External Peer Review of the USGS Land Cover Trends Research Project:
Panel Report

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Overall, we are very enthusiastic about the USGS Land Cover Trends Project (the "Project"). It has the potential to be very useful, indeed essential, to various research communities. It can be influential in several areas related to land cover/land use dynamics in the U.S., such as climate change, biodiversity, resource management and planning, resource security, and disaster planning. This excitement and potential should be communicated to the research, user, and stakeholder communities, including top administrators within USGS and key decision makers in Washington. This could be accomplished via high-profile publications and workshops to convene different disciplines, user groups, and stakeholders to build a larger and stronger constituency. It was a pleasure for us to meet and discuss this project with the research and leadership team; we applaud them for the progress made to this point.

We begin by putting the bottom line, our recommendations, at the top.

Recommendations:

Our recommendations begin from the observation that this research is already valuable, although mostly in the context of the larger land-change science community and the potential for supporting broad policy/planning activities. Most of these recommendations will require increased resources and/or a re-prioritization of existing resources (funds and/or personnel).

- Seek greater funding by promoting the project's products and significance to a broader audience of scientists and decision/policy makers
- Clearly identify the potential users of the data, analyses, and results; determine their needs and frame questions and hypotheses to address these needs, at the appropriate scale(s).

  - It may be possible to leverage additional funding from user groups/agencies/stakeholders by extending this study to include mid-decadal (2005-6) and possibly 2010 data, but such work should be done only to address specific, well-formulated questions that address the needs of well-defined user groups who are prepared to use the results.
- Engage users and stakeholders in various ways:
  - Consider an external steering committee comprising likely users of Land Cover Trends products. This should include a range of disciplines, and would be charged to work with the project team to identify, engage, and work with experts in the scientific community to advance efforts to understand causes and consequences of land cover trends
  - Sponsor workshops to explore ways to use the land cover trends database to investigate causes and consequences. A focus of the workshops should be on developing incisive questions that could be addressed using the Land Cover Trends database. The Land Cover Trends Program Team should use these workshops as an opportunity to expand partnerships with experts outside of USGS and outside the land cover field.
  - Integrate the Project more fully with similar efforts being conducted by other agencies or organizations.
  - Work with communities generating and using other databases (e.g., NRI, FIA, Agriculture census) to investigate (1) linking Land Cover Trends data to land *use* data and (2) conducting an accuracy assessment using these independent data
- Increase the number of people, both inside and outside of USGS, involved in the project. Possible avenues for accomplishing this are:
  - Seek funds to support a fellowship program for graduate students, post-doctorals, and professors in universities.
  - Increase the visiting scientist programs.
  - Establish an extramural research program that would fund projects to foster development of application of Land Cover Trends data in other agencies or in universities or NGOs.
  - Propose internal USGS budget enhancements to enhance the number of people in the project and bring in new, specific skills in the participants.
- Generate recognition and excitement via a series of visible and useful Land Cover Trends products by:
  - Publishing compelling results in high profile peer-reviewed publications (Science, PNAS, etc.). Every effort should be made to reach as broad an audience as possible, so publications should be placed in peer-reviewed journals other than those focused solely or primarily on remote sensing.
  - Ensuring that land cover interpreters recognize the importance of their work to the project and the importance of the project to important issues
  - Identifying two to four or more major synthesis papers based on Land Cover Trends data. These potential syntheses could lead to workshop themes or could be an outcome of workshops. (See above.)

  - Organizing a symposium (or at least complimentary presentations) at national/international scientific meetings to promote awareness of the Land Cover Trends products and their potential utility.
  - Bringing the web site up-to-date, and ensuring the capability to quickly, easily, and reliably access the data as downloadable files and/or via web services or existing federal portals such as the National Map or the Geospatial One Stop. Also ensure that those data and metadata are in a format that fosters their appropriate use. Include access to photos, field notes, and other data. Link the web-site to other applications, partnerships, workshops, etc.
- Prioritize efforts to maximize accomplishments
  - Priority should be given to efforts that broaden the user base and the types of applications for the Land Cover Trends products.
  - Make supporting investigation of causes and consequences of land cover and land use change the top priority for USGS scientists. Invest in understanding the data that have been generated.
  - Move ahead on the analysis of the determinants or drivers of land-cover change, incorporating modeling and multivariate statistical analyses, and incorporating more investigators both from within and outside USGS.
  - Developing a clear plan for how this work can benefit and leverage efforts to update NCLD should be an organizational priority.
  - Redoing the 20x20 areas and improving the assessment with different forms of auxiliary data are lower priorities than investigating causes and consequences.
- Develop ways of analyzing future trajectories of land-cover change
  - Employ scenario analysis and other models to project possible future land use change trajectories.
  - Link land-cover change with climate change analyses more fully, by collaborating with the groups (in USGS and elsewhere) conducting climate change research.

- Test the approaches and methods currently being used
  - Test the efficacy of the ecoregional scale of analysis by comparing the results with those obtained from other categorization systems; aggregate and disaggregate samples in different ways to evaluate land-cover changes at different scales of resolution.
  - Consider a hybrid approach by which the initial image classification is performed by software, and the resultant product is then manually checked to make corrections using a GIS recoding or on-screen digitizing approach.
  - Consider making use of coarser resolution MODIS/VIIRS data in tandem with finer resolution Landsat data to get broader coverage and fill gaps.
  - In a selected set of ecoregions, conduct tests to assess the accuracy and completeness of a sampling approach in relation to the results obtained from a wall-to-wall analysis.

- Compare random versus systematic sampling schemes to determine which would give a better or more even coverage of the ecoregions.

Background

According to the USGS Fact Sheet 2006-3020, the project has four primary objectives:

1. Develop a comprehensive methodology for estimating regional land-cover change (for five periods from 1973 to 2000, nominally 1973, 1980, 1986, 1992, and 2000);
2. Characterize the spatial and temporal characteristics of conterminous U.S. land-cover change for five periods from 1973 to 2000 (nominally 1973, 1980, 1986, 1992, and 2000);
3. Document the regional driving forces and consequences of change; and
4. Prepare a national synthesis of land-cover change.

The science questions for the project are:

1. What are the types and geographic distribution of change?
2. What are the overall rates of change by region and by sector?
3. How do the rates vary locally, regionally and temporally?
4. What are the driving agents and consequences?

These objectives and questions frame the context for any evaluation of the Project. We have been asked to comment on several specific aspects of this research, so this report is organized according to the questions that were posed to us.

It is not appropriate to criticize the Project for not doing something it was not intended to do, but at the same time it *is* appropriate to ask whether the Project would yield greater returns by modifying what is being done to address these objectives, or whether the objectives themselves should be adapted to address emerging needs. Our comments are offered in the spirit of reinforcing what is being done well but also encouraging a re-examination of activities and objectives where that may be appropriate.

We respond below to the questions posed to the peer review team, in turn:

1. **What is the scientific and/or technical merit of the research? In particular,**
    a. Does the research address important questions in land-cover change?
    b. How does the research compare with other research projects in its field, both in terms of scientific/technical merit and originality?
    c. Do the results enable analysis?
    d. Are there any new insights?
2. **Methodology**
    a. Is the experimental design sound?
    b. Is the statistical design appropriate?
3. **Management, infrastructure, and support**
4. **Budget: was the money well spent, is the budget adequate, excessive?**
5. **Publications, outputs and outcomes:**

   a. What is the quality and appropriateness?
   b. Is the number of publications appropriate for this stage of the project?
6. **Are there improvements to strengthen project, results, impact, scientific standing?**
7. **What are some suggested future directions for the project?**

We divided these questions up among ourselves, with each panel member taking on certain sections for a preliminary draft. The entire panel then commented on, added to or edited the entire report. This is a consensus report from the panel.

1a. Scientific/technical merit: Are the questions addressed important?

At this stage in the research, the Project has focused almost entirely on developing a sound method for assessing land-cover change and characterizing (i.e. describing) change (the first two objectives above), on assembling regional profiles of land-cover change, and on working toward combining these into a nationwide assessment of land-cover trends (the fourth objective). The third objective, determining the causes and consequences of land-cover change, has so far received much less attention. In other words, the emphasis has been primarily on describing the geographic and temporal *patterns* of land-cover change rather than analyzing the *processes* that are important in determining the geography, direction, and rates of change. As a result, the questions that have been asked are primarily "how" or "what" questions: How has land cover changed in particular regions? What elements of land cover have shown the greatest change? And so on.

During the Project review, we were told repeatedly that the goal of the Project is to *understand* the geography and rates of land-cover change. Understanding requires answers to "why" questions as well: Why is land cover changing more rapidly in some regions than others? Why is a particular cover type changing in a particular way? Some attempts have already been made to address such questions, but more rigorous, quantitative analyses are needed. This will require that a broad array of data – on social and economic factors (e.g., commodity production, land prices, demographics) as well as physical and biological environmental features – be arrayed against the information on land-cover trends. Statistical analysis and modeling (e.g., multivariate analysis to create state spaces to track land-cover changes; AIC (Akaike's Information Criterion) modeling to determine the best fit of multiple-factor models to land-cover trends), GIS analysis, and scenario models should be used to gain a better understanding of past land-cover changes and potential future trajectories of trends.

The Project is focused at an ecoregional scale of analysis. Ecoregions are the strata within which sampling is conducted, and although the samples can be combined and analyzed in different ways, this has generally not been done. In addition, the level of categorization of land-cover types is relatively coarse (Anderson level 1). These restrictions may be necessary in order to conduct analyses that can be standardized across the nation, but the coarseness of the spatial scale and of the cover categorizations does

constrain the questions that can be asked and the conclusions that can be drawn. One conclusion from the analyses to date, for example, is that land-cover change is a rare event – most areas and cover types show little change over the 27 years considered. Yet this is clearly a consequence of scale. At an ecoregional scale, or even at the scale of the 10 km x 10 km sampling units within ecoregions, there may be many cover types or areas in which cover change is simply unlikely to occur, especially within or between relatively coarse land-cover categories. This may mask substantive changes that occur in restricted areas or between different uses within a general cover category. In other words, the choice of scale has minimized the capacity of the approach to detect land-cover changes at local or subregional scales in the interests of enabling analyses at broader regional or national scales. The conclusion of little change may be correct given the scale of analysis, but is it really an appropriate or relevant conclusion? The scales of land-use decisions and land management, and the scales at which social, economic, and environmental factors drive or constrain land-cover change, are generally much more local than entire ecoregions. In addition, socioeconomic data are assembled according to political, not ecological, boundaries, so arraying them against the land-cover trends is not a seamless exercise. All this does not mean that ecoregions are an inappropriate choice for analysis (see below); it simply argues for thinking (and conducting analyses) at scales other than that of ecoregions.

"Does the research address important questions?" Yes, but "important" to whom? The Project has been designed and implemented as scientific research – so far, primarily as descriptive (pattern documentation) research. If the Project is to avoid being drawn into an orgy of data-dredging or fishing, which is always a temptation with a large and complex data set, it must do more than describe patterns for the sake of description. The research has reached the point where it is perhaps as or more important to assess the drivers *and the consequences* of land-cover change rather than the patterns of such change. The research should also be relevant and useful to some group(s) of users, which means that it should be organized about a central set of questions or hypotheses.

It is not clear who the intended users of the results of the Project really are. The land-cover data and results of the analyses are indeed being used to explore some user-relevant issues (e.g., a US carbon assessment, NEON site selection, US albedo change, threats to the US National Wildlife Refuge System), but these seem to have been developed in an *ad hoc*, opportunistic fashion rather than through a strategic program design process. The goal of the Project is to create a unified analysis of land-cover trends for the nation at an ecoregional scale of resolution. This implies that those making land use decisions at regional or national levels will be the primary users of the information, but it is not clear exactly who these people are or what decisions require land-cover trend information. What are their questions; what is "important" to them? What about potential users at other levels or scales of resolution? Although the Project is clearly headed in this direction, more attention should be given *now* to identifying a broad spectrum of potential users of the information, assessing their needs (i.e. their questions), and developing a strategy to target the analyses to address the most critical needs. For example, the Project is contemplating extending the analysis to include 2005 and, potentially, 2010 data; this should be done *only* to address specific, well-formulated questions that address the needs

of well-defined user groups who are prepared to use the results and possibly could contribute to funding the research.

So the answer to the question, "Does the research address important questions in land-cover change?" is "yes," if the questions have to do with patterns of land cover at broad (ecoregional) scales. The answer is "no," however, if the questions are about what determines or drives land-cover change or what is most important to particular user groups, at multiple scales.

1b. Scientific/technical merit: compared to other projects in its field, including originality

This research is unique and original for a land-cover analysis in the United States. By adopting a standardized methodology and giving close attention to the properties of the land-cover data used, it enables a nationwide coverage that has the same level of rigor and reliability everywhere. By adopting ecological units (ecoregions) as the basic spatial level of analysis, it breaks away from the political frame of reference (states and counties) that is often employed in national analysis. Because ecoregions are defined by similarities and differences in biophysical properties, this approach automatically imposes a certain level of biophysical uniformity on the sectors for which land-cover change is assessed. Although this does not in itself establish links between driving variables (particularly socioeconomic ones) and land-cover change, it provides a framework that is, in a sense, pre-adapted for such analyses.

The efficacy of ecoregions as a framework for land-cover trend analysis, however, should be more rigorously tested. How do the results of the USGS analyses for, say, eastern United States (20 ecoregions) compare with those that might be obtained using other classifications (e.g. Bailey's ecoregions, or those used by The Nature Conservancy) or with analyses based on gradients of environmental factors derived for the individual sampling units over the region? We understand that some such tests have been done but not published; evaluating the effectiveness of the ecoregional approach and publishing the results should be a priority.

The Project has developed largely independently of other efforts that, while not entirely similar, are relevant because of their geographic scope and relation to land cover and land use. In particular, greater efforts should be made to link the results and the activities of the Project with the *State of the Nation's Ecosystems* project being led by the Heinz Center, with the Natural Resources Inventory, with the US Forest Service Forest Inventory Assessment (FIA), with LANDFIRE, and with similar nationwide environmental evaluations. Comparisons should be made with other international, national, or regional efforts to assess and project land use and land-cover changes, such as (to name a few) the LUCC Project of IGBP/IHDP (now superceded by the joint IGBP-IHDP Global Land Project (GLP)), the analysis of changing land cover in the American Midwest conducted by the US Forest Service, the national land use monitoring project of the Natural Resources Conservation Service (National Resource Inventory, NRI), or the

GEOMOD model developed by SUNY ESF and applied in Northeastern USA by the Yale Global Institute of Sustainable Forestry.

1c. Scientific/technical merit: Do the results enable analysis of impacts?

One very important question has been left out of the charge: **Do the products enable analysis and understanding of the processes and mechanisms that cause the patterns of change described in the major part of the work?** This unasked question forms the basis for evaluating of the contribution of the Project to fundamental science, and is implied by the first part of the Project's fourth scientific question: "**What are the driving agents and consequences?**" "Driving agents" are the processes and mechanisms that generate changes in landscapes, and "impacts" are the consequences. The land-cover and land-cover change data products of the project, which describe in spatially explicit terms the land-cover changes in several hundred 100 (or 400) km^2 areas, are the dependent variables in this analysis. Understanding driving agents is necessary for the broader societal objectives of land-use policy, landscape and regional planning, ecosystem conservation, and other activities to create specific desired landscape outcomes. Understanding the impacts of land-cover change can tell us what is harmful and what is helpful, but we will not be able to direct change without understanding its causes.

The panel's answer to this unasked question, as well as that posed for this section (impacts), is a qualified "yes" to both, for the same reasons. The description of the actual patterns of land-cover change in all ecoregions of the conterminous U.S. is a necessary, but not sufficient, set of data for understanding processes and mechanisms of change. We must know the changes that have occurred. This project is generating the most comprehensive and widespread description of the changes of land-cover classes in specific areas that exists anywhere in the world.

There are two qualifications to this statement. First, because the product is land-cover change maps for square 100 km^2 or 400 km^2 areas, investigation of drivers that act at scales with larger extents will be difficult to do without clever methods. Lots of intervening space is not analyzed; edge effects are increased because of small areas; influences of factors outside of sampled areas are difficult to quantify, and some areas of important change may be missed by sampling because the locations of land-cover change are not random but clustered. For a simplistic example, if we hypothesize that the distance to a river determines whether a piece of land can be irrigated, and rivers do not appear in the study areas but do flow between them, we may not be able to analyze the effect of the distance from the river on the conversion of land in arid areas to agriculture. A clever approach might be to use wall-to-wall coverage of river channels to calculate distances to land parcels, but the analysis will be hindered by not knowing what happened outside of the sample area but near the river. Another example is that hydrological drivers that operate at drainage basin scales cannot be analyzed unless the drainage basins of interest are completely included in the sample area. Furthermore, many drainage basins span multiple ecoregions, so analysis of either causes or consequences of land-cover change will not be possible with these data. Another

example: suburban development by definition occurs near cities, but cities actually take up very little space in an ecoregion, so randomly placed samples are likely to miss them. This is not to say that the sampling scheme or change map products are inadequate to answer many questions related to land-cover change. The demonstrated extreme difficulty of automated land-cover classification means that multitemporal, wall-to-wall land-cover change analysis is very difficult now, and the sampling approach does give us one of the best possible data sets for conducting many analyses of drivers at the scale of ecoregions.

Second, the research project products do not include the factors that are hypothesized to drive land-cover change. These factors include social, economic, and environmental conditions and processes, at all scales (local, regional, national, global), and their interactions within and between scales. A large literature, mostly focused on regional-scale case studies, shows how complex the driving forces in land-cover and land-use change can be. In some cases these data are available, from, for example, TIGER/Line data from the US Census Bureau, other USGS data sets on biophysical landscape characteristics, and a myriad of data sets from other local, state, and national government agencies as well as NGOs and private firms. Some of the Project team's publications (especially Sohl et al. 2007) show how these kinds of studies can be undertaken, but it will take a much larger number of people, from both government agencies and academia, and sufficient funding, to conduct these studies.

This comment should not be taken negatively. The land-cover change products are a fundamental resource for the land-change science research program, and a continuation of this project would be an extraordinarily important contribution to our scientific understanding of landscape dynamics. The data products are the only fine-scale description that we have of land-cover *change* at a national level and ecoregional scale, which is very important.

That the Project products exist should stimulate the kinds of research that will allow for much better understanding of the drivers of land-cover change at all scales from local to national, and with clever approaches, to global drivers. The products can overcome some of the need for each individual research project to create its own land-cover change maps. It should be said that the land-cover classification that the Project uses may or may not meet the needs of other researchers asking different questions. No land-cover classification can address all potential research or policy questions because appropriate spatial, temporal, and taxonomic scales vary among different questions. Changes within a class can be important and have different causes and consequences. For example, the class of "agriculture" can include everything from low-till, low-impact farming to cultivation with intensive fertilizer and pesticide applications. Nonetheless, the classification used by the Project group is able to capture much of the variation of land-cover relevant for many questions, and will be sufficient for many of the possible future studies.

The answer to the question "**Do the results enable analysis of impacts?"** mirrors the answers given above: a qualified "yes" but perhaps with less qualification than the

previous answer had. The data products are again necessary, but not sufficient, for the analysis of "impacts" or "consequences" of land-cover change. In this sense, the land-cover and land-cover changes are the independent variables. Many of the papers published by the Project team demonstrate the effects of land-cover change on many ecosystem characteristics (the section on "Applications of Trends Data" in the selected annotated bibliography includes seven papers that describe the climatic, carbon cycle, aquatic ecosystem, and wildlife conservation consequences of land-cover change in specific areas). But again, the products do not contain the dependent variables, which must come from other sources. In addition, the existence of the Project data sets will enable the analysis of impacts or consequences of land-cover change because they will remove the need for many investigators to create their own change data. The caution about land-cover classification described above also is relevant.

An additional issue about the impact analysis is that the kinds of impacts, and the audience or users of the information, are not well-identified in the project. The work so far on impacts/consequences (carbon, aquatic systems) has been done because individual investigators were interested in these questions and funding was available to support them. The peer-review panel emphasizes (and the Project team recognizes) that working with potential users is necessary, both to show that the data products are useful for the users' questions, and to help modify the data products to make their use easier or more appropriate for particular questions. Modification might include changing the classification scheme to include changes within categories, changing the scale of spatial resolution, translating data files to other formats, etc.

Identifying potential users is also not straightforward. Certainly the water-quality, carbon cycle, climate modeling, and land-use research communities can all find excellent uses for the data products if they know that they exist. Publication by the Project team in widely read journals such as *Science, Ecological Applications, Water Resources Research,* etc., will attract the attention of these user communities. Policy and planning users, on the other hand, will have to be notified and shown the use of the data products in other ways, such as workshops or personal contacts.

1d. Scientific/technical merit: Does the research provide new insights to improve land-cover change analysis?

To some degree, the answer to this question is "yes," but mostly it is too soon to tell. The sampling approach itself is novel and useful, and may have applications in other contexts, e.g. similar large-area studies in other parts of the world. On the other hand, the random selection of study areas may underrepresent the amount of change that has occurred in an ecoregion because land-cover change does not occur randomly but is more likely near recently converted areas that represent small areas within an ecoregion. Land-change scientists accustomed to using complete landscape coverage generated by satellite instruments for their smaller study areas are often skeptical when confronted with sampled data from very large areas because they worry about missing important changes, even if they are shown that the statistical properties of the sample and the universe are

identical. At least one new insight from the Project study is that sampling is a useful approach to the national-scale land-cover change question, even if it is more limited than a wall-to-wall effort, which in itself is very difficult to do. The tradeoffs between classification accuracy and complete coverage were not well known before this study; the work is shedding light on questions that we have not considered before.

The use of auxiliary information for some of the sampling effort is innovative and may result in new insights, but it may also add spatial bias to the results. Auxiliary information was not used in the current within-ecoregion sampling, so we do not know whether it will improve land-cover change analysis. A study that did incorporate auxiliary information in a sampling scheme would be useful.

The emphasis on land-cover categories is traditional, and appropriately so, because it is the method with which most land-change scientists and probably all of the public are familiar. With all the Landsat data already processed to the level where other calculations could be done quickly, the use of continuous-field information (e.g., NDVI, thermal properties, change-vector analysis) would be straightforward. A part of the land-change science community is investigating how to use continuous-field data for land-change analysis, and the data sets would provide excellent material for conducting these studies.

2. Methodology: do the (a) experimental design and (b) statistical methodology support the goals

As stated in objective 1 above, the methodology employed is based on probability sampling to extract land cover and detect changes. A stratified random sampling scheme is used. The strata used are based on ecoregions developed by Omernik (1987). At the beginning of the project, each ecoregion was fitted into 20x20 km square grids, and a random sample of 10 to 11 grids was selected. Land-cover information is then extracted from the sample grids. The rates of change of land cover in each sampled grid for the five periods between 1973 and 2000 are calculated and extrapolated to the ecoregion as a whole. The sampling approach has been adopted because of the financial difficulty and time constraints to conduct a complete land cover mapping of the entire ecoregion. Although the sampling design appears to be sound, the use of random sampling will probably miss some important land-cover change because the size of the sample is small relative to the size of the ecoregion. The decision to reduce the grid size to 10x10 km will improve the inadequate coverage problem, but the scaling issues mentioned in section 1 remain. It is possible that systematic sampling would be better than random sampling in giving a more even coverage of the ecoregion; *this should be tested*. It would be interesting to compare random sample results with systematic sample results. To verify the usefulness of the probability sampling, a complete land-cover map of several ecoregions (wall-to-wall analysis) should be attempted for comparison with the sampled results of the same ecoregion.

For this project, the land cover for each sample grid is manually (visually) interpreted from the images by many workers. We were told that consistency in interpretation is maintained through group discussions and field verifications. Manual interpretation has the advantage that minor details can be detected easily, but it is very time consuming. Some types of automation using new image classification software packages (e.g., Feature Analyst or e-Cognition) should be explored. Usually the manual interpretation approach is adopted when aerial photographs are used. Aerial photographs have very high spatial resolution that facilitates visual interpretation. In this project, the spatial resolution of the image data is resampled to 60 m from MSS and TM/ETM data, which is really not that suitable for manual interpretation. An approach that combines computer-assisted image classification with manual corrections of errors is probably the best, and should make it possible to do a complete analysis of the ecoregion rather than rely on probability sampling.

Finally, as noted above, the use of ecoregion as the stratum should be investigated more thoroughly. Are other regionalization schemes more suitable? Can a landform region scheme (e.g., mountains, plateaus, plains) be used? Is it possible not to base on any region at all?

To summarize: In response to Question (a), the research design is sound, but some fine-tuning is needed. In response to Question (b), the basic assumptions of the statistical procedures should be tested more thoroughly (as detailed above).

3. Personnel, management arrangements, environment, and support:

From talking with both current and former employees, we understand that the project has a supportive and congenial work environment at the EROS Data Center. We do not know the work environment at the other USGS locations involved, but hope the positive environment at EROS spills over to impact the other sites. The identification of 'a dilemma' associated with the project leads us to wonder about management arrangements and support. That dilemma concerns the status of the effort as a research investigation or as an operational effort to assess every ecoregion in the conterminous US. *Not enough funding (in the way of personnel and time allocation) is being provided to the USGS Land Cover Trends research project to accomplish both the research and country-wide analysis efforts*. Concern also exists when individuals assigned to work on the project have other work assignments that they might consider to be more important or involve a greater percentage of their time. Our impression is that the current workforce has been well-trained, but that the workforce is just adequate in size to accomplish the country-wide assessment aspect of the effort in the timeline that exists. More resources should be invested in building the pool of those trained to work on this project. We recognize that there is a relatively steep learning curve in order for a new employee to be useful to the effort. Project management needs to recognize the need to maintain the pool of employees who can contribute to this or similar spin-off projects and anticipate the need to increase the pool if future projects demand a larger cadre of employees. It is important that these

employees not only know how to do the land cover interpretation, but that they be trained to recognize the importance of their work in helping us better understand how the world is changing and the related implications. Management should ensure that mechanisms are in place to provide the psychological rewards that will enhance employee performance and maintain a commitment to the Project. Finally, the value of the research findings should be communicated up the 'chain of command' within USGS, to a broader scientific audience, and to key decision-makers in Washington, DC.

With regard to the scientific leadership of the project, there is some concern over the fact that the originator and major driving force of the proposal, Dr. Thomas Loveland, is no longer officially associated with the project, although he is clearly heavily involved. His possible absence from the project would be unfortunate.

4. Budget:

We believe that this project is under-funded. However, it is difficult to assess the Project's overall budget. We recognize that the research team wants to accomplish more than they are able to do, given the current funding level. It would be helpful to compare the cost of this effort with other efforts, such as the USDA's Census of Agriculture or the Natural Resource Inventory (also believed to be under-funded).

5. Publications, Outputs and Outcomes:

Overall, the quality and quantity of publications is good for this stage of the Project. However, the research team should strive for publications in higher profile and more widely read journals such as *Science*, *Nature*, or *PNAS*. USGS reports and publications in remote sensing journals are important and appropriate first steps, but the journals mentioned above are more likely to have a greater impact by reaching a broader community of users that can utilize the results of the existing work and driving the need for and direction of future work. The USGS should also make a more concerted effort to form partnerships with potential users and academia to highlight the usefulness of the research products and to examine the causes and consequences of the documented changes. The work already completed in the Eastern U.S. makes it a prime location to probe these issues more deeply; addressing these issues should be a priority for future work. Developing a regional center (real or virtual) could be an appropriate vehicle to engage regional users and researchers in this process. We also believe that the USGS should do more to develop a web presence for the Project and its results. For example, few of us previously knew that much of the data and notes developed in the Project were actually available through an EPA website, partly because this information was not directly linked to or highlighted on the USGS site.

6. Improvements to strengthen the project, its results, national and international standing

The Land Cover Trends Project Team has done an exemplary job of documenting rates of land-cover change for many regions across the US. The crucial next step is to engage the broader scientific community to focus future efforts on investigating the causes and consequences of land-cover change.

One member of the review team stated that the next research steps are really about moving from study of land-cover change patterns to studying land-cover change as a process. Only by understanding the factors that drive land-cover change can accurate land-cover change forecasts be made. Such work must involve more than anecdotal relationships between past land-cover change and distal causes (e.g., land-cover changes in Florida driven by sugar policies). The team has begun to work with non-USGS researchers to employ the database to assess consequences of land use change. Activities like the climate projects assessing how land-cover change impacts mesoclimate and NOAA weather station data or the carbon modeling work are just the types of collaborative activities that will enhance the scientific standing of the Land Cover Trends products.

Expanding the scope of work investigating causes and consequences of land-cover change trends will greatly advance the national and international scientific standing of the project, and will pay real dividends in terms of the value added to end-use applications described in the research proposal. Obviously, completing a nationwide assessment using the current methods is a necessary goal. However, we also recognize that the power of this work comes from understanding *why* land-cover change happens and what it means for those areas that are changing. There are several real, on-the-ground issues that matter to people (urban sprawl, agricultural expansion for biofuels, wetland loss, etc.). Quantitatively documenting the impacts of such change and advancing the ability to forecast future change by understanding the drivers will have a much more substantial impact than any of the proposed methodological improvements mentioned. Documenting the value of the work done by furthering understanding of land-cover change causes and consequences is very likely to increase use of the data set, which will in turn bolster support for new database development work both within USGS and across the scientific community.

7. Future directions (given new data, technology, etc.):

The amount of new data and technological developments are accelerating. A clear set of questions and clear articulation of the most important uses/analyses of the land-cover data are needed to make the best use of these new developments.

Some of the future directions the panel would suggest are clearly those that the team has considered or is already moving toward. As noted in Section 1, answering the “why” questions requires moving toward statistical analysis and modeling, GIS analysis, and scenario modeling to gain understanding of past land-cover changes as well as potential future trends. Can a model be developed with this Project to facilitate looking back at historical data going back to the 1930’s? A retrospective model could be used to

develop hypotheses with respect to drivers of land-cover change, and to develop models of future changes to test causal hypotheses.

Future directions also should be dictated in part by the needs of potential users of the information, developing strategies and targeting specific needs of these communities. The team should consider broadening the context of the research with the perspective of fitting into longer term trends. The research team mentioned the possibility of including the mid-decadal (circa 2005-2006) dataset that is expected to be available by mid-2009, as well as other future datasets. As we have noted elsewhere in this report, this should be done to address well-formulated questions and specific needs of user groups, some of whom may be expected to contribute funding to the research effort.

Wall-to wall land-cover estimates would clearly be useful to address many user needs. The team should explore technologies to bridge the gap between random sampling and wall-to-wall estimates. One idea would be to use more frequently acquired but coarser spatial resolution data (e.g., MODIS vegetation index products) to collect more frequent estimates of land change and use these in tandem with the finer spatial resolution Landsat data products. Using new software and classification algorithms for initial image classifications combined with manual checks and corrections can also lead to complete land-cover maps without adding an unreasonable amount of work/time.