

July 8, 2008

Michael D. Schulman, PhD
Editor, *Rural Sociology*
Dept of Sociology/Anthropology
North Carolina State University
Box 8107
Raleigh NC 27695-8107

Dear Dr. Schulman:

I am submitting for review by *Rural Sociology* the manuscript, "Consumer Trust in the U.S. Food System: An Examination of the Recreancy Theorem." This submission includes a title page that lists all authors and their institutional affiliations. The manuscript has not been submitted or published elsewhere.

I will be serving as the corresponding author for the manuscript. The co-authors have agreed to the byline order and to the submission of the manuscript in this form. I have assumed responsibility for working with them through the editorial review process. The co-authors and I do not have any interests that might be interpreted as influencing the research and ASA ethical standards were followed in the conduct of the study.

The scholars cited in the list of references would be appropriate to review the manuscript with the exceptions of Peter F. Korsching and Wendy J. Harrod, who would have conflicts of interest.

Sincerely,

Stephen G. Sapp, PhD
Professor of Sociology

September 14, 2008

Dear Steve and Co-Authors:

Thank you very much for submitting your manuscript "Consumer Trust in the U.S. Food System: An Examination of the Recreancy Theorem" for review by Rural Sociology.

We have received three reviews of your paper on consumer attitudes about risk in the food system. As in many cases, the reviewers' are not in complete agreement about the manuscript. Some of the more critical comments relate to the limitations of the survey data and sample and the connections between the theory and the empirical analysis.

Accordingly, I invite you to revise and resubmit your paper to Rural Sociology. I have to caution you, however, that a revise-and-resubmit is no assurance of an ultimate acceptance, especially in the case of major revisions. When you revise and resubmit the paper, please detail for me and the reviewers how you addressed our concerns. Please submit the response to comments as a separate file and not part of the cover letter. Also, please add "table 1 about here" in the text to indicate where table should be placed.

Sincerely,

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In-House Reviewer

In cases where reviewers disagree, we do an intensive in-house review, including the solicitation of additional comments from a local reviewer. These additional comments from the local reviewer are as follows:

1. How can a comprehensive model for "trust" be developed when there is no general agreement within the literature as to how this concept is defined? Research on trust stems from a variety of academic disciplines such as philosophy, psychology, economics, and management information systems. Therefore, it is not surprising that "trust" is a topic of considerable discussion with no universally accepted scholarly definition (Grabner-Kraeuthner, 2002; Rousseau, Sitkin, Burt, & Camerer, 1998). For instance, some treat trust as static and some posit more dynamic development over time (Gallivan & Depledge, 2003; Gefen, 2000) which was not addressed in this article. Thus, it seems unlikely that two variables such as perceived competence and responsibility of institutional actors would be effective in predicting public response during naturalistic risk communication.
2. The literature review could be a bit more comprehensive. A number of theoretical frameworks (not described in this manuscript) have been used to describe public response to warning messages (Lindell & Perry, 2004; Mileti & Peek, 2000; Wogalter, DeJoy, & Laughery, 1999).
3. The authors seem to believe that the public will encounter only one source of information at a time and this is simply not true. Because credibility varies between individuals, warnings may be more believable to a larger segment of the population if they come from a mixed panel of scientists, public officials, reputable organizations, and familiar persons (Drabek & Stephenson, 1971). In fact, people are more likely to pay attention to warnings when they perceive that the source of information is "in the same boat" that they are; thus, shared involvement between the source and the receiver is likely to enhance risk perception (Aldoory & Van Dyke, 2006).
4. Page 8, the risk communication literature within human factors psychology (see Wogalter, 2006) and disaster warnings (Lindell & Perry, 2004) indicates that social-demographic factors play a key role in determining how people respond to public risk communications such as warnings. For instance, previous research has reported that women are more likely than men to believe disaster warnings (Turner, Nigg, & Heller-Paz, 1986) and that older adults react differently to warnings than other age groups (see Mayhorn, 2005).
5. Page 10: The sample is very homogenous: primarily women with internet access from home (suggestive of higher SES). While gender and age were collected as part of this study, it is troubling that other demographic data was not collected or included for analysis! To illustrate this point, a robust finding from a growing body of research is that trust and message credibility varies quite significantly by racial and ethnic status (Spence, Lachlan, & Griffin, 2007). For instance, African-Americans frequently cite a distrust of government institutions and describe incidents of past exploitation such as the Tuskegee syphilis trials or Hurricane Katrina as explanations for an unwillingness to attend to or believe messages (Andrulis, Siddiqui, & Gantner, 2007; Freimuth et al., 2001). Likewise, differences in warning information exchange and dissemination have been observed between Mexican-Americans, Caucasian-Americans, and African-Americans (Fothergill, Maestas, & Darlington, 1999).

Thus, it is doubtful that the authors can effectively argue that their data can be generalized: a point they make on Page 16 themselves.

6. Pages 15-19, the authors should provide more usable suggestions that can be implemented by institutional actors responsible for risk communication. At no point within the manuscript do the authors go beyond a discussion of building generic partnerships between the public and industry/government. This finding is not novel! Past research has indicated that the use of participatory techniques that entail active collaboration between communities and other stakeholders such as government entities and aid organizations can combat the effects of mistrust (George, Green, & Daniel, 1996). Given this previous information, the authors can make a contribution by specifying what regulatory agencies can specifically benefit from this research.

Reviewer #1 (Remarks for the Author):

Introduction

Page 1- please provide references for the story told in the first paragraph.

Page 2- as the paper is long, I believe it would help the reader to simply state only the two questions you plan to investigate in the paper and drop the third. It would also help to clarify these questions - be more specific than 'can the recreancy theorem explain public trust [in what]?

Page 3, line 80- Thus because risk communication should be ... the problem is most risk communication by government agencies is not linked to theory or uses outdated theoretical approaches.

Page 3 line 98-100 please provide a reference

General comment on theoretical approaches - it would help the reader to note you are reviewing five+ theoretical approaches and then to use headers or bullets to alert the reader of the introduction of that theoretical section. If I have the number of theoretical approaches I list is wrong this reinforces the idea that the reader gets lost in this review. This review could also be tightened up and shortened.

One problem with the introduction is that the authors assume the public response to problems with the food supply are going to be explained by factors involved in much different risk situations like nuclear waste. The Bredahl/Frewer work was in GE foods as was some of Seigrist's, which at least some Frenenburg's work focused on environmental risks and Fischhoff's was around radon among other things. Although the factors might have similarity, the context of the risk situation (type of risk, possible ways to adjust risk, etc) affects consumer response and their possible reactions. I think this needs to be addressed in this introduction to theory.

Page 8, lines 209-211 - this is a bit of an overstatement. Other studies have included the idea of social trust as a major determinant of risk perception.

Page 9, line 228 - however too simple an approach can eliminate factors that are just as important in risk communication and result in communication disasters. See if you can come up with a better reasons for looking at this and stick to the first idea. One thing that is not explained here is what 'fiduciary responsibility' is.

Page 9 line 236- I can envision a situation where one can have great trust and confidence in an institution and still realize they can't do anything about risk because of other actors or regulatory constraints etc, etc. So the question is whether this trust really has a great impact on the situation when the context of the risk is specified. We are a world of lawyers and insurance companies that make money off of insuring against disasters and speak to the idea that trust is not enough.

Page 9. Line 244-246. An example here would help - what sort of recommendation ? to avoid tomatoes? One can still distrust an agency and follow notices to avoid tomatoes.

Page 9, line 247- page 10, 255. So this theorem does not really break with previous work as specified on page 8.

I think your introduction would be more convincing if you instead outline how social trust or trusts is a common element in many theoretical approaches to risk assessment and communication and you are testing this theorem to evaluates its inclusion in the risk assessment firmament.

Methods

Page 11- bottom of page. Among these assessment statements, competence appears clear but on looking at all the actors you list and ask them to evaluate I would have a hard time making a reasonable judgment of confidence, trust and support since no specifics are given.

Page 12 - lines 314-319- please clarify - did you randomly assign a subset of the 41 situations to individuals on the panel so perhaps 200 were evaluation on subset and another 200 a different subset. This paragraph is unclear. The tables are presented as if all 2008 women completed all 41 situations, a long and tedious questionnaire.

Results

Page 13, line 335- 'Most' is an understatement - please point out that only 1.5% of the respondents were male. So this is a female sample and controlling for sex means little.

Page 13- table 13- you will have to address the issue of the self selection of this sample to have more women with interest and concern than those who are not concerned. Not all women are concerned about nutrition, etc. This appears to be a highly biased sample or one that likes to give socially acceptable answers.

I also think we need a clearer explanation of how these values in table 2 were derived - in the methods the focus is on asking these general questions in regard to actors. How were these general opinions derived?

Page 13- table 3- again a clearer explanation of how these factor analyses were done would help. I can guess but please be explicit. Each of these factors competence etc were one question, not a scale so is this alpha based on all estimates of competence for farmers across five domains? This is not clear. It would help to have examples of questions used to make this estimate at the bottom of the table.

Page 14- Table 4- the data suggests you have identified the impact of confidence on trust but that trust does not fully explain the idea of support. The measure of competence or concern does not appear to contribute much to trust.

This again seems to stand out in table 5. The low impact of concern suggests that the sample did not vary enough in this variable as per your data.

Page 14- line 373 on - the inclusion of education as a sign variable is not surprising and reflects the range of education among the women in the sample. However reporting the finding on sex suggests the men in the sample were very unusual for 1.5% to produce such an effect. I would remove the men from the sample and use just the results from the women who are 98.5% of the sample.

Page 15, line 401 - please provide a reference for the statement that the public are largely ignorant.

Conclusions

Page 16 line 409 please temper this statement about strong support. The model was set up to be simple and the confidence statement was not clearly defined for the participant - the sample was biased and the most you can say is that this sample of women confidence in the actors was a factor in trust. This can be assumed to be 'blind' confidence and thus 'blind' trust since it is not clear what any of these women knew about the actual role of 'grocers' etc.

I would like to see more emphasis on the weaknesses of this study.

Page 17 - somehow the recommendations made beginning on line 429 sound like the old model - if we just increase their knowledge of how great a job we do and how expert we are that you should not worry. I do not think the reference to Covello of 1988 is an up to date take on current risk communication. Wall street companies, the Fed reserve, Banks are constantly trying to convince us they know what they are doing and generally create messes.

Page 18-19 while this closing discussion is of interest and actually seems to highlight the weakness of the model, what exactly is 'confidence', the strongest factor in the model? What does the public think makes up fiduciary responsibility or is it even such responsibility that affects confidence? The big question is what are consumers using as a basis for confidence and why does trust have low impact on willingness to follow recommendations?

Reviewer #2 (Remarks for the Author):

This is a well written paper. The findings are interesting and provide some insight into approaches one might take to effectively engage with the public. The main weakness of the paper is in the review where a considerable amount of time and effort is spent reviewing theories and topic related to risk assessment but which are not totally germane to the current paper and the exploration of the Recreancy Theorem. This is compounded by the limited explanation of the theorem and the lack of exploration of the salient constructs. The meaning of trust and the types of trust should be explored further and more attention should be given to the importance of both perceived competence and confidence in consumer trust

The confidence measure requires some explanation, how is it measuring confidence? I believe if more attention was given to the main constructs in the review section this would clearer.

Further to this the conclusions could benefit from a more critical evaluation of this uncomplicated model. In particular what determines confidence and perceived competence in organisations? Is the relationship a little circular in that once trust is broken the organisation is not viewed with confidence and will not be perceived as competent?

Minor points:

The term 'Fiduciary responsibility' changes to 'confidence'- it is best to be consistent

Page 4 Fishbein and Ajzen are noted for the Theory of Reason Action and not the Theory of Rational Expectations. I believe that this was originally proposed by John F. Muth.

Reviewer #3 (Remarks for the Author):

This paper presents results from an internet-based survey designed to assess the extent to which US citizen trust various institutional aspects/members of the food system. In particular, the paper tests a theory of trust called the recreancy theorem that links perceptions of competence and confidence to trust and trust to support. The paper reviews relevant literatures on risk perceptions and trust, describes the empirical methods and presents results from simple regressions of the variables to conclude that there is evidence in support of the recreancy theorem.

Although the objectives are clear, there are a number of weaknesses with the paper that need to be addressed before it can be considered for publication.

First, there are a number of unsubstantiated statements in the paper. For instance, in the first paragraph the author(s) states that US consumers voice concerns about food safety externalities, and that they "worry that the news is not good." These, as well as others like this, need citations. Media stories of e.coli and other problems with food may give the impression that there is widespread concern about food, but this is not scientific evidence.

Second, the review of the trust and risk literature is very long, more than 6 pages. This should be abridged with key insights and clear statements of holes in the literature provided. In its current form, the reader can get lost in the lengthy discussion of which author said what about trust, risk, and food.

Third, there needs to be more said about recreancy theory. As I see it, the theory simply says that public perceptions of the competence and confidence of some actor affects the degree to which the public trusts the actor, and that trust, in turn, affects public support, holding other things constant. What makes this "theory" different from the many, many other studies of public trust that consider perceptions of competence and trustworthiness/integrity as factors of trust? For example, on page 4 the author(s) describes "psychometric approaches" that consider "perceived expertise and trustworthiness" of information sources. On page 5 the author(s) notes that the diffusion of innovations approach considers "competence and integrity." Is the real distinction of recreancy really related to the factor other than competence? That is, "confidence" instead of trustworthiness or integrity? I don't see a significant difference here, unless interpretation of results, or what measures are used to operationalize the variables, is really what makes recreancy distinct.

Fourth, (related to point 3) the author(s) considers "confidence" as a factor of trust. There are a number of studies that use confidence as the measurement of trust. Also, the question the author(s) asks to get at confidence is unclear. It asks respondents to consider "values" and doing the "right thing". Perhaps a belief that an actor will do the right thing is confidence (might also be a form of trust), but I do not know how perceptions of shared values should be useful in understanding confidence.

Fifth, why are concern and responsibility included in the empirical model? Are they a part of recreancy theory?

Sixth, the author concludes that the evidence supports recreancy theory. What evidence would have lead to a conclusion that recreancy is not supported? Also, notwithstanding the fact that the variables for competence are shown to be statistically significant at the 1% level, as standardized coefficients, the values are relatively small.

Finally, the author(s) conclude, in part, by noting that the estimated model suggests that making consumers aware of the "skills and expertise" of representatives within the food industry will go a long way in increasing trust in the food system. This is interesting given the author(s) criticism of the existing literature (on page 8), that "risk perceptions cannot be explained solely by differential levels of knowledge ..." But isn't this what the author(s) say are important? Consumers need more knowledge about the food system and the actors involved in it. Related, it is not clear how the conclusion the author(s) makes about knowledge derives from the empirical analysis, since knowledge is not a variable included in the analysis. Rather, "perceived competence" is. Is the author(s) saying that increasing the public's knowledge of food actor skills will increase the public's perception of their competence? In my view, competence is more than this, since even highly skilled and trained people can and do make mistakes.

November 24, 2008

Michael D. Schulman, PhD
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Dear Dr. Schulman:

I am submitting for review by *Rural Sociology* the revised manuscript, "Consumer Trust in the U.S. Food System: An Examination of the Recreancy Theorem."

I have made revisions to the paper in accordance with the suggestions offered by the external reviewers. Also, I have provided a reply to each of these suggestions that informs the reviewers of the changes made to the manuscript.

I look forward to hearing from you regarding your decision about this manuscript.

Sincerely,
Stephen G. Sapp, PhD
Professor of Sociology

November 24, 2008

To the Reviewers of *Consumer Trust in the U.S. Food System: An Examination of the Recreancy Theorem*:

Thank you for your excellent quality review of the initial version of this manuscript. We agree with your suggestions for improving it and have incorporated them into the revised paper. This letter lists the suggested revisions and describes how we responded to them.

We apologize for the delay in responding to you. We awaited the completion of a second study that used the same methodology as the first. The advantage of incorporating the results of this second study into the manuscript is that the sample in Study 2 was about 50% male and 50% female. We believe that the nearly all female sample in Study 1 was a significant limitation to the paper; so, we wanted to report also on the findings of Study 2.

Central Themes for the Reviewers

- **The literature review could be more comprehensive.**
- **The literature review could be tightened up and shortened.**
- **More needs to be said about the recreancy theorem.**
- **The main weakness of the paper is in the review where a considerable amount of time and effort is spent reviewing theories and topics related to risk assessment, but which are not totally germane to the current paper and exploration of the recreancy theorem.**
- **The review of the trust and risk literature is very long, more than 6 pages. This should be abridged with key insights and clear statements of holes in the literature provided.**

After viewing the literature review from the perspectives of the external reviewers, we realized that we attempted to combine too many topics within a single review. Simply put, in this study we investigated a model of trust. Although it is true that the results of this analysis have implications for risk communication and the reception of risk messages, our reviews of the literature regarding these latter topics created confusion when they were presented at the beginning of the paper. The literature review now is more focused on the topic at hand.

- **How can a comprehensive model for trust be developed when there is no general agreement within the literature as to how this concept is defined?**
- **One problem with the literature review is that the authors assume the public response to problems with the food supply are going to be explained by factors involved in much different risk situations.**

It is not our intent to develop a *comprehensive* model of trust anymore than we would attempt to build a comprehensive model of any other complex construct, such as community, for example. Indeed, we noted in a lengthy discussion that various approaches have been taken in defining and modeling trust.

The central thesis of this manuscript is that it is worthwhile to *add* the recreancy approach to the literature on modeling trust for two reasons:

- a. The recreancy theorem is more embedded within the sociological literature than are other approaches, wherein psychometric approaches dominate previous social science literature and quantitative risk assessment is the most widely accepted approach within the field of engineering. Because the recreancy theorem focuses upon perceptions of the quality of societal institutions in protecting the public it offers a relatively novel approach to understanding trust.
- b. The recreancy theorem is parsimonious in comparison with other approaches to understanding trust. Models in previous literature are more complex than the recreancy theorem. As we noted in the manuscript, we speculate that food system actors will find a simple approach more appealing. Our reasoning is that, if the recreancy theorem receives empirical support in a large-scale study such as this one, then risk communication scholars might find success in helping institutional actors understand a simple approach to trust.

We have clarified these points in the introduction section of the paper.

- **The authors seem to believe that the public will encounter only one source of information at a time.**
- **The paper does not address the dynamic nature of trust; that it develops over time.**

We found nothing in our paper that indicates that we assume the public encounters only one source of information at a time. Still, because we have a cross-sectional data set, this paper does not address the social construction of risk assessments. We note this fact in the revised paper.

- **The sample is primarily women.**

Since the submission of the original manuscript, a second nationwide study, using the same methodology and measures of variables, has been completed. In "Study 2" the sample is approximately 50% male and 50% female. The results of both "Study 1" and "Study 2" have been incorporated into the revised manuscript.

- **The risk communication literature indicates that social-demographic factors play a key role in determining how people respond to risk communications, such as disaster warnings.**
- **A growing body of research shows that trust and message credibility, including warning information exchange, vary quite significantly by racial and ethnic status.**

The literature indicates that, although social-demographic factors have statistically significant zero-order correlations with risk *perceptions*, they often do not have significant partial coefficients in models that include psychometric variables, such as sense of voluntariness, control, or trust. And this is what the results show here, at least insofar as we are able to measure social-demographic influences (race and ethnicity were not measured in this study). Nevertheless, the reviewer is correct in noting that we understated the importance of social-demographic influences on *reception* to risk communication messages and thereby on the

potential applications of the recreancy theorem to risk communication. We have corrected our misleading statements.

- **The authors should provide more usable suggestions that can be implemented by institutional actors. The suggestion of building generic partnerships and engaging in corporate social responsibility are not novel.**

We did not imply that engaging in corporate social responsibility was novel. The empirical support found for the recreancy theorem simply provides further support for this form of trust building. We have expanded this discussion to more clearly describe what we mean by partnership building.

Specific Comments

- **Page 8, lines 209-211. A bit of an overstatement.**
We agree and have corrected the statement.
- **Page 9, lines 228: Recreancy might be too simple.**
Correct. We have revised this section and the points made in the discussion section.
- **Page 9, line 236: One can have trust, but still not comply.**
Correct. We are using compliance as just one of many possible variables that could be used to assess external construct validity. We note this fact in the revised manuscript and offer a citation from a recently conducted study.
- **Page 9, line 247; page 10, lines 255: Seemingly contradictory statements.**
Corrected with a more clear statement.
- **Page 11: It would be difficult to assess competence and confidence for all these actors!**
You make a good point and we agree, but did not say so in the initial manuscript. The revised manuscript is corrected.
- **Page 12: Clarification of the values in Table 2 is needed.**
Done.
- **Table 2: Clarification of values in Table 3 is needed.**
Done.
- **Table 3: Clarification is needed on how the factor analysis was conducted.**
Done. And we used a more intuitive approach in the revised paper.
- **It seems like the measure of concern did not vary much in your data [because] concern did not have much effect on trust.**
The standard deviation for concern equaled about 2 on a 10-point scale across the five areas of the food system. This amount of deviation seems sufficient to account for the variance in

trust, if concern was in fact highly related to trust. Therefore, compared with confidence and to some extent assessments of competence, it seems like concern is not very important in explaining trust. That is, one might be concerned all the time, but concern does not necessarily explain trust.

- **Page 15, line 404. Needs a citation.**

Done.

- **Page 16, line 409: Statement needs to be tempered. I would like to see more emphasis on the weaknesses of this study.**

Agreed and done.

- **I do not think the reference to Covello 1988 is an up to date take on current risk communication. Wall Street companies, the Fed reserve, banks are constantly trying to convince us they know what they are doing and generally create messes.**

The reference is listed with respect to a statement regarding the *purpose* of risk communication; it is not a statement that the purpose is always fulfilled. Indeed, it is the point of this paper that simple proclamations by institutional actors that "we know what we are doing" are not sufficient to gain trust—that some form of behavior that instills a sense of confidence in fiduciary responsibility also is needed. Nevertheless, this paragraph seemed redundant to what had been said previously and was therefore omitted from the revised version.

- **Page 17: Sounds like you are suggesting that the public needs more education about "how great a job we do."**

- **The authors conclude by noting that the estimated model suggests making consumers aware of the skills and expertise of representatives within the food industry will go a long way in increasing trust in the food system. This is interesting given the authors' criticism of the existing literature.**

The average influence of perceived competence on trust across the 55 models (about .200) is sufficient to support claims that becoming more aware of the competence of institutional actors within the U.S. food system might improve trust. After stating this finding, we assert that the results seem to indicate that confidence building activities are an even more important element of trust building in the U.S. food system.

- **What exactly is "confidence"?**

- **The confidence measure requires some explanation. How is confidence measured?**

The recreancy theorem states that trust results from institutional actors displaying expertise and fiduciary responsibility. We are assessing whether consumer perceptions of these traits affects their trust in institutional actors. Therefore, we measure consumers' perceptions of competence and their *confidence* that institutional actors will behave with fiduciary responsibility. That is, we have no measures of the *actual* fiduciary responsibility of actors; rather, we have a measure of the *confidence* held by consumers that institutional actors will behave with fiduciary responsibility. We recognize that, without multiple indicators of our constructs, we must rely upon the definitions given in the instructions to the respondents (see

methods section) as the definitions of competence and confidence. We were not clear about this limitation, however, and need to point it out more directly.

- **The big question is why does trust have a low impact on willingness to follow recommendations?**

We disagree that an average standardized estimate of .683 for the effect of trust on willingness is "low." Also, we disagree that an average R-Square value of about .5 for explained variance in willingness, wherein trust accounts for 96% (Study 1) to 98% (Study 2) of this explained variance, indicates a "low impact" of trust on willingness to follow recommendations.

- **Some statements need citations (i.e., that consumers worry).**

Good point. Done.

- **Why are concern and responsibility included [as control variables] in the model?**

We clarify our position about these variables in the revised manuscript.

February 18, 2009

Dear Professor Sapp and Co-Authors:

I am sending you three reviews of your revised paper, "Consumer Trust in the U.S. Food System: An Examination of the Recreancy Theorem."

As you will see, the reviews are somewhat mixed. Reviewers 1 and 4 are very positive: these two reviewers find considerable improvement in the revised version of the paper. They are a short set of suggestions that involve clarification of concepts, rewriting and simplifying some sentences, some additional clarification of the concept of confidence (granted that you addressed this in your response), and some attention to the tables and discussion. Reviewer #5 is more critical, especially with regards to the links between theory and the large number of equations in the tables.

My own reading of the paper leans more towards Reviewers 1 and 4, but I do think some greater integration and description of the equations in the tables are needed.

- Do you really need all the tables and can you do a better job of explaining the results of all the structural equation models to folks who may not understand the underlying statistical methodology?
- I am concerned that the complexity of the tables in the paper will "turn-off" potential readers who otherwise would find your article interesting and important. Please explain how you have 41 structural equation models (see old abstract that was part of the original AllenTrack submission) and 55 tests of the theorem and why you need all of these models and tests to accomplish the purposes of the paper?
- In addition, you need to identify where in the paper the tables and figures need to be inserted (e.g., "figure 1 about here").
- In the discussion, please explain how the recreancy theorem will revise theory and research on trust and push it towards a more sociologically sophisticated treatment of this issue.

I am pleased to conditionally accept your paper for publication in Rural Sociology. When you send the final version of your paper, please detail for me how you addressed the reviewers' comments. I will not send the next revision out for review again but rather make the final decision in the editorial office. In addition to the substantive revision, please make sure that all references and tables conform to the style of RS. That will save the copy editor and me a lot of time and efforts-and you would need to do that anyway.

If you have any questions, feel free to contact us at ruralsociology@ncsu.edu.

Sincerely,

Michael D. Schulman
Editor, Rural Sociology

Reviewer #1 (Remarks for the Author):

Some specific comments

Page 3- lines 9-25 and page 4- lines 1-12. The authors present four definitions of trust. Then two of these forms are defined with parallel use of names. However relational property is not linked back to one of the four original forms. Then on the next page a new term is introduced to describe what they are investigating. This inconsistency in terms is confusing. If one introduces the four types then keep the following descriptors parallel and if you change the term for your trust please related it to the original four or make it clear that only two of the original four are useful in this paper and then explain these clearly.

Page 5- lines 11-18- please simplify this sentence - it might help to put the point at the beginning instead of at the end of many phrases that confuse the reader.

Page 10- line 10-12- it would help to italicize concern and responsibility within this paragraph.

Page 11- lines 14-21-the second sample had more young people in it -- this might have affected the difference in responses seen in table 2.

Page 12- line 13- the authors speak of 55 models but Table 4 specifies 41 models.

While the data appears to support the model, I have a nagging concern about the statement used to define confidence - Do they have the same values as you do so you know they will do the right thing? This is actually two questions- do they have the same values as you? Do you feel they will do the right thing? Most respondents would not be answering the compound question but one or the other of the sub questions. I know I would have difficulty with the question as written and this is probably true of the survey respondents.

I think the authors need to address this problem in the discussion and perhaps suggest that the definition of confidence will have an impact on whether this model holds up. I made a similar point in my original comments and do not believe the authors have addressed this issue.

Reviewer #4 (Remarks for the Author):

This is an unusually significant contribution to genuinely sociological thinking on risk.

Although well-known grand theorists such as Giddens and Beck have published provocative ruminations on risk, those ruminations have so little relevance to empirical reality that sociological theories have had remarkably little influence on actual risk studies; instead, most of the influential thinking, to date, has come from psychology, and to a lesser extent from economics. Work on what this paper calls "the recreancy theorem" has long offered the potential for explicitly sociological insights, and past empirical research has been reasonably promising, but as this paper points out, the amount of work explicitly testing the perspective has remained decidedly modest to date.

That is one of the reasons why this paper is so important. The other is that it is well and thoughtfully done.

The paper uses LISREL to test the recreancy theorem across 55 models, broadly representing the U.S. food system, assessing respondents' views on food safety, the nutritional quality of food, the fair treatment of food-related workers, the humane treatment of animals grown for food, and the protection of the environment. Contrary to the assumptions that still have too much of a hold on the thinking of non-social scientists, the authors accurately note (p. 7) that trust "rests upon the actions of the agents held responsible for risk rather than upon statements issued by quantitative risk assessors about risk" - or as they rephrase it, near the end of the paper, that actions ought to be expected to speak louder than words.

The paper finds surprisingly widespread support for the recreancy theorem, but it also accomplishes something else that is worthy of greater emphasis. Freudenburg's early work on recreancy was unable to test which of the two elements of Barber's trust/trustworthiness - technical competence or fiduciary responsibility - was more influential. Freudenburg argued at the time, in essence, that it didn't really matter - that is, he argued that if people felt they couldn't trust important societal institutions, the overall absence of trust would be what mattered. That argument was plausible, but it's also unsatisfying. The authors of the present paper, by contrast, have been able to go beyond that early work, showing something that has been the focus of considerable speculation but that, to the best of my knowledge, still has not before been empirically demonstrated until now. They find that competence - the question of whether important institutions CAN carry out the work they are expected to perform - is only about a third as significant, empirically, as the issue of fiduciary responsibility, or the question of whether they can actually be counted on, or whether they WILL carry out the jobs they are expected to carry out. On reflection, that finding makes a good deal of sense, but it ought to be emphasized more strongly. Given the increasingly complex and interdependent nature of the society, it is one that deserves a good deal of attention in other contexts, as well.

Aside from having just done something that is altogether uncharacteristic for me - urging the authors to be LESS modest about the significance of their findings - I see little other need for further revisions. The authors show a fine comprehension of the risk literature, which they encapsulate far more sharply than had been the case in the previous submission. By adding a

second study, they have dealt with the most significant shortcoming of the first one, namely the absence of male respondents. In the process, moreover, they have shown their findings to be significantly more robust than I would have expected.

Perhaps the most important changes that still need to be made are two in number. The first and more important of the two is that their references to "confidence" have the potential to create confusion. The authors explain their reasoning well enough in the text, but readers who go straight to the tables will see only a reference to a causal path from "confidence" to "trust." To avoid needless confusion - and to further integrate this paper with earlier literature - I suggest the authors go back to the terminology originally used in Barber's landmark book on the Logic and Limits of Trust, namely "fiduciary responsibility." The second problem is much less significant, but it is worthy of correction nevertheless. Although the paper is for the most part a model of clear writing, the final sentence before the heading on p. 5 ("The Recreancy Theorem") is excessively complex and unclear.

In other respects, including all of the important ones, this is an eminently publishable paper. I congratulate the authors on their fine and careful work and on the significance of the paper that they have now produced, and I look forward to seeing the final version in print in the not-too-distant future.

Reviewer #5 (Remarks for the Author):

recr^{eant};re^{ant} (rkr-nt) adj. 1. Unfaithful or disloyal to a belief, duty, or cause. 2. Craven or cowardly. n. 1. A faithless or disloyal person. 2. A coward. [Middle English recreant, defeated, from Old French recreant, present participle of recroire, to yield in a trial by combat, surrender allegiance, from Medieval Latin recrdere, to yield, pledge : Latin re-, re- + Latin crdere, to believe;]mean-spiritedness; apostasy; treachery

I believe that the term recreancy is still not widely used or understood. You must give a one sentence nominal definition that the reader can rely on to understand the argument and the data. Simply because there is a small stream of literature on the topic should not mean that you should take the reader's understanding for granted. Was katrina-N.O. recreancy? The current peanut processing debacle certainly seems to be.

I think that there is institutional failure that is due to deliberate malfeasance or cravenness; there are other failures that stem from anomie or entropy, perhaps neglect. Is recreancy at heart institutional misbehavior? To be useful in research, a concept must be clearly defined and measurable. If recreancy is a theoretical construct at a high level of abstraction, then its measurable subcomponents should be articulated.

The epistemic correlation between your argument and the measures used in the analysis does not seem to be high (or cogently transparent to the reader),

When a paper presents 55 regression equations I am led to believe that this is not a deft or elegant test of a hypothesis.

There is a missing step in data reduction or explanation that the paper does not provide. It is not my task to identify just what exactly that is; only to say that something is missing.

The data are not unreasonable, but a footnote explaining representativeness would be helpful -- age, sex, race, education, region should be commented on.

Tables 3 & 4 a better formatted than others that seem to be missing a line under the column headings. Three decimal places are fine, some places you provide only two. If you persist with the 55 regressions, then perhaps you should reduce the number of numbers by reporting only two places, i.e., .55 instead of .553. By the way, why say R-squared when R² where 2 is superscript is what you mean.

Occam's razor also applies to analytic techniques. What is the added value of Joreskog over OLS? Or over direct ordinal regressional techniques (better fitting the measurement levels of the data you have) that are readily available in SPSS and other statistical packages. There are many features of Joreskog (indirect effects etc.) that you do not use or report.

"The study examines these questions with respect to five related topics: food safety, human nutrition, environmental protection, employee care, and treatment of livestock." Does this mean

five tests of the same hypothesis? You do some averaging of ratings across issues? It is not clear what this means.

What you tell us up front should directly map to what you present in your analysis. And discuss in your conclusion.

February 23, 2009

Michael D. Schulman, PhD
Editor, *Rural Sociology*
Dept of Sociology/Anthropology
North Carolina State University
Box 8107
Raleigh NC 27695-8107

Dear Dr. Schulman:

I am submitting for review by *Rural Sociology* the second revision of the manuscript, "Consumer Trust in the U.S. Food System: An Examination of the Recreancy Theorem."

I have made revisions to the paper in accordance with the suggestions offered by you and the external reviewers. I provide below a reply to each of these suggestions that informs you of the changes made to the manuscript.

I look forward to hearing from you regarding your final decision about this manuscript.

Sincerely,
Stephen G. Sapp, PhD
Professor of Sociology

Reply to Reviewers of the Revised Manuscript

I have responded to the comments regarding the revised paper, and it is a better paper for it. I appreciate the comments offered by the reviewers and the Editor.

Editor

1. The paper includes too many tables with too many numbers.
 - I agree. Although a presentation of many details can assist reviewers in evaluating the scientific rigor of a paper, it can become distracting to the reader. Accordingly, I have deleted Tables 3 and 5, and have noted in the manuscript that these results are available upon request.
2. The discussion section needs to emphasize the sociological importance of testing the recreancy theorem.
 - Good point. Two statements added to the beginning of the Measurement and Data Analysis section help clarify the sociological importance of the study. Also, the first paragraph of the Discussion section provides a better explanation of the sociological importance of the study than had been provided in earlier drafts of the paper. Additionally, the final paragraph of the Discussion section has been rewritten to refer to sociological issues in general rather than to the more specific topic of the U.S. food system.
3. The paper needs to be presented in accordance with the editorial guidelines of the journal.
 - Done. [I wonder if a style sheet can be posted to the "Submission" page of the *Rural Sociology* web site?]

Reviewer 1

1. Clarify terminology related to the four definitions of trust.
 - Good point. Done.
2. Simply and clarify sentences on pages 5 and 10.
 - Done.
3. Note limitations of the sample.
 - Done.
4. Clarify descriptors in the tables.
 - Good eye; thank you. The tables are corrected.
5. Note concerns about the definitions of "confidence" (now: "fiduciary responsibility").
 - This point was raised also by Reviewer 5 and is a valid one. The model concepts are measured using single indicators. The paper notes this limitation, advises caution in interpreting the results, and advises further research on the construct validity of measuring competence and fiduciary responsibility.

Reviewer 4

1. Use "fiduciary responsibility" rather than "confidence."
 - Done.
2. Rewrite sentence on page 5.
 - This sentence was long and awkward. Also, it was not critical to the discussion; so I deleted it.
3. "This is an unusually significant contribution to genuinely sociological thinking on risk."
"That is one of the reasons why this paper is so important. The other is that it is well and thoughtfully done."
"In other respects, including all of the important ones, this is an eminently publishable paper. I congratulate the authors on their fine and careful work ..."
 - Wow! These comments felt great and made my day!

Reviewer 5

1. The term recreancy needs a more thorough explanation.
 - Correct. I assumed familiarity with the 1993 Freudenburg article, which is unfair to the reader. I have added a more complete explanation of the theorem and the use of the term "recreancy."
2. Explanation needs to be given for using 55 models to test the theorem.
 - An explanation of desiring to test the recreancy theorem across a broad range of institutional actors is provided in the methodology and discussion sections. Briefly, the efficacy of a theory is enhanced by successful evaluations of it across a broad range of applications.
3. "There is a missing step in data reduction or explanation that the paper does not provide. It is not my task to identify just what exactly that is; only to say that something is missing."
 - This comment is not a constructive one.
4. The paper should mention the limitations of the sample.
 - Done.
5. Clarify the formatting of the tables.
 - Done.
6. Use OLS regression rather than SEM.
 - The recreancy theorem requires SEM analysis. OLS analysis would test another causal model rather than the one specified by the recreancy theorem.

February 25, 2009

Dear Steve:

We are delighted to accept this manuscript for publication in a forthcoming issue of *Rural Sociology* (74 Issue 3 or 4). We will likely make some minor changes in wording, but otherwise we plan to publish the revised manuscript as submitted. You will next hear from our copy editor regarding edits and changes in the manuscript. Be patient, we are currently working on issue 74(2).

Please fill out our copyright agreement form and fax it to our office (919-515-2610). The form is located at: http://rs.allentrack2.net/letters/rs_copyright_form.pdf. The first author may sign for all other authors.

Please send us current information by email on the professional affiliations of all the authors. Please make sure that all your tables, figures, and references are correctly labeled and are consistent with our publication style. Check past issues of the journal or contact us if you have any questions. This will assist with the copy-editing and page proof process.

Again, congratulations on this good work and many thanks for submitting this manuscript to the journal!

Very sincerely,

Michael D. Schulman
Editor, *Rural Sociology*
William Neal Reynolds Professor
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NCSU Campus Box 8107
Raleigh, NC 27695-8107
ruralsociology@ncsu.edu

August 27, 2009

Dear Prof. Sapp:

Attached is the copyedited version of your article "Consumer Trust in the U.S. Food System: An Examination of the Recreancy Theorem" for the next issue of *Rural Sociology*. I edited using Microsoft Word's "Track Changes" function; on screen you will see underscoring for new material and overstrikes for deleted material. Questions to you appear in bold within angle brackets. Vertical rules in the margin indicate sections of the text where changes were made. Please note that the copyedited manuscript has been locked to Track Changes, so any changes you make to the text will appear on screen as underscored or overstricken, in a color different from the one I used, so I'll be able to tell what changes you've made.

For editing that is acceptable to you, you do not need to do anything. If you disagree with a specific change, please delete the change and insert text as needed or highlight material you'd like to keep as it originally was and insert "stet" next to the highlighted text. Please respond to each query as specifically as possible, by either making a change in the text or writing a clear answer in the query within angle brackets. Please do not simply say "OK" if several options are possible. Please make sure also to supply any missing information in references that I've asked about.

When you've finished reviewing the article, save it with your changes and e-mail it back to me; please save without changing the file name. We're on a very tight schedule for this issue, so please return it by Tuesday, September 1.

Let me know if you have any questions, and thanks for your review of the edited material.

Sincerely,
Bob Milks

Robert Milks
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