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Route To:

Subject: Response to Peer Review Comments on the Socioeconomic Monitoring Interpretive Report

To: Pacific Northwest Research Station Director's Office

From: Susan Charnley, Socioeconomic Monitoring Team Leader

This letter responds to comments by four external peer reviewers on the Socioeconomic Monitoring Interpretive Report. The report is divided into 6 volumes. I address comments from the reviewers by volume.

General

We found most of the peer review comments to be quite helpful and welcomed the opportunity to receive feedback on the monitoring report, and to use that feedback to improve the final document. The comments ranged in scope from minor suggestions for improving small details of the report, to more substantive reflections on the validity of our key findings. I begin by addressing some of these more rigorous comments.

Two reviewers state that we need to be more explicit about our ability to make connections between changes in local communities and economies, and federal forest management practices. One reviewer in particular questions whether we can reach some of the conclusions we do based on our monitoring approach, and suggests reframing the document. S/he states that the design of our monitoring program focused on issues that were important in 1993, and that we have therefore been unable to keep pace with evolving forest management issues, and relations between management policy and economic conditions. The team did not formulate the evaluation questions; we were required to focus our monitoring on the evaluation questions in the ROD, and on the socioeconomic goals set out in the Plan, which were developed a decade ago (reflecting the issues of the time).

The reviewer suggests that because of this focus we found what people expected we would find (negative effects), but that these findings are wobbly. We did indeed find in many instances that there were negative trends in the socioeconomic benefits to rural communities from federal forests during the first decade of the Plan, and these findings are based on solid empirical data



which are presented throughout the monitoring report. The findings are neither wobbly nor assumed.

For example, our data clearly show that during the first decade of the Plan, the volume of timber offered for sale by the agencies did not meet Plan expectations; that agency jobs and budgets declined sharply; that more timber-industry jobs were lost than expected; that a number of FS offices closed; and that FS procurement contracting declined. The direction of these trends is clear, and their negative effects were described by numerous community members and agency employees interviewed.

To what extent were these trends caused by the Plan? We draw on qualitative data derived from key respondent interviews to discuss the ways in which the Plan contributed to these trends; we point out other contributing factors; and where we are uncertain, we clearly say so. We also disclose the limitations of our data. We believe the conclusions we make are well-supported by our data, and that the reviewer has perhaps over-looked the qualifications we are careful to make about the limitations of our data, and our ability to draw the conclusions we do.

The reviewer portrays our argument as follows: the main socioeconomic impacts of the Plan were its negative effects on jobs, income, and other forest benefits; and the economies of timber-dependent communities would have been better had reductions in federal timber harvest not occurred. S/he uses a number of quotes from the text as examples of this argument. One quote refers to the effectiveness of the Northwest Economic Adjustment Initiative in mitigating the effects of reduced timber harvest on job loss. It implies nothing about whether the economies of timber-dependent communities (not a term we use to refer to the sample case communities we monitored) would have been better off had no reductions occurred. The other quotes reflect what long-term community members described as being negative effects from reduced federal timber harvesting.

The reviewer asks, would community economies have declined anyway had there been no reduction in timber harvest? And, did federal timber harvests produce sustainable local jobs prior to the Plan? These are interesting questions, but not the ones we were assigned. The question we were assigned was, did rural communities and economies experience positive or negative changes associated with the Plan? It was not, how would rural communities and economies have fared in the absence of the Plan and reductions in timber harvest?

The reviewer suggests that we should have done a with-versus-without analysis to better understand Plan effects. Were it possible to test hypotheses about Plan impacts using “affected” and “control” communities (the with or without analysis), this might be possible, but we do not believe such a methodological approach is feasible or realistic. Nor do we wish to engage in “informed speculation” about what would have happened with versus without the Plan, as the reviewer recommends. That was not our purpose. There was a Plan; we were charged with investigating its effects on rural communities and economies; and we did that to the best of our

ability by examining changes in the flow of socioeconomic benefits from federal forests and their managing agencies to communities; the ways in which the Plan may have influenced those trends; and their effects on case-study communities. Our results are supported by both quantitative and qualitative data presented in the report. I do not believe we try to over-reach the limits of our data; quite the contrary, I believe we take great care to avoid over-generalizations, and to fully disclose the limitations of our data in drawing conclusions.

We clearly acknowledge that it is impossible to isolate the effects of the Plan from the effects of other social, economic, and ecological factors that affect communities, and make no attempt to do so. Instead, we describe how the Plan contributed to change in 12 case communities (or didn't); we cannot and do not pretend to measure the quantitative Plan impacts on communities relative to the impacts of other variables.

We do not compare levels and trends in socioeconomic variables for communities inside the Plan area with those outside the area, as the reviewer recommends. S/he suggests that had we done so, all else being equal, we might have found that reduced logging on federal lands has improved – not diminished – economic conditions in local communities. But when is all else equal? Such a comparison was outside the scope of our analysis; the geographic scope of our analysis was defined by the Pacific Northwest Interagency Regional Monitoring Program as being the Northwest Forest Plan area, and specifically, federal forests within the Plan area and their management effects on rural communities and economies. The monitoring question was not, how do trends in the Plan area compare with those elsewhere?

Two reviewers state that we should have said more about the positive impacts of the Plan. Our report shows that many Plan-area communities, including those within five miles of FS and BLM lands, improved in their socioeconomic well-being scores between 1990 and 2000. We do not claim that there were no positive effects of the Plan on communities, but rather, that we were unable to adequately document or measure those effects. Had data been available to measure some of the forest benefits that could have shown positive trends over time - such as jobs and income associated with recreation, improvements in scenic quality, improvements in water quality flowing off of the national forests to communities, or improvements in wildlife numbers - we would have included them and made the point. However no such data are available to our knowledge. Nor did most of the community members interviewed describe positive effects from the Plan on their communities. Instead, most reported concerns about increasing wildfire risks, deteriorating forest health conditions, and so on. We agree with the reviewers' arguments that the Plan may well have had positive effects on some communities by enhancing the amenity values of nearby federal forest lands, and contributing to conditions favorable for an influx of amenity migrants, which can have positive economic effects on communities. Amenity migration did occur in several of the communities we monitored, as noted in the report. However, we do not have the data to measure its effects. Nor did community members report much in the way of positive effects from amenity migration. Instead, they noted that the people who had built second homes in their communities and who had moved in and become long-

distance commuters spent most of their money elsewhere – ie., in the places where they worked – rather than locally, as resident timber workers had.

The reviewer states that there are inconsistencies between some of our data and our conclusions, and illustrates this point by comparing conclusions from the community case study-analysis with conclusions from the regional analysis of 1,314 communities. This is a comparison of apples and oranges, however, and it is no wonder that the reviewer finds inconsistencies. We have edited some of the language in Volume 1 to make it more clear when we are summarizing findings from the 12 case-study communities (which we do not try to generalize to the Plan area as a whole), and when we are summarizing findings from the 1,314 Plan-area communities. We do not link the two analyses. Nor do we make any statements about the impact of the Plan on the 1,314 communities. We document regional trends in forest benefits under the Plan; regional trends in community socioeconomic change; and use 12 case-study communities around 4 forests to investigate if and how these trends might be linked in these specific cases. I believe the reviewer thinks we are trying to do more than we intended.

Despite the fact that the case communities were randomly selected, the socioeconomic well-being scores declined for four, increased for two, and showed little change for six between 1990 and 2000. This may account for the fact that community members reported little in the way of positive benefits from the Plan. But again, we very clearly state that we are not using the case-study results to scale up to the regional level and make generalizations about Plan effects. I believe a careful read of Volume 3 Chapter 8 will demonstrate that our conclusions for the 12 cases are based on solid data.

We agree with this reviewer, that one weakness of the report is a failure to fully contextualize our monitoring findings within the existing literature on social and economic change in forest communities in the west. We hope to do so before the document goes to press, but have been unable to improve this part of the document prior to posting on the web.

This reviewer suggests that we should have included a discussion of the distribution of Plan effects on individuals and families. No matter what unit of analysis one chooses, there are always tradeoffs involved. We chose the community as our unit of analysis for two reasons: the ROD identifies communities as the appropriate unit of analysis, and Phases I and II of the monitoring program identified the need to conduct community-scale monitoring in order to answer the ROD evaluation question. It was not within the scope of the monitoring program to undertake an analysis of Plan impacts on individual workers and their families; we could not do everything. Moreover, the agencies want a regional-scale analysis of Plan effectiveness. In Volume 3 Chapter 8 we have added a paragraph acknowledging that by focusing on the community, we overlook Plan effects on individuals, as well as broader-scale socioeconomic changes occurring in the region that were perhaps more positive.

Finally, the reviewer asks us to expand our discussion of Plan impacts on communities by saying more about how it affected issues relating to equity; potentially irreversible effects like species extinction and permanent unemployment; and any associated exponential (non-linear) costs and benefits over time. We do not have the data to comment on any of these topics, and they were outside the scope of our monitoring questions.

Another reviewer asks us to discuss the spatial character of Plan consequences. Our original intent was to include an analysis of trends in socioeconomic benefits from the forests at the Province scale. However, this quickly became problematic from a methodological standpoint. The majority of our data are for individual FS and BLM units, or for counties (the exception being the community-scale US Census data). The Planning province boundaries do not correspond to National Forest or BLM District boundaries; nor do they conform to county boundaries. The methodological complexity of trying to aggregate county- and forest-scale data at the Province scale given these inconsistencies proved to be more than we could sort out given time limitations. We recognize the value of analyzing trends and impacts at a finer scale than was possible in this interpretive report. In Volume 6 we recommend that future monitoring investigate the potential for analyzing sub-regional variations in monitoring trends.

Finally, some of the reviewers commented that our work was not really “monitoring” per se given that a socioeconomic monitoring program was not established early in the Plan period, and designed to collect a decade worth of relevant indicator data. Instead, it was a retrospective analysis and we had to make the best use of already-existing data to try and answer the monitoring questions. This is a good observation and we have added a paragraph to the Methods section of Volume 1 to clarify what this monitoring program was and wasn’t, and how that affected our ability to answer the monitoring questions.

We have added summaries to the front of each volume in response to reviewer recommendations and Station requirements. The summaries include the monitoring questions and associated indicators addressed in each chapter of the volume. They also list Plan expectations, and summarize the monitoring results for each question.

We have adopted many of the reviewers’ more minor recommendations for improvement.

Volume I Key Findings

Volume I is a compilation of monitoring results and key findings from Volumes II-VI of the socioeconomic monitoring report. Any reviewer comments on Volume I link to comments on the other volumes, which contain the detailed material that is summarized here. Thus, I address

most of the comments on Volume I under the associated volume, below; the reader can assume that changes made in Volumes 2 through 6 are reflected in the revised Volume 1.

Volume II Timber and Nontimber Resources

Three of the reviewers commented that the Interpretive report emphasized the negative socioeconomic effects of the Northwest Forest Plan, and said little about its positive effects. One potential positive effect of the Plan relates to scenic quality. Natural amenities draw migrants to rural communities and can therefore help promote economic development there. The Plan was expected to improve scenic quality on the national forests, thereby enhancing natural amenity values. One reviewer points out that the failure of the socioeconomic monitoring report to monitor scenic quality on federal forest lands is a major gap. I agree; yet for reasons explained in Volume II Appendix A, it was not possible to do so because of the lack of agency data on this topic, and because of the guidelines imposed on the team regarding the monitoring approach. I have added a discussion of this topic to Volume VI on future monitoring, and recommended that scenic quality be monitored in the future.

The same reviewer points out the weaknesses in Volume II Chapter 6 associated with monitoring recreation, another important activity on the national forests and for rural economic development, and one that the Plan may have had positive effects on. S/he suggests strengthening the chapter in several ways. First, s/he asks for our rationale in choosing the indicators we did. I have added a paragraph to the methods section that appears in Chapter 1 of Volume II (the Introduction) to explain that our choice of indicators for recreation and all other resources discussed in Volume II was based on the availability of existing agency data. The reviewer also asks for more specific information on trail miles, apparently having overlooked the information on FS trail miles contained in the section “Developed Sites”. While I agree that it would be nice to know how many trail miles provide what kinds of recreation opportunities, detailed data of this nature were simply not available from the FS regional and BLM state offices, nor were historic data available that would make it possible to quantitatively measure Plan impacts on trails miles and associated uses. The reviewer requests a discussion of how trails and associated recreation opportunities were affected by the Plan. I provide this information in the Discussion section for the four case-study forests – more detailed information can be found in the associated case-study General Technical Reports. We do not have the data to support such a discussion at the regional scale.

The reviewer asks us to remove acres of Wilderness as a recreation indicator, and to address it elsewhere in the report. I have left it in as a recreation indicator because we were charged to monitor whether recreation opportunities under the Plan were predictable (whether or not these opportunities were directly affected by Plan implementation). Even though recreation is not the only use of Wilderness, Wilderness data are all we have in the way of recreation opportunity spectrum data. Furthermore, this is the most logical place in the report to address the topic of Wilderness. The reviewer also criticizes our failure to use the Recreation Opportunity Spectrum (ROS) for recreation monitoring, stating that we don't even mention it. Volume II Appendix D contains a detailed discussion of the agency data available for answering the evaluation question that is the focus of Volume II, including indicators not monitored. There I explain why we could not use ROS data for monitoring recreation opportunity in the interpretive report – the reviewer apparently overlooked this explanation.

This same reviewer also comments on the section on ski areas. I have edited the document to explain why ski area capacity data are not included in the report. The reviewer notes that the nature of the skiing opportunity may have changed significantly during the decade; I point out at the start of the chapter that we did not monitor the nature or the quality of recreation experiences on federal forest lands.

I have not organized the chapter in a manner that separates recreation demand and supply data, as suggested by the reviewer, because I think it makes more sense to organize the chapter by type of recreation opportunity – these are the indicators.

One reviewer asks me to expand the scope of the analysis regarding the predictability of supplies of nontimber resources produced to include water and forest habitat. Our choice of resources monitored in relation to the evaluation question on use levels of natural resources was based on the list of monitoring items contained in the Record of Decision. Water and habitat were not on the list, and we did not have the resources to go beyond the ROD requirements for monitoring associated with this interpretive report. However, I have noted in Volume VI that future monitoring might include additional resources.

This same reviewer comments that I fail to provide evidence to support the claim that a predictable level of timber sales was not produced during the first decade of the Plan. There are two ways in which I discuss predictability with regard to timber. One has to do with the total volume of timber offered for sale by the agencies on an annual basis from all land-use allocations. I have added a paragraph to Chapter 2 of Volume II (the Timber chapter) that compares the variability in the volume of timber sold by the agencies in the 1980s and since 1994. The analysis shows that the variance since 1994 has been far more than in the 1980s, and that the Plan was not successful in increasing the predictability of timber harvests. The second way in which I discuss predictability is as it relates to PSQ estimates. The premise here is that by offering a volume of timber for sale that was consistent with PSQ estimates, a predictable and

sustainable level of timber would be produced. The conclusion that PSQ estimates were not met during the first 9 years – for which the best available data are reported in the chapter as evidence – can be interpreted as a failure to produce a predictable supply of timber.

Otherwise, I have made small edits to Volume II to respond to reviewers’ suggestions and concerns. I believe I do a good job in the analysis section of clearly stating what the limitations of the data are for answering the monitoring questions, and identifying cause-effect relationships.

Volume III Rural Communities and Economies

Because this volume is large, the team has organized its responses to comments by chapter.

Chapter 2 – Socioeconomic Conditions and Trends for Communities in the Northwest Forest Plan Region, 1990 to 2000

One reviewer questions our classification of “forest-based” and “nonforest-based” communities. We used the terms “forest-based” and “non-forest based” as a shorter way of saying “communities less than or equal to 5 miles from FS and BLM lands,” or “communities greater than 5 miles from FS and BLM lands”. We defined the terms as such in several places. Clearly readers did not feel comfortable with these descriptors, and perhaps that is understandable given that in other contexts the terms have other meanings. However, we talked about the various ways that the term “forest-based” is used. We have omitted all use of the descriptors “forest-based” and “non-forest based” in the chapter, and “nonmetropolitan”, and gone back to the original way we were describing the unit of analysis. Those terms are lengthier, and earlier reviewers didn’t like them, but hopefully the terms are now less value-laden.

Given the scale of the project, we had to rely on fairly straight forward ways to characterize communities in the region. We chose to characterize them based on their proximity to FS and BLM lands using a spatial analysis. Of course, collecting primary data would have been better, but it was completely beyond the scope of the project. We feel that this reviewer got too caught up in the descriptor “forest-based”, even after we had tried to be clear on its use. We modified the text considerably and hopefully will avoid this type of confusion in the future.

Furthermore, there is a two-paragraph discussion of the evolution of terms used to characterize communities and their relation to forest. That is the only time we talk about timber dependency. We have moved that discussion, and hopefully readers won’t mistake our purpose. We are not analyzing timber dependent communities, and never intended to do so. The best we could do

with this scale of analysis was to speak to the proximity of communities to FS and BLM lands.

The same reviewer suggests various interpretations of the data on change in socioeconomic well-being, and its possible relation to federal forest management policy. We found that communities in close proximity to federal forests are not doing as well as communities farther away, in general. It is not clear if the reader understood that point. We tried to make the writing more clear. The reviewer offers some interesting interpretations of some of the data, but we found many of those interpretations to be beyond the scope of what we could say, given our data and analysis.

There were some additional questions from this reviewer: 1) “Is the Team asserting that, absent the NWFP, population in non-metropolitan areas would have grown even faster, and even more communities would have exhibited an increase in socio-economic characteristics?” We don’t make this assertion, nor even attempt to. I’m not sure what this person is getting at. 2) “If so, via what mechanism[s]? If it is not making this assertion, then perhaps changes in population and socioeconomic conditions have been independent of the NWFP.” Yes, they may well be independent of the Plan, we did not suggest otherwise.

Another reviewer noted that the analysis in Chapter 3 begged for more analysis with such a rich data set. S/he asks, could the authors also buffer the metropolitan areas and look at the communities within 5 miles (or whatever makes sense) of the cities? Might that help explain the patterns of change in well-being status? It would have been interesting to look at the metropolitan communities and compare them to the region, but it was beyond the scope of the project.

Regarding the methods section, we’ve added supplemental detail to Appendix C, and changed some of the methods description in the text. There remains a considerable amount of information on the methods in the text, but we think that detail is important (at least for some audiences). We kept the methods references to other GTRs, because they are necessary to reference, as we would anything else.

Another reviewer went through Chapter 2 and made specific comments by line. We respond to them accordingly:

Page 2-4 line 7. You talk about communities in 72 counties. Are there parts of these counties that aren’t included in the analysis? Did you deal with partial counties? This was unclear.

It is clear in the document. We have moved the paragraph up so that it is more evident.

Page 2-4 line 27. You say that block groups by themselves are “not particularly meaningful.” So how did aggregating them make them meaningful. Block groups may not be meaningful for your analysis, but it must be for someone’s. Is the explanation found in Donoghue 2003?

We have removed some redundancy in the text to clarify this point. Perhaps the reviewer did not read the paragraph that described in detail the aggregation process. We have moved some paragraphs around to make that part more visible.

Page 2-5, lines 4-6. Why even bring this up about local experts not being consulted. It just raises questions. You already said that you modified the earlier process—that's enough. I would eliminate this whole paragraph. They can go do Donoghue 2003 for more discussion.

We have omitted some of the paragraph, and moved some of it to Appendix C.

Page 2-5, lines 19+. You talk about aggregating, and that if you were unsure you decided not to aggregate, but unsure of what? You aggregated until what? I know this is probably in Donoghue, but I think it needs a few lines here.

This was explained, and perhaps the rearranging of paragraphs we have done will make it more clear.

Page 2-6, lines 4+. In general in this section there is just way too much information. Much of it could be shown in bar graphs. For example, lines 17-23 you discuss percentage of communities that fell within population ranges. This could easily be shown in a bar graph and just referred to. Also, you use these ranges throughout the report, I'd give them a name, like you did for your well-being classes, perhaps: very small, small, medium, large, very large.

We created bar charts for some of these data.

Page 2-6, lines 29+. This discussion of size of communities and loss of population could be easily shown in a bar graph.

We left this as is.

Page 2-9, line 18. I feel you down play the concept of sense of rurality. This sense is very important to quality of life. You chose not to include it—that's OK, but I don't think it's unimportant.

The point of the sentence was to show that public lands contribute to open space, thus enhancing the sense of rurality. We modified it somewhat to make this more clear.

Page 2-11, lines 1-13. I think this is an example of where you can just point people to the figure and just say these trends are the same as for most of the U.S. and in has implications for demand for services. You don't have to put in words what is in the figure—let the reader look at that.

We omitted some text.

Page 2-12, lines 12-21. On page 11 you talked about an increase in younger people, and here you mention a growing school enrollment. Sounds to me like people are staying. This is usually a positive sign. Can you shed light on this from the interviews/case studies. Throughout this section, I think you could have used examples from the case studies to add texture to the discussion and make the reading of the statistics more interesting....The idea is to combine the qualitative and quantitative data to present more depth and to explain what's going on with the numbers. I don't think most of the authors did a good job of this.

It would have been interesting to better integrate the case-study material with this regional analysis, but given our time and resources, we tended to compartmentalize aspects of this project, and for a variety of strategy and efficiency reasons (and others) we did not integrate the two as much as we could have. However, we made the point that Parts II and III of Volume III focus on a regional-scale analysis, and Part IV focus on the community-scale analysis. In Volume III Chapter 8 we present community-scale data that mirrors the regional analysis in Parts II and III and we integrate those quantitative data (on changing social and economic conditions, procurement contracting, agency budgets and staffing, community economic assistance, and payments to county governments) at the local scale with qualitative data from the interviews to develop a picture of what the numbers mean. I believe the reviewer will find the analysis s/he is looking for in Chapter 8.

Page 2-14 lines 26+. Many of the trends discussed in this chapter are mirroring what is going on nationwide. I think the local and regional trends need to be nested in this larger discussion. What may seem like something unique may not actually be unique. However, if you can use the case studies to show how this is different from what we're seeing elsewhere, you should do so.

It was beyond the scope of our analysis to compare trends for the Northwest Forest Plan region with national trends. We don't suggest that Plan-area trends are unique.

Page 2-16, lines 15-16. It is strange to group E. Washington Cascades and SW Oregon with Klamath because the rate of unemployment is so different. Klamath seems to be in a class by itself.

We have omitted the province-level analysis because reviewers and readers were getting bogged down in too much detail, too much data, and too many tables and figures.

Page 2-19, lines 17 through Page 2-20, line 20. This should all go in an appendix.

We left in the text because the methods are so closely linked to the results. However, we created a table that will hopefully make the paragraphs easier to read.

Page 2-20, lines 7-15. Regardless or whether this discussion is in an appendix or the text, the whole discussion of categories should go in a table. It's difficult to follow in text.

We have made a table.

Page 2-23, lines 3-6. The discussion of percent of communities that increased or decreased in well-being should be put in a table. You could have difference categories of change: >50% decrease, <50% decrease, no change, <50% increase, >50% increase, or whatever categories you want to sue. The size of dots for categories just don't work very well here.

We didn't use size of dots, we used color and shape. We tried to balance tables with maps and decided that another table was not necessary here.

Page 2-28, line 23. Here, where you discuss non-metropolitan communities, are the “communities in close proximity” the “forest-based communities”—be consistent with terms.

We reviewed the entire document and removed all reference to forest-based communities as a descriptor of a unit of analysis. Also, reviewers got hung up on the use of the term “nonmetropolitan”, expecting that we would offer a counter analysis at the metropolitan scale (which was beyond the scope of this project). We removed the use of the term “nonmetropolitan”.

Page 2-33, lines 7-12. In the text you talk about communities with between 250-200 people, but in Figure 2-2 your breakdowns are 88-500, 501-2000, so in the text you’re talking about part of the first category and all of the second. I think this is confusing—pick some categories and stick with them. Also, note the earlier suggestion to name these communities so it’s easier to refer to them.

We made the breakdowns more consistent. We tried to provide more information in the text than was in the figure, but apparently that was confusing. So, we simplified it.

Finally, this reviewer comments that because our use of terms in referring to communities is confusing, we should include a diagram to make our units of analysis more clear. We don’t think a diagram is necessary. We have come full circle in the definitional process. Early reviewers did not like the lengthy descriptor of “communities within 5 miles of FS and BLM lands.” So, we labeled them “forest-based communities” and made it very clear that that was a label, not a sociological construct. The terms were consistently used and defined throughout Chapter 2. However, if readers did not pay attention to the definitions, they could have resorted to their own interpretations of the terms (i.e, forest-based). Because peer reviewers had trouble with these labels, we’ve gone back to the basic, simple (but long) way to describe the different community units of analysis. We no longer are using “forest-based” and “non-forest based”.

Similarly, the reason that we used “nonmetropolitan communities” was because in early reviews people did not like the lengthy descriptor of communities in the NWFP region. Given that we had divided the entire region into 1314 communities and 10 metropolitan areas, it seemed reasonable to call the 1314 “nonmetropolitan communities.” But it appears that when people read the word “nonmetropolitan” they assume that there will be a similar analysis done at the metropolitan level, or some sort of comparative analysis (even when we’ve said clearly that the focus is on nonmetropolitan communities). So, we removed all use of the term nonmetropolitan, and only used metropolitan in the few cases where we were actually addressing the entire population in the region.

Another reviewer recommends we clear up the confusion between socioeconomic well-being and community wellbeing. Part of the confusion regarding the terms is a result of changes made after a previous version went to the Station editor. The editor felt the term community socioeconomic well-being was too long, and she also did not like the acronym SEWB. So, there were times in the text where we shortened the term to refer to community well-being. Clearly this added more confusion, as people familiar with the debate over sociological terminology can

sometimes get easily caught up in that debate, even if we have specifically defined our terms. We did define our terms and usage of them, but we have gone through the document and added back in the word “community” in many cases, and rewritten some of that discussion so it is more clear.

This reviewer also expressed a concern about our reliance on census data to estimate wellbeing. We suggest that census data are a useful way to begin to get a feel for socioeconomic well-being. In fact, we say that some of the indicators, such as education and travel time, may serve as useful proxies for the social and human capital dimensions of community well-being. We also talk about the limitations of census data. We feel we addressed this concern in the existing draft, but have gone through and clarified the writing more.

A related concern expressed by this reviewer was the question of whether community well-being was the appropriate variable to analyze, or whether community capacity or resiliency might be more appropriate, and asks for our rationale for focusing on community well-being. We reorganized and rewrote part of the discussion about social and human capital, and moved it to the beginning of the measurement section. Hopefully this will clear up some of the confusion about our use of well-being.

Finally, the reviewer suggests that the data displayed in figures 2-2 to 2-4 could be displayed better by using polygons rather than “circles” or the point orientation they currently show. We tried many iterations of how to display these data and determined that polygons, contrary to the reviewer’s suggestion, were the most confusing. All community polygons in the region are contiguous. Thus a community of very small population size may have a very large polygon, because much of the land ownership within their polygon is public lands. This would distort how the reader viewed population data, and would complicate, rather than simplify their interpretation.

Chapter 3 - Jobs and Income Associated with Resource and Recreation Outputs

One reviewer questions the finding in this chapter that the Plan had caused the loss of 20,000 direct timber industry jobs. S/he also asks for clarification about the reasons for the loss of 11,000 jobs in the late 1990s. This section has been substantially revised to improve clarity and focus the discussion about the reasons for and the amounts of job losses. During the review and rewrite, a spreadsheet calculation error affecting the 1995-2000 discussion was found and corrected. Additional documentation for the calculations was added.

Another reviewer makes detailed comments on Chapter 3, by line. We respond to these comments accordingly:

Page 3-2, line 17. You are talking counties here. I think after all the discussion about communities in Chapter 2 that you need to be clear here that the unit of analysis has changed.

That aggregations of counties are the unit of analysis is clearly stated in this line and in the methods section.

Page 3-3, lines 3-5. You talk about wages falling up in the previous chapter average medium household income went up. Can you draw from the case studies to discuss this a bit more?

The discussion here is about the relative portion industries make up in the total economy. The growth in wages and household income is not related.

Page 3-11, lines 10-22. Is there a way to put this whole paragraph in a table or figure? It's very confusing. I would start with the number of jobs in 1994--??. Some of those jobs (about 5%?) were dependent on BLM and FS harvests, so you show that breakdown—of X jobs in 1994, Y were supported by BLM and FS harvests, X-Y were not dependent on harvests. In 2000 you have 80,500 jobs, approximately 4,000 dependent on BLM and FS harvest, 76,500 not dependent (if your 5% figure is correct). You talk about 11,000 jobs lost, but I don't know if this is from the total or from the number dependent on BLM and FS harvests. Anyway, there must be a better way to illustrate this.

The paragraph was substantially rewritten to improve clarity.

Figure 3-2, page 3-23. I always have a problem with line graphs for discrete variables—this should be a bar chart.

I did not make any changes.

Page 3-13: “Because the FS and BLM no longer play significant roles in the supply of timber in the Plan area, changes in the supply of timber from these federal lands are not likely to affect the timber industry and related employment in the future.” This statement needs further clarification regarding scale. Clarify by making the following change (in bold italics): “supply of timber in the Plan area ***as a whole***, changes ...” Also, see page 8-75. “One of the shortcomings of using ‘the community’ as the unit of analysis in this study is that it obscures, and potentially devalues, the experience of individual community members.” Although FS and BLM may not have a significant role in regional timber industry employment, it is noteworthy that some local mills and communities do rely heavily on federal timber for employment. It would be helpful to incorporate this concept (i.e., the effect of scale of analysis) in the discussion on page 3-13 relative to the effects of the federal timber supply.

I incorporated these suggestions in the conclusion to the timber effects and overall conclusion.

A third reviewer also commented on this chapter. One comment was that it was not clear and up front what the bottom line was in terms of job loss. This section has been substantially revised to improve clarity and focus the discussion about the reasons for and the amounts of job losses. During the review and rewrite, a spreadsheet calculation error affecting the 1995-2000 discussion was found and corrected.

A second comment concerned a lack of clarity about the conclusions regarding the relationship between predictability of timber outputs and timber industry employment. The predictability of jobs is directly and strongly related to predictability of timber. The added discussion about

timber industry adjustments during the 1995-2000 period identifies that many of the job losses occurring during that period were the result of industry recognizing the permanence of the reduced timber supply pre-1995. Additional documentation has been provided on the calculations.

The reviewer also observed that there was not a sufficient analysis of timber industry timber sources (e.g. what about imports?) to justify the statement that changes in the supply of federal timber would not likely have a strong industry effect in the future because the agencies no longer play a significant role in the regional supply. S/he also said we should acknowledge the unevenness of impacts of reduced timber harvests. The uneven distribution of effects was originally identified in the section on employment by state and rural/urban differences. Discussion of the effects at the level of the individual firm was not a purpose of this analysis. The author added acknowledgement of the ecological fallacy to the conclusion on the last page.

Finally, s/he states that it may be true, as the author suggests, that the regional economy in the Plan area is not forest-based and therefore changes in the forest-based economy would have little impact – but if that is the basis for the statement then it should be made clear. The author has added justification for this statement.

Chapter 5 Procurement Contracting

The authors appreciate the positive feedback reviewers gave on this chapter.

One reviewer asked whether the approach used in the chapter for analyzing agency procurement contracting practices could also be used for analyzing agency timber sales. An identical approach could be used with timber sales. The most important data required for this type of analysis is the location of the timber sale (national forest/BLM district), and the zip code of the purchaser, and the value of the sale. These are all available for timber sales. We agree that this can and should be done for timber sales, but we did not think of it until quite late in the analysis because the timber sale and the procurement contracting analyses were done separately. However, in ongoing monitoring work around the Okanogan-Wenatchee National Forest we have added (a somewhat abbreviated) analysis of timber sales.

A reviewer asks about the role of the size of contract. Past research as well as the more detailed statistical analysis that comes from this research suggests that the size of contract plays a role in the distances that contractors travel to work on national forests, even after controlling for type and location of work. We have added a comment to that effect to the text.

One reviewer comments that the section on challenges to creating community benefit is too brief. We too would have liked to have a more detailed discussion of challenges. We have incorporated all the information we have into this section. Identifying the challenges more clearly and in more detail was difficult with the limited qualitative data available. One lesson learned from this process that has been incorporated into subsequent case studies is to have the procurement contracting data analyzed and available for reactions by interviewees, which will allow for more concrete, focused discussions with key informants about the causes of particular trends.

Another reviewer suggests we use bar graphs instead of line graphs for some of the figures. We re-evaluated the figures in this chapter to consider whether they should be line or bar graphs. We decided to keep the line graphs as they are because the line graphs that were designed to show trends over time, which we believe is better done with a line than bars in these instances. In other cases, where change over time was not the primary focus, we used bar graphs.

Chapter 6 – Community Economic Assistance

One reviewer suggested that we make the monitoring question in this chapter more clear. We have followed the suggestion, and changed the monitoring question to “How did agencies assist with long-term economic development and diversification in rural communities affected by cutbacks in timber harvest on federal forest lands and what were the outcomes?”. This Chapter 6 review of the community assistance programs presents what was done over the last decade to address this goal, and also does reflect on some of the outcomes of this assistance. In addition, the case studies assess the outcomes from the perspective of community members.

Another review comment stated that the author draws heavily on other assessments in the chapter, but at times it is not clear how the descriptive statements or conclusions are reached. There are numerous citations of the assessments from which the information in this chapter was summarized. Summaries and conclusions of the individual programs are in the “Results”, “Assessment of Other Initiative Components”, and “Assessing the Effectiveness of the Initiative” sections of the chapter. Since there are no specifics mentioned with this comment, the author made the following changes that should address the following two bullet items as well: some minor wording changes have been made to the report, and the conclusions section has been revamped to clarify.

One reviewer observed that unlike other chapters, Chapter 6 did not revisit the expectations. The new conclusions summary should clarify long and short-term mitigations and their outcomes.

Finally, a reviewer states that the suggestions for monitoring. While process is important, ideally suggestions for future monitoring should include outcomes data to determine if processes are producing desired outcomes. Case studies from cited assessments, and those completed as part of this monitoring effort are important components of this review of how well outcomes have been met from community members’ perspectives. The author is interpreting “outcomes” to mean effectiveness in meeting Plan socioeconomic goals.

Volume IV Collaboration

Several reviewers made small suggestions for improving the chapter on Federal Collaborative Efforts (Chapter 2). We adopted nearly all of these recommendations in revising that chapter.

One reviewer commented on Chapter 3, stating that the case studies were most informative, and in particular the successes suggested in the Coos Bay case study could serve as the basis for more focused inquiry. We rewrote the sentences about the Coos Bay case that gave explanations for why the Coos Bay situation was different from the other cases. We tried to keep the discussion general, however, as a prior draft of this chapter was criticized as having too much case-study detail. And, we didn't want the discussion to seem unbalanced. We agree, however, that the Coos Bay District would be a useful case study for more focused inquiry into collaboration since the Plan.

Volume V Public Values and Forest Management

Perhaps the best way to monitor changing public values and preferences relating to federal forest lands and their management in the Plan area, as well as issues and concerns relating to forest management under the Plan, would be to implement a survey. A survey would provide quantitative measures associated with different viewpoints, and would (in theory) reach a broad cross-section of the population. The constraints under which the monitoring team operated in order to produce this interpretive report made it impossible to gather primary data using survey methods. Had it been possible to conduct a survey, the team could have produced the more comprehensive quantitative data that two reviewers were looking for, providing a better context for discussing the qualitative interview results on these topics. It also would have been possible to obtain more detailed information about public perceptions of specific aspects of the Plan and its consequences. The best we could do given the circumstances was to review the literature on this topic, summarizing the survey findings of other researchers, and obtaining qualitative data from interviews with community residents. Admittedly this approach limited our ability to respond to the monitoring questions posed in Volume V, as the studies reviewed here were not designed with the same questions in mind, and we did not interview a random sample of the local population. I have added to Volume VI (Future Directions) a discussion of using survey methods in future socioeconomic monitoring to address some of the monitoring questions, including those relating to public values and forest management.

Several of the agency managers who reviewed an earlier draft of the interpretive report expressed the opinion that the socioeconomic monitoring program should not conduct effectiveness monitoring relating to the goal of protecting forest values and environmental qualities associated with late-successional, old-growth, and aquatic ecosystems, viewing this as the domain of biophysical monitoring only. Second, several agency reviewers did not find the qualitative interview data regarding local peoples' perceptions of forest environmental quality, forest management issues, and the effectiveness of the Plan at protecting forest ecosystems valuable and useful for decisionmaking. This caused the authors to remove a substantial amount of qualitative data from Chapter 3 of Volume V – details that this reviewer is asking for more of. These details can be found in the associated case-study forest GTRs.

One reviewer recommended that we strengthen Chapter 2 by adding in specific quantitative data, such as percentages of respondents who expressed different views, that were associated with the survey findings reported. I have done so.

Volume VI Socioeconomic Monitoring and the Northwest Forest Plan: Program Development and Future Directions

Several reviewers made suggestions of things they believed the socioeconomic monitoring program should address, but did not, in the interpretive report. I have captured many of these suggestions in Chapter 3 on future monitoring. Otherwise, there were few comments on Volume VI. We did make some substantial changes to Chapter 3 of Volume VI, though only a few of these were triggered by reviewers' suggestions.