A Good Review Process

I am attaching here the review process, nearly complete, from my first paper published in the *AJS*, so you can see how a good review process works. The University of Chicago Press holds that the copyright of reviews belongs with the review writers, which means that we cannot share them without the permission of these reviewers. I am extremely grateful to Susan Allan for tracking down the authors of these reviews (who are still unknown to me), or their literary executors, to request permission. Because of the intervening years, and the way life leads people to new places, it was not possible to reach two of them: Reviewer D (round #1) and Reviewer C (round #2). I will give short descriptions of the emphasis of their reviews in place of the originals. These are all from my own copies. I very much appreciate the willingness of those involved to share these reviews, letters, and memos.

THE STATE UNIVERSITY OF NEW JERSEY
RUTGERS

Department of Sociology
54 Joyce Kilmer Avenue Piscataway New Jersey 08854-8045
Main Office Tel #: (732) 445-4035/4029
Fax: (732) 445-0974

April 20, 2000

Dr. Roger V. Gould
Editor, American Journal of Sociology
5835 South Kimbark
Chicago IL 60637

Dear Dr. Gould:

Enclosed for consideration by the American Journal of Sociology is a paper, "Power, Authority, and the Constraining of Beliefs." It attempts a formal and comparative analysis of the relation between culture and social structure, in specific, a mapping of the constraint in belief systems to formal properties of network structures. The techniques used to produce measures of formal properties of the belief structure and of network structure have been published elsewhere, but are summarized in appendices added just for reviewers. This makes the paper appear somewhat longer than it is. Where it was necessary to refer to these published methodological works, they were listed in the reference section; other references to works that were not of crucial methodological significance are simply listed as "Author."

Thank you for your time; if there is anything else I can do, please do not hesitate to tell me.

Sincerely,

John L. Martin

e-mail: JLMartin@rci.rutgers.edu

AJS

American Journal of Sociology

Editor: Roger V. Gould • Managing Editor: Susan Allan

5835 S. Kimbark Avenue
Chicago, IL 60637

tel. 773 702-8580
fax 773 702-6207
ajs@src.uchicago.edu

Professor John Levi Martin
Department of Sociology
Rutgers University
54 Joyce Kilmer Ave.
Piscataway, NJ 08854-8045

August 11, 2000

Dear Professor Martin:

The editors have reached a decision concerning your manuscript, "Power, Authority, and the Constraint of Belief Systems." I am very sorry that we will not be able to accept it for publication in the *American Journal of Sociology* in its present form. I am enclosing the reviewer comments that provide the principal rationale for our decision.

The referees, however, found substantial merit in your manuscript. Therefore, we would be willing to consider a revision of the manuscript, especially if you take into account the comments enclosed here. Although we don't wish to give you narrow instructions for revision, we would like to point out two problems we view with great interest. First, C asks why we should accept your two-dimensional view. What would things look like in a more complete space? Second, we will expect the revision to include a report of correlations of your measures.

If you do decide to revise, we will return your paper to at least one reader from this round and one new reader. In the second round, about half of all the manuscripts are accepted; nevertheless, this invitation does not constitute a guarantee for publication. Please let us know whether or not you plan to revise the manuscript.

At this point, we find it helpful to remind authors that *AJS* publishes only original research. We assume that (1) this paper is not under review elsewhere and (2) this paper has not been previously published in whole or in part. Please contact us before undertaking any revisions if either of these conditions is not true.

One final note. If you do revise your paper, a separate outline or account of your responses to the reviewers may help expedite the decision-making process. I do appreciate very much your interest in our Journal and the opportunity to consider your manuscript.

Yours sincerely,

Roger V. Gould

enc./MS AJS-A050270

B

American Journal of Sociology

COMMENTS FOR AUTHOR(S)

Re: "Power, Authority, and the Constraint of Belief Systems" AJS-A050270

Thank you for the opportunity to read your paper. You might have a contribution to make with respect to your ideas about the formal properties of belief systems. To this end, I suggest you re-structure the paper somewhat:

(a) Rewrite the beginning of the paper to emphasize right up front that you will discuss the systematic properties of belief systems, and how this approach differs from other approaches to beliefs and meanings. I didn't find it helpful to read about the sorry state of the field of knowledge studies, problems of imputation, etc. First, because it strikes me as overstated, but also because it doesn't set up your argument very well. You don't get to the point of what you are going to do until page 6. Cut down these six pages and move what's left of this material to later in the paper (or eliminate it).

(b) In setting out your general theory, please keep in mind that you will be testing it with data from belief systems (in communes) that the reader has EVERY REASON (not no reason, as you argue) to believe are quite different from everyday belief systems of people in the street. This was not so much a problem with your discussion of consensus and tightness, but I found your discussion much too general on the ideas of power and authority and how these might affect belief systems. I would suggest you temper your general claims; the paper will be stronger as a result, because your empirical example and general theory will fit together more tightly.
(c) I did not find the hypotheses on power and authority, which you "derived" to be particularly convincing. Can you do more to lead me to accept these as hypotheses? I especially didn't think that they might apply to all groups (and, indeed, there is a good deal of evidence that people's beliefs shift from one social setting to another).

Specific comments:

P. 4: I didn't understand, at this point, what you meant when you quoted Scheler. Best to lay out your ideas clearly first. Also, I did not find the discussion of Simmel's ideas on the formal radicalism of the masses to be helpful. Again, perhaps if I had a clearer idea at this point what you meant by a formal analysis then I might find Simmel's ideas supporting your own.

P. 7 forward: I found the style of this section (and others) -- where you lay out your argument according to a model of formal logic -- to be unhelpful. Why, for instance, must we take only propositional beliefs rather than others? Why discuss an imaginary survey of every propositional belief there is? I would find it more helpful if you just presented your model, supporting it with citations, rather than trying to deduce it.

P. 8: You mention "constraint", but then don't say what it is until after you've told us about methodological controversies over it. I'd like to see the definition closer to the introduction of the concept.

P. 12: Could you define your two concepts, consensus and tightness, more clearly here? I wasn't clear on what you meant. The figures were helpful, but I had to work out the concepts by studying the figures.

P. 13: How people decide when beliefs go together is an interesting one (and one I might like to see answered at the detailed, rather than the structural level you propose -- but that is a different paper!). I don't disagree that power and authority might operate at some times, but I don't think that authority is the main factor, at least in any kind of direct way as you propose, for most Westerners. This doesn't mean that authority might not work in the case of communes, as in your data set, or even for religious people in the case of religion (or science). But I think it would work better for you to scale back the level to which you propose these arguments are generalizable. And, again, I found your style of deriving the hypotheses (which might be actually labeled as such), rather than arguing them, with supporting research and theory from the literature, to be less than ideal.

P. 14: Are "ALL" contradictions (rather than many, or some) really resolved by authority? I doubt it.

P. 17: (Minor) Your example of four wheeling on the beach didn't resonate with me! Perhaps better: driving versus flying.

P. 18: Might you want to say here, or in the discussion, what other factors you might guess might lead to homogenization of beliefs, besides power? Here's one for your communes: Living close together and taking meals together might lead to a regression to the mean. Things like tightness or looseness of social ties, the sanctions (social or otherwise) that are related to holding some types of beliefs in some settings, etc. I realize that this takes you away from the focus of your paper, but it might be useful to give some thought to some other potential structural effects on belief systems.

P. 20: As I mentioned above, there may be no reason to believe that these groups operate with respect to the propositions you suggested, but there is clearly every reason to believe that these groups do not operate like other groups in society.

P. 21: Make it clear that Appendix A contains the belief items that you use in your analysis.

P. 23: Because it wasn't clear what Appendix A was and what it contained, I was unsure of what you meant about the six topic clusters, until I looked back.

P. 24: I think there may be many good reasons to think that reported power in dyads might have some problems. They might still be pretty good measures, but overstating the case weakens it.

P. 27 (Minor) I'm not sure that "clarity" is the best term here. Isn't it the power distance among

individuals? Some groups with closer levels of power might, nevertheless, have clearer distinctions, no? Or am I misreading the visual metaphor in Figure 3? Also, of course, in most groups there is more than one kind of "power" (e.g. formal vs task vs sociometric).

P. 28: One important difference between communes and other group is that people who don't agree can be cast out of the group. (This is a selection bias, different from the entry selection you discuss.)

P. 29: "Presence of an absentee leader" -- oxymoron? Also, I wonder if communes with absentee leaders are more likely to be established cults than other types of communes?

P. 30: Why no statistical tests on the first pass at the data?

P. 31: Rather than show us the data as a whole, which appears not to support your work, why not build in a variable for the political groups and present the new data only. You can say something to the effect of preliminary analysis shows that political communes are different than the other kinds. This would make your presentation cleaner, and no less honest.

Also, the analysis of the lefty groups suggests another variable with respect to power and belief systems: The attitudes of believers towards power and authority. Leftists are well known for questioning authority.

P. 33: I agree that Guruland does answer, to some degree, the charge of self-selection, but it is not incontrovertible evidence. I would suggest you say that "there is SOME evidence that self selection does not account for the differences." Here are two potential counter-arguments: (1) Perhaps the selection occurs after people move to a particular location, those with disparate beliefs are chucked out of the group when there is a strong leader who cares a lot about conformity. (2) If all members of Guruland are pliable, you'd expect more consensus in places where someone is plying them then where no one is. Perhaps greater clarity indicates stronger leaders in these kind of ways?

Also, is there a statistically-significant difference between .582 and .492? More important, is there a meaningful difference between these two? I'm not sure exactly what the numbers mean.

P. 36-37: The idea of the secularization debate being mis-specified is very interesting. Say more.

Review of "Power, Authority, and the Constraint of Belief Systems"
AJS MS# 050270

Summary:

This paper identifies a two dimensional belief space that characterizes the way beliefs hang together for any population. The first dimension, tightness, refers to how well we can predict a person's beliefs given that we know some of their other beliefs. The second, consensus, refers to the range of possible beliefs (along any given dimension) that people hold. Thus, if belief A ranges from 0 to 100, consensus tells us what sub-set of the range of A people hold, while tightness relates A to any other set of beliefs, showing how well one predicts the others. The author(s) then identify how particular features of the formal and informal authority structure of a group might impact the structure of beliefs in any given group. The model developed, based on theory drawn from the sociology of knowledge, suggests that tightness is a function of legitimate authority, while consensus is a function of the informal power structure within the group. The propositions are tested on a set of 40 commune networks.

Critique:

This paper raises interesting questions about how the belief set of a given population is structured. The author(s) link this idea to Durkheim, noting that external social facts are constraining, and thus observed constraint in the things that people believe reflect the contours of this social constraint. The implications of this work are wide, in that properly understanding how belief systems are structured, *as systems*, would provide a strong theoretical mechanism for the development and maintenance of norms, for example. Substantively, one could imagine similar work in multiple areas, such as within schools (looking at how school systems develop unique adolescent cultures), families (identifying priors for sibling life trajectories), or politics (understanding how congressmen vote).

While substantively intriguing, I have issues with some of the theoretical and empirical details of the paper. I will summarize these briefly here, the detailed comments are below. First, it is not clear that the two dimensions of the belief space the author(s) identify adequately describe the structure of any given belief space. The paper would be strengthened by delving more deeply into various ways that the belief system could be organized, and explaining why these two dimensions are of particular interests. This is really simply a framing problem, and the author(s) should be able to write through this. More troubling, perhaps, is that I'm not really convinced that they two dimensions are rely different. It seems that when consensus is low, it is possible for there to be wide ranging variation with respect to tightness. However, when consensus is high, it seems that tightness should be high as well. If I know you are not in region X of the belief space, then I should be better at predicting what beliefs you have (consider changing figure 1d to a single point, for example). If we are really just focusing on the correlation between multiple belifes, then this should be expanded more clearly, and it would be nice to know how well the measure handles non-linear, step or other strange functional forms that could conceptually be thought of as 'tight' but would have a low correlation. This may sound to strong, as I like the general contours of the two dimensions, but I would have liked to have been more convinced they were really *seperate* dimensions.

Given that we accept the two dimensions, the theoretical linkage between authority, clarity of power and belief structure is somewhat incomplete. Most importantly, it would be nice to know how these factors relate to other network-based theories of the patterns of belief. The peer influence tradition (see work by Friedkin), for example, would lead us to expect strong correspondence of beliefs among those people who interact frequently (as is the case in a commune). But no discussion of such non-power effects on belief are given. This leaves us with no theoretical reason to favor an authority view over an already well established peer influence view. This is unfortunate, given that the author's data are likely the single best source for testing these two approaches.

Empirically, there are a number of moments in the paper that were troubling. First, I was not convinced that the author(s) should have calculated their indicator of tightness on each of the 6 areas seperately, and combined them in an average. Instead, I think much of the theoretical interest comes from how beliefs in seemingly disparate regions of the space are related. As I discuss belwo, I would recommend running the measures using cross-context questions, preferably after using something like factor-analysis to reduce the dimensionality of your data. Second, after all the careful work of constructing a continuous measure of tightness and consensus, I was disappointed to see that information largely thrown away by dichotomizing the dependent variable. I would have much rather seen the author(s) analyze the data in it's original continuous form. While the author(s) argue that this is too complex given a 'two-

C2

dimensional' dependent variable, all of their hypotheses concern single dimensional effects. Thus, there is no theoretical need to focus on the joint relation between the two, and one could simply use OLS to analyze the continuous variables. Third, given the dichotomies, it is not clear the relations theoretically predicted hold as strongly as the author(s) claim. Net of consensus, there is no relation between the presence of an authority and tightness (based on the data presented in table 2). Furthermore, given the important composition findings about political groups, it is clear that the authors should explore other composition effects (for example, if there is a relation between race and power, then we would expect different authority structures in racialy mixed groups than in single race groups). No effort is made to examine the underlying relation net of group composition (or network influence) effects, and thus my faith in the robustness of the findings is small.

Details:

p1-6, intro. Too long and too broad. By p.6 the reader still does not have a good sense of what this paper is going to do. While the broad theoretical frame is good, it needs to be tightened considerably. Move the discussion from the 2nd ¶ up. This is what you are doing, so let us know early.

p.7 cut "breathtakingly bold". Is this discussion of a belief space any different than what one would get from Bourdieu's work in *Distinction*? The last half of this paragraph (on getting a sample) is obvious. Not needed here.

p.7, fn 6. Not sure I buy this distinction, and without spelling out why non propositional beliefs would be different, why make the distinction?

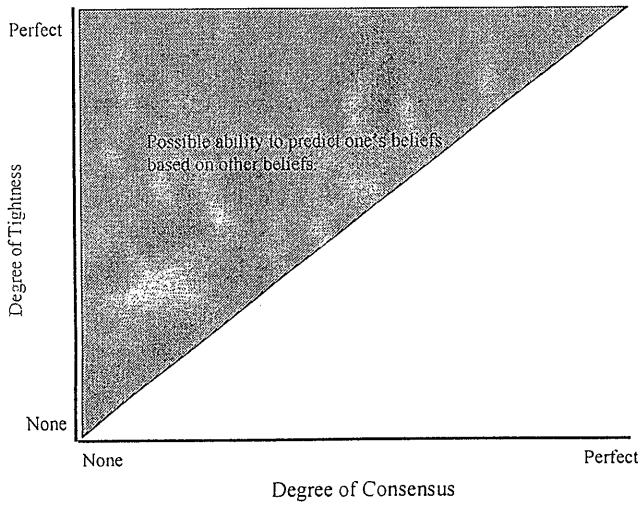
p.8, par 2 "Let us imagine...". Not clear what this paragraph does for your argument. Seems to me that the belief system *is* a system - just perhaps not one that has been measured or formalized. Are you trying to distinguish between a random and non-random (i.e. systematic) pattern? Parenthetical statement "(Here we must assume..." is not clear.

p.9 Constraint is defined as "the absence of dispersion among the N points in the M-dimensional space." Can you re-word this? You are defining your main concept in terms of the absence of a property instead of a positive characteristic of the system.

p.9-10. "an otherwise formless distribution." Why is this formless? Surely there are logical connections among concepts that puts some form to the belief space.

p.10 - 11. Constraint (**Q**) = consensus (**C**) + tightness (**T**). Some more discussion of why constraint breaks down in this way would be useful. While we "May see this constraint ... as stemming from two sources" we may see it as stemming from any number of sources. Lead us into these particular two. In general, this § of the paper moves too fast -- don't worry about the size if N yet. Stick to the conceptual issues of the belief space.

In general, it seems that consensus and tightness are not independent qualities. If tightness is defined as the ability to know a person's beliefs in one area given you know their beliefs in another (essentially the ability to predict belief *i* given belief set **J**), the it seems you are faced with a situation that would look something like this:



That is, when consensus is low, tightness could be low or high (A strong correlation, such as implied by figure 1a, for example) But if consensus were perfect, if everyone agreed, then it seems that the ability to predict their beliefs would be curtains well. That is, it seems to me that figure 1d has both high consensus and high tightness (imaging making it more extreme, but putting the cloud of points on a single xy coordinate in the space). Why is this not the case?

p.12 "But these beliefs are still tightly connected, in that movement in one implies movement in another" Throughout, you discuss "pressure", "Change", "Movement" as in the sentence above. I think a more accurate wording would substitute "position" for "movement" here. You have no information on ideational change.

figures 1 & 2 could be combined -- no need to make them separate figures.

p.13 "We know have the possibility ..." Yes. This is the strongest statement of why this paper is important. This is a good question, and I think you would be well served by highlighting this early in the introduction.

p.13. "this assumes that.." make this a footnote.

fn 11. I'm not sure this gets you out of the problem of logical interconnection, instead it seems to simply move it back a step (what allows a particular compartmentalization strategy).

p.15. Proposition 1: "beliefs can only be interrelated when they are unified in a single domain of cognitive authority. Since such relations of implication and contradiction will lead a belief system to have 'tightness' as defined above, this tightness should thus be a product of authority."

Couldn't a unified system of beliefs emerge through a simple peer influence process? Consider for example Friedkin's (1998) model, where an individual's opinion is a function of exogenous individual features and endogenous influence processes. If people are deeply embedded in a relational context, then discussion should lead to influence, under most circumstances a convergence in opinion within the group, all without any need for an external authority. If it is also true that groups with high authority may also have more intense interaction (as might be suggested by Lofland and Stark 1965), then any relation between tightness and authority is simply spurious on the association structure of the group.

p.16. Make this distinction (legitimate power → tightness, unlegitimate power → consensus) more clear.

p.17. Parenthetical statement on variance dependent should be cited and made a footnote.

p.17 Again, it is not clear that 'inability to move away from a privileged zone of the belief space" is any different from "the imposition of rules of movement" since any such rule likely applies to regions of the belief space. Lay out how each depends on the other.

p.18. the last ¶ of this section moves too fast. Take the time to spell out your propositions more clearly.

p.18. Just out of curiosity, could you apply this idea within families, where authority is probably fairly well known (parents dominate children, at least. Could get more exact information) to explain the belief profiles of siblings? That is, why are some families very united in ideas while others exhibit much less consensus?

p.19. You say 40 communes had data, but your tables only use 35 and 38 cases.

p.21 Were there absentee leaders in non-religious groups?

p.22, fn 15. Doesn't this fact (a latent propensity to answer strongly) bear directly on the question of belief structure? Why wouldn't this be useful information for understanding how tight the system of beliefs would be?

p.23. Decision to treat the 6 areas separately. While I understand the need for data reduction, I think this may be the wrong way to go about the process. It seems that much of what is interesting about the constraint of a belief system would be that beliefs across domains are linked. Thus, through our religious beliefs we also have particular political beliefs. As such, we would be much more interested in looking at tightness/consensus *across* these domains. You could approach this in multiple ways. For one, if the 6 areas really tap some underlying dimension, then a factor score on each would provide a summary for the domains, then tightness could be measured using the reduced factors. Or, one could use a sample of items from each and average across the N samples. (Much as you do now, only use an item from each domain instead of using all items from a single domain).

It is also not clear that your current approach really "avoids spurious results which might arise were we to compare the organization of one group's central beliefs with another group's peripheral beliefs". As the case with political organizations show in the end, you will actually magnify these problems averaging a sequence of 'periphery' beliefs with one strongly held 'core' belief. That is, by separating by topic, your average may be particularly un-representative of the overall organization of the belief space.

p.24-25. The clarity of power measure is good, but I would like to see it more directly compared to well known direct measures of hierarchy, such as those presented in Krackhardt (1994).

p.28 - 20. "To simplify the discussion, given the complex case of a two-dimensional dependant variable, " But you never make two-dimensional predictions. Your core argument is that authority → tightness and clarity of power → consensus. As such, you should be able to measure your effects separately, perhaps conditional (net) of the other dimension (or, you could use a multiple equation approach, where you allow consensus and tightness to correlate, though I suspect your sample size too small). In general, the massive reduction in information that occurs by dichotomizing your variables seems troubling. You have gone to a great deal of work creating continuous scales, then you throw most of that information away. I would recommend using your continuous measures directly, and you could likely use simple OLS to estimate the equations. If you are committed to the dichotomy, it's not clear your results hold. Your first claim is that authority→tightness. If we cross-tabulate the two (using the counts presented in table 2) we get the following:

	Tightness	
	LOW	HIGH
No Authority	14	9
Yes Authority	3	9

which results in a Fischer's Exact test probability for the full table of .039. However, the same table for consensus and tightness produces the following:

	Tightness	
Consensus	LOW	HIGH
Low	6	12
High	11	14

for another significant table (Fisher's Exact test of .05). However, if you look at the relation of authority to tightness (the main theoretical claim) *net of consensus*, there is no significant association. Granted we are approaching small Ns here, but the lack of any *independent* relation between authority and tightness is troubling for your hypotheses. While it is true that the majority of authority groups fall into the Low Consensus / High Tightness cell, you provide no prior theoretical linkage between authority and consensus that would lead us to this 'two-dimensional' result.

I suspect that your results would be stronger if you were not throwing away so much information by dichotomizing the variable, but as it stands the current relation between these two variables seem pretty weak.

A similar exercise reveals no association between your next two variables (clarity of power and consensus), even if we do not net out the association between consensus and tightness. As you go on to show, the relation is stronger once we account for political groups, but this is an unsatisfactory result. For one, it seems that you should have similar results with any group that is formed around one of your main issues (thus, we would expect religious based groups to have much greater consensus on religious issues, for example), and thus group composition and type need to be factored in.

While a sample size of 40 makes many multivariate methods difficult, you have a sufficient sample for analyzing the data with OLS and a handful of controls (type of group, male-female composition, density of the friendship relations (that would help counter the peer influence critique). Given that OLS is usually quite robust to even major assumption violations, you could increase our confidence in your central findings by fleshing out your models more carefully, in a way that would demonstrate that the relation between authority/power structure and belief system are not spurious on the group composition and interpersonal (non-power) relational structure.

Friedkin, N. E. 1998. *A Structural Theory of Social Influence*. Cambridge: Cambridge.

Krackhardt, D. 1994. "Graph Theoretical Dimensions of Informal Organizations." *Computational Organizational Theory*, Editor Kathleen Carley and Michael Prietula. Hillsdale, N.J: Lawrence Erlbaum Associates.

Lofland, J. and R. Stark. 1965. "Becoming a World Saver: A Theory of Conversion to a Deviant Perspective." *American Sociological Review* 30:862-75.

Reviewer D (first round) returned a short (half page) review asking why I did not use loglinear tests, and thought that it would help to include Appendix B.

THE STATE UNIVERSITY OF NEW JERSEY
RUTGERS

Department of Sociology
54 Joyce Kilmer Avenue Piscataway New Jersey 08854-8045
Main Office Tel #: (732) 445-4035/4029
Fax: (732) 445-0974

November 26, 2000

Dr. Roger V. Gould
Editor, American Journal of Sociology
5835 South Kimbark
Chicago IL 60637

Dear Dr. Gould:

Thank you for allowing me to resubmit a revised version of the paper, "Power, Authority, and the Constraint of Belief Systems." It is enclosed, along with a memo detailing the revisions. Because all of the reviews were on the mark, and each point apposite, I thought it worth addressing each one. I also believe that I have been able to make every change called for, with one exception, namely grounding the hypotheses more in pre-existing research. While I have now pointed to important claims by both Durkheim and Weber that lead in the direction I have taken, such research does not, so far as I have been able to find, yet exist.

Most importantly, I have addressed two key issues pointed to by yourself in your letter. The first was whether the two-dimensional analytic belief space loses information. This is addressed at some length in the memo, and in abbreviated fashion in the text. The short answer is that while reduction to two dimensions may fail as a descriptive device, the entropic approach is intended as a measurement, and here it does not fail, for there is no form of organization that will not be measured. Further, the decomposition to two dimensions is one that is understood by almost all other researchers to be a necessary one; the only difference is that the entropic approach does this in a consistent and complete way.

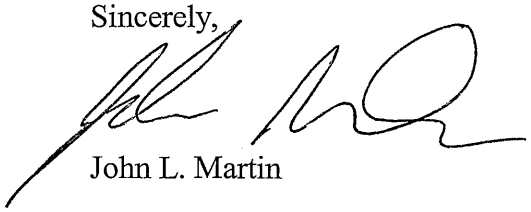
The second point you stressed was to include correlations of the measures: they have been added as a new appendix. In keeping with the suggestion of reviewer D, I have included for the general reader a shortened version of the appendix originally intended only for reviewers. The longer version for reviewers is included separately.

But I have also made the other revisions pointed to by the authors. Most of these pertained to clarifying and streamlining the discussion or improving the analyses, but one was fundamental, namely Reviewer C's suggestion that my logic implied that cognitive authority should increase the tightness across domains. I now examine this, and am led to a revision of my claims which is

along the lines suspected by the reviewers, namely an acknowledgement of the importance of pre-existing cultural definitions of what "hangs together."

These were very challenging reviews that have led to the paper being a bit decreased in scope, but increased in depth and validity. Than you for your time, and I look forward to hearing from you.

Sincerely,



John L. Martin

e-mail: JLMartin@rci.rutgers.edu

Revision Memo:
Power, Authority and the Constraint of Belief Systems

Three reviewers returned comments: these comments generally reinforced one another, and only at one point did suggested directions diverge so that a choice arose (furthering the qualitative analysis via loglinear tests suggested by D or furthering the quantitative analysis via regression analysis as suggested by C). Some matters touched (I) exposition, some touched (II) possible alternative explanations, and some touched (III) core issues of data analysis. In this memo,

(I) I describe how I have altered the exposition in line with the suggestions made by the reviewers;
(II) I discuss evidence pertaining to alternative explanations at greater length than was possible in the revised text (since the suggested alternatives raised by the reviewers are very reasonable, but turn out not to hold in these data);
(III) Regarding core analytic issues, I (1) allay reasonable doubts held by Reviewer C as to the properties of the two-dimensional belief space analysis; (2) discuss how pursuing this reviewer's point that cross-domain tightness should be examined led to an elaboration and clarification of the analysis; (3) discuss how new analyses demonstrate the strength of the evidence in support of (revised) hypotheses.

Each section of this memo first focuses on the most important points made by multiple reviewers, and then treats other comments or suggestions made. Points are referred to by the page number in the first version, so that comparison can be made to the reviewers' comments.

I. MATTERS OF EXPOSITION

All reviewers suggested tightening up the writing, especially the beginning. B in particular thought that the introduction of the theoretical claims had a number of flaws besides length. In particular,

1) The claims were advanced at too general a level.
I have tried to make the analytic strategy of development clearer and have specified the scope conditions under which the hypotheses advanced may hold, namely groups in which there are not well-institutionalized procedures for the corroboration of beliefs in the absence of personal authority. I have consequently distinguished the case being pursued from the special cases in which an institutional structure supports individual empirical inquiry, and when knowledge is accessible in materialized form (paradigmatically, authoritative texts).

2) It was not plausible that all contradictions resolved by authority.
I of course agree, and hence I stressed that this was only a tentative approximation for the purposes of derivation, but to remove any confusion, I have begun the section on authority and the tightening of beliefs by specifying the scope conditions referred to above.

3) The technique of deriving them via logic was not convincing.
I have generally tried to follow this reviewer's suggestions for revising the exposition; however, I still thought it important to introduce the idea of sampling from the belief system to make the point that a formal analysis may lead to such sampling giving unbiased results (where a content analysis generally will not).

4) It was not clear why only propositional beliefs were included (Reviewer C also made this point at (7, ft 6)).
   While philosophers have found it impossible to wholly separate all statements into two classes, those that express no evaluations and those that express only evaluation and not matters of fact, this distinction works reasonably well for sociological research, and is necessary for the investigation of formal organization, since the relationship that I examine may be found only between propositional beliefs. (It may be found elsewhere, but it is more difficult to explain how it would arise for other cases.) I have followed reviewer C's suggestion to spell out why this distinction is necessary.

Reviewer B suggested
1) scaling back the claims,
2) acknowledging the difference between these groups and others (13, 20),
3) that the hypotheses should be gleaned from research, not derived.

The first two suggestions have been scrupulously followed, but not, unfortunately, the third suggestion, as there has been very little research along these lines. Instead, I complement the (shortened) deductive argument with (what else) an appeal to authority, by noting that the work of both Durkheim and Weber suggests that authority may be necessary for the organization of belief systems.

In addition, I have condensed the introductory discussion greatly; while following a somewhat similar beginning, I follow B's suggestion to emphasize immediately that this will be a formal analysis of belief.

Other matters of exposition:

Reviewer B

(4) Reviewer B pointed to the confusing nature of the treatment of Scheler; I have deleted this unhelpful section. I have also tried to make the treatment of Simmel better serve the purpose of illustrating the basic idea of a formal analysis.

(8, 12) Reviewer B asked for definitions to be placed earlier and/or more clearly. This has been done.

(17) The metaphor of four-wheeling on the beach did not resonate with reviewer B; nor does it with me. However, the analogy is productive; unfortunately the alternative suggested by the reviewer (flying) is not analogous, as planes travel in fixed flight paths.

(21, 23). I have made sure that the first reference to Appendix A is clear.

(27) Reviewer B found the terminology of clarity confusing, and the discussion did not make sufficiently clear that the "distance" between statuses was simply a way of parameterizing certainty, and that it is not possible in this model for groups with closer levels of status to have clearer distinctions. I have tried to make the discussion clearer at this point.

(29) The oxymoronic phrase "the presence of an absentee leader" has been straightened out.

(36-37). This reviewer also likes the idea that the secularization debate is misspecified, and urges me to say more. At this one point, and this only, I demur from following the suggestion of a reviewer, much as I would like to, as to do so would necessarily lengthen the paper.

Reviewer C:
(7) The hackneyed phrase "breathtakingly bold" has been omitted as suggested by this reviewer. (8, 9) Reviewer C pointed to a weakness in a paragraph on this page; I have condensed and eliminated this material to move quickly and clearly to a definition of the key theoretical term, "constraint." (Use before definition was also pointed to by Reviewer B.) The definition of constraint has also been reworded to avoid the double-negative pointed to by Reviewer C.

(9-10) Reviewer C asks why logical connections would not lead to some formal structure (or nonrandomness). I had intended this to be a definition of constraint, not a claim, and so I have removed the gratuitous words specifying that the "reigning in" of beliefs is the result of unnamed social factors. Logical factors may also lead to constraint; this is an empirical question.

(12) Reviewer C points out that the words "change" and "movement" are used where there are no longitudinal data. The slippage between this temporal language and the cross-sectional data is quite important; unfortunately, the conventional interpretation of constraint and tightness is premised on the equation of atemporal association and temporal change. Trying to work around this temporal language becomes extremely awkward. However, I have added (earlier) an explicit mention of the key assumption which allows for this linkage, namely that the overall distribution is more or less fixed; then and only then does the cross-temporal association give us an indication of expected patterns of temporal change. This assumption underlies most analyses of association of beliefs, but is not generally made explicit.

I tried combining figures 1 and 2 as suggested by this reviewer, but having two different sets of axes displayed simultaneously seemed confusing to some readers.

(13) I have tried to stress the centrality of a wholly formal analysis earlier as suggested by this reviewer. I have also followed this reviewer's suggestion to make the end of the last sentence of the first paragraph in the new section a footnote (a similar change was made at [17]).

(Ftn11.) I agree with Reviewer C that compartmentalization only begs the question of what can be compartmentalized; my argument is that such compartmentalization requires differentiated authorities.

(16) I have tried to clarify the distinction pointed to by this reviewer, but do so below after the second hypothesis (unlegitimated power→consensus) has been introduced.

(17) I have tried to be clearer regarding the difference between a "gross" inability to move in the belief space (consensus) and the rules of movement associated with tightness.

(18) The two hypotheses have been spelled out at the end of the section on the shaping of belief systems, as suggested by Reviewer C.

Reviewer D:

Reviewer D suggested that appendix B be included in the paper, as this aided comprehension. As the basic results have been published elsewhere, I have made a shortened version of this appendix to be included with the article if the editors agree with Reviewer D.

II. ALTERNATIVE EXPLANATIONS

Both Reviewer B (e.g. 18) and Reviewer C suggested that other possible factors involving group life might be responsible for the shaping of belief systems, such as eating together. I was able to examine these questions in great detail, including some of the exact factors suggested by reviewers. In particular, I examined the correlation of consensus with the age of the group, the density of various relations, norms regarding sharing of meals, the ethnic and racial composition of the group, the degree to which ideology was considered a crucial focus of the group, the degree of interaction in the group both in prescriptive and descriptive terms, the number of meetings held a month, the optimal frequency of meetings held by the group, the current number of members, and the degree of explicit instruction given new members. Only the density of loving ties seemed to affect consensus, and this turns out to be explained by other features of the power matrix not analyzed in this paper. In general, I have been quite surprised to find that time-related variables never make a difference for such matters in these data. (Thus the correlation of consensus and the average number of meetings per month is -.001.)

Similar group level analyses of possible correlates of tightness were carried out, and again did not uncover any contextual factors that might explain away the relation between authority and tightness, with one exception which could not be understood (the gross number of persons who have left the group is associated both with tightness and with the presence of a cognitive authority). Given the number of tests run, it is likely that some relations would be statistically significant merely on the basis of chance, and so a theoretically unpredicted relation that depends on the specification (only the gross number of leavers, and not the percent turnover, has this effect) should not be given too much weight. The Lofland-Stark hypothesis may be valid, but interaction itself does not seem to affect tightness or consensus of beliefs.

Related to this, Reviewer C (espec. re p. 15) suggests the Friedkin peer influence tradition as another possible explanation for tightness. While it is true that peer influence can lead to a unified system of beliefs (as this reviewer says), it is important to bear in mind (as this reviewer has already pointed out) that unification and tightness are not the same, and that indeed tightness requires some disunity to be measured. Tightness, it must be recalled, is not the intensity of belief but its pattern of organization.

However, this point may be expanded as follows: the Friedkin approach posits not only agreement, but structured disagreement due to differential position in social space. An aggregation over all such space might lead to the presence of apparent tightness simply due to clusters of like-minded persons. Within any cluster, there is no actual tightness, it is only the juxtaposition of clusters that leads to an apparent association. It is impossible to dismiss the possibility of such occurrences, but they are a complication only in regard to measurement strategy, not theoretical definition, for while a measurement of tightness that ignored differences between clusters would be non-zero, according to a theoretical definition, a correct measure of

tightness would be zero. (And it was in the context of a theoretical claim that this point was made.) A (shortened) discussion in the text now makes this important concession.

But this reviewer is more generally troubled by the absence of non-power effects, peer influence being the most likely alternative. Extensive analysis of these data has found that
(a) peer influence is important in shaping beliefs, but
(b) this influence, far from explaining away effects attributed to power or authority structures, only takes place where a power relation is present.
To explain, there is evidence of interpersonal influence by which some (non-leader) member X influences some other member Y. But one might be initially surprised to find out that this influence does not depend on intuitively reasonable variables such as the amount of time the two spend together. (This may be because as everyone spends so much time together, the "floor" is so high that it ceases to discriminate, but I am not sure of this.) Instead, it seems that this influence requires that Y acknowledge that X is superior to him- or herself. But even taking this dyadic level influence into account, the global clarity of power relations still has a strong, significant effect on consensus. Further, extensive efforts to map the pattern of formal organization to influence structures in these data have proved unsuccessful. Whether this is because of limitations in the data or whether the interpersonal influence processes work within a more general pattern set by authority, I do not know.

To conclude, analyses reported in Author (1997) suggest that interpersonal influence, while certainly occurring in these groups and analyzable in these data, does not explain away these group-level patterns.

Other Comments, Organized by Reviewer

Reviewer B:

(24) While Reviewer B does not criticize the measures of power used, this reviewer does caution that overstating their quality is not helpful; in agreement, I changed "extremely good" to "good."
(28) Reviewer B suggests that in contrast to other groups, here members can be tossed out, leading to another type of selection. While it is true that this in an important type of selection in principle, it is not clear to me that these groups are thus different from other groups. Very few groups make it impossible to expel members; this is especially true of thought communities, whether religious or scientific. Many communes are among the few that formally prohibit expulsion of dissident minorities!

(33) Reviewer B points to possible weaknesses in the evidence against self-selection, and suggests changing the claim to "there is some evidence that..." Agreeing, I have made this emendation, but more extensive comparisons than those reported here demonstrate that the two reasonable scenarios posed by this reviewer, both of which involve the intervention of strong leaders, do not invalidate the pseudo-experiment. Most of these communes had the same pattern of leadership, and even holding leadership constant, we find here a strong relation between clarity of power and consensus. This reviewer asks whether there is a significant difference between the two consensus-clarity correlations of .582 (within Guruland) and .492 (the other communes). There certainly is not, but my point was only that the former is not lower than the latter, which would be expected if the relation between clarity and consensus was spurious. I have changed the text to make this clear.

Reviewer C:
(18) Reviewer C asks whether this idea might apply within families; an interesting question that was once asked by someone else and has had me stumped since. I have wondered whether a differentiation of cognitive authority associated with egalitarian parenting should lead to a single differentiated system of thought as opposed to one undifferentiated sphere or, in the case of each parent having authority over one sphere, two independent spheres. This may not follow, since as the child forms a true cognitive system, there is a generalization of authority away from these particular others. But I don't know.

(24f). Reviewer C asks how the measure of clarity of power might compare to other well known measures of hierarchy, such as those of Krackhardt (1994). The choice of comparison is extremely apposite, because few attempts to measure hierarchy in networks have started from the distinct qualities of (potentially) anti-symmetric relations, but instead have used properties that have to do with hierarchy induced by relations that are not themselves hierarchical (e.g. "attractiveness" in the Holland-Leinhardt sense) (this is true of the interesting measure of hierarchy introduced by Coleman in Mathematical Sociology). In contrast, Krackhardt's measure of hierarchy focuses on the directionality of the overall structure of relations. In this way, the measure reaches towards an aspect of structure similar to that measured by my "clarity." However, there are three differences:

1. Krackhardt's measure does not assume a single undifferentiated ordering of statuses, as does the measure introduced here. While it is possible to generalize the approach used in this paper to cover differentiated hierarchies, as was done in the article cited, the structures in this data set do not appear to be differentiated. Accordingly, the generality of Krackhardt's approach would be wasted here, where a more focused measurement can be made.

2. Relatedly, Krackhardt's measure of hierarchy focuses on reachability: a natural interest given the beginning with organizational theory. The measure thus tells us how closely a network approaches a command tree. But in the gemeinschaftliche groups studied here, reachability is not of great importance in itself.

3. The final difference that Krackhardt's approach is not stochastic. This makes sense given his starting point in organizational theory: it makes quite a difference whether 4 is connected to 5, if 4 must transmit a directive coming from 3 if 6 is ever to receive it. But to understand the relative spread of status in a group, we are happy to admit that 4 may simply have forgotten that she had power over 5 on the day she filled out the questionnaire.

It is still quite possible that the two measures will reinforce one another. This would be the case if there were no errors other than those pertaining to calling relations "equal," since Krackhardt's measure of hierarchy is a percentage of all paths observed that are directed, and there would still be no paths of the wrong direction (hence the denominator would decline with the numerator). But errors in perceiving which of two persons is "above" the other may be treated differently by the two models, since there is frequently

something semi-catastrophic in the way that communication networks respond to "damage," which is what error would be. Since other readers may be interested in the question, a brief note to this effect has been added when the measure of clarity is introduced.

(22) Reviewer C asks why the latent propensity to answer strongly (which I'll call "sureness") doesn't bear on the degree of tightness of the system of beliefs. This is a good question with a good answer. My answer here is longer than that added to the text, but I have clarified the remarks there accordingly.

First of all, there is empirical reason to dissociate the two: "sureness" (as the percentage of all opinionated responses which were "strong" for any group) was strongly correlated with consensus, but not with tightness (this is even true when we examine the sureness and the tightness for any domain individually; there, two of the six correlations are actually negative). In other words, groups where people held their opinions more firmly were not necessarily groups in which beliefs were interconnected.

But should "sureness" be considered a possible formal dimension of the belief system in principle distinct from consensus? This would require that group members be able to firmly agree with one another that they weakly believe that, e.g., reform is preferable to revolution (e.g. high consensus, low sureness). I have not found any evidence that such a separable dimension of sureness exists: there are no structural correlates of this sureness that are not spurious results of correlations with consensus. It seems more likely that the average sureness in any group may be seen as due to (1) the degree of group consensus and (2) some latent individual "propensity-to-answer-strongly." Accordingly, it seems that ignoring degree-of-sureness simply removes noise introduced by individual heterogeneity.

III. ANALYSIS OF BELIEF SPACE

(1) The reduction of form to two dimensions

(a) Is it sufficient?

Reviewer C has two questions regarding the approach to the quantification of the constraint in beliefs, which decomposes the total constraint into two portions, each termed a dimension in an "analytic belief space." Since both of these are important issues that may trouble other readers, I discuss these (more briefly) in the revised text. Reviewer C's first question is: are two dimensions sufficient to adequately describe the structure of any belief space? The answer here is that (a) two dimensions do not necessarily identify the important structural features of any given belief space, but (b) the approach laid out here does not make any assumptions as to the limited dimensionality of the structural features in the belief space. It must be stressed that the analytic belief space is a two-dimensional measurement, and not a description. As a measurement, it is wholly general, in that there is no form of organization that will not be measured. It will not, however, describe what the form of organization is. (Space forbids a discussion of possible forms of organization as suggested by reviewer C, but I can cite a recent article laying out some ideas if that would help.)

In other words, there may be a complex three (or more)-dimensional interaction in some belief space. This will not be distinguished as such by the techniques used in this paper; for example, this particular belief space may have the same tightness as a belief space in which there are only two-dimensional interactions. However, in contrast to other techniques which define interaction as two-dimensional, the degree of this three-dimensional interaction is correctly measured.

For example, consider in a three dimensional belief space a distribution that happens to form the (admittedly unlikely) shape of the surface of a sphere. The bivariate association between any two dimensions will be exactly zero, and a conventional approach that assumes that tightness means bivariate association will conclude that there is no tightness in this system, despite that fact that knowing two of the three beliefs leaves possible only two values that a respondent might hold on the third. The techniques used here, however, will consider the tightness to be extremely high (and consensus will be inversely proportional to the cubed radius of the sphere). They will not identify the form as three-dimensional, but they will quantify it correctly.

(b) Are the dimensions separable?

The second point reviewer C makes is also quite pertinent, namely that the two dimensions may not be separable. The answer here is that while it is true that the two dimensions are not absolutely separable, in that absolute consensus is not compatible with any tightness (since there is only one possible distribution compatible with the observed marginals), in practice, the entropic approach allows for great independence between the two dimensions. This is in contrast to conventional approaches, that tended to force strong negative correlations between consensus and tightness. In contrast, for these data, there were positive correlations between the consensus and the tightness for three of the domains, and negative correlations for the other three—so there is no "iron law" associating the two as is the case for measures based on bivariate correlations.

Despite the fact that absolute consensus makes it impossible to measure tightness, it is thus not necessarily the case that tightness decreases as consensus increases. This is because the (small *N*) measure of tightness is relative to the number of macrostates compatible with the observed marginals. As concentration increases, and degrees of freedom decrease, the denominator of this measure decreases (and not only the numerator).

The discussion of the decomposition of the overall constraint has been changed not only to improve exposition as mentioned above, but to make these points, although in greatly abbreviated fashion.

(2) Cross-domain tightness

I found this critique most troubling: Reviewer C pointed out that my logic implied high association between beliefs in different domains, while I had only looked at association of beliefs within domains. This had been done because (a) I did not think any random selection of 4 beliefs would be likely to tap the general tendency for beliefs to be associated; (b)

computing power then made running a large number of such selections unfeasible; (c) I don't think I was really convinced that such inter-domain connection would exist.

But reviewer C was absolutely correct: I should look at cross-domain tightness. Increases in computing power now make this tractable: as discussed in the revised text, I eliminated two of the domains that had beliefs that were less concrete and propositional, "general life" and "intellectual," so that I was left with four topics; hence drawing one question from each topic would lead to the same four-dimensional space I had determined to be the maximum feasible given the *N*s. I then computed the tightness for all 4x4x4x3=192 spaces formed by all possible combinations of these beliefs (the gender category only has three items). A linear average was then constructed of all of these.

As currently analyzed in the paper, the results are quite interesting: the same pattern is found, with one exception: groups with a formal resident but no absentee formal leader has the highest mean tightness. Now it is not clear how reliable this finding is: a comparison of median values restores the order retrieved by within-domain analysis. Furthermore, the explained variance is lower for this across-domain measure, which could mean that this is simply a poorer measure than the average within-domain tightness. But the results are also compatible with the revised hypothesis that (a) cognitive authorities can systematize beliefs, but they have to contend with pre-existing cultural domains of relevance. This is supported by a demonstration that the presence of cognitive authority has a greater "tightening" effect for domains that are "relevant" to the group—religious beliefs for religious groups, and political beliefs for political groups. (This gets to another valid point made by this reviewer regarding central vs. peripheral beliefs [23].)

(b) the formation of across-domain connections is more difficult, and here the singularity of a leader is more important than his or her ability to construct an elaborated belief system: resident formal leaders were often charismatic, but less likely to have formed an entire cult than an absentee leader. They were, however, "on the spot" to draw together beliefs from different domains in the process of everyday life.

I now conclude with this interesting and plausible hypothesis, without being able to conclusively demonstrate its truth.

(3) Strength of evidence

Reviewer C (28ff) said that it was disappointing to have the resulting variables dichotomized, thus losing so much information, and suggested analyzing the original continuous data. I now do exactly this; the tables presenting the distribution across the four quadrants of the belief space are no longer printed (though some of these numbers are still used to facilitate discussion of the distribution of groups across the analytic belief space), and hypothesis-testing now relies on detailed continuous analyses of single dimensions. (As a result, analyses of the old tables suggested by D have not been pursued, as said below.) This reviewer also suggests controlling for group composition variables such as gender composition. As noted above, however, important commune heterogeneity is almost never associated with things like sex ratio or ethnic balance (which have been examined), while what is termed "ideology" but more fundamentally refers to commune "type" is of the

utmost importance. Consequently, it is this factor which is examined (as said above), and which has led to the most significant challenge to the hypotheses with which I began.

(30) Reviewer B pointed to the absence of statistical tests—in focusing on the one-dimensional continuous analyses (as suggested by Reviewer C), these tests have been added.

Other points, organized by the Reviewer

Reviewer B:
(32) Reviewer B suggested handling the exceptional case of the political groups from the start, as opposed to generating a disconfirmation and then explaining it. I have happily adopted this more concise form of explanation. This reviewer also suggests examining the attitudes of believers towards power and authority. Unfortunately, the belief items only ask about attitudes towards those currently in positions of power and authority, not attitudes towards power and authority per se.

Reviewer C:
(19) Reviewer C pointed to the shifting *N*'s; I have noted this in the text at the place indicated. In turning to this question, I realized that cases lacking data on power relations were unnecessarily eliminated from analyses pertaining to cognitive authority; these have been added, raising the *N* to 44.

(21) Reviewer C asked whether there were absentee leaders in non-religious groups. In the text I now indicate that such leaders were also found in political and psychological groups.

Reviewer D:
Loglinear analysis

Reviewer D suggested a loglinear analysis of the tables that present the distribution across the four quadrants of the belief space. This reasonable suggestion cuts somewhat against the implication of Reviewer C's suggestion to have a more rigorous one-dimensional analysis of the continuous data. I have decided to follow Reviewer C's direction since the data in these tables is formed by a more-or-less arbitrary dichotomization intended merely to facilitate discussion. Statistical tests are more appropriate for the uncollapsed data.

Appendix B for Purposes of Review ONLY: The Entropic Approach to the Constraint in Belief Systems.

I have defined constraint as the degree of resistance to arbitrary movement in the belief space. For the purposes of exposition, assume a one-dimensional, discrete space such as that associated with a polychotomous variable (we can always redefine a multidimensional space as a unidimensional space for these purposes). If we assume very large samples, so that we can imagine the probability of the total that is in any one cell being a quantity (here called a probability) that changed continuously, we would want a measure of this constraint to have the following properties: (1) It should be at a maximum when there is unanimity; (2) it should be at a minimum under equiprobability; (3) it should change continuously as the cell probabilities change continuously; (4) given marginal distributions, it should be at a minimum when items are independent; (5) given equiprobability, it should decrease as the number of possible categories goes up. Shannon (1948) has demonstrated that there is only one form for such a measure to take, for it is the negative of the entropy. This entropy is defined as follows:

$$Entropy = H = -\sum p_i \ln(p_i) \qquad \textbf{b-1}$$

(the summation is over all cells). This is the form of entropy that is used in information theory, and which has been applied to contingency table analysis, but seems to have fallen into relative disuse. Because of our small samples, however, we cannot assume that we have continuous probabilities, and instead use the thermodynamic version of entropy.

Classical statistical thermodynamics begins with a distinction between microstates and macrostates—macrostates are the recognizable orderings of sets of particles, the microstates are the various ways in which indistinguishable particles, by their specific arrangement, produce the same aggregate macrostate. The analogy to contingency table analysis is quite straightforward—

what we have in the cell counts of the table is a macrostate—the microstates are all the possible arrangements of individuals in the table. If it is assumed that all microstates are equally probable, the "thermodynamic probability" of any macrostate can be derived from the combinatorial formulae as follows:

$$Thermodynamic\ Probability = W = \frac{N!}{N_1!\,N_2!\,N_3!\ldots N_M!} \qquad \textbf{b-2}$$

where N_1 is the number of particles (or persons) in the first state (or cell), etc, N is the total number of persons, and M is the total number of cells. The total number of microstates is M^N, and the probability of any macrostate is W divided by M^N. The thermodynamic entropy S is then equal to $k\ln(W)$ (where k is Boltzmann's constant), i.e. it is proportional to the logarithm of the thermodynamic probability, and it is easily demonstrated that the relation between informational and thermodynamic entropy is that $H=S/(k\mathrm{N})$. (The rest of this paragraph proves this result for purposes of review.) Since $S=k\ln(W)= k[\ln(N!)-\sum\ln(N_i!)]$; by Stirling's approximation, when $x>15$, $\ln(x!) \approx x(\ln[x]-1)$; hence $S \approx k[N[\ln(N)-1]- \sum N_i[\ln(N_i)-1]]= k[N\ln(N)-N- \sum N_i\ln(N_i)+\sum N_i]$ $= k[N\ln(N)-N- \sum N_i\ln(N_i)+N] = k[- \sum N_i\ln(N_i)+ N\ln(N)]$. Since Shannon's entropy is

$$H = -\sum p_i \ln(p_i) = -\sum \frac{N_i}{N}\ln(\frac{N_i}{N}) = -\frac{1}{N}\sum N_i(\ln N_i - \ln N) =$$
$$-\frac{1}{N}\sum N_i \ln N_i + \frac{\ln N}{N}\sum N_i = -\frac{1}{N}\sum N_i \ln N_i + \ln N \qquad \textbf{(b-3)}$$

H=S/(kN).

It would seem that there is no reason to prefer one form—the thermodynamic or the information—over the other. However, while Shannon's entropy is the correct measure for continuous probabilities, and the observed probabilities (=N_i/N) are the maximum likelihood

estimates of the continuous probabilities which approach these probabilities as N becomes very large, for small samples Shannon's entropy is different from the entropy given by the thermodynamic approach. This has consequences for the estimation of the relative improbability of any state, to which we will next turn our attention. It is for this reason that S may be preferred over H for contingency table analysis when some counts are small (<15).

However, once the connection of entropy to probability (i.e. the underlying thermodynamic probability) has been made, there is no reason to prefer entropy to probability, since there is a monotonic relation between the two. In fact, it turns out that it is better for contingency table analysis to use the probability itself. This is because entropy is a purely relative quantity. Only changes in entropy or differences in entropy across similar tables are meaningful. For this reason, social scientists have proposed "normalizing" entropy by comparing it to the maximum possible entropy (for example, Coleman 1964). But there is nothing intrinsically reasonable about such a procedure or meaningful about the results. A superior form of standardization would be not by reference to the maximum possible value, but the whole range of possible values.

We then want to see what proportion of observed macrostates are more improbable than the observed macrostate (hence one advantage of keeping things in terms of probability, as opposed to shifting to logarithmic entropies). We can compute the distribution of possible microstates associated with all macrostates, and then determine the number of macrostates that are less probable than the observed one. This is in effect to do a significance test, but unlike a normal chi-square test of independence, this is not conditioning on the marginals, but on a uniform distribution. The number of less probable macrostates (weighted by their microstates),

expressed in terms of a proportion of all possible microstates, is our "p value". This is what should be taken as the degree of constraint in the belief space.

Of course, it would be far easier computationally to have a formula that directly expressed the p-value of the probability associated with any microstate in terms of N and M (as opposed to generating all of the probabilities of the microstates, ordering them, and counting, which is non-trivial in terms of time). This seems not to be possible—instead, we must use a generating function to produce all the microstates associated with a macrostate (or with the "equivalence class" of macrostates with identical numbers of microstates[1]). Such a complete enumeration of equivalence classes is tractable for such small cases.

The same technique can be used to test whether there is constraint above the marginal level—just as the above method was equivalent to an exact test of the hypothesis of equiprobable distribution, so this would be an exact test of multiway independence given a set of marginals. In this case, we simply make permutations that have the marginals fixed to the observed values, and compare the number which have greater constraint than observed to the number which have less constraint (since the constraint due to the marginals is the same in all cases in this new distribution, the constraint above-and-beyond the marginals is reflected in the total constraint). This is our measure of tightness referred to above.

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[1]. A combinatorial approach using equivalence classes (Feller 1957: 38) (which, as far as I can tell, does not lend itself to easily encapsulated in a formula for the number of distinct equivalence classes, though it can be automated) may be used for some cases; in others, a large number of simulations can retrieve an unbiased estimate.

References:

Coleman, James S. 1964. Introduction to Mathematical Sociology. New York: The Free Press of Glencoe.

Feller, William. 1957. An Introduction to Probability Theory and Its Applications. Second Edition. New York: John Wiley and Sons.

Shannon, Claude E. [1948] 1963. "The Mathematical Theory of Communication." reprinted in Claude E. Shannon and Warren Weaver, The Mathematical Theory of Communication. Urbana, Illinois: The University of Illinois Press.

A J S
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Editor: Andrew Abbott • Managing Editor: Susan Allan

5835 S. Kimbark Ave.
Chicago, IL 60637

tel 773 702-8580
fax 773 702-6207
ajs@src.uchicago.edu

Professor John Levi Martin
Department of Sociology
Rutgers University
54 Joyce Kilmer Ave.
Piscataway, NJ 08854-8045

March 21, 2001

Dear Professor Martin:

The editors have completed their consideration of your revised manuscript, "Power, Authority, and the Constraint of Belief Systems." Unfortunately, we believe that the manuscript, although promising, is still not ready for publication. We would like, however, to see an additional version of the paper, and to that end we encourage you to undertake a second revision.

It is unusual for *AJS* to request a second revision. Usually when a paper is not publishable on the second round it is rejected. But in light of your responsiveness to the first round of reviews and the importance of your topic, we would like to extend our normal process. While all the reviewers make helpful suggestions, attention to the substantive and editorial problems outlined by reviewer A will make the paper a much stronger contribution.

Of course, I must add the caveat that no guarantee for publication can be given at this point. But since this is a second revision, we will limit readers on the next round to persons who have already read the paper; no new readers will be added. This ought to cut down the turnaround time considerably. I also recognize that, in spite of our continued interest in the paper, you may not wish to pursue a second revision; if this is the case, could you please let us know?

Thank you for your patience with this process and for your interest in *AJS*. I hope we will have the opportunity to see this paper again.

Sincerely

Andrew Abbott

AJS MS 050270

REVIEWER A

Review for Power, Authority and Belief Systems (AJS 050270, Revised).

This paper is intriguing and interesting, and is potentially and important contribution to the formal analysis of ideas. The revision answered many of the points raised by reviewers last round, resulting in a tighter and more convincing paper.

In general, I think the author still needs to focus a great deal more attention on the writing and macro-organization of the paper. It is a hard paper to read. Harder than it should be. Moving away from colorful turns of phrase, focusing paragraphs, and generally staying on-topic within sections will provide a simple fix to this problem. Secondly, the response to reviewers memo is much more convincing (as it should be since it is directed at this end) with respect to the point made last round about alternative explanations than is found by simply reading the paper. Especially with regard to alternative explanations for the findings, including a short section that summarizes the failure of alternative results, with appropriate cites to Author's previous work, would be helpful. I organize comments on this paper by topic below, and end with detailed comments about specific sections.

The dimensions of the belief space.

While the author does a better job this round of explaining substantively the difference between tightness and consensus, they are still not intuitively clear. The AJS audience is broad and general, and the author could go a great distance in clarity by adding a small number of simple examples in appendix B. For example, for each of a set of three by three tables, work through the calculations for Q, T and C. This will make the work more useful for future scholars interested in applying the ideas in other contexts (since they would have a source to check their calculations) and ground the concepts in examples. I would suggest four tables, along the lines of:

Equiprobable: Low Q, low T, Low C.

	A	B	C
1	5	5	5
2	5	5	5
3	5	5	5

Unanimous: High Q, High T, High C.

	A	B	C
1	37	1	1
2	1	1	1
3	1	1	1

Correlated: constraint high (?), tightness high (?)

	A	B	C
1	14	1	1
2	1	14	1
3	1	1	14

Uncorrelated: constraint high (?), tightness low (?)

	A	B	C
1	14	14	14
2	1	1	1
3	1	1	1

While this is done conceptually with figures 1 and 2, making it concrete would help.

Relating power structure to domain position.

Given a clear conception of the belief space, the central arguments need to be made more transparent.

Two hypotheses are made:

1) That the presence of a unifying cognitive authority will increase tightness ("The systematic organization of beliefs *requires* that they be within the domain of judgment of the same authority figure." (p.14 – emphasis added))

2) That the clarity of power will increase the consensus of the belief systems. ("An inability to perceive of the possibility of alternative to the status quo translates into an inability to perceive of the possibility of alternatives to beliefs." or later "...those who cannot move in social space may be incapacitated when it comes to moving in the belief space." (p.17))

The author works through two related points to arrive at the first hypothesis. First, that propositional beliefs rest on appeals to authority – at least the ones that are sociologically interesting (p.12 – note that this borders on a circular definition, but we'll leave that aside). This follows because, *and this is where the argument needs to be clarified*, no person has the ability to judge most matters for themselves.[1] Second, the author uses contradictions and compartmentalization to show that beliefs are linked. For two beliefs to contradict, the meaning of one belief must relate to the meaning of another belief. The most common solution to contradictions is to compartmentalize, which only sets the problem back a step since now instead of one we have two cognitive authorities. The final statement, then, that "the systematic organization of beliefs requires that they be within the domain of judgement of the same authority figure" rests on the initial claim that sociologically interesting propositional beliefs rest on a cognitive authority – in fact it reduces to the same statement. That is, there is no *argument* here, only a claim that beliefs are held together by cognitive authorities. The paper would be stronger if this point made more clearly, which I think, would be easy to do through well-known examples (in addition to the appeals to Durkheim and Weber, spell out the reasons why knowledge rests on authority. You could likely draw on Kuhn's work on paradigms, work on expert witnesses, and so forth.) Do not shyg away from the (perhaps quite limited, but for an audience of academics most salient) cases where people *do* adjudicate beliefs based on their own logical or empirical resources.

The second hypothesis rests on a different logic and is made in one paragraph on p.17. The author links two ideas: (1) that an inability to perceive of the possibility of alternatives to the status quo translates into an inability to perceive of the possibility of alternatives to belief, and (2) that the clarity of power = an inability to perceive of the possibility of alternatives. Both parts of the argument that really needs spelled out more clearly. For example, I can imagine situations where power is very clear (the rank system in the military, for example) but where people can imagine very different alternatives (promotion). While not stated, I assume (perhaps incorrectly?) that we would expect places with a *clear* power structure to also be a *stable* power structure – that is, one that does not change over time. If the author can spell this out, and perhaps link it to these other settings (military, police, ranks within the priesthood, etc.) it will enrich the paper for further research into these substantive settings as well. There is also, by implication from p.16, a claim that this type of power is *not* legitimate. But need we make the distinction here that *clear* power is *illegitimate*? Or is that stretching the implication? This needs to be made clear in the argument.

Both of these arguments, as pointed out on p.16, relate power to a dimension of the belief space. Legitimate power = authority ==> tightness, illegitimate power = 'knowing one's place' = inability to perceive of something else ==> consensus. The reader is left wondering about the independence of these two effects. That is, what relation should authority have to consensus (positive, I would suspect, since any orthodoxy must be a small part of the potential belief space) or illegitimate power to tightness (none, I would suspect)? But this needs to be spelled out, especially in light of the discussion of figures 4 and 5.

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[1] Aside: the author misses opportunities to draw in readers with well known examples here, such as the common practice of getting expert witnesses for court cases – which invariably offer two competing cognitive authorities.

that place groups with respect to both dimensions (else, one could simple give a difference in means or the box-plots for each single dimension).

The empirical results

First, the presentation of the analysis is awkward, making the reader work a great deal to find out what is really going on with the data. I would recommend re-organizing the results sections to follow the hypotheses exactly. That is, first talk about tightness (and not tightness intersecting with consensus), then about consensus, then (after having expanded the discussion in the hypothesis section) about the joint effects – ***or*** drop the discussion of which *quadrant* of the belief space a group falls in (since you are not making any predictions about the quadrants).

Tightness

The main finding here is presented in tables 1 and 2. Since this is your strongest finding, I would recommend expanding it just a little. First, give the simple total means for each column at the bottom of table one (readers can calculate this from the numbers given, but why make them work this hard?). Then talk first about the simple difference in means (this is, after all, the main hypothesis), then talk about the difference controlling for presence of resident leadership. N of course, limits the regression, which is fine since we simply want a basic feel for the relations in this data. However, while you make the claim that this result holds across each type of group, why not show this in the table? That is, by adding dummy variables for each type of group (political, religious, and psychological) and showing that cognitive authority still matters, you have a much stronger finding. With 35 cases you can easily add the extra variables needed to control for the different types of groups.

Consensus

Removing the political groups, or calculating consensus without political items, both show that clarity of power correlates with consensus. Again, it seems that you can present this finding more clearly by replicating tables 1 and 2 (which, I was surprised *NOT* to find in the paper), with consensus as the dependent variable and clarity of power as the independent variable, controlling for type of group. This is implicit in the scatter plots (that is. the reader can work out some of the numbers) but why make them work so hard?

Selectivity

The natural experiment is a nice finding that is convincing, and helps to allay fears about selectivity within this data, but you should discuss selectivity more generally. Again, the military might be a good counter example, as there is clear selection of the type of people who choose to join the military (social conservatives) which means you have higher than random consensus from simple selectivity. Because all of your groups are selected into a communal situation, this may wash out in your sample, but in the general case in needs to at least be acknowledged that some level of consensus is likely due to selection. (as an interesting aside: you could test this on the military by looking at differences in consensus among differing branches, where the clarity of power might reasonably be expected to be less salient – i.e. I would expect higher consensus among fighting units like the Navy Seals than among logistic support crews for the Air Force).

Cross-Domain results

I like these findings, and think that you may be downplaying their effects. True, eta is smaller here, but the effect is still clear. Given the small number (3) cases in the lower left cell of table 3, it seems that the median is the right measure, which then matches perfectly. A footnote identifying the characteristics of the case that raises the mean might be in order. You need to slow down at the beginning of the last paragraph on p.35, discussing the division into local cognitive authorities, and be clear whether table 4 includes these local cognitive authorities or not.

In general, reviewers last round asked the author to dispel the worries that alternative explanations (peer influence, for example) could account for these results. While s/he does a good job of discussing this in the response to reviewers, a *short* section at the end of the analyses on alternative explanations, that repeats the

substance of these remarks would be useful. Also, add the citation (mentioned in the memo to reviewers p. 7 about possible forms of organization).

Minor details

p.3, par 3. strike 'more specifically'

p.3, par 2. strike 'grave'

p.4 first sentence "In contrast,...". Reads awkward, simplify.

p.5 and throughout : change 'set of data" to "data set"

p.6, first full par, "If we..." Still not sure this is needed, but appreciate the shorter version. Cut if you need space.
Appendix B-1. See above, provide simple examples to make the distinction between tightness and consensus computationally clear.

p.10. The statement that these two are "independent" is false given the observed (statistically significant) negative correlation between the two in appendix d.

p.11. sentance "The entropic approach is, as said above, ..' is not needed.

p.12 change 'to' to 'two' top line.

p.12 first full paragraph. This paragraph trails off, and needs reorganized.
Strengthen argument around p.13.

p.14. Do you need 'requires' in this sentence? It unnecessarily raises questions about how other factors (logical, empirical, etc.) *could* lead to tightness.

p.15. I would suggest sticking with 'a base of power' over 'a form of power' here, unless you flesh out the argument about legitimate and illegitimate power.

p.17 expand the argument linking illegitimate power to consensus.

p.24. Move the parenthetical ("While these are self-reports..') to a footnote.

p.24 change 'be such that'

p.27. "to recapitulate' is an ugly phrase, can you come up with something else?

p.27 drop "I now put these two hypotheses to the test."

p.28. last par. Here you start talking about quadrants (the intersection of the two domain spaces) but you have not theorized the intersection, only the dimensions one at a time. See again p.29, 2nd par.

p.30. Change 'reasonable' to 'reasonably' (?)

p.31. Move parenthetical to a footnote. "(this dichotomization.."

p.24. first sentence of first full paragraph: "This is not to ..." is awkward. Re-word.

p.35. Need to be very clear, and slow down, starting with last paragraph on this page.

p.38. Restate the first hypothesis in the positive, instead of the current negative specification.

A5

p.38. "In striking confirmation" is overstated.

The statement ending paragraph 1 on p.39 ("It seems of the greatest...") could be fleshed out by telling us what types of groups these were.

REVIEWER B

American Journal of Sociology

COMMENTS FOR AUTHOR(S)

AJS-A050270

Re: "Power, Authority, and the Constraint of Belief Systems"

It is difficult for me to see the point of all the formal machinery that takes up most of this manuscript. For example, the abstract states that the paper offers two theoretical claims for the relationship of consensus and "tightness" with the formal properties of social structure. Might it matter what that relationship was and what formal properties were involved? By page 10, I still do not know what the paper is about. What does the theory explain and how does it explain it?

The discussion of power, authority and legitimacy is loose. It is difficult to see how the intense focus on formalizations applied to such loose ideas could produce much progress. The hypotheses are vague as befits the theoretical ideas but negatives any value the formalization might have had.

It is not clear that the empirical test is closely related to the general theory. There is a big leap from absentee leader to legitimate cognitive authority.

There are some interesting ideas here that if presented clearly and concisely could make a contribution.

Reviewer C (second round) wrote a two-page, single-spaced, positive reviews, first emphasizing the strengths of the work, and defending the new methods against reviewers who had urged me to more conventional approaches. Then this reviewer points out that the paper still had an old-fashioned vision of beliefs as reified, and that the writing was still vague and the paper front-heavy.

THE STATE UNIVERSITY OF NEW JERSEY
RUTGERS

Department of Sociology
54 Joyce Kilmer Avenue Piscataway New Jersey 08854-8045
Main Office Tel #: (732) 445-4035/4029
Fax: (732) 445-0974

October 18, 2001

Dr. Andrew Abbott
Editor, *American Journal of Sociology*
5835 South Kimbark
Chicago IL 60637

Dear Dr. Abbott:

Thank you for allowing me to resubmit a second revision of the paper, "Power, Authority, and the Constraint of Belief Systems." It is enclosed, along with a memo detailing the revisions. The reviewers pointed to possible improvements pertaining to overall exposition and organization, to the introduction and discussion of the theoretical hypotheses, and to certain analyses. In all cases, I found the reviewers (especially reviewer A) to be making excellent points that could be addressed.

In particular, for purposes of derivation I had assumed that individuals could not empirically adjudicate between beliefs. Reviewer A rightly found this implausible and strongly suggested that the argument be clarified here. While this assumption is *sufficient* to problematize the existence of "tight" belief systems, it proved not to be *necessary*, and has been eliminated.

Reviewer A had also suggested organizing the paper as a more direct test of the two main hypotheses, and following more conventional procedures for testing alternative hypotheses through the introduction of controls. While reviewer C's defense of my original methods warmed my heart considerably, I believe that this is one of those fortunate cases in which adding controls does precisely what it should do, namely satisfy reasonable concerns that something else is going on. In addition to quieting such concerns, following A's general advice here improved the overall organization of the paper.

I have once again enclosed an expanded version of Appendix B for purposes of review only, though I imagine the reviewers have seen it already. Thank you for your time, and I look forward to hearing from you.

Sincerely,

John L. Martin

e-mail: JLMartin@rci.rutgers.edu

Second Revision Memo:
Power, Authority and the Constraint of Belief Systems

Once again, the comments made were insightful, mutually reinforcing and pointed to places where the paper could be greatly improved. Three reviewers returned comments: A and B emphasized further changes. In general, three areas of improvement were raised. First and most generally, all reviewers suggested that the paper could be clearer and/or more concise. Second, reviewer A pointed to weaknesses in the theoretical argument. Third, reviewer A suggested a more straightforward presentation of the results. In all cases, I concur with the reviewers' points and have revised accordingly. This memo discusses each of these three areas in turn, closing with some minor points.

General Discussion

B's brief comments reinforce A's more specific criticisms that the writing be made more clear and concise. Like C, B suggests more of a sense of what is to come. Accordingly, I have added a statement in the abstract as to what the hypothesized relations are, and a "road map" in the early portion of the text as suggested by C.

Reviewers made other suggestions for the improvement of the general discussion. A and C asked me to include a short mention of the fact that other possible factors affecting the belief system were examined and found to be insignificant; a shortened version of the relevant portion of the first revision memo has been included in the section on "other possible explanations." Reviewer A called for the elimination of colorful turns of phrase: several have been identified and liquidated; one (contentious content) is preserved because it is space efficient even if cute. Finally, changing the presentation of the empirical results (discussed below) had the side-effect of streamlining the paper.

Since reviewer A made the most detailed criticisms, I organize this remainder of this memo in terms of the headings used by this reviewer, and discuss his or her points and how I have responded to each in the order of reviewer A's original comments.

The dimensions of the belief space

Reviewer A suggested that the description of the entropic measurement strategy was still not clear, and proposed supplementing the figures with actual numerical examples from 3x3 tables. I have done this, indeed using examples close to those suggested by the reviewer, adding the calculations for the consensus and tightness. This has been added to appendix B, along with a formula that will allow the reader to recreate the numbers, as suggested by reviewer A.

Relating power structure to domain position

Authority

In particular, Reviewer A urged strengthening the explication of the main theoretical claims. Regarding the first claim (pertaining to authority), Reviewer A pointed to weaknesses in the argument, urged me not to willfully ignore cases where people adjudicate beliefs based on their own resources, and suggested some classes of examples that were worth considering.

Pondering these points led to a recasting of this derivation. I had, as I think this reviewer uncovered, confused the issue of adjudication with the issue of implication. An individual may adjudicate between two contradictory beliefs according to some empirical test—as Reviewer A indicated, this is far from implausible as an everyday occurrence. But what is essential for the production of tightness is that a person understand that the beliefs are contradictory (or, conversely, that one implies the other). It is the construction of this relation of positive or negative implication—that one belief can be "authoritative" for the believer—that is at issue.

Such implications basically fall under the category of synthetic *a prioris*, to use Kant's terminology; that is, we are connecting one concept to another **before** making empirical inquiry. Kant argued that the such synthetic *a prioris* could not be proved on the basis of logic though certain ones *had* to be true, and concluded that these must form the universal conditions for all thought. Durkheim tried to come up with a sociological explanation for these *a prioris*, arguing that such fundamental concepts could only acquire their authoritativeness from some social authority. I suggest generalizing this to the thesis that relations of implication only acquire their authoritativeness from some social authority.

The example suggested by the reviewer of the case of expert witnesses exemplifies the relation between tight belief systems and cognitive authorities—cultural anthropologists would never rely on a single informant to give a portrait of the folk beliefs of some society in the way we would allow a single chemist to pronounce judgement on the DNA "fingerprint" of some hair. It seems reasonable to suppose that the difference is that the latter belief system is "tighter" than the former. Despite the nice fit with my claims, I have not actually relied on empirical work on expert witnesses, for the reason that the claims being made in this paper are general, while relying on this analogy might imply a restricted domain of applicability to cases in which knowledge production is carried out by roughly equally matched adversaries.*

But more importantly, I realized that the claim that experts were needed to *validate* beliefs was not necessary to support my argument that experts are needed to *connect* beliefs. Eliminating this assumption made this whole section simpler and more plausible. In general, I here try less to axiomatically derive my claims than to (1) begin with empirical literature that casts doubt on the ubiquity of logical solutions to the questions of how two beliefs are connected, (2) propose authority as an alternative by generalizing Durkheim's response to Kant, and then (3) go on to empirically test this proposed hypothesis.

Clarity of Power

Reviewer A found that I had collapsed the **clarity** of the power structure with its **fixity**. Like other researchers examining small groups (some cited in the paper), I made the equation

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* Interestingly, it is in civil cases that the use of expert witnesses best approaches the model of competing cognitive authorities, since the amount of money in question tends to make it rational for both sides to hire real experts—an NSF study finds that around 86% of civil trials use such expert witnesses, with actually an average of *four* per trial. But in criminal cases—even capital ones, despite a Supreme Court ruling that indigent defendants have a right to investigative experts if the defense demonstrates their necessity—there tends to be an overwhelming imbalance in the use of experts, with defendants having none, fewer, or less qualified [local] "experts."

certainty=objective social fact. In other words, the clarity or certainty that the power relations go in a certain direction leads these relations to seem to be an objective social fact; I then went beyond this and concluded that this social objectivity translated to a sense of fixity. I believe that this is indeed the case, but the reviewer has pointed to cases in which there can be a clear power structure that allows for movement (although non-arbitrary movement) such as promotion. Examples are formal organizations such as the military.

It seems that for the case of **informal** groups, the equation clarity=fixity is reasonable. I have now made this important clarification that what is essential is really the sense of fixity, and in the cases in which there is a combination of high mobility and high clarity (more likely in formal than in informal groups), we would **not** expect the homogenizing of beliefs. Further, I have linked this to the fundamental Durkheimian idea that what is crucial is the linkage between constraint in the social sphere and constraint in beliefs.

Some other points of clarification regarding the relationship between power and consensus were made as asked for by this reviewer:

- Reviewer A asked about the relation between clarity and stability. Stability can be understood in two senses: according to one, the structure may be stable if it has a long lifespan, even if this entails changes in internal organization; according to the other, the structure is stable only if it does not undergo internal reorganization. I have made it clear in the text that I mean only the second of these (indeed, there is good reason to think that clarity of power relations **decreases** the lifespan of these groups).

- Reviewer A saw the discussion on old page 16 as implying that the power in question was not legitimate, when I had intended only that it was not **necessarily** legitimate; I have clarified this as asked by this reviewer in a note.

- Reviewer A suggested using the French and Raven notion of authority being a base of power unless the current discussion is fleshed out. Here (and only here) I stick with the original formulation, and hence want to make clear in this memo why I think this is preferable, even though I agree with this reviewer that for the case at hand, the French and Raven definition has no drawbacks.

  As formulated, the passage in question indicates while authority may be a *base* of power, it is also possible to consider authority a *subset* of the more general case of power *without* its being the base of power. This leaves open the possibility that in the eyes of an outside observer, A may have power over B for reasons that might fall into one of French and Raven's other categories (e.g. coercive power); if B considers this power legitimate, however, we might reasonably call this power A's **authority** over B. For any specific interaction in which A influences B, we might attribute B's compliance to this sense of authority and hence call the authority a "base" of A's power. However, many researchers would argue that it is putting the cart before the horse to claim that in general, A's power **comes from** this authority. (An example might be patron-client systems linked to landlord-tenant relations. A client may consider the relation legitimate, but the patron would have power simply by virtue of owning the land, even if the client did not accept the legitimacy of the patron's position.)

Now for the case at hand, it would seem reasonable to consider authority as the base of the relation of power between the commune leader and the followers, for other forms of power that the leader has stem from this initial authority. But since the discussion at this point is general, and I have found readers to be sensitive to the precise formulation of the terms at this juncture, I wish not to foreclose the case of authority as legitimated power formed on other bases.

- Finally, Reviewer A later called for the discussion of the effects of clarity of power on consensus to be expanded. I have somewhat extended the treatment with the distinction between fixity and clarity and the emphasis on the connection to the Durkheimian idea of social constraint, but otherwise have made few changes, for the simple reason that unlike the argument regarding the relationship between authority and tightness, my hypothesis regarding the connection between clarity and consensus is not derivable from even exaggerated and simplified first principles, but is instead an attempt to parameterize imperfectly understood aspects of lived experience. While I hope in the future to have a more detailed account of this process, the hypothesis now must remain (as acknowledged in the text) incompletely specified.

Overall theoretical claims

I recognize that this still may not be quite as powerful a derivation of the claims as reviewer A would have hoped, particularly regarding the way in which clarity of power homogenizes beliefs. I believe, however, that a more convincing *a priori* justification of their correctness is unlikely to be possible, and that acceptance of these arguments must, for better or worse, rely on the data analysis. While I hope that the introductory discussion can make these hypotheses *plausible*, I cannot demonstrate that they are correct on theoretical grounds alone.

Finally, Reviewer A feels that "the reader is left wondering about the independence of these two effects." In this draft, I explicitly note that I make no hypotheses regarding the effect of authority on consensus, nor regarding the effect of clarity of power on tightness. Instead, after examining each hypothesis, as suggested by this reviewer (and discussed below), I then examine the relation in the two-dimensional analytic belief space to determine whether the hypothesized relation is either spurious or open to a different interpretation.

Empirical Results

General Presentation and Organization

Reviewer A suggested re-organizing the presentation of results so as to first test the two main hypotheses, and then to examine the position of the groups in the two-dimensional analytic belief space…if indeed there was a compelling reason to do so. I have both followed this organizational suggestion and made clear the reason for examining the distribution of groups across both dimensions of the analytic belief space. This exploration answers the very reasonable objection, "perhaps the high tightness in groups with cognitive authorities is all due to the acceptance of some orthodox set of beliefs." That is, perhaps there is really no tightening in the sense we would understand, which would imply constrained disagreement, because there is really very little disagreement in the first place. It is therefore most illuminating to find that groups with cognitive authorities have **low** consensus (but high tightness). I briefly make the

parallel point about groups with a high clarity of power, namely that it can homogenize, but not organize, beliefs. This section has also been reduced.

Tightness
Reviewer A asked for a regression demonstration that commune heterogeneity did not lie behind the findings regarding presence of an absentee leader and tightness. This has been done, exactly in the manner suggested by the reviewer; additional interaction coefficients support the point made in the previous draft that the relationship between authority and tightness is the same in all groups. Reviewer A also asked for totals for Table 1, which have been added.

Consensus
Reviewer A suggested recasting the results here as models similar in form to Tables 1 and 2, and adding controls for commune heterogeneity. I have done this, making the analyses roughly parallel. While the discussion of the relation between consensus and clarity in the political groups is still carried out via correlations in the text (since there are only 5 cases, no controls will be added), the other analyses are in the form of regressions which demonstrate that controlling for commune heterogeneity, and for tightness, does not lead to any important changes in the results.

Selectivity
Reviewer A suggests a more general discussion of selectivity, including the general "floor" level of consensus. Since the hypothesis being tested does not have to do with the absolute level of consensus, but with patterns of variation in the consensus, any selectivity according to beliefs that is not related to a corresponding selectivity in terms of power arrangements will not, if I am not very much mistaken, affect the results discussed. However, to the extent that there is a pre-existing high level of consensus due to selectivity, as proposed by the researcher, this makes for a relatively conservative test of the hypothesis that clear power arrangements homogenize beliefs (because there are fewer potential disagreements to convert into agreements). I now note this in the section introducing the data set.

Cross-Domain Results
Reviewer A suggests that the section discussing the results of the cross-domain tightness replication downplayed the effect of authority. Agreeing, I have eliminated the section that assumed the hypothesis to be sufficiently weakened so as to require a possible alternative explanation (one involving resident leaders). Instead of focusing on this possible effect of resident leadership (there really aren't enough cases), I focus on the stronger evidence in support of the domain-specificity of authority. Since this discussion of the effect of resident leaders has been eliminated, other reasonable suggestions made by reviewer A, such as to be clearer regarding the interpretations and to examine outliers, are no longer relevant.

Minor details
In addition, reviewer A made 23 more minor comments not discussed above, some correcting typographical mistakes, some suggesting rewording, and some calling for clarification. Of these, 4 call for some explanation of the correction made.

- Reviewer A correctly took me to task for calling the dimensions of the belief space 'independent,' when I meant that the dimensions were *potentially* independent, in contrast to other methods which force a negative correlation between the two dimensions. This has been corrected in the text.

- Reviewer A suggests having the paragraph on sampling formal properties of belief system on deck for cutting; it remains in but under the sword of Damocles.

- Reviewer A asked for the citation to the paper discussing forms of organization; this has been added but as it is identifying in nature, it is only in the form of Author 2000.

- Reviewer A asked for a specification of those groups that have high tightness without authorities. By this, I only meant to point to the imperfect association between tightness and the presence of authority (and not to introduce a new finding regarding other paths to tightness). I have rewritten the text slightly to make this clear.

All the other 19 of these have been amended as suggested by this reviewer, or refer to sections now eliminated.

IDENTIFYING REFERENCES LISTED AS "AUTHOR" IN TEXT

Martin, John Levi. 1994. "Descriptions Through Loglinear Modeling of Belief System Constraint as Homogeneity, Unidimensionality, and Association." Paper presented at the Annual Meeting of the American Sociological Association, August 5-9, Los Angeles.

_____. 1997. *Power Structure and Belief Structure in 40 American Communes*. PhD Dissertation, University of California, Berkeley.

_____. 1998b. "Authoritative Knowledge and Heteronomy in Classical Sociological Theory." *Sociological Theory* 16:99-131.

_____. 2000. "The Relation of Aggregate Statistics on Belief to Culture and Cognition." *Poetics* 28: 5-20.

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Editor: Andrew Abbott • Managing Editor: Susan Allan

5835 S. Kimbark Ave.
Chicago, IL 60637

tel 773 702-8580
fax 773 702-6207
ajs@src.uchicago.edu

October 25, 2001

Professor John Levi Martin
Department of Sociology
Rutgers University
54 Joyce Kilmer Avenue
Piscataway, NJ 088854-8045 Fax: 732-445-0974

Dear Professor Martin:

We are glad to report that "Power, Authority, and the Constraint of Belief Systems" has been scheduled for publication in the January 2001 (107:4) issue of *AJS*. We will send you the copyedited manuscript for inspection and approval before we begin working on the typeset version.

You will be asked to return the page proofs within about five days. Therefore, to avoid delays, please let us know promptly of any address changes you foresee between now and the publication date. The proofs will be made available to you via a special web site, and you will be able to print them as .pdf files. Most authors have found this an agreeable and convenient way to manage the publication process.

Please complete the publication agreement and return the white copy to us, along with a brief biographical note. I enclose an envelope for your convenience.

Sincerely,

Susan Allan