RESOURCE DEVELOPMENT

Peer Review Report

Prepared by the Office of the Geothermal Technologies Program

[Actual Publication Date]

U.S. Department of Energy
Energy Efficiency and Renewable Energy
Bringing you a prosperous future where energy is clean, abundant, reliable, and affordable

[Page Intentionally Left Blank]

RESOURCE DEVELOPMENT

Peer Review Report

Prepared by the Office of the Geothermal Technologies Program

[Actual Publication Date]

Leland (Roy) Mink, Program Manager
Raymond Fortuna, Technology Manager

[Page Intentionally Left Blank]

Table of Contents

Appendices

[Page Intentionally Left Blank]

Geothermal Program Overview

The mission of the U.S. Department of Energy (DOE) Geothermal Technologies Program is to work in partnership with U.S. industry to establish geothermal energy as an economically competitive contributor to the U.S. energy supply. The Geothermal Program goal is to improve technology performance and reduce market entry costs of geothermal energy to competitive levels, thereby making the large geothermal resource available to the Nation.

To achieve this goal, the Geothermal Technologies Program concentrates its efforts on two key initiatives: *Technology Development* and *Technology Application*. Technology Development serves the Program's mission through the design, construction, and testing of innovative technologies that reduce the cost of geothermal energy to competitive levels or secures geothermal resources available for production. The program is divided into three research areas: *Enhanced Geothermal Systems Technology, Systems Development,* and *Resource Development*.

Resource Development deals with finding, characterizing, and assessing geothermal resources through understanding the formation and evolution of geothermal systems. The work in this area builds on continuing geothermal research that investigates seismicity, isotope geochemistry, 3-D magnetotellurics, and remote sensing as exploration tools. Available exploration technology from related industries (e.g., petroleum, mining, and waste management) is evaluated for adaptation to geothermal environments.

The budget for Resource Development for FY 2005 is approximately $2.5 million. Budget information on individual projects is given in the FY 2005 Annual Operating Plan for the Geothermal Technologies Program.

Research Development research is led by Mr. Raymond Fortuna, Technology Manager. Dr. Leland (Roy) Mink, Program Manager, leads the Geothermal Technologies Program.

The Peer Review

The Geothermal Technologies Program conducted a peer review of its Resource Development activities on July 26-28, 2005, at the University of Nevada-Reno, in Reno, Nevada. The review covered 21 projects. The results are intended to provide an independent perspective to the managers of the Geothermal Technologies Program and to offer valuable feedback to researchers.

The Panels

Reviewers were selected specifically for their expertise in the subject program activities. A panel of six experts reviewed the Resource Development projects. In keeping with the Energy Efficiency and Renewable Energy (EERE) guidelines, participation as a reviewer also required the reviewers to have no financial relationship with DOE or presenting research organizations, or conflict of interest with these entities. The names and affiliations of the reviewers are listed below:

Reviewer	Affiliation
Marion Bone	President and Owner TimeSlice Technology, Inc.
Karen Rae Christopherson	President Chinook Geoconsulting, Inc.
Mariana Eneva	Founder Imageair, Inc.
Allen Glazner	Professor of Geology University of North Carolina
Frank Monastero	General Co-Chairman Naval Air Weapons Station
Jeffrey Unruh	Principal Geologist William Lettis and Associates

Review Process

The peer review process followed the August 2004 EERE Peer Review Guide. SENTECH, Inc., a private consulting firm, was contracted by the DOE to provide logistical support and to assist in the preparation of this report. Mr. Patrick Quinlan, of Sentech, Inc., facilitated the peer review meeting.

Information about the peer review process, including tentative agenda, evaluation criteria, scoring methodology, written summary format, and presentation format was provided to the reviewers and the principle investigators in advance. Information about the Geothermal Technologies Program (GTP) was also supplied at that time, including the GTP Strategic Plan, the GTP FY2005 Annual Operating Plan (AOP), and the Resource Development portion of the GTP Multi-Year Program Plan (MYPP). This information provided the overall context for the projects that were reviewed. Reviewers received six-page summaries of every project two weeks prior to the review. A project evaluation form (See Appendix B) following the template contained in the EERE Peer Review Guide was provided in workbooks and distributed to the reviewers at the peer review.

Rules and Criteria

The reviewers evaluated the principal investigators' work on the basis of their oral presentations and written documentation. Written documentation consisted of both a report and a PowerPoint presentation file. These written materials were forwarded to the reviewers prior to the review, to afford the reviewers an opportunity to study the materials. Previous projects that had been concluded or new proposals were not evaluated. Past work or new project proposals were only considered in placing current work in context relative to the purpose and objectives of the project or the plans for completion.

An evaluation form for each project was provided in workbooks distributed to the reviewers. These evaluation forms serve as the official records of the reviewers' evaluations of each project, both quantitative and qualitative. Review criteria, described in the form, included:

1) Purpose and Objective (25%)
2) Work Plan (20%)
3) Results (30%)
4) Impact/Technical Merit (15%)
5) Plans for Completion (10%)

Seven projects were evaluated without a "Results" criterion. Due to these projects' recent start dates; there were no results to present at the time of this review. For these seven projects, the criteria were weighted as follows:

1) Purpose and Objective (25%)
2) Work Plan (25%)
3) Results (0%)
4) Impact/Technical Merit (30%)
5) Plans for Completion (20%)

A numerical score for each of the five criteria was assigned, based on a scale of 1 – 10 as follows:

Excellent:	**9 – 10**
Good:	**7 – 8**
Fair:	**5 – 6**
Poor:	**2 – 4**
Unsatisfactory:	**1**

After the meeting, review scores were tabulated and weighted using the percentages shown above for each criterion.

Comments

Space for written comments was allocated in the evaluation form. Specific comments on the strengths and weaknesses of a project and recommendations to improve it were strongly encouraged. Reviewers wrote comments into the evaluation forms (or typed into electronic forms) immediately following each review. After the comments were transcribed, each reviewer was guaranteed a final opportunity to edit and extend his comments. The comments included in this report are exactly as finalized by the reviewers. As with the scores, there is no attribution of comments to an individual reviewer.

Calculation of Scores

Scoring was done on an individual basis; panel consensus on scoring was neither required nor encouraged. For each project, the collective scores for each criterion were multiplied by the criterion's corresponding weighting factor and summed to yield the project's final score, which was calculated to two significant digits.

Individual reviewer's scores for each project are summarized by a weighted average score. The standard deviation of the weighted total score is calculated on the basis of the resulting collective scores.

The scores on each criterion and the overall scores are reported in the format of [Average Score] with [Standard Deviation]. The complete tabulation of scores is given in Appendix D.

Results

Appendix D lists the reviewer scores, the resulting weighted scores, and the final score calculation for each project. The range in scores is 43 to 85, and the median score is 69.

The score tables also list the standard deviation of reviewer scores. The standard deviation was not used to evaluate projects, but is useful as a measure of variation across the review panel. Lower standard deviations indicate greater panel uniformity in the score for the project.

Score Tables and Figures

Table 1. 2005 Resource Development Peer Review Project Scores and Rank

1.1	William Pickles	Advanced Remote Sensing Methods for Geothermal Exploration	49	8.7	20
1.2	Gary Oppliger	Satellite InSAR Ground Displacement Analysis for Geothermal Reservoir Management and Development	62	5.0	14
1.3	William Foxall	Localized Strain as a Discriminator of Hidden Geothermal Resources	66	11	13
1.4	Wendy Calvin	Remote Sensing for Exploration and Mapping of Geothermal Resources	77	5.4	6
2.1	Gregory Newman, Michael Hoversten	Characterization of Geothermal Resources through Integrated 3D Geophysical Modeling and Inversion	68	6.9	12
2.2	John Pritchett	Exploring for "Hidden" Geothermal Resources in the Basin and Range	62	10	15
2.3	Gary Oppliger	Crustal Strain Rate Analysis through Deep Electrical Anisotrophy Mapping: An Alternative Tool for Identifying the Orientation of Critically Stressed Fractures for EGS	43	14	21
3.1	Mack Kennedy	Gas and Isotrope Geochemistry	80	7.0	3
3.2	Paul Lechler	Geothermal Applications of Multi-Gas Geochemistry	56	11	18
3.3	Lisa Shevenell	Geochemical Sampling of Thermal and Non-Thermal Waters in Nevada: Continued Evaluation of Geothermal Resources	77	7.7	7
4.1	Geoff Blewitt	Targeting of Potential Geothermal Resources in the Great Basin from Regional to Basin-Scale Relationships between Geodetic Strain and Geological Structures	83	6.2	2
4.2	Jim Faulds	Geologic and Geophysical Analysis of the Desert Peak-Brady Geothermal Fields: Structural Controls on Geothermal Reservoirs in the Humboldt Structural Zone	79	5.2	4
4.3	Jim Faulds	Characterizing Structural Controls on Geothermal Systems in the Northwestern Great Basin through Integrated Geologic and Geophysical Analyses	77	5.8	5
4.4	John Louie	Assembling Crustal Geophysical Data for Geothermal Exploration in the Great Basin	75	14	8
4.5	Ernie Majer	Seismic Imaging	51	12	19
5.1	William Foxall	Data Fusion for Geothermal Exploration: The Stochastic Engine	60	8.9	16
5.2	Mark Coolbaugh	Revival of Grass-roots Geothermal Exploration in the Great Basin – A New Approach to Assessing Geothermal Potential Using GIS - Parts IV and V	85	3.0	1
5.3	Greg Arehart	Dating of Young Igneous Rocks Associated with Geothermal Systems in the Great Basin	69	7.9	11
5.4	Colin Williams	Developing and Updating Techniques for Databases for Geothermal Resource Assessments	57	15	17
5.5	David Blackwell	Application of Thermal Techniques for Exploration, Evaluation, and Assessment of Basin and Range Geothermal Resources	72	11	9
5.6	Joel Renner	Exploration Statistics	70	8.5	10
All Projects			67	8.8	-

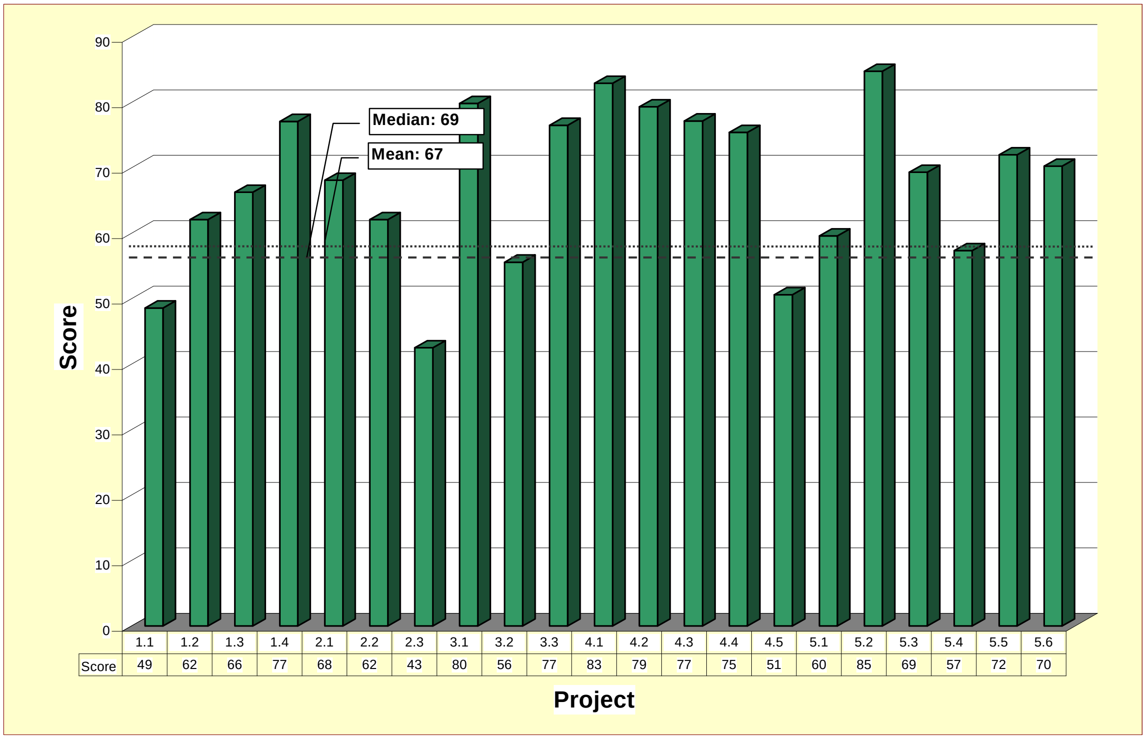

Figure 1. 2005 Resource Development Project Scores by Project Number

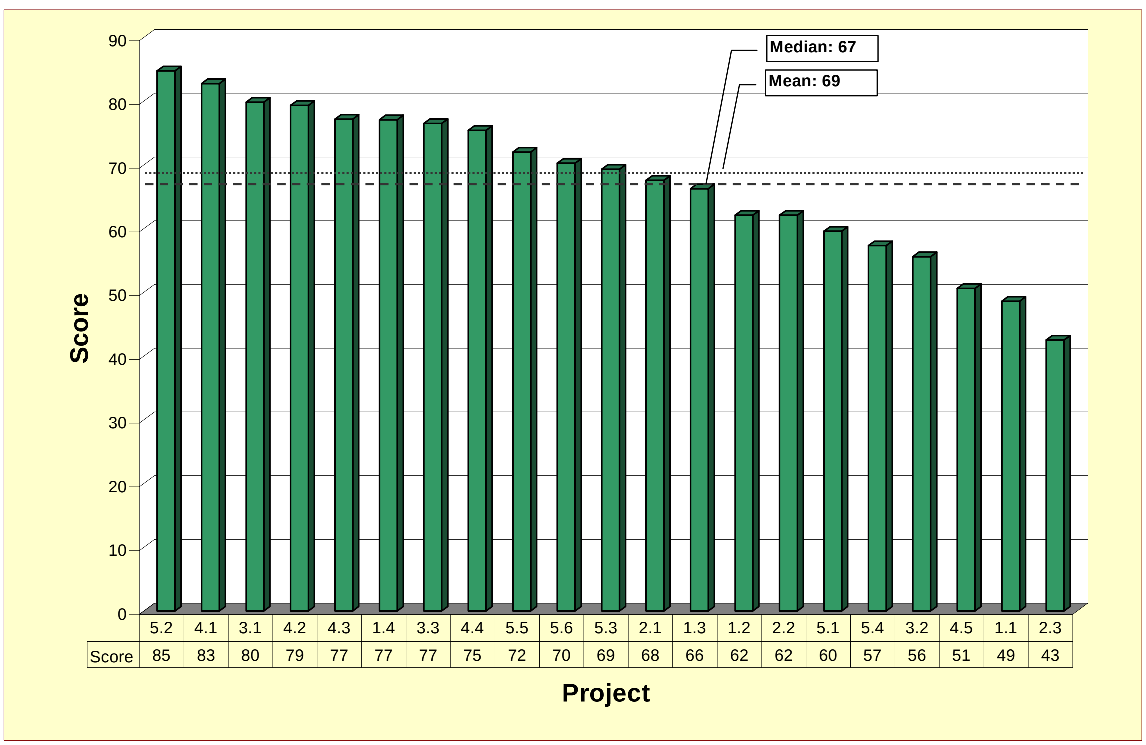

Figure 2. Resource Development Project Scores By Rank

Peer Review Panel Scores and Comments

Project 1.1 Advanced Remote Sensing Methods for Geothermal Exploration

Overall Score: 49
Standard Deviation: 8.7

CPS Project/Agreement #: 17464-11180
Principal Investigator: William L. Pickles
Performing Organization: Lawrence Livermore National Laboratory

Project Description

The purpose of this project is to develop new advanced remote sensing methods that can screen large spatial regions and pinpoint promising locations for traditional geothermal exploration and existing field expansion. Its goal is to establish satellite imagery techniques that can be used for geothermal exploration of the entire Western U.S., Alaska, and Hawaii. Project objectives include:

- Evaluating the utility of high resolution QuickBird satellite imagery and comparing its results to airborne hyperspectral imagery results for subtle fault system mapping;
- Evaluating the use of LIDAR (light detection and ranging) high resolution digital elevation models (DEMs) to locate subtle faulting systems;
- Hyperspectral imaging analysis of Humboldt/Rye Patch.

These satellite techniques may be able to map important structures such as cross-cutting faulting systems and rotating block fault systems, as well as provide an overview of the geomorphology of the region to be explored. By targeting traditional geothermal exploration techniques, it could lower costs and increase the probability of success in finding new geothermal power resources.

Review Panel Comments

This project uses available technology and based on the objectives and results to date, the review panel felt that it provides no new insights into geothermal resource exploration. The reviewers agreed that the PI and team lack the required expertise in this area, both in terms of understanding basic geology and how to interpret the data. The panel emphasized the need for a clear value/relevance of the project to exploration. The panel further concluded that the project should be discontinued at the end of the existing contract.

1. Purpose and Objective (25%)

The research uses off-the-shelf software to process commercially acquired data. The work presented shows no evidence of significant advances in methodology or analysis. There is very poor integration of new data with existing geologic and geophysical work.

This project is more along the lines of EGS instead of exploration. There is nothing unique about what these folks are doing as regards geothermal resource delineation—they're just grinding out project-level analyses.

It is not clear how this project will lead to new insights in exploration.

The objective, to screen large areas with remote sensing and identify areas of interest for further exploration, is very valuable for geothermal exploration purposes.

Purpose is OK but does not clearly define an objective that is unique in any way.

LIDAR appears to be the most useful for multiple purpose and broader range.

2. Work Plan (20%)

It is not clear from the presentation that the project team has the right expertise to gain the maximum interpretive value from the data.

The schedule seems protracted and focused on graduate student needs instead of timely products. Only budget shown was one year. No "decision points" per se.

Too brief to really evaluate.

Total work plan is good, but there may not be time to achieve goals stated in FY05 plan.

High cost for what seems a poorly focused project. Is the objective just to map faults? And if so, aren't there already much cheaper/easier ways to do this?

Limited in scope and narrowly focused.

3. Results (30%)

No significant results from FY05 presented except completion of data acquisition milestone. Based on results to date, it is not obvious that the methodology is superior to traditional, lower-cost approaches.

The results show that LIDAR can be used to map subtle faults that otherwise could go undetected. This capability has been well documented elsewhere. Results are very pedestrian and don't advance the DOE exploration goals very much at all.

The results presented were disappointing. The PI could not tell the panel what new information was learned from the LIDAR and other data. In particular, the faults shown on the LIDAR data look obvious from the air photo shown. The PI did not seem to know if this area had already been examined on the ground, as seems likely, nor if air photos are available. Integration with previous work is necessary.

This project has been going for 5 years and expires very soon. Yet specific examples of presumably new potential geothermal sources were not shown. Clearer markers of geothermal potential should have emerged by now.

They need to compare to results that can be gained by other means. Does this approach have added value? Needs better ground truthing of small features.

Results appear to be unproven at this stage.

4. Impact/Technical Merit (15%)
The work presented provided little evidence that the research will lead to new insights/approaches that will significantly advance geothermal exploration.

Industry will barely take notice of these results unless they own property/leases in the specific areas studied.

The LIDAR part of the project seems to lag well behind other projects with which I am familiar. In particular, the off-the-shelf processing can be improved. There were no scales on the images and the PI did not know what the scales were.

I think merit is still not demonstrated sufficiently for exploration purposes. Examples of newly discovered faults were shown, but their connection with potentially new geothermal sources has not been substantiated.

Poor understanding by PI of his students' work.

LIDAR—if applied, could be a potential method for broader exploration effort.

5. Plans for Completion (10%)
Work can probably be completed as scheduled (Oct 05).

They should be able to finish by Oct. 2005, but we really didn't hear about any later work. Let this be finished.

It seems the team does not have much time left (only 2 or 3 months) to analyze the QuickBird and LIDAR images, and then write a report on the whole project.

Not a well-designed project.

Plans seemed uncertain.

Strengths
Methodology does hold promise for certain exploration applications.

Few. MacKnight thesis very nicely done.

None noted.

Using a variety of remote sensing data to look for signatures of faults possibly associated with geothermal systems.

LIDAR could be a possible "first indicator".

Weaknesses
The project team appears to lack the expertise necessary to make best use of data. Existing geologic data not fully integrated into the research program to maximize value for cost.

Very mundane. Lacks creativity and originality. Applicability to achieving DOE objectives is sorely lacking.

Unfamiliarity of the PI with basic geology is a major flaw of this project. This is not the place for amateur work. The hypothesis that intersecting faults produce enhanced permeability is plausible but (1) it has been explored before by experts and (2) there are thousands of significant faults in the Basin and Range and probably millions or trillions of fault intersections. Should we explore all of them? Typos in report are distracting.

It seems PI did not have enough specific knowledge of the work carried out by his students. Also, one of the presumed LIDAR images he showed looked very much like an aerial photograph?

Poor grasp of real work application issues; poor understanding of the exploration problem.

1) Knowledge of parallel use of techniques. 2) Integration for wide exploration effort.

Recommendations
I would not recommend this project for further funding under the DOE geothermal exploration program.

Integrate with other subject areas; in particular, the decades of basic field geology and geophysics that have been done. One gets the feeling that this work is proceeding in a vacuum. Reading PowerPoint slides off of the screen is a terrible habit and is offensive to the audience.

Re. value in exploration: Show that features identified are uniquely associated with faults relevant to geothermal systems. Make it clear—so far have they discovered reliable markers for possible sources or not?

Discontinue funding of any future work.

Expand LIDAR focus.

Project 1.2 Satellite InSAR (Interferometric Synthetic Aperture Radar) Ground Displacement Analysis for Geothermal Reservoir Management and Development

Overall Score: 62
Standard Deviation: 5.0

CPS Project/Agreement #: 12339
Principal Investigator: Gary Oppliger
Performing Organization: University of Nevada

Project Description

The purpose of this project is to develop and test innovative applications of satellite radar interferometry (InSAR) technology to support increased utilization of geothermal resources in the United States. The project addresses a subset of potential areas for InSAR geothermal research designed to deliver immediate results, improve understanding of the technology's application, and lead to the possibility of new technical and earth process discoveries. The investigation seeks to improve InSAR anomaly definition in the study area, advance the processing and interpretation methodologies, and improve understanding of the geometry and spatial extent of production-induced fluid flow at the Bradys-Desert Peak geothermal fields. The creation of basic measurement datasets and processing and interpretation methodologies will assist reservoir management and expansion and serve as technology templates for other geothermal fields.

Review Panel Comments

The review panel agreed that while this analysis of InSAR data has merit, as applied in this project it has more value for reservoir management than exploration. The panel identified that some good results have been obtained to date and may lead to advances in InSAR as a tool in other areas of energy resource development. However, the panel felt that the results failed to demonstrate a clear application to geothermal exploration.

1. Purpose and Objective (25%)

The research has intrinsic scientific merit and obvious applications to geothermal reservoir management. Project is not clearly directed to geothermal exploration, however.

This project is more suited to reservoir management than to exploration. Outside of the few improvements in data processing, there is nothing new here. There are much better volume-strain modeling techniques than the Mogi body approach.

InSAR clearly has a great deal of utility in geothermal studies and is a worthwhile technique to pursue. However, this project is, as its title says, a reservoir management study and it is not clear how this work will bear on exploration for new resources. I guess the main point is that this tool will allow reservoir managers to expand drilling locally around known resources.

Clearly stated objectives in connection to InSAR application to a producing reservoir. Connection to exploration is not clear.

This is not really an exploration tool—it is a reservoir management/production method.

Appears to have valid need and could play a critical role if merged with other methods.

2. Work Plan (20%)
Clear, logical work plan; however, it should be more specifically focused on geothermal exploration.

This will be around $335, 000 of total funding by the FY06 is done. They still have not done the 3 "key modeling milestones" for '05, let alone complete the FY06 objectives.

Work plan seems ambitious and reasonable.

Plan needs to address exploration.

Is the work plan being followed? We weren't shown many modeling results or algorithms/codes.

Appears to restrict findings of method to localized areas.

3. Results (30%)
Very interesting results with promise of technology transfer to industry (especially through development of more user-friendly modeling programs). Approach appears to be better suited to reservoir management than exploration.

They still have not done the three "key modeling milestones" for '05, let alone complete the FY06 objectives. The way they have applied InSAR is more in the view of resource management than resource exploration.

Work to date is scientifically interesting although, as noted above, relevance to exploration is unclear.

Good results were demonstrated relevant to management of existing reservoirs ("real" reservoir outlines vs. assumed ones). However, I did not detect in the presentation any examples relevant to exploration.

Again—didn't see a good demonstration of how modeling is supporting the results and hypotheses.

Information of environment effects on quality of results is lacking.

4. Impact/Technical Merit (15%)
Again, research has more applications for reservoir management than geothermal exploration per se.

Results will have some technical impact at the reservoir level, but not at the exploration level. Industry will notice this work, but it will not make a major impact on how they do business overall.

This work has the potential to enhance interpretation of InSAR data, a rapidly developing field. The problem is, of course, that solutions are highly not unique and therefore must rely on other observations (e.g., GPS or leveling) to cut down on the number of possible models. At this stage of the project one would expect some "real" publications.

Important impact on detecting true reservoir outlines and making decisions related to expansion of producing reservoirs. I do not see impact on exploration yet.

Probably not for exploration. Could be useful in production.

5. Plans for Completion (10%)
PIs state that the FY03 – 04 work led them to focus FY05 work on applications to "reservoir definition and management". The completed work thus will not directly contribute to exploration science.

They should be able to complete all of this work by the end of FY06.

Again, ambitious.

Need to include exploration application in plans—they have 1.5 or 2 more years of the project.

Seems on track if they can wrap up modeling and correlation.

Limited information provided to define plan's ultimate direction.

Strengths
Research will contribute to development of InSAR as a reservoir management tool, and will facilitate transfer of technology to industry.

Some nice technical advances in InSAR processing. Modeling is somewhat primitive compared to the work of Fialko and Simons (CalTech) or Hager and his students at MIT (cracked rock model).

Innovative use of InSAR data; direct ties to a producing field; potential for using mechanical modeling to understand complex InSAR signals.

Good application of InSAR to existing geothermal systems in terms of management and expansion.

Well presented.

If method can be proven to accurately perform at depth. Results may apply to other energy supplies.

Weaknesses
Research probably will have negligible impact on geothermal exploration.

Not really developing exploration-level tools or techniques.

Lack of utility to exploration.

I do not see connection to exploration yet. Presentation was repetitive and did not address main goal of the program.

Method did not focus on projects of exploration size.

Recommendations
Seek explicit exploration application.

Look at using on larger areas to ascertain effectiveness for exploration.

Project 1.3 Localized Strain as a Discriminator of Hidden Geothermal Resources

Overall Score: 66
Standard Deviation: 11

CPS Project/Agreement #: 11179
Principal Investigator: William Foxall
Performing Organization: Lawrence Livermore National Laboratory

Project Description
The purpose of this project is to investigate the relationship between strain rate fields along active faults and geothermal systems in the western Basin and Range, and to develop a technique to identify hidden geothermal resources through the detection and analysis of strain anomalies on a regional basis. If there is a relationship, then mapping the strain rate field can be used as a tool to find hidden resources. The surface strain rate field can be mapped directly by measuring surface displacements by geodetic techniques and the strain at seismogenic depths indirectly by analyzing seismicity. These methods are complementary in that they provide different characterizations of the strain field at different spatial resolutions. This project assesses the feasibility of using satellite repeat orbit synthetic aperture radar interferometry (InSAR), a space-based geodetic technique, and stacked interferograms to map localized strain concentrations in the Basin and Range. This research is designed to provide a cost-effective regional exploration tool that will increase the success rate in finding economic resources.

Review Panel Comments
Several reviewers stated that some of the assumptions that framed the project are not proven. Yet, the panel agreed that the project could have merit for exploration on a regional scale, particularly if used in conjunction with other methods. The panel noted

that the PI did not sufficiently explain the anomalies. Several reviewers expressed concern that the level of effort and funding level appeared to not correlate.

1. Purpose and Objective (25%)
Research has intrinsic merit as a method for screening large areas for anomalous high tectonic strain rates. Work proceeds from a clearly stated genetic model for geothermal occurrences in the western U.S.

The prospects for this work positively impacting geothermal exploration are very high. This approach (using InSAR or some other strain discriminator) is what is genuinely needed for the first-order geothermal prospect identification.

The stated objective is fairly clear, but the initial assumption, that geothermal resources are to be found at areas of high strain, seems far from proven and is a dubious framework for the project.

Using InSAR to look for localized strain possibly associated with hidden geothermal sources. Value for exploration, if concept works, is high.

Good objective but some poor basic assumptions.

Purpose defined as exploration, but seemed to focus on field development improvement.

2. Work Plan (20%)
Work is logically organized and reasonably paced. Price seems high.

This represents $1 million in funding. I expect that this next year's effort should yield some very specific, highly-functional InSAR tools and techniques that can be applied on a regional exploration scale.

The work plan seems reasonable and the link to continuous GPS stations is both useful and necessary.

Good, but I don't understand reference to FY07 (Foxall summary report - Page 6), while funding table shows FY 2002 to 2006 (Foxall summary report - Page 2). Is this project a six-year one?

High cost for results to date. Should have clearer goals—data vs. objective.

Has potential, but restricted scope. Plans did not reflect use in virgin areas.

3. Results (30%)
Results to date indicate that the research has a good chance of successfully testing InSAR as a screening tool for identifying areas of localized high strain rates.

Results are high quality and speak directly to the DOE Exploration Program objectives.

The results are rather minimal given how far into the project we are. Results presented to the panel show little; it is curious that the PI did not manage to show the frame he had intended, yet when this one was put up it mainly seemed to show the Dixie Valley production area, a sort of result known from several other geothermal fields. He has not demonstrated that new regional signals can be teased out of the data. There seem to be no publications other than one internal technical report out of this project in spite of its 2002 inception.

Results are intriguing, but reliability has not been demonstrated yet. Need to bring fast more clarity to results for the remaining duration of the project. Uniqueness of results should be addressed.

Needs better demonstration that results are correlative with strain and faults.

Definitive results were open to interpretation by visual methods. Project will require broader use in areas where historical information is at a minimum. If discoveries are achieved by the results of the method, more credibility would be forthcoming to use in exploration.

4. Impact/Technical Merit (15%)
Industry may use these results for future exploration in the western U.S. It is questionable that individual geothermal companies will undertake similar investigations, however.

This represents the first-order geothermal prospect identification tool of the future. High marks for scientific quality of results.
This method has potential for exploration, but there has been little bang for the buck out of this project.

If technique is shown to work, it will be a valuable tool for pinpointing future exploration areas. Most likely it would only work in a package with additional markers of potential geothermal sources.

If they can establish patterns truly related to strain it could be useful.

The method has high value if proven to help in minimizing search for high potential sites. It would appear that this is not a "stand alone" concept. It has to be used in conjunction with other methods to have a high "value added" mark.

5. Plans for Completion (10%)
Phase 3 should be focused on using simple models to relate potentially significant strain rate anomalies to fault slip rates.

Good, but I don't understand reference to FA07 (Page 6), while funding table shows FY 2002 to 2006 (Page 2). Is this project a six-year one? Otherwise tasks outlined are reasonable.

Needs better work or correlation—data, anomalies, what does it mean?

Plans do not appear to reflect a goal to produce a method for exploration. It appears the plans point to restricted area use.

Strengths
Goal of research is to develop a screening tool to assess large areas for geothermally prospective sites.

InSAR has great potential, and the PI seems aware of this.

Potentially important application of InSAR to exploration, with a cost-effective, semi-continuous spatial coverage of relatively large areas.

Good idea.

If proven as a consistent exploration indicator the method could be possibly utilized in other energy related exploration.

Weaknesses
Value to industry will primarily come from publishing results. Widespread adoption of method by industry doubtful.

Little progress has been made. It is curious that the strongest signal seen in the cross sections presented is the sinusoidal variation at 5 – 10 km wavelength, yet the PI seems to not have wondered about this, focusing instead on the longer-wavelength variations. The longer-wavelength stuff may indeed be the real signal, but the investigators must determine whether the shorter-wavelength variation is real or is a processing artifact, as several members of the panel suspect.

Seems project has been going for three years now, but benefits are still vague.

Needs better analysis method. Expensive for results to date.

Focus on use was narrow and related primarily to developed projects.

Recommendations
Avoid developing complex models in Phase 3, and focus on evaluating deformation in "frontier" areas rather than existing geothermal fields.

Focus on how this tool, some variant of this tool, or a totally different remote sensing tool can be developed or refined as a robust first-order prospect identification device/approach, assuming geothermal resources occur in areas of high strain accumulation. InSAR has some serious drawbacks that may make it poorly suited to this task. On the other hand, some modifications to InSAR data processing may dramatically improve this tool. The investigators should "think outside the box: in an attempt to make this tool more useful. 1 – 2 mm variation in the InSAR data across the entire area is insufficient for delineating specific areas of strain accumulation.

There is a great deal of obvious data treatment that has not been done, such as making cross sections in other orientations, determining whether the sinusoidal variation is a processing artifact, working more directly with other geodetic techniques, fitting trend surfaces to identify anomalies, etc.

Analyze comprehensively uncertainty and uniqueness of results.

Focus remaining time toward exploration use for broader areas.

Project 1.4 Remote Sensing for Exploration and Mapping of Geothermal Resources

Overall Score: 77
Standard Deviation: 5.4

CPS Project/Agreement #: 12339
Principal Investigator: Wendy Calvin
Performing Organization: University of Nevada, Reno

Project Description

The purpose of this project is to enhance geothermal exploration and development by using known surface signatures such as sinter, thermal anomalies, and hydrothermally altered minerals to map surface expression at known regions and explore potential in previously unmapped sites. The work seeks to define surface identifiers of geothermal resources through analysis of remote sensing imagery to characterize mineral, vegetation, and thermal properties at known source regions to establish markers of potential in other areas. New potential targets will be identified using the complementary signatures, and geologic and fault mapping can be achieved using regional and local scale image information. This information can be used to determine high-priority targets in existing resource areas and to identify new targets for geothermal exploration and development. The project is expected to identify new resource regions and bring down costs by helping to focus development in existing resource areas.

Review Panel Comments

The panel generally agreed that the project could yield benefits for exploration if expanded to a regional scale. They felt that the methodology should be tested in unknown areas to determine its true utility. Other reviewers suggested that new tools or techniques be investigated that could expedite data processing, improve the speed and reliability of initial procession, and perhaps integrate with other aerial methods to simplify data acquisition and processing. Drill core analysis is less relevant to project goals.

1. Purpose and Objective (25%)

Method appears to be best suited for screening prospective sites for detailed investigations.

This study has demonstrated the usefulness of remote sensing techniques in delineating alteration patterns at a scale that is useful in focusing on individual thermal seeps. None of this is particularly new, but they have done it very convincingly. Now they need to be bold and expand their view to regional scale.

This project seems to have some potential for exploration, if the techniques can be clearly defined to take much of the subjectivity out of them.

Looking for surface identifiers in remote sensing imagery—very valuable for exploration purposes.

Seems like a good way to recon large areas, for larger resources.

The direction was clear. The method utilized already known by community. Previous results from utilization of method appears to be minimum

2. Work Plan (20%)
Research is generally sound and well integrated. Task 5 (Drill Core Analysis) is less relevant to the overall project goals.

The investigators are too focused on completion of analyses for specific prospects. They need to step back and take a longer view of the applicability of their techniques. They also need to apply these techniques to a totally new, undeveloped area.

Work plan has been systematic and logical.

Good work plan.

The plan of activities moved from concept into a result tied to known values. Does not appear to be an original concept for exploration

3. Results (30%)
Results to date indicate that the analytical approach and methodology may be useful for geothermal exploration.

The work needs to be generalized to be applicable in geothermal exploration. The usefulness of the tools has been demonstrated at a scale useful for individual thermal seeps. Now it needs to be expanded to a regional scale.

Results are reasonable, although not particularly surprising. More work will tell if new anomalies, rather than known anomalies, can truly be identified. Initial results are encouraging, and there have been significant publications (although not in peer-reviewed journals).

Very good results within 3 years, at a relatively low level of funding (especially compared with some of the other projects).

Seems to have application in the project areas, and also new areas.

Results appear to be valid—uncertain on how much subjective effort went into results.

4. Impact/Technical Merit (15%)
Clear benefits and potential applications to geothermal prospecting.

This approach has proven that the technologies that they investigated are viable in prospect delineation.

This project has significant potential.

Significant technical merit.

Although a recon application was not shown, they stated that this has been done and was effective.

Results could be a "link" in a broader effort using additional methods.

5. Plans for Completion (10%)
Analysis of SAFOD and Blue Mountain cores appear to be less relevant to geothermal exploration than the other tasks. Is this the best use of exploration research funding?

They will achieve their goals, but they need to adjust their future goals to apply the concepts more broadly.

Reasonable, given funding level.

No time left for more work, but so far so good.

On track, apparently.

Details were scarce. Sounds as though he has plans but failed to clearly communicate them.

Strengths
The methodology has potential for applied geothermal exploration.

Good productivity, including thesis work; good characterization of known areas.

Identifying areas of geothermal potential in remote sensing imagery is potentially very valuable for exploration. Good results so far.

Seems application based.

Appears to be a method to both look at targets in a broad arena and also for detailed analysis area.

Weaknesses

Science and methodology are well established. Analytical methods should be tested in frontier areas to assess utility for exploration.

No demonstration (yet) that the technique can truly be useful when the answer is not already known. This is a common problem with projects that study known occurrences of a feature (geothermal area, ore deposit, etc.) and attempt to generalize.

Seems didn't venture in truly new areas.

PI would have been preferable for presentation.

Could be subjective, based on some answers to "artistic" questions.

Recommendations

Reconsider analysis of SAFOD and Blue Mountain cores as the primary focus of the remaining work.

This work (approach) needs to be generalized to be applicable in geothermal exploration. These investigators have demonstrated the utility in characterizing thermal manifestations in known geothermal areas using off-the-shelf remote sensing techniques. Now they need to take it to the next level for broad-scale evaluation. Based on this presentation, I suggest these investigators take a hard look at developing tools and/or techniques that can be used to expedite the manpower-intensive data processing associated with the application of the hyperspectral and satellite-based tools.

Do some blind tests to test how unique the mineral characterization is.

To demonstrate better value to geothermal exploration, in future work need to examine imagery over areas outside well known fields.

Can improve how initial procession/view of images is done to make it quicker and more reliable.

Integrate method with other aerial methods to help in reducing "labor" intensive efforts.

Project 2.1 Characterization of Geothermal Resources through Integrated 3D Geophysical Modeling and Inversion
Overall Score: 68
Standard Deviation: 6.9

CPS Project/Agreement #: 11178
Principal Investigators: Michael Hoversten and Gregory Newman
Performing Organization: Lawrence Berkeley National Laboratory

Project Description

The purpose of this project is to demonstrate the full potential of an integrated geophysical modeling and inversion approach for characterizing geothermal systems in three dimensions. The project's objectives are to:

- Develop an integrated three-dimensional (3D) geophysical model of the Coso geothermal reservoir.
- Determine if time-lapse measurements of streaming potentials (SP) made on a surface grid simultaneously with flowing geothermal wells having a deterministic flow rate signal can be used to delineate the spatial location of the primary flow pathways feeding the wells.

Data and resources from two current cooperative projects are leveraged to carry out multidimensional magnetotelluric (MT) modeling and inversion at Coso and to interpret controlled source audio magnetotelluric (CSAMT) and SP data sets at Fort Bidwell. The Navy provided the geophysical data sets. The goal is to image large geothermal systems in a single self-consistent 3D geophysical model, integrating seismic, gravity, and electrical-electromagnetic data with log, geochemical, and flow data.

Review Panel Comments

The review panel agreed that the project is well planned and executed but felt that it did not show clear relevance to exploration. A few reviewers thought the results to date were interesting while others noted that the results were somewhat unexpected. The panel recommended that model be further refined and better integrated with available geologic data.

1. Purpose and Objective (25%)

Good science with high intrinsic merit, but application to geothermal exploration appears limited. Primary value of method appears to be in defining the location and geometry of hydrothermal upwelling (i.e., reservoir characterization).

The objectives of this project are, indeed, relevant to the overall DOE geothermal program. Definition of the structure of a geothermal field is essential. The reason this project doesn't get a 9 or 10 is that it may better belong in the reservoir characterization part of the program, not exploration.

This project, while good science, does not seem to relate well to the goals of the exploration program.

Clearly stated objective for 3D MT as a geothermal tool, but I did not see connection with exploration.

Good idea—the integration—but this is being done on an already developed field, hence it is more of a reservoir project.

Parallel efforts in oil and gas community for reflected signals have proven to be an important approach.

2. Work Plan (20%)
Research appears to be proceeding logically. Cost is high.

They are fundamentally on schedule. They have encountered problems with availability of massive parallel computing capability. The cost is outrageously high. There are other 3D codes that are available at a much lower cost to the customer.

Reasonable, as far as I can tell.

Good plan for the project as stated. (Again, no connection to exploration, yet, but it is a new project, so maybe this concern can be addressed in future work).

A good plan not yet implemented. Integration not yet done. Could apply to exploration

Work plan seemed clear.

3. Results (30%)
Good progress on data acquisition and analysis. More interpretation of data is required, with emphasis on synthesis with geologic data. PIs should address the significance of conductive anomalies for geothermal exploration in less-studied areas.

Confusing results are 180 degrees opposite of expectations, and the PI's can't explain it. Very coarse resolution.

Results are interesting and seemingly anomalous, which is not necessarily a bad thing. However, again I wonder how these results relate to exploration.

This is a new project. Results so far appear interesting, but there were some concerns expressed by others on the panel, about results appearing to be the opposite of what is expected.

The only result shown was the 3D inversion of MT data—no integration or inversion of gravity, seismic, SP, or CSAMT.

The results were reflective of the physical field locations. Surprises exist in some of the modeled results.

4. Impact/Technical Merit (15%)
Most likely benefit is in imaging the dimensions and geometry of hydrothermal convective cells. Method is not a simple (or cheap) first-order exploration tool, however.

This is a massive inverse problem that has yielded results that are technically sound given the technology employed. However, there are myriad solutions for the data that have to be constrained by geological and geophysical data from numerous other sources. Industry might be interested in this work, but not for exploration.

This may lead to a better understanding of fields that are already in development, when coupled with other types of studies.

The same concern—connection to exploration is not clear.

If they integrate in the inversion, it will provide another 3D example. Needs to be published; needs true integration

Primarily in field development rather than exploration.

5. Plans for Completion (10%)
More integration of geologic data required. Based on comments by PI, it is unclear who will perform this task. Responsibilities of PIs and U.S. Navy for analysis and interpretation need to be clearly defined in order to obtain maximum benefit.

A bit soft on how and when they will complete this project. It is all contingent on beating the model results against real data, and whether they can get access to the massive computing resources they need.

Appears good.

A good concept if they complete all tasks.

Appeared to be still seeking a firm position to promote to end user.

Strengths
Innovative approach for (possibly) imaging the dimensions of hydrothermal convection cells in geothermal fields.

Technically superior modeling capability of Newman.

Apparently competent project on 3D MT as a geothermal tool.

The future of MT interpretation is 3D modeling—this is a step in the right direction.

Well suited for "field development".

Weaknesses
Expensive; better suited for reservoir characterization and management.

Low resolution of the model. Huge expense.

No connection to exploration (yet).

The project is very expensive for the product—commercial cost would be 10 – 25% of this project.

At present exploration budgets for activity, establishing "cost effective use" is most difficult.

Recommendations
PIs should make greater effort to integrate all available geologic data into their interpretation of the inversion results.

See if exploration can be addressed at later stages of work.

Do the integration.

Continue to refine model and cross discuss with other energy programs.

Project 2.2 Exploring for "Hidden" Geothermal Resources in the Basin and Range

Overall Score: 62
Standard Deviation: 10

CPS Project/Agreement #: 12354
Principal Investigator: John Pritchett
Performing Organization: SAIC

Project Description
The purpose of this project is to assess the feasibility of finding "hidden" geothermal reservoirs using a combination of existing electrical exploration methods. The objectives of the project are to:

- Theoretically investigate the feasibility of finding "hidden" geothermal reservoirs in the Basin and Range using electrical surveys;
- Identify operating geothermal fields in the Basin and Range for which both electrical exploration surveys and adequate reservoir information are available; and
- Study the correlation between the subsurface geothermal reservoir and the surface electrical surveys using detailed numerical modeling.

Attainment of these objectives will help determine the feasibility of locating "hidden" geothermal reservoirs using electrical methods such as conventional DC resistivity, magnetotellurics, and self-potential. The project will thus contribute to meeting the technical goal of removing barriers to identification of hidden hydrothermal resources.

Review Panel Comments
The review panel questioned the originality of the project approach. Several felt the project was poorly designed and executed. The panel concluded that any value of the

project lies in demonstrating its real world applications, and could result in quick and inexpensive exploration tools.

1. Purpose and Objective (25%)
Goals of Phase 2 and 3 are to identify fields with existing EM surveys and essentially remodel the data. How will the new modeling improve on existing analysis?

I can see where the PI's were headed with this project, but the simple models done in phase 1 have been done elsewhere (and earlier) and the underlying assumptions are too simplistic.

This forward modeling is a useful exercise for evaluating how to use various exploration techniques.

Purpose is to theoretically evaluate the feasibility of using a combination of electrical methods to identify new geothermal sources. This is valuable for geothermal exploration.

The work has been done before; regardless this approach as outlined is not cost effective for recon of large areas.

It appears this activity was an "after thought" use of information.

2. Work Plan (25%)
Research appears to be proceeding logically.

There is not much of a plan presented. I can't really tell the details if none are available.

Reasonable.

Doable.

Poor approach: 1) Work has been done before; 2) It is not applicable to large areas (say 100 x 100 km) since not cost effective; 3) Review of existing data will not be comprehensive—not enough and then not applied to large scale problem.

Once focused on desired direction, conclusions would reflect a plan followed to completion.

3. Results
Phase 1 (FY04) analysis of generic models suggests that blind systems may have distinctive geophysical signatures. Not clear from the PI's presentation that the models are actualistic analogues of geothermal resources in the Great Basin, however.

I am unsure how one might apply these results in the real world. Forward modeling a simple system seems to have led to resolvable differences in various geophysical parameters, but in the real world, where faults are not planar and so forth, it is unclear that one could really resolve these differences. Real-world tests are needed, certainly.

Results so far relevant to exploration.

4. Impact/Technical Merit (30%)
Applications to designing EM and other geophysical surveys to more effectively assess blind resources.

Any technical merit that this project has is in the proof-of-concept of using a relatively inexpensive electrical method to confirm the existence of a hidden resource. I'm not so sure that these models (Phase I) are realistically parameterized.

It will be difficult to assess the merit of this work until it is applied to the real world.

If electrical surveys are demonstrated to be useful, impact is important.

Maybe it will provide some added value. But the entire premise is misguided; not a good understanding of available geophysical, geochemical, and geological tools.

Will be a method that will require supporting techniques to realize end benefit.

5. Plans for Completion (20%)
Their planning—"trust me."

Clearly stated.

Just a poorly constructed concept from beginning, and bad attempt at work that's already been done.

Phase 1—completed, Phase 2—waiting for data.

Strengths
Phase 1 work suggests there is value in using multiple EM methods in geothermal exploration. Modeling also provides insight into the lateral extent of surveys required to identify blind resources.

Could result in quick, inexpensive tools for substantiating the presence of blind geothermal resources.

Clearly outlined tasks.

Initial models (forward models seem to be of interest to some end users).

Weaknesses
Results of modeling generally confirm intuitive understanding of high-temperature hydrothermal convection systems. Value added by remodeling existing data not clearly demonstrated.

Technically not very strong. Concern over model realism.

I detected none. However, some panel members expressed doubts about how new this approach is and if this type of work has not been already done.

Too expensive. Very poor presentation.

Seems to be working; isolated to the end user.

Recommendations
In view of the above, novelty of approach and/or modifications of existing approaches need to be demonstrated. Also, compare usefulness with non-electrical methods.

New data for evaluation against model generation would be helpful.

Project 2.3 Crustal Strain Rate Analysis through Deep Electrical Anisotropy Mapping: An Alternative Tool for Identifying the Orientation of Critically Stressed Fractures for EGS Projects

Overall Score: 43
Standard Deviation: 14

CPS Project/Agreement #: 12339
Principal Investigator: Gary Oppliger
Performing Organization: University of Nevada, Reno

Project Description
The purpose of this project is to research the use of a modified magnetotelluric sounding method as a tool for more rapidly and accurately characterizing critical stress at EGS sites, improving the success rate at previously un-drilled sites. The odds of establishing productive EGS wells can be significantly improved if wells can be sited within the paths of propagating fractures. This project will determine the utility of the modified magnetotelluric sounding method for identifying the orientation of critically stressed fractures and the relative amount of crustal strain accumulation. Magnetotelluric sounding will also be used to characterize critical stress at one active EGS development site to verify appropriate well siting. The derived information on critical stress directions will be used to help optimize methods of interpolating crustal strain from GPS-station velocity measurements, and will also be compared with existing structural and tectonic models of the Great Basin. Finally, the improved crustal strain and tectonic models will be incorporated into the regional geothermal favorability model. This project may provide a means for mapping the character of critical stress in the earth's crust. That information can be used to assess how much of the earth's crust might be amenable to various EGS development programs, assisting in the development of supply curves.

Review Panel Comments

The review panel agreed that project as planned is ill conceived. The panel believed that applying MT in this way is flawed and will not yield desired results. They recommended that if the project is continued it be taken in a different direction. They also objected to the organization and delivery of the presentation.

1. Purpose and Objective (25%)

PI is to be commended for a creative proposal. As presented, however, the research does not be appear to be focused or analytically sound.

The stated purpose/objective of this project is laudable and consistent with DOE goals. You cannot, in my opinion, achieve the objective, however.

It is not at all clear that this project will work. I am surprised that it was funded without even the most basic forward modeling or analysis of existing datasets.

Appears interesting. I am surprised though, that the MT modification as described has not been done already somewhere. And I am not sure that the method would work as intended.

Completely flawed idea to use MT for this purpose.

Discussed purpose and objective—program is just beginning.

2. Work Plan (25%)

Serious (possibly fatal) flaws in understanding of basic science.

It is dragged out too much. This entire project should be completed in 18 months (maximum).

Little to go on here. The PI did not communicate what the study is about very well. However, it does not seem that the PI has tied this project to any other fields of study and this is a big flaw.

Appears reasonable.

Flawed technique—MT will not provide the results he thinks it will.

Plan appears to be conservative in approach.

3. Results

Few to report, but the lack of preliminary work is damning.

Appears reasonable.

4. Impact/Technical Merit (30%)
Unlikely to be adopted as an exploration approach by industry.

I don't think this is technically feasible and the PI didn't give any reasons during the presentation to change my opinion. There are some serious technical shortcomings in the approach.

It is unlikely that this project will lead to anything that is not already known. It looks like a project where the results, if any, will at best agree with what is already known. The PI could not answer the question "what if you come up with fracture orientations that do not agree with the geology?”

Will be valuable if it turns out to be a truly complementary technique. However, I have doubts about it.

This is the wrong approach—the PI doesn't understand what MT can/can't do and how to apply it—it won't work for the problem as the PI explained it.

Could be effective when used in conjunction with other techniques for wide spread exploration.

5. Plans for Completion (20%)
They might be able to acquire the data, but it will be almost useless in meeting the objective.

Unrealistic.

Milestones outlined appear reasonable if this work is to be pursued.

Won't work and will be more expensive than he has anticipated.

Hopes for the method are high for focused area, but exploration use may be limited.

Strengths
If successful, this MT approach would image geometry of "permeability anisotropy" (i.e., trend and connectivity of open fractures).

None noted.

If this is a novel approach not used so far, and if it is shown to be complementary to other approaches, it will be a valuable addition to a set of geothermal exploration tools.

Weaknesses
Analytical approach is highly speculative and based (in part) on erroneous assumptions about the mechanics of strain accumulation and release during the seismic cycle.

Technical deficiencies that are probably fatal flaws.

I am dumbfounded that this project was funded without basic preliminary work—forward modeling to show that the project might work; analysis of existing datasets to show preliminary results.

I have problems with this presenter—presentations appear repetitive and arm-waving at times, and do not seem to nail down the benefits to exploration. Also, I am concerned that the bulk features detected this way may be too unspecific.

No understanding of MT recording and data.

Recommendations
Work should have been more rigorously questioned during the proposal evaluation process. DOE should insist on external peer review of proposals submitted for GB Center monies at UNR.

PI really needs to work on presentation skills—he was unable to communicate anything about this project to us.

Modify presentation style—too many words in slides, figures only shown at the end. Reminder: "a picture is worth a thousand words …"

Take a different route.

Rather than seeking "cheap" methods, analyze and compare cost effectiveness of the techniques relative to risk costs for drilling.

Project 3.1 Gas and Isotope Geochemistry

Overall Score: 80
Standard Deviation: 7.0

CPS Project/Agreement #: 17465-11175
Principal Investigator: B. Mack Kennedy
Performing Organization: Lawrence Berkeley National Laboratory

Project Description
The purpose of this project is to evaluate and develop the use of helium isotopes as an exploration tool applicable to hidden geothermal systems characteristic of the Basin and Range Province and surrounding areas. Helium isotopes may provide the best indication of deep permeability, fluid circulation, and the possibility of deeper, higher-temperature fluid reservoirs. The project incorporates helium isotopic data into a detailed hydrologic study of a known fault-hosted Basin and Range geothermal system to better understand helium compositions in surface fluids and their relationship to the known geothermal resource. The project also uses the geochemical and noble gas isotope data collected from surface springs, fumaroles, and accessible wells throughout the area to generate a helium

isotope map of the Basin and Range. This map could be used to identify extensional faults with deep permeability that would be most suitable for future exploration projects.

Review Panel Comments

The review panel praised the planning and execution of the project, noting the value of developing the helium isotope database. A few panel members expressed concern as to the ultimate usefulness due to difficultly of analysis and expense. The panel recommended that the data be integrated with other work and measurements be repeated to both fill in data gaps and confirm conclusions.

1. Purpose and Objective (25%)

Research is based on a clear, well-defined physical model relating the observable (helium ratios) to physical process (deep hydrothermal circulation). Approach provides a regional screening tool for active faults that host hydrothermal convection systems.

Helium is one of the unequivocal links that we have to the primary heat source (asthenosphere) in geothermal systems. The PI's approach is a solid effort to provide a very useful first-order exploration tool.

Clear and important.

Helium isotopes were presented as the best, or even only, indicators of deep permeability, and thus possibly deep, hidden geothermal sources. So, this project appears important for geothermal exploration.

Good objective to use this method to map prospective areas. However, it may be impractical for true implementation.

Appears to be a measurement that is unique to the problem of sourcing.

2. Work Plan (20%)

Good, solid science. Logical next step is to analyze fluids from strain-rate "hot spots" identified via InSAR and GPS (to further test hypothesis of association of hydrothermal convection with active faults).

Logical, well-reasoned. Will definitely result in successful completion of the project.

A little difficult to evaluate from the proposal, as the nuts and bolts of sampling and analysis are not covered.

Sound work plan.

Sampling seemed to be focused on areas already identified as high potential.

3. Results (30%)

Excellent results to date.

Quality data that are meaningful immediately. Results have direct relevance to discerning regional trends.

Very interesting and useful. I agree with the PI that this work provides a smoking gun for a magmatic signature. An enhanced regional map is a very useful thing.

Project has been going for 4 years out of 5. Uniqueness of helium isotopes demonstrated. Regional helium isotope map potentially very important.

Seems like good data, producing a semblance of regional map.

Measurements seem to be uniquely identifiable such that is necessary for tracking sourcing.

4. Impact/Technical Merit (15%)

Development of helium database is important work and will have general benefit to end users. PI acknowledges that approach is unlikely to be adopted by industry as a general prospective tool, however.

Industry can use this product now. Technology transfer will be a bit of a problem because the analytical procedures are so sensitive.

Potentially very significant. The difficulty of analysis is a drawback, because it means that this cannot be widely exploited.

Important for identifying potential deep permeability.

Not a practical way to produce a regional map. Prospective areas are already known. Small but prospective areas could be missed. Not practical for commercialization on large-scale use. Very expensive results.

It appears further study to understand "mixing" will be required to provide the major impact to field development.

5. Plans for Completion (10%)

Might add (or refocus) some additional effort in areas of known high strain rate.

Intend to expand helium map.

Two objectives: develop a tool for exploration and create a map of the Basin and Range. Neither of these will be met. The tool is impractical for recon work, and the "map" does not have near enough points to cover the Basin and Range. A good academic study, but it is way too expensive for impractical implementation!

Follow-on study should provide insight into how reliable this tracking of "sourcing," these measurements, can/will contribute to the exploration effort.

Strengths
Method provides direct evidence for deep hydrothermal circulation.

Unequivocal results—a true indicator of asthenospheric mantle influence.

Of the presentation I've seen so far, this one has perhaps the greatest potential benefit to large-scale exploration as it is not based on looking at a given geothermal area and then hoping to extrapolate.

Unique and valuable tool for identifying deep permeability.

Good science, well presented.

Appears to be an identifier that is less prone for error.

Weaknesses
Aside from development and dissemination of helium map, work involves no technology transfer to industry (per PI comments).

Analyses can only be done by a very small number of labs in the country because of equipment and methods requirements.

Likelihood that this will not be a widely used technique.

I don't see any.

How practical is it? Interesting study, but it may not aid overall effort of finding new resources.

Samples taken without good knowledge of structure/obstacles in sub-surface.

Recommendations
Synthesize helium data with other work (InSAR, GPS, etc) to identify active range-front faults with deep circulation to further test physical model for non-magmatic Basin and Range resources.

Talk to some geologists about specific areas to target. It would be useful, for example, to study particular large fault systems along their length.

Repeated measurements in the same locations are needed if this is not being done already (the presentation did not comment on that).

Try to get some data in the "holes" in the map.

Seek sub-surface structure information and recheck conclusions.

Project 3.2 Geothermal Applications of Multi-Gas Geochemistry — **Overall Score: 56** / **Standard Deviation: 11**
CPS Project/Agreement #: 12339
Principal Investigator: Paul J. Lechler
Performing Organization: University of Nevada, Reno

Project Description
The purpose of this project is to expand on the success of using a mercury soil gas survey to trace concealed geologic structures at Desert Peak, Nevada. This project will use a suite of additional gases to characterize and fingerprint geothermal systems, map geologic structures conducting geothermal fluids, and define resource boundaries. The use of hydrocarbon gases, sulfur gases, and radon emanating through soils from subsurface geothermal reservoirs will enable geochemical characterization of geothermal systems of possible magmatic and amagmatic types. Soil gas samples will be collected and processed, and the efficiency of the methods will be analyzed. Once systems are characterized and methods refined, multi-gas surveys can be used to map important geologic structures and define lateral boundaries of these systems. It is anticipated that results of this project will have very direct application to exploration for blind geothermal resources and expansion of existing fields.

Review Panel Comments
The review panel generally agreed that while the work plan is strong, the data acquisition and analysis is unreliable. They recommended that in addition to more data sampling, more robust statistical analyses be performed to determine if results are reliable or simply noise.

1. Purpose and Objective (25%)
Method appears to be best suited for assessing the boundaries of geothermal fields, as well as on-strike continuation of permeable ("leaky") faults beneath alluvial and eolian cover.

The PI makes assertions that the techniques can and will be used to map important geologic structures. The most prominent example of this is made with Hg. Diffusion pathways don't always mimic structural patterns. The menu of compounds that this PI is measuring won't "characterize and fingerprint mangmatically-heated systems..." as the summary explains.

The stated objective is fairly clear.

Idea to look at several gases is potentially useful for exploration.

Good idea and cost-effective method.

Clearly defined, however, appears to be a development tool.

2. Work Plan (25%)
Follow-up work should address the issues of data and analytical reproducibility.

Serious shortcomings in data acquisition. Results may not be directly comparable from year to year.

The data analysis is not convincing because there has been no replication of the analyses. The signal-to-noise ratio is pretty low, and it is critical to demonstrate that the signal you think you see is real. A skeptic would say no.

Work plan for field work is good, but needs to include specifics on reliable data analysis.

Plan is fine, except it doesn't really outline the data procedure, such as processing and interpretation.

Surface/locations appear to heavily influence samples.

3. Results
Results to date are intriguing, but the soil-gas anomalies used to infer faults may not be very robust. Complex fault geometry derived from distribution of anomalies may be an over-interpretation of the data.

As noted above, the results so far are not convincing. It would be difficult to draw a fault map based on the Hg grid alone. Better data analysis might ameliorate this.

Work plan for field work is good, but needs to include specifics on reliable data analysis.

Are the results even real data? He made no effort to substantiate them

4. Impact/Technical Merit (30%)
Not clear if method will be adopted by industry without further testing of data and documenting analytical reproducibility. Application appears to be limited to mapping extent of known resources, rather than finding new resources.

Although the collection technique for Hg was clever, it is not unique. The analysis of the data was second rate, at best. Didn't use best statistical tests to determine correlation, independence, or repeatability. Overall, a very amateurish approach. Plots show normal scatter that is being interpreted as signal.

If the multi-gas analyses give a stronger signal, then this might lead to a new method for prospecting for faults. If not, then this is not a terribly useful technique.

If significant patterns are discovered associated with geothermal sources, the impact on exploration will be important. So far this has not been demonstrated.

Are the results even real data? He made no effort to substantiate them.

Impact appears to be low unless information is coupled with other identifiers.

5. Plans for Completion (20%)
Based on presentation, goals and tasks of remaining work appear to be in flux.

Uncertain exactly where this project is going. Not enough thought has gone into the future.

Difficult to evaluate owing to the major change in techniques.

Reasonable.

In order to see if this is an effective tool, he needs to upgrade the data analysis NOW.

Appears to know what will be required to have this method become a useful confirmation tool.

Strengths
Method may be applicable for identifying blind, permeable structures and mapping the extent of known resources.

Innovative technique.

Potential importance for geothermal exploration if various gases are found to be significantly associated with geothermal areas.

A fast and simple way of mapping for resources.

Not too difficult to collect data.

Weaknesses
Based on material presented, not clear that method is robust and dependable.

This is a geochemical "turkey shoot" with multiple targets that have not been shown to be diagnostic for geothermal systems. Many uncertainties regarding the efficacy of the choices of compounds to be measured. Sulfur in hydrothermal and normal ecological systems has both complex pathways and complex kinetics.

Data analysis. It would be good to bring in someone with expertise in statistics and geostatistics.

Data analysis demonstrated in presentation very weak. Interpretation is rather wishful thinking.

Does the method work? Are they really measuring something, or just noise?

One "fingerprint" does not appear to be sufficient. Maybe 2 or 3 different "finger-prints" that collectively agree would/could prove concept. Also, the weather appears to influence samples.

Recommendations
Repeat surveys to assess reproducibility of results. Perform random sampling analysis of data to assess how robust anomalies are.

This program has little hope of providing anything useful for the future.

Work on statistical techniques for analysis, such as subsampling. It is critical that you demonstrate that there really is a signal in there and not just noise that is being amplified during plotting.

Devise a plan for reliable data analysis, with quantitative estimates of uncertainties and uniqueness.

Needs to repeat data, as in repeat a line from previous year or survey, to check results. Also needs to make sure that the data are not just noise, and to do some form of statistical analysis.

Prior knowledge at interpretation time could be useful for identifying "noise". The knowledge should include faults, etc.

Project 3.3 Geochemical Sampling of Thermal and Non-thermal Waters in Nevada: Continued Evaluation of Geothermal Resources

Overall Score: 77
Standard Deviation: 7.7

CPS Project/Agreement #: 12339
Principal Investigator: Lisa Shevenell
Performing Organization: University of Nevada, Reno

Project Description
The purpose of this project is to update and maintain a database of geochemistry from thermal waters in Nevada, evaluate geothermometers using historical data as well as new site data collected, and identify sites with geothermal potential. Its objective is to obtain geochemical data from springs and wells for which data are not publicly available, or for which the analyses are incomplete, poor, or non-existent. With these data, geothermometers are calculated and a preliminary assessment of the geothermal potential and ranking of the sampled areas is being conducted. Samples are also collected at sites identified by other components of geothermal research as having high potential for

geothermal development to support more detailed work and assessment at those sites. Cold waters are collected as well, as some have shown anomalously high geothermometer temperatures, and may reveal blind geothermal systems in areas that lack surface expressions of the systems. The results are expected to help identify new areas requiring more detailed exploration and support other geothermal exploration projects as they identify additional high priority areas.

Review Panel Comments

The review panel called the project worthwhile and its results useful to exploration. A few reviewers suggested expanding the scope and increasing the effort, and one suggested creating a map of the parameters.

1. Purpose and Objective (25%)

Research uses established geochemical analyses to expand various Great Basin geothermal databases. Products are of general value to explorationists.

This project may not advance technology, but it will certainly provide a solid, useful, working database that will be of great value in geothermal exploration.

The basic purpose of this is clear, and I understand the difficulty of trying to get "legacy" data into a modern dataset.

Objective with clear importance for exploration.

A good project to provide updated maps.

Fits definition of "exploration tool" as well as "development" tool.

2. Work Plan (20%)

Work plan appears to be reasonable with attainable goals.

Plan to date has been adequate. Effort ($) has been relatively low level.

Seems adequate for the task.

Tasks well thought out.

Well organized; lots of data acquired and analyzed.

Cost seems reasonable.

3. Results (30%)

Results to date are a good contribution to knowledge base for geothermal exploration.

Geothermometer comparisons are especially useful. They dispel some widely-held industry myths that needed overhaul for years.

Not particularly surprising, but worthwhile. The analysis of various geothermometers is useful.

Results are good.

Lots of data help to provide coverage that can be filtered; then easier to throw at suspect data. Good project so far.

Results seem to be consistent.

4. Impact/Technical Merit (15%)
Work is basic reconnaissance geochemistry; no new methodologies or analytical approaches used. Analysis of cold springs and fluids from playas is promising. PI did not elaborate on technology/information transfer to industry.

Careful quality control on sampling data, analysis, and reporting will ensure a useful, functional database well into the future.

This result will have some impact on geothermal prospecting. Every state should have a good database like this.

Important basic work.

Could be a good recon tool—project cost is very low compared to others for what it might provide to exploration.

This, used in conjunction with other information, could be very useful to exploration efforts.

5. Plans for Completion (10%)
Work appears to be on track to completion.

Increase budget—increase scope—accelerate schedule.

Adequate.

Adequate plan for completion.

Looks on track.

Develop confidence in data samples.

Strengths
Solid contributions to geochemical databases for use by explorationists.

This is a "bread and butter," grind-it-out kind of project that is the foundation of good science and successful exploration. Determining influence of playas is particularly important!

Compilation of data into a modern GIS database. This leverages old and new data in a strong way.

Much needed, although basic work. Clear connection to exploration goals.

Well presented and documented to panel. Seems like an ambitious project, yet well done so far.

This appears to fit the definition as a potential "exploration tool".

Weaknesses
Work is not specifically targeted toward finding new resources.

Seems rather mundane and unlikely to lead to significant new science without some new ideas that are outside the standard geothermal "box".

None detected.

Utilizing samples from multiple sources could taint results. "Noise" in samples could be characterized and removed and information would be less influenced by bad samples.

Recommendations
The work on cold water springs and masking of geothermal signatures could be particularly important. More effort needs to be put into this database compilation. Work with isotopes of O and H, for instance, is just scratching the surface.

Everything necessary included in plan.

Map up all parameters and look for regional/area trends.

Project 4.1 Targeting of Potential Geothermal Resources in the Great Basin from Regional to Basin-Scale Relationships between Geodetic Strain and Geological Structures

Overall Score: 83
Standard Deviation: 6.2

CPS Project/Agreement #: 12339
Principal Investigator: Geoffrey Blewitt
Performing Organization: University of Nevada, Reno

Project Description
The purpose of this project is to assess the value of using crustal strain rates derived from GPS stations as an exploration tool for high-temperature geothermal systems in the Great Basin. The mapping of strain rates, based on GPS stations mounted in bedrock, offers a

promising new alternative for identifying active zones of crustal extension. The project develops and assesses the value of a custom-designed GPS-based observation system that measures strain accumulation with < 1 mm/yr accuracy at a much more detailed scale (~15 km resolution over ~200 km), the scale of individual faults, basins, and geothermal resource areas. The primary project objective is to develop a new GPS-based system of geodetic tools for geothermal exploration in the largely non-magmatic setting of the Great Basin. This would improve conceptual models of non-magmatic geothermal systems and thus enable the discovery of more exploitable geothermal resources.

Review Panel Comments

The review panel recognized the strength of planning and technical achievements of the project. They noted the importance of identifying areas of high strain rate and understanding the relationship between strain and geothermal activity through GPS monitoring and modeling. Cost and complexity of method probably prohibitive for widespread adoption by industry.

1. Purpose and Objective (25%)

Very commendable, solid science. Refinement of GPS methodology to obtain sub-mm/yr precision within two years is a substantial step toward commercial applicability. Facilitates use of GPS for geothermal exploration.

Technology advance in low-cost continuously recording GPS stations is a big step forward. Will provide (and has provided) basic data to understand strain distribution which is a fundamental factor in understanding geothermal occurrences.

Objectives are clearly laid out.

Custom-designed GPS network (MAGNET) to map strain rates at much more detailed scale than before can be very useful for exploration.

If accuracy is achieved on such small scale, use may increase.

2. Work Plan (20%)

Work is well organized and all milestones completed on schedule.

On target with plan. Good progress to date shows resourcefulness.

Thorough.

Very good.

Well done.
Innovative developments for cost reduction seem to have been achieved.

3. Results (30%)
Results to date are excellent. The PI has accomplished significant technical goals. The planned final phases of work will apply the methodology and data in ways that are directly relevant to geothermal exploration.

Submillimeter results with high precision and accuracy is a huge plus when working with GPS data. High quality results that should form the basis for significant future data interpretation.

Development of the new GPS network seems to be proceeding on schedule or ahead of schedule. Results from the new network are just coming online and look to be reasonable.

Work so far exceeded plan (60 stations instead of 20).

Measurements are consistent; however, how much this will contribute is apparently not proven.

4. Impact/Technical Merit (15%)
General impact will be to identify areas of localized high strain rate that may host active faults and hydrothermal convection systems. GPS probably is still years away from being adopted by individual operators for exploration, however.

I don't know whether industry will recognize importance of these data or not; they may have to be shown. However, that does not diminish their real importance to understanding the relationships between strain and geothermal occurrence.

It is difficult for me to predict how useful this work will be. Good continuous GPS data is proving to be full of surprises, and I suspect that this network will generate its own surprises. The strain modeling is just that, modeling, and therefore it is not certain that it will lead to new insights in geothermal prospecting, although it will probably allow one to rationalize why certain fields are located where they are.

GPS strain rate mapping is potentially important tool for geothermal exploration.

Good that data have been published; other results found (i.e. the amplitude changes at SLID); integration with other projects (Faulds).

This development could be useable by other entities without many changes to the system.

5. Plans for Completion (10%)
Based on past performance, there is reason to believe the PI will complete the proposed remaining work in a timely fashion. Co-investigators are excellent scientists.

Should be able to complete all tasks as scheduled with available resources. Future modeling should be carefully coordinated with knowledgeable geologist working in the Great Basin, i.e. Faulds.

Phase 3 clearly outlined.

Well-designed program in that they figured out a way to make receivers move cost effective and integrating with other data sets.

New technology will further enhance measurements.

Strengths
Refinement and application of GPS specifically for geothermal exploration. Use of GPS to test a model that links occurrence of geothermal systems to areas of high strain rate.

Continuous GPS data are revolutionizing geodetic studies nearly as much as campaign GPS did—they provide a great step forward. I like the approach taken in this project. Completely continuous data are not as important as long-term data acquisition, and this seems to provide a clever compromise. I eagerly await the data.

Very important to map strain rate at scale achieved by MAGNET.

I like the approach of being cost effective. Designed a cheaper, better receiver.

Mechanics are sound. Hardware is readily available; cost is reasonable.

Weaknesses
Cost and complexity of method probably prohibitive for widespread adoption by industry in the near future. Dissemination of results by PI's will mitigate this problem.

None detected.

The use of GPS to make the fine measurements could be easily influenced.

Recommendations
Focus modeling of data in final phases of work on key aspects of the occurrence model (i.e., separating upper crustal fault slip rates from deeper postseismic relaxation, etc).

Monitor hardware improvements and implement as feasible.

Project 4.2 Geologic and Geophysical Analysis of the Desert Peak-Brady Geothermal Fields: Structural Controls on Geothermal Reservoirs in the Humboldt Structural Zone

Overall Score: 79
Standard Deviation: 5.2

CPS Project/Agreement #: 12339
Principal Investigator: James E. Faulds
Performing Organization: University of Nevada, Reno

Project Description

The purpose of this project is to characterize links between thermal reservoirs and individual structural features, to better define the boundaries of the geothermal reservoirs in the Desert Peak-Brady fields, and to elucidate the late Cenozoic 3D strain and stress fields. The project is Phase II of a comprehensive geologic and geophysical analysis of the northern Hot Springs Mountains. Phase I involved detailed mapping, structural analysis of faults and folds, a new gravity survey, and GIS compilation of geologic and geophysical data for a narrow transect between the Brady and Desert Peak fields. Phase II expands the detailed mapping to the remaining critical parts of the northern Hot Springs Mountains, does more thorough stratigraphic-structural analyses, acquires additional gravity data, continues GIS compilation, and adds a micro-earthquake study. Comprehensive analysis of the area has significant potential for characterizing the structural setting (strain and stress fields) and structural controls on geothermal reservoirs in the Humbolt structural zone (HSZ). The project also contributes to applied research areas, including the inventory of existing geothermal resources in GIS context, geologic mapping and fault characterization, and assessment of controls of reservoir boundaries.

Review Panel Comments

The review panel acknowledged the importance of the project and the ability of the PI. Several panel members recommended that project include results from 3D reflect seismic survey and dirt scrap analysis. Another reviewer suggested that the project consider magnetics in the study area. Also, no new technology involved and focused only on known geothermal fields.

1. Purpose and Objective (25%)

Multidisciplinary geologic study to assess relationship of active faults to resources in known geothermal fields. No new methodology; goal is testing a model for association of structure with geothermal resources.

This study addresses one of the most basic questions facing geothermal developers in the Basin and Range: What are the fundamental structural and tectonic controls on geothermal occurrences? The project objectives are straightforward and on target.

The objectives of this project seem consonant with DOE goals.

Integration of geological and geophysical methods to identify features associated with geothermal fields—important for exploration.

Understanding the structural subsurface picture is critical.

2. Work Plan (25%)
Work appears to have been well organized and completed on schedule.

Work plan is well conceived. Questionable need for paleomagnetic work. More emphasis on analysis of strain as expressed in analysis of seismicity. Mapping of field structures in crucial—this is the heart of meaningful interpretation of structural control.

It appears to me that this project is too lengthy given the relatively modest goals—this is just a standard geologic study, but it is being funded at a much higher level than, say, an NSF-funded study (of course, those are usually way underfunded).

Numerous methods combined.

Good idea of integration—maybe move data could have been added to program (free available data).

Geophysical elements could further enhance understanding.

3. Results
Work product is a significantly improved geologic map (database) for the Bradys/Desert Peak area.

Recognition of releasing bend geometries (in known fields) as structural setting will permit extrapolation to new areas that are unexplored/undeveloped.

Results have generally been good—the PI is a solid investigator and seems more in tune with other aspects of the science than many of the others.

Geothermal systems are controlled by step-overs in normal fault systems. PI could pinpoint a possible new geothermal source.

4. Impact/Technical Merit (30%)
Difficult to assess impact. Not known if operators are using data to improve field management or site new wells. Possible insights into relationship between production and fault complexity at Bradys.

No new technology here—this is "dirt-dog" geology at its finest. However, results including interpretation are essential to moving ahead with finding new resources.

A solid geologic understanding of one of these fields, integrated with other datasets, will be an excellent contribution.

Important for discovery of blind geothermal systems. Collaboration with industry.

Provides good mapping in an area and understanding of structure. Gravity could be used better; needs move MEQ; use may even if regional and downsized; use temp data.

5. Plans for Completion (20%)
Essentially done.

Although the plans for completion are readily achievable, they are not visionary. There is an essential element that testing the hypothesis that needs attention. 3D reflective seismic survey-dirt scarp analysis (neo-tectonics). Should include both of these in this project. They are both tools that will yield valuable data.

Close to the end (Dec 2005). Several tasks left—all feasible to complete.

Should provide a good structural focus of area.

Utilizing reflection seismic could substantially improve fault definition at depth.

Strengths
Application of multidisciplinary geologic investigations to test a general occurrence model for geothermal resources.

Breadth of disciplines involved.

A combination of seven geological and geophysical methods to identify features associated with geothermal fields, both existing and potential.

Good mapping and structural analyses.

A sound "exploration" approach. Integration of various data sets was achieved to enhance results.

Weaknesses
Work primarily is focused on known geothermal fields.

None detected.

Needs better integration and use of other data.

Lack reflection seismic to create direct image of subsurface for use in interpreting faults and structural definition.

Recommendations
3D reflective seismic survey—dirt scarp analysis (neo-tectonics). Should include both of these in this project. They are both tools that will yield valuable data.

Acquire more MEQ Integrate with magnetics or at least look at magnetics in the area. See if any temperature/thermometry data (or anything else) is available to integrate.

Include plan to collect and include results from reflection seismic (3D).

Project 4.3 Characterizing Structural Controls on Geothermal Systems in the Northwestern Great Basin through Integrated Geologic and Geophysical Analyses

Overall Score: 77
Standard Deviation: 5.8

CPS Project/Agreement #: 12339
Principal Investigator: James E. Faulds
Performing Organization: University of Nevada, Reno

Project Description
The purpose of this project is to characterize links between thermal reservoirs and individual structural features, to better define the boundaries of the geothermal reservoirs in the Desert Peak-Brady fields, and to elucidate the late Cenozoic 3D strain and stress fields. The project is Phase II of a comprehensive geologic and geophysical analysis of the northern Hot Springs Mountains. Phase I involved detailed mapping, structural analysis of faults and folds, a new gravity survey, and GIS compilation of geologic and geophysical data for a narrow transect between the Brady and Desert Peak fields. Phase II expands the detailed mapping to the remaining critical parts of the northern Hot Springs Mountains, does more thorough stratigraphic-structural analyses, acquires additional gravity data, continues GIS compilation, and adds a micro-earthquake study. Comprehensive analysis of the area has significant potential for characterizing the structural setting (strain and stress fields) and structural controls on geothermal reservoirs in the Humbolt structural zone (HSZ). The project also contributes to applied research areas, including the inventory of existing geothermal resources in GIS context, geologic mapping and fault characterization, and assessment of controls of reservoir boundaries.

Review Panel Comments
The review panel thought this project overlapped with work by this team as well as others. The panel felt that the project will improve understanding of regional tectonics in the Basin but suggested integration with other information such as reflection seismology, structural criteria and geophysics. In addition, the work only characterizes known geothermal fields.

1. Purpose and Objective (25%)
No narrative provided to review panel. Oral presentation by PI emphasized previously published work on regional tectonics of the Walker Lane and northern Basin and Range. Details of planned future work not provided.

This "50,000 ft. view" of structural-tectonic analysis is essential in developing a first-order approximation of where to look for geothermal resources. Faulds and his colleagues have made excellent progress in identifying key tectonic controls.

This project is difficult to separate from the other study by the same PI. This one is more regional in scope but closely related. The projects actually complement each other pretty well.

Clearly defined paths the program will undertake. Clearly an "exploration" approach.

2. Work Plan (25%)
Difficult to evaluate. No detailed work plan for Phase III provided. Oral presentation emphasized previous work.

Basic methods that make sense. It is a little surprising that there isn't a stronger seismicity analysis component in this work.

Earlier phases have shown good results from present approach.

3. Results
Results of previous phases have been synthesized as general observations about the regional tectonic and structural geologic controls on geothermal resources in Nevada.

Lots of overlap with project 4.1 (Blewitt). Insights are trying to push the state of knowledge, but fall short. Salt wells and Fly Ranch/Gerlach efforts show promise as analogs for other unknown systems, but need a technical boost that may come from use of reflection seismology and/or neo-tectonic analysis.

This has been an interesting synthesis of structural relations in the Great Basin.

4. Impact/Technical Merit (30%)
Characterizing structural controls on known fields is a valuable contribution. Research would have more impact on exploration science if future phases build on existing work by applying criterion to frontier areas.

Some good new information—some rehash of old stuff. Lots of overlap with project 4.1 (Blewitt). Insights are trying to push the state of knowledge, but fall short. Salt wells and Fly Ranch/Gerlach efforts show promise as analogs for other unknown systems, but need a technical boost that may come from use of reflection seismology and/or neo-tectonic analysis.

This work gives a framework for understanding some of the resources of the Great Basin.

Could be very useful if conclusions are based on integration of all the data.

It appears that fault definition at depth may be very important in determining long-term potential. Surface and near-surface certainly can serve as a strong guide for new "exploration" areas.

5. Plans for Completion (20%)
The final product of this work is vague. Existing fields in the study area already are treated as exploration analogs. PI did not make a strong case that a new synthesis of these known fields will aid exploration for blind resources in other areas.

Not enough detail provided to judge whether they will/will not meet objectives. Based on what has been done to date, they should.

Well organized approach. Lacking reflection seismic to add to results.

Strengths
Work will improve understanding of the tectonic and structural settings of geothermal resources in the Walker Lane and northern Basin and Range.

Innovative PI who is good at synthesizing diverse datasets and bringing in other people.

Good plan for overview of area. Could be design for other regions. Integration of data.

This is an "exploration" tool.

Weaknesses
Work apparently will characterize known geothermal fields, for which much is already known. Incomplete documentation provided for assessment.

Lacking reflection data for subsurface definition.

Recommendations
Use tectonic and structural criteria developed in previous phases to identify potentially prospective areas.

Need to add reflective seismology-neotectonics-seismicity tectonic analysis—cooperate with regional strain folks.

Look for all other existing data in area, especially geophysics, and integrate into program. There are a lot of data available for free from USGS and other universities (Colorado School of Mines, for example).

Seek impact from reflection data.

Project 4.4 Assembling Crustal Geophysical Data for Geothermal Exploration in the Great Basin

Overall Score: 75
Standard Deviation: 14

CPS Project/Agreement #: 12339
Principal Investigator: John N. Louie
Performing Organization: University of Nevada, Reno

Project Description

The purpose of this project is to assemble a three-dimensional reference model of crustal seismic velocity for the western Great Basin region of Nevada and eastern California. This continuing project focuses on refining a geophysical model of the western Great Basin and making it available to industry and other groups conducting regional exploration and assessment. The seismic velocity model consists of simplified, rule-based representations of some of the region's crust to 50 km depth, and more detailed characterization of geothermal areas and sedimentary basins. With the more complete sampling of the crustal geophysical characteristics of geothermal resources in the Great Basin resulting from this study, geophysical measures can contribute to quantitative analyses of the associations between different geophysical parameters. Parameters such as crustal thickness may serve as regional geothermal indicators. The development and successful testing of new, cost-effective regional seismic-surveying technology will have a major impact on regional geothermal-assessment projects

Review Panel Comments

The review panel commended the approach and goals of the project. The panel generally agreed that the results to date have been valuable but several suggested that they be compared to other data and refraction and receiver function studies. One reviewer recommended trying different inversions or modeling of data. In addition, the product has only marginal application to exploration.

1. Purpose and Objective (25%)

Good, solid science (should be funded by NSF). Only marginal application to geothermal exploration, however.

Well stated. Clearly tied to the most basic precept in geothermal system formation—the asthenospheric mantle as a heat source. Mapping topography on the Moho will be extremely valuable in relating geothermal system occurrence with thinner crust.

Objective is to compile a Great Basin-wide map of crustal thickness. This sort of compilation has not been done in a while and is worth doing.

Obtaining more detailed map for crustal thickness is potentially important for geothermal exploration.

Very good approach (plan).

Plan clearly stated and documented as completed.

2. Work Plan (20%)
The early (higher-cost) phases of work were dedicated to establishing a crustal refraction facility at UNR. Again, this is only marginally relevant to geothermal exploration and development. Appropriate use of DOE exploration funding?

Schedule is a bit protracted, but not unreasonable given the need to acquire in-house instruments. Subsequent progress has been satisfactory.

This has been a very ambitious program. Running large-scale seismic experiments is quite a task.

Detailed and well thought plan.

Good use of equipment and resources. Could use some integration with other deep crustal work.

Work plan appears to have been met.

3. Results (30%)
Preliminary versions of the crustal thickness map are intriguing and provocative. Results need to be tested against independent geologic and geophysical data.

Highly successful in getting quality results using refraction "shots of opportunity." Preliminary Moho map is promising and incorporates much new data.

This project is contributing a lot of useful new data. I am a little worried about the interpretation, or should I say overinterpretation. Comparison of refraction and receiver-function studies would be worth pursuing, as there is confusion about which technique is more suitable for this task.

Several important results already.

Good results, which should improve.

To make existing data relatively quick to access is significant and may entice wider usage as this will be put into a system for added access.

4. Impact/Technical Merit (15%)
The work has intrinsic scientific merit, but only marginal utility to geothermal exploration. The product (crustal thickness map) will not be used as a first-order exploration tool.

This will be a very valuable product and industry should use it. It is a simple, but elegant, product that attempts to characterize a key geothermal system indicator.

Although this project will likely not lead to information that can be used directly to prospect for new fields, it will produce important data for regional interpretations.

Located a specific site of interest to exploration in an area of thin crust.

Lots of papers published and impact on students.

Information is available for context of programs that are underway.

5. Plans for Completion (10%)
Based on results presented to date, it appears that the final map will be completed on schedule. PI states that scope of project does not permit use of independent data to test/refine map.

On target. On budget. In the "payoff" years.

Solid.

Clear plan for completion.

Apparently on track—would be nice if they could show integration of the 3 datasets, as well as with other data.

Time window appears to be reasonable.

Strengths
Excellent science that contributes to understanding of lithospheric structure and dynamics of geothermally productive areas in the Great Basin.
Solid scientific effort from a highly capable PI. The work is thoughtful and relevant.

Modern, clever approach to an old problem.

Important topic, cost-effective project, publications, extensive involvement of students.

Excellent resource use, phones and sources.

It is an "exploration" type tool. Integrated multi-data volumes. Utilized students for cost effective field work.

Weaknesses
Product has only marginal application to exploration. Local relief on Moho shown on map needs to be tested against independent data.

None detected.

Restricted to refraction generated velocities. Should be even higher value with reflection velocities.

Recommendations
Adjust scope of remaining work to include tests of map with independent data.

Give some careful thought to how data should be contoured and analyzed.

Show comparison of lines. Try different inversions/modeling on data.

Integrate reflection seismic velocities.

Project 4.5 Seismic Imaging

Overall Score: 51
Standard Deviation: 12

CPS Project/Agreement #: 11177
Principal Investigator: Ernest Majer
Performing Organization: Lawrence Berkeley National Laboratory

Project Description
The purpose of this project is to extend and adapt current seismic imaging techniques (passive and active) to improve the capability for enhanced imaging of subsurface structure to explore for and develop geothermal resources. This work seeks to identify and apply cost-effective seismic imaging methods to locate the zones of highest potential production and faults controlling subsurface fluid flow. This is important for both production and injection activities, as it will allow more accurate well placement in current and future geothermal fields. Single and multi-component, 2-D, 3-D, and 4D seismic imaging methods for geothermal application will be considered, as well as well bore technology. The project is being carried out in close cooperation with geothermal operators at actual sites of application of seismic methods. Improved understanding of the subsurface will allow identification of new resources in other areas with different geologic settings, optimal well placement will reduce cost, and the characterization of current and future geothermal resources will increase the amount of geothermal energy.

Review Panel Comments
Several of the reviewers were concerned that the project was directed to a specific site and would not have broad application or benefit. Others felt that the project was not following its stated objectives. However, reflection can be critical for understanding subsurface faulting. The panel generally recommended that either the project scope be broadened or the project be discontinued.

1. Purpose and Objective (25%)
Work is narrowly focused on improving reflection data quality for a single geothermal prospect. General applications poorly addressed by PI. Appears to be a DOE-funded consulting project for the benefit of the company developing Hot Sulfur Springs.

Objective doesn't match what they're doing. This is a site-specific, client-specific effort that will likely not have benefit to industry as a whole.

The objectives of this project do not seem to be particularly related to the goals of the exploration program.

Useful for well placement in already existing geothermal filed, but I do not see how this project relates to exploration of unknown potential fields.

Good purpose and objective but not followed.

Reflection data was and is critical to success.

2. Work Plan (20%)
The work plan appears to be driven primarily by the needs of the company developing Hot Sulphur Springs, rather than making a general contribution to exploration science.

Poorly defined based on what the prospect developers can do financially.

Reasonable, for this project.

Good overall plan in view of intended work.

The plan is too focused on one area and a problem that may not be common to many areas—nothing new.

3. Results (30%)
The work to date has succeeded in producing a migrated reflection image that looks like the conceptual model. The PI has not convincingly shown that additional data collection and analysis will improve the imaging.

Next to useless so far, and little hope for much better in the future.

Rather unexciting with regard to exploration. This seems to have potential as a tool for resource management, but I cannot see how it would be used for exploration.

New project.

Doesn't contribute anything—you could tell without modeling that this method would not be economic exploration.

2D imaging was fairly successful. Interpretation of the results was representative of the reflection data.

4. Impact/Technical Merit (15%)
This work is so narrowly focused on the problems of a specific area and specific seismic data set that any broader impact for geothermal prospecting in the western U.S. appears to be extremely limited.

Wrong tools! Problem is intractable using the tools and techniques suggested by the PI. The entire approach is eclectic and not well reasoned.

This seems to have potential as a tool for resource management, but I cannot see how it would be used for exploration.

I see little relevance to exploration.

Nothing new.

Modeling can be helpful in correctly imaging the reflection seismic data.

5. Plans for Completion (10%)
Plans for 2006 are extremely vague and apparently depend on the whim of the company developing Hot Sulfur Springs and another property in Nevada.

Poorly defined based on what the prospect developers can do financially. Using the wrong tools and techniques, not a well-reasoned approach.

Plan follows well logics of project. However, I cannot see the value for exploration.

Overall plan is probably adequate—details will need changing.

Strengths
A laudable goal of the work is to improve seismic imaging of faults and fractures in geothermal fields.

Close connection to industry.

Reflection data can be the critical element for understanding subsurface faulting. An "exploration" tool.

Weaknesses
As presented by the PI, this work does not have broad applications to geothermal prospecting.

Value for existing fields is clearer, but value for exploration is not.

Nothing new—not an exploration effort.

Noise recorded in the reflection data was not sufficiently handled. Using a minimum configured system is a severe hindrance.

Recommendations
Do not fund proposed FY06 – 07 work.

This project is a waste of precious financial (DOE) resources on an approach that is ill conceived and likely to fail.

The project should be broader in scope to contribute more to overall geothermal effort, not just one area.

1) Use longer off sets of sources relative to receivers. 2) Use recoding system that allows geophase arrays. 3) Consider using surface source.

Project 5.1 Data Fusion for Geothermal Exploration: The Stochastic Engine — Overall Score: 60, Standard Deviation: 8.9
CPS Project/Agreement #: 12321
Principal Investigator: William Foxall
Performing Organization: Lawrence Livermore National Laboratory

Project Description
The purpose of this project is to adapt the stochastic engine to jointly invert multiple exploration data sets to better define drilling targets and improve success in exploration for economically viable geothermal resources. The stochastic engine utilizes Bayesian inference implemented with MCMC sampling. This modeling method used in geothermal exploration enables more complete analyses of existing or new disparate data sets that will yield model solutions that are consistent with all the data, and hence more accurate. Improving the data on which to base well siting reduces the number of non-productive wells that are drilled, and will therefore help to achieve the DOE goal of a 40% success rate in finding economic geothermal resources.

Review Panel Comments
The review panel agreed that the project objectives have merit, particularly for risk management, but felt that the tool would not be useful for exploration because it is limited to sites where features are known. The panel recommended a demonstration of the stochastic engine either in a real world setting or through a comparison to conventional methods to determine its value.

1. Purpose and Objective (25%)
Use of existing LLNL probabilistic software ("Stochastic Engine") may be useful for reservoir modeling and evaluating risk. Application to pure exploration could be more difficult and comes with a steep learning curve for the industry.

Stochastic modeling results are commonly used in the risk-management decision-making process. Industry will likely be interested in the results.

This seems like a pie-in-the-sky project; the ultimate goal is laudable, but it is very difficult to see how one would get from our present state to the state necessary for the intended usage of this product. It is also not clear how this relates to exploration as opposed to resource management.

Promise to more fully and accurately analyze multiple data sets, including uncertainties involved, can be of great value to exploration.

A noble idea, but other work is being done in this area.

Should use this process to develop an all-inclusive model. It would probably make a noticeable impact.

2. Work Plan (25%)
Work plan appears reasonable to achieve desired objectives.

This, like so many other projects, is going to be dragged out over 3 years. It needs to be shortened to two years so results can be tested in the real world sooner.

Adequate.

Sequence of conceptual framework, synthetic data testing, and applications to real data at Dixie Valley appears a logical plan. However, test is necessary in unexplored areas.

Needs to be tailored for exploration—different inputs, unknown structural models.

It appears to stretch algorithm capabilities from a realistic approach.

3. Results
Work to date has focused on developing a conceptual framework and parameterizing a model of the Dixie Valley system. The Dixie Valley study will test applicability of SE for reservoir management, however, not pure exploration.

4. Impact/Technical Merit (30%)
Adoption of methodology by industry for exploration will require a clear example of this analysis applied to a frontier region in addition to a well-studied geothermal field.

This is not an exploration tool, per se. It is based on the availability of data sets, a priori, that likely will not exist in the exploration stage.

We are some years away from finding a use for this product, as was clear from the questions that followed the presentation. I do not deny that the approach is worth a try, but, as stated above, it is difficult to see how one would apply this to exploration.

Important for developing exploration strategies. However, application in this project is only related to an existing site.

Impact potential is unknown because of the high chance of not being successful in closure of the modeling software work on diverse characteristics.

5. Plans for Completion (20%)
Trajectory of project is reasonable for demonstrating utility for reservoir management. Applications to exploration not explicitly addressed.

They have laid out a plan, but as I stated earlier, it needs to be done sooner rather than later. Their plan is basically OK.

Reasonable.

Application to both synthetic and real data is planned. However, real data include only an existing sites, i.e. value for exploration may not get demonstrated convincingly by the end of the project.

Too many unknowns appear to be utilized.

Strengths
Application of state-of-art probabilistic methods to the geothermal industry. Can be applied to risk management.

Offers a range of alternative solutions that are tested against real data.

Intends to extract most accurate information from multiple data sets, in a data fusion scheme, and evaluate uncertainties.

Can be commercialized.

The modeling software is robust, but the application of this algorithm may be difficult with multiple unknowns.

Weaknesses
Industry has to buy into methodology; as presently formulated, work will not provide a clear example of how SE can be used for exploration.

Takes too long to get to the endpoint. Not likely to be applicable in exploration for geothermal resources.

Applicability to real-world problems. This money could be spent more profitably.

Idea is interesting, but I am not sure that results will indeed reveal more information relevant to exploration than those obtained the usual way.

How applicable is it to numerous geothermal exploration projects?

Although modeling can help in matters where the information is understood, modeling of information that is discontinuous is questionable. This is not an "exploration" tool as presentative because the lack of critical parameters required to "start" the model.

Recommendations
Provide an example of how an exploration prospect can be evaluated using SE, with emphasis on how models are parameterized.

Shorten by one year.

Come up with a real-world demonstration. Reproducing the "truth model" is not a real-world test, and the resistivity example shown is far, far simpler than a geothermal system.

Provide specific comparisons of results from the application of the stochastic engine (SE) and standard results to demonstrated superiority of SE.

The synthetic model should be run with only parameter bounds and not model concept to see if the result will produce the structure.

Start with fewer "like" parameters, and if successful, expand.

Project 5.2 Revival of Grass-roots Geothermal Exploration in the Great Basin (where to look for new geothermal fields)—A New Approach to Assessing Geothermal Potential using a Geographic Information System- Parts IV and V

Overall Score: 85
Standard Deviation: 3.0

CPS Project/Agreement #:12339
Principal Investigator: Mark Coolbaugh
Performing Organization: University of Nevada, Reno

Project Description
The purpose of this project is to generate new exploration targets for high-temperature geothermal systems by analyzing regional data in a GIS. Geothermal potential maps will be created with spatial statistics, including weights of evidence, logistic regression, and other techniques. In the process of the spatial analysis, correlations will be identified that in turn yield clues as to which geological conditions are most important for forming high-temperature geothermal systems. The spatial statistical analyses help improve and quantify understanding of the factors controlling the location of geothermal systems in the Great Basin. Ultimately a geothermal supply curve for the entire Great Basin could be created based on a comprehensive data set of geothermal systems and their estimated subsurface temperatures.

Review Panel Comments

The review panel lauded the purpose and objectives of the project, noting the value of GIS to exploration. The panel agreed that the database would have broad application. They recommended integration with other databases. But, the "geothermal favorability" analysis is less strongly predicated on physical models of heat transfer and development of hydrothermal convection systems.

1. Purpose and Objective (25%)

Use of GIS to find empirical relations between geothermal occurrences and various geological and geophysical spatial data.

The cornerstone of any exploration program is the ready availability of a comprehensive, flexible database. This particular project hits that target precisely.

This project provides an excellent basis for exploration. Spatial analysis is a powerful tool and this project seems to use it well.

More efficient exploration and explore for new fields. Spatial analysis within GIS—predictive maps of geothermal potential.

Very good.

Information gathering is generally a time-consuming effort. To gain access to—and use of—critical information is extremely valuable.

2. Work Plan (20%)

Essentially a continuation of previous multi-year study.

The team is following the work plan very closely. They have a well-conceived, directed work plan that they execute in a timely way.

Strong.

Supply curves. Geothermal favorability maps. Multiple geothermal environments. Statistical analysis (cluster analysis) of hot spring characteristics. Identification of grass-roots targets. Website.

Very good.

Projects points to "exploration" oriented project use in harmony with field development.

3. Results (30%)

Results to date have significant value for focusing exploration efforts on areas with relatively high geothermal potential in Nevada.

Quality of products and thought behind the preparation of those products is first rate. This project is not a self-serving effort; it has spawned a number of other very useful projects.

This project is far enough along that it is time to really put the methods to the test. This is beginning but is in its infancy and needs to be a primary focus.

Fourth year of a five-year project. Seven-layer density function model—gravity, GPS and fault strain, temperature gradient-hybrid maps, drill hole maps, depth to water table, regional aquifers. Predicted density of undiscovered geothermal systems.

Cross discipline integration of critical information can/may be a significant benefit to exploration as well as field development.

4. Impact/Technical Merit (15%)
Work provides a valuable database and data framework to support exploration research and development of exploration strategies in Nevada.

This is a model effort for geothermal exploration database development and utilization. Data analyses show creativity and thought regarding directed exploration.

Potentially very high impact, especially if the project continues to interact with other projects (structural geology, strain analysis, geochemistry, etc.).

New exploration tools, identification of previously unknown geothermal sources.

Should provide some great information for future work, especially if updated as new data are received.

Access to information is critical in any exploration or development programs. Making information available in interactive mode improves utilization of information.

5. Plans for Completion (10%)
Goal of developing specific target areas in FY06 – 07 is laudable.

Specific plans are not provided. Based on historical effort, there is little doubt that they will make progress. Need to factor in amalgamation of other goescientific databases into this library.

Detailed future milestones. Walker Lane (strike-slip), Great Basin Interior (normal faults). Several targets.

What appears to be solid performance in the past seems to be an encouragement that it will happen on schedule.

Strengths
Database product is extremely valuable for exploration research and vectoring exploration efforts. Statistical analysis of correlation among datasets also is very valuable.

Synthesis of several different fields. My sense from many other presentations is that the PIs are working on little research islands, isolated from others. This project really seems to be synthesizing a lot of diverse datasets.

Strong connection to exploration.

Great idea—appears to be useful approach for compilation.

Has not tried to reinvent, but has effectively utilized existing information and has made the system easily updated—adaptable to both exploration and/or development efforts.

Weaknesses
Much of "geothermal favorability" analysis is based on establishing empirical and statistical relationships. Analysis is less strongly predicated on physical models of heat transfer and development of hydrothermal convection systems.

None detected.

Concern of long-term adaptability of exclusive software/system to computer hardware/change. Ongoing transfer of expertise/use may need increase effort to prevent reinventing in future.

Recommendations
Consider expanding this effort to California, i.e. other geothermal target areas. Closer tie with other academically-based scientific databases, i.e. NAVDAT, Zoback's stress database.

Carry on! But please throw away the silly PowerPoint backgrounds—they are extremely distracting.

Integrate with other similar databases.

Has imitated oil and gas information inclusion; additional effort should be pursued.

Project 5.3 Dating of Young Igneous Rocks Associated with Geothermal Systems in the Great Basin

Overall Score: 69
Standard Deviation: 7.9

CPS Project/Agreement #: 12339
Principal Investigator: Greg Arehart
Performing Organization: University of Nevada, Reno

Project Description

The purpose of this project is to undertake a detailed examination of the relationships between geothermal systems and young spatially-associated igneous rocks. Ar-Ar dating will be conducted on selected volcanic rocks throughout the Great Basin that are thought to be potentially Quaternary in age. By clarifying the space-time relationships between volumetrically minor Quaternary volcanic eruptions and on-going geothermal activity, it may be possible to better understand why they are spatially related, and determine whether that relationship is primarily based on a shared structural control or on a deep magmatic heat source for geothermal activity. The answer to this question will have a significant impact on future exploration for non-magmatic geothermal systems in the Great Basin and elsewhere.

Review Panel Comments

The review panel identified the project as providing basic data acquisition that will add to existing data resources. The panel noted that the project is behind schedule due to some technical issues. Also, the rationale for collecting samples was not clear. Some panel members recommended possible expansion of the project and increasing the sampling.

1. Purpose and Objective (25%)

Straightforward data-collection project to assess relationship between young volcanism and geothermal occurrences in the Great Basin.

The data they are acquiring will fill some gaps in the database. Industry will use the data. Objective is logical and relevant to DOE goals.

Objective is clear.

Ar-Ar dating for improving timing of Quaternary volcanism. Exploration connection—presence of young basaltic volcanic rock (<1.5 Ma) is apparently correlated with extensional geothermal systems in the Great Basin.

Good idea.

Could be a secondary confirmation of potential geothermal sites.

2. Work Plan (20%)

Two-year project with moderate funding level. Sample acquisition and testing follow established protocol.

This is way behind schedule. Not atypical for radiometric dating projects—not an excuse, just a fact. Need to work harder on fixing the technical problems that have caused the project delays.

Collect 24 samples, obtain Ar-Ar dates. Two-year project, no continuation intended. DOE funding component small. Clear work plan.

More areas should have been sampled; more consistency needed in sample minerals.

Conscience direction was/appears to have been successfully followed.

3. Results (30%)
Age data have intrinsic value for understanding geothermal occurrences in the Great Basin.

Moderate quality so far. There needs to be better integration of existing geochronologic data, i.e. NAVDAT.

Behind schedule, not the PI's fault. But I wonder why they tried to date some rocks they know are too young to date.

Samples collected in proximity to known or suspected extensional geothermal systems. Data so far confirm that there are several basalt vents in the Carson Desert that are Quaternary.

Results seem disappointing—more data are needed to make this useful.

Some successful identifications of key items.

4. Impact/Technical Merit (15%)
Work will contribute to geothermal database being compiled at UNR. No new methodologies; no direct application for geothermal exploration. Marginal applicability to industry.

Results will add to the geochronologic database, which is always useful. However, this is one very small piece of a much larger effort being conducted by the scientific community as a whole.

This will not have a large impact, but the data will be useful to geothermal efforts and to others.

Work can have a significant impact on future exploration for non-magmatic geothermal systems in the Great Basin.

It will be hard to draw conclusions with so few data.

Uncertainty appears to exist in this community of contribution of this action to the active Exploration program.

5. Plans for Completion (10%)
Essentially complete, except for delayed receipt of lab results.

Needs work! Get the technical issues resolved.

Reasonable.

Pending Ar-Ar results from six areas. Project will be completed by September 2005.

Behind schedule; not great results for $.

Program appears to be in final phase with no future pursuit.

Strengths
Data will test empirical association of geothermal occurrences and Quaternary volcanism.

Clearly stated, specific, small, focused project.

Good idea.

Appears to be an "exploration" tool.

Weaknesses
Project primarily fills in "data gaps" rather than explicitly advancing exploration science. Timely reporting of project results dependent on external labs to produce results.

Rationale for collecting what you did is not real clear.

None detected.

Undersampled. Not well planned.

Equipment requirements are/appear to be major.

Recommendations
This is, and should be, a one-year, stand-alone effort.

Tie into regional patterns to see how these areas fit in and what you might expect.

Get more samples.

After final results are received, evaluate expansion of additional areas for sampling.

Project 5.4 Developing and Updating Techniques and Databases for Geothermal Resource Assessments

Overall Score: 57
Standard Deviation: 15

CPS Project/Agreement #: 11184
Principal Investigator: Colin F. Williams
Performing Organization: U.S. Geological Survey

Project Description

The purpose of this project is to develop new techniques for geothermal resource assessments in order to characterize the extent to which they can contribute to the increasing demand for electric power. Working with state and local agencies and industry, relevant data from past geothermal and exploration activities will be collected, analyzed and incorporated in a comprehensive database as a first step towards producing a new national geothermal resource assessment. New assessments will present a detailed estimate of electrical power generation potential and an evaluation of the major technological challenges and environmental impacts of increased geothermal development.

Review Panel Comments

The review panel agreed that the project is both too expensive and too long, though its objective is of value. The panel further agreed that the project is very important in terms of policy and future planning, and that it either needs to be simplified or integrated into other related projects.

1. Purpose and Objective (25%)

Assessment of geothermal resource base in U.S. is useful for federal and state policy makers. Does not directly contribute to exploration science or finding new resources, however.

The objective speaks to the highest-level DOE objective of "greater use of geothermal energy," and it does not address the "supply curve" exploration objective. However, this very expensive, protracted effort takes valuable money away from real exploration projects.

This project is generally worthwhile, but it is difficult to justify taking money out of the exploration budget to fund it—especially when the chunk of money is this large.

Important work, but the funding is so large and the project seems so protracted, that it would be better if this project is outside the exploration program, in a category of its own.

Needed update.

Not for "exploration" improvement.

2. Work Plan (20%)
Ample time in schedule to accomplish goal of completing national assessment by Sept. 2008.

Too protracted!

Work plan spreads this work out over a long time span that seems unnecessary.

All three tasks (revisit and revise assessment methodology, establish national database, and develop new classifications) are very important for the future of geothermal energy.

Overwhelming—isn't it most important to just get a new assessment? Very expensive!

Information availability improvements through this plan appear questionable.

3. Results (30%)
Difficult to assess. First year of multi-year project.

None, to date, of any value. Identifying questions that need to be addressed is not sufficient progress.

Not much.

On the outside it seems as if the work progresses too slowly.

Doesn't contribute anything—you could tell without modeling that this method would not be economic exploration.

Results from supposable use of database and assessment efforts do not appear to support results in sufficient strength to allow firm or near-firm, supportable answers sought through the use.

4. Impact/Technical Merit (15%)
Primary impact in political arena where energy policy (i.e., funding) decisions are made. Only indirect impact on exploration per se. Intrinsic merit in refining assessment methodology.

Improving resource assessment technologies is very important. Also coping with the "undiscovered" resource part of the equation.

It seems clear from previous attempts to quantify geothermal resources that the numbers people come up with don't mean a whole lot. This money would be better spent on exploration.

Important basic project for all geothermal issues.

This would obviously be a good and needed product, if it can be delivered in a timely manner.

Questionable results based on presentation.

5. Plans for Completion (10%)
Appears reasonable.

Time, time, time…is money...is of the essence…etc.

Sounds like a real bureaucratic mess to me.

Final results within three years. Depending on approval of appropriations, project value for exploration in three years is not clear.

Do they really have a clear focus and achievable goal? May be difficult to complete such a daunting task when good supporting data are not necessarily available. Expensive.

Track record does not appear very solid.

Strengths
Intrinsic merit in updating state of knowledge regarding geothermal resources in the U.S., as well as improving assessment methodology.

Very important work in terms of policy, future planning, etc.
Obviously, the USGS is one good way to go with project.

History documentation in multiple states. May be for use at budgeting level.

Weaknesses
Does not directly contribute to exploration science or finding new geothermal resources. Does funding of national assessment through the exploration program prevent more relevant projects from being funded?

Looks like a lot of money put toward yet another assessment that will probably miss the mark by a wide margin.

Appears work has progressed too slow and somehow disconnected from the other projects.

Timing, costly, just seems like it will get bogged down.

Not a major element for "exploration."

Recommendations
This should be funded from a special, earmarked pot of money that represents a plus-up of the DOE geothermal budget. It has broader policy implications for the U.S. DOE. $3.27 million!

Divert money to exploration.

Again, it seems too protracted and too expensive.

Focus on a task, such as just the assessment, and get it done.

Integrate this information into system described in paper #17 and drop separate efforts unless mandated by other directives.

Project 5.5 Application of Thermal Techniques for Exploration, Evaluation, and Assessment of Basin and Range Geothermal Resources

Overall Score: 72
Standard Deviation: 11

CPS Project/Agreement #: 12004
Principal Investigator: David D. Blackwell
Performing Organization: Southern Methodist University

Project Description
The purpose of this project is to decrease the cost of development of geothermal systems in an extensional setting such as the Basin and Range province in the Western United States by reducing the number of wells drilled. An exploration oriented model of the structure (i.e. the reservoir) of normal fault related geothermal systems will be developed, in addition to a model of transient fluid and heat flow in extensional systems giving a 4-dimensional understanding of these systems in their natural state. New information and any new publicly available thermal data will be made available on a timely basis in a database of geothermal thermal gradient well sites that the SMU Geothermal Laboratory operates to serve as a resource for the potential geothermal developers and governmental resource assessors. Regional and national maps that focus on various aspects of Geothermal Energy, conveying information in easily accessible forms, will be produced.

Review Panel Comments
The review panel found that the database generated by the project has value as an exploration tool. A few cautioned that the PI had not differentiated between the database work and the structural interpretation as outlined in the project plan. Most reviewers wanted to see continued updating of the database and integration with other data sources.

1. Purpose and Objective (25%)
The PI's thermal database is very valuable and its update and maintenance should be funded. He has not shown that what he has done in structural interpretation meets the program objectives.

Consistent with the program.

All tasks important for exploration—create exploration structure model for geothermal systems associated with normal faulting. New data for SMU database. Model transient fluid and heat flow in extensional system.

Exploration tool.

Stated goals are to develop general models for fault-dominated systems based on study of analogs (.e.g., Dixie Valley), improve BHT database, etc. Very similar to past work by PI in this area. Not clear what is significantly new.

2. Work Plan (20%)
There is not enough information to evaluate the work plan. It is very poorly addressed by the PI.

2003 – 2005. Maintain and update website. Geothermal resource maps requested by users.

Consistent for "exploration effort".

Difficult to assess. Presentation dwelled on database development; no significant discussion of other project goals.

3. Results (30%)
Once again, a distinction must be made between the thermal database and the structural work that the PI wants to do. He is not a structural geologist, nor is his co-investigator. It shows in their work. Yet another model of Dixie Valley structure. Why? This one was stolen from another source. It is not the PI's original work.

Both the modeling results and the geothermal map seem to be useful components of the program.

Maps of heat flow, temperature at depth, thermal gradients for geothermal potential. Numerical modeling.

More results than anticipated.

Integration of multiple information sources is utilized. Utilization for early stages of "exploration" has been underway by third-party entity.

Results presented focus on expanding BHT database development. No significant progress in evaluating fault (extension) dominated systems discussed.

4. Impact/Technical Merit (15%)
The database is valuable, but the work he has done in structural interpretation has not been shown to meet program objectives

Refining regional heat flow data is an important project (although some people seem to have given up on heat flow owing to problems with advection). I think that this part of the project will have lasting benefit. I know less about the modeling of Dixie Valley, which is, of course, model dependent.

One company used their prognosis and did find resources. Working with industry.

Very useful.

Now partially "market force" utilized.

Difficult to assess impact on exploration science based on material presented. Database has intrinsic merit, but only indirect impact on exploration.

5. Plans for Completion (10%)
There is no timetable—no way to assess this. Need to let this project expire. If there is a desire to fund the database work, then do it under a different mantle.

Complete entry of AMEX data over 1500 well sites. Complete temperature maps.

Further use by third-party entities may promote further activity.

Not clear how goal of addressing fault-dominated systems will be accomplished between now and end of project period.

Strengths
Rigorous update of database, available to everyone. Link to ore deposits (something I haven't seen from other PIs).

Very valuable database.

Great databases and comprehensive.

Exploration tool and/or development support tool.

Work has both intrinsic merit and application to geothermal exploration. SMU database and maps are very important contributions.

Weaknesses
None.

Very difficult to assess goals and progress based on material presented.

Recommendations
Further merge with other data volumes applicable to prospect should be pursued.

Project 5.6 Exploration Statistics

Overall Score: 70
Standard Deviation: 8.5

CPS Project/Agreement #: 17466-10962
Principal Investigator: Joel Renner
Performing Organization: Idaho National Laboratory

Project Description
The purpose of this project is to improve the current 20% success rate for finding economic geothermal resources at previously un-drilled sites to a 40% success rate. The project will evaluate past exploration activity to determine a verifiable success rate for geothermal exploration. Historic files will be reviewed and early explorers interviewed for information on exploration targets, exploration activities, deep production drilling, and success rates of drilling. The Geothermal Technologies Program will utilize the verified success rate in planning exercises.

Review Panel Comments
The review panel noted that the project would have little impact on exploration science but would be of value to exploration decision-making. The panel was satisfied with the approach and plan for this limited duration project.

1. Purpose and Objective (25%)
Goal of bringing more rigor to assessment drilling success rates in geothermal exploration is commendable. Presumably an important goal of DOE policymakers. Very marginal impact on exploration science, however.

It is good to know this project is being done, even if it is after the fact. It will be very useful.

Determine historic success rate for geothermal exploration. Verify that success rate is really 20% (as DOE states). A lot of these assumptions are anecdotal.

Driven by DOE—not much to comment on.

Historical perspective.

2. Work Plan (25%)
Appears appropriate for scope of work presented.

Reasonable, practical approach. Straightforward. It is great to see a "get it done" attitude.

They seem to have hired the right person in Combs.

Will compete by the end of Sep 2005—essentially a two-month study.

A bit vague and not well defined.

3. Results
No results to date. Close of project period rapidly approaching.

4. Impact/Technical Merit (30%)
No anticipated impact on exploration science. Work is primarily for benefit of policy makers, not industry.

This will provide quite useful results for exploration decision making and financing.

Modest.

Important to know what real success rates of exploration have been.

I guess for DOE baseline.

Not an exploration tool.

5. Plans for Completion (20%)
Appears appropriate.

Reasonable, practical approach. Straightforward. The PI has a "get it done" attitude.

Seems too ambitious given the time available.

Clear two-month task.

Strengths
Important to know.

Historical perspective.

Weaknesses
No direct application to exploration science.

None.

Vague.

Information for budgeting—not exploring.

Recommendations
Evaluate at some point also the unsuccessful cases—why, how, etc.

Better definition of goals, data.

Remove "anecdotal uncertainty" from estimate of drilling success rate.

[Page Intentionally Left Blank]

Appendix A. Guidelines for Principal Investigators

U.S. Department of Energy
Energy Efficiency and Renewable Energy
Bringing you a prosperous future, where energy is clean, abundant, reliable, and affordable

Geothermal Technologies Program
Resource Development Peer Review
Instructions for Principal Investigators

The following contains instructions for the preparation of written papers that will be provided to peer reviewers before the Peer Review and the preparation of oral presentations to be made at the Peer Review. As a courtesy to the reviewers and fellow presenters, each presenter should follow these instructions exactly as given to assure consistency of format and content throughout the Review.

WRITTEN PAPERS

Please follow the guidance outlined below when preparing your paper(s) for the Peer Review. For your reference, a template is attached and should be used as a framework for each paper.

1. Formatting Guidelines

- Text spacing: Single-spaced
- Page Size: Letter-size (8.5X11)
- Font: Arial, 12 Point
- Margins: 1 inch on sides, top and bottom
- Footer: Exploration Peer Review Paper, Last Name – Page #
- Paper length: Minimum of two pages; maximum of **six pages**. The length limit includes tables, charts, graphics, references, etc. incorporated with the text. If more than six pages are submitted for a project, ONLY the first six pages will be given to reviewers.

2. Report Contents

Include the following sections, exactly as titled**,** using the template:

Project Title: Name of the research project.

CPS Identifier: Unique project/agreement number from DOE CPS tracking system. Provided by DOE.

Principal Investigator: Person responsible for carrying out the project

Organization: Identify the organization the Investigator is affiliated with in carrying out the project.
Co-Investigator(s): List any others who contributed to the project along with their organization. Identify collaborators and cost-sharing partners under separate headings.

Purpose: This section should be used for background discussion. Explain the rationale for doing the project. Describe the problem under investigation, the expected results of the research, and the benefits of the results. The discussion may include previous work by yourself and others which does not fall under the current project provided that work contributes to an understanding of the project's purpose and results.

Objectives: List and describe the project's objective(s) approved for the current award. Indicate how the project's objective(s) serves the exploration program goals as specified in the draft multiyear plan. While the linkage of a project to the program goal may not be immediately obvious, a logical connection should be made to justify the work.

Duration: Specify the date the project began and the planned completion date.

Funding: Give the full funding history of the project, including any cost share, for each fiscal year using the following table. The table should cover every year funding was provided, through FY05, and out year funding needs by fiscal year to the end of the project.

Fiscal Year	**DOE Funding ($k)**	**Cost Share ($k)**	**Total**

Work Plan and Approach: Describe your research plan and how the plan was designed to meet the project's objectives. Discuss the methods, techniques, and equipment you used to carry out the plan. Indicate the length of time needed to conduct various phases of the work; identify milestones and any go/no-go decision points.

Results: Describe your results to date, explaining how you have progressed toward achieving the project's objectives. Compare your actual progress to your work plan; discuss the work accomplished to date and the work remaining to be done. Identify any problems/issues encountered and your strategy for dealing with them. As appropriate, provide summaries of your data and analyses.

Impact/Technical Merit: Present evidence of new knowledge, technology, or other accomplishments resulting from the project. List the key papers, reports and other publications, awards and/or patents stemming from the research. Indicate if any industrial partnerships have been formed due to the project, how and why partners were chosen, and how effective the partnerships have been. Explain how the project's results will advance the exploration program goals.

Plans for Project Completion: Outline future work plans to complete the project and how they build on progress to date. Identify key technology barriers to be addressed, and specify any major changes in scope, objectives, or approach. (Caution: Do not address new or follow on proposals; they are not topics for this review.) If your project will be complete at the end of the fiscal year, indicate how the results will benefit the exploration program.

3. Other Information

Within the 6-page limit for the report, you may include the following information to assist in describing your project:

- Figures, tables, photographs, and charts from related work by others, as appropriate.
- Pertinent technical references.
- **Proprietary data**. Company confidential or proprietary data may NOT be used unless a release is obtained in writing from the source. If this data is critical to your presentation, please notify Raymond Fortuna, Department of Energy, telephone: 202-586-1711, of your intent to use such data and provide him with a copy of your release.

4. Due Date for Papers

Papers will be provided to reviewers prior to the meeting, so your paper(s) must be submitted on or before **June 30, 2005**. Electronic copies of your paper(s) in **MS WORD** format should be sent to Cybilline Aclan (caclan@sentech.org).

ORAL PRESENTATION GUIDELINES

Your oral presentation must follow the same outline as that of your written paper. Specifically, the presentation should contain individual slides labeled to address your project's purpose, objectives, plan and approach, funding, results, impact, and completion. These are the topics that will be scored by the reviewers, and they are required for the presentation. An omission of any topic will result in a reduction in your score. Within the structure of the presentation there is flexibility to include any other information deemed important. Note: several new projects do not have results and will be evaluated using modified criteria.

The agenda schedule and allotted times for each project will be strictly enforced. Each project is allotted 35 minutes total time.

Time allocation: 20 minutes for presentation
15 minutes for Q&A

If the entire 15 minutes are not needed for questions and answers, the presenter will step down. If you believe you need more time on the agenda due to the size and complexity of your project, contact Raymond Fortuna, Raymond.fortuna@hq.doe.gov by **June 24, 2005**. Note that only a few projects will be given extra time on the agenda.

Visual slides accompanying oral presentations will use electronic media and must be created in **MS PowerPoint**. **No Other Media Will Be Used**. Provide a copy of your presentation slides to Cybilline Aclan no later than **July 14, 2005**. All presentation materials will be assembled into binders for distribution to reviewers at the meeting.

Please submit your presentations via e-mail or on a CD. CDs should be sent for presentations larger than 4.5 MB to the following address:

Cybilline Aclan
Sentech, Inc.
7475 Wisconsin Avenue, Suite 900
Bethesda, MD 20814
240-223-5536
CACLAN@sentech.org

In addition, provide a copy to Raymond Fortuna. Ms. Aclan should be able to answer any logistical questions you may have regarding the submission of papers. Mr. Fortuna will answer questions about the content of the reports.

Your cooperation in following these instructions will help assure a productive and successful review.

U.S. Department of Energy
Energy Efficiency and Renewable Energy
Bringing you a prosperous future, where energy is clean, abundant, reliable,

Geothermal Technologies Program
Resource Development Peer Review
RESEARCH PAPER TEMPLATE

Project Title:
CPS Identifier:
Principal Investigator:
Sponsoring Organization:
Other Investigators: {List}

Project Purpose
{Text}

Project Objective(s)
{Text}

Funding

Fiscal Year	DOE Funding ($k)	Cost Share ($k)	Total

{Note: Add more rows as needed; see instructions}

Plans and Approach
{Text}

Results
{Text}

Impact of Work/Merit
{Text}

Plans for Completion
{Text}

{Note: Tables, graphs, etc may be incorporated into the text portions of the paper or included at the end. The format/page layout/font size/color of these materials is optional, but they must be clear, legible, and use Arial font.}

[Page Intentionally Left Blank]

Appendix B. Project Evaluation Form

U.S. Department of Energy
Energy Efficiency and Renewable Energy
Bringing you a prosperous future, where energy is clean, abundant, reliable, and affordable

GEOTHERMAL TECHNOLOGIES PROGRAM
RESOURCE DEVELOPMENT PEER REVIEW

PROJECT EVALUATION FORM

PROJECT TITLE:____
PRINCIPAL INVESTIGATOR:____
PERFORMING ORGANIZATION:____
PRESENTER:____

1. **Purpose and Objective (25%)**

Excellent: 9-10
The project meets a critical need in advancing the technology and use of geothermal energy. The project's objective is clearly stated and understandable; links to the Geothermal Program's goals are well demonstrated.
Good: 7-8
The project meets an important need of the Program and industry. The objective is logical, relevant to the need, and relates to the Program's goals.
Fair: 5-6
The project's purpose and objective are adequately defined, but their relevance to industry's needs or Program goals is not well demonstrated.
Poor: 2-4
The project's utility is limited or questionable, and the objective has little or no relevance to needs or Program goals.
Unsatisfactory: 1
The project serves no useful purpose.

Circle the appropriate number for your rating

1 2 3 4 5 6 7 8 9 10

Unsatisfactory **Excellent**

Supporting Comments

____

____

____

____

2. Work Plan (20%)

Excellent: 9-10
The plan is complete with a comprehensive work scope, decision points, attainable milestones, and a reasonable budget. The plan is highly likely to succeed. The approach uses best available practice with little need for improvement. The cost provides exceptional return on investment.

Good: 7-8
The plan contains all the essential elements needed to meet the stated objectives. The approach is generally well thought out and effective, but could be improved in a few areas. The cost is reasonable.

Fair: 5-6
The plan has some flaws that could prevent reaching the objectives. The approach has shortcomings, but they can be overcome. The cost is relatively high for the expected benefit.

Poor: 2-4
The plan has serious flaws or may be unreasonable and is unlikely to succeed. The approach is incomplete or inadequate. The cost is questionable.

Unsatisfactory: 1
Not worth the time or money.

Circle the appropriate number for your rating

1 2 3 4 5 6 7 8 9 10

Unsatisfactory **Excellent**

Supporting Comments

____

____

____

____

____

____

3. Results (30%)

Excellent: 9-10
The project has made significant progress toward meeting project objectives; to date all key milestones have been achieved within budget. The results are significant and have demonstrated utility. The objectives will be achieved as planned.

Good: 7-8.
The project has made significant progress toward meeting objectives, but some milestones may have slipped and adjustments to work scope or approach had to be made. The results show reasonable progress toward meeting objectives, and they reflect expenditures.

Fair: 5-6
The project has made some progress toward meeting objectives, but schedules, budget, work scope, and/or approach have had to be changed. The results suggest further changes to the work plan may be required.

Poor: 2-4
The results are weak, inconsistent, or open to question. Project has demonstrated little or no progress toward meeting its objectives.

Unsatisfactory: 1
Nothing here of value.

Circle the appropriate number for your rating

1 2 3 4 5 6 7 8 9 10

Unsatisfactory **Excellent**

Supporting Comments

____

____

____

____

____

____

4. Impact/Technical Merit (15%)

Excellent: 9-10
Results and progress to date indicate the project will have a major impact on geothermal technology. General recognition by peers of accomplishment has been achieved. Extensive technology transfer and information dissemination has occurred. Interested stakeholders are directly involved in the work. Industry will adopt the product

Good: 7-8
The project has the potential to provide significant benefit to geothermal technology. Technology transfer is documented by presentations, papers, and efforts to establish collaborative ties or other links with interested stakeholders. Industry has shown active interest in commercial applications of the work.

Fair: 5-6
When completed, the project is likely to provide some benefit to geothermal technology. Some technology transfer has occurred through presentations, papers, contacts with industry. At least one member of industry has expressed some interest in applying the results.

Poor: 2-4
Based on results and progress to date, the project is unlikely to make any contribution to geothermal technology. Attempts at technology transfer have resulted in little or no interest on the part of stakeholders.

Unsatisfactory: 1
No one cares

Circle the appropriate number for your rating

1 2 3 4 5 6 7 **8 9 10**

Unsatisfactory **Excellent**

Supporting Comments

____

____

____

____

____

____

5. Plans For Completion (10%)

Excellent: 9-10
Future work plan builds on progress to date and is sharply focused toward meeting all remaining objectives as planned. Project is very likely to be completed on schedule and within budget.

Good: 7-8
Future work plan is complete, oriented toward meeting objectives, but subject to uncertainties. Some modifications to plan are likely with impacts on schedule and budget.

Fair: 5-6
Future work plan has shortcomings that may affect outcome. Achievement of objectives is in doubt. Substantial changes to schedule and budget will be required.

Poor: 2-4
Future work will not succeed; objectives will not be achieved. Expect major slippages in milestones and/or large budget overruns. Proceeding with project would be questionable.

Unsatisfactory: 1
Project has no future.

Circle the appropriate number for your rating

1 2 3 4 5 6 7 8 9 10

Unsatisfactory **Excellent**

Supporting Comments

____

____

____

____

____

____

Strengths:

Weaknesses:

Recommendations on Additions or Deletions to Work:

My understanding of the subject matter of this project is: High Moderate Low (circle)

Reviewer's Signature: ______________________________

Reviewer's Name: ______________________________
(print)

Appendix C. Agenda

U.S. DOE Geothermal Technologies Program
Resource Development Peer Review
Reno, Nevada
July 26-28, 2005

Review Agenda

Tuesday, July 26, 2005

7:30 am Registration & Breakfast ***(DOE/Staff & Peer Review Committee – Informal meeting in the breakout room)*

8:00 am Opening Remarks

8:15 am Geothermal Program Overview

Session 1: Remote Sensing

8:30 am 1.1 Advanced Remote Sensing Methods for Geothermal Exploration – *William Pickles – Lawrence Livermore National Laboratory*

9:05 am 1.2 Satellite InSAR (Interferometric Synthetic Aperture Radar) Ground Displacement Analysis for Geothermal Reservoir Management and Development – *Gary Oppliger – University of Nevada – Reno*

9:40 am 1.3 Localized Strain as a Discriminator of Hidden Geothermal Resources – *William Foxall –Lawrence Livermore National Laboratory*

10:15 am BREA K

11:00 am 1.4 Remote Sensing for Exploration and Mapping of Geothermal Resources – *Wendy Calvin – University of Nevada – Reno*

12:00 pm LUNCH (On your own)

Session 2: Electrical Surveys

1:30 pm 2.1 Characterization of Geothermal Resources through Integrated 3D Geophysical Modeling & Inversion – *Gregory Newman and Michael Hoversten – Lawrence Berkeley National Laboratory*

2:05 pm 2.2 Exploring for "Hidden" Geothermal Resources in the Basin and Range – *John Pritchett – SAIC*

2:40 pm 2.3 Crustal Strain Rate Analysis through Deep Electrical Anisotropy Mapping: An Alternative Tool for Identifying the Orientation of Critically Stressed Fractures for EGS Projects – *Gary Oppliger University of Nevada – Reno*

3:15 pm Wrap - Up

4:00 pm Adjourn

5:00 pm No-host Group Dinner

Wednesday, July 27, 2005

7:30 am Registration & Breakfast

Session 3: Geochemistry

8:00 am 3.1 Gas & Isotope Geochemistry – *Mack Kennedy – Lawrence Berkeley National Laboratory*

8:35 am 3.2 Geothermal Applications of Multi-Gas Geochemistry – *Paul Lechler – University of Nevada Reno*

9:10 am 3.3 Geochemical Sampling of Thermal and Non-Thermal Waters in Nevada: Continued Evaluation of Geothermal Resources – *Lisa Shevenell – University of Nevada – Reno*

9:45 am BREAK

Session 4: Structure, Tectonics and Analysis

10:30 am 4.1 Targeting of Potential Geothermal Resources in the Great Basin from Regional to Basin-Scale Relationships between Geodetic Strain and Geological Structures – *Geoff Blewitt – University of Nevada – Reno*

11:05 am 4.2 Geologic and Geophysical Analysis of the Desert Peak-Brady Geothermal Fields: Structural Controls on Geothermal Reservoirs in the Humboldt Structural Zone – *Jim Faulds – University of Nevada – Reno*

11:40 am 4.3 Characterizing Structural Controls on Geothermal Systems in the Northwestern Great Basin through Integrated Geologic and Geophysical Analyses – *Jim Faulds – University of Nevada – Reno*

12:15 pm LUNCH (On your own)

1:30 pm	4.4 Assembling Crustal Geophysical Data for Geothermal Exploration in the Great Basin – *John Louie – University of Nevada – Reno*
2:05 pm	4.5 Seismic Imaging – *Ernie Majer – Lawrence Berkeley National Laboratory*
2:35 pm	**BREAK**

Session 5: Database Compilation and Analysis

3:05 pm	5.1 Data Fusion for Geothermal Exploration: The Stochastic Engine – *William Foxall – Lawrence Livermore National Laboratory*
3:40 pm	**Wrap – Up**
4:00 pm	**Adjourn**

Thursday, July 28, 2005

7:30 am	Registration & Breakfast

Session 5a: Database Compilation and Analysis

8:00 am	5.2 Revival of Grass-roots Geothermal Exploration in the Great Basin (where to look for new geothermal fields) -- A New Approach to Assessing Geothermal Potential using a Geographic Information System- Parts IV and V – *Mark Coolbaugh – University of Nevada – Reno*
8:35 am	5.3 Dating of Young Igneous Rocks Associated with Geothermal Systems in the Great Basin – *Greg Arehart – University of Nevada – Reno*
9:10 am	5.4 Developing and Updating Techniques and Databases for Geothermal Resource Assessments – *Colin Williams – U.S. Geological Survey*
9:45 am	**BREAK**
10:30 am	5.5 Application of Thermal Techniques for Exploration, Evaluation, and Assessment of Basin and Range Geothermal Resources – *David Blackwell – Southern Methodist University*
11:05 am	5.6 Exploration Statistics – *Joel Renner –Idaho National Laboratory*
11:40 am	**Wrap – Up**
12:00 pm	**LUNCH (On your own)**

Session 6:	**Peer Review Deliberation (Closed Session)**
1:00 pm	Peer Review Deliberation
3:00 pm	Peer Review Program Debrief for DOE/HQ
4:00 pm	Adjourn

Appendix D. Project Summary Scores

Session 1

1.1 *Advanced Remote Sensing Methods for Geothermal Exploration*

	1. Purpose and Objective (25%)	2. Work Plan (20%)	3. Results (30%)	4. Impact/Technical Merit (15%)	5. Plans for Completion (10%)	Weighted Total
	Reviewer Scores					
	80	70	60	70	50	68
	40	40	40	50	50	43
	50	50	30	30	70	43
	40	40	60	40	40	46
	50	50	50	40	50	49
	50	40	40	50	40	44
Collective Score	52	48	47	47	50	**49**
Standard Deviation	13	11	11	12	10	**8.7**

1.2 *Satellite InSAR Ground Displacement Analysis*

	1. Purpose and Objective (25%)	2. Work Plan (20%)	3. Results (30%)	4. Impact/Technical Merit (15%)	5. Plans for Completion (10%)	Weighted Total
	Reviewer Scores					
	60	60	60	60	70	61
	70	50	60	60	50	60
	60	60	50	50	60	56
	60	60	60	60	70	61
	60	80	80	60	80	72
	60	60	70	70	50	64
Collective Score	62	62	63	60	63	**62**
Standard Deviation	3.7	9.0	9.4	5.8	11	**5.0**

1.3 *Localized Strain as a Discriminator of Hidden Geothermal Resources*

	1. Purpose and Objective (25%)	2. Work Plan (20%)	3. Results (30%)	4. Impact/Technical Merit (15%)	5. Plans for Completion (10%)	Weighted Total
	Reviewer Scores					
	70	60	40	50	50	54
	70	60	60	50	70	62
	80	80	90	80	80	83
	80	70	70	70	70	73
	50	50	60	50	50	53
	80	80	60	80	70	73
Collective Score	72	67	63	63	65	**66**
Standard Deviation	11	11	15	14	11	**11**

1.4 *Remote Sensing for Exploration and Mapping of Geothermal Resources*

	1. Purpose and Objective (25%)	2. Work Plan (20%)	3. Results (30%)	4. Impact/Technical Merit (15%)	5. Plans for Completion (10%)	Weighted Total
	Reviewer Scores					
	70	60	70	70	60	67
	90	80	90	80	80	86
	80	70	80	80	70	77
	80	80	80	70	80	79
	80	70	80	70	80	77
	80	70	80	80	80	78
Collective Score	80	72	80	75	75	**77**
Standard Deviation	5.8	6.9	5.8	5.0	7.6	**5.4**

Session 2

2.1 *Characterization of Geothermal Resources*

	1. Purpose and Objective (25%)	2. Work Plan (20%)	3. Results (30%)	4. Impact/Technical Merit (15%)	5. Plans for Completion (10%)	Weighted Total
	Reviewer Scores					
	80	80	50	50	70	66
	60	70	50	60	70	60
	80	80	90	80	70	82
	80	80	60	60	70	70
	60	80	60	60	70	65
	60	70	70	60	70	66
Collective Score	70	77	63	62	70	**68**
Standard Deviation	10	4.7	14	9.0	0.0	**6.9**

2.2 *Exploring for "Hidden" Geothermal Resources in the Basin and Range**

	1. Purpose and Objective (25%)	2. Work Plan (20%)	3. Results (30%)	4. Impact/Technical Merit (15%)	5. Plans for Completion (10%)	Weighted Total
	Reviewer Scores					
	60	70	N/S	70	70	68
	60	60	N/S	60	60	60
	60	70	N/S	50	60	60
	50	40	N/S	50	40	46
	70	50	N/S	60	60	60
	80	80	N/S	80	80	80
Collective Score	63	62	N/A	62	62	**62**
Standard Deviation	9.4	13	N/A	11	12	**10**

* These projects were weighted using the following percentages: Purpose and Objective (25%); Work Plan (25%); Results (0%); Impact/Technical Merit (30%); and Plans for Completion (20%).

Criteria: 1. Purpose and Objective (25%); 2. Work Plan (20%); 3. Results (30%); 4. Impact/Technical Merit (15%); 5. Plans for Completion (10%)

2.3 ***Crustal Strain Rate Analysis****

	Reviewer Scores					Weighted Total
	30	20	N/S	20	20	23
	60	70	N/S	60	70	65
	40	30	N/S	30	20	31
	30	50	N/S	40	40	40
	50	60	N/S	50	50	53
	60	40	N/S	40	40	45
Collective Score	45	45	N/A	40	40	**43**
Standard Deviation	13	17	N/A	13	17	**14**

Session 3

3.1 ***Gas & Isotope Geochemistry***

	Reviewer Scores					Weighted Total
	70	70	80	60	70	72
	80	80	80	80	80	80
	90	80	90	90	90	88
	70	70	80	60	50	70
	90	90	80	80	80	85
	90	80	90	80	80	86
Collective Score	82	78	83	75	75	**80**
Standard Deviation	9.0	6.9	4.7	11	13	**7.0**

3.2 ***Geothermal Applications of Multi-Gas Geochemistry****

	Reviewer Scores					Weighted Total
	70	60	N/S	60	60	63
	60	50	N/S	60	70	60
	80	60	N/S	60	70	67
	50	40	N/S	20	30	35
	70	60	N/S	60	60	63
	60	50	N/S	40	40	48
Collective Score	65	53	N/A	50	55	**56**
Standard Deviation	10	7.5	N/A	15	15	**11**

3.3 ***Geochemical Sampling of Thermal and Non-thermal Waters***

	Reviewer Scores					Weighted Total
	70	70	80	80	80	76
	80	90	80	80	90	83
	70	70	60	70	70	67
	70	60	70	60	70	67
	90	80	90	90	80	87
	80	80	80	80	80	80
Collective Score	77	75	77	77	78	**77**
Standard Deviation	7.5	10	9.4	9.4	6.9	**7.7**

* These projects were weighted using the following percentages: Purpose and Objective (25%); Work Plan (25%); Results (0%); Impact/Technical Merit (30%); and Plans for Completion (20%).

Criteria: 1. Purpose and Objective (25%); 2. Work Plan (20%); 3. Results (30%); 4. Impact/Technical Merit (15%); 5. Plans for Completion (10%)

Session 4

4.1 *Targeting of Potential Geothermal Resources*

	1	2	3	4	5	Weighted Total
Reviewer Scores	90	90	80	90	70	84
	70	80	80	80	90	79
	90	90	90	90	90	90
	90	80	90	90	90	88
	80	80	90	90	80	85
	60	70	80	70	80	72
Collective Score	80	82	85	85	83	**83**
Standard Deviation	12	6.9	5.0	7.6	7.5	**6.2**

4.2 *Geologic and Geophysical Analysis**

	1	2	3	4	5	Weighted Total
Reviewer Scores	90	80	N/S	80	80	83
	70	80	N/S	70	80	75
	80	80	N/S	90	80	83
	80	50	N/S	80	70	71
	90	80	N/S	90	80	86
	80	80	N/S	80	80	80
Collective Score	82	75	N/A	82	78	**79**
Standard Deviation	6.9	11	N/A	6.9	3.7	**5.2**

4.3 *Characterizing Structural Controls on Geothermal Systems**

	1	2	3	4	5	Weighted Total
Reviewer Scores	80	80	N/S	90	90	85
	90	80	N/S	80	70	81
	80	70	N/S	80	80	78
	80	80	N/S	80	80	80
	80	60	N/S	80	70	73
	70	70	N/S	60	70	67
Collective Score	80	73	N/A	78	77	**77**
Standard Deviation	5.8	7.5	N/A	9.0	7.5	**5.8**

4.4 *Assembling Crustal Geophysical Data for Geothermal Exploration in the Great Basin*

	1	2	3	4	5	Weighted Total
Reviewer Scores	80	70	90	70	70	79
	70	70	70	70	80	71
	80	90	90	90	90	88
	90	80	90	90	90	88
	40	40	60	40	50	47
	80	90	80	70	80	81
Collective Score	73	73	80	72	77	**75**
Standard Deviation	16	17	12	17	14	**14**

4.5 *Seismic Imaging*

	1	2	3	4	5	Weighted Total
Reviewer Scores	90	60	60	60	70	69
	50	50	30	30	30	39
	50	50	40	40	40	45
	70	50	40	40	40	50
	60	80	N/S	60	70	67
	40	50	40	50	30	43
Collective Score	60	57	42	47	47	**51**
Standard Deviation	16	11	9.8	11	17	**12**

* These projects were weighted using the following percentages: Purpose and Objective (25%); Work Plan (25%); Results (0%); Impact/Technical Merit (30%); and Plans for Completion (20%).

Criteria: 1. Purpose and Objective (25%); 2. Work Plan (20%); 3. Results (30%); 4. Impact/Technical Merit (15%); 5. Plans for Completion (10%)

Session 5

5.1 *Data Fusion for Geothermal Exploration: The Stochastic Engine**

	Reviewer Scores					Weighted Total
	50	50	N/S	40	50	47
	60	60	N/S	60	70	62
	80	70	N/S	80	70	76
	60	50	N/S	50	50	53
	60	70	N/S	50	70	62
	70	50	N/S	50	70	59
Collective Score	63	58	N/A	55	63	**60**
Standard Deviation	9.4	9.0	N/A	13	9.4	**8.9**

5.2 *Revival of Grass-roots Geothermal Exploration in the Great Basin*

	Reviewer Scores					Weighted Total
	80	80	80	80	70	79
	90	90	80	80	80	85
	90	80	90	80	90	87
	90	80	90	80	80	86
	90	90	90	90	80	89
	90	80	80	90	80	84
Collective Score	88	83	85	83	80	**85**
Standard Deviation	3.7	4.7	5.0	4.7	5.8	**3.0**

5.3 *Dating of Young Igneous Rocks Associated with Geothermal Systems in the Great Basin*

	Reviewer Scores					Weighted Total
	70	80	70	60	90	73
	70	60	60	50	60	61
	70	60	60	60	60	63
	80	N/S	50	70	70	66
	80	70	60	70	60	69
	80	90	80	90	90	85
Collective Score	75	72	63	67	72	**69**
Standard Deviation	5.0	12	9.4	12	13	**7.9**

5.4 *Developing and Updating Techniques and Databases for Geothermal Resource*

	Reviewer Scores					Weighted Total
	70	50	50	70	60	59
	70	80	70	90	60	74
	70	60	60	70	60	64
	30	30	30	30	40	31
	70	50	80	80	70	71
	50	50	40	40	50	46
Collective Score	60	53	55	63	57	**57**
Standard Deviation	15	15	17	21	9.4	**15**

5.5 *Application of Thermal Techniques for Exploration, Evaluation, and Assessment*

	Reviewer Scores					Weighted Total
	60	60	60	60	50	59
	80	60	70	70	70	71
	90	80	80	80	80	83
	80	70	80	70	70	76
	90	80	90	90	90	88
	70	50	50	60	50	57
Collective Score	78	67	72	72	68	**72**
Standard Deviation	11	11	13	11	15	**11**

5.6 *Exploration Statistics**

	Reviewer Scores					Weighted Total
	70	80	N/S	60	80	72
	90	80	N/S	80	90	85
	70	70	N/S	50	70	64
	80	70	N/S	70	80	75
	80	60	N/S	70	70	70
	60	60	N/S	50	60	57
Collective Score	75	70	N/A	63	75	**70**
Standard Deviation	9.6	8.2	N/A	11	10	**8.5**

* These projects were weighted using the following percentages: Purpose and Objective (25%); Work Plan (25%); Results (0%); Impact/Technical Merit (30%); and Plans for Completion (20%).