Review:

What is the intellectual merit of the proposed activity?

The PI proposes to study a series of detailed hypotheses about how interethnic interactions shape ethnic norms and cultural markers, which will address some classic questions about ethnicity, ethnogenesis, culture change, and culture conflict from the social sciences as well as predictions from recent mathematical modeling efforts and psychological studies. The study site is one where there is relatively peaceful interaction between indigenous Matsigenka living in Manu National Park and Mestizos living outside the park. Because of their peaceful interaction, they may share norms with non-coethnics with whom they interact. Hypotheses also examine predictions related to status; whomever receives the most benefit in an exchange has less power. The proposal also includes new Agent Based Models.

This is the second time I have reviewed this proposal. Last time I gave it an E (Excellent). This time I would like to give it a W, which stands for, "Wish I had thought of that!" I am specifically referring to the methods, which sound like a feasible way to identify and quantify norms and markers, aspects of culture that have received much ink over the history of the social sciences, but have rarely been measured. The PI proposes to do so with some clever experiments and vignettes. The PI has demonstrated the feasibility of these methods in some additional preliminary fieldwork.

In my previous review I mentioned that some of the predictions seemed counter-intuitive. For example: yes, I understand that in an interaction between two individuals, the one seeking the most benefit (larger marginal utility of a gain) has less power than the one seeking the least benefit (smaller marginal utility of a loss). But on the ground, the one seeking the most benefit (absolute quantity) might be the one with the greater power, as a physical, social, or psychological bully. In other words, there could be existing systems/structures of power in place by which those who have a lot and who face low marginal value of gains nonetheless seek and succeed in taking more. Actually, come to think of it, the marginal model of tolerated theft by Winterhalder (1996) also makes contradictory predictions: the one with the more utility at stake will work harder to win a conflict. I mention this to say that this project will only work if the hypotheses are tested fairly. If I had to criticize this project for anything, it would be that it does not seem to anticipate failed (rejected) hypotheses, nor clarify what will have been learned from rejecting or not rejecting hypotheses.


What are the broader impacts of the proposed activity?
While am skeptical that Machiguenga communities can (or even should) use the social science knowledge acquired from this study to decide whether a schoolgirl should be permitted to travel with her teacher for three weeks (I am referencing the final page here), I do think this is useful knowledge for understanding cultural conflict and change more broadly. The PI would teaching English to Machiguenga people and help train a grad student in ABM, which is positive.

Summary Statement

This is a well-conceived and very creative project. The creative aspects of the methodology make this a bit of a risky bet for NSF, but the risk seems low as demonstrated by the previous fieldwork of the PI in the region.

I do wonder whether the ABM work belongs as part of the same project or not. It seems the PI could do the ABM part while doing his normal duties at UC Davis-- is grant money really required for this? If the overall NSF Cultural Anthropology budget is unforgiving, it seems to me that this part of the proposed project could be scrapped and the rest funded. This may be the bias of a fieldworker speaking here, though.

Data management plan seems adequate.
Title: Ethnic Boundaries and Cultural Change in an Amazonian Population

Institution: University of California-Davis

NSF Program: Cultural Anthropology

Principal Investigator: Bunce, John A.

Rating: Excellent

Review:

What is the intellectual merit of the proposed activity?

This research proposal aims at analyzing whether patterns of interaction among people of different ethnicities (specifically Mestizo and Matsigenka in the lowlands of Peru) relate to the actual and perceived distributions of ethnic typical norms of behavior and symbolic markers. The research addresses the topic of cultural change from an innovative perspective, bringing together insights from historically-based descriptive explanations of cultural change, the understanding of how perceptions influence inter-personal and inter-group behavior, and formal agent-based models of cultural evolutionary theory. The methods of analysis combine ethnography, experimental games, and agent-based modelling.

The topic of research — cultural change, and more specifically the conceptual model of how cultural change occurs through inter-ethnic interactions — is of key importance for anthropological research. The case study seems adequately chosen for the research question. The PI — who is fluent in Spanish and conversational in Matsigenka — will collect the data himself. He has considerable first-hand knowledge of the area, including previous unrelated research and considerable pilot testing of the ideas proposed here. He seems to be well aware of logistical challenges that might hamper the execution of the research, and have the appropriate contacts to make the research feasible. The proposal is clearly conceptualized, well-written, and organized. The research itself is original, ambitious, carefully thought, and tightly written. All the parts of the proposal are well developed, from the theoretical framework, to the plan for data analysis. In sum, by most standards this is an excellent proposal.

Despite its many qualities, I would like to highlight several points that the PI might want to consider, should the proposal be funded.

First, the proposal set Mestizo and Matsigenka in a kind of vacuum environment, ignoring interactions with other actors. Actors that might potentially interact with residents from the two proposed communities of study include other mestizo and Matsigenka (not mentioned) and members of other cultural groups (tourists, members of NGOs, anthropologists, park employees, and missionaries). I think it is important to take into account consideration with all these other actors, and I would recommend to incorporate a) a description of interactions with actors outside the two study communities, and b) considerations on how interactions with those other groups might affect findings of this work. Similarly, the author should consider how historical relations between the two groups (or with other groups) might affect the pattern of inter-ethnic interactions that we see nowadays. For example, the Matsigenka seem to have been avoiding contact with other groups, at least in the past. What does this historical situation means for the study of inter-ethnic relations nowadays?
Second, the PI might also want to give more thought to the selection of participants. As it is now, 30 participants would be selected at random. But many of the hypotheses depend on having enough variability in inter-ethnic interactions. A way to increase variation would be to separate the population in two groups (high and low level of interaction) and then select at random half of the sample from each of those two groups.

Third, although the methods of data collection and data analysis are generally well developed, I see several places where they could be improved. The author mentions that ethnographic information will be analyzed through content analysis. I think this deserves a little more explanation. Similarly, the author wants to use Agent Base Modelling to model inter-ethnic relations. The choice of this type of modelling is not well explained (nor justified). Why Agent Base and not other type of modelling? How will the author proceed to set the model? How well the model be run? In sum, more detail on the selection and execution of the ABM will be useful. Finally, at some point the author mention that he will collect information to elaborate ego-centric networks, but this idea is not followed up, nor well integrated in the proposal.

What are the broader impacts of the proposed activity?

The author seems to have experience in interacting with the communities to provide them with some benefits, and he expresses his willingness to do the same again. But, while the intention might be there, the issue is not very well developed. It also seems that one of the two communities (the Matsigenka) will have more activities than the other. Is there any reason for that?

The PI ties with local partners seem strong, and this project will support and strengthen them for potential long time research in the area. Thus, the project contributes to enhance long-term research and commitment to local people, two much-needed attributes in research.

Summary Statement

This is a generally well-organized and conceived project. I have some concerns regarding methodological issues, but since the PI seems familiar with the area and the challenges of the proposed research, I have no doubt that he can convincingly address those issues.
Title: Ethnic Boundaries and Cultural Change in an Amazonian Population

Institution: University of California-Davis

NSF Program: Cultural Anthropology

Principal Investigator: Bunce, John A.

Rating: Very Good

Review:

What is the intellectual merit of the proposed activity?

The last thirty years has seen the development of a large body of theory on cultural evolution (e.g., Feldman, Boyd, Richerson, Laland). Unlike prior theory of cultural evolution in anthropology (e.g., Leslie White, Julian Steward), the recent work has focused on population-level processes, as opposed to group-level processes. These models study interactions between individuals and the population-level consequences. A reasonable criticism of this paradigm is the lack of empirical support.

I think that the kind of research proposed by Dr. Bunce is exactly what is needed. Dr. Bunce's research seeks to understand how patterns of interaction among individuals within and between ethnic groups relate to the actual and perceived distribution of ethnic norms and markers. The study of a group-level phenomenon like ethnicity requires a micro-level study of individuals and their interactions, a study like the one Dr. Bunce proposes. This research should be relevant to a broad swath of social scientists, including anthropologists, social psychologists, and behavioral and developmental economists.

Dr. Bunce cites the relevant background theory and his proposed studies are clearly specified. While I don't see any previous research on Dr. Bunce's CV related to this proposal, he seems to be well qualified to conduct the project. He has spent a fair amount of time over the last few years working with a Matsigenka community in Peru. He seems to be proficient in the language as well as fluent in Spanish. Given that his wife works in the same community (on a different project), I think that Dr. Bunce will be able to carry through with the project.

What are the broader impacts of the proposed activity?

I have only recently completed my PhD and have never conducted ethnographic field work, and so I don't feel myself well-qualified to address this issue. I feel like the research project has strong intellectual merit. It's not clear to me how the project will directly meet this, second criterion. That said, Dr. Bunce has been teaching English to the Matsigenka children. This seems like a merit.

Summary Statement

I feel that this project has strong intellectual merit and should be funded. The development of a population-based theory has revitalized the study of cultural evolution. What is now needed is follow-up ethnographic research, allowing for the testing and improvement of models. Dr. Bunce's proposal does this.
The intellectual merit of this proposal is very good. I am very excited about the data that could be collected in terms of their implications for advancing our understanding of how culture change occurs in association with 'ethnic boundaries'. This issue is of increasing relevance as previously isolated populations increasingly become integrated with wider regional political economies. There is indeed very little empirical individual-level evidence of the mechanisms by which culture change occurs, and this study could do much to further that limited understanding. The intellectual merit is limited, however, by a rather cursory overview of existing theories of culture change, from evolutionary perspectives or otherwise. I would advise the author to explore the cultural transmission and related literatures more thoroughly, and to tie together his hypotheses more explicitly, taking care to consider Tinbergen's 'levels of analysis' and the extent to which hypotheses are compatible or mutually exclusive. The PI seems sufficiently prepared to carry out the proposed methods. Access to resources within Peru is adequate. The proposal would be strengthened if the PI drew more from the intellectual resources at his home institution (UC Davis) in terms of advisory personnel (e.g., Peter Richerson, Richard McElreath, etc.) Further suggestions are summarized below.

What are the broader impacts of the proposed activity?

The PI has highlighted several important impacts that this study could make to the scientific community and the communities with which he proposes to work. It will contribute to the training and career potential of an advanced graduate student at UC Davis and to training of field assistants and students of English in Peru. The theoretical and practical implications of understanding mechanisms of culture change are presented almost too weakly in my view. One wonders how a community becomes 'empowered' by knowledge of the association between ethnic markers, boundaries, and associated norms and behaviors, for example. At the same time, the projects is of clear relevance to significant concerns the world over regarding 'cultural preservation', discrimination and coercion directed toward members of smaller 'out-groups', etc. The PI could make the links between his work and those areas clearer to strengthen the broader impacts of this research.

Summary Statement

This project proposes to examine the mechanisms underlying culture change, including associated norms and markers as it occurs in the presence of 'ethnic boundaries' and cooperative pursuits ('coordination interactions'). Its hypotheses are motivated by evolutionary and sociocultural
anthropology and by social psychology, insuring a holistic perspective on culture change and broadening the potential audience for associated findings. The intellectual merit of the study is strong in that it could advance empirical understandings of hitherto largely theoretical phenomena and the broader impacts, while stated most clearly at the end of the proposal, are clear in the context of global market integration. The proposal's main weaknesses have to do with limited contextualization of the theoretical paradigm (i.e., the literature review is too narrow) and with small sample sizes and potentially limited variation in inter-ethnic associations that are proposed to affect adoption of new behavioral norms and markers. The PI is competent to carry out the proposed research, which, inasmuch as it is somewhat limited theoretically, would nonetheless produce results of substantial interest to scholars interested in processes of culture change if the study population can reveal enough variation to disentangle the factors/mechanisms associated with general processes of culture change. I would thus offer two main suggestions to improve this proposal: 1) think more synthetically and more broadly about the existing models of culture change and the various factors which may motivate it; and 2) convince your reviewers that your population is not so limited by small sample size and inter-cultural exchanges that it will fail to reveal anything about the mechanics of culture change. More specific comments follow.

1) The author proposes to synthesize theories from several disciplines, but has missed some important literature within evolutionary anthropology that could be of interest. In particular, Marc Feldman and colleagues have discussed general models of 'cultural diffusion' that should probably be referenced in terms of their applicability here. Dual-inheritance transmission models are of obvious relevance to adoption of new norms and markers and are only briefly referenced in this proposal. Rogers' Diffusion of Innovation is also interesting for the insights it gives on how individual attributes affect the likelihood of adopting new 'innovations'. Richerson and Newson also propose a model of culture change that has to do with the diminishing influence of kin with 'modernization'. This is of obvious relevance given the vignettes that have been used in pilot studies for this project.

2) Models have been proposed that assess the relationship between 'perceptions' and 'behaviors' (e.g., Melissa Brown and colleagues' discussion of son preference and associated behavioral markers). This might provide a useful guide in endeavours to assess the content of norms, which are obviously difficult to assess, even via questionnaires of well-known individuals.

3) What are the economic influences on individual behavior and adoption of new norms? Would culture change occur even in the absence of those motivations?

4) The link to historical aspects of culture is unclear and, in my opinion, unnecessary.

5) The examples provided on page 5 of culture change in the US context are somewhat distracting and should either be cut or more explicitly related to the study predictions. For example, do these two contexts indicate that the content of inter-ethnic interactions is important? If two groups converge in unsaturated territory are they more likely to retain markers than if one group is forcibly subsumed by another?

6) The mechanisms provided by the social psychology aspect of the study are interesting and worth pursuing.

7) The motivations for culture change might be discussed more thoroughly. The context of cooperation or 'coordination interactions' could be fleshed out, but other motivations (e.g., see point 3 and 5) could be considered.

8) I would be aided by a final summary of the variables, including individual attributes, that the author suggests may be associated with norms, markers, etc.
9) The methods are generally well-justified, but I have concerns about sample size (35 households and 75 adults) for the main community under study. It is very unlikely in this context that something as complex as the HLM proposed will pick up meaningful differences with such a small sample. More importantly, given how isolated the Matsigenka community is, how likely are these temporary interactions to affect permanent or observable norm and marker changes? Is there any variation in the display of markers within the community? If so, why? Is there enough variation in interethnic exchange in this small community to see how this covaries with the adoption of markers and norms in a manner more systematic than the summaries provided from the pilot studies? Are individuals strategizing about long-term benefits of adopting new customs? The motivation for adopting new markers within the isolated community setting is still very unclear.

10) The gathering of social network data is well-justified and interesting.

11) Too long is spent among the Matsigenka relative to among the Metizos, given the authors’ previous familiarity with the former and the relative complexity of the latter. The length of time should be better justified or adjusted. The PI might also consider whether he needs time to perform preliminary data analyses and adjust methodology. Too little time is given to the agent-based modeling.

12) The agent-based models sound interesting and should complement the cross-sectional data nicely. Does the PI intend to use R for the ABM? Other software (e.g. Repast) might be better suited to the task.

13) The broader impacts are much better stated in the concluding sections of the proposal than in the abstract. The PI is contributing meaningfully to the study community and to the scientific community.

14) The budget seems reasonable.
Title: Ethnic Boundaries and Cultural Change in an Amazonian Population
Institution: University of California-Davis
NSF Program: Cultural Anthropology
Principal Investigator: Bunce, John A.
Rating: Good

Review:
What is the intellectual merit of the proposed activity?
This research represents an empirical test of a formal model of ethnic markers as indicators of norms. Because social scientists have often made conclusions about these topics without the benefit of empirical evidence, further ethnographic research along these lines is needed.

What are the broader impacts of the proposed activity?
In addition to the PI's efforts to enhance the education of Matsigenka children and the inclusion of students in the project, the research has potential to shape recommendations for inter-societal interactions, not only in remote settings but also in political arenas.

Summary Statement
This proposal addresses a topic that merits additional attention, specifically the ability of ethnic markers to convey information about anticipatable norms and behavior, as well as the extent to which increasing contact between distinctive populations affords opportunities for the situational display of markers. Empirical evidence that speaks to these questions would likely appeal to a broad variety of anthropologists while providing a basis for predictions about the effects of globalization on the cultural norms and markers of formerly semi-isolated indigenous populations. This proposal draws on a theoretical model that was developed by anthropologists who have given careful consideration to these problems, and the research design includes a mix of qualitative and quantitative approaches that will likely lead to valid, replicable results.

Despite my overall enthusiasm for this proposal, I have a few concerns and suggestions.

First, as someone who has worked in mestizo/caboclo communities in which the residents regularly encounter indigenous people, I suspect that the mestizo residents of Boca Manu first notice the phenotypic differences of the Matsigenka people that they encounter and that any subsequent discernment of ethnic markers primarily allows for modifications of their general expectations for indigenous people. For example, as a Matsigenka woman approaches a shop in Boca Manu, the shopkeeper probably discerns immediately that she is indigenous and begins to calibrate his expectations accordingly. If he observes that she is wearing a digital wristwatch, though, then he may infer that she has had experience dealing with the norms of market interactions in mestizo Peru, which may lead him to expect different (and likely preferable) behavior than if the woman appeared more
"traditional." But ethnic markers such as wristwatches only complement the information conveyed by the phenotypic differences, which presumably leads to different expectations of behavior for the Matsigenka than for identically-clad mestizo women. I therefore wonder to what extent this research would be better-suited for a setting in which different ethnic populations are (for the most part) phenotypically indistinguishable, given the relatively greater potential for ethnic markers to serve as a differentiating signal in such settings.

Meanwhile, the Matsigenka individuals from Tayakome who regularly travel to Boca Manu presumably do not constitute a representative sample of Tayakome residents. That is, those who go to Boca Manu are probably more motivated than other Matsigenka residents to adopt mestizo norms. Indeed, the data from the PI's pilot study seem to suggest this interpretation, and the proposed "out-group interaction index" addresses this variation. This leads to two suggestions. First, the (random) sample size of 30 participants in each community (total n = 60) might reveal only strong effects for the variables of interest (e.g., the interaction of the out-group interaction index by ethnic group) given the inclusion of other covariates in the model (and certainly variables such as age and sex are likely to predict propensities for emulating mestizo norms as well). As always, expanding the sample size, even modestly, would increase statistical power. Second, when asking residents of Boca Manu about interactions with Matsigenka (from across the "ethnic boundary"), the majority of their interactions will likely be with fairly acculturated Matsigenka residents. A research design that allows the PI to more closely investigate the rarer interactions with less acculturated (and less "marked") Matsigenka visitors might be warranted.

Perhaps the most problematic aspect of the research design is the proposal to elicit ego-centered social network data that will be used to generate a "social network grouping variable" that will be used as a random effect in the mixed-effects logistic model(s). Given that networks are often composed of overlapping cliques, considerable attention has been given to the methods for identifying discrete subgroups within a network (that is, the kind of categorical variable suggested by the PI's plan for inclusion in a mixed-effects model). Most of the currently-favored methods, such as the Girvan-Newman algorithm (2002. PNAS 99:7821-7826), were developed after the publication of the Wasserman and Faust (1994) reference that was cited by the author. It is not clear how the PI plans to identify subgroups, which appears to be especially complicated in this case when working with a unique kind of ego network (that is, the data collection begins by limiting ties to those who are considered knowledgeable about a domain and which further does not include ties among ego's alters). Given the lack of whole network data, greater consideration to the challenges of subgroup identification within the network(s) will likely be needed.

In terms of the analysis of the vignettes, multilevel logistic regression seems appropriate. Rather than analyzing each of the six vignettes separately, perhaps the PI should consider aggregating all of the binary responses into a single dataset, then including "vignette" as a categorical covariate while also including the identity of the participant as a random effect. From my perspective, this approach would have one key advantage, namely that by developing "empty" models that include only the random effect and the intercept, the PI could first characterize the extent to which individuals vary in their responses to the multiple vignettes, then determine the extent to which covariates (such as ethnicity) explain the individual-level variance. In other words, the individuals' responses to the vignettes are likely to be correlated (if you know how an informant answered one vignette relative to others'
responses, then you can probably predict responses to other vignettes as well), which can be advantageous given that individual-level characteristics are the primary focus of this study. If the PI adopts this approach, then I would encourage him to consider interaction terms with the "vignette" categorical variable (for example, interaction terms of "Vignette 1 by ethnic group", "Vignette 1 by worldview," "Vignette 2 by ethnic group," etc.) in order to account for the anticipated variation in responses to the different vignettes. Such models can be cumbersome (and perhaps unnecessary in many cases), but these interaction terms essentially allow researchers to generate the same kinds of predictions that one can make with separate analyses while gaining the benefit of insight into individual-level variation across the vignettes.

In terms of the proposed timeline, it seems odd that the PI would spend a disproportionate amount of time in Tayakome (as opposed to Boca Manu) given that (1) he has already spent considerable time in the community and (2) few of the Matsigenka-Mestizo interactions occur in Tayakome. The PI writes: "Twelve months are required in order for Bunce to assure (through extensive participant observation and discussion with informants) the accuracy of his interpretations of the many unique Matsigenka norms and markers." Is there an assumption that the Matsigenka norms will be more difficult to discern and interpret than the Mestizo norms? Such assumptions might be justifiable (if we assume that Mestizo culture more closely resembles the PI's and that his relatively greater fluency in Spanish than Matsigenka will facilitate his research in Boca Manu), but given that the research design is otherwise symmetrical, it is not clear why the timeline would be heavily skewed toward Tayakome.

Regarding the concerns of the panel about the previous submission of this proposal, my impression is that the concerns have been satisfactorily addressed in this resubmission.
Title: Ethnic Boundaries and Cultural Change in an Amazonian Population

Institution: University of California-Davis

NSF Program: Cultural Anthropology

Principal Investigator: Bunce, John A.

Rating: Very Good

Review:

What is the intellectual merit of the proposed activity?

This study examines the intercultural dynamic between Matsigenka and mestizo populations in southeastern Peru, specifically in terms of the distributions of ethnic-typical norms of behavior and symbolic markers across an ethnic boundary. The PI seeks to field test various predictions from his simulations of McElreath et al.'s (2033) agent-based cultural evolutionary model. This is a resubmission that benefits from an additional pilot study and further data collection. As I wrote in the earlier review, "The PI recognizes that the relationships and dynamics he proposes to study are complex, yet by examining these patterns via concrete, measureable variables (e.g., individual variation in the frequency and nature of inter-ethnic interactions, covariance of markers and norms), he can potentially elucidate trends in inter-ethnic contact to move our understanding forward, away from prevalent assumptions of unidirectionality. Bunce's research design is clearly explained and well developed, and the pilot study results are intriguing."

Some concerns:

In the earlier proposal, there was mention of an advisory committee of senior anthropologists. What has happened with this idea?

Where is the letter of support from FENAMAD?

What are the broader impacts of the proposed activity?

In the earlier version, the broader impacts entailed dissemination of reports to the study community, administration at Manu National Park and a local university, as well as the PI's involvement in teaching environmental education, English and math. In this revision, the PI mentions dissemination in communal meetings in the study communities, and summaries for the villages and the indigenous federation (in addition to the park and PUCP). What other benefits potentially exist for the Matsigenka and mestizo participants? For instance, how can this research be made more participatory?

Summary Statement

This proposal has solid intellectual merit in the commendable development of a research design that examines hypotheses about the relationships among individual interactions, individual perceptions, and ethnic group-level cultural characteristics. The PI is willing to undertake long periods of field research, and has made efforts to undertake more ethnographic research. I would like to see the PI relate the
study to larger issues and real world implications, with stronger broader impacts.
Title: Ethnic Boundaries and Cultural Change in an Amazonian Population

Institution: University of California-Davis

NSF Program: Cultural Anthropology

Principal Investigator: Bunce, John A.

Rating: Very Good

Review:

What is the intellectual merit of the proposed activity?

The objective of the proposed research is to explore the process of cultural mixing between Matsigenka Amerindians and nearby Mestizos in Manu Park (Peru). It attempts to relate patterns of social and economic interaction with the distribution of perceived and actual cultural norms and ethnic markers of members from both groups. The broader goal is to understand how culture changes and impacts decisions. The project's theoretical basis stems from cultural evolution models where shared norms are thought to help improve coordination and group cooperation, but data collection will likely not just test a model, but hopes to make models more realistic. There are ten hypotheses to be tested and an agent-based model to be constructed subsequent to data analysis.

Overall, this is a creative proposal that should shed some insight into the role of ethnic identity in structuring social relations. To my knowledge, there hasn't been a proposed project like this before, that attempts to explicitly measure actual and perceived norms and markers, relate these to patterns of inter-ethnic interaction, while testing some ideas about the conditions that should lead to norm adoption of the dominant group (Mestizos). This is a revision of a prior submission that I did not read. It seems like the main obstacles from the previous critique (lack of ethnographic experience and Matsigenka language skills, clarity on how to identify norms, lack of consultation with indigenous federation) have been adequately dealt with.

I applaud the efforts of the PI to embark on an important and interesting study. I do, however, have a few concerns (that prevent me from giving an Excellent score):

1. The PI will spend a relatively long time in the field, yet the sample size of interviewees will be quite small (n=30 in each of the two sites). The PI should attempt to collect a full sample (should be a little more than double the stated sample size given the census numbers given), as the PI will lose statistical power once controlling for sex, age, ethnicity, worldview, experience with outsiders, status, etc. Without a power analysis, it's hard to know whether research goals can be accomplished with an n of only 30.

2. The proposal repeatedly refers to "cultural change" but it is unclear to me how to really measure change in a cross-sectional study. How will the PI distinguish between whether Mestizos and Matsigenka share markers and norms out of common heritage, convergent beliefs or similar ecology, joint exposure to missionaries, versus cultural transmission from inter-group interaction, etc.? As the PI writes, most Tayakome residents speak Spanish and so presumably their exposure is already multifaceted.
3. The agent-based model is an interesting addition and I appreciate the desire to mix empirical data with a simulation, but the model is not clearly motivated. What will be its goal and how will data/results influence model specification?

4. The PI states that the remote location and isolated communities make the Manu region an ideal location for study. It's unclear, however, how much Matsigenka exposure Mestizos have had from Matsigenka living in other villages. And what is the context of Matsigenka-Mestizo relations? It doesn't appear that Matsigenka and Mestizo are competing over land or other resources, and so is there anything other than proximity that structures their interactions?

What are the broader impacts of the proposed activity?

First, the proposed project will help improve understanding of cultural transmission and ethnic identity among two groups living in close proximity to each other in the Peruvian Amazon. Second, it will hire and train an indigenous translator, and will employ and train a graduate student. Third, the PI will teach English (as requested by the study participants) in the study communities. The PI will also produce Spanish and Matsigenka-language reports for communities, local universities and indigenous organizations. The aspiration is that the research will help lead to the designing of “effective cultural management plans”. Finally, the research will be of interest to cultural anthropologists, evolutionary anthropologists, social psychologists and indigenous rights activists.

Summary Statement

The proposed project assesses how cultural norms of groups vary based on patterns of social interaction. Research conducted among Matsigenka and Mestizos in the Peruvian Amazon over two years will collect data on group-specific norms, ethnic markers, inter-group perceptions, worldviews, and test a series of hypotheses concerning the strategic use of ethnic markers and shared norms in structured (coordinated) interactions, and about perceptions of out-group norms. The research stands to contribute broadly to the social sciences, and so its broader impacts are not trivial. While previous problems with the original submission are adequately addressed, a few concerns remain: sample size, lack of clarity regarding cultural variation versus change, more detail needed about the goals of the agent-based model, and greater context needed concerning Matsigenka-Mestizo interactions (to better motivate understanding of the inter-ethnic relations).
Title: Ethnic Boundaries and Cultural Change in an Amazonian Population

Institution: University of California-Davis

NSF Program: Cultural Anthropology

Principal Investigator: Bunce, John A.

Rating: Excellent

Review:

What is the intellectual merit of the proposed activity?

Intellectual Merit

1. How important is the proposed activity to advancing knowledge and understanding in Anthropology?

Strengths

Will lend important empirical findings for a new area of study—the modeling of diffusion and evolution studies that trace the influence of group-oriented signs and customs through time.

Takes into account inter-group political/power hierarchies, which is seldom included in modeling studies.

Studies of ethnic interaction are a long-standing concern in anthropology, and are addressed here in important new ways.

An emphasis on strategic adoption in individual and group culture change is included, which is too often missing in the modeling of diffusion/adoption studies.

The selection of study site is well suited to the research question while allowing for the integration of conventional ethnographic methods. The study will leverage traditional ethnographic techniques to important historical and contemporary questions.

The study concludes with the development of an agent-based model based on the data collected that will allow for further experimentation based on the results. This work is based on established models which will be supplemented by new agent-dynamics, which will likely prove influential in future modeling of cultural diffusion.

Weaknesses

The lack of inclusion of state-level actors and directly addressing the legacy of colonialism (past as well as present) is a weakness. This need not necessarily impact the data collection, but may be important in understanding the dynamics uncovered. Put simply, differences in hierarchy are not always generic—the creation of in-group out-group markers and individual levels of adoption of these markers can have very different trajectories depending on how they came about. The PI may wish to look at work by Gerald Sider on indigeneity more generally, and Steven Rubenstein on Amazonian groups—both of whom have tried to address some of these questions in the specific context of ongoing and historic colonialism. This is more central to the interpretation of the findings than the data collection and analysis described in the proposal.
2. Beyond Anthropology?

Strengths

This area of research is drawn upon by many fields including historical sociology, evolutionary psychology, social psychology, studies of cultural evolution, and modeling studies. This project will contribute important field observations and data too often lacking in overly speculative work in these fields.

Weaknesses

The small scale nature of the study group will somewhat limit the findings for application to larger macro-scale concerns with borders and integration/conflict.

3. How well qualified is the researcher to conduct the project and what is the relation to prior work?

Strengths

A pilot project has been carried out, including testing of the experimental methods. The PI has worked in the region as a Physical Anthropologist, and has familiarity with the region. Interestingly, the PI's background as a Physical Anthropologist has contributed an important emphasis on methodological rigor that is often missing in more open-ended ethnographic approaches. This is a very welcome strength of the proposal.

Weaknesses

The primary background of the PI is in Physical Anthropology, which may indicate some lack of depth in his background in issues of ethnic boundary theory. Clearly, though, he has developed much of this background independently and will gain more as the development and presentation of the study findings move forward.

4. To what extent does the proposed activity suggest and explore creative, original, or potentially transformative concepts?

Strengths

The integration of novel statistical modeling with ethnography remains discouragingly rare in Anthropology, and will be advanced by this project. The fact that some of this analysis will take place in the field and serve as the means to modify and develop the direction of the research is especially good. The use of experimental techniques—asking respondents to project the opinions and reactions of others (within and across groups) based on varying amounts of marker/context information—is innovative and interesting.

Weaknesses
No weaknesses

5. How well conceived and organized is the proposed activity?

Strengths

This is a very well described, clear, hypothesis-driven study that links research questions to specific research protocols.
The use of Principal Component Analysis as a way of modeling in-group and inter-group cohesion is interesting and novel.

Weaknesses

No weaknesses.

6. Is there sufficient access to resources?

Strengths

Yes, as planned the process is highly feasible.

Weaknesses

No weaknesses.

What are the broader impacts of the proposed activity?

1. How well does the activity advance discovery and understanding while promoting teaching, training, and learning?

The PI is currently working as a research affiliate with little access to graduate students. This is a shame because the projects innovation could easily be taught, and greater participation (labor!) at the field site would enhance the project. Not being in a position to influence students, the current impact of the project is somewhat diminished. However, this is high quality work and it would seem that the impact, while delayed, is likely to be significant.

2. How well does the proposed activity broaden the participation of underrepresented groups (such as gender, ethnicity, disability, geographic, etc.)?

See above.

3. To what extent will it enhance the infrastructure for research and education, such as facilities, instrumentation, networks, and partnerships?

See above.
4. Will the results be disseminated broadly to enhance scientific and technological understanding?

The PI shows an early but ambitious publishing record. He writes well and there is no reason to suspect that the results of the project will be readily available.

5. What may be the benefits of the proposed activity to society?

Discussions of culture change, particularly those arising from Evolutionary Psychology, have had a significant (and generally negative) effect on public discourse and, sometimes, policy. This study will contribute empirical data to a topic that is currently largely informed by uninformed opinion. As is often the case for Anthropology, the most important social impact is likely to be a much needed correction on public understandings of past (and therefore present) social and emotional dynamics.

Summary Statement

This is a very well-thoughtout proposal that is likely to make an important contribution to an emerging field that centers on anthropological topics and is engaged in by many beyond anthropology.