Groundwater contributions to baseflow in Sierra Nevada Rivers: Processes, flow paths and residence times

Martha H Conklin

Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0203: Groundwater contributions to baseflow in Sierra Nevada Rivers: Processes, flow paths and residence times

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The technical reviewers generally felt that the goals, objectives and hypotheses are clear and consistent, and the work is timely. There is, however, concern that the conceptual model is weak because is it does not provide an adequate link between the surface water, groundwater and atmospheric factors. The approach appears to be relatively straight−forward and has been used successfully by the authors in other systems. The reviewers suggested, however, that this approach has not been documented form mountain block drainage systems, and the details are not sufficiently documented to allow one to evaluate whether it is feasible for the basins that are to be studied. The approach to monitoring and data collection appears to be sound. Publication of peer−reviewed scientific papers will be valuable, but a larger data management system would improve the usefulness of the work. One reviewer expressed concern that the interpretive phases of the project were not explained in sufficient detail. The authors appear to be highly qualified and capable of successfully carrying out the work. The reviewers expressed concern that the budget may be high when compared to the potential outcomes of the work. The reviewers generally felt that the work would provide valuable results, but, because of the apparent weakness of the conceptual model, they suggest

that the goals and objectives may not be achievable.

Additional Comments:

The technical reviewers generally felt that the goals, objectives and hypotheses are clear and consistent, and the work is timely. There is, however, concern that the conceptual model is weak because is it does not provide an adequate link between the surface water, groundwater and atmospheric factors. The approach appears to be relatively straight−forward and has been used successfully by the authors in other systems. The reviewers suggested, however, that this approach has not been documented form mountain block drainage systems, and the details are not sufficiently documented to allow one to evaluate whether it is feasible for the basins that are to be studied. The approach to monitoring and data collection appears to be sound. Publication of peer−reviewed scientific papers will be valuable, but a larger data management system would improve the usefulness of the work. One reviewer expressed concern that the interpretive phases of the project were not explained in sufficient detail. The authors appear to be highly qualified and capable of successfully carrying out the work. The reviewers expressed concern that the budget may be high when compared to the potential outcomes of the work. The reviewers generally felt that the work would provide valuable results, but, because of the apparent weakness of the conceptual model, they suggest that the goals and objectives may not be achievable.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The research team has the capabilities to perform the proposed research and the proposed research could produce valuable results. The proposed study also has well defined goals, objectives and hypotheses that address important topics. However, for the proposed research, some of these goals may be unattainable, and the panel had some significant concerns. These included concerns regarding the conceptual model, which

does not adequately account for linkages between surface water, groundwater and atmospheric factors. In addition, as suggested by the reviewers, the modeling approach has not been documented for mountain block drainage systems, and insufficient details are provided in the proposal to allow an evaluation of the approach's feasibility in the basins that are to be studied. There also was no discussion of distribution of the data outside of peer reviewed journal articles, and such distribution would increase the value of the proposed research.

Ranking: Adequate

Technical Review #1

proposal title: Groundwater contributions to baseflow in Sierra Nevada Rivers: Processes, flow paths and residence times

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The proposal is to perform research on processes in the Sierra Nevada linking precipitation (snowmelt and rainfall) to river base flow, to develop tools for estimating residence times and flow paths linking precipitation and base flow, and to estimate groundwater contributions to the flow of representative rivers with headwaters in the Sierras. Results from this work would improve hydrologists' ability to evaluate the effects of long term (for example, climatic) changes in the Sierras on river flow. The hypotheses stated are certainly relevant to these goals but the proposal does not clearly describe how the tasks outlined would test the hypotheses.
Rating	very good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	Certainly present knowledge relating climate to river flow could be improved, so the proposal's goals are laudable. However the proposal gives only sketchy

	descriptions of how data to be collected would be interpreted to achieve these goals, and does not give the reviewer much confidence that the goals would be achieved.
Rating	fair

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The isotope tracer and water chemical data to be collected will be interpreted as groundwater ages, and by using two-component mixing models and the statistical technique of end-member mixing analysis (EMMA). The success of some of the isotopic dating techniques have been demonstrated elsewhere although successful applications of some of the proposed tracers are less common. The application of two-component mixing models to hydrograph separation is well known. One of the co-investigators (Liu) has successfully applied the EMMA method to determine source waters and flow paths contributing to stream flow in an alpine basin. Although the general hypotheses linking the several possible components of recharge to flow are described, the lack of a more quantitative description of the interpretative approach limits the extent to which it can be evaluated.
Rating	fair

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The approach is not fully documented and so it is possible to evaluate the feasibility of only parts of the project. Data collection should be routine and the collection program readily achievable. Interpretation of groundwater residence times from some of the tracers proposed is also routine, though the successful application of others less certain. Two−component hydrograph separation using isotopes and dissolved concentrations is also routine. Applying the EMMA technique requires judgement and may not be successful. In addition, the contribution of modeling to the interpretation of results of geoundwater dating and EMMA are not discussed in sufficient detail for evaluation. The co−investigators appear to be capable of carrying out the field and interpretive phases of the project, to the extent that they are described, and the sub−contracting laboratories capable of the analytical work required.**
Rating	**fair**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Discharge measurement and collection sites for snow, rain, soil, ground and surface water will be established and maintained for the two−year life of the project. The techniques are not described but should be routine and so adequate for project purposes.**
Rating	**very good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The data to be collected may of themselves be of interest, but the truly valuable product would be a quantitative understanding of the water and solute balances of the sources of stream flow in the Sierras. The proposal does not describe the interpretive phases of the project in sufficient detail to provide assurance that such a quantitative understanding will be reached.**
Rating	**fair**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The technical qualifications of the co-investigators and subcontractors and their institutions appear capable of supporting their tasks insofar as the tasks are described in detail. There is no evidence that the Lead Investigator has previously managed a project of this magnitude, but this has not affected this evaluation of the proposal. The responsibilities of two of the subcontractors (Flint and Flint) are not described in sufficient detail to evaluate their qualifications, while the responsibility of a third (Fogg) is not described at all.**
Rating	**good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**Of the overall budget of $1.3 M, about half is poorly supported in the tasks described. About 60% of Task 2 ($300 K) is for isotope and gas analyses the utility of which in problems of this sort have not been convincingly demonstrated. All of Task 5 ($400 K) are costs for two subcontractors (Flint and Flint) whose contributions to the project are described in too little detail to evaluate such costs. No costs at all are associated with a third subcontractor (Fogg).**
Rating	**poor**

Overall

Provide a brief explanation of your summary rating.

Comments	**The proposal overall can be considered at best only fair. The goals are stated would be valuable but the proposal does not argue convincingly that they are achievable. Furthermore, about half of the requested budget is not adequately supported in the proposal. Should the proposal be of interest in spite of these deficiencies it is strongly recommended that it be revised to address the shortcomings noted in these comments before being considered further.**
Rating	**fair**

Technical Review #2

proposal title: Groundwater contributions to baseflow in Sierra Nevada Rivers: Processes, flow paths and residence times

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**Objectives of the project is clearly stated. Idea is timely and important. However, the hypotheses do not include the system analysis linking processes of snowmelt, rainfall, surface water and groundwater. The hypotheses do not explicitly recognize that atmospheric, surface and subsurface portions are three dynamically linked water reservoirs. Uncertainty is treated only via mathematics, although it is well known fact that the physical meaning not always can be interpreted via math only. In addition, there is no recognition of system organization of river basins. In system analysis numbers and boundaries create scientific logic.**
Rating	**good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The study is definitely justified; however the study main focus is age dating of water and use of tracers. The tracer’s method may not provide a unique solution and need to be compared with system analysis approach. The study is very fragmented and includes several**

	hypotheses. The quantification of parameters from the site specific to regional or stream basin is not clearly articulated.
Rating	fair

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The approach is completely dependent on tracer's analysis. The baseflow measurements do not provide unique solution and not easily transferable to watershed and basin. The approach could be dramatically improved if the system analysis and the pattern recognition of water balance components could be incorporated into the project. For example, the recharge/discharge rates as constant could be determined based on pattern recognition and association with geomorphologic, geologic, and ecohydrologic organizations. Information in this project will be definitely useful, but I do not see any novel ideas. The approach is traditional mix of lab, field and modeling.
Rating	fair

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	Yes, it is fully documented and technically feasible. As proposed by authors the project is likely to be successful and consistent with objectives. The scale issue is nor fully addressed in the proposal.

Rating	**good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Monitoring information is the part of the project and will be developed as information from the result of the project.**
Rating	**good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The products even in the form proposed will be valuable. However, they could be even more useful and defensible if the system analysis and regional approach could be incorporated.**
Rating	**good**

Additional Comments

Comments	**The main concern, as stated above, is that the project is mostly dependent on age dating methods. There is a need for shift in thinking, that it is possible to quantify water balance components via coupled topographic, drainage network, geologic and ecohydrologic organization. The project uses traditional methods and from the surface it appears that it is designed as Ph.D. thesis.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The authors are very highly qualified professionals; the problem is they thinking so to speak is "spinning in the scientific eddy" and lack new thinking.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	Budget is very high if one compare with the product. There is no map that can show a qunatitative analysis at a regional and local scales.
Rating	fair

Overall

Provide a brief explanation of your summary rating.

Comments	The rating is based that authors do not address uncertainty of results, do not use innovative thinking, do not provide quantitative analysis at regional and local levels, and do not address the problem of non-stationarity. The project do not demonstrate the understanding of surface, subsurface and ecohydrological organization. The water balance components in this organization are linked by probabilistic features whose basic characteristics remain unchanged regardless of scale, geology, or climate.
Rating	fair

Technical Review #3

proposal title: Groundwater contributions to baseflow in Sierra Nevada Rivers: Processes, flow paths and residence times

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	Yes, the objectives are clearly stated and realistic. The proposed research is considered to be an essential and timely step toward better understanding of the water resources of the region.
Rating	very good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	Much of the research to date has focused on understanding of processes in the lower river. This initiative proposes to look in more detail at the upper drainage networks that have not been extensively studied. The conceptual model is acceptable, but is a little weak in explaining how the stream−oriented hydrograph separation work will be linked to the shallow to deep groundwater sampling, and how these activities together will be used to establish and quantify flow paths. The conceptual model is fine for a small river system but needs refinement to fully explain potential range of conditions (e.g. interplay between tibutary and source−water mixing/evolution

	effects) as water evolves along a large river system.
Rating	fair

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The approach is straight-forward and feasible. Results will certainly assist in understanding the age spectrum and flow paths associated with mountain block drainage systems. This is a novel contribution. I am sure that this study will contribute to refinement of conceptual models for understanding the isotopic and geochemical evolution along large rivers. Moreover, this will empower decision makers with better idea of the vulnerability of the water resources to human impacts and natural variability.
Rating	good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The basic tools and approach have been extensively documented elsewhere, but have never been applied with such detail to examine mountain block drainage systems. The research will undoubtedly improve understanding of the upper Sacremento and San Joaquin river basins. The degree of success of the study in the eyes of the national/international scientific

	community may be the degree to which the approach is transferable to a wider range of river systems. This is certainly within the grasp of the authors.
Rating	good

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	The planned monitoring strategy is likely adequate to achieve the stated objectives. Specific details on some aspects of the monitoring approach (time/frequency of sampling during snowmelt period, sampling methods (e.g. grab, depth− or width−integrated, weighted according to discharge flux etc.) are somewhat lacking and so are difficult to assess.
Rating	fair

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	Yes. Publication of a significant number of peer reviewed scientific papers is likely to be of immense value to scientists, water managers and planners. As far as I see, no mention is made of larger data management systems. Interpretable outcomes will be significant.
Rating	good

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**Performance is well above average within the science community. The project team is capable and worthy of implementing the proposed project. Infrastructure and other aspects of support are accessible to these organizations.**
Rating	**good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The proposed budget is judged to be reasonable and adequate, given the substantial in-kind support available within the organizations involved.**
Rating	**good**

Overall

Provide a brief explanation of your summary rating.

Comments	**The proposed research plan is considered to be a good investment that will yield important outcomes that may contribute significantly to water resources management activities in the region. A strengthened, scale-dependent conceptual model will likely be an important early target for the study team.**
Rating	**good**