Hi Jason,

Just got the reviews back, E. Nadin and A. Chapman were reviewers and their response was significantly positive. One unnamed reviewer also had a positive response. Minor revisions are requested.

The overall comments can be summarized as “should be shorter but can’t figure out how to change”. We could maybe migrate figures to supplement following Alan’s recommendations (which I have not yet read).

Comments about the length do not seem fatal. I suggest including a “higher ground” comment in your cover letter to the editor to the likes of: We understand that the manuscript is both dense and broad in scope, but feel that G-cubed is *the* ideal venue for this research since it integrates geochemistry, geophysics, numerical modeling and integrative geoscience.

One point that Alan finds confusing is our argument on 5.2 about the link between the slab window and the Crystal Knob basalts. I find this one of the weakest points of the paper also, and I am not sure how we should address. I have added a line or two at appropriate point specifically aimed at Alan’s good comment!

Unnamed reviewer #3 suggests that the reorganization of the paper outside the traditional  Methods/Results/Discussion framework is not wise, but I don’t think it’s worth changing. I agree with you.

Overall, I’d appreciate your comments on the specific changes. For the next draft, I will need to have several new items. Particularly, I will need to describe the grants that funded this research in a statement at the end. I don’t know what needs to be included really, but speculate that some TO grant numbers will need to be dug up. Ask Heather Steele, and/or JP Avoac about the best way to acknowledge the TO for support. We probably want to include the Gordon and Betty Moore Foundation in the acknowledgements, too, but check this as well.

Wooo!

Also, I have pushed back my defense until March (writing about Mars is difficult!) so your participation is absolutely on the table if you’re feeling well enough to join. If not, Paul A. will likely join to represent this paper’s contribution in the proceedings.

As a side note, I have just received an offer to go work at UW Madison with Brad Singer for a postdoc. I will likely accept. I anticipate some spirited discussion with Basil Tikoff about this!

Best,

***Daven P. Quinn***

PhD candidate ◦ Caltech GPS division

<http://davenquinn.com> ◦ (704) 920 8487 *m*

On November 17, 2017 at 1:09:10 PM, g-cubed@agu.org ([g-cubed@agu.org](mailto:g-cubed@agu.org)) wrote:

Dear Dr. Quinn:  
  
Thank you for submitting your manuscript entitled "Late-Cretaceous construction of the mantle lithosphere beneath the central California coast revealed by Crystal Knob xenoliths" [Paper #2017GC007260] to G-Cubed. I have now received three reviews of your manuscript, which are included for you here below and/or located at the following link: <https://www.dropbox.com/sh/d4tgb2nsdii5kgo/AABbwzUnvZmD0JODNilYW4qFa?dl=0>.  
  
Based on these reviews and my own reading of your manuscript, I find that it may be suitable for publication after minor revisions.  
  
The generally detailed and constructive comments of the reviewers are overall consistent and should provide sufficient guidance as to how to revise your manuscript. Please make an effort to address all the comments and suggestions raised by the reviewers. All three reviewers note that your paper would benefit from being shortened! Although none of the reviewers cold offer details on how to shorten, other than shifting more material to the Appendices.   
  
Please submit a revised manuscript that responds to or addresses the comments by November 26, 2017.  
  
In your revision, please follow our [Checklist](https://publications.agu.org/author-resource-center/checklists-and-templates/" \t "_blank) and use our [Templates](https://publications.agu.org/author-resource-center/checklists-and-templates/" \t "_blank) for the main file and any supporting information. Please provide the following:  
  
1. A response to reviewer file that lists each major comment and describes how the manuscript has/has not been modified in response to those comments.  
  
2. A copy of the manuscript with the changes noted (e.g., highlighted, "track changes," italics or bold changes).  
  
3. The final revised manuscript with the changes incorporated and separate final figure files (figure parts should be combined into a single file), which will be used for publication if the manuscript is accepted. If final figures are already uploaded, they can be easily copied over to the next revision version.  
  
4. If any, supporting information text figures, captions, and small tables in a single PDF file using AGU's template. Large data tables, and multimedia should be uploaded separately in their native format (preferably .xlsx or .csv for tables).  
  
AGU requires that data needed to understand and build upon the published research be available in public repositories following [best practices](http://publications.agu.org/author-resource-center/publication-policies/data-policy/data-policy-faq/" \t "_blank). This includes an explicit statement in the Acknowledgments section on where users can access or find the data for this paper. Citations to archived data should be included in your reference list and all references, including those cited in the supplement, should be included in the main reference list. All listed references must be available to the general reader by the time of acceptance. AGU [requires](https://eos.org/agu-news/agu-opens-its-journals-to-author-identifiers" \t "_blank) the corresponding author, and encourages all authors, to register for an [ORCID](https://orcid.org/" \t "_blank).  
  
When you are ready to submit your revision, please login to your account (<https://gcubed-submit.agu.org/cgi-bin/main.plex>"Revise 2017GC007260."  
  
I look forward to receiving your revised manuscript. If you have any questions, or need additional time to complete your revisions, please contact us at g-cubed@agu.org.  
  
Yours sincerely,  
  
Janne Blichert-Toft  
Directeur de Recherche CNRS  
Laboratoire de Geologie de Lyon, CNRS UMR 5276  
Ecole Normale Superieure de Lyon  
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<http://perso.ens-lyon.fr/janne.blichert-toft/index.htm>  
  
------------IMPORTANT INFORMATION------------------------  
Additional information on text preparation, formatting, acceptable file formats, supporting information, graphics preparation, and AGU style, is [here](http://publications.agu.org/author-resource-center/" \t "_blank) AGU has recently [updated](https://publications.agu.org/agu-grammar-and-style-guide/" \t "_blank) its styles.  
  
Sharing your work is an important part of the research process, and AGU leverages and shares published research to promote the [broader importance](https://eos.org/editors-vox/earth-and-space-science-for-the-benefit-of-humanity" \t "_blank) of Earth and space science. Learn how you can [promote your paper](https://publications.agu.org/author-resource-center/promote-your-paper/" \t "_blank), including how your paper can be considered for additional publicity or for the issue cover.  
  
-------------------------------------------------------------------------------  
Reviewer #1 Evaluations:  
Recommendation (Required): Return to author for minor revisions  
Significant (Required): Yes, the science is at the forefront of the discipline.  
Supported (Required): Yes  
Referencing (Required): Yes  
Quality (Required): Yes, it is well-written, logically organized, and the figures and tables are appropriate.  
Data (Required): Yes  
Accurate Key Points (Required): Yes  
  
Reviewer #1 (Formal Review for Authors (shown to authors)): Also see attachment, found at <https://www.dropbox.com/sh/d4tgb2nsdii5kgo/AABbwzUnvZmD0JODNilYW4qFa?dl=0>.  
  
QUINN ET AL. REVIEW - A. CHAPMAN  
  
November 11, 2017  
  
This manuscript by Quinn and coauthors contributes a large amount of new petrographic, thermobarometric, geochemical, Sr and Nd isotopic, and Ar-Ar geochronologic data, from peridotite xenoliths recovered from the Crystal Knob locality (central coastal California). These new datasets, plus derivative geochemical modeling and thermal modeling of the central California lithosphere, are aimed at deciphering 1) the petrogenesis of Crystal Knob xenoliths and host basaltic lavas and 2) evaluating three models for the Late Cretaceous to Quaternary evolution of the lithosphere beneath central California. The authors of this paper make compelling arguments that Crystal Knob xenoliths likely represent fragments of sub-oceanic (Farallon) mantle lithosphere imbricated beneath the margin in Late Cretaceous time and reheated during passage of the Neogene slab window. Strong arguments are also made against xenolith origins as fragments of mantle derived from beneath the remnant Monterey microplate or new material added during passage of the slab window. The integration of new petrologic data, new modeling, and existing seismic data in this effort is very thorough and the story derived from wrangling these datasets is very well articulated. For those achievements, in addition to the paper being beautifully illustrated, the authors are commended. The authors further argue, based on existing seismic data plus the absence of xenolith evidence for sub-Monterey plate lithosphere beneath central coastal California, that the Monterey slab does not continue a great distance inland and likely has nothing to do with the Isabella high velocity anomaly (more likely a foundering blob of Sierra Nevada batholith root material) beneath the southern San Joaquin basin. This point is important, as the Monterey microplate vs. drip debate is an active and controversial one. I'd think highlighting this point in the abstract and also as a "key point" would elevate the reach of this paper. This is up to you, if you want Daven  
  
For the above reasons, I think that this effort will be of great interest to the readers of g-cubed. I have made a number of specific comments in the uploaded pdf file. The authors will find that most of the comments involve minor phrasing issues or points requiring clarification. More generally, as the authors revise this effort, I'd encourage them to reduce the paper in size (I've included some thoughts on what could be cut - perhaps some of the figures [there are 22!] could be migrated to the supplementary material), as it has a few dense and intimidating portions. Also, section 5.2, which explores the issue of the 20 myr (!) time lag between opening of the slab window and eruption of ca. 2 Ma basalts, is less developed than other sections. I read it several times and I'm still unclear how the authors want to explain the time lag (somehow slip along the San Gregorio-Hosgri fault either induced melting or facilitated upward melt migration?). One thought I had was - perhaps basaltic eruption and a profound ca. 2 Ma uptick in denudation rate (e.g. Ducea et al., 2003) may be related? Yes good point that I meant to include! Let’s insert this at line 1297:

….early Neogene slab window. Delete: due to extension in the mantle lithosphere, and continue with: Rapid uplift of the central California coastal region at ca. 2 Ma (Ducea et al., 2003) is likely to have contributed to such mobilization as well.  
  
I sincerely enjoyed reading this paper and I think that, with a minor amount of revision, it will serve as a quality contribution to our understanding of how continental margins are perturbed by changes in slab dip and subduction of spreading ridges. Following revision, this paper will be an excellent contribution to g-cubed.  
  
  
-------------------------------------------------------------------------------  
  
Reviewer #2 Evaluations:  
Recommendation (Required): Return to author for minor revisions  
Significant (Required): Yes, the paper is a significant contribution and worthy of prompt publication.  
Supported (Required): Yes  
Referencing (Required): Yes  
Quality (Required): Yes, it is well-written, logically organized, and the figures and tables are appropriate.  
Data (Required): Yes  
Accurate Key Points (Required): Yes  
  
Reviewer #2 (Formal Review for Authors (shown to authors)): Also see attachment found at <https://www.dropbox.com/sh/d4tgb2nsdii5kgo/AABbwzUnvZmD0JODNilYW4qFa?dl=0>.  
  
Comments on Quinn et al., 2017 (by Elisabeth Nadin)  
  
This is a manuscript that contains interesting new information on xenoliths from a coastal California basaltic center. The analyses and modeling are compelling by themselves, and simple explanations of 3 possible scenarios for the xenolith origins would have satisfied me. The authors devote about half the manuscript to detailing the geological contexts of two of these scenarios. I think that this effort contributes to a "literature trail" that reinforces their preferred tectonic scenario for the region, and I wouldn't want to push them to remove it, but on the other hand I find it a bit exhausting to read through and somewhat distracting to the analytical results. I tend to prefer offering readers the scientific results and a more cursory overview of their potential implications in order to not OVERTELL a story, so it may just be a personal preference.  
  
I think that this manuscript should be published with minimal rearrangement and some editing. I have added edits and comments to the manuscript and summarize some of the bigger points here.  
  
Abstract:  
In lines 5-6, xenolith studies are said to link crustal structure, but I found this confusing because they are supposed to be mantle xenoliths. Clarify. X  
In line 8-9, the xenoliths are reported to be underplated sub-oceanic mantle, but I was left wondering here (as well as later) what happened to the intervening oceanic crust.X  
In the final line, "Neogene ridge subduction" is invoked, and I wondered if the Neogene ridge has a name, or if the oceanic segments on either side are named (Farallon-Pacific? Juan de Fuca-Pacific?). X  
  
Key points:  
I would just say "The 1.65 Ma Crystal Knob volcanic pipe hosts xenoliths from the deep mantle lithosphere"X  
  
Intro  
Lines 3, 14: the "crustal column" is confusing - is this a contribution of the submitted manuscript, or reported elsewhere? Specify or cite a reference. These are generic statements that do not need references. Asking for references in these specific applications would be analogous to asking a stratigrapher to provide references for use of terms like “stratigraphic columns” when introducing their work on a section of strata.  
  
Line 17: citing only Ducea et al 2015 suggests this work is the only one to say that the Farallon plate subducted eastward beneath western North America. Use "e.g." or list more refs. OK  
  
Line 22: I don't think the quotes around Salinian terrane or Salinia are necessary We strongly disagree. Salinia has traditionally been used for a mantle lithosphere rooted semi-intact displaced terrane, and many researchers not current with the most pertinent literature still envision it as such. We are studying mantle lithosphere xenoliths here. In terms of expected compositions and P-T history an intact batholithic terrane (the traditional view of Salinia) would root into a fundamentally different mantle lithosphere than a long-lived subduction accretion assemblage (the Franciscan, including its recently recognized roof nappes of Salinia arc rocks).

Line 36 (and elsehere): pay attention to the order of the references DQ  
  
Lines 86: Also cite Ague and Brimhall for gradient in the SNB optional, not necessary  
  
Lines 81-83 and 94-97: The contrast is not apparent as phrased. Try, "In addition to Cretaceous mantle wedge, eastern margin xenoliths include ..... " Lines 98 to 102, plus references given in lines 97-98 and 102 adequately

Line 116: provide a reference for upper-mantle convection driving Owens Valley rifting, or specify "we presume"

Le Pourhiet, L., Gurnis, M., and Saleeby, J., 2006, Mantle instability beneath the Sierra Nevada Mountains in California and Death Valley extension: Earth and Planet. Sci. Lett., doi:10.1016/j.epsl.2006.08.028.

, and Jones et al., 2014, already in references  
  
Line 131: explain why the dunites are cumulate - is this a textural determination? And how do they "grade"? is it like there are layers of basalt with large cumulate xenoliths, and then layers with smaller ones, and then layers with small chunks, and then layers with single crystals? Something like: The dunites have textures and Mg#’s typical of cumulates, and are observed to diminish in size to single grain xenocrysts, or apparent phenocrysts.  
  
Line 137: Unless I am missing something, 1 basalt and 2 dunites is not "several". Just list how many samples of each you analyzed, or delete "several". Delete “several”  
  
Lines 160-161: I think this could be made clearer by saying that you prepared thin sections of two dunite cumulate samples hosted in basalt X  
  
Lines 173-179: Is there a figure for this? Or results (only results) in a table format? If you already have this, which table is it? Fig. 5?  
  
Line 178: I have no problem with discarding, but as a reader I prefer to know which samples you KEPT and why you decided to keep them. Not worth increasing length of manuscript over this, particularly with Taylor (1988) reference  
  
Line 185: "section" - is this a thin section? ~5 cm xeno in thin section seems quite large delete “within the section”  
  
Line 196: what does "between fragments" mean here? Delete "between fragments”  
  
197: within a dunite inclusion? …dunite inclusions encase…  
  
202: are the microphenocrysts of plag here different from the plag lathes in line 187? X  
  
204: "measurements" here refers to more cpx grains? X  
  
208: relatively what? X  
  
210: why the quotes on phenocrysts? X  
  
222: can you rule out strain after formation? How about: …..in most cases indicates little to no srain.  
  
229: what mineral is the intergranular fill? DQ  
  
237: determined how? "as indicated by .... " X  
  
260: "shows" or IS? ??  
  
269-270: try to avoid using the same word >1x in a sentence. ??  
  
273: it would be nice to know the total # of points - maybe include this column in Table 6? DQ  
  
---ORGANIZATIONAL NOTE---- My preference would be to read sections 2.4.2 and 2.4,3 before 2.4.1. I like the progression from larger to smaller scale observations X  
  
317-318: I'd prefer to read the interpretation after the data/results X  
  
320-322: why would granite have the same Sr/Sr as basalt or any kind of mafic xemolith? Coleman and Galzner (1998?) argue that eastern Sierra granites attained LIL enrichments and evolved Sr from enriched underlying sub-continental mantle lithosphere. Our text is very pertinent!  
  
337: where does the reader find these averages? (table 7? Figs 10, 11?) DQ  
  
Section 2.6.1: I feel that the T and P determinations need to come first so the reader knows why you fed certain variables into pMELTS. OR explain the utility of the generic model first. DQ  
  
351; 353: depletion and re-enrichment in what? Covered in previous paragraph!  
  
354: what does it mean to be "depleted in whole-rock composition"? this is confusing: the cpx is depleted in REEs; what is the WR depleted in? X  
  
371: I am not sure what the utility of the "model composition" is. Modal compositions are fundamental to petrology!  
  
375: remind the reader what rock type CK-2 is DQ  
  
386: maybe specify that CK-2 follows the trend of modelled depleted peridotite DQ  
  
432: "we show the full range in results of all thermometers in Table 9 and Figure 13. X  
  
487-498: this needs to be stated at the first mention of Fig. 8. It may mean rearranging figures and/or results. X  
  
515: typo? ?  
  
516: typo ? ?  
  
----General Note ---- At some point in 2.8.1 or 2.8.2 you need to list the T results  
  
548-549: I am not sure what this means. X  
  
563: a "depth deep within" – awkward change to: ….sourced relatively deep within the spinel stability field…  
  
609: awk phrasing change to: To characterize the variability….  
  
635: whose preference? Change to: This is further suggested by depth estimates of the ….  
  
636: is there a reference for regional heat flow? DQ  
  
691: what is "it" here? X

725: what do you consider "small"? insert a number (or range) here. Change to: … depletion of this depth range….  
  
727: awk; implies "temperature degrees" change degrees to temperatures  
  
749: again, would be nice to know what "the crustal column" IS I suggest a couple of minor changes here: ……that lie shingled above the Franciscan accretionary complex which constitutes the regional crustal section in the region.  
  
753: awk; I had to read several times to understand this mix of words Maybe change to:…., with the mantle lithosphere of the region representing underplated Farallon plate mantle lithosphere.  
  
754: choose "alternatively" or "another" X  
  
755: can you provide a time/age range for establishment of SAF system? ….system in the late Cenozoic  
  
768: awk X  
  
769: what is the heat flow in the Central Valley and Sierra Nevada? X  
  
780: merge this graf with the one before OK  
  
794: note a recent paper by Leutkemeyer et al (2016; Tectonophysics) shows fluid transport along the SAF fracture network is downward from the surface, which may actually be cooler ... DQ, this may be a big nothing, but maybe check. If the downward fluid transport referenced is only near surface then it is non-applicable to our problem.  
  
800-802: redundant X  
  
803: I'd like to see you commit to what you think an acceptable geothermal gradient for the region is X  
  
804: the features described in 3.2-3.5 do indeed affect the thermal structure, and in Sect. 4 you finally get back to that, but here I would suggest that you will be outlining several tectonic features that affect the potential mantle lithosphere origins for the xenoliths. Done in some detail below, don’t need to add more text!  
  
807: perhaps specify the location (southern CA) for non-CA readers X  
  
829: different authors, or different spellings? Add accent mark to Trehu  
  
860-866: this is a level of detail that I think detracts from the main message(s) of the paper. This passage is crucial to the understanding of the origin, and tectonic setting of he Monterey plate, and therefore needs to be in the text.   
  
874: can you at least give a short prelude as to why? There's no way I will remember this part until sect. 5.2 We prefer not to add unnecessary text.  
  
------General Note on Sect. 3.4 ------ It takes a really long time to figure out what is the main message of this section. Find a way to state it near the top, and try to trim this section to ~ 2x the length of 3.3 This is stated clearly in the first sentence (line8 76)  
  
918: what is the "Nacimiento Franciscan"? (typo?) maybe change to ….beneath the adjacent Franciscan complex.  
  
919: rephrase for clarity: These observations all suggest that a lower-crustal mafic layer must be structurally discontinuous/disrupted ...X  
  
928: join this graf to the former. DQ  
  
943-959: I feel that this whole part can be deleted because it distracts from the main message of the paper.

We disagree! Extraordinary claims have been made in the literature about a large dipping mass (slab) having been translated horizontally through the mantle as a “dangling slab” by edge forces propagated across the Pacific-Monterey intra-oceanic rift, Hosgri dextral fault, Rinconada dextral fault and San Andreas dextral fault, with the translated slab now represented by the Isabella mantle anomaly. This extraordinary claim has been made with no reference to related surface deformation, because there is none other than that of the California Borderland and Transverse Ranges immediately proximal to the cite of Monterey plate genesis (Fig. 19 compared to Fig. 1). Meanwhile there is a massive body of geomorphic, structural, stratigraphic, volcanological, and numerical modeling data that closely link the Isabella anomaly to proximal surface deformation arising from underlying mantle lithosphere convective mobilization. We do not want to perpetuate the “blind spot” analysis that has been used in the literature to argue for the “dangling slab” model for the Isabella anomaly.

933-942 are enough to reject the "dangling slab" as a mechanism for bringing mantle lithosphere to the xenolith source region. X  
  
955-56: which mechanism? X

***DAVEN***, *Line 992 should start “Figure 21”, not Figure 19, and line 1006 should start Figure 21 c and d, not 19c and d, right?*  
1014: I am a bit confused by the reference to Fig. 19 (confusion because of Figure 19/21 error as noted above?) here, since nothing on that fig is 40-50 Ma (reviewer missed that we are referring to m.y., not Ma)

also, I don't think that the Burgmann & Dresen (2008) ref works here because it's a review paper. The 700-800˚ B-P transition in olivine that you mention is cited in B&D as coming from Warren & Hirth (which you cite) but also from Dijkstra et al. (2002). It is merely summarized in B&D. DQ  
  
1028: at the end of 3.5 I would love to hear you just come out and say "We favor mantle duplexing, as shown in Figure 19D, as the source of mantle xenoliths in the Crystal Knob site." DQ  
  
1103: list older age first OK!  
  
1136: typo? DQ  
  
1156: typo? DQ  
  
1194 + 1195: is "it" here "the model results"? on both lines just replace “it” with “the results”  
  
1205: how far is far? Can you give a km range? Change to: extending at 100 km-length scales to the east….  
  
1218-1219: I would rephrase this to explain why you can't rule out mantle slab. Something like "Despite the inability of our thermal modelling to rule out a stalled Monterey plate, we conclude based on geological and geophysical reasoning that this is an untenable scenario." (or something) DQ

***DAVEN, there are mistakes with Figure numbers on lines 1221, 1222, 1228, and 1230!***  
  
1279: like Fig. 18c? citing this figure does not seem relevant here. Figure miss-numbered here again  
  
Conclusion: Please mention the age of the Crystal Knob basalt again, and suggest an age for the xenoliths. X  
  
1324: I would re-list the MOST relevant X  
  
1339: mention asthenosphere upwelling at x km depth somewhere in here again. X  
  
1341: at what age/time? DQ  
  
Comments on Figures are made directly in the manuscript.  
  
  
Some general issues to look out for:  
Figures seem to numbered out of sequence ***Yes, and miss-numbered in some places***  
Tables (esp Table 1) could be referenced more  
References are sometimes not ordered by date  
  
The dunite-basalt relationship is not abundantly clear. Be specific, especially on p. 5 near the middle (lines 160-161)  
  
  
-------------------------------------------------------------------------------  
  
Reviewer #3 Evaluations:  
Recommendation (Required): Return to author for minor revisions  
Significant (Required): Yes, the paper is a significant contribution and worthy of prompt publication.  
Supported (Required): Yes  
Referencing (Required): Yes  
Quality (Required): Yes, it is well-written, logically organized, and the figures and tables are appropriate.  
Data (Required): Yes  
Accurate Key Points (Required): Yes  
  
Reviewer #3 (Formal Review for Authors (shown to authors)):  
  
The manuscript by Quinn et al., explores the geochemistry of a suite of xenoliths erupted through the Franciscan belt in central California and investigates a series of 1D thermal models in order to evaluate possible tectonic scenarios for the upper mantle that were previously proposed for the region. These xenoliths have not previously been investigated and the analysis carried out here will be of significant interest to workers interested in western U.S. tectonics. The mantle duplex idea explored here will be of interest to a wider community. The methods and conclusions seem reasonable to this (non-geochemist) reader. The language is clear (some minor issues pointed out below), but the document seems longer than necessary. I do not have any specific recommendations on how to shorten it, but I encourage the authors to condense it for the sake of future readers. One general comment is that interpretations have been mixed in with methods and descriptions throughout the text in a way that sort of works but is also a little unconventional-I'm not sure I agree that it was necessary to dispense with the traditional sequence/existence of sections dedicated to methods-> observations-> interpretations. The existing format helped in reducing the overall text length by not having to review various items multiple times in order to pose our various data acquisition pursuits as well as our interpretations.   
  
Overall this is an important new contribution deserving of publication in G^3.  
  
line 93 - new paragraph OK as is  
  
179- figure 5 should be mentioned here i think OK  
  
192 "thin section" instead of "sample?" DQ  
  
various places, e.g. 265: distinguishing them as "dunites" vs "peridotites" is problematic since the term peridotite encompasses dunite. Is there a better word choice to replace "peridotite?" Nonsense!  
  
194. "textures similar to those in samples CK-2 to CK-7". Have we been introduced to these samples? Don't expect reader to memorize the sample numbers. Maybe cite Table 1.  
  
201-202. is "microphenocryst" a word? hyphenate? Has this guy ever taken a course in petrography?  
  
208-209. Some consideration of diffusion rates in olivine could be useful here to put a more quantitative timescale on this. Also a word ("recently?") is missing from the sentence. ..relatively late, (?)  
  
213-215. interpretation is mixed up here with descripition. **X,** although the sentence could read better, how about:…..entrained peridotites. We suspect that the dunite inclusions represent cumulates that formed and then were entrained as multiple drafts of magma ascended up the eruptive conduit. OK??  
  
218. allotriomirphic is considered an obsolete synonym of "xenomorphic" (glossary of geology). use xenomorphic instead. Double check this Daven!  
  
226-229. "alteration channel" or "melt channel"? DQ  
  
233. "intergrown" sounds like a genetic interpretation. also i can't visualize it. probably there are lots of arrangements that could be categorized as intergrown. Since it is seen in all the samples, a figure or reference to previous literature describing this would help. DQ  
  
262-264, and 237-238(X). these interpretative statements need more support (at least they are not self evident to this non-petrologist reader). Cite a reference where similar observations and conclusions have been made, or support the arguments more. For lines 262-264 I leave it up to you.   
  
296. what is "net of possible refertilization?" DQ, this could be stated more clearly  
  
340 add "in olivine" after "compatible" OK  
  
371 "a" not "an" OK  
  
530. By "perform well" i think it means that they give similar temperatures, but since they are potentially measuring different times (e.g. the possible fossil high T event detected only with the REE mentioned in next paragraph) it's not actually expected that they should give the same numbers, so one can't really judge it as a good or bad performance. A different word/phrase should be used here. Maybe cut this sentence since it is the topic of the next section anyway. DQ  
  
REE temperatures should be given in table form? (are they?) DQ  
  
531-533. reference to a table or figure would be useful here yes  
  
538. how much higher? put quantitative values in parentheses DQ  
  
548. evidence of a melt channel is not apparent to me in figure 5. this feature should be described in the figure legend or pointed out in the figure Fair point. I see the alteration channel in Figure 5 (CK-4). Is this the “melt channel”. Could simply clarify this in the figure caption.  
  
637-639 Not a very motivating introduction to this section. lots of things "can be" done. what's the point of this thermal modeling? perhaps add something like "to provide an additional constraint on xenoliths extraction depth" or "to investigate possible tectonic scenarios..." X  
  
782 brady ref? X  
  
783. unclear how regional heat diffusion associated with possible fluid circulation on the San Andreas would cause Coast range heat flow estimates to overshoot X (we say San Andreas transform system, implying SAF, Rinconada, Hosgri, and other small splays, which would spatially disperse the effect)  
  
788-804. highly speculative Maybe in line 797 replace demonstrates with suggests (?)  
  
897-908. This paragraph is hard for me to follow, starting with identifying the location of the "narrow slab window shown along the eastern edge of the partially subducted plate" (line 899). After reading it 5 times I think this is referring to the narrow "slab window" area west of the peninsula ranges batholith, better to describe it as such. DQ, it seems pretty clear to me as worded!  
  
919-932. There is some repeated information here that needs some editing. Some citation to the papers putting forth the conflicting notion should be given at lines 919-921. (I think these are on the next sentence/paragraph, so maybe this would be resolved by editing this section). Maybe continue line 927 to 928 as same paragraph, and make the following edits:

……shown to be steeply oriented (Dietz and Ellsworth, 1990; Brocher et al., 1999; Yan et al., 2005; Tituset al., 2007; Yan and Clayton, 2007; Ozacar and Zandt, 2009). Furthermore, this proposal cites a continuous mafic lower crustal layer from the offshore region through the Coast Ranges as evidence of a structurally continuous Monterey plate at depth, which is inconsistent with seismic data cited above. A corollary to this proposal speculates that the Monterey……  
  
1011. "accelerated rollback is driven by the formation of Farallon-plate mantle duplexes." What "drives" what here is pretty speculative and unsupported by arguments. Focus on establishing that there IS a duplex or that it makes sense before going into the dynamics. Also I'm not sure it's really "rollback" in the sense most think of... such duplexing would be more of a shallow mantle "pile-up" phenomena, perhaps involving a break in what is generally thought of as subduction (where dense material is carried down).  
  
1012-1015. Remove these sentences about the brittle ductile transition. This occurs in all subducting stabs, so there's no argument presented explaining how this could explain the formation of duplexes here but not elsewhere.  
  
1015-1019. I doubt that coseismic transients along faults are relevant to the presumably longer-term stress conditions within a slab that might lead to formation of a duplex.  
  
1020-1023. confusing. if they are lacking as xenoliths then why are thought to be "in this region?" Because widely cited seismic data referenced above documents such rocks to be widespread in the lower crust of the region.  
1025 spelling error "suspect" DQ  
  
A big question here that isn't really addressed is why these duplexes would form. Duplexes are typical of compressional situations, not extensional environments such as those occurring in the crust at this time. Not so, duplexes are common in extensional environments, use Gabrielsen and Clausen (2001 GSA Mem 193) to break into literature, although our application is different from the cited paper here. Here's a possibility: For several million years (~5 m.y. according to figure 19) following flat subduction of the overthickened lithosphere, duplexing resulted from slowing of the subducting plateau's trajectory to the east and resistance to restarting steeper subduction (to initiating a new path for the slab downward) all the while maintaining the convergence rate dictated by ~~PAC~~FAR-NAM plate motion. These boundary conditions a created situation where the slab underwent shortening-this compressional event in the mantle lithosphere would likely cause uplift and further destabilization of the overlying crust. Could this even be the MAIN cause of late Cretaceous upper crustal extension in the Salinia- Mojave region. This was to be model B, in my mind, but I did not want to lengthen an already long manuscript. Let’s see if we can fit it in. First let’s delete the not totally necessary sentence on megathrust temperatures to give us a little room (lines 1001-1005). ***Here is new text to insert starting line 1001:***

***….***mantle wedge. Two viable alternatives for the underplating of Farallon plate mantle nappes within this tectonic setting include: 1. Increased shear stress resulting from the initial buoyancy-related resistance to oceanic plateau subduction promoted transient imbrication of Farallon plate mantle lithosphere followed immediately by the establishment of a new basal megathrust surface. The stacking of the mantle duplex drove rapid elevation increase in the upper plate, which shortly thereafter collapsed gravitationally promoting upper plate extension (not detailed in sequence in Fig. 21). And 2. negative buoyancy-driven focused rollback of the slab trailing the subducted oceanic plateau led directly to mantle lithosphere duplex formation as well as upper plate extension (detailed below and in Fig. 21).

In Figure 21c and d we adopt…..***(back to line 1006 with edits below)***

***1006:*** In Figure 21c and d…

***1011:*** change “driven” to “accomplished”, difference being that the rollback process drives duplex formation

***1019: add text:*** …..potential role of tensile stresses within subducting slabs in nucleating structures that could lead to nappe detachment and underplating.

1066. Need a reference and definition of the variables in the equation. DQ  
  
1076-1077. These are more like "initial parameters" or "evolving boundary conditions" than things that "track the evolution". The finite difference model code tracks the evolution. DQ  
  
1079-1080. More information on the model should be given here and less given in an appendix. X  
  
1145-1146. Clarify: Why does scenario C incorporate these constraints but B doesn't? These same rocks were there at the same time in B, so shouldn't B be similarly constrained by that data? DQ  
  
1193-1195. Informal language, change two instances of "it" to "modeled geotherm" DQ  
  
1229. missing "the" DQ  
  
1321-1322. if the effects of exhumation are important enough to include in the conclusion, they shouldn't be relegated to the supplement. Either cut here, or integrate that into the paper. Already done in lines 1191-1195

Figure 5. The "coregistering" process needs to be better explained. Is it an automated procedure? A methods section would be a good place for this. DQ