apply) *

Attended SNAMP meetings, Just the last meeting

Next, please tell us how much you agree or disagree with the following statements. (Strongly agree, Agree, Neutral, Disagree, Strongly Disagree).

- This document is well written: Agree
- This document summarizes research findings well. Agree
- The research findings described in this document are understandable. Agree
- The research findings described in this document can be applied to Sierra Nevada forest management. Agree

The most interesting research findings in this document are: I'm completely biased and drawn to the fisher work.

The most important findings from this document to apply to Sierra Nevada public forest management are: The ways to incorporate the habitat needs of multiple (at least the fisher and SPOW) species in an uncertain environment.

The research findings that I found least applicable to Sierra public forest management are: The quotes of the four people interviewed in the study units. It wasn't clear how the four were selected, and it seemed more like gossip than generalizations from a random sample. For example, a long-timer saying 'city people are unfriendly' could easily be paired with a newcomer stating that "long-timers are unwelcoming." I would prefer there was a clearer study design, or just put these anecdotal comments in an appendix.

The biggest questions I have after reading this document are: How will this research be integrated with other research and how will the management implications be distributed?

The biggest concerns I have after reading this document are: 1. The graphics (particularly in the integration section) were chosen to simplify the results, which is a good idea. However, it doesn't have the intended result because in this study, the changes in forest structures were not consistent by treatment. As a result, the simplification may lead a manager to make a decision opposite of what was intended. Instead of using the four treatment types, I suggest grouping results by the changes in forest structure that came about as a result of the treatments. 2. The spatial scale of the treatments was limiting in making conclusions. The document states this several times, but it is critical to note that this was one of several studies that can inform management. 3. For managers, the Integrated Management Recommendations will be the most useful section - I made my comments on this section at the last public meeting. 4. It isn't clear how research results from other owl studies were integrated with results from this owl study to form the management recommendations. Several studies aren't cited. It was much clearer to see the integration with the fisher work.

Additional comments for the authors: This looks like an enormous amount of work. Congratulations on finishing!

Commentor: Please keep my name and affiliation confidential in any summary of these comments.
Comment 14: Dave Martin, retired District Ranger, Bass Lake Ranger District, comments on SNAMP report

I did not do as thorough a review of the various SNAMP documents as I (perhaps) should have. However, the more I reviewed the various documents, the more satisfied I was with the depth of science, the thoroughness of the evaluation methods and the conclusions reached. That being said, I have a couple of general comments, generated from my 10,000 foot overview. I hope this is helpful.

In the FFEH Draft Report (page ii), the conclusions about the 4% reduction in fire behavior with and without SPLATS being characterized as “modest”, is not necessarily how I would characterize such a small reduction. Indeed, there is other science demonstrating greater basal area reductions resulting in greater effects to fire behavior, notwithstanding the issue of studying SPLATS. A more thorough acknowledgement/discussion regarding the nature of the limitations placed on management of SPLATS (in SP) and whether or not it would result in a higher % reduction would, at least point out a limitation of the study. From a fuel hazard reduction/prevention of catastrophic fire management perspective, 4% is unacceptable. I know that was not SNAMP’s charge to make such judgments but clearly articulating the limitations may give greater perspective to the results.

Another area where additional science assists land managers in making management decision and something that, perhaps SNAMP could not really measure (and/or draw conclusions upon) is the idea of sustainability of the stand structure in the key Fisher denning and foraging habitat stands, which by design and management restrictions were effectively off limits from any substantive vegetation treatments. Most of these stands are overstocked from a tree-health perspective. Catching overstocked stands “in the act” of losing vigor and increasing mortality in a 7 year period, even with 4 below-average rain years is difficult, if not impossible but is something that I also feel could have been highlighted as a limitation/ caveat in the conclusions not only in the FFEH but also the Wildlife sections. Many of the stands presented to SNAMP, although looking like mixed conifer, were in fact pine stands. Railroad logging and fire suppression changed that so a different profile was presented. It is one of higher stand densities (typical of MC) and larger trees (owing to the timing of RR logging and the removal of the dominant, more representative pine). This structure lends itself to much larger trees, higher Basal Areas and conditions much more favorable to quality fisher habitat. Much of the reason white fir was not reduced as much, as would be preferred, is the fact that in all areas, especially key fisher areas, many that those trees are too big to be able to manage in lower, more ecologically sustainable number and sizes (for a pine stand). They were either > 30” DBH or they were the primary component of the dominant/codominant canopy structure needed for retention. Both of these restrictions are part of the management box we had to play in. The control area (Nelder) is GROSSLY overstocked and we feel may put the giant sequoias in jeopardy not only from fire but also competition-induced tree mortality. Literature review over the past many years (and decades) does not support the long-term sustainability of such “artificial” stands, especially in view of the reality of climate change. At least putting greater emphasis on a potential management challenge of sustaining what is likely “artificial” fisher habitat would help elevate the true nature of tradeoffs in managing for such habitat and the potential effect of FFEH.

Again, the vast store of knowledge and the enormous benefits to be gained and the real questions that still need to be answered (good science always brings up the harder questions!) are a real triumph of the SNAMP study. I am so lucky to have been part of it and my hat is off to the UC Study Team! SUPER JOB!
Comment 15: Forest Service Region 5 comments on SNAMP report

Chapter 1
Good job of setting up the background and describing the design for non-scientists.

p. 2 (2005) Should be “Forest Service Pacific Southwest Region” -- as well as “Forest Service Pacific Southwest Research Station”. Or, “Forest Service, Pacific Southwest Region and Pacific Southwest Research Station”. Both are separate entities of the FS. Similar corrections at the top of p. 3. Other than references related to MOU signatories, you can probably just say “Forest Service” to mean region and station collectively. Also in other chapters.

p. 5 last para – have not yet defined “Participation Team” and its relation to UCST.

p. 6 last para -- Just FYI… We usually just refer to the SNFPA without reference to the Record of Decision. In fact, fireshed is not mentioned in the 2004 ROD and receive only casual reference (not as a “spatial unit of management”) in the FSEIS, so this statement is incorrect. Similar corrections needed in Chapter 2….

p. 7 para 2 – you might also mention that the fisher BACI assessment will be completed at a later date in collaboration with the PSW station.

Figure 1-1. UC did not actually sign the MOU. Figure caption should define all the acronyms (e.g., MOUP has not been mentioned in the chapter). Adaptive management adjustments..: adjustments are not to policy (as stated) but to management. Suggest delete “to policy”. Also, I think that parties other than USFS will do this – for example stakeholders, USFWS are already doing this.

Thanks for providing Box 1.

Chapter 2
p. 11 – Why does the LC site have two pairs of firesheds and SP only one pair? See figures and tables.

This is a really good description of the sites, especially the explanations for the expanded areas for owl and fisher studies. The appendixes for owl and fisher may not be entirely consistent with this description.

Figure 2-9 – Please include the dates (i.e., 2007-2013?) that encompass the treatments displayed in the figure caption.

I also like the descriptions of the communities and socioeconomic information associated with each site. The stories (anecdotes) about perceptions of interviewees are also interesting.

Chapter 3
These extended abstracts are a good idea, and they are mostly well-done.

p.4 Results para 1 – You should explain the positive and negative percent values – are they increases or decreases?

p. 26 – Explanation that “revised approach” was due to delays in implementing treatments contradicts with Chapter 2 and the fact that this change occurred around 2008-2009, well before the delays were known. The number of owls in the Last Chance site was just too small to follow the original plan.
p. 28 – “…amount of edge… higher demographic rates” – Does this mean that all demographic rates increase (i.e., survival, reproduction, colonization, extinction,…)? Could use an explanation.

p. 28 Discussion – Medium intensity harvests include more intense treatments than SPLATs. How many SPLATs resulted in decrease to <70% CC? You should be able to give us that information even though you had to use medium intensity in the demographic model.

p. 30 – I think it would be a good idea to explain why the objectives for fisher are so different from the other modules. You could reference the original work plan that did not refer to the treatment x fire design of SNAMP.

This fisher extended abstract is MUCH better than the executive summary in the appendix. You might consider modifying the former to use for the latter.

Chapter 4
Running out of time here… My overall impression is this is a good summary of the integration process and results. It was good to discuss, however briefly, the limitations of the approach you took and your results.

p. 23 – You recommend a larger spatial scale for wildlife and mention the greater costs for such studies and more cost-effective approaches. You might also discuss the greater cost for these two at-risk species (one proposed for listing) of imposing potentially detrimental treatments on a larger number of individuals.

Chapter 5
I appreciate the conditional recommendation approach (“If your goal is…”) you took. Science is only part of the decision space that managers use, and this approach helps us with that message. Also, being clear about what comes from the study and what is expert opinion is helpful, although I suspect some of the recommendations in the second part are going to be problematic. Some of these recommendations should be quite useful.

#2. Integrate what across firesheds? Can you suggest what we give up without optical or ground data?
#5 If SPLATs are placed as suggested, do we know that fire will behave as predicted to protect owls? I suspect not.
#22 I’m pretty sure this was not the first SPLAT treatment network implemented.
#24 Stand thermal conditions?
#29 I think I disagree with this one – or maybe don’t fully understand it. The definition of AM may differ for different uses. Can we be adaptive in all aspects of the AM cycle? See papers by Lindenmeyer & Likens on adaptive monitoring.

Appendix A – FFEH
Nothing from me… Good job.
Appendix B – Spatial
Linking Lidar data to CWHR classes may not be the best strategy – CWHR is used because it’s all we have. Can Lidar give us a better way to describe wildlife habitat? Also, I do not see where you actually accomplished the link to CWHR classes.

Please make figure 4 bigger so we can see details.
Figures 6 & 7 – need a scale for colors.

3.7 & 4.7 – How do results of investigating point density to predict forest metrics at the plot scale translate to stand or landscape scale metrics?

5.1 – Are tools used (Matlab, etc.) available for other users? What is “multi-path effect”? Combination of “high resolution multi-spectral … imagery” with what?

5.2 – Are there results to support this paragraph? Seems results are not here -- may be more appropriate for wildlife appendices.

5.3 – I’m not sure that managers would need this level of detail about fuel treatments to justify the cost.

5.4 & 5.5 – Link to results is weak. Will adequacy of use for fire behavior models be discussed in the integration report?

Appendix C – Owl
There is no need to invoke the delay in implementation of treatments to explain your change in approach. The change was made before the delay and short post-treatment time was known. The real reason for the change is that there were too few owls to do the original design, and additional years of post-treatment data would not improve the situation. See Chapter 2 (p. 26).

In the executive summary, please explain the apparent paradox that SPLATs may provide long-term benefit if fire occurs and long-term negative effects if fire does not occur. Better yet, instead of saying that there were long-term negative effects if fire does not occur, say that the negative effects of SPLATs decline over time and persist for at least 30 years – as your figures 11 & 12 show. The effects of SPLATs and fire on owls are similar to those for fisher and the language used to describe those effects should also be similar.

I did not review the previously published paper that is reproduced here.

Methods. How much of this is in other chapters? Could you reference them for details?
p. 52 – Why the 2001 Star fire?
p. 54 – Why include CC in the model despite being ns in the model? Is CC ns perhaps because LT is significant and there is considerable collinearity between CC and LT? In effect, this model conflicts with the demographic model.
p. 55 – Results are based on only 4 territories – discuss later? Discrepancy between 30.5 cm dbh in owl habitat and the 71.3 cm dbh in the model (p. 54)? Maybe this is why the models conflict?

Discussion (pp 59 ff) – The discussion is written as for a scientific paper. That is fine for the appendix if a more general discussion occurs in the report chapters. If not, you need to add here.

p. 61 – You should make clear that the Roberts et al (2011) paper was mostly low to moderate severity. The next sentence about your modeled declines being overestimated doesn’t clearly follow.

p. 61 – Please know that “species of conservation concern” has specific meaning for the Forest Service, and the species you mention are not SCCs for us. Another term would be helpful.

p. 62 – I don’t I agree that your results have “broader applicability” to other species – those species use large trees and canopy cover differently.

Figures 11 & 12 – please show standard errors on fig 11 and indicate significant differences on both.

Appendix D – Fisher
p. iii – What about the effects of fire objective? Was it really necessary to expand the study area into YNP just because an animal dispersed there? Tell us what the “management indicators” and “focused science efforts (or topics” are – better yet, leave that out. Are there 4 or 5 of the latter (I count 4)? These details are just confusing in an executive summary. The executive summary is much too long, with too much detail on some things like methods and little overall focus. The executive summary should give us the take-home messages.

p. iv – The fifth mortality is only “suspected” – don’t you know that it died? What do you mean by “five deterministic population growth rates”? I count somewhere between 3 and 6. Again, too much detail about methods and results.

p. v – You need citations for statements in the first paragraph, except that you should not review the literature in an executive summary. Most of this paragraph does not belong here at all and is out of order – it should come before discussion on p. iv.

p. v – vii – This is better, but still too detailed.

p. vii – starting with “Management indicator 1…” This just does not make sense. How are these management indicators? None of this belongs here.

p. viii – starting with “Occupancy modelling…” is better again.

At this point, I am ending my review of the executive summary, which should be re-written. The other team reports provide examples of good summaries. An executive summary should be just that – a high-level summary of the study that requires limited technical knowledge of the science. It should also not require the reader to have studied the entire report in detail. It should not be a literature review. There is so much rich, new information in the report that is not mentioned in this executive summary or is lost in all the extraneous material. Please give us the major findings and take-home messages.

Starting on p. 7:

General Comments: The authors should be aware that it is now 2015 and references to things that are “expected”, etc. in 2014 or earlier must be updated. Also, update references. There were
changes to Spencer et al. (2014) between draft and final (Spencer et al. 2015) that should be updated here.

Need to distinguish the two Zielinski et al (2013) papers.

Why such a long introduction? We do not need another version of the DRAFT Fisher Conservation Assessment (Spencer et al. 2014) because it distracts from this report and perpetuates some errors in the draft that have been corrected in the final (the final version was published in January 2015 and is readily accessible). Focus on the introduction needed for this report. You also have results, discussion, and conclusions of the SNAMP project in the introduction – save them for later. Or maybe replace the executive summary.

In general, references to Spencer et al (2014 or 2015) should be kept to the minimum needed to provide context for this work, so that readers are clear about the work that was conducted for SNAMP.

p. 10 – “fishers in this part of California may have expanded in the late 20th century (Tucker et al. 2014)” and “expanded during the 1990s” are misrepresentations of the research – I believe the genetic study identified that it was some time during the 20th century (about 100 years). This error must be corrected multiple times in the appendix.

p. 10 – “The Zielinski et al. (2013) analyses suggest…” The paper did not suggest what follows. You suggest these things based on an extrapolation of the analyses. In fact, the time scale of the monitoring is much shorter than 60 years and the suggestion is inappropriate.

p. 12 – It’s Sierra Nevada Forest Plan Amendment (SNFPA), not “amended Sierra Nevada Forest Plan” – there is no Sierra Nevada Forest Plan.

p. 13 – The reductions (% BA and CC) for treatments (SPLATs) are not allowed in many habitats used by fishers and spotted owls in the SNFPA.

p. 15 -- Was it really necessary to expand the study area into YNP just because an animal dispersed there? Sounds like you didn’t really extend the study area, just the area for aerial tracking.

p. 17-18 –This material is really more appropriate to the paper introduction section than much of what you have there now. Also discussion material here. The METHODS section should be renamed to reflect the actual content.

p. 26 – Reference to an Appendix that is not included. “Appendices” are in the TOC, but not in the document. We can’t understand this sampling scheme without the appendix. There are other references to Appendices that are not in the document.

p. 31 – “fishers … may not be in numeric or spatial recovery” – recovery from what? In this paragraph, you should take another look at Spencer et al (2015) to see how what you extracted from the 2014 draft may have changed.

p. 43 – Where is table 31? Should be nearby.

p. 52 – At some point, you should explain that the spacing of grid points is smaller than a fisher home range and how your models and inferences may be affected by a single animal showing up
on several grid cells – or how you dealt with that. Your colonization and extinction estimates are not really what we expect them to be.

Fig 10 – Legend should note that each bar represents a 2-week period. Also, bars should be grouped by population years in Table 9 to be consistent and less confusing to readers. Might also be good to mention why the # of fishers monitoring in Nov & Dec 2013 did not rebound as in previous years.

Table 13 – What is N?

Table 16 is difficult to understand. How does a den structure have an “aspect orientation”?

Table 24 – the label for Leslie Matrix λ should be up two lines – to align with the estimates (0.87, 0.88, …0.90) instead of the range. And some spacing between Fertility and λ.

p. 97ffl – Where are hazfuels.5, log.5, etc. defined?

By the way, the last sections are mostly quite good.

Appendix F – Public Participation

Good job on the executive summary of this very long report.

Parts of this Appendix are inconsistent with Chapter 1 of the report.

p. 8 – It’s not necessary to distinguish between the SNFPA and the ROD. We generally just refer to the SNFPA. I’m pretty sure that there was not a legal battle between the FWS and FS – that would be one federal agency suing another. The last three sentences of this paragraph make no sense to me and are at odds with the collaborative nature of SNAMP. What is a non-negotiable boundary (or boundaries in general)? Hardly autocratic. Limitations on democratic forms of participation?

p. 9 – Paragraph starting “Clear communication…” is also hard to understand. It seems to be a mish-mash of jargon and opinion with little explanation of the statements. It is also out of place with the rest of the section.

p. 11 – It’s a bit of a stretch to say that “the Sierra urbanizes”. It’s still mostly locked up in federal land ownership. I think most local residents would not say they are “far removed from … forest management”.

Section II. – This section generally needs more citations to support the statements made and opinions expressed. I’m going to share some of my opinions. I’m really not sure why all this detail is necessary to this report.

p. 15 – I think it’s inaccurate to claim that the early FS policy of frequently moving Rangers “was a problem for SNAMP”. That was a historic policy. The FS personnel changes observed with SNAMP were either promotions to higher levels of responsibility and pay or retirements. Similar changes occurred with other agencies and even some stakeholder groups. Outside academia these changes are common in all organizations.

Figure F-3 – This figure requires much more explanation to be able to understand it. Where does it come from?

p. 17 – “By the end of the decade…” – which decade? I wouldn’t put much emphasis on “outdoor recreation” being listed first in the MUSY act – it’s an alphabetical list. It’s also important to know that professionals in the FS were also driving (and continue to drive) the shifts to multiple use of the forests. Some employees were quite pleased with the legislated changes.

p. 18 – “by the late 1960s” there was actually a national enthusiasm for “technology and science” driven largely by the successes of the space program (and not just sending men to the moon). What was happening, with respect to the FS, was a distaste for industrial forestry and its visual
and environmental consequences – and a general distrust of the federal government because of Vietnam and other policies.

p. 20 – “Concerns of …” – again, USFWS was not part of “pending litigation”. The Forest Service is a federal agency. The FS and other agencies didn’t “support the SNAMP effort in 2005” – they worked with UC to develop an “adaptive management and monitoring process” between 2005 and 2007 (via the MOU). The proposal for SNAMP came later, in 2007, as a result of that collaboration.

p. 21 – The Forest Service is not a “representative of Congress”; it is an agency of the USDA and part of the executive branch. Still, the FS can’t delegate decisions to others. Here and elsewhere, terms like collaboration and consultation are used without definition, which would help us understand your points and decide if we agree. Collaboration isn’t in figure F-4. I am rather confused about this paragraph. It starts by saying that the FS is constrained from co-management and ends with a discussion of the Science Team. I think you are also saying that the Science Team is also constrained to consultation (or gather input and decide) in F-4, though not because of legal doctrine (as stated in the next paragraph). That could be said better. At the end of the page, past tense is used in references to management recommendations – wrong tense because we are just now seeing the recommendations. Where results were available, they have (past tense) been used as listed on the next page.

p. 22 – What is FLPMA? This sentence is out of date; the 2012 rule for NFMA now requires much more public participation (see 36CFR219.4).

p. 23 – Congress hardly has “supreme authority”. See recent Supreme Court decisions.

p. 27 – Another term in widespread use is multi-party monitoring. Citizen science (p. 28) is another thing altogether.

Figure F-9 c) -- What does the year represent? The last time discussed? Most frequent? I don’t understand c) at all. Odd that you highlight funding, process, and MOUP in b) – fire & water are what stand out for me. Nor do I understand the second lessons learned about the figure.

p. 115-116 – General comment, not specific to this page: At this point, I am struck by how much of the discussion is based on anecdotal evidence, like the example here of a single (or very few) respondent. I’m not very confident that this evidence is the basis for any sort of valid conclusion.
July 24, 2015

Dear SNAMP:

Thank you for the opportunity to provide feedback on the SNAMP Project Report. Below are comments regarding the fisher (Appendix D).

The SNAMP (2015) fisher appendix, on pages 51, 58-62, and 97-99, describes camera-survey results for fishers, concluding that mechanical thinning to reduce “hazardous fuels” has a negative effect on fisher occupancy, and suggesting some impact of wildland fires on fisher occupancy as well. The report (pages 114-117, and 119) then minimizes adverse effects of mechanical thinning and instead suggests that wildland fire is a substantially greater “threat” or “risk” to fishers than thinning, such that landscape-level thinning, and even potential expansion of thinning, might be justified from a net benefits/risks standpoint. However, there are four significant problems with this conclusion.

First, the results described on pages 98-99 do not support such a conclusion. Those results state that the single top model regarding local extinction identified “only” thinning (“hazfuels.5”), and that fisher persistence decreased with increasing thinning. The results (pages 98-99) indicate some effect of fire but state that the “relative importance” of this variable was “low”. Therefore, there is not a sound basis for the conclusion that thinning is justified to mitigate fire effects; in fact, the results contradict this conclusion.

Second, we followed the methods described in the SNAMP fisher appendix with regard to fires in the camera-survey study area, and digitized the grids shown in Figure 12 of the SNAMP report. We used GIS to overlay fires on the camera-survey grids, and used Google Earth satellite imagery to evaluate whether post-fire logging had occurred in any of the fires. We found that post-fire logging—often extensive—had been conducted in numerous camera-survey grids within fire areas, e.g., in portions of the 2001 North Fork fire and in the 2004 Nehouse fire (see figures below). Therefore, there is a significant methodological issue in the study, in that it states it is only testing the effects of fire on fisher occupancy, but is actually instead testing effects of post-fire logging in numerous grids. This is important, given that all but one of the grids that are approximately 50-100% within fire perimeters, and that we identified as having been post-fire logged, had no documented fisher occupancy, according to Figure 12 of the SNAMP (2015) fisher appendix.
Figure 1. Post-fire logging (above and below the center of image) in the North Fork fire of 2001. The center of the grid, where the fisher survey camera was located, is the post-fire clearcut just south of the center of the image. There were no fisher detections in this grid.
Figure 2. Post-fire logging in another grid within the North Fork fire of 2001 (post-fire clearcutting, and burning of logging slash piles, can be seen in the center and left of center portion of the image). There were no fisher detections in this grid.
Figure 3. Post-fire clear-cutting in the Nehouse fire of 2004 (post-fire logging skid trails are shown to the northeast and southwest of the center of the image). There were no fisher detections in this grid.

Third, among camera-survey grids mostly/wholly within fire perimeters, a disproportionately large cluster are in a fire (the 1990 Steamboat fire in Yosemite National Park) at the northern extreme of the fisher’s range in the southern Sierra Nevada (generally, north of 37 degrees and 39 minutes latitude). Figure 12 of the SNAMP fisher appendix shows that fishers are almost completely absent in this area, and only one camera-survey grid had a fisher detection—and it is partially in the fire area. None of the unburned grids in this northernmost portion of the study area had any detections. The effect of this was to create a bias in the results toward reporting lower fisher use of fire areas.

Fourth, the SNAMP fisher appendix does not incorporate Hanson (2013) or Hanson (2015), which are peer-reviewed studies (see references below) conducted on fishers and fire based upon field data (as opposed to modeling assumptions). These two research studies regarding fishers and burned forests have found that post-fire landscapes, when left unlogged, can provide important, critical habitat to fishers.

In Hanson (2013), using specially trained scat-detecting dogs to assess the frequency of Pacific fisher scat detections/km of survey transect in different habitat types, I analyzed Pacific fisher habitat selection in post-fire habitat and adjacent unburned forest on Sequoia National Forest. Specifically, I analyzed fisher habitat selection by fire severity and pre-fire structure/composition – both dominant size class and canopy cover – and forest type (see Abstract, and Results at p. 26,
of Hanson 2013). In my Methods (see p. 25 of Hanson 2013), I described the fire severity categories analyzed, and the level and range of tree basal area mortality associated with each, where higher-severity was defined as 50-100% basal area mortality. I found that, in unburned forest, fishers selected areas of dense, mature/old conifer forest significantly more than open or young forests (Hanson 2013, Table 2b). I also found that, within fire areas (not subjected to post-fire logging), fishers similarly selected areas where the pre-fire condition was dense, mature/old conifer forest, as opposed to areas where the pre-fire forest was open or young (Hanson 2013, Table 2a). This relationship was not statistically significant at the 0.05 significance level, but was significant at the 0.10 significance level (Hanson 2013, Table 2a), which is often used in studies pertaining to wildlife habitat selection. This indicates that fishers are selecting dense, mature/old conifer forest in its unburned, and burned, states. Further, I found that the proportion of higher-severity fire was significantly higher within 0.5 kilometers of fisher detection locations than at random locations, indicating that fishers are selecting areas with relatively higher levels of higher-intensity fire for foraging (Hanson 2013, Figure 2). When fishers are near fire perimeters, they strongly select the burned side of the fire edge (Hanson 2013, Table 3). Both males and female fishers are using large mixed-intensity fire areas, such as the McNally fire, including areas >10 kilometers into the fire area.

Moreover, I gathered additional data in the 61,000-hectare McNally fire of 2002, using the same methods described in Hanson (2013), but focused mainly in large higher-severity fire patches, and found very strong use of large, intense fire patches in dense, mature/old conifer forest, especially by female fishers. My findings are in-press in a new study, Hanson (2015). In Hanson (2015), the current hypothesis among land managers that fishers will avoid higher-severity fire areas was rejected, and fishers used unlogged higher-severity fire areas at levels comparable to use of adjacent unburned dense, mature/old forest. Female fishers demonstrated a significant selection in favor of the large, intense McNally fire over adjacent unburned mature/old forest, and the highest frequency of female fisher scat detection was over 250 meters into the interior of the largest higher-intensity fire patch (over 5,000 hectares).

For the above reasons, the SNAMP fisher appendix conclusions regarding fishers, fire, and logging (mechanical thinning) should be changed.

Please let us know if you have any questions about the information we have provided. Thank you.

Sincerely,

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References

Hanson, C.T. 2013. Pacific fisher habitat use of a heterogeneous post-fire and unburned landscape in the southern Sierra Nevada, California, USA. The Open Forest Science Journal 6: 24-30.

July 24, 2015

Dear SNAMP:

Thank you for the opportunity to provide feedback on the SNAMP Final Report. Below are comments regarding the California spotted owl (Appendix C).

1. “We conclude that SPLATs may provide long-term benefits to spotted owls if fire occurs under extreme weather conditions, but can have long-term negative effects on owls if fire does not occur.”

The above statement should reflect that mechanical treatments can be ineffective in extreme weather, and therefore the perceived benefit may not even be realized. For example, Lydersen et al. 2014 states: “Our results suggest that wildfire burning under extreme weather conditions, as is often the case with fires that escape initial attack, can produce large areas of high-severity fire even in fuels-reduced forests with restored fire regimes.”

Explain also that even if fire does occur, there may be no long term benefits from mechanical treatments. Instead, fire (including mixed-severity fire with a high-severity component) can occur in ways that provide benefits to owls (e.g. foraging habitat), and therefore, in addition to causing harm to unburned owl habitat, mechanical treatments could also cause harm in terms of how they impact the post-fire condition.

2. “Therefore, we recommend adopting a landscape approach that restricts timber harvest within territory core areas of use (~125 ha in size) that contain critical owl nesting and roosting habitat (Berigan et al. 2012) and locates fuels treatments in the surrounding areas to reduce the potential for hazardous fire to spread into PACs.”

Discuss that mechanical treatments outside the 125 ha acre could have significant negative impacts to owls as well, while mixed-severity fire (in the absence of mechanical treatments) could have benefits to owls. The following bullet points, for example, discuss mechanical thinning in relation to spotted owls:

- California spotted owl populations are declining (e.g., Conner et al. 2013), and the only area not experiencing declines is Sequoia Kings-Canyon National Park, which has an active mixed-severity fire regime and no mechanical thinning fuels treatment or post-fire logging programs.
• Gallagher (2010) examined foraging-site selection of 10 radio-marked California spotted owls in the Meadow Valley Project area on the Plumas National Forest. The project was governed by the Herger-Feinstein Quincy Library Group Forest Recovery Act of 1998. Treatments included 1) DFPZs, landscape-scale forest thins designed to function as fire breaks by a reduction in canopy cover, tree density, and ladder fuels; 2) understory thin, prescribed as removal of shrubs and trees < 10 inches diameter; 3) understory thin followed by underburn; and 4) group selection, a removal of all trees < 30 inches diameter in < 0.8-ha patches. Gallagher (2010) found that across all birds and both study years, only 8 percent of owl foraging locations were located within fuels treatments, and nearly half of those locations were accounted for by a single owl. Owl foraging sites were primarily located in mixed-conifer forest dominated by trees 12–24 inches diameter with an additional large proportion of trees > 30 inches diameter and with a multi-layered understory with numerous small trees. The mean proportion of owl locations in DFPZs was lower than expected by chance, thus the owls avoided foraging in this treatment type.

Gallagher (2010) found that home-range size of California spotted owls in the Meadow Valley Project area of the Plumas National Forest increased as the total area of fuels treatments within the home range increased, particularly of DFPZ (mechanical thinning) and group selection treatments under the 2004 Sierra Nevada Forest Plan Amendment, indicating lower territory fitness in such areas.

• Keane et al. (2012) reported that the Meadow Valley fuels treatment project on the Plumas National Forest, under the 2004 Sierra Nevada Forest Plan Amendment, began in 2006 (Keane et al. 2012, Fig. 10) and was completed in 2007–2008 (Keane et al. 2012, p. 88). After the logging, from 2007 to 2011, the total number of territorial sites of California spotted owls in the Meadow Valley project area declined from 9 to only 4—in just four years (Keane et al. 2012, Table 7).

• Tempel et al. (2014) found that mechanical thinning is significantly harming California spotted owls. The authors noted also that the adverse effects of mechanical thinning on California spotted owls is likely even larger than their results indicated: “Understory removal is generally an important component of fuel-reduction strategies, but we caution that medium-intensity harvesting with understory treatments occurred on only 5.2% of the total area within owl territories, which could have limited our power to detect effects . . . ” In other
words, the adverse effects of mechanical thinning were apparent even with a relatively small portion of the study area affected by such logging. The authors further noted the following: “In addition, only 42.8% of medium-intensity harvests occurred in high-canopy forests; thus, over half of these harvests occurred in habitats that might be less important to spotted owls (Fig. 5c). When medium-intensity harvests were implemented within high-canopy forests, they reduced the canopy sufficiently for mapped polygons to be reclassified into a lower-canopy vegetation class in 90.1% of these treated areas (Fig. 5d). As described above, such changes were associated with reductions in survival and territory colonization rates, as well as increases in territory extinction rates. As a result, we believe the most appropriate inference about the influence of medium-intensity harvesting practices is that they appear to reduce reproductive potential, and when implemented in high-canopy forests, likely reduce survival and territory occupancy as well.” The results of Tempel et al. (2014) indicated that some high-intensity logging on a very small percentage of the landscape, where dense brush had been allowed to grow after logging (possibly facilitating habitat growth for some small mammal species), was associated with lower levels of territory extinction, but the authors cautioned about such logging. Owl survival was positively associated with the juxtaposition of mature forest and brushy shrub/sapling habitat areas (not necessarily associated with past logging). Finally, Tempel et al. (2014) found no effect of wildland fire on spotted owl reproduction, survival, occupancy, or territory extinction. They did report an adverse effect of fire on territory colonization, but the fire covariate was “unestimable” due to very small sample size, meaning that the model could not be fitted and therefore the beta estimate for fire was not valid. The authors noted that territory colonization was low in fire-affected areas for two reasons: (1) in the largest fire that accounted for most of the fire-affected territories, 5 of the 9 territories remained occupied in every single year after the fire, thus “colonization could not occur by definition”; and (2) the authors noted that the main reason that the “effect of wildfire on territory colonization was strongly negative” was “due to a high-severity fire that occurred on our study area in 2001 and completely burned two territories, which were subsequently never colonized by owls”, and two other territories had very low post-fire occupancy and colonization. The modeling result of the study, however, did not account for the fact that the loss of occupancy (and no colonization) in the two “completely burned” territories, and the two other territories with very low post-fire occupancy/colonization, was associated with intensive logging after the fire (see, e.g., Sierra Club v. Eubanks, 335 F.Supp.2d 1070, 1075 (E.D. Cal. 2004) [noting that all of the heavily burned forest in the Star fire of 2001
had been subjected to post-fire logging on public and private lands outside of the Duncan Canyon Inventoried Roadless Area, which is the portion of the Star fire that is outside of the Tempel et al. 2014b study area). Google Earth imagery also shows heavy post-fire logging within 1.5 kilometers (and much closer) of the two territories that completely lost occupancy (PLA055 and PLA075) and the two with near-complete loss of occupancy and colonization post-fire (PLA016 and PLA099) (see Appendix A attached at end of these comments). Moreover, the study does not address other research—Lee et al. (2012)—which found that mixed-severity fire (dominated by moderate- and high-severity fire effects) did not reduce California spotted owl occupancy in the Sierra Nevada, while observational data suggested that post-fire logging did reduce occupancy. Unlike in Tempel et al. (2014), the fire covariate in Lee et al. (2012) was estimable because the sample size was much larger.

• A recent analysis by Stephens et al. (2014) found a 43 percent loss of California spotted owl occupancy within a few years following mechanical thinning and group selection logging in a study area in the northern Sierra Nevada. Specifically, the authors found the following: “In the Meadow Valley study area, the number of territorial owl sites declined after treatment. Prior to and throughout the implementation of the treatment, the number of owl sites ranged from seven to nine. Between the final year of the DFPZ and group-selection installations (2008) and two years after treatment (2009–2010), the number of owl sites declined by one (six territorial sites), and by 3–4 years after treatment (2011–2012), the number of sites had declined to four—a decline of 43% from the pretreatment numbers”. The authors noted that, while spotted owl populations have been declining in the northern Sierra Nevada as a whole, the steep rate of decline in this fuels treatment study area were of “a greater magnitude” than elsewhere on the landscape.

3. “For millennia, low- to moderate-severity wildfires occurred at frequent (often less than 20-year) intervals in many western forests, naturally removed fuels such as woody debris, shrubs, and small trees, and shaped the ecology of these forests (Agee 1993, Noss et al. 2006). However, decades of wildfire suppression have disrupted historic fire regimes, increased the amount of surface and ladder fuels, and have led to more frequent high-severity wildfires that now threaten ecological and human communities (Westerling et al. 2006).”

The above statement gives the impression that modern fires are not ecologically valuable. That implied message does not reflect a) recent research (e.g., Odion et al. 2014; Baker 2014) that finds that high-severity fire (including large patches of high-severity fire hundreds or thousands of acres in size), as a component of mixed-severity fire, is important to ecological communities in the Sierras, or b) extensive research finding that severely burned post-fire areas (including large patches of it) are critical for species such as black-backed woodpeckers (Siegel et al. 2011, 2012, 2013, 2014, 2015; Tingley et al. J212
numerous avian species (Hanson 2014; Burnett et al. 2010, 2011, 2012; Roberts et al. 2015; Siegel et al. 2011), bats (Buchalski et al. 2013), as well as owls (Bond et al. 2009) and fishers (Hanson 2015). Moreover, recent fires that are often referred to by the Forest Service as harmful – the McNally, the Moonlight, the Rim, and the King – all contain wildlife habitat created by high-severity fire that has been found to be inhabited by many species, including very rare species (see wildlife articles referenced above which occurred in part in the McNally and Moonlight fire areas; see also recent woodpecker data from the Rim and King fires). It is important therefore to reflect in this Appendix that high-severity fire is not categorically harmful to ecological communities and instead to explain the importance of high-severity fire to the ecological integrity of the Sierras; otherwise, it will continued to be perceived as a complete loss when in fact high-severity fire is essential and important.

4. “there is an urgent need to understand the effects of fuel-reduction treatments on old-forest-associated species so that fire risk can be managed”

The urgent need exists because the Forest Service seeks to expand the use of mechanical treatments, and wants to do so far away from human communities. But it is important to differentiate fire impacts to humans from fire impacts to the forest. The former does require serious mechanical treatments very close to structures to protect those structures, while the latter does not require mechanical treatments and instead requires efforts that ensure that low, moderate and high-severity fire will be a substantial component of management in order to ensure ecological integrity of the forest. Outside of human communities, fire risk is not the issue; rather, in the backcountry, lack of fire (due to fire suppression) is the issue. Therefore, it should be explained that fire (of all severities) is needed on the landscape outside of human communities.

5. “While fuels management may provide long-term benefits to such species by reducing future habitat loss from high-severity fires”

Reemphasize here, as is done elsewhere in this Appendix, that fuels management (in the form of mechanical treatments) may not provide any long-term benefit at all, and instead might only provide short and long term harm due to impacts to habitat. Moreover, it is worth explaining to readers that different management techniques will have different impacts—prescribed fire and managed wildfire, for example, will have much different outcomes and impacts than will mechanical treatments, and readers should be educated on the differences. Furthermore, not only are mechanical treatments harmful to owls, it may also be harmful, not beneficial, to reduce high-severity fire in light of the findings in the literature regarding owls and high-severity fire (e.g., Lee and Bond 2015, Lee et al. 2012, Bond et al. 2009, 2013).

6. “As with other western forests, the area burned by high-severity fires in the Sierra Nevada has increased over the past several decades (Miller et al. 2009).”

First, recent research contradicts Miller et al. 2009—e.g., Hanson and Odion 2014; Hanson and Odion 2015. Moreover, even if high-severity fire were increasing, that is not necessarily a problem given the deficit of high-severity fire in the Sierras (e.g., Odion et
al. 2014) and the importance of high-severity fire to biota. It is therefore important to reflect in this Appendix that an increase is likely a positive for Sierra ecosystems and is not at all categorically a negative.

7. “Long-term benefits will depend on both the risk that fire poses to spotted owls and the extent to which fuel treatments reduce high-severity fires.”

Again, this reflects an assumption that high-severity fire will occur in ways that solely are harmful when that is not accurate—high-severity, when it occurs, can occur in ways that are beneficial to owls.

8. Appendix C describes a prospective modeling effort regarding spotted owls. The report claims that thinning might have a net positive effect on owl occupancy if and where fire occurs. However, the model is based upon assumptions that are not consistent with current science. For example, on page 2, the Appendix states that the model was based only on spotted owl nesting habitat, not on the combination of nesting/roosting and foraging habitat -- fire can provide foraging habitat (Bond et al. 2009) and demographic benefits to owl territories. By basing the model only on nesting habitat, and excluding foraging habitat, the model necessarily treated any moderate/high-intensity fire as a negative/adverse effect that removes habitat and reduces occupancy. However, we also know from a large dataset of owls in territories in fire areas of the Sierras, in the absence of post-fire logging, mixed-severity fire does not reduce spotted owl occupancy--in fact, occupancy in mixed-severity fire areas was numerically higher than in unburned mature forest, but the difference was not statistically significant (Lee et al. 2012). For these reasons, the model does not represent the current state of scientific knowledge, and thus the statements/conclusions from it – that high-severity fire adversely affects owl occupancy, and that thinning benefits spotted owl occupancy – should be changed.

Please let us know if you have any questions about the information we have provided. Thank you.

Sincerely,

Chad Hanson, Ph.D., Director  Justin Augustine
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References

Baker, W.L.  2014.  Historical forest structure and fire in Sierran mixed-conifer forests reconstructed from General Land Office survey data.  Ecosphere 5: Article 79


Hanson, C.T., and D.C. Odion. Sierra Nevada fire severity conclusions are robust to further analysis: a reply to Safford et al. International Journal of Wildland Fire 24: 294-295


Appendix A
I have participated in SNAMP in the following ways *(choose all that apply)*

- Heard a presentation about SNAMP at another meetings
- Read SNAMP science articles or briefs
- Read SNAMP newsletters
- Read SNAMP emails
- Other - Sequoia Forest Keepers was instructed by the lead fisher researcher on the protocol, so our summer intern teams could document Pacific fisher presence in Sequoia National Forest by the same protocol being used by SNAMP.

Next, please tell us how much you agree or disagree with the following statements. *(Strongly agree, Agree, Neutral, Disagree, Strongly Disagree).*

- This document is well written: Neutral
- This document summarizes research findings well: Strongly Disagree
- The research findings described in this document are understandable: Disagree
- The research findings described in this document can be applied to Sierra Nevada forest management: Strongly Disagree

The most interesting research findings in this document are:

The most important findings from this document to apply to Sierra Nevada public forest management are:

The research findings that I found least applicable to Sierra public forest management are:

Appendix D: Fisher Team Final Report on page viii, asserts that further logging treatments of different intensities may need to be studied.

The biggest questions I have after reading this document are:

The biggest concerns I have after reading this document are:

The SIERRA NEVADA ADAPTIVE MANAGEMENT PROJECT (SNAMP) Appendix D: Fisher Team Final Report on page viii, asserts that further logging treatments of different intensities may need to be studied.

“We found that SPLATs caused an immediate 6% reduction in potential fisher habitat. However they also moderated the impact of fire, resulting in greater available fisher habitat within 30 years. In the absence of simulated fire, the amount of habitat steadily increased over time due to forest succession, and was actually slightly greater on the treated landscape in year 30 than in year 0. The net benefits of SPLATs for the Pacific fisher will depend upon the true, but unknown, probability that high-severity fire effects will occur on a given portion of the landscape. However, future probabilities for specific fire behaviors (e.g., crown-fire initiation) are difficult to estimate, and it is therefore difficult to quantify trade-offs associated with SPLATs in absolute terms (Finney 2005). We further note that the SPLATs which were implemented at Sugar Pine appeared to have relatively modest impacts on forest structure and simulated fire behavior, and that it may be necessary to evaluate additional SPLATs of different intensities over a larger scale to fully assess the effects of SPLATs on fisher habitat.”

The SNAMP fisher report does not consider the analysis provided by the 2008 study Fire Probability, Fuel Treatment Effectiveness and Ecological Tradeoffs in Western U.S. Public Forests by Jonathan J. Rhodes and William L. Baker, which indicates that few treatment areas ever encounter fire (2.0% to 7.9%) during their assumed 20 year period of reduced fuels.
Similar to the statement questioned above, the SNAMP fisher report on, pages 117 and 118, contains a section titled “Integration” that is based on models that do not factor in the low chance of probability that the treatment area will experience fire.

“Integration We found that the SPLATs at Sugar Pine slightly reduced simulated fire behavior and resulted in greater amounts of projected fisher habitat up to 30 years after the fire. In the absence of simulated fire, we found that the SPLATs had an immediate, negative effect on the amount of fisher habitat, but SPLATs did not generally have a negative effect on fisher habitat when we modeled future forest growth for 30 years. In all scenarios, the differences between the treated and untreated landscapes were small. Our results were in general agreement with prior findings. Thompson et al. (2011) performed an analogous study to ours, in which they modeled fire and forest growth under treatment and no treatment scenarios and assessed fisher habitat suitability in the southern Sierra Nevada. They projected that fuels treatments had slight negative effects on fisher habitat in the absence of fire, but provided significant positive benefits up to 37 years after simulated fire. Truex et al. (2013) suggested that less fisher resting habitat was present immediately after mechanical fuels treatments were implemented in the Sierra Nevada. However, fishers consistently used areas in the southern Sierra Nevada where some timber harvest had occurred, so it may be possible to implement fuels-reduction treatments at an extent and rate that achieves fire-hazard-reduction goals (Zielinski et al., 2013).

As we noted in Appendix C for the California spotted owl, the net benefits of SPLATs for the Pacific fisher will depend upon the true, but unknown, probability that high-severity fire effects will occur on a given portion of the landscape. However, future probabilities for specific fire behaviors (e.g., crown-fire initiation) are difficult to estimate, and it is therefore difficult to quantify trade-offs associated with SPLATs in absolute terms (Finney 2005). We further note that the SPLATs which were implemented at Sugar Pine appeared to have relatively modest impacts on forest structure and simulated fire behavior, and that it may be necessary to evaluate additional SPLATs of different intensities over a larger scale to fully assess the effects of SPLATs on fisher habitat. Nonetheless, we have no reason to believe that Forest Service managers should alter their current policy of avoiding the placement of SPLATs near known fisher denning sites (U.S. Forest Service 2004) because these sites have significant biological importance for this species.

It will be necessary to mitigate for major threats to fisher survival while maintaining contiguous expanses of suitable fisher habitats, and detailed analyses using realistic and empirically developed data on population parameters are necessary for evaluating the long-term viability of fishers in the southern Sierra Nevada.”

The SNAMP fisher report fails to acknowledge the existence of Rhodes and Baker, 2008 as well as the other studies of fuel treatment impacts (Law and Harmon, 2011; Campbell et al., 2011; Price et al., 2012; Price, 2012; Restaino and Peterson, 2013) that have also shown: a) that analysis of the probability of fire affecting treated areas, such as that of Rhodes and Baker (2008), is essential to assessing the potential effectiveness of such treatments, and b) that the probability of fire affecting treated areas while fuels are reduced is relatively low. Yet the SNAMP fisher report continues to promote treatments in the hope that the treated areas will experience fire, so the models can hopefully work. The SNAMP fisher report projects fire in treatment areas even though the likelihood is low and the impact to the fisher habitat from treatment would be immediate and negative and it would take 37 years for the habitat to recover from the treatment and show positive benefits.

It would not be difficult to estimate the probability of fire affecting treatments; Rhodes and Baker, 2008 laid out how do it. Done properly, it will show a very low probability of fire affecting treated areas.
The conclusions of Rhodes and Baker, 2008 should be considered for inclusion in the SNAMP fisher report. If your team were to consider Rhodes and Baker (2008), the presumption in your final report that SPLATS of different intensities may need to be evaluated should be revised.

In addition, the SNAMP fisher report clearly indicates on page 32, that, “Data from radiocollared animals from the study area indicated that female fishers commonly die by 6-8 years of age.” Contrary to what the report implies, this means that more than four generations of Pacific fisher will not be able to use the treatment areas. While treating the forest may result in “relatively modest impacts on forest structure,” the impact of treatments on Pacific fisher would be extremely significant!

**Additional comments for the authors:** Sequoia ForestKeeper’s summer intern team has continued to follow your protocol and has again photo-documented fisher in Sequoia National Forest where the agency continues to propose logging projects. Thank you for all of your help guiding us with our project to document fisher presence.

**Commentor:** Mr. Ara Marderosian, Sequoia ForestKeeper® P.O. Box 2134 Kernville, CA 93238-2134
Rhodes and Baker (2008) statistically analyzed GIS data for more than 40,000 fires occurring over about 20 years in ponderosa pine forests on western USFS lands to quantitatively assess the regional probability of fire affecting treated areas during the window when fuels have been reduced in these forests, which provides an upper bound on potential fuel treatment effectiveness. Since publication, this paper has been repeatedly cited in peer-reviewed studies of: 1) fire frequency in forests (e.g., Williams and Baker, 2012), 2) the potential efficacy of fuel treatments (e.g., Price et al., 2012; Price, 2012), and 3) the impacts of wildfire and forest fuel treatments on forest carbon budgets (e.g., Law and Harmon, 2011; Campbell et al., 2011; Restaino and Peterson, 2013).

Several other studies of fuel treatment impacts (Law and Harmon, 2011; Campbell et al., 2011; Price et al., 2012; Price, 2012; Restaino and Peterson, 2013) have also shown: a) that analysis of the probability of fire affecting treated areas, such as that of Rhodes and Baker (2008), is essential to assessing the potential effectiveness of such treatments, and b) that the probability of fire affecting treated areas while fuels are reduced is relatively low.


Abstract

Sequestration of carbon (C) in forests has the potential to mitigate the effects of climate change by offsetting future emissions of greenhouse gases. However, in dry temperate forests, wildfire is a natural disturbance agent with the potential to release large fluxes of C into the atmosphere. Climate-driven increases in wildfire extent and severity are expected to increase the risks of reversal to C stores and affect the potential of dry forests to sequester C. In the western United States, fuel treatments that successfully reduce surface fuels in dry forests can mitigate the spread and severity of wildfire, while reducing both tree mortality and emissions from wildfire. However, heterogeneous burn environments, site-specific variability in post-fire ecosystem response, and uncertainty in future fire frequency and extent complicate assessments of long-term (decades to centuries) C dynamics across large landscapes. Results of studies on the effects of fuel treatments and wildfires on long-term C retention across large landscapes are limited and equivocal. Stand-scale studies, empirical and modeled, describe a wide range of total treatment costs (12–116 Mg C ha⁻¹) and reductions in wildfire emissions between treated and untreated stands (1–40 Mg C ha⁻¹). Conclusions suggest the direction (source, sink) and magnitude of net C effects from fuel treatments are similarly variable (−33 Mg C ha⁻¹ to +3 Mg C ha⁻¹). Studies at large spatial and temporal scales suggest that there is a low likelihood of high-severity wildfire events interacting with treated forests, negating any expected C benefit from fuels reduction. The frequency, extent, and severity of wildfire are expected to increase as a result of changing climate, and additional information on C response to management and disturbance scenarios is needed improve the accuracy and usefulness of assessments of fuel treatment and wildfire effects on C dynamics.

See also (easy to find pdfs for them on google scholar):


*In conclusion, we found that leverage [prob. that fire affects areas with reduced fuels] is positively related to the mean annual area burnt and cases where leverage occurs that is sufficient to effectively reduce area burnt by unplanned fire are rare. Generally leverage occurs and is stronger where annual burnt area is high and also if fuel recovery periods are long. [NOTE: Price et al., specifically found low probability of fire affecting fuel treatments in Sequoia]*

All indicate that the probability of fire affecting areas with fuel reductions is small.

See also:


It demonstrates that fire affects less than 2% of forests in an ecoregion, making it highly unlikely that fire affects beetle-affected forests. (But it is the same for any type of forest)

*Note that Rhodes and Baker (2008) paper assumed a 20 yr window of fuel treatment effectiveness, which we noted was the likely upper limit of duration effectiveness. It’s much more likely that fuels are only actually reduced for about 10-12 yrs. The shorter the duration of fuel reduction, the lower the probability of fire affecting these areas while fuels are reduced, given a constant probability of fire.*

(You can use the equation in Rhodes and Baker (2008) to calculate this effect)

For instance, with the annual probability of higher severity fire at 0.3%:
The SIERRA NEVADA ADAPTIVE MANAGEMENT PROJECT (SNAMP) Appendix D: Fisher Team Final Report on page viii, asserts that further logging treatments of different intensities may need to be studied.

“We found that SPLATs caused an immediate 6% reduction in potential fisher habitat. However they also moderated the impact of fire, resulting in greater available fisher habitat within 30 years. In the absence of simulated fire, the amount of habitat steadily increased over time due to forest succession, and was actually slightly greater on the treated landscape in year 30 than in year 0. The net benefits of SPLATs for the Pacific fisher will depend upon the true, but unknown, probability that high-severity fire effects will occur on a given portion of the landscape. However, future probabilities for specific fire behaviors (e.g., crown-fire initiation) are difficult to estimate, and it is therefore difficult to quantify trade-offs associated with SPLATs in absolute terms (Finney 2005). We further note that the SPLATs which were implemented at Sugar Pine appeared to have relatively modest impacts on forest structure and simulated fire behavior, and that it may be necessary to evaluate additional SPLATs of different intensities over a larger scale to fully assess the effects of SPLATs on fisher habitat.”

The SNAMP fisher report does not consider the analysis provided by the 2008 study Fire Probability, Fuel Treatment Effectiveness and Ecological Tradeoffs in Western U.S. Public Forests by Jonathan J. Rhodes and William L. Baker, which indicates that few treatment areas ever encounter fire (2.0% to 7.9%) during their assumed 20 year period of reduced fuels.

Similar to the statement questioned above, the SNAMP fisher report on, pages 117 and 118, contains a section titled “Integration” that is based on models that do not factor in the low chance of probability that the treatment area will experience fire.

“Integration
We found that the SPLATs at Sugar Pine slightly reduced simulated fire behavior and resulted in greater amounts of projected fisher habitat up to 30 years after the fire. In the absence of simulated fire, we found that the SPLATs had an immediate, negative effect on the amount of fisher habitat, but SPLATs did not generally have a negative effect on fisher habitat when we modeled future forest growth for 30 years. In all scenarios, the differences between the treated and untreated landscapes were small.
Our results were in general agreement with prior findings. Thompson et al. (2011) performed an analogous study to ours, in which they modeled fire and forest growth under treatment and no treatment scenarios and assessed fisher habitat suitability in the southern Sierra Nevada. They projected that fuels treatments had slight negative effects on fisher habitat in the absence of fire, but provided significant positive benefits up to 37 years after simulated fire. Truex et al. (2013) suggested that less fisher resting habitat was present immediately after mechanical fuels treatments were implemented in the Sierra Nevada. However, fishers consistently used areas in the southern Sierra Nevada where some timber harvest had occurred, so it may be possible to implement fuels-reduction treatments at an extent and rate that achieves fire-hazard-reduction goals (Zielinski et al., 2013).

As we noted in Appendix C for the California spotted owl, the net benefits of SPLATs for the Pacific fisher will depend upon the true, but unknown, probability that high-severity fire effects will occur on a given portion of the landscape. However, future probabilities for specific fire behaviors (e.g., crown-fire initiation) are difficult to estimate, and it is therefore difficult to quantify trade-offs associated with SPLATs in absolute terms (Finney 2005). We further note that the SPLATs
which were implemented at Sugar Pine appeared to have relatively modest impacts on forest structure and simulated fire behavior, and that it may be necessary to evaluate additional SPLATs of different intensities over a larger scale to fully assess the effects of SPLATs on fisher habitat. Nonetheless, we have no reason to believe that Forest Service managers should alter their current policy of avoiding the placement of SPLATs near known fisher denning sites (U.S. Forest Service 2004) because these sites have significant biological importance for this species.

It will be necessary to mitigate for major threats to fisher survival while maintaining contiguous expanses of suitable fisher habitats, and detailed analyses using realistic and empirically developed data on population parameters are necessary for evaluating the long-term viability of fishers in the southern Sierra Nevada.”

The SNAMP fisher report fails to acknowledge the existence of Rhodes and Baker, 2008 as well as the other studies of fuel treatment impacts (Law and Harmon, 2011; Campbell et al., 2011; Price et al., 2012; Price, 2012; Restaino and Peterson, 2013) that have also shown: a) that analysis of the probability of fire affecting treated areas, such as that of Rhodes and Baker (2008), is essential to assessing the potential effectiveness of such treatments, and b) that the probability of fire affecting treated areas while fuels are reduced is relatively low. Yet the SNAMP fisher report continues to promote treatments in the hope that the treated areas will experience fire, so the models can hopefully work. The SNAMP fisher report projects fire in treatment areas even though the likelihood is low and the impact to the fisher habitat from treatment would be immediate and negative and it would take 37 years for the habitat to recover from the treatment and show positive benefits.

It would not be difficult to estimate the probability of fire affecting treatments; Rhodes and Baker, 2008 laid out how do it. Done properly, it will show a very low probability of fire affecting treated areas.

The conclusions of Rhodes and Baker, 2008 should be considered for inclusion in the SNAMP fisher report. If your team were to consider Rhodes and Baker (2008), the presumption in your final report that SPLATS of different intensities may need to be evaluated should be revised.

In addition, the SNAMP fisher report clearly indicates on page 32, that, “Data from radiocollared animals from the study area indicated that female fishers commonly die by 6-8 years of age.” Contrary to what the report implies, this means that more than four generations of Pacific fisher will not be able to use the treatment areas. While treating the forest may result in “relatively modest impacts on forest structure,” the impact of treatments on Pacific fisher would be extremely significant!

Ara

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(760) 376-4434
ara@sequoiaforestkeeper.org
I have participated in SNAMP in the following ways (choose all that apply) *

Attended SNAMP meetings, Attended SNAMP fieldtrips, Read SNAMP science articles or briefs, Read SNAMP newsletters, Visited the SNAMP website

Next, please tell us how much you agree or disagree with the following statements. (Strongly agree, Agree, Neutral, Disagree, Strongly Disagree).

- This document is well written: Agree
- This document summarizes research findings well. Agree
- The research findings described in this document are understandable. Agree
- The research findings described in this document can be applied to Sierra Nevada forest management. Neutral

The most interesting research findings in this document are:

One of the four territories in the prospective analysis, and the only territory within the treatment network, had more high severity fire with treatment than without.

The most important findings from this document to apply to Sierra Nevada public forest management are:

Moderate intensity timber harvests, consistent with fuels treatments, are correlated with CSO declines.

The research findings that I found least applicable to Sierra public forest management are:

The prospective analysis is mired by too many caveats and assumptions to be applicable anywhere.

The biggest concerns I have after reading this document are:

The only management recommendation from the prospective analysis is to simply avoid the PAC, a strategy that has led to a 50% decline on the ELD study area and there is no evidence a threshold has been reached. In addition, such a recommendation ignores the use of prescribed fire as an alternative to mechanical treatments to reduce fuels, despite the ever-growing body of literature that suggests low and moderate fire severity fire, with some high severity fire patches, do not affect occupancy. The authors mistakenly assume there would be a long-term fuels treatment benefit of 30 years for spotted owls, but Collins et al. (2011), a study on the effectiveness of the fuels treatments in the LCSA, found that fuels treatments were only effective for about 20 years, at which time re-treatment would be necessary; meaning, between years 20 and 30 post-treatment the treatments effects to spotted owls are entirely negative.

Commentor: Please keep comments confidential in any public summary.
Comments on SNAMP Appendix A-Fire and Forest Health

I have participated in SNAMP in the following ways *(choose all that apply)*

- Heard a presentation about SNAMP at another meetings
- Read SNAMP newsletters
- Read SNAMP emails

Next, please tell us how much you agree or disagree with the following statements. *(Strongly agree, Agree, Neutral, Disagree, Strongly Disagree).*

- This document is well written: Agree
- This document summarizes research findings well. Agree
- The research findings described in this document are understandable. Agree
- The research findings described in this document can be applied to Sierra Nevada forest management. Agree

The most interesting research findings in this document are:

Left blank

The most important findings from this document to apply to Sierra Nevada public forest management are:

Left blank

The research findings that I found least applicable to Sierra public forest management are:

Left blank

The biggest concerns I have after reading this document are:

Given the increasingly broad impact of climate change, tree die off and catastrophic wild fire on the environment, are there new opportunities for SNAMP to have an even greater impact on the Sierra Nevada?

Commentor:
Comments on SNAMP Chapter 5 – User 809683

I have participated in SNAMP in the following ways (choose all that apply)

- Attended SNAMP meetings
- Attended SNAMP fieldtrips
- Attended a SNAMP workshop
- Heard a presentation about SNAMP at another meetings
- Read SNAMP science articles or briefs
- Read SNAMP newsletters
- Read SNAMP emails
- Visited the SNAMP website

Next, please tell us how much you agree or disagree with the following statements. (Strongly agree, Agree, Neutral, Disagree, Strongly Disagree)

- This document is well written: Agree
- This document summarizes research findings well: Agree
- The research findings described in this document are understandable: Agree
- The research findings described in this document can be applied to Sierra Nevada forest management: Agree

The most interesting research findings in this document are:

The most important findings from this document to apply to Sierra Nevada public forest management are:

The research findings that I found least applicable to Sierra public forest management are:

The biggest questions I have after reading this document are:

The biggest concerns I have after reading this document are:

Additional comments for the authors: In monitoring R5 forest health and fuels reduction accomplishments, I find that R5 is removing less than 10% of annual growth so the forests continue to get denser and denser and denser, which is leading to the megafires that are being experienced. With an average of 266 trees/ acre (FIA) across all R5 forests in productive forest lands, SPLATS are too little, too late. We need to take over 1/2 the vegetation off the landscape in a manner that leaves a heterogeneous pattern.

Commentor: Steve Brink, California Forestry Association
Comments on SNAMP Appendix A Fire and Forest Health – User 812964

I have participated in SNAMP in the following ways (choose all that apply) *

- Attended SNAMP meetings, Attended SNAMP fieldtrips, Attended a SNAMP workshop, Heard a presentation about SNAMP at another meetings, Read SNAMP science articles or briefs, Read SNAMP newsletters

Next, please tell us how much you agree or disagree with the following statements. (Strongly agree, Agree, Neutral, Disagree, Strongly Disagree).

- This document is well written: Agree
- This document summarizes research findings well. Agree
- The research findings described in this document are understandable. Agree
- The research findings described in this document can be applied to Sierra Nevada forest management. Agree

The most interesting research findings in this document are: There was not enough treatments to statistically indicate anything.

The most important findings from this document to apply to Sierra Nevada public forest management are:

The research findings that I found least applicable to Sierra public forest management are:

The biggest questions I have after reading this document are:

The biggest concerns I have after reading this document are:

Additional comments for the authors:

P. 16, line 352 - I believe the French Fire vegetation burn severity information is available from the Forest.

Commentor: Steve Brink, California Forestry Association.
Comments on SNAMP Appendix B Spatial – User 823867

I have participated in SNAMP in the following ways *(choose all that apply)*

- Attended SNAMP meetings
- Attended SNAMP fieldtrips
- Heard a presentation about SNAMP at another meetings
- Read SNAMP science articles or briefs
- Read SNAMP newsletters
- Read SNAMP emails
- Visited the SNAMP website
- Other

Next, please tell us how much you agree or disagree with the following statements. *(Strongly agree, Agree, Neutral, Disagree, Strongly Disagree).*

- This document is well written: Strongly Disagree
- This document summarizes research findings well. Agree
- The research findings described in this document are understandable. Neutral
- The research findings described in this document can be applied to Sierra Nevada forest management. Agree

The most interesting research findings in this document are:

The most important findings from this document to apply to Sierra Nevada public forest management are:

The research findings that I found least applicable to Sierra public forest management are:

The biggest questions I have after reading this document are:

The biggest concerns I have after reading this document are:

Additional comments for the authors:

- Linking Lidar data to CWHR classes may not be the best strategy – CWHR is used because it’s all we have. Can Lidar give us a better way to describe wildlife habitat? Also, I do not see where you actually accomplished the link to CWHR classes.
- Please make figure 4 bigger so we can see details. Figures 6 & 7 need a scale for colors.
- 3.7 & 4.7 – How do results of investigating point density to predict forest metrics at the plot scale translate to stand or landscape scale metrics?
- 5.1 – Are tools used (Matlab, etc.) available for other users? What is “multi-path effect”? Combination of “high resolution multi-spectral ... imagery” with what?
- 5.2 – Are there results to support this paragraph? Seems results are not here -- may be more appropriate for wildlife appendices.
- 5.3 – I’m not sure that managers would need this level of detail about fuel treatments to justify the cost.
- 5.4 & 5.5 – Link to results is weak. Will adequacy of use for fire behavior models be discussed in the integration report?

Commentor: Please keep my name and affiliation confidential in any summary of these comments.
I have participated in SNAMP in the following ways (choose all that apply)

- Attended SNAMP meetings
- Attended SNAMP fieldtrips
- Attended a SNAMP workshop
- Heard a presentation about SNAMP at another meetings
- Read SNAMP science articles or briefs
- Read SNAMP newsletters
- Read SNAMP emails
- Visited the SNAMP website

Next, please tell us how much you agree or disagree with the following statements. (Strongly agree, Agree, Neutral, Disagree, Strongly Disagree).

- This document is well written: Disagree
- This document summarizes research findings well: Disagree
- The research findings described in this document are understandable: Disagree
- The research findings described in this document can be applied to Sierra Nevada forest management: Disagree

The most interesting research findings in this document are:

The most important findings from this document to apply to Sierra Nevada public forest management are:

The research findings that I found least applicable to Sierra public forest management are:

The biggest concerns I have after reading this document are:

Commentor:
Comments on SNAMP Chapter 5 User 836008

I have participated in SNAMP in the following ways (choose all that apply)*
Heard a presentation about SNAMP at another meetings, Read SNAMP science articles or briefs

Next, please tell us how much you agree or disagree with the following statements. (Strongly agree, Agree, Neutral, Disagree, Strongly Disagree).

- This document is well written:
- This document summarizes research findings well.
- The research findings described in this document are understandable.
- The research findings described in this document can be applied to Sierra Nevada forest management.

The most interesting research findings in this document are:

The most important findings from this document to apply to Sierra Nevada public forest management are:

The research findings that I found least applicable to Sierra public forest management are:

The biggest concerns I have after reading this document are:

I am uncertain how the integrated recommendation of limiting mastication is justified. The appendix on fisher states that occupancy following mastication was relatively high (0.65). Further, others have found that masticated material decomposes at relatively fast rates (depending on site productivity, cover, etc.). The management implication does not seem to consider that masticated material decomposes within the timeframe of expected fuel treatment longevity. Other work has demonstrated mastication to be effective at reducing fire severity, especially following the period of decomposition. The suggestion to limit its use does not seem warranted.

Additional comment:

Commentor:
I have participated in SNAMP in the following ways *(choose all that apply)*

Attended SNAMP meetings, Other - Provided funding for water team and some of the spatial LIDAR acquisition

Next, please tell us how much you agree or disagree with the following statements. *(Strongly agree, Agree, Neutral, Disagree, Strongly Disagree).*

- This document is well written: Disagree
- This document summarizes research findings well: Disagree
- The research findings described in this document are understandable: Disagree
- The research findings described in this document can be applied to Sierra Nevada forest management: Neutral

The most interesting research findings in this document are:

I think there is some good information buried in the document, but it is written more as a thesis than as a report to an agency. From what I understand, it would appear that forest treatments by themselves alter runoff production in the watershed to a small degree but mitigate runoff changes resulting from a fire.

The most important findings from this document to apply to Sierra Nevada public forest management are:

Forest treatments would seem to offer a way to improve the resilience of the watershed to fire and mitigate the negative runoff outcomes associated with fire events. However, I am not sure if this was the finding of the water team given the report I read.

The research findings that I found least applicable to Sierra public forest management are:

Left blank

The biggest concerns I have after reading this document are:

Additional comment:

I would have liked to see the management application section expanded beyond the single paragraph offered up. A distillation of the modeling results into the possible scenarios of no treatment, treatment, no treatment with fire and treatment with fire would have been really helpful.

Commentor: Michael Anderson, California Department of Water Resources
Comment 27: Leslie Reid, Forest Service Pacific Southwest Research Station comments on SNAMP Appendix E- Water

A few comments after a quick read-through of the Water Quantity section of Appendix E

1. Even if the study is outlined outside of the appendix, it would be useful to provide a brief outline here of the overall strategy for the water-quantity study so the reader starts off oriented with respect to how the pieces described in the appendix fit together.

2. It would be quite useful to provide a clear description of what parameters needed to be estimated or defined by calibration, the values used, and the uncertainty associated with each. This might be most easily presented by an expanded version of Table 12.

3. I’m confused by what weather conditions were actually modeled over the 30-yr period—were these the “average” condition? If so, is that meaningful, given the variation in responses over the 4 measurement years? Is the response to “average” conditions equivalent to the average of responses to the distribution of weather conditions likely?

4. It seems odd that all relations in Fig. 17 (or Fig. 16) would be turn out to be significantly different at the 0.05 level; this should be checked. In particular, relations for 2012 and 2013 in Fig. 17 appear to be nearly colinear while showing significant variance about each. In addition, the units need to be identified.

5. The calibrated discharge predictions in Fig. 18 do not look particularly close to the measured discharges. What is the percentage error for calculated annual water yields for the individual years used in calibration? The depiction of standard error bands for SWE and soil storage provide a very useful depiction of the uncertainties in those values—could something similar be done for discharge?

6. Given that there are so many parameters that had to be established by calibration or other means, it would be extremely useful—in fact, possibly critical—to carry out a sensitivity analysis to identify the level of uncertainty associated with the reported results.

7. Calculated discharges appear to be much less accurate for the validation period at KREW than for the calibration period, suggesting that the model may be significantly overfitted. A statistical analysis (maybe cross validation—check with a statistician on this) should be carried to determine whether this is indeed the case; this would also provide information useful for evaluating the uncertainty associated with reported results.

8. The pattern of variance in Fig. 5 suggests that a linear fit is inappropriate.

9. What is meant by treatment and control in Figs. 16 & 17? Wasn’t treatment carried out after the major runoff period in 2012? It would be useful to distinguish data from before and after treatment.

10. It is not clear whether the 2013 (post-treatment?) data were used in calibration.

11. If the point of the correlations between headwater discharges and main-stem discharges was to establish a basis for estimating the downstream change associated with a change in runoff in the headwater drainages (and this is not clear in the text), then there needs to be very careful consideration of the difference between association and causation. The severity of my sunburn on a given day may be closely correlated with how much sunscreen you apply that day, but if you forget to put on sunscreen one day, that won’t affect my sunburn. This may indeed be the most effective means available for scaling up results, but the potential pitfalls (and associated uncertainties) at least need to be discussed.
12. Figure 15 is difficult to follow because solid shapes are used rather than lines. At the point that the upper margin of shapes cross, the graph becomes uninterpretable.
13. The units need to be identified in Figure 7.
14. There appear to be some critical typos in Table 11.
15. Why does Sugar Pine control ET decrease in the with increasing LAI, while Last Chance control ET does the opposite (p.42)?
16. It would be very useful to provide some discussion of the groundwater loss component. What is it? How is it estimated? What are the implications of the calculated changes?
August 28, 2015

Ms. Susie Kocher  
Central Sierra Cooperative Extension  
1061 3rd Street  
South Lake Tahoe, California 96150

Dear Susie:

Thank you for the opportunity to comment on the Sierra Nevada Adaptive Management Project (SNAMP) Water Team final report. The Water Team did a very good job compiling their extensive data set and modeling work in a readable document. The field results are consistent with the literature and they advance our knowledge about the impacts of low intensity forestry operations on water yield and water quality in the Sierra Nevada. I offer the following specific comments on the document based on my knowledge of the literature and field experience in the Sierra Nevada.

- The entire document should be reviewed by a technical editor to correct grammatical problems (e.g., capitalization).
- The entire document should have consistent use of units. Metric units should be provided first, with English units in parentheses if desired.
- Pg 6: MacDonald (1987) is incorrectly cited in the document and the reference in the literature cited section is also incorrect.
- Pg 7, literature review. I suggest citing and discussing Heard’s (2005) study documenting that water yield changes associated with a prescribed burn covering 60% of a 250 ac basin were not detected in the Sierra Nevada, and including Burgy and Papazafiriou’s (1970) study, that found that chemically treating and burning a small (210 ac) watershed at Hopland Field Station had an average 50% increase in water yield (converting chaparral to grass).
- Pg 8, top of page. I suggest citing Ziemer 1987, providing a realistic expectation regarding water yield increases at larger scales in the Sierra Nevada.
- Pg 9, study sites. More detailed precipitation data for the study sites would be beneficial to the reader. An estimate of annual precipitation can be obtained from the OSU PRISM site: http://www.prism.oregonstate.edu/explorer/. This site appears to provide an estimate of approximately 64 inches per year for Last Chance; how was the 78.3 inch estimate derived?
- Pg 22, model simulations: The modeled water yield increase following a wildfire without SPLATs at the Last Chance site that reduces vegetative cover by
approximately 50% is significant at roughly 67% (35.9 area-inches vs. 21.7 area-inches). The fireshed scale at Last Chance is approximately 10,000 acres. It would be beneficial for the reader to see what water yield changes in instrumented watersheds have been for basins either burned or converted to grass—with different drainage areas—to put this estimated percent increase in water yield in perspective. For example:

- Anderson et al. (1976) and Beschta (1990) reported only a 9% (8 acre-inches) increase for the intensely burned Tillamook Fire in northwestern Oregon for the first 16 years following the burn. These increases were for the large Trask and Wilson River watersheds (91,520 acres and 101,760 acres, respectively).
- Helvey (1974, 1980) reported that during the first post fire year total water yield from the intensely burned Entiat Experimental Forest in Washington was 50% greater than predicted. During water years 1972–1977, measured runoff predictions by 4.2 to 18.6 area-inches. Drainage basin size was approximately 1,200 acres (500 ha).
- Lewis (1968) reported that a 99% vegetation removal in an oak grass woodland in the central Sierra Nevada foothills increased annual water yield 4.5 area-inches. Basin size was only 12 acres.

- The text should include verbiage stating that the large estimated increase in post wildfire water yield at the fireshed scale at Last Chance (approximately 67%), while possible at this scale, will not translate to much larger basins (that are not totally burned), where significant water storage occurs in the Sierra Nevada.
- Pg 43, first paragraph: I suggest referencing Minear and Kondolf (2009) regarding California reservoir sedimentation rates.
- Pg 43, second paragraph: Reference should be Dunne and Leopold 1978.
- Pg 44, first paragraph: Include Reid and Dunne 1996 as a reference indicating that in-channel erosion is an important sediment source in the Sierra Nevada. They completed a sediment budget for the Kings River Experimental Watershed's Teakettle Creek.
- Pg 44, second paragraph: Cite MacDonald et al. 2004 as a Sierra Nevada study that examined sediment transport at the small hillslope plot scale.
- Pg 45, first full paragraph: Several other Sierra Nevada citations should be considered for inclusion here, including Hunsaker and Neary 2012, Reid and Dunne 1996, Euphrat 1992, and Nolan and Hill 1991.
- Pg 46, top of page: Oliver et al. 2011 should be included as a Sierra Nevada example of wildfire (2007 Angora Fire) where sediment, turbidity, and nutrient level changes were documented.
- Pg 46, first full paragraph: Lewis et al. 2001 should be included here as an example of California clearcut logging impacts on sediment yields.
- Pg 63, channel bed movement: No suspended sediment concentrations are reported in the document, and it is not stated what percent of the total sediment yield is composed of suspended sediment. Other work in the Sierra Nevada at the Kings River Experimental Watershed should allow some conclusions to be made regarding this topic.
• Pg 63, management implications: No information is provided in the chapter regarding the locations of forest roads in the studied basins, the number and location of road-stream crossings, the surfacing of the forest roads, road density, etc., and the reasons why sediment input from forest roads was very low for this study. Forest roads and particularly road-stream crossings are known to be significant sediment source areas in Sierran watersheds. For example, Korte and MacDonald (2007) reported that for one KREW sub-watershed, roads were estimated to contribute 25-50 percent of the total sediment yield, and nearly all of the road-related sediment came from a single mixed surface road that crossed the stream a short distance above the weir pond. Detailed information on the forest roads in both the Sugar Pine and Last Chance sites would be beneficial to the reader.

• Added discussion in the Management Implications section on what changes could occur to water quality parameters with severe wildfire should be included (including discussion on key factors affecting erosion and sedimentation, such as vegetative cover, precipitation intensity and storm numbers the first two years after the fire, soil texture, slope, etc.).

Please do not hesitate to contact me if you have any questions regarding these comments.

Sincerely,

Peter H. Cafferata
Watershed Protection Program Manager

References


Comments on sediment aspects of the SNAMP Appendix E

Note: I did not evaluate aspects of the Water Quality section that did not relate to sediment.

p.43 ph.2: If snow cover is the only seasonal condition to be evaluated, it would be clearer if that were stated directly rather than being referred to parenthetically; if it is not the only seasonal condition of interest, then “i.e.” should be replaced with “e.g.”

p.44 ph.1: Even if this is not intended to be a complete list of sediment production processes for the area, it would be useful to include a few more of the potential major sources, such as landslides, sheetwash erosion, road-related erosion, and gullies. In addition, “overland flow” isn’t a sediment production process, but is instead a transport medium (it’s the water, not the sediment)—this would be replaced by “sheetwash erosion.” Soil creep is an odd one—it doesn’t produce sediment, but it transports sediment to a place from which other processes can “produce” it to the stream—that’s where the bank erosion or landsliding kicks in. Bioturbation could use a bit more explanation if you think it important here. Most of the bioturbation going on in a landscape doesn’t produce sediment and ordinarily is implicitly included in creep as a transport mechanism. Do you mean animal burrows and tree throws? Also, mass-failure adjacent to streams would usually be considered a hillslope process if it’s contributing colluvial material, even though the slope may be destabilized by undercutting from bank erosion. The distinction gets a bit arcane, and you probably don’t want to get into it here. Bottom line is that hillslope processes move sediment down the slopes to where it can be transported by streams, and streams can then carve it away by eroding their banks and beds; streams can also directly produce sediment by mobilizing bedrock and saprolite particles. I suspect that the point you’d like to make is that you’re hypothesizing that sediment mobilized high on the slope gets redeposited before it reaches the stream, so that sediment mobilized and transported by the stream during the storms you looked at most likely came from in- or near-channel sediment storage elements during the storm? If so, stating it directly would get you efficiently around much of the complication in categories. You’d still need to say something about landslides, gullies, and road-related erosion, though, since it would be startling if they didn’t play some role here.

p.44 ph.1: “Previous work in stream systems similar to those in this study has suggested that under normal conditions the hillslopes are not very connected to the channels”: this would be unlikely to be valid for the site unless streams are bordered by wide floodplains. If the stream isn’t continually widening its channel, then erosion of colluvial banks is supported by transport of sediment down the hillslopes, and erosion of alluvial banks is mining sediment likely to have originally been sourced from hillslopes farther upstream. Maybe what you mean is that most sediment eroded from sources high on hillsides is likely to be redeposited before reaching a stream channel? That doesn’t mean that hillslopes and channels are at all disconnected.

p.44 ph.1: Stafford (2011) would not have a valid basis for supporting the general statement that “in-channel erosion of the bed and banks are the more important processes in forested mountain headwater catchments”—that’s just not true. There are plenty of forested mountain headwater catchments where landsliding is currently the more important process, and over a period relevant to landform development, hillslope and channel processes generally must be fairly balanced or you’ll either get a slot-canyon or a playa. To establish whether the statement is supportable for a particular period of interest in these watersheds, one would need to make a variety of field observations over a period that includes a range of storm and snowmelt event sizes.
I'm now confused about the timing of treatments—were the years referred to in the previous section hydrologic years or calendar years? When is the hydrologic year defined to begin at these sites? It sounds here like the treatment was carried out before the 2012 mmts (i.e., 2 post-treatment mmt years), but in the previous section I’d been under the impression that there was only one post-treatment mmt year.

Figure 34: Please rescale the discharge units into something more standard than 10.6/3—the current presentation is not useful.

Figure 41: There’s something odd about this figure. Notice the synchronicity between creeks for small peaks in the “kg sediment” trace, despite the lack of a similar synchronicity in discharge traces. In addition, there’s an apparent periodicity to these fluctuations. I wonder whether the “kg” trace might actually reflect the effect of ambient temperature on the circuitry? Are there any cross-section measurements that could be used to validate the interpretation of results for these sites? Or it might be useful to try plotting “kg” against a measure of temperature to see if this might help explain the pattern. Are there other environmental variables that might influence calibration of the instruments or otherwise help explain the odd short-period synchronicity?

Figure 41: kg per what? A 10”x6” Rickly load cell sensor? Check my estimates here, since they were quite literally back-of-the-envelope, but if that’s the case, the 20-kg-or-so change per sensor would translate to a sediment depth change of 20 to 30 cm, which would be the fluctuation in bed elevation. Typical snowmelt peakflows of 50 to 70 L/s shown in the figure are around 2 to 3 cfs (sorry about the mish-mash of units—I have a hard time visualizing 0.05-0.07 m³/s). A bed elevation change of nearly a foot is really big in a channel that has typical snowmelt peaks of 2-3 cfs—it’d be hard to get consistent measurements of discharge if the bed elevation is fluctuating on the order of the water depth. Again, I wonder if the Rickly sensors are measuring something other than what you think they are. Or are the discharge units actually a discharge per unit area?

Figure 41: the caption refers to WY2014, but the graphs don’t go there.

Actually, it’s not possible to infer source from the style of hysteresis. Another likely cause for hysteresis is simply the shift in flow source that occurs as a storm progresses (if I remember right, this was described by Mizumara 1989: Hydrologic approach to prediction of sediment yield. J Hydraulic Engineering 115(4):529-535). In that case, the hysteresis would be present largely irrespective of the source locale. You’ll need field observations of the sediment sources in action if you’re going to diagnose them.

The lack of significant difference seen between the pre-and post-treatment turbidity event means”: it’s not clear exactly what you tested for here. I’m assuming that it wasn’t a straight comparison of means, since that wouldn’t account for differences in storm character and so wouldn’t provide a valid test. What did you test for and how did you do it?

There really needs to be a statistical power analysis here—without that, it’s not possible to state that “The lack of significant difference seen between the pre-and post-treatment turbidity event means” performed and the dry conditions in the post-treatment years.” You would first need to determine 1) the level of change that would be considered operationally significant, and 2) the minimum level of change that could be detected, given the characteristics of the data and analytical method. If the minimum detectable change is greater than the operationally significant change, you’d need to conclude the results are indeterminate: there may be an operationally significant change present that just couldn’t be detected because of the weather and the experimental design.
p.63: There’s not a long enough period of record to determine whether the bed elevations are stable over multi-year periods; that cannot be inferred from figure 41. Here too, you’d need a power analysis. A more efficient method for determining this would be to evaluate the morphology of floodplain and channel—is there field evidence of long-term incision? aggradation?

p.63: As discussed above, because hysteresis likely results in part from changing flow sources, “analysis of turbidity hysteresis loops” cannot be used to infer that “in-channel erosion is main sediment source with sediment accumulation and depletion cycles tied to low and high flows.” You’d need field observations of the distribution of active processes to draw this kind of conclusion.

p.63: “Because in-channel sources dominate sediment supply, it is thought that any increases in sediment transport from treatments will be due to increases in discharge.” Because evidence for the dominance of in-channel sources is missing, the conclusion can’t follow. It may very well be true, but at this point it’s still an unsupported hypothesis. You might want to avoid the construction “it is thought that…” since readers have a tendency to translate that to “I think this is the case but don’t have the evidence to support it.”

p.64: “SPLATS as implemented in SNAMP had no detectable effect on turbidity.” Here’s where the power analysis will be really important—it would allow you to identify the minimum change that could be detected, given the conditions you were operating in. In any case, you can’t conclude that the SPLATS had no detectable effect on turbidity. All you can conclude is that the methods you used did not detect an effect—that’s a very big difference in implications.
A. Response to comments regarding Owl Chapter from Center for Biological Diversity and John Muir Project

Point 1 in Comments

SNAMP REPORT: “We conclude that SPLATs may provide long-term benefits to spotted owls if fire occurs under escaped wildfire conditions, but can have long-term negative effects on owls if fire does not occur.”

COMMENT: The above statement should reflect that mechanical treatments can be ineffective in extreme weather, and therefore the perceived benefit may not even be realized. For example, Lydersen et al. 2014 states: “Our results suggest that wildfire burning under extreme weather conditions, as is often the case with fires that escape initial attack, can produce large areas of high-severity fire even in fuels-reduced forests with restored fire regimes.”

COMMENT: Explain also that even if fire does occur, there may be no long term benefits from mechanical treatments. Instead, fire (including mixed-severity fire with a high-severity component) can occur in ways that provide benefits to owls (e.g. foraging habitat), and therefore, in addition to causing harm to unburned owl habitat, mechanical treatments could also cause harm in terms of how they impact the post-fire condition.

RESPONSE: We changed the statement to clarify that the potential benefits will be if fire occurs under “escaped wildfire conditions.” Lydersen et al. (2014) is one study that only looked at the effects of prescribed burning on severity patterns in a large wildfire. There are numerous studies that demonstrate effectiveness of fuel treatments under high to extreme fire weather conditions, or more generally “escaped wildfire conditions” (e.g., Ritchie et al. 2007, Safford et al. 2009, Safford et al. 2012, Martinson and Omi 2013). Regarding the second-half of the comment, the survival of large overstory trees following wildfire and the potential for treatments to reduce large, homogenous patches of stand-replacing fire are the long-term benefits we are referring to.


Point 3 in Comments

SNAMP REPORT: “For millennia, low- to moderate-severity wildfires occurred at frequent (often less than 0-year) intervals in many western forests, naturally removed fuels such as woody debris, shrubs, and small trees, and shaped the ecology of these forests (Agee 1993, Noss et al. 2006). However, decades of wildfire suppression have disrupted historic fire regimes, increased the amount of surface and ladder fuels, and have led to uncharacteristic proportions and patch sizes of stand-replacing fire wildfires that now threaten ecological and human communities (Mallek et al. 2013, Stephens et al. 2013, Stephens et al. 2014, Stephens et al. 2015).”

COMMENT: The above statement gives the impression that modern fires are not ecologically valuable. That implied message does not reflect a) recent research (e.g., Odion et al. 2014; Baker 2014) that finds that high-severity fire (including large patches of high-severity fire hundreds or thousands of acres in size), as a component of mixed–severity fire, is important to ecological communities in the Sierras, or b) extensive research finding that severely burned post-fire areas (including large patches of it) are critical for species such as black-backed woodpeckers (Siegel et al 2011, 2012, 2013, 2014, 2015; Tingley et al. 2014), 2014), numerous avian species (Hanson 2014; Burnett et al. 2010, 2011, 2012; Roberts et al. 2015; Siegel et al. 2011), bats (Buchalski et al. 2013), as well as owls (Bond et al. 2009) and fishers (Hanson 2015). Moreover, recent fires that are often referred to by the Forest Service as harmful – the McNally, the Moonlight, the Rim, and the King – all contain wildlife habitat created by high-severity fire that has been found to be inhabited by many species, including very rare species (see wildlife articles referenced above which occurred in part in the McNally and Moonlight fire areas; see also recent woodpecker data from the Rim and King fires). It is important therefore to reflect in this Appendix that high-severity fire is not categorically harmful to ecological communities and instead to explain the importance of high-severity fire to the ecological integrity of the Sierras; otherwise, it will continued to be perceived as a complete loss when in fact high-severity fire is essential and important.

RESPONSE: We revised the text in the report to be more clear with our language and added several references to support the revised text. Each of the fires referenced in this comment demonstrates our point regarding uncharacteristic proportions and patch sizes of stand-replacing fire (see references cited in revised text).
Point 6 in Comments

SNAMP REPORT: “As with other western forests, the area burned by high-severity fires in the Sierra Nevada has increased over the past several decades (Miller et al. 2009).”

COMMENT: First, recent research contradicts Miller et al. 2009—e.g., Hanson and Odion 2014; Hanson and Odion 2015. Moreover, even if high-severity fire were increasing, that is not necessarily a problem given the deficit of high-severity fire in the Sierras (e.g., Odion et al. 2014) and the importance of high-severity fire to biota. It is therefore important to reflect in this Appendix that an increase is likely a positive for Sierra ecosystems and is not at all categorically a negative.

RESPONSE: We believe that the weight of evidence supports our assertion that area burned by high-severity (stand-replacing) fire is increasing in the mixed conifer region of the Sierra Nevada (see Miller et al. 2009, Miller and Safford 2012). The Hanson and Odion (2014) study that purportedly contradicts Miller et al. (2009) has been comprehensively rebutted by Safford et al. (2015) and rendered unsound scientifically. Beyond the issue of increasing stand-replacing fire effects in contemporary forests, the proportions of stand-replacing fire are well outside of the historical range of variability for Sierra Nevada pine-mixed-conifer forests (Mallek et al. 2013). The notion of a current deficit of stand-replacing fire is unclear, but what is ubiquitously agreed upon is the deficit of low-moderate severity fire (see Mallek et al. 2013).


B. Response to comment from Sequoia Forestkeeper regarding fire ignition probability

SNAMP REPORT (bold from Comments): As we noted in Appendix C for the California spotted owl, the net benefits of SPLATs for the Pacific fisher will depend upon the true, but unknown, probability that high-severity fire effects will occur on a given portion of the landscape. However, future probabilities for specific fire behaviors (e.g., crown-fire initiation) are difficult to estimate, and it is therefore difficult to quantify trade-offs associated with SPLATs in absolute terms (Finney 2005). We further note that the SPLATs which were implemented at Sugar Pine appeared to have relatively modest impacts on forest structure and simulated fire
behavior, and that it may be necessary to evaluate additional SPLATs of different intensities over a larger scale to fully assess the effects of SPLATs on fisher habitat. Nonetheless, we have no reason to believe that Forest Service managers should alter their current policy of avoiding the placement of SPLATs near known fisher denning sites (U.S. Forest Service 2004) because these sites have significant biological importance for this species.

COMMENT: The SNAMP fisher report fails to acknowledge the existence of Rhodes and Baker, 2008 as well as the other studies of fuel treatment impacts (Law and Harmon, 2011; Campbell et al., 2011; Price et al., 2012; Price, 2012; Restaino and Peterson, 2013) that have also shown: a) that analysis of the probability of fire affecting treated areas, such as that of Rhodes and Baker (2008), is essential to assessing the potential effectiveness of such treatments, and b) that the probability of fire affecting treated areas while fuels are reduced is relatively low. Yet the SNAMP fisher report continues to promote treatments in the hope that the treated areas will experience fire, so the models can hopefully work. The SNAMP fisher report projects fire in treatment areas even though the likelihood is low and the impact to the fisher habitat from treatment would be immediate and negative and it would take 37 years for the habitat to recover from the treatment and show positive benefits.

RESPONSE: The Rhodes and Baker (2008) study is more limited than described above. 1) It assumes that the fire probability observed in the recent past is a reasonable predictor of future fire probability. This is not the case for most of the future fire-climate predictions. Furthermore, for the Sierra Nevada, the fire perimeter data maintained by CALFIRE show a 4-fold increase in fire probability over the last 10-15 years (D. Sapsis, unpublished data). 2) It ignores the fact that initial treatments, particularly those that focus on ladder fuel removal, allow for easier and cheaper follow-up or maintenance treatments, namely prescribed burning. 3) There is an implied assumption that if left untreated fire risk, which includes both hazard (potential loss of key ecosystem components) and probability of occurrence, is static. This is also not the case. We know that hazard continues to increase (see Stephens et al. 2012), and there is reason to believe that probability may also increase due to increased potential spread rates and greater difficulty for fire control (primarily line construction). For these reasons, we consider the Rhodes and Baker (2008) estimate of fire ignition probability not reliable enough to base management decisions. We agree that obtaining a better estimate is a priority and note this point in the SNAMP report. In regard to the probability of fire entering a treated area with reduced fuel loads, we explicitly quantify the longevity of the fuel treatments and illustrate how their effectiveness in modifying fire behavior declines with time (Fig. A20, Appendix A).


C. Response to comments from Dave Martin.

COMMENT 1: In the FFEH Draft Report (page ii), the conclusions about the 4% reduction in fire behavior with and without SPLATS being characterized as “modest”, is not necessarily how
I would characterize such a small reduction. Indeed, there is other science demonstrating greater basal area reductions resulting in greater effects to fire behavior, notwithstanding the issue of studying SPLATS. A more thorough acknowledgement/discussion regarding the nature of the limitations placed on management of SPLATS (in SP) and whether or not it would result in a higher % reduction would, at least point out a limitation of the study. From a fuel hazard reduction/prevention of catastrophic fire management perspective, 4% is unacceptable. I know that was not SNAMP’s charge to make such judgments but clearly articulating the limitations may give greater perspective to the results.

RESPONSE: The overall 4% reduction in potential fire behavior after SPLAT treatments were installed at the Sugar Pine site is small and does not reduce the potential for high severity fire as much as intended by the project. Since this southern Sierra Nevada site is within the Pacific fisher’s range the intensity of fuels treatments were limited. Almost no change in the forest canopy was detected and surface fuels were still moderate after treatment because the anticipated prescribed fires were not applied because of air quality constraints. Ladder fuels were the main component removed at this site which can lower the probability of passive crown fire (Agee and Skinner 2005, Stephens et al. 2009) but can still leave the overall landscape at relatively high risk to severe fire.

The overall goal of protecting the Pacific fisher is logical but leaving large forested landscapes that are the core of its habitat with high fire hazards is likely to fail in the long-term, especially with warming climates. A recent paper that analyzed 1911 landscape-scale (> 25,000 acres) forest structure from mixed conifer and ponderosa pine forests in the southern Sierra Nevada found high heterogeneity in structure before the impacts of harvesting or fire suppression (Stephens et al. 2015). In 1911, total tree basal area ranged from 4 – 261 ft² acre⁻¹ (1 to 60 m² ha⁻¹) and tree density from 1 – 67 trees acre⁻¹ (2 - 170 ha⁻¹)(based on trees > 12 inches dbh). Comparing forest inventory data from 1911 to the present indicates that current forests have changed drastically, particularly in tree density, canopy cover, the density of large trees, dominance of white fir in mixed conifer forests, and the similarity of tree basal area in contemporary ponderosa pine and mixed conifer forests. Average forest canopy cover increased from 25–49% in mixed conifer forests, and from 12–49% in ponderosa pine forests from 1911 to the present. Current forest restoration goals in the southern Sierra Nevada are often skewed toward the higher range of these historical values, which will limit the effectiveness of these treatments if the objective is to produce resilient forest ecosystems into the future, as was found in the Sugar Pine site. Allowing more of the mixed conifer forests in the Sugar Pine area to received treatments that produced forest structures similar to those found in 1911 would have reduced potential fire behavior more than the 4% observed in this study.

COMMENT 2: Another area where additional science assists land managers in making management decision and something that, perhaps SNAMP could not really measure (and/or draw conclusions upon) is the idea of sustainability of the stand structure in the key fisher denning and foraging habitat stands, which by design and management restrictions were effectively off limits from any substantive vegetation treatments. Most of these stands are overstocked from a tree-health perspective. Catching overstocked stands “in the act” of losing vigor and increasing mortality in a 7 year period, even with 4 below-average rain years is difficult, if not impossible but is something that I also feel could have been highlighted as a limitation/ caveat in the conclusions not only in the FFEH but also the Wildlife sections. Many of the stands presented to SNAMP, although looking like mixed conifer, were in fact pine stands. Railroad logging and fire suppression changed that so a different profile was presented. It is one of higher stand densities (typical of MC) and larger trees (owing to the timing of RR logging and the removal of the dominant, more representative pine). This structure lends itself to much larger trees, higher Basal Areas and conditions much more favorable to quality fisher habitat. Much of the reason white fir was not reduced as much, as would be preferred, is the fact that in all areas, especially key fisher areas, many that those trees are too big to be able to manage in lower, more ecologically sustainable number and sizes (for a pine stand). They were either > 30” DBH or they were the primary component of the dominant/codominant canopy structure needed for retention. Both of these restrictions are part of the management box we had to play in. The control area (Nelder) is GROSSLY overstocked and we feel may put the giant sequoias in jeopardy not only from fire but also competition-induced tree mortality. Literature review over the past many years (and decades) does not support the long-term sustainability of such “artificial” stands, especially in view of the reality of climate change. At least putting greater emphasis on a potential management challenge of sustaining what is likely “artificial” fisher habitat would help elevate the true nature of tradeoffs in managing for such habitat and the potential effect of FFEH.

RESPONSE: The dramatic increase in growth efficiency observed at Sugar Pine in response to modest-to-minor reductions (Fig. A21B) in stand density supports the contention that trees in the Sugar Pine forest are under severe competitive stress. We also agree that the Nelder Grove (control area) is even more crowded than Sugar Pine (15% higher basal area than the reference, Table A9). But these differences in competitive stress have not yet manifested themselves in increased tree mortality (Fig. A8). Projections from the growth models for Sugar Pine lend further support to the argument that the forest is crowded. Under the “no treatment, no fire” scenario, there is no projected future increase in basal area (Fig. 3-4, Chapter 3). In short the forest is not growing due to competitive constraints. We emphasize the value of SPLATs to improving forest health in Chapter 5 (12th management recommendation). The question of the sustainability of the forest structure at Sugar Pine is crucial, but our empirically informed modeling results suggest stasis at Sugar Pine not decline (Fig. A21B). Also the long-term data from old-growth mixed conifer forests in nearby Yosemite and Sequoia-Kings Canyon National Parks suggest that these highly stocked stands have persisted for the last three decades (Fig. S2, van Mantgem and
Of course van Mantgem and Stephenson (2007) also point out the recent increases in tree mortality driven apparently by a warming climate. Thus, while there is some support for the notion of an at-risk, “artificial” stand structure at Sugar Pine, our results are not definitive. While we intentionally frame management recommendations in terms of a specific goal, the trade-offs between managing for fisher habitat and tree growth efficiency are made implicitly. This approach follows the framework developed collectively among the SNAMP stakeholders.


D. Response to comments from Peer Reviewer #9.

Thank you for the positive review.

E. Response to comments from Peer Reviewer #10

GENERAL COMMENT 1: The authors may not have been tasked with addressing how the treatment effects may be altered as climate changes, but any modern assessment of multi-decadal treatment effects should have such an assessment, in my opinion.

RESPONSE: We agree with Reviewer #10 about the impact of climate change, especially for results from models that project effects of SPLATs and fire thirty years forward. Climate is a pervasive driver of forest and fire dynamics. However our charge, as broad as it was in the Memorandum of Understanding among the agencies, did not include nor fund analyses that included climate change. In our summary statement (pages A61-62), we did acknowledge the importance of global environmental change to both the ecology and management of the Sierran forests.

GENERAL COMMENT 2: The Appendix to the report, titled “Appendix A.1, Spatial and temporal components of historical fire regimes in a mixed conifer forests [sic], California”, describes a fire history reconstruction study done in a portion of the SNAMP landscape. The study in the Appendix appears to have been carried out competently in terms of sampling and some of the analysis. However, there are additional analyses that would have been important to include. In addition, the text suffers from some unsupported statements, some exaggeration, and a remarkable number of spelling and grammatical errors. In its current form, the Appendix is not suitable as a professional report.

RESPONSE: These comments are general and do not offer any specifics for improving the report. The reviewer did generously offer to provide detailed suggestions later, but we did not receive them in time to respond here. We believe that the disconnect is due, at least in part, to expectations of the reviewer. Instead of doing a traditional analysis that would be similar to nearby published fire history studies, we produced spatially explicit fire return interval maps for both sites. This method provides additional information on how fire historically varied across the landscape. Nonetheless, we removed Appendix A.1 to revise more thoroughly and will submit separately as a peer-reviewed publication.
ITEMIZED RESPONSES TO DETAILED COMMENTS
(note that line numbers refer to the draft version of Appendix A).

1. General comment on units: in part of the text, both English & metric units are used, in one part metric only (e.g., L. 429), and in the Tables English only. Please be consistent.

RESPONSE: Good point. Given our goal of informing management, we have used English units for the measurements and provided metric conversions in the text. However in the tables and figures, the goal of a clean, uncluttered design prevents the inclusion of metric conversions. Thus we have revised all our tables and figures with English units as the primary measure.

2. Abstract p. i: should read “in order to quantify…” , not “quantity”

RESPONSE: Fixed typo.

3. L. 16: of the three references here, only one is relevant to the species in the present study and mentioned on L. 12-13. The Brown and Naficy papers are on ponderosa pine forests in South Dakota and Montana, respectively.

RESPONSE: Ponderosa pine is a major component of forests in the SNAMP study but the reviewer is correct in noting the differences between the forest communities. We have deleted the Brown and Naficy references and added the Knapp et al. (2013) reference, a study conducted in the Sierra Nevada.


4. L. 25: would be helpful to give a quantitative range of reduced fire risks associated with treating 30% of the area, rather than just saying fire risk “can be decreased”. Presumably fire risk would be “decreased” due to treating 1% of the landscape, or 99%. The point Finney was arguing is that “strategically” located treatments would give a disproportionate benefit in term of reducing the likelihood of severe fire.

RESPONSE: Good point. We have revised the sentence and added a specific example to illustrate the “disproportionate benefit.”

5. L. 38-40: awkwardly worded sentence starting “This concern…”

RESPONSE: Sentence revised.

6. L. 45: besides adding sediment (due to soil disturbance associated with treatment?), in what other way would the treatment “lower water quality”?

RESPONSE: Sentence revised.
7. L. 57: should read “forests OF the Sierra Nevada…”

RESPONSE: Fixed.

8. L. 61: Westerling’s study was over a much larger area, the western US.

RESPONSE: True but the conclusions regarding earlier spring snowmelt and increases in large fire frequency specifically pertain to the Sierra Nevada. Indeed some of the largest effects seen are in the Sierra Nevada (Westerling et al. 2006) based on the maps depicted in Fig 4 (forest vulnerability to changes in spring timing) and Fig S2 (fire reporting by location). Thus we have retained the reference.

9. L. 64: the “fire deficit” described by Marlon et al. appears to be mainly the result of fire suppression, as the authors stated, but it reflects a deficit of surface fires, the theme of the present study, as well as severe fires, which in the context of this study are to be avoided. The Marlon reference blurs the distinction between surface vs. severe fire regimes & therefore is not helpful in the present context (in my opinion).

RESPONSE: The point we were making with this reference, which includes the following sentences in the paragraph, is that there is currently less fire on the landscape than would be expected given the current climate. This, combined with other evidence about departed fire patterns in these forests relative to historical conditions, suggest that there is real potential for future fires to not only continue current trends, but perhaps worsen. We added a sentence to clarify this point at the end of the paragraph.

10. L. 93: would be useful to give a quantitative figure, rather than “noticeable reductions”.

RESPONSE: We disagree on the need for more a more quantitative description. The inconsistencies in metrics used in each of the studies (i.e., fire size, conditional flame lengths, conditional burn probabilities, etc.) limit the comparisons to qualitative descriptions.

11. L. 106-110: great point about buying time. Note the reference to “restoration activities”, see comments below.

RESPONSE: See response below.

12. L. 115-158: the authors give a reasonable presentation of the literature on “forest health”, a concept with strengths and weaknesses. They make a logical case for the selection of tree mortality and tree growth as useful indicators of tree health and, roughly, “forest health” in this study. However, the review is brief and limited. A surprising omission, in the light of the
Forest Service’s emphasis on ecological restoration and the references to restoration in the text itself, is the literature on ecological restoration. A basic element of restoration is the reference condition, which includes as much information as possible about ecosystems prior to recent human-caused degradation: plants, animals, forest structure, ecological processes, influences of non-industrial human societies, etc. See for example the Primer by the Society for Ecological Restoration. Many of these elements are mentioned in the text here and in the Discussion, but not in an integrated manner.

RESPONSE: Forest health is an over-arching theme of SNAMP addressed from multiple perspectives by the various research teams. Clearly the status of sensitive wildlife species is relevant to forest health as is the quality and quantity of the water. Given that the term is meant to help convey a synoptic sense of forest condition to stakeholders, there is also an element of public perception to the concept of health. Even the Spatial Team with their deconstruction via remote sensing of spatial heterogeneity contribute to the evaluation of health. We acknowledge that our efforts to quantify forest health take a tree-centric approach. This approach is grounded on the premise that healthy trees are a necessary but not sufficient quality of a healthy forest. Thus, we focus on the vegetation response in this report. As part of the SNAMP integration process (Chapter 4), we bring together these elements in a consistent framework, one that more closely matches the reviewer's understanding of the term.

Regarding the link between forest health and ecological restoration, we agree that the status of North American ecosystems before colonization by Europeans is often considered an example of a healthy ecosystem. We also agree the ecological restoration is a priority for the US Forest Service. However, we disagree that the goal of SPLATs as envisioned in the 2004 Amendment to the Sierra Nevada Forest Plan is to restore the forest to a desired future condition defined by pre-European conditions. Instead, SPLATs are more a step in the right direction toward modifying an altered fire regime and improving tree survival and growth by reducing tree density and surface fuel loads. SPLATs is a form of triage designed to stabilize worrisome trends in fire hazard and tree morbidity until a more lasting solution can be found (see pages A1ff). Thus SPLATs are a step in the right restoration direction but they are not designed to return the landscape to a desired future condition. Thus detailed descriptions of potential target forest composition and structure are not relevant here.


RESPONSE: Revised.

14. L. 183: not clear whether the figure of 1182 mm refers to total precipitation (water equivalent) or to snow depth?

RESPONSE: Revised to make clear that 1,118 mm is the mean annual precipitation.

15. L. 208-218: it’s not clear what is the relationship between the cited studies (e.g., Scholl and Taylor 2010) and the fire history study in Appendix A1. Would be helpful to clarify the distance to the previously studied sites, the species, etc. As written, since the cited studies are
used to describe the pre-Euro-American fire regime, it is not clear to the reader why new fire history data were collected. (Incidentally, given the long history of Spanish colonization, what do the authors consider to the date of “Euro-American settlement”?).

RESPONSE: We revised the sentence to make it clear that we referring more generally to similar forest within the Sierra Nevada. The revised sentence now reads: “Fire history, inferred from fire scars recorded in tree rings, suggests the fire regime prior to systematic fire suppression and widespread timber harvesting in Sierra Nevada west-side pine-mixed conifer forests was dominated by frequent, low-severity fires occurring at regular intervals (Stephens and Collins 2004, Scholl and Taylor 2010).”

16. L. 220, Methods section: field sampling was logically designed and a substantial amount of data was collected.

RESPONSE: No response needed.

17. L. 239: “were navigated to using…” is awkward.

RESPONSE: Revised.

18. L. 240: should read “Garmin”.

RESPONSE: Corrected.

19. L. 264: please give more detail about prescriptions: was there a target forest structure? Residual basal area?

RESPONSE: Targets for residual basal area and canopy cover retention added. Note that these are descriptions of the planned treatments. The actual treatments applied are quantified in great detail later (Tables A5-9, Fig. A7).

20. L. 282-292: the authors did a good job of assembling the treatment boundary data but this section highlights the ridiculous situation of the lack of detailed spatial information about treatments in the National Forests. The Forest Service doubtless retains all receipts for repair of its vehicles or purchase of a desk, but is incapable of adequately describing the contracts for management treatments on FS lands!

RESPONSE: We agree. It would be helpful if there were a more streamlined way to track treatments on public lands.

21. L. 299: the Lidar data should be explained to the reader. The report references an Appendix B which was not included for this review.

RESPONSE: We appreciate this criticism. We do not provide much explanation of the lidar data and how it is used our section. However, in the complete report on the SNAMP project, the
acquisition of remote sensing products and spatial analysis are described in detail. This chapter immediately follows ours. Thus, we refrain from repeating information here.

22. L. 323: why were numbers of seedlings randomly generated? A citation is given but the logic should be explained and justified.

RESPONSE: The numbers of seedlings were randomly generated within bounds that varied by species. This was done to attempt to represent the variable regeneration conditions observed across the studied landscapes. We added a sentence to explain this reasoning.

23. L. 326-331: please clarify the role of FVS & FFE. Were any fires simulated in FVS-FFE? Or were these models used solely to grow trees & track fuels, in the absence of fire, with all fire behavior simulations occurring in FARSITE or FlamMap?

RESPONSE: We have revised the methods describing our fire modeling (pages A18ff). First we explain the use of FVS to generate landscape forest structure maps for fire behavior modeling, for each of the treatment scenarios. Next, we explain our dual approach to fire behavior modeling with FARSITE and FlamMap. FVS was used to simulate fire effects (i.e., changes to forest structure) using the fire behavior outputs from FARSITE. We acknowledge that this is not clear in the text so we added several sentences to describe this step.

24. L. 342: describe the use of Lidar to describe topography. My (limited) understanding of Lidar was that different measurement approaches would be appropriate for measuring vegetation vs. measuring the surface underneath the vegetation.

RESPONSE: The reviewer is correct. Lidar alone was used to predict topography. The vegetation map used a combination of remote sensing products (lidar, NAIP imagery) and field plots. To avoid redundancy, we reference Appendix B of this report for the details of the lidar applications.

25. L. 344: the use of a “problem” fire is traditional & reasonable, but the reader would expect a discussion of the limitations of the concept.

RESPONSE: This is a good point. We added the following text at the end of this paragraph in the start of the next one: “This approach of using a single simulated fire for each treatment scenario (with and without treatment) limits inference that can be drawn from these results due to potentially different fire spread and behavior associated with different ignition locations. We used a single fire in order to obtain specific predictions on how fire would impact forest structure via tree mortality, as opposed to probabilistic predictions on fire occurrence at a specific location (e.g., Ager et al. 2007). By having spatially explicit predictions of fire effects on forest structure, we were able to track the impacts of fire on owl habitat and make more direct assessments of owl demography over time.

26. L. 355: was spotting enabled in the FARSITE simulation? See later comment about windspeed. In a real fire, higher windspeeds and spotting would be a critical combination.
RESPONSE: Yes, spotting was enabled with a 2% ignition frequency and a 2 min. delay. This is already in the text.

27. L. 359: should read “used A command-line…”

RESPONSE: Fixed.

28. L. 415: true that the design “accounts for” changes, but the BACI design is limited, with no or limited replication.

RESPONSE: Agreed but for ecosystem-scale research, true replication is hugely expensive. The BACI design used here (the paired firesheds) is a well-established alternative to replication that does provide some measure of reference to statistical evaluate the impact of treatments. However, there are limitations. Work by Murtaugh (2000, 2002) suggests that BACI designs tend to overestimate significant impacts (although see Stewart-Oaten 2003). Most of our statistical tests of treatment impact were not significant. Thus our conclusion of limited treatment impact on forest-wide composition and structure are conservative with respect to the statistical bias of BACI analyses.


29. L. 430: is the term “conditional burn probabilities” a standard term? It sounds confusingly similar to the concept of “conditional crown fire” suggested by J. Scott. Please clarify and please also explain how the authors dealt with the situation called “conditional crown fire”, when the canopy bulk density is sufficient to carry crown fire horizontally but the canopy base height is too tall for simulated vertical propagation of the fire.

RESPONSE: Burn probability is a standard term (see FlamMap supporting documents) and is computed by dividing the total number of times a pixel is burned by the total number of simulated fires. The probabilities are “conditional” on the occurrence of an ignition within the larger buffered study area, under the modeled moisture and wind conditions. Issues with accurately estimating canopy base height are known, as well as the limitations in fire behavior models. The canopy base heights used in the model are from out plot data. The most common approaches to deal with the underestimation are to increase fuel models, fuel moisture, or weather conditions. We chose to adjust our fuel model assignments similar to the methods described in Collins et al. (2011, 2013), where assignments are skewed towards a more active fuel model (e.g., TL9 in place of TU2).


30. L. 434: where does the 2-m flame length threshold come from?

RESPONSE: As mentioned in the text, we used methods described in Collins et al. (2011, 2013). A 2m cutoff allows us to separate out more problematic simulated fire occurrence, both from a fire effects and a fire suppression standpoint. Flame lengths >2 m typically correspond with crown fire initiation and present substantial challenges for suppression efforts (NWCG 2004). Further, using a fixed cutoff based on individual stand conditions (e.g., Ager et al., 2007, 2010) such as canopy base height places too much weight on a single, influential variable that can often be problematic to measure and may exhibit unrealistic ranges in calculated values given small changes in stand conditions (Rebain 2010).


31. L. 446: it seems quite illogical to calculate the trees that were intentionally cut as “mortality”. Why was this done?

RESPONSE: We included the harvested trees in our estimate of mortality as another means to quantify the impact of the treatments on the entire fireshed. Thus, in Fig. A8 we are able to show that annual mortality (harvest trees NOT included) between the control and treated firesheds did not significantly differ over time. When we include harvest mortality, we do get significant differences between treatment and control. We realize that harvested trees are not typically considered as mortality, but we need a consistent metric. We use one term and provide a clear description of how it was calculated.

32. L. 495: delete the word “on”.

RESPONSE: Fixed.
33. L. 512: LAI appears here as a critical variable, yet it was not measured in the Methods. Where did the LAI values come from? How reliable are LAI estimates?

RESPONSE: We added details on the calculation of the LAI values in the text. The data came from a recently published study (Jones et al. 2015) that destructively sampled trees from across the Sierra Nevada. The LAI estimates are very reliable with generalized $R^2 \geq 0.87$.


34. L. 644-647: complicated way to say that trees from dense stands have a large growth response when released by thinning...

35. L. 693: appropriate discussion of the weaknesses of the fire behavior model. See also comment on windspeed below.

RESPONSE: No response needed.

36. L. 709: reasonable discussion of CBH modifications.

RESPONSE: No response needed.

37. L. 720: good discussion of fuel model issues.

RESPONSE: No response needed.

38. L. 738: Lidar needs more explanation.

RESPONSE: We have revised and expanded the explanation of the problem of abrupt transitions between stands. These transitions are introduced by the necessity of converting a finely resolved pixel-based vegetation map to the more coarse-scale stands needed for fire modeling. The breaks across stands are artefacts of the scaling but they can contribute to unpredictable fire behavior.

39. L. 762-764: authors say there is no explanation beyond self-thinning, but what about drought effects during these years?

RESPONSE: The interval between pre and post-treatment measurements included no more than the first two years of the current drought (remeasurements were conducted during the 2013 growing season). Thus, drought may play a role but the decrease in understory density was observed only at one site (LC). If it were drought, we would expect a more regional consistent response.
40. L. 798 & 806: here, very late and only mentioned in passing, are key issues of ecological restoration and global change. These themes merit more discussion in the context of the treatments.

RESPONSE: See previous responses regarding 1) the role of SPLATs in ecological restoration and 2) the charge to the science team in relation to climate change.

41. Table 2: these windspeeds are extraordinarily low, averaging 6-10 mph. Are these mid-flame or 20-ft windspeeds? Are the problem fires in the Sierra Nevada really associated with such low windspeeds? Please clarify, justify, or change if appropriate.

RESPONSE: Wind data used in the fire modeling were from nearby RAWS, which are considered to be 20-ft windspeeds. In both models, we used either actual wind data (FARSITE) or 95th percentile weather conditions (FlamMap) and these are the methods typical used. There are several limitations of wind measurements and use in the fire modeling programs, including averaging measurements over time which underrepresents wind gusts, wildfire induced winds are not considered, and a poor understanding of wildfire-winds interactions, and wind patterns over complex terrain.

42. Table 4: the Last Chance site seems quite open at the beginning, BA 133 and TPA 252. Is this really a fuel problem?

RESPONSE: At the landscape scale, the LC forest definitely has a more open structure with smaller canopy trees than SP. Certainly, part of the difference is due land-use history with a greater emphasis on timber cutting at LC. However, in absolute terms, the contemporary forest is more closed than pre-fire suppression forests (Collins et al. 2011). Also this fireashed average includes the shrubland and woodland patched in the landscape. There are extensive tracts of dense mixed conifer forests -- 59% of the area has canopy cover >60%. These dense stands do present a fuel problem.


43. Table 4: what is Lorey height & why was it used? Please also present actual height values.

RESPONSE: Lorey height is the mean tree height weighted by the tree's basal area. Thus, Lorey height is calculated by multiplying the tree height by its basal area and then dividing the sum of this calculation by the total stand basal area. It is a more stable measure of stand height since the removal of small trees has only a minor influence on the mean. We report it because the Spatial Team found Lorey height (as estimated via lidar) to be one of the three primary determinants of vegetation type. However, we agree with the reviewer that mean tree height would be a better descriptor of the stand. We have revised Table A4 as suggested.

44. Table 6 & 7: why such a big difference in mastication effects on fuels, with a doubling at LC but little effect at SP?
RESPONSE: Fuels were measured using line transects before and after treatments. There were 5 masticated plots at LC and 10 at SP, and we relied on treatment polygons from the FS and field observations from our field crew to identify treatment type. It’s unclear why there are such large differences in the temporal changes between sites. Two possibilities include the chance that the few masticated plots at LC were located in a productive area of the forest with high shrub cover, and undocumented pile burning may have occurred at SP that removed activity fuels from those plots.

45. Table 7: column headers out of line.
RESPONSE: Corrected.

46. Table 8 & 9: shouldn’t the treatment impact be “delta mu” rather than “mu”?  
RESPONSE: Corrected.

47. Figure 7: repeats data given in Table.
RESPONSE: Figure A7 shows plot data averages for pre- and post-treatment conditions, by treatment type. Data illustrated in the tables are summarized by site and fireshed.

48. Figure 9: illustrates the minimal (negligible?) impact of these treatments.
RESPONSE: Yes we agree that there were no changes in tree-size distribution due to the SPLATs. As we state on page A36: “There were no changes in tree size distribution in pre-to-post treatment greater than 10% in any of the firesheds.”

F. Response to comments from Steve Brink, California Forestry Association.

COMMENT: The most interesting research findings in this document are: There was not enough treatments to statistically indicate anything.

RESPONSE: We presume this comment refers to the lack of statistically significant treatment effects on forest structure (e.g., canopy cover, basal area) reported in Tables A8 and A9 in the report. The comment is not entirely accurate. There was a significant reduction in understory tree density at Last Chance (Table A8) as well as significant increases in overstory tree mortality at both sites due to the treatments (Figure A8). However, the general sense of the comment is valid – the impacts of the treatments on forest composition and structure were modest. As we explain in the report (page A13), treatment area was limited to only a fraction of the landscape. So the majority of the treated fireshed (> 70%) is identical to the control fireshed in that it was not treated. Thus, detecting significant change is a very high bar. Also, it is important to note that the most relevant impact of SPLATs is not changes in composition and structure but on wildfire behavior. This response is measured in our fire simulations and reported in Table A10. Given that the results are from models, statistical
tests of the treatment impact are inappropriate. We extend the immediate pre-post effects reported in Table A10 with the results from our simulation scenarios over 30 years (Figure A20). In aggregate, these analyses are consistent; they demonstrate modifications to fire behavior due to SPLATs.

COMMENT: P. 16, line 352 - I believe the French Fire vegetation burn severity information is available from the Forest.

RESPONSE: As with all of our fire severity images we download the data from Monitoring Trends in Burn Severity (http://www.mtbs.gov/). At the time of this report the French Fire image was not available from the website.
SNAMP Spatial Team  
Chapter Revision Document

Maggi Kelly and Qinghua Guo, Co-PIs of the Spatial Team of SNAMP

August 19, 2015

We received two peer reviews for our Spatial Team Final Report (Reviewer #2 and Reviewer #7), and several comments via the online survey tool. We address these comments in this document, and have revised the chapter.

Reviewer #2  
General Comments:

1. Did the Spatial Team provide the best available ALS products to the Science Teams?  
a. In short, the answer is yes. ALS systems measure topography (elevation, slope, aspect) and a wide variety of vegetation canopy heights and densities directly, i.e., no models need to be developed to calculate these height and density numbers once a ground surface is defined. These include many, though not all, of the height, percentile, and pulse density metrics listed in Table 1, page 16 and 17. From these measurements, a second suite of useful estimates can be derived, e.g., Lorey’s height, tree volume, tree biomass, canopy base height, canopy volume.

   Response: These are good suggestions.

b. The Spatial Team developed their multi-temporal ALS measures and estimates using a multi-stop ALS system. They write repeatedly in their final report that a small-footprint waveform system might have provided better results. Such waveform systems are now commercially available (e.g., www.riegl.com), but I am not convinced that the extra data load and post-processing requirements provide a significant advantage except in situations where users want to summarize vertical vegetation profiles on relatively small raster cell sizes, e.g., smaller than a 5m x 5m cell. Admittedly, my previous statement is arguable, however Skowronska et al. (2011, Remote Sensing of Environment 115: 703-714) have reported some success estimating canopy bulk density and canopy fuel weight with an ALS multistop system. I suggest that the authors not characterize a small-footprint waveform system as a possible solution to multistop limitations until direct comparisons can be made. The authors may be correct, but unless you can cite a refereed study where waveform outperforms multistop as regards prediction of, for instance, forest fuels variables, e.g., canopy base height, canopy bulk density, you should not make the claim.

   Response: Good point. We have revised the text.  

   Here are some references we have that showing waveform outperformed the discrete lidar in estimating forest parameters, such as biomass, detecting individual trees, etc.

   We agree that a discrete lidar system can produce accurate enough forest parameter estimations in most cases. However, there are studies that show that small footprint full-waveform lidar system outperformed the discrete
airborne lidar system in mapping forest aboveground biomass, delineating individual trees, etc. (Cao et al. 2014; Reitberger et al. 2009).

References

2. Could SNAMP have been managed better so that Spatial Team products better addressed the concerns of the four Science Teams?
   a. Yes. The USFS should have implemented their forest fuels treatments on schedule so that the original BACI approach could have been implemented. As written, the seven year SNAMP was designed (1) to study empirically (via field sampling and remote observations) post-treatment effects over a 0-5 year time period and (2) to model post-treatment effects out to 30 years. The UC field teams started pre-treatment data collection in 2007. The fuels treatments were not done until 2011 and 2012. Only 1-2 years of post-treatment fieldwork was done. The fact that the fuel treatments were delayed for 3-4 years certainly significantly impacted the 5 year empirical study and most likely impacted the long-term models. This comment is not meant to reflect badly on the Spatial Team since they most likely had little control over treatment implementation, but it does reflect poorly on overall project management.
   Response: Thanks for the comment.

   b. The multitemporal ALS flights should have been timed to be seasonally coincident to improve comparability. To manage costs and logistics, SNAMP wisely took a case study approach, selecting two study areas, one in the north (Last Chance), and one south (Sugar Pine). The Last Chance ALS data collects were done in September 2008 - pre-treatment, and November 2012 and August 2013 (post-treatment). The Sugar Pine collects were done September 2007 (pre-treatment) and November 2012 (post-treatment). Both north and south sites are most likely predominantly coniferous, but if there are significant hardwood components on each area, then leaf-on / leaf-off differences pre- and post- fuels treatment will be convolved, adding noise to any comparisons made and uncertainty to any of the conclusions reached. Project managers should have insisted that multitemporal ALS data collects be done in the same month years apart, preferably leaf-on, preferably mid-summer.
   Response: We agree.

   c. Although the Spatial team provide rasterized products to the Science Teams at requested cell sizes, I was surprised to see that you base these raster products on individual tree delineation and measurement. I wonder why you made this choice as opposed to using an area-based approach used by Næsset and many others. The individual tree delineation numbers that I see in the literature vary from about 60 - 80%, meaning that individual trees are properly located and identified for
measurement only 60-80% of the time. Understory trees and trees in closed canopy situations are typically undercounted. Somewhere in your report I would suggest that you add a paragraph that explains what you gained by delineating individual trees in the ALS data. I do not say that what you did is wrong, but your individual tree approach brought along with it a host of problems that included individual tree omission and commission errors, increased sensitivity to relatively small GPS misregistration errors in the field and in the lidar data, a more complicated field protocol that included logging locations of all individual stems and crowns. What did you gain, and in hindsight, would you do it the same way again or make some changes such as considering an area-based approach with a minimum cell size? In your defense, I understand that at least two of your Science Teams - Wildlife and the fire fuels treatment team - would be interested in changes as regards specific individual trees, and that consideration may have drove your decision to identify and measure individual canopies.

Response: We agree that area-based individual tree segmentation method is a more commonly used method. These area-based methods relied on the fine-resolution rasterized products (i.e., canopy height model, CHM) to identify the crown location of each individual tree. However, as mentioned by the reviewer, the problem of significantly underestimating the understory trees has been reported by many studies (Vauhkonen et al. 2011). Furthermore, the CHM-based method uses lidar data that only describe the outer surface of tree crowns, and thus, the advantages of lidar data are not fully exploited. In SNAMP, we developed a new method to delineate individual trees directly from the raw lidar point cloud. These kind of vector methods have been reported to improve individual tree segmentation accuracy (Jakubowski et al. 2013), and have been used in other studies more and more widely (Vega et al. 2014; Lu et al. 2014)

References


3. Is the report clearly written? Can changes be made which might improve Spatial Team Final Report readability?
a. In general the report is clearly written, however the specific suggestions below should improve readability. By way of generalities, the authors should clearly draw a distinction between measured variables and estimated variables since the latter incorporate model error. Too often, the variables are muddled together in tables and text. For instance, you measure maximum height or average canopy height or various height or density deciles (or quintiles or quartiles) on a given cell by accumulating first-return and secondary-return laser ranging information as needed. You’d estimate that cell’s biomass or canopy bulk density or Lorey’s Height by developing an equation that relates field estimates of biomass or canopy bulk density or basal-area-weighted tree height to some subset of ALS measurements listed in the previous sentence.

Response: We agree. The lidar-derived topographic and forest parameters can be generally categorized into two groups, directly computed from lidar range measurements (e.g., digital elevation model, digital surface model, canopy height model, and canopy quantile matrices) and indirectly computed from regression method between the directly obtained lidar products and field measurements. We have clarified the method used to calculate each product in the corresponding text and Table 2.

b. Finally, define acronyms when first used. This report may be read by folks (like me) not familiar with SNAMP.

Response: We have endeavored to do this throughout.

Specific Comments:

1. Should the title of this report be “Sierra Nevada Adaptive Management Project (SNAMP) - Spatial Team Final Report” (instead of Plan)?

Response: Done.

2. pg 5. Define AGB. I realize that AGB = aboveground biomass, but is it total aboveground dry biomass, green biomass, stem only, all aboveground components including leaves/needles? Does tree volume equal stem volume to a certain top limit, or are you actually talking about the volume of space defined by the outer periphery of the tree crown?

Response: Defined AGB. Tree volume refers to the volume of space defined by the outer periphery of the tree crown.

3. pg 5. Use of Lidar for biomass estimation: You write the following: “… the availability of, and uncertainly in, equations used to estimate tree volume allometric equations influences the accuracy with which Lidar data can predict biomass volume.” Two things: First, the majority of the uncertainty associated with biomass estimation using lidar data has to do with the fact that, with ALS data, we don’t know the diameter of the tree. With ALS, we estimate biomass (or stem volume, for that matter) based on height and canopy density. The primary driver in ground-based allometry is diameter, not height. The choice of allometric equation certainly makes a difference, but our inability to measure or infer dbh drives the uncertainty in ALS estimates of biomass. Second, what is biomass volume? Do you mean biomass density, i.e., biomass weight per unit area, e.g., 250 t/ha? Or biomass weight within a certain crown volume? I’ve worked in this field for 30+ years and have never heard the term biomass volume. Define.
a. Response: We agree that the ALS cannot obtain the tree diameter at breast height (DBH), which means that the DBH-based allometric equations cannot be used directly to estimate aboveground biomass (AGB). The regression methods based on field allometric-based AGB measurements and ALS-derived tree volume and height matrices are usually used to generate AGB from ALS data instead of DBH. Therefore, the aboveground biomass estimations from ALS data are highly reliant on the field-measured AGB. However, by comparing AGB estimates with Jenkins allometric equations, we found that the commonly used regional (FIA) allometric equations might favor species with more published allometric equations, and consequently influence the ALS-based AGB estimation (Zhao et al. 2012). This is the reason we conclude that the selection of allometric equations can influence the capacity of lidar data to estimate biomass significantly, and a careful selection of the equations is necessary.

b. Response: We agree that there is no such term as biomass volume. We have revised the text.

References

4. pg 6, 2. Wildlife: I suggest that you qualify your first Wildlife bullet. Two points. First, ALS data can only be used to map potential habitat, not actual habitat. We can’t measure critters, we can only identify/map areas that might make a particular critter happy if it should choose to show up in a given area. Second, we can only map potential habitat if we can define a particular set of habitat characteristics that can be measured or estimated by the ALS, e.g., particular height, density, overstory/understory, biomass criteria.

Response: We have revised that paragraph as suggested.

5. pg 6, 4. Forest Management: Actually, standard lidar products do currently meet the requirements of at least some forest managers, just not, in general, public sector foresters here in the US. This might change soon if USFS managers adopt laser-assisted ground inventory procedures to inventory undersampled areas in Alaska. Scandinavian companies routinely map/inventory forests with ALS, producing stand-level volume maps for sale to private landholders, and their public sectors are actively transferring that technical know-how to selected countries in Africa, SE Asia, and South America under the auspices of U.N. REDD+ and carbon programs. Your point is correct as far as CONUS goes; just be aware that some European countries are way ahead of us when it comes to operationally using ALS data in conjunction with ancillary (e.g., optical) data.

Response: Agreed. This is an important point. We have revised the text to say this.

6. pg 9, 2nd to final paragraph: As previously discussed, I’m not sure that I agree with your statement that a waveform lidar can provide a better description of forest structure. And as noted above, small-footprint waveform lidars are available and it’s my understanding that some lidars can be set up as either multistop or waveform, depending on the needs of the mission. In other words, the same laser system can serve as a waveform lidar on one mission and a multistop on the next (they cannot sequentially toggle between these two modes from one pulse to the next).
Response: As we responded in previous comment, we agree that a discrete lidar system can produce accurate enough forest parameter estimations in most cases. However, there are studies showing that small footprint full-waveform lidar system outperformed the discrete airborne lidar system in mapping forest aboveground biomass, delineating individual trees, etc. (Cao et al. 2014; Reitberger et al. 2009). Moreover, you are correct that discrete lidar and waveform lidar data can be collected through the same lidar sensors. However, this is true of certain full-waveform lidar sensors. Discrete lidar sensors can only record certain amount of return signals, and cannot digitize the full returned lidar waveform.

References

7. pg 10, bottom: You write that there are no standard ALS metrics that capture forest structure. That’s not really true; you list many of the “standard” variables in Table 1, specifically the height deciles and density deciles. Most of the remaining height and density variables listed in Table 1 are typically very highly correlated with these height and density deciles.
Response: We agree. The “standard” metrics here do not include the lidar derived metrics (e.g., canopy quantile metrics). Instead, our list includes standard forest structure parameter metrics. We have revised the text to clarify this.

8. pg 11, 4th paragraph: Waveform lidar systems are typically sampled at 1 ns, a sampling interval that corresponds to a vertical distance of 15 cm - true. What is not true is the suggestion that this distance depends on maintaining a typical flying height. It has nothing to do with flying height and everything to do with the speed of light, ~30 cm/ns, regardless of the altitude of the aircraft.
Response: Agreed. We have revised the text.

9. pg 12, 2nd paragraph: I think that you meant to say that your Optech GEMINI collected up to 4 discrete returns per pulse. Sometimes you’d receive only a first return, sometimes 2 or 3 returns, and, I suspect only rarely, 4 returns per pulse. Perhaps you could provide a percentage breakdown of 1-, 2-, 3-, and 4-return pulses, though do this only if that information is readily available. Also report the maximum scan angle considered in your analyses, e.g., ±7.5°, ±15°
Response: We agree, and have revised the text.

10. pg 12: In Section 2.2, report the nominal XYZ accuracy of a given Optech pulse. Also report in 2.3.1 the XYZ accuracy of a given GPS reading.
Response: We agree. We have added this information in the section.

11. Considering all error sources, can you provide an estimate of location error, ground versus ALS near the bottom of page 12?
Response: Done.
12. pg 17, Table 1: Suggest that you identify Lorey’s height as a modeled variable with a superscript, e.g., *.
   
   Response: Done.

13. pg 20, 5th line from bottom: change depended to dependent, 4th line from bottom: change expansive to expensive.
   
   Response: Done.

14. Section 4, general comment: You discuss the accuracy of many products in this section and report accuracy in Table 2. In order to assess accuracy, you need some sort of ground reference measure, i.e., a validated product that you trust more than the comparable ALS product being evaluated. In Section 4.3, you compare ground-based tree counts to ALS-derived tree counts. This is good, though I believe that you should report the range of percentage of trees under- or over-counted on each plot so that the reader gets a better feel for site variability; a scatterplot would be more informative. In Section 4.1, you conclude that the accuracy of DTM and DSM products increase with sampling density. This makes intuitive sense, but how do you know this? Did you compare the ALS DTM products to field-measured ground elevations? Did you compare, on a per-tree basis, DSM measures derived from various pulse density products to tree height measurements + ground GPS elevations? My point here is that, in each section and in Table 2, tell the reader what “truth” is and what lidar metrics specifically are compared to that ground reference information. When I look at Table 2, I see many $R^2$ values, but that’s not really a measure of accuracy; it’s a measure of percentage of variability explained by a linear model. On a per-tree basis, you can compare field-measured maximum height to the lidar maximum height for the same tree. But tell me how you’re going to measure mean tree height in the field. Where is the mean height of a tree when you are on the ground looking through an angle-finder? You can’t measure mean tree height in the field, though you certainly can with a laser which takes multiple height measurements on a single tree. So you move to a regression approach as you indicate in Table 2, but what are you regressing? Scatterplots would help greatly here for those comparisons denoted by “Indirect: from regression”. An explicit identification of the ground reference data set would be most helpful for those ALS metrics directly compared to a reference data set. And the reader should not be forced to go to the NCALM report or references 4, 6, 24, 13, or some unnamed report yet to be submitted to find out how you assessed accuracy, or your surrogate for accuracy.

   Response: The accuracy of the DTM, DSM and DEM are provided by the National Center for Airborne Laser Mapping. They were evaluated by hundreds of ground measured GPS transects. All the vegetation related products (including the forest parameters and vegetation maps) were evaluated by the field plot measurements. We have revised the text and tables in the report to clarify this.

15. This table is the backbone or the skeleton of your report. Spend some time and column inches on it so that the reader knows explicitly what comparisons were made. It’s very important that the reader knows, for instance, that some very critical measures of forest fuels cannot be reliably characterized using ALS measures.

   Response: As mentioned in our previous comment, we have revised the text and tables in the report to clarify this.

16. Many of your comments made in Sections 5 and 6 have already been addressed above. As noted previously, I disagree with your statement that standard lidar products do not
operationally meet the requirements of forest managers. They can, and in the future, they will. The only items currently stopping their use in the US is cost, need, and the technological intransigence of state and federal forest managers. Airborne lidars can not only tell you where the wood is and approximately how much is there, but for no additional cost will report topographic challenges of interest to forest engineers. Perhaps in 10 years, public sector forest managers will realize this and begin to come up to technical levels attained by Norwegian foresters 10 years ago.

Response: This is a great point and we agree.

Reviewer #7

- Overall, the spatial team has done a lot of work and definitely have fulfilled the goal of the SNAMP project. The number of peer-reviewed papers (11 published) speaks by itself. However, I wouldn't claim the work as technical breakthroughs. To promote the wide adoption of lidar for forest management, two goals have to be achieved: the high accuracy of the derived veg products compared to the ones from conventional methods, and the capability of applying the lidar-based methods to larger area. Unfortunately, the two goals are rarely met at the same time in most of the current methods. Most methods can get good accuracy at local scale with lidar, but they often require a lot of inputs from the user and fine tuning. When the methods are applied over larger scales, they could break down simply because of the computation demand and the intensive inputs from the users to achieve reasonable accuracy (e.g., how long does it take to apply the point cloud-based method to map trees for the whole study area instead of just those in the field plots? how accurate is it to apply the OBIA method to detect down logs over other areas?). I have no intent to play down the works the spatial team has done, but simply mention a direction in which remote sensing scientists all need to make big breakthroughs.

- Responses:
  1. We have changed the wording “technical breakthroughs” to “technical advances”. We meant by the wording to distinguish between those advances that are ecological, and those that are technical.
  2. Scale issue. We do agree that with the increase in area, the computational burden to map the vegetation unit, detect forest treatment extents, and estimate forest parameters will increase significantly. We have used parallel processing and other computer techniques to increase the computational efficiency. Moreover, we have developed our own software which can process large amounts of lidar data. In addition, the vegetation mapping procedure developed in the SNAMP can be used to update and map vegetation units in large areas quickly. We have tested this method in an area over 5 000 km² in the Plumas and Lassen National Forests of the Sierra Nevada. The results show that the algorithm can produce reliable and consistent vegetation maps efficiently.
  3. Specific questions:
     a. How long does it take to apply the point cloud-based method to map trees for the whole study area instead of just those in the field plots?
        i. We have made the individual tree product for the SNAMP study areas. We have developed efficient software that can process a large amount of lidar data simultaneously. For the SNAMP two
study sites, it only took around one hour to process the lidar data and obtain the individual tree information.

b. how accurate is it to apply the OBIA method to detect down logs over other areas?

i. Unfortunately, we do not have multi-temporal lidar data to do the test for other areas. However, we believe that it can be used to detect the forest treatment extent in other areas as well considering its insensitivity to any specific parameters.

On-line Comments

Please make figure 4 bigger so we can see details.

- Response: We have modified the figure in the report.

Figures 6 & 7 -- need a scale for colors.

- Response: We have added the color scale in the figure.

3.7 & 4.7 – How do results of investigating point density to predict forest metrics at the plot scale translate to stand or landscape scale metrics?

- Response: The methods to reduce the lidar point density and generate the vegetation products are based on the landscape-scale airborne lidar data. The influence of lidar point densities on the accuracy of various calculated vegetation parameters was evaluated by comparing with plot measurements in Jakubowski et al. 2013. We didn’t explicitly evaluate the impact of point density on stand scale or landscape scale metrics, but this is a good idea. We believe that the results obtained should be also applicable to landscape scale.


5.1 – Are tools used (Matlab, etc.) available for other users? What is “multi-path effect”? Combination of “high resolution multi-spectral … imagery” with what?

- Response(s): Yes, the Matlab code for the developed individual tree segmentation algorithm is free to other users.

- The multi-path effect of lidar data is that the emitted laser pulse can be reflected and scattered by multiple objects before being received by the lidar sensor. This effect can significantly reduce the received intensity of the signal, meaning that it does not reflect the real reflectance characteristics of the object.

- The text should read “the combination of high resolution multi-spectral aerial/satellite imagery and lidar data”. We have revised the text.

5.2 – Are there results to support this paragraph? Seems results are not here -- may be more appropriate for wildlife appendices.

- Response: We agree. We have added the related references from the SNAMP project to support this.

5.3 – I’m not sure that managers would need this level of detail about fuel treatments to justify the cost.

- Response: The fine resolution forest structure estimations from lidar can help forest managers to understand the forest heterogeneity and forest change, and may be not necessary for the forest managers to justify the cost. To help the forest managers to justify the cost of lidar data acquisition based on the mission objective and coverage, we
conducted a study on how different lidar pulse densities influence the accuracy of the obtained vegetation parameters (Jakubowski et al. 2013).


5.4 & 5.5 – Link to results is weak. Will adequacy of use for fire behavior models be discussed in the integration report?

- **Response:** The pre-treatment vegetation maps and forest fuel treatment extents developed by the spatial team were used in the fire behavior modeling. We investigated the use of lidar in fire behavior modeling in Jakubowski et al. 2013.


Chapter 5, Recommendation #2. Integrate what across firesheds? Can you suggest what we give up without optical or ground data?

- **Response:** The vegetation maps and detected forest fuel treatment extents were used across the firesheds by all Science Teams. The vegetation maps were produced through the combination of lidar data and aerial imagery, and vegetation type data derived from field data; the forest fuel treatment extents were directly detected from the lidar data and further validated from the treatment areas from Forest Service data. Some specific lidar products were used by specific teams. For example, the LAI product was used by the water team; the individual tree information was used by the owl and fisher team, etc. The ground data provided the ground truth measurements for vegetation species and structure parameters. Although airborne lidar can be used to generate accurate forest height-related (e.g., tree height and canopy quantile metrics) and gap-related measurements (canopy cover and LAI), the individual tree species and certain vegetation parameters, e.g., DBH, height to live crown base, and aboveground biomass, cannot be directly obtained from lidar data. Classification/regression methods based ground measurements and lidar-derived height measurements are the commonly used method to generate these vegetation products. Moreover, the optical imagery can provide canopy surface reflectance measurements, which cannot be obtained from lidar data directly. This information can help to different tree species in the vegetation mapping process.
SNAMP Owl Science Team
Response to peer-reviewed, public, and agency comments

Dr. M. Zach Peery
Dr. R. J. Gutiérrez
Dr. Douglas J. Tempel

August 27, 2015

We appreciate the excellent comments provided by two anonymous peer reviewers, a non-profit organization, and the U.S. Forest Service pertaining to the California spotted owl component of the Sierra Nevada Adaptive Management Project (SNAMP). We address in this cover letter what we believe to be the most important comments.

Reviewer #1 (“Peer Review 1”) made a number of comments related to the statistical analyses that we conducted as part of our retrospective analysis, which we address sequentially as follows:

1) Reviewer #1 noted that the standard error for the beta parameter coefficient for the effect of fire on territory colonization was unestimable. While this does make it difficult to draw definitive conclusions regarding the effect of fire on territory colonization, we observed that the two territories where >90% of the territory burned at high severity during the 2001 Star Fire have never been recolonized, which suggests a threshold of high-severity fire within a territory that can make the territory unsuitable for spotted owls. This view is reinforced by our survey results from the 2015 field season at territories affected by the 2014 King Fire. We found owls in 2015 at only one of the nine territories occupied by owls in 2014 but burned at >50% high severity by the King Fire.

2) Reviewer #1 noted that if all of our territories were initially occupied, then our estimates of occupancy would be biased high at the beginning of our study period and could affect the extinction and colonization rates in the early years of our study. As we noted in the Owl Team Appendix, we identified owl territories as sites where reproduction occurred at least once during our study period (1993-2012). The majority of owl territories in our analysis (40 of 74) were located on our Eldorado Density Study Area (EDSA), and all of these territories within the EDSA had been occupied at least once by 1997. We surveyed
the EDSA from 1986-1992 but did not achieve sufficient sampling effort across the entire study area until 1993 (see Tempel and Gutiérrez 2013). Therefore, we only used data from 1993 onwards for our analysis to avoid the problem of biased estimates of occupancy, extinction, and colonization due to inadequate survey effort in the early years of our study. We further note that two of the EDSA territories were not occupied in 1993.


3) We did not include highly correlated \((r > 0.60)\) habitat covariates in our analysis, but reviewer #1 suggested that we could have included highly correlated habitat covariates. While this may be technically true, we do not believe it is a sound analytical practice to include highly correlated covariates in the same model because this can result in spurious effect sizes for the beta parameter coefficients. When two covariates were highly correlated, we opted to include the covariate that we believed to be most biologically relevant. For example, the amount of shrub/sapling vegetation was highly correlated \((r = 0.86)\) with the amount of edge between shrub/sapling vegetation and forest dominated by trees \(\geq 30.5\) cm (12 in) dbh. We opted to only include the amount of edge in our analysis because previous research suggested that owls may forage along such edges.

4) We used AIC, rather than \(AIC_c\), to rank our competing models because there is no agreement on how to calculate “effective sample size” in occupancy models, and in fact, “effective sample size” may vary for different model parameters (e.g., occupancy and detection probabilities; Dr. James Nichols, personal communication).

5) Reviewer #1 suggested that we run survival models where survival was constant or varied annually. As noted in the Methods section of the retrospective analysis in Appendix C of the SNAMP final report (owl chapter), we included a null constant model in stage 1 for all of our demographic rates, but we failed to include the constant model in our model list (Table 2). We corrected this omission. We did not include the constant model in our final list of top-ranked models (Table 3) because it was not the top-ranked model in stage 1 of our survival analysis. In fact, it was 7.16 AIC units behind the top-
ranked model in stage 1, and we now note this in the Results section. For annual survival, we compared the models \([\phi(age+sex), p(age+sex+year)]\) and \([\phi(age+sex+year), p(age+sex+year)]\) in Program MARK, and found that the model containing a year covariate for survival was 5.34 AIC units behind the model without a year covariate. This reviewer also suggested that we include the results from each modeling stage in Table 3 for each demographic parameter, so the reader can see how much improvement was made during each step. We omitted these results from the SNAMP report to conserve space, but we refer the reader to the appendices of the published version of the retrospective analysis (Tempel et al. 2014), which does contain the results from each modeling stage.


6) Reviewer #1 commented that our reported survival estimates (0.73, 0.66, 0.63, and 0.56 for adult males, adult females, subadult males, and subadult females, respectively) seemed low. The reviewer is correct, and we have added the following sentence to the Results section (survival): “These values were lower than previous estimates of annual survival (cf. Tempel and Gutiérrez 2013) because we removed portions of the capture history for 14 individuals that switched territories during our study but did not reappear on the new territory until a number of years had elapsed (see Methods—Statistical modeling), and thus we lost information on their survival during the intervening period.”

7) We assessed the goodness-of-fit for the survival, reproduction, and occupancy analyses in ways suitable to each analytical method. For the survival analysis, no methods exist for estimating overdispersion (\(\hat{\phi}\)) in Cormack-Jolly-Seber models containing individual covariates (Dr. Jeff Laake, personal communication), but we estimated overdispersion in Program MARK using a fully time-dependent global model and found no evidence for overdispersion (\(\hat{\phi} = 0.998\)). For the reproductive analysis, we used methods identical to those of Blakesley et al. (2010), who noted that McDonald and White (2010) reported
that analysis of variance (ANOVA) procedures based on a normal distribution performed well for small count data. We further note that our use of different variance-covariance structures in the ANOVA allowed us to account for heterogeneity and serial correlation in the model residuals. For the occupancy modeling, we assessed two critical model assumptions: 1) occupancy status at each territory did not change during the survey season (i.e., closure); and 2) detections at each territory were independent (MacKenzie et al. 2006). Because nearly all of the owls on our study area were marked, we could determine when individuals moved among territories during the survey season. Such movements only occurred 10 times in 20 years, and we only considered one of the territories to be occupied in these situations (i.e., where the individual was most frequently detected). In addition, we excluded nocturnal detections > 400 m from a territorial core area (see Methods—Spotted owl surveys) to help ensure independence of detections at territories. We discuss all of these points in the Methods section of the owl appendix (retrospective analysis).


Reviewer #6 (“Peer Review 6”) made no critical comments on our retrospective analysis and agreed with our major conclusions. This reviewer also agreed with our major conclusions for the prospective analysis, but did have some minor suggestions. We followed this reviewer’s suggestion to estimate an ad hoc $R^2$ for the logistic regression of owl nesting habitat (equation 4). Using the $R^2$ formula suggested by the reviewer, we found that our regression based on
canopy cover and large tree density had an estimated $R^2 = 0.32$. The logistic equation for survival (equation 5) was based on the mark-recapture modeling that we conducted for the retrospective analysis, where we found no evidence for overdispersion in the data (see above). Finally, we note that we did not need to ‘assign’ a female age for the stage-based, matrix model; the value of lambda was simply calculated as the dominant eigenvalue of the matrix.

The most substantive and lengthy public comments were submitted as a 12-page document by the John Muir Project and the Center for Biological Diversity (“Comment 17”). We respond to their major comments as follows:

1) They request that we add language stating that even if fire does occur, there may be no long-term benefits from mechanical treatments. We believe that we have stated this in the Discussion section of the prospective analysis in Appendix C of the SNAMP final report (owl chapter), where we presented a lengthy paragraph on several caveats pertaining to our research. In this paragraph, we stated that two fires which burned on our study area (2001 Star Fire, 2014 King Fire) burned much differently than our simulated fire, in that large, contiguous areas burned at high severity in the actual fires. We conclude that: “Indeed, our past experience suggests that existing fire models are generally incapable of replicating the burn patterns seen in the most extreme real fires. Thus, improved fire models are needed to more reliably assess how fuels treatments modify fire behavior and effects on forest structure especially under extreme conditions.” We also refer the reader to the responses to comments provided by the FFEH Team, where they address the potential benefits of fuel-reduction treatments under “escaped wildfire conditions.”

2) They request that we explain that mechanical treatments could have significant negative impacts to owls, while mixed-severity fire could benefit owls. We state several times in our report that fuel treatments can have long-term negative effects on owls if fire does not occur (e.g., Executive Summary at beginning of Appendix C, final paragraph of Discussion for prospective analysis). Furthermore, we included a lengthy paragraph in the Discussion section (prospective analysis) on the potential benefits of fire, which included the following: “However, the effects of wildfire on spotted owls are undoubtedly complex and owls may benefit from the presence of a mosaic of habitat
types promoted by mixed-severity fire, and particularly from shrub patches and early-seral forests that harbor diverse prey assemblages (Roberts et al., 2015). For example, Bond et al. (2009) found that spotted owls in the southern Sierra Nevada selectively foraged in burned areas, even those that burned at high severity. We further note that not all previous studies of spotted owls have found reduced occupancy rates in burned areas relative to unburned areas (Roberts et al., 2011; Lee et al., 2012). Therefore, to the extent that low- or moderate-severity fire may benefit owls, the modeled declines in territory fitness and occupancy in our fire scenarios might be overestimated, and by extension the long-term (30-year) benefits of fuels reduction treatments overly optimistic.”

3) They commented that our Introduction for the retrospective analysis suggested that modern fires are not ecologically valuable. We concur that modern fires may indeed be beneficial, but our statement concerned fires that burn large areas at high severity. We have modified the sentences in question to read: “For millennia, low- to moderate-severity wildfires occurred at frequent (often less than 20-year) intervals in many western forests, naturally removed fuels such as woody debris, shrubs, and small trees, and shaped the ecology of these forests (Agee 1993, Noss et al. 2006). However, decades of wildfire suppression have disrupted historic fire regimes, increased the amount of surface and ladder fuels, and have led to uncharacteristic proportions and patch sizes of stand-replacing fire that now threaten ecological and human communities (Mallek et al. 2013; Stephens et al. 2013, Stephens et al. 2014; Stephens et al. 2015).” We further acknowledge that large areas of high-severity fire may indeed benefit species such as the black-backed woodpecker, but we believe that a reasonable ecological basis exists for inferring that simplification or elimination of large areas of high-canopy-cover forest by high-severity fire could adversely affect spotted owl populations.

4) Many of their statements share the common theme that fire, including high-severity fire, is beneficial to spotted owls. As we noted above, we believe that fire can indeed benefit (or at least have a neutral effect) on spotted owls, including fires that have high-severity effects interspersed throughout the burned area. However, our concern is that fires that burn large, contiguous areas at high severity (such as the Star Fire and King Fire) can negatively affect spotted owls. As we noted in our response to Reviewer #1, the two territories where >90% of the territory burned at high severity during the 2001 Star Fire
have never been recolonized, and we found owls in 2015 at only one of the nine territories occupied by owls in 2014 but burned at >50% high severity by the King Fire.

5) We acknowledge that the effects of the Star Fire on territory colonization in our retrospective analysis were confounded with post-fire salvage logging. However, we maintain that the negative effect on colonization was primarily due to the habitat loss that occurred as a result of the fire itself. Thus, we added the following sentence to the Discussion section of the retrospective analysis (seventh paragraph): “Post-fire salvage logging occurred within two years of the Star Fire, and its effects on territory colonization were confounded with the effects of the fire itself. However, we believe that the negative effect of the fire on colonization was primarily due to habitat loss that resulted directly from the fire.”

6) They commented that in our prospective analysis we modeled the effects of simulated fire on spotted owl nesting habitat, not the combination of nesting and foraging habitat. This is a good point, but we note that changes in owl habitat under the four scenarios were very similar to changes in territory fitness and occupancy (Figures 11 and 12 in Appendix C), and that high-canopy-cover (≥70%) forest was by far the best correlate of territory fitness and occupancy in our retrospective analysis.

7) We refer the reader to the responses to comments provided by the FFEH Team, where they address the contention that high-severity (i.e., stand-replacing) fire has not increased in the Sierra Nevada over the past several decades.

Finally, an anonymous public reviewer (“Comment 19”) noted that avoiding management activities in PACs has not prevented a 50% decline on the Eldorado Study Area. We did not intend, however, that our recommendation for the continued use of PACs be construed as the only possible management strategy to conserve spotted owls in the Sierra Nevada. We view the use of PACs to be a necessary component of owl management, but not a sufficient component in and of itself. This reviewer also noted: “The authors mistakenly assume there would be a long-term fuels treatment benefit of 30 years for spotted owls, but Collins et al. (2011), a study on the effectiveness of the fuels treatments in the LCSA, found that fuels treatments were only effective for about 20 years…” We did not mistakenly assume a fuels-treatment benefit for spotted owls after 30 years; we inferred it from the results of our prospective analysis which clearly show
slightly higher rates of territory fitness and occupancy on the treated landscape 30 years after a simulated fire (see Figure 12 in Appendix C). The reviewer is correct in pointing out that these results differ somewhat from Collins et al. (2011), but our prospective analysis was based on actual, post-treatment vegetation conditions. Because the analyses and simulations in Collins et al. (2011) occurred before the treatments were actually implemented at Last Chance, they necessarily simulated the effects of the treatments on vegetation before conducting their fire and forest-growth simulations.


We appreciate the time and effort expended by all of the individuals who submitted comments on the owl section of the final SNAMP report, and we believe that our responses and revisions have addressed these comments.

Sincerely,

Dr. M. Zach Peery
Dr. R. J. Gutiérrez
Dr. Douglas J. Tempel
SNAMP Fisher Team

Response to peer-review, public, and agency comments

Dr. Craig Thompson
Dr. Rick Sweitzer
Dr. Kathryn Purcell
Dr. Reginald Barrett

13 August, 2105

We appreciate the comments provided by two anonymous peer reviewers, several non-profit organizations, and the USDA Forest Service. Several comments were ubiquitous among multiple respondents. In particular, these focused on reducing the length and detail of the executive summary and introduction, and being consistent with terminology both within the Appendix and between the Appendix and other chapters. We attempted to address these concerns but will not discuss them at length here. Below, we provide detailed responses to what we considered the most substantive comments. In cases where we disagreed with the suggestions given, we attempted to clearly state our reasons.

Peer Review #4:
Reviewer #4 provided extensive comments and suggestions on language clarification, explanation of methods, and terminology, for which we are grateful. Additional comments included:

Comment 1: “I had expected to see specific demographic and land use measures for fishers before and after SPLAT implementation, but those data were not presented. I am hopeful that further research will allow for those comparisons to be made. I think they could be important for fisher conservation in California.”

Response to comment 1: Given the delays in treatment that occurred and the protracted timeline over which it is necessary to evaluate management impacts on a species such as fisher, insufficient post-treatment data were available to support the original BACI design. However post-treatment monitoring is ongoing, extended through 2016, in hopes of supporting these analyses.

Comment 2: “I was surprised to see how large the home range sizes were for the fishers in your study area. Not having the home range sizes for other CA fisher populations committed to memory, I wondered if they were comparable to those in your study area or if they were considerably smaller. I ask because I wondered if there was some ecological phenomenon that
exists at the northern extent of the SSN fisher population (your study area) that results in fishers using larger home ranges at this margin of their range. Generally larger home ranges imply lower habitat quality for the species in question."

**Response to comment 2:** The home ranges reported here are significantly larger than those reported elsewhere in California. The most likely cause is the extensive use of aerial telemetry. Aerial telemetry generates a significantly larger sample size than ground telemetry, the method on which most prior home range estimates in California have been based. While ground-based telemetry can provide precise locations, animals are often lost for extended periods of time and topography can make it difficult to obtain locations in specific parts of the study area. Aerial telemetry does not suffer these handicaps, animals are relocated reliably and the increased ranges reported here most likely reflect a greater degree of monitoring “excursions”, when animals break their typical movement pattern and explore less frequently used areas. It is debatable whether these excursions represent part of a home range, or whether they represent the 5% of locations that is traditionally excluded in home range analyses as outliers. Regardless of this, a collective analysis between SNAMP and KRFP data is ongoing to develop a “correction factor” for regional conservation planning.

**Peer Review #8:**

Reviewer #8 provided extensive comments on clarity of terminology and interpretations. He also provided a detailed review of the use of location data and the application to dispersal and home range analyses which helped improve the presentation greatly. Many of the suggested changes have been made in the text. Below we present what we felt were the most substantive, generalized comments and our response.

**Comment 1:** “For your Leslie matrix, you used single values for adult reproduction and adult survival. Thus, $F_4 = F_5 = F_6 = F_7 = F_8$ and $P_4 = P_5 = P_6 = P_7$. You should use just $F_4$ and $P_4$ in your matrix on page 33. In building your matrix, you have assumed that no fishers live beyond page 8, which is not true. If you replace the 0 in the bottom right corner of the matrix with $P_4$, your matrix will allow survival beyond age 8. Given that you use constant adult reproduction and survival, you can actually collapse the matrix to a 5x5 matrix, assuming you are willing to let model fishers live beyond age 8.

Given that $F_1 = 0$, why not simply put 0 in the matrix? As far as I can tell, $F_2 = 0$, too, for your matrix. Although you state that the matrix was built to estimate the population at 1 month following birth (approximately 1 May), your estimates of litter size appear more accurate for later ages, probably 2-3 months old. And your estimates of kit survival starts at age 6 months. In the end, I am confused as to whether your matrix estimates population size just before or just after reproduction. Do your fishers die and then reproduce or do they reproduce and then die? Leslie matrices can work either way.

You should state that your estimates of litter size are biased low because cameras do not always show all kits in a litter. Opposing that bias, your estimates of kit survival are biased high,
because you have few data on survival of kits to weaning and no data on survival of kits from weaning until trapping in October.

Also related to the biased estimates of reproduction and kit survival, the elasticity analyses that I did for the population model used by Lewis et al. (2012, PloS One) and Powell et al. (2012, Martes 2009 book), showed that the model was most sensitive (via elasticity calculations) to estimates of litter size and kit survival. I do not know why you found your model not to be most sensitive to those variables, contrasting with our results. Sensitivity analyses sometimes yield results that contrast with elasticity analyses (a substantial literature on the differences exists, dated to 10-15 years ago). I encourage you to do an elasticity analysis if you did not.”

**Response to comment 1:** We appreciate the suggestions on how to simplify the presentation of the Leslie matrix analysis. We limited the survival of females in the matrix to 8 years old because that was the upper limit of commonly observed lifespan. While it is possible that individuals may live beyond that, in fact we did observe several females living beyond 10 years of age, it was uncommon and those individuals generally showed signs of deterioration such as severely worn or missing teeth and emaciation. We chose to limit the matrix in this manner to better reflect the observed patterns of reproduction and senescence. Finally, a more detailed analysis of demographic parameters, including sensitivity and elasticity analyses, is in progress but is beyond the scope of this report.

We also attempted to more clearly describe potential biases in the first year survival and litter size estimates in the text.

**Comment 2:** “Writing about the biases for litter size and kit survival reminds me that your estimates of dispersal distance are biased low. This point is worth making clear right from the start. Your bias is undoubtedly smaller than that for research not so flight-based. When trapping fishers for reintroduction in the northern Sierras, we trapped a male 50 km from where he had been marked as a kit on the Hoopa Reservation.”

**Response to comment 2:** We believe that the data we collected on dispersal distances for fishers are among the most accurate yet recorded, due to the daily aerial telemetry flights and our ability to follow dispersing animals easily. Longer dispersal events undoubtedly occur, but our data suggest they are infrequent at least in this region. There are also likely shorter dispersal events than what we observed. We present the mean and range for two different methods of evaluation. If there is a bias in the data, it likely reflects the fact that we presume to know the path taken. That bias is clearly presented in the text.

**Comment 3:** Page 90, Figure 31 and elsewhere – You must justify defining a core as the 60% contour. Why 60% and not 55, or 42, or 73.1415927? Really? Using 60% is arbitrary and not based on the behavior of the fishers, as far as I can tell. If you zeroed in on 60% after doing some undescribed analyses of your fishers’ utilization distributions, then that could be OK but you need to explain the analyses.
Response to comment 3: In the footnote for Table 31, we refer the reader to two references regarding our approach to evaluating core use areas. We followed the approach described by Bingham and Noon (1991), which identifies the kernel estimate, for each individual animal, at which locations go from being under-dispersed to over-dispersed. Essentially this is the point at which area of the home range exceeds the expected level of use assuming a uniform distribution of locations. The closest 10% kernel estimate was then selected as the appropriate boundary to define a core use area. In all cases, this was either the 60% or 70% isopleth as noted in Table 31.

Agency Comment #15:
Comments from the USFS Region 5 focused on shortening the introductory and summary sections, updating references, and being consistent with terminology. One additional comment deserves mention here.

Comment 1: p. 10 – “fishers in this part of California may have expanded in the late 20th century (Tucker et al. 2014)” and “expanded during the 1990s” are misrepresentations of the research – I believe the genetic study identified that it was some time during the 20th century (about 100 years). This error must be corrected multiple times in the appendix.

“The Zielinski et al. (2013) analyses suggest…” The paper did not suggest what follows. You suggest these things based on an extrapolation of the analyses. In fact, the time scale of the monitoring is much shorter than 60 years and the suggestion is inappropriate.

Response to comment 1: This hypothesis, presented by Tucker et al. (2014) in their discussion, was an effort to reconcile genetic data with survey results for the region. It centers on the fact that genetic subdivision in the fisher population between Yosemite and the North Kings River could reflect recent founder events; i.e. fishers moving northward over Shuteye Peak post-1990. It is one of several competing hypotheses regarding the current state of the southern Sierra Nevada fisher population. Throughout the chapter, we attempted to revise the language regarding this hypothesis to better capture the current state of knowledge. We have also attempted to clarify which statements summarize published work and which reflect our own interpretation.

Public Comment #16
Comments from the Center for Biological Diversity and the John Muir Project fell into two categories, interpretation of the impacts of thinning on fisher habitat and our failure to account for the Hanson 2013 and Hanson 2015 publications regarding fisher use of post fire landscapes on the Kern Plateau.

Comment 1: “First, the results described on pages 98-99 do not support such a conclusion. Those results state that the single top model regarding local extinction identified “only” thinning (“hazfuels.5”), and that fisher persistence decreased with increasing thinning. The results (pages
98-99) indicate some effect of fire but state that the “relative importance” of this variable was “low”. Therefore, there is not a sound basis for the conclusion that thinning is justified to mitigate fire effects; in fact, the results contradict this conclusion.”

**Response to comment 1:** It is accurate to say that our analysis indicated that hazardous fuel reduction increased the likelihood of an occupied grid cell becoming unoccupied at some point in a 5-year window. It is also accurate to say that fisher persistence was negatively associated with hazardous fuel reduction activity. However it should be pointed out that the probability of persistence dropped from 0.89 to 0.65 as a grid cell went from 0% to 100% treated. Persistence or occupancy rates of 0.65 are considered a positive indication of population stability in other regions (Zielinski et al 2013). As described in the discussion, these results suggest that fishers are locally flexible, abandoning certain parts of their home range when unsuitable, then returning to use these areas after several years of recovery. Given the potential for limited thinning activities to reduce or mitigate fire effects, as well as further the goal of ultimately reestablishing a mixed-severity fire regime, and the limited negative impacts to fishers, we stand by the conclusions as presented.

**Comment 2:** “Second, we followed the methods described in the SNAMP fisher appendix with regard to fires in the camera-survey study area, and digitized the grids shown in Figure 12 of the SNAMP report. We used GIS to overlay fires on the camera-survey grids, and used Google Earth satellite imagery to evaluate whether post-fire logging had occurred in any of the fires. We found that post-fire logging—often extensive—had been conducted in numerous camera-survey grids within fire areas, e.g., in portions of the 2001 North Fork fire and in the 2004 Nehouse fire (see figures below). Therefore, there is a significant methodological issue in the study, in that it states it is only testing the effects of fire on fisher occupancy, but is actually instead testing effects of post-fire logging in numerous grids. This is important, given that all but one of the grids that are approximately 50-100% within fire perimeters, and that we identified as having been post-fire logged, had no documented fisher occupancy, according to Figure 12 of the SNAMP (2015) fisher appendix.”

**Response to comment 2:** We do not believe that anywhere in the Fisher Appendix does it state that we are “only testing the effects of fire on fisher occupancy”. In fact numerous grids were exposed to various combinations of hazardous fuel reduction, extractive thinning, and fire. Furthermore, we are unclear how post-fire salvage logging could be accurately determined from Google Earth. Instead, we calculated areas on which post-fire salvage logging occurred using the FACTS database, and included that in the “Extractive thinning” composite variable (log.5).

**Comment 3:** “Third, among camera-survey grids mostly/wholly within fire perimeters, a disproportionately large cluster are in a fire (the 1990 Steamboat fire in Yosemite National Park) at the northern extreme of the fisher’s range in the southern Sierra Nevada (generally, north of 37
degrees and 39 minutes latitude). Figure 12 of the SNAMP fisher appendix shows that fishers are almost completely absent in this area, and only one camera-survey grid had a fisher detection—and it is partially in the fire area. None of the unburned grids in this northernmost portion of the study area had any detections. The effect of this was to create a bias in the results toward reporting lower fisher use of fire areas.”

**Response to comment 3:** If we understand the comment correctly, the concern stems from the belief that results from a cluster of 17 grid cells at the northernmost boundary of the study area will unduly bias the result against fisher use of burned areas. A subset of these cells, approximately 8, fall within the boundary of the 1990 Steamboat Fire, and in only one of those cells was a fisher detected. The concern is that fishers are “almost completely absent” from that area for reasons unrelated to the Steamboat fire, and therefore fishers were not detected in 7 of 8 grid cells.

As stated in the Methods: Fisher response to fuel management: Occupancy modelling section, occupancy models were developed using only data from grid cells that were surveyed for multiple seasons. And as stated in the Results: Basic Camera Survey Results section, surveys within Yosemite National Park were only conducted in winter 2009 under supplemental funding from the California Department of Fish and Wildlife. The Yosemite data were not included in the occupancy analysis and therefore could not have biased the results. We appreciate this confusion being pointed out, and have attempted to clarify the language.

**Comment 4:** “Fourth, the SNAMP fisher appendix does not incorporate Hanson (2013) or Hanson (2015), which are peer-reviewed studies (see references below) conducted on fishers and fire based upon field data (as opposed to modeling assumptions). These two research studies regarding fishers and burned forests have found that post-fire landscapes, when left unlogged, can provide important, critical habitat to fishers.”

**Response to comment 4:** The authors are familiar with the Hanson 2013 and Hanson 2015 publications, and chose not to reference them or utilize the information for several reasons.

Methodologically, both analyses are seriously flawed. The author used scat dogs to locate fisher scats in burned and unburned areas. Dogs are an extremely effective tool for this, however scat locations are spatially autocorrelated and the associated clustering leads to pseudoreplication. This problem is particularly acute with small sample sizes and with species such as fishers that use latrines and mark territorial boundaries with scat. Instead, when evaluating scat data without genetic information to identify individuals, it is necessary to identify some independent spatial sampling unit appropriate for the species (e.g. a transect, grid cell, etc.) and then to examine occupancy or abundance within that unit (Thompson et al. 2011, Long et al. 2007, Smith et al. 2005). The author’s choice of a one-tailed chi-square analysis to assess habitat selection further inflates this problem, and the author fails to account for the fact that multiple statistical tests
were conducted by reducing the significance level accordingly. Essentially this means that in Hanson (2013), the author’s conclusions are based on nonsignificant statistical results; all p-values in the manuscript should be multiplied by 2 and then compared to a p-critical of 0.0125. Furthermore, Hanson (2015) relies heavily on the absence of statistical significance to claim that fishers select burned and unburned habitat equally. Given the small sample sizes and inappropriate analytical techniques applied, non-significant results were a forgone conclusion.

The conclusions in Hanson 2013 are further confused by the fact that the author altered the traditional definitions of fire severity classes. Hanson defines low-severity fire as areas with less than 15% basal area mortality (<316 RdNBR), moderate-severity fire as areas with 15% to 50% basal area mortality (316 – 477 RdNBR), and higher-severity fire as areas with greater than 50% basal area mortality (> 477 RdNBR). Management agencies, on the other hand, generally define low-severity fire as less than 25% basal area mortality, moderate as 25% to 75% basal area mortality, and high as greater than 75% mortality. While there is no precise agreed upon threshold of high severity fire, Safford et al. (2008) recommended a 75% basal area mortality threshold, given that the most commonly used thresholds in the literature range from 70% to 80%. Hanson cites Miller et al. 2009 as justification for his fire severity definitions, despite the fact that his chosen values do not match the reference, particularly with respect to the threshold between moderate and high severity. After applying these novel definitions, Hanson further confounds his conclusions by combining the ‘moderate’ and ‘higher’ severity fire categories into a single broad category. By combining these two categories, the study then only differentiates between areas with less than 15% basal area mortality (arguably ‘very’ low severity) and areas with greater than 15% basal area mortality (which combines areas of low (<25% BA mortality), moderate (25-75%), and high (>75% BA mortality) severity fire). Hanson does not indicate relative representation of these three classes in the ‘moderate/higher’ severity category, thereby preventing any possible conclusions from being drawn about fisher use of any given fire-severity class. As a result of these changes, subsequent interpretation is biased toward higher severity fire (Fule et al. 2014). This pattern of altered and combined fire severity classes was continued in Hanson (2015), making those results equally difficult to interpret.

Hanson (2015) continues the same approach of creating false dichotomies and misrepresenting references. He cites a USFS document stating the agency’s intention to ‘implement commercial logging at an “unprecedented scale”’ to advance the goal of saving species such as the Pacific fisher from the effects of higher severity fire’, despite the fact that what the document actually says is that increases in the number of human-caused fires is threatening fire-adapted chaparral with replacement by non-native grasslands, and “Only an environmental restoration program of unprecedented scale can alter the direction of current trends” (http://www.fs.usda.gov/Internet/FSE_DOCUMENTS/stelprdb5351674.pdf). He incorrectly cites Truex and Zielinski (2013) as stating that fishers avoid mechanically treated areas, when what the authors actually wrote was “predicted resting habitat suitability was significantly lower
for mechanical plus fire treatments, but the control did not differ from the fire only or the mechanical only treatment” (emphasis added) and that “fisher foraging habitat … was unaffected by treatments at either site.” Hanson goes on to state that “forest management of fisher habitat, at present, is predicated on the belief that mechanical thinning, though not ideal for Pacific fishers, is better than allowing mixed-severity wildland fire”, ignoring the evolution of management thought over the past 20 years and the reality that current management is based on the goal of combining mechanical thinning with managed wildfire and prescribed burning to restore a mixed-severity fire regime (North et al. 2009).

We do acknowledge one important result from the Hanson 2013 and 2015 efforts; the author located female fisher scats within the boundary of a high severity fire, 10+ years post-fire. This result warrants further investigation to determine if such female fishers are transient or resident, whether they successfully reproduce, or whether they simply venture into historic burns in search of prey. We don’t necessarily doubt that a well-designed study of the use of burned and unburned areas by fishers, with adequate design and sample size, could arrive at the same or similar conclusions. However without such a robust foundation, we are forced to conclude that the interpretations presented in these papers are suspect and should not be used to guide management actions.

Public Comment #18:
Comments from the Sequoia Forest Keepers centered on the idea that the probability of any particular area experiencing wildfire is relatively low and therefore treating areas in the “hope that the treated area will experience fire, so that the models can hopefully work” are not justified. They stated “The SNAMP fisher report does not consider the analysis provided by the 2008 study Fire Probability, Fuel Treatment Effectiveness and Ecological Tradeoffs in Western U.S. Public Forests by Jonathan J. Rhodes and William L. Baker, which indicates that few treatment areas ever encounter fire (2.0% to 7.9%) during their assumed 20 year period of reduced fuels.”

“The SNAMP fisher report projects fire in treatment areas even though the likelihood is low and the impact to the fisher habitat from treatment would be immediate and negative and it would take 37 years for the habitat to recover from the treatment and show positive benefits.”

“In addition, the SNAMP fisher report clearly indicates on page 32, that, “Data from radiocollared animals from the study area indicated that female fishers commonly die by 6-8 years of age.” Contrary to what the report implies, this means that more than four generations of Pacific fisher will not be able to use the treatment areas. While treating the forest may result in “relatively modest impacts on forest structure,” the impact of treatments on Pacific fisher would be extremely significant!”
Response to Public Comment #18

A detailed response to the criticism is outlined in the FFEH: Response to Comments letter regarding shortcomings of the Rhodes and Baker (2008) analysis with respect to management decisions. In response to the statements regarding fisher habitat recovery, we believe the respondents misunderstood the data and conclusions drawn. The SNAMP Integration modeling showed that in the absence of fire, treatments reduced fisher habitat immediately (year 0) by 13%. However in the continued absence of fire, by year 10, there was 5% more fisher habitat available on treated landscapes than on untreated landscapes. And in the presence of fire at year 10, there was 10% more habitat available on the treated landscape than the untreated. Beyond year 10, regardless of the presence of fire, differences in the availability of fisher habitat between the treated and untreated landscapes were negligible (1-4%).

The statement that it would take 37 years for the habitat to recover is incorrect. That number stems from a separate analysis (Thompson et al. 2011), and reflected the conclusion that under the scenarios modelled in that analysis, fuel reduction efforts “provided significant positive benefits [to fisher habitat availability] up to 37 years after simulated fire”. ‘No treatment’ is a management choice that results in the homogenization of a forest landscape, over time, into a dense, closed canopy forest. Disturbances, whether natural or anthropogenic, serve to disrupt this homogenization and maintain a patchwork of seral classes. Fishers require a variety of habitat and seral types to meet their life history requisites, and the Thompson et al. (2011) analysis indicated that in the absence of fire, landscapes could quickly (10-25 years) become more homogenous than that what fishers were observed using. Fuel treatment not only provided significant benefits in the event of fire, they also slowed the forest homogenization and resulted in an additional 30-40 years of functional fisher habitat in the absence of fire.

We would add that in the past 18 months three significant fires have impacted fisher habitat within the SNAMP Fisher study area. The French (2014), Sky (2015), and Willow (2015) fires all consumed fisher denning habitat. Furthermore the Aspen/French fire complex (2013/2014) burned over 30,000 acres of habitat including a critical linkage zone spanning the San Joaquin River. The Sky Fire, threatening the Nelder Grove area (a significant source population for fishers in the region) was partly stopped by fuel treatments recently completed in the Cedar Valley area.

Public Comment #25

“I am uncertain how the integrated recommendation of limiting mastication is justified. The appendix on fisher states that occupancy following mastication was relatively high (0.65). Further, others have found that masticated material decomposes at relatively fast rates (depending on site productivity, cover, etc.). The management implication does not seem to consider that masticated material decomposes within the timeframe of expected fuel treatment
longevity. Other work has demonstrated mastication to be effective at reducing fire severity, especially following the period of decomposition. The suggestion to limit its use does not seem warranted.”

Response to Public Comment #25.

The recommendation to limit mastication is closely associated with the stated goal of increasing fisher foraging activity. The negative relationship between fisher persistence (continued use of an area) and hazardous fuel reduction was clear in our multi-season occupancy analysis. There was also evidence that this was a short-term impact; the integration analysis indicated that at larger temporal scales the effect of fuel treatment on fisher habitat availability was negligible. We recognize that mastication is an effective tool for reducing fire severity, however there is no natural analog to this activity; a masticated landscape is in an unnatural condition for some period of time determined by site productivity, local decomposition rates, weather, etc. The data we collected showed that fisher use of an area declined following mastication. It also showed that they did not abandon these areas entirely. We assume they simply moved around the masticated area until the forest floor recovered to a more natural state. Therefore limiting mastication if possible, dispersing masticated areas, and promoting forest floor recovery are reasonable recommendations if a manager’s objective is to increase fisher activity in an area.

We appreciate the comments provided and the efforts of the commenters to help improve the Fisher Appendix. In particular, the two anonymous reviewers provided excellent feedback, and their help is greatly appreciated.

Dr. Craig Thompson
Dr. Rick Sweitzer
Dr. Kathryn Purcell
Dr. Reginald Barrett


Hanson, C.T. 2013. Habitat use of Pacific fishers in a heterogeneous post-fire and unburned forest landscape on the Kern Plateau, Sierra Nevada, California. The Open Forest Science Journal 6: 24-30

Hanson, C.T. 2105. Use of higher severity fire areas by female Pacific fishers on the Kern Plateau, Sierra Nevada, California, USA. Wildlife Society Bulletin online release. DOI: 10.1002/wsb.560.


SNAMP Water Quantity and Quality Team
RESPONSE TO COMMENTS/REVIEWS

Roger Bales
Martha Conklin
Phil Saksa
Sarah Martin
Ram Ray

December 9, 2015

A. Response to comments from Peer Reviewer #11.

We thank the peer reviewer for their time and contribution to the Water Team Appendix. The site descriptions and study site references have been updated to clarify the location and arrangement of the catchments and firesheds. The Last Chance headwaters are nested within the firesheds, but the Sugar Pine headwaters drain to the Merced River, adjacent to the Fresno River drainage where the firesheds are located. We have added maps that include study basins (headwater and fireshed) and the locations of stream gages used for scaling up.

The limitations of scaling from the headwaters to the firesheds are the elevation ranges, controlling rain or snow precipitation, the travel time of precipitation to stream runoff, and the storage potential of the underlying geology. We address these limitations with comparisons of the hydrologic regime of both headwater and the closest downstream gaged discharge location, and an analysis of individual basin lithology to determine transferability of model parameters. The topography, soils, vegetation type, vegetation cover, and vegetation density are all individual to each basin, thus we use a continuous spatial dataset across the headwaters and firesheds. The spatially discrete topography and vegetation characteristics are developed from the LiDAR data and the soils are from the USDA Natural Resources Conservation Service. We have expanded discussion of the limitations of scaling in the results section.

The meteorological data used to drive the model are from the stations installed within the study sites (added to the site map), except for precipitation, which was pulled from the outside stations that had more robust instrumentation to handle both rain and snow events. The external precipitation data were used for both water balance characterization and modeling. We clarified this in the methods section.

We agree that the LiDAR-based datasets provide invaluable information on forest structure and changes. The collection of waveform LiDAR, which would have allowed for an assessment of understory vegetation, would have also been useful but was not part of the original study design. Although the relationship between forest structure and understory appears low, the trend of greater understory cover with lower overstory density, is conceptually accurate. The high number of forest plots used to develop these equations provide additional confidence in the predictions, and the correlation is not unexpectedly low for relationships between two vegetation types. We do encourage the use of waveform LiDAR in future studies, however, for better assessment of understory vegetation.
The hydro-ecological model does underestimate peaks, calibrating snow accumulation and melt in these elevations that transition from rain- to snow-dominated precipitation, proved to be very challenging. We agree that for this region with highly variable topography, temperatures, and precipitation, a more sophisticated snow process model may be necessary. We also suggest locating measurements within modeled basins where possible, and adding distributed temperature measurements to the snow and soil moisture observations. We have also added some discussion of the benefit of constraining the model with multiple parameters, while reducing the fit compared to calibrating on just discharge, it captures basin behavior.

B. Response to comments from Michael Anderson, California Department of Water Resources [Survey Response 836287]

We appreciate you taking the time to review the water team report and provide feedback. This appendix was written with the intention of providing the scientific background, and basis, for the management-focused results presented in chapters 3 and 4 of the full SNAMP report. Table 13, and figures 15-17 have the specific results of all the possible scenarios, and the discussion of management implications has been substantially expanded in the text.

C. Response to comments on the Water Quantity section from Leslie Reid, Forest Service Pacific Southwest Research Station

COMMENT 1. Even if the study is outlined outside of the appendix, it would be useful to provide a brief outline here of the overall strategy for the water-quantity study so the reader starts off oriented with respect to how the pieces described in the appendix fit together.

RESPONSE: Additional introductory text has been added to provide a more comprehensive overview of the study approach for water quantity.

COMMENT 2. It would be quite useful to provide a clear description of what parameters needed to be estimated or defined by calibration, the values used, and the uncertainty associated with each. This might be most easily presented by an expanded version of Table 12.

RESPONSE: Calibration parameter descriptions are provided in the text (as described in Methods). The range of possible parameters has been added to Table 12, and uncertainties have been addressed by using the multiple parameter sets calibrated for each basin. The 95% confidence intervals have been added to the model results in Table 13 to help interpret the significance of responses.

COMMENT 3. I’m confused by what weather conditions were actually modeled over the 30-yr period—were these the “average” condition? If so, is that meaningful, given the variation in responses over the 4 measurement years? Is the response to “average” conditions equivalent to the average of responses to the distribution of weather conditions likely?
RESPONSE: Each 10-year interval over the 30-year time period represents a point-in-time snapshot of the vegetation conditions. For each vegetation condition, the climate conditions modeled were the observed conditions over the four-year observation period (as stated in Methods), which included average (2010), wet (2011), and dry (2012-13) years. The average conditions referred to in the model are the mean of the observation years.

COMMENT 4. It seems odd that all relations in Figure 17 (or Figure 16) would be turn out to be significantly different at the 0.05 level; this should be checked. In particular, relations for 2012 and 2013 in Figure 17 appear to be nearly colinear while showing significant variance about each. In addition, the units need to be identified.

RESPONSE: The result of significant differences between the linear equations is correct; however, the figure (now Figure 13) was incorrect and has been updated. The units are log-transformed daily discharge in centimeters, and has been added to the figure caption.

COMMENT 5. The calibrated discharge predictions in Figure 18 do not look particularly close to the measured discharges. What is the percentage error for calculated annual water yields for the individual years used in calibration? The depiction of standard error bands for SWE and soil storage provide a very useful depiction of the uncertainties in those values—could something similar be done for discharge?

RESPONSE: The figure you are referring to is now Figure 11. The error of the predicted discharge was restricted to 20% of the annual discharge over the calibration period (Methods). The percentage errors on an annual basis were 4.9%, 18.9%, and 19.7% in Last Chance and 2.9%, 19.8%, and 18.1% in Sugar Pine for 2010, 2011, and 2012, respectively. We were able to calculate standard error bands for the SWE and soil storage because of the distributed measurements, but discharge is measure in a single location. Calculated error bands would be an assessment of discharge estimation or measurement error, which does exist, but would not be the same analysis as the spatial variability of SWE or soil storage.

COMMENT 6. Given that there are so many parameters that had to be established by calibration or other means, it would be extremely useful—in fact, possibly critical—to carry out a sensitivity analysis to identify the level of uncertainty associated with the reported results.

RESPONSE: The Monte Carlo-style calibrations, testing 5000 possible parameter sets and resulting in multiple calibrated parameter sets, addresses the issues of parameter sensitivity, equifinality, and uncertainty. The range of parameters are listed in Table 12 and the range of potential responses to changes in vegetation structure using a 95% confidence intervals have been added to the results in Table 13.

COMMENT 7. Calculated discharges appear to be much less accurate for the validation period at KREW than for the calibration period, suggesting that the model may be significantly overfitted. A statistical analysis (maybe cross validation—check with a statistician on this) should be carried to determine whether this is indeed the case; this would also provide information useful for evaluating the uncertainty associated with reported results.
RESPONSE: The difference between calibration and validation accuracies for Providence Creek are with the expected range of decreased statistical performance (e.g., Morán-Tejeda et al., 2014).

COMMENT 8. The pattern of variance in Figure 5 suggests that a linear fit is inappropriate.

RESPONSE: The objective of Figure 5 (now Figure 6) was not to develop a predictive relationship between the headwaters and downstream discharge sites, but rather to compare the daily discharges between the sites. The linear relationship is specifically used to highlight the similarities or differences of daily discharge response to the entire range of seasonal precipitation conditions.

COMMENT 9. What is meant by treatment and control in Figures 16 & 17? Wasn’t treatment carried out after the major runoff period in 2012? It would be useful to distinguish data from before and after treatment.

RESPONSE: The figures you are referring to are now combined into Figure 14. Treatment and control refer to the different study catchments in the Before-After-Control-Impact (BACI) study design, not the pre- and post-treatment period. The upper portion of Last Chance treatment was thinned in 2011, with the remainder of the headwater thinning completed in 2012. The only year of post-treatment in Big Sandy was 2013, figure labels and caption are updated to clarify.

COMMENT 10. It is not clear whether the 2013 (post-treatment?) data were used in calibration.

RESPONSE: Water year 2013 data were not used in calibration, only 2010-2012 (Methods), which does include some upper catchment thinning in Bear Trap from 2012.

COMMENT 11. If the point of the correlations between headwater discharges and main-stem discharges was to establish a basis for estimating the downstream change associated with a change in runoff in the headwater drainages (and this is not clear in the text), then there needs to be very careful consideration of the difference between association and causation. The severity of my sunburn on a given day may be closely correlated with how much sunscreen you apply that day, but if you forget to put on sunscreen one day, that won’t affect my sunburn. This may indeed be the most effective means available for scaling up results, but the potential pitfalls (and associated uncertainties) at least need to be discussed.

RESPONSE: The correlations between headwater and downstream discharges were one of the approaches we used to characterize hydrologic catchment similarity (the other was geology). Although we determined the calibration parameters used in the basins could be transferred, the soils, vegetation structure, and vegetation disturbances due to treatment and fire are individual to each headwater and fireshed catchment. We do recognize, however, there are uncertainties associated with this type of approach, and have added some additional discussion regarding ungauged catchment modeling.
COMMENT 12. Figure 15 is difficult to follow because solid shapes are used rather than lines. At the point that the upper margin of shapes cross, the graph becomes uninterpretable.

RESPONSE: Correct, it is difficult to distinguish between cumulative stream runoff when the values are close. For quantitative assessment of annual values, refer to Table 11. This figure has been combined with other observations for ease of interpretation (Figure 11).

COMMENT 13. The units need to be identified in Figure 7.

RESPONSE: The units are unit (area-normalized) discharge in centimeters. This has been added to all the appropriate captions.

COMMENT 14. There appear to be some critical typos in Table 11.

RESPONSE: Table 11 has been reviewed and errors fixed.

COMMENT 15. Why does Sugar Pine control ET decrease with increasing LAI, while Last Chance control ET does the opposite (p.42)?

RESPONSE: Updated results are posted in Table 13, Sugar Pine ET was not sensitive to changes in LAI, because of the lower precipitation. LAI affects other processes, however, such as snowpack persistence. The higher LAI can result in earlier snowpack melt, and reduced soil storage in the spring season of highest potential transpiration, reducing actual transpiration. These effects are extremely subtle however, and transpiration is reduced incrementally. Adding the 95% confidence interval to the table shows the uncertainty in transpiration is greater than any changes in Sugar Pine transpiration.

COMMENT 16. It would be very useful to provide some discussion of the groundwater loss component. What is it? How is it estimated? What are the implications of the calculated changes?

RESPONSE: The groundwater loss comes from two sources, the calibrated groundwater parameters (gw1,gw2), the control the fraction of water infiltrated to the soil being routes to groundwater storage, and the fraction of groundwater storage released to stream discharge. The second source is groundwater infiltration at the bottom of the modeled soil column, which infiltrates at a rate proportional to soil saturation (higher saturation = faster groundwater infiltration). Longer periods of soil saturation, which can occur from reduced evapotranspiration, can result in higher groundwater loss. Shorter periods of soil saturation, which can occur from faster snowmelt, result in lower groundwater loss. Additional descriptions have been added to the text.

Thank you for your “cursory” but extremely insightful comments.
D. Response to comments from Pete Cafferata, California Department of Forestry and Fire Protection comments

COMMENT: Thank you for the opportunity to comment on the Sierra Nevada Adaptive Management Project (SNAMP) Water Team final report. The Water Team did a very good job compiling their extensive data set and modeling work in a readable document. The field results are consistent with the literature and they advance our knowledge about the impacts of low intensity forestry operations on water yield and water quality in the Sierra Nevada. I offer the following specific comments on the document based on my knowledge of the literature and field experience in the Sierra Nevada.

- The entire document should be reviewed by a technical editor to correct grammatical problems (e.g., capitalization).
- The entire document should have consistent use of units. Metric units should be provided first, with English units in parentheses if desired.

RESPONSE: Units have all been converted to metric.

COMMENT: Pg 6: MacDonald (1987) is incorrectly cited in the document and the reference in the literature cited section is also incorrect.

RESPONSE: MacDonald (1987) tests the effects of extending snowmelt on streamflow in Onion Creek, and has a good assessment of the early Sierra-based studies on forest and snowpack. His assessment and results of forest influence on snowpack are inconclusive, however, and the referring sentence has been updated to better reflect that assessment.

COMMENT: Pg 7, literature review. I suggest citing and discussing Heard’s (2005) study documenting that water yield changes associated with a prescribed burn covering 60% of a 250 ac basin were not detected in the Sierra Nevada, and including Burgy and Papazafiriou’s (1970) study, that found that chemically treating and burning a small (210 ac) watershed at Hopland Field Station had an average 50% increase in water yield (converting chaparral to grass).

RESPONSE: We attempted to limit our references to peer-reviewed literature, as much as possible, as required by journal reviewers when submitting research manuscripts. Ziemer (1987) is of the opinion that any benefit of managing forests with respect to water yields should be dismissed, similar to studies we referenced (Marvin, 1996; Kattelmann et al. 1983). We recognize the management challenge for both large-scale forest treatment and subsequent verification of impacts to water yields. Forest landscapes (e.g., recent wildfires) and policies are continually evolving, however, along with improvements in technology for better hydrologic measurement accuracy and precision. Research into forest growth and disturbance effects on hydrologic processes helps to fill gaps in the knowledge base that inform decisions made by forest managers.
COMMENT: Pg 9, study sites. More detailed precipitation data for the study sites would be beneficial to the reader. An estimate of annual precipitation can be obtained from the OSU PRISM site: http://www.prism.oregonstate.edu/explorer/. This site appears to provide an estimate of approximately 64 inches per year for Last Chance; how was the 78.3 inch estimate derived?

RESPONSE: The 78.3 inches (now 199 cm) of mean annual precipitation was the average of the four years observed (2010-2013) at the Blue Canyon meteorological station, used for water balance characterization and model input. Annual estimates for Blue Canyon are very similar to Greek Store meteorological station, which is much closer to the study sites, but had significant data gaps that rendered it unusable. PRISM data was analyzed within the study boundaries, but precipitation values were much more variable on an inter-annual basis and did not fit well with the trends from an assessment of four nearby stations in the first section of the discussion (page E18).

COMMENT: Pg 22, model simulations: The modeled water yield increase following a wildfire without SPLATs at the Last Chance site that reduces vegetative cover by approximately 50% is significant at roughly 67% (35.9 area-inches vs. 21.7 area-inches). The fireshed scale at Last Chance is approximately 10,000 acres. It would be beneficial for the reader to see what water yield changes in instrumented watersheds have been for basins either burned or converted to grass—with different drainage areas—to put this estimated percent increase in water yield in perspective. For example:

- Anderson et al. (1976) and Beschta (1990) reported only a 9% (8 acre-inches) increase for the intensely burned Tillamook Fire in northwestern Oregon for the first 16 years following the burn. These increases were for the large Trask and Wilson River watersheds (91,520 acres and 101,760 acres, respectively).
- Helvey (1974, 1980) reported that during the first post fire year total water yield from the intensely burned Entiat Experimental Forest in Washington was 50% greater than predicted. During water years 1972–1977, measured runoff predictions by 4.2 to 18.6 area-inches. Drainage basin size was approximately 1,200 acres (500 ha).
- Lewis (1968) reported that a 99% vegetation removal in an oak grass woodland in the central Sierra Nevada foothills increased annual water yield 4.5 area-inches. Basin size was only 12 acres.

RESPONSE: Additional references have added to the text regarding forest disturbance effects, although there is some difficulty with direct comparisons to post-fire runoff in other regions. We allow the forest to recover for a 10-year period first, as there are many additional considerations not addressed in this study with immediate post-fire effects, few wildfire studies exist beyond 10 years. Annual precipitation levels, precipitation regime (e.g., Mediterranean, Maritime, Continental), temperature range, vegetation type, and geologies all have important influences on the magnitudes of disturbance effects on water yields, limiting direct comparisons.

COMMENT: The text should include verbiage stating that the large estimated increase in post wildfire water yield at the fireshed scale at Last Chance (approximately 67%), while possible at
this scale, will not translate to much larger basins (that are not totally burned), where significant water storage occurs in the Sierra Nevada.

RESPONSE: The results of the wildfire modeling on water yield are presented as an indication of potential increases from wildfire at the fireshed scale, and are not intended to represent responses at larger scales. With the increasing area and intensity of wildfire events, it would not be unexpected that significant portions of much larger watersheds may be at some stage of recent wildfire or recovery, making assessments at these bigger scales more practical. Although we make the connection that this research is relevant to the larger water supply issues in California, we make no claim that these results are directly applicable (or inapplicable) to larger scale basins.


RESPONSE: Minear and Kondolf (2009) examined assumptions in existing reservoir sedimentation models and proposed a new model with additional terms to estimate reservoir sedimentation rates. While, modeling of these rates is important to water supply management in California, our focus is on the upstream sediment production and transport at the headwater scale. We believe that Sierra Nevada Conservancy is a more relevant citation for putting sediment measurements from this study in context of downstream water supply infrastructure.

COMMENT: Pg 43, second paragraph: Reference should be Dunne and Leopold 1978.

RESPONSE: Citation was corrected in text.

COMMENT: Pg 44, first paragraph: Include Reid and Dunne 1996 as a reference indicating that in-channel erosion is an important sediment source in the Sierra Nevada. They completed a sediment budget for the Kings River Experimental Watershed’s Teakettle Creek.

RESPONSE: We have been unable to locate Reid and Dunne 1996 (a book only available in hardcopy) so we did not include this reference.

COMMENT: Pg 44, second paragraph: Cite MacDonald et al. 2004 as a Sierra Nevada study that examined sediment transport at the small hillslope plot scale.

RESPONSE: This citation was added as an additional example of work done at the hillslope plot scale. Related work on connectivity between hillslopes and stream channels is cited elsewhere in this chapter.

COMMENT: Pg 45, first full paragraph: Several other Sierra Nevada citations should be considered for inclusion here, including Hunsaker and Neary 2012, Reid and Dunne 1996, Euphrat 1992, and Nolan and Hill 1991.
RESPONSE: The intent of this paragraph was to discuss existing literature that focus in on the relationship between sediment and discharge rather than hillslope processes. Text was updated to clarify.

COMMENT: Pg 46, top of page: Oliver et al. 2011 should be included as a Sierra Nevada example of wildfire (2007 Angora Fire) where sediment, turbidity, and nutrient level changes were documented.

Pg 46, first full paragraph: Lewis et al. 2001 should be included here as an example of California clearcut logging impacts on sediment yields.

RESPONSE: Oliver et al. (2011) and Lewis et al. (2001) show similar results to other cited references but for basins in the Sierra Nevada. Both references were added to the chapter text.

COMMENT: Pg 63, channel bed movement: No suspended sediment concentrations are reported in the document, and it is not stated what percent of the total sediment yield is composed of suspended sediment. Other work in the Sierra Nevada at the Kings River Experimental Watershed should allow some conclusions to be made regarding this topic.

RESPONSE: Suspended sediment movement is infrequent and episodic in these systems. Attempts were made to collect suspended sediment samples during events. The number of samples was low and there was considerable of scatter within the data. With our limited samples the relationship was exceedingly poor and we chose to use raw turbidity values instead. Full sediment budgets and sediment yield estimations were not within the scope of this work.

COMMENT: Pg 63, management implications: No information is provided in the chapter regarding the locations of forest roads in the studied basins, the number and location of road-stream crossings, the surfacing of the forest roads, road density, etc., and the reasons why sediment input from forest roads was very low for this study. Forest roads and particularly road-stream crossings are known to be significant sediment source areas in Sierran watersheds. For example, Korte and MacDonald (2007) reported that for one KREW sub-watershed, roads were estimated to contribute 25-50 percent of the total sediment yield, and nearly all of the road-related sediment came from a single mixed surface road that crossed the stream a short distance above the weir pond. Detailed information on the forest roads in both the Sugar Pine and Last Chance sites would be beneficial to the reader.

RESPONSE: No information was provided as we did not specifically look at the sediment impacts of road crossings because there were none that crossed streams in the headwater basins. The roads that did exist above the water quality monitoring locations were far removed from the stream channels. In addition, there was no new road construction, road maintenance or vegetation removal near the headwater streams.
COMMENT: Added discussion in the Management Implications section on what changes could occur to water quality parameters with severe wildfire should be included (including discussion on key factors affecting erosion and sedimentation, such as vegetative cover, precipitation intensity and storm numbers the first two years after the fire, soil texture, slope, etc.).
RESPONSE: Scaling up water quality results to the fireshed and modeling future effects was beyond the scope of this project. We purposely did not address implications of wildfire on water quality as management implications were reserved for fireshed products.
Thank you for your thorough comments.

E. Response to comments on the Water Quality section-sediment from Leslie Reid, Forest Service Pacific Southwest Research Station

COMMENT: Note: I did not evaluate aspects of the Water Quality section that did not relate to sediment.
p.43 ph.2: If snow cover is the only seasonal condition to be evaluated, it would be clearer if that were stated directly rather than being referred to parenthetically; if it is not the only seasonal condition of interest, then “i.e.” should be replaced with “e.g.”

RESPONSE: Suggested change was adopted; “i.e” was revised to "e.g”.

COMMENT: p.44 ph.1: Even if this is not intended to be a complete list of sediment production processes for the area, it would be useful to include a few more of the potential major sources, such as landslides, sheetwash erosion, road-related erosion, and gullies. In addition, “overland flow” isn’t a sediment production process, but is instead a transport medium (it’s the water, not the sediment)—this would be replaced by “sheetwash erosion.” Soil creep is an odd one—it doesn’t produce sediment, but it transports sediment to a place from which other processes can “produce” it to the stream—that’s where the bank erosion or landsliding kicks in. Bioturbation could use a bit more explanation if you think it important here. Most of the bioturbation going on in a landscape doesn’t produce sediment and ordinarily is implicitly included in creep as a transport mechanism. Do you mean animal burrows and tree throws? Also, mass-failure adjacent to streams would usually be considered a hillslope process if it’s contributing colluvial material, even though the slope may be destabilized by undercutting from bank erosion. The distinction gets a bit arcane, and you probably don’t want to get into it here.

RESPONSE: The chapter test was revised to provide clarification. "Sediment production processes" was changed to "sediment processes" to avoid confusion where there is an overlap between production and transport processes. Additional processes of landsliding, gullying, road erosion were included as suggested. "Overland flow" was revised to "sheet wash". Bioturbation was referred to parenthetically as a /processes within the soil creep heading. Clarification was made that the in-channel mass failure processes referred to are "mass failures of banks."

COMMENT: Bottom line is that hillslope processes move sediment down the slopes to where it can be transported by streams, and streams can then carve it away by eroding their banks and
beds; streams can also directly produce sediment by mobilizing bedrock and saprolite particles. I suspect that the point you’d like to make is that you’re hypothesizing that sediment mobilized high on the slope gets redeposited before it reaches the stream, so that sediment mobilized and transported by the stream during the storms you looked at most likely came from in- or near-channel sediment storage elements during the storm? If so, stating it directly would get you efficiently around much of the complication in categories. You’d still need to say something about landslides, gullies, and road-related erosion, though, since it would be startling if they didn’t play some role here.

p.44 ph.1: “Previous work in stream systems similar to those in this study has suggested that under normal conditions the hillslopes are not very connected to the channels”: this would be unlikely to be valid for the site unless streams are bordered by wide floodplains. If the stream isn’t continually widening its channel, then erosion of colluvial banks is supported by transport of sediment down the hillslopes, and erosion of alluvial banks is mining sediment likely to have originally been sourced from hillslopes farther upstream. Maybe what you mean is that most sediment eroded from sources high on hillsides is likely to be redeposited before reaching a stream channel? That doesn’t mean that hillslopes and channels are at all disconnected.

RESPONSE: We agree that over long time scales, hillslopes are in fact connected to stream channel, however it is the direct connection of hillslope material being transported downslope and directly deposited in the stream channel that we are referring to and that was very limited in these systems (during the period of study) as well as in the Stafford (2011) study. The text on hillslope-channel "connectedness" was reworded and additional explanation was added to clarify that hillslope sediment is re-deposited in riparian areas before reaching stream channels. As the term "connectedness" was used by the study cited, we chose to keep the terminology of the original authors.

COMMENT: p.44 ph.1: Stafford (2011) would not have a valid basis for supporting the general statement that “in-channel erosion of the bed and banks are the more important processes in forested mountain headwater catchments”—that’s just not true. There are plenty of forested mountain headwater catchments where landsliding is currently the more important process, and over a period relevant to landform development, hillslope and channel processes generally must be fairly balanced or you’ll either get a slot-canyon or a playa. To establish whether the statement is supportable for a particular period of interest in these watersheds, one would need to make a variety of field observations over a period that includes a range of storm and snowmelt event sizes.

RESPONSE: For the conditions and time scale observed in the period of study (for this study and for Stafford, 2011), the direct link between sediment and stream came from in-channel erosion processes rather than erosion and direct deposition of hillslope material into the channels. The text has been updated to clarify these processes and the statement that in-channel processes are "more important" in "forested mountain catchments" has been eliminated. Additional text was added to address that landslides and other extreme events could result in different processes and conclusions than those observed in this study.

COMMENT: p.54 I’m now confused about the timing of treatments—were the years referred to in the previous section hydrologic years or calendar years? When is the hydrologic year defined to begin at these sites? It sounds here like the treatment was carried out before the 2012 mmts
(i.e., 2 post-treatment mmt years), but in the previous section I’d been under the impression that there was only one post-treatment mmt year.

RESPONSE: The text referenced and corresponding table has been updated to correct this inconsistency.

COMMENT: Figure 34: Please rescale the discharge units into something more standard than 10.6/3—the current presentation is not useful.

RESPONSE: Units have all been converted to metric.

COMMENT: Figure 41: There’s something odd about this figure. Notice the synchronicity between creeks for small peaks in the “kg sediment” trace, despite the lack of a similar synchronicity in discharge traces. In addition, there’s an apparent periodicity to these fluctuations. I wonder whether the “kg” trace might actually reflect the effect of ambient temperature on the circuitry? Are there any cross-section measurements that could be used to validate the interpretation of results for these sites? Or it might be useful to try plotting “kg” against a measure of temperature to see if this might help explain the pattern. Are there other environmental variables that might influence calibration of the instruments or otherwise help explain the odd short-period synchronicity?

RESPONSE: We also had noted the rough synchronicity of the 4 to 10 day oscillation cycles in the graphs. These data were plotted against temperature, and barometric pressure, however, little to no synchronicity was observed between the load cell pressure sensor data and temperature or barometric pressure (after barometric pressure corrections had been performed). Text was added to chapter that further discusses these patterns.

COMMENT: Figure 41: kg per what? A 10”x6” Rickly load cell sensor? Check my estimates here, since they were quite literally back-of-the-envelope, but if that’s the case, the 20-kg-or-so change per sensor would translate to a sediment depth change of 20 to 30 cm, which would be the fluctuation in bed elevation. Typical snowmelt peakflows of 50 to 70 L/s shown in the figure are around 2 to 3 cfs (sorry about the mish-mash of units—I have a hard time visualizing 0.05-0.07 m³/s). A bed elevation change of nearly a foot is really big in a channel that has typical snowmelt peaks of 2-3 cfs—it’d be hard to get consistent measurements of discharge if the bed elevation is fluctuating on the order of the water depth. Again, I wonder if the Rickly sensors are measuring something other than what you think they are. Or are the discharge units actually a discharge per unit area?

Figure 41: the caption refers to WY2014, but the graphs don’t go there.

RESPONSE: The figure caption was edited to better explain what the "kg" data represents and to correct the water year. The reviewer’s assessment of sediment depth change is incorrect as it does not take into account the angle of repose and cone of influence of the sensor. Text explaining cone of influence was added to provide clarification.
COMMENT: p.58: Actually, it’s not possible to infer source from the style of hysteresis. Another likely cause for hysteresis is simply the shift in flow source that occurs as a storm progresses (if I remember right, this was described by Mizumara 1989: Hydrologic approach to prediction of sediment yield. J Hydraulic Engineering 115(4):529-535). In that case, the hysteresis would be present largely irrespective of the source locale. You’ll need field observations of the sediment sources in action if you’re going to diagnose them.

RESPONSE: Mizumara (1989) demonstrates that a hysteresis loop can be due to water sources shifting from predominately [sediment laden] overland flow to a higher proportion of sub-surface/groundwater flow. In the Mizumara conceptual model a lag in the sub-surface/groundwater flow reaching the channel creates a clock-wise hysteresis loop. We do not believe a shift in water sources causes the hysteresis loops in our systems because there are multiple lines of evidence supporting the fact that little to no overland flow occurred under the climate and land use conditions observed in this study. This includes significant duff layers, sandy soils with high infiltration capacities, and a lack of qualitative field evidence for soil surface erosion. In addition, Kattelmann and Embury (1996) state that sheetwash is likely "rare" in the Sierra Nevada especially in locations (such as those in this study) where riparian vegetation is intact. The low connectivity results seen in Stafford (2011) further support the limited delivery of sediment directly into streams by overland flow. In cases where most water enters stream as sub-surface/groundwater flow, it is implied that hysteresis is due to shifts in sediment sources rather than water sources. Hysteresis due to shifting sediment sources or availability has been well documented (Wood, 1977; Williams, 1989; Dooman et. al., 2008; Smith and Dragovich, 2009; Fang et al, 2011; Gao and Josefson, 2012).

COMMENT: p.60 ph.1: “The lack of significant difference seen between the pre-and post-treatment turbidity event means” it’s not clear exactly what you tested for here. I’m assuming that it wasn’t a straight comparison of means, since that wouldn’t account for differences in storm character and so wouldn’t provide a valid test. What did you test for and how did you do it?

p.60 ph.1: There really needs to be a statistical power analysis here—without that, it’s not possible to state that “The lack of significant difference seen between the pre-and post-treatment turbidity event means in the treatment watersheds is likely due to the light treatments performed and the dry conditions in the post-treatment years.” You would first need to determine 1) the level of change that would be considered operationally significant, and 2) the minimum level of change that could be detected, given the characteristics of the data and analytical method. If the minimum detectable change is greater than the operationally significant change, you’d need to conclude the results are indeterminate: there may be an operationally significant change present that just couldn’t be detected because of the weather and the experimental design.

RESPONSE: A statistical power analysis indicates that the number of events for the pre-treatment period and the post treatment period in both the control and the treatment watersheds are substantially lower than that required to conduct a statistically meaningful comparison. Text was updated to state this inability.

COMMENT: p.63: There’s not a long enough period of record to determine whether the bed elevations are stable over multi-year periods; that cannot be inferred from figure 41. Here too, you’d
need a power analysis. A more efficient method for determining this would be to evaluate the morphology of floodplain and channel—is there field evidence of long-term incision? aggradation?

RESPONSE: There was no field evidence of long-term incision or aggradation at the study sites. Text was updated to include these observations as well as clarify that data suggests stability but we recognize that additional years of data are required for a conclusive determination of stability.

COMMENT: p.63: As discussed above, because hysteresis likely results in part from changing flow sources, “analysis of turbidity hysteresis loops” cannot be used to infer that “in-channel erosion is main sediment source with sediment accumulation and depletion cycles tied to low and high flows.” You’d need field observations of the distribution of active processes to draw this kind of conclusion.

p.63: “Because in-channel sources dominate sediment supply, it is thought that any increases in sediment transport from treatments will be due to increases in discharge.” Because evidence for the dominance of in-channel sources is missing, the conclusion can’t follow. It may very well be true, but at this point it’s still an unsupported hypothesis. You might want to avoid the construction “it is thought that…” since readers have a tendency to translate that to “I think this is the case but don’t have the evidence to support it.”

RESPONSE: As stated above, the limited amount of overland flow and lack of direct connectivity between hillslopes and channel does suggest that hysteresis loop shape is tied to shifts in and/or depletion of sediment sources. Text was updated to indicate it is the direct sources we are referring to here (there are likely other indirect sources).

COMMENT: p.64: “SPLATS as implemented in SNAMP had no detectable effect on turbidity.” Here’s where the power analysis will be really important—it would allow you to identify the minimum change that could be detected, given the conditions you were operating in. In any case, you can’t conclude that the SPLATS had no detectable effect on turbidity. All you can conclude is that the methods you used did not detect an effect—that’s a very big difference in implications.

RESPONSE: The limited post-treatment observation period and the confounding factor of unusually low water year makes meaningful statistical tests difficult. We are not attempting to make a conclusive statement here, only to convey the observation that given the conditions we were operating in, we did not detect any effect in our data. Text was revised to clarify that the lack of detectable effects on turbidity was for the methods used and specific conditions of the study.
SNAMP Participation Team

Response to reviews and comments

Authors: Adriana Sulak, Susie Kocher, Lynn Huntsinger, Kim Ingram, Anne Lombardo, Maggi Kelly, and Shufei Lei

August 2015

The comments we received, from the Forest Service (Comment 15), anonymous peer reviews (Peer Reviews 3 and 5), and others (Comment 13), have been constructive and very helpful in improving and revising our report. Most of them we were able to address, some we were unable to address due to the timeframe and scope of the report, and a few we disagreed with. Some improvements and changes will be most appropriate in the context of developing publications. The following are explanations of how we addressed some of the comments.

General comments:

--Qualitative vs. quantitative data (anecdotalness). This topic was the subject of our presentation at the final meeting. Both types of data are limited in what they convey. Quantitative data provide estimates of the proportion of a particular group that responds a certain way. Qualitative data provide more depth and nuance, and in our case, were specifically used to capture variation in opinion. (It is, in fact, a questionable assumption that the opinion of a majority is the most important opinion.) Both kinds of data in this project were collected and presented according to the standards in the field; we have published results from each independently. Both do require, as does all science, confidence in the integrity of the analyst. We believe the two kinds of data complement each other and enrich each other, providing for a robust analysis and well-rounded insight into the participant experience in SNAMP.

--Citations and citations to pages: we added some, but in general, believe we have enough for this report. We will be able to focus more on citations for future publications, depending on journal specifications.

Peer Review 3:

The executive summary was extensively revised in response to comments. Explanations of the purpose of each section of the report were added throughout the document. A table of contents, making it clear that the appendix is not a monograph, has been provided. Parts II and III have been revised to address comments that are within the scope of the report. Standardized data are presented in Table F-2. Clarification that the distance outreach data also applied to core elements was provided. An overview of the strengths and weaknesses of different outreach methods is in Tables F-3 and F-4. The point about the need for more development of the discussion of the
second model is well taken and will be developed in future publication. Forest Service reviewers (Comment 15) commented that there was already “too much detail” in the appendix. Some modifications were made. The three party model remains a developing hypothesis, one developed as a result of SNAMP.

For the email survey, a non-response check was conducted using wave analysis. The close fit of the results of the two surveys and the meeting evaluations adds to our confidence in the consistency of the results. Reviewers and journals differ on where and how they want p-values presented. We chose to use the more complete information here. We clarified that the email survey does not represent the “full population of stakeholders.” There is no such list.

As to the possible use of the t-test: The quantitative data collected were mostly ordinal and categorical, and do not meet the assumptions for t-test analysis. Chi-square is a simple and straightforward test that requires few assumptions about the data. More sophisticated analysis is possible with logistic techniques and demographic data but is not essential to this report. Likewise, the full capacity of NVivo-based qualitative analysis has also not as yet been fully exploited, but is not needed for reporting purposes. More demographic information about interviewees was added. Sections VI and VII were revised to address the comments.

With the remaining time, we had to prioritize other changes over changing all the figures to make them clear in black and white. We believe the report will be primarily used online. Similarly, we cannot change figure layering. We have addressed all further comments with modifications in the text and thank the reviewer for the suggestions.

Final formatting was not done by the authors. We did make the changes that were within our purview.

Peer Review 5:

We hope that the efforts we have made to improve organization and clarity will address many of this reviewer’s very thorough comments. We substantially revised Part II to address comments. We added a figure to clarify what is meant by some of the language unique to SNAMP and tried to minimize our use of it. “Management workshops” were retitled to be “subject matter workshops” and were not limited to SNAMP but were on topics related to SNAMP.

As to a suggested guide, Section VIII on lessons learned at the end is the closest the authors are able to get to a short guide for others carrying out a CAM process within the limited time and funding for this report. We also point to the CAM curriculum and workbook as an excellent source of information on how to implement CAM (please see Appendix F-6: SNAMP Collaborative Adaptive Management Curriculum or http://snamp.cnr.berkeley.edu/documents/574/). However, any process developed to carry out CAM projects in other contexts would necessarily be constrained by different institutional, timeframe, and budget factors and require additional thought and guidelines. We added some additional lessons learned to address this reviewer’s comments.

As the appendix is a compendium rather than a monograph (though this was not clearly stated in the original version—we hope we have improved that), the comments about theory and
development will be applied to future publications derived from the various somewhat independent studies described in the appendix. Some clarity in main points should be achieved through the other SNAMP final report chapters and our revised executive summary. We have also striven to remove hyperbole from the document. We have addressed the remaining comments, with the caveat that some of the thoughts and ideas for further analysis will be used in future work—for those thoughts and ideas we are very grateful.

**Forest Service (Comment 15) and Public (Comment 13):**

For our Appendix F, we have substantially revised the text in part II as described above also based on comments received from the Forest Service. Most comments from the Forest Service not mentioned above were also addressed.

Suggestions regarding the interview data used in Chapter 2 and management recommendations in Chapter 5 were also addressed and those sections updated.

Please also see the qualitative vs quantitative data explanation above that responds to both the comment for Chapter 2 and the Forest Service comment about social science research methods.

We greatly appreciate the time and effort it took to review the Participation Team appendix drafts and thank our reviewers for sharing their insights with us.
In this response letter, we respond to comments relating to Chapters 1 through 5 of the SNAMP final report that were not addressed in the other team response letters. We appreciate the comments provided by an anonymous public commentator (Comment 13), Forest Service staff (Comment 15), and the California Forestry Association (Comment 21).

Comment 13, Anonymous comments [Survey Response 819751]:
Comment 1. The graphics (particularly in the integration section) were chosen to simplify the results, which is a good idea. However, it doesn't have the intended result because in this study, the changes in forest structures were not consistent by treatment. As a result, the simplification may lead a manager to make a decision opposite of what was intended. Instead of using the four treatment types, I suggest grouping results by the changes in forest structure that came about as a result of the treatments.

RESPONSE: We agree that the integration graph does not capture the differences in treatment intensity between the two sites. Our extended abstract (Chapter 3) does include these differences with additional graphs documenting treatment impacts on basal area and leaf area index. Also details of treatments and their impacts are included in the final report on fire and forest health (Appendix A).

Comment 2. The spatial scale of the treatments was limiting in making conclusions. The document states this several times, but it is critical to note that this was one of several studies that can inform management.
RESPONSE: The spatial scale was determined by management priorities, namely to modify fire behavior. Thus, the fireshed was the common scale used to report integrated results. For the empirical aspects of the water response to treatments, the small watershed was more appropriate. Scaling-up to the fireshed is common exercise in hydrology. The area where the extent of the fireshed was limiting was in regard to the spotted owl and the fisher. Both of these animals have such large home ranges that the firesheds encompass only a handful of individuals. The two studies did sample at a larger spatial scale and then used the results to develop indices that predict population responses to habitat changes caused by treatments and simulated wildfire. Given the diversity of scales, we are careful to note the potential limitations. At the same time, we made a dedicated and robust effort to report our findings at the scale most useful for management. Thus we agree with the comment that despite the limitations, our results do inform management.

Comment 15, Forest Service Region 5 staff comments:
Chapter 1
p. 2 (2005) Should be “Forest Service Pacific Southwest Region” -- as well as “Forest Service Pacific Southwest Research Station”. Or, “Forest Service, Pacific Southwest Region and Pacific Southwest Research Station”. Both are separate entities of the FS. Similar corrections at the top of p. 3. Other than references related to MOU signatories, you can probably just say “Forest Service” to mean region and station collectively. Also in other chapters.
p. 5 last para – have not yet defined “Participation Team” and its relation to UCST.
p. 6 last para -- Just FYI… We usually just refer to the SNFPA without reference to the Record of Decision. In fact, fireshed is not mentioned in the 2004 ROD and receive only casual reference (not as a “spatial unit of management”) in the FSEIS, so this statement is incorrect. Similar corrections needed in Chapter 2,…..
p. 7 para 2 – you might also mention that the fisher BACI assessment will be completed at a later date in collaboration with the PSW station.

Figure 1-1. UC did not actually sign the MOU. Figure caption should define all the acronyms (e.g., MOUP has not been mentioned in the chapter). Adaptive management adjustments... adjustments are not to policy (as stated) but to management. Suggest delete “to policy”. Also, I
think that parties other than USFS will do this – for example stakeholders, USFWS are already doing this.

**RESPONSE:** All suggestions have been accepted and the text and figure amended.

Chapter 2
p. 11 – Why does the LC site have two pairs of firesheds and SP only one pair? See figures and tables.

**RESPONSE:** We have added the following text to Chapter 2 to clarify this issue:

Sugar Pine had a classic paired-fireshed approach: one fireshed was treated and the immediately adjacent fireshed served as a control. At Last Chance, the topography limited the availability of a classic control. The best control in terms of matching vegetation, soils, terrain, area, and management history was to use the two adjacent watersheds (one north and one south of the treatment fireshed) to represent the "control" fireshed. The two watersheds were not spatially connected, but they did meet the criterion for fireshed designation in that we expected similar wildfire behavior in the two watersheds.

**Comment 21, Steve Brink, California Forestry Association:**
Chp. 5 – Integrated Mgt Recommendations Findings on p. 2 regarding benefits of SPLATs in reducing high severity wildfire is inconsistent with the Findings on p.4 where it states that vegetation reduction was small and regrowth in the first decade was more than the fuel reduction treatment. “SPLAT networks will reduce the risk of uncharacteristically severe fire.” (p. 2) Vegetation density from the implementation of SPLATs – “The small reductions in vegetation from treatments were temporary, with regrowth exceeding the original pre-treatment vegetation density in the first decade.” (p. 4).

In monitoring R5 forest health and fuels reduction accomplishments, I find that R5 is removing less than 10% of annual growth so the forests continue to get denser and denser and denser, which is leading to the megafires that are being experienced. With an average of 266 trees/ acre
(FIA) across all R5 forests in productive forest lands, SPLATS are too little, too late. We need to take over 1/2 the vegetation off the landscape in a manner that leaves a heterogeneous pattern.

RESPONSE: It is important to note that the most relevant impact of SPLATs is not changes in forest composition and structure but in wildfire behavior. We document both an immediate change in wildfire behavior post-treatment and our models suggest that the effect is sustained to some extent for two to three decades. We do note the limited impact in regard to forest structure. We also discuss the link between the nature of the treatments (e.g., Recommendations #20 and #21 in Chapter 5) and the extent of the treatments in terms of their effectiveness in modifying fire behavior (Table 8, Table 9, Figure 20 in Appendix A). As for the comment about accomplishments of the US Forest Service in the Pacific Southwest (R5), it is unrelated to the SNAMP report and thus not within the purview of the UC Science Team to respond.