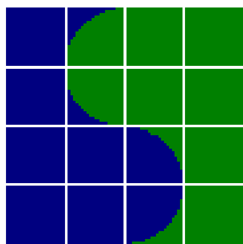
Newsletter of the Social Psychology Section of the American Sociological Association



Social Psychology

Spring 2002

SECTION OFFICERS

Chair
Phillip Bonacich
UCLA
Bonacich@soc.ucla.edu

Chair-Elect
Carmi Schooler
NIMH
schooec@irp.nimh.nih.gov

Secretary-Treasurer
Karen A. Hegtvedt (2004)
Emory University
khegtve@emory.edu

Council

Spencer Cahill (2004)
University of South Florida
scahill@luna.cas.usf.edu

Kristen A. Marcussen
(student member)
University of Iowa
Kristen-Marcussen@uiowa.edu

Terri L. Orbuch (2002)
Oakland University
orbuch@umich.edu

Timothy J. Owens (2002)
Purdue University
towens@purdue.edu

Jane Sell (2003)
Texas A&M University
j-sell@tamu.edu

David Snow (2004)
University of California-Irvine
dsnow@uci.edu

Jan E. Stets (2003)
Washington State University
stets@wsu.edu

SPQ Editor
Cecilia Ridgeway
Stanford University
ridgeway@leland.stanford.edu

Newsletter Editor
Jane Sell
Texas A&M University
j-sell@tamu.edu

CHAIR'S REMARKS

Phillip Bonacich
UCLA
Bonacich@soc.ucla.edu

I first want to thank all of you who have been working on the social psychology section committees. You will notice that the committees and their members are listed at the bottom of this page. We have a remarkable slate of candidates for next year's positions, and they are also listed. (See page 2.) Additionally, the University of Iowa folks are organizing the group process meetings for August and they have informed me that the meeting will be held on Thursday, August 15, the day before the ASA meetings in Chicago.

I have just attended the Annual International Sunbelt Social Network Meetings this year in New Orleans. Many social psychologists have presented papers and been active at these meetings. This year, there are sessions on social support, deviant and criminal networks, networks and identity, networks and culture, and networks and attitudes that should be of interest to social psychologists. The Sunbelt meetings are delightfully interdisciplinary and international. I encourage you to submit papers and attend next year's meeting.

2001-2002 Social Psychology Section Committees:

Nominations
Jan Stets, chair, stets@wsu.edu, Marta Elliot, Noah Friedkin, Gretchen Peterson, Teresa Tsushima

Professional Affairs
Geoffrey Tootell,chair, gtootell@email.sjsu.edu, Jane Piliavin, Alison Bianche

Graduate Student Affairs
Scott Feld,chair, sfeld@lsu.edu, Linda Francis, Jeffrey Houser, Shirley Keeton, James Moody, Blane DaSilva

Cooley-Mead Award
David Willer, chair, willer@gwm.sc.edu, Karen Cook, Jeremy Freese, Robert K. Shelly, Shane Thye

Membership
Matt Hunt, chair, mohunt@lynx.neu.edu , Kathleen Crittenden
Anna LoMascolo, Lisa Rashotte, Shane Thye

EDITOR'S COLUMN

Jane Sell
Texas A&M University
j-sell@tamu.edu

In this issue of the newsletter, Louis Gray discusses the cost equalization research that he and his colleagues have been actively engaged in for many years. The cost-equalization approach suggests an interesting dynamic that implicates actors' framing of alternatives and their experience. For the teaching column, Michael Lovaglia discusses his use of student journals to focus students upon the self as both subject and object. The use of journal and Point of View encourages students to talk about issues that are important to them personally. Our student profile features Anna Johansson from Texas A&M University. She details how she combines research from sociology, organizational psychology and management to address processes of legitimation.

We feature two articles on ethics and research. Continuing our discussion of internal review board and the federal guidelines for human subjects, Karen Hegtvedt provides a history of her (considerable) experience at Emory. Her column is an interesting documentation of the relatively rapid changes that various universities have undergone. She argues that the new era of increased surveillance results from increased federal money in research, egregious failures to protect study participants, and institutional disregard of federal rules and monitoring. We have had three different people address the issue of IRB's and human subjects monitoring, and I am struck by how different the institutions and their responses are. Here at Texas A&M we have gone through many phases to sort out different issues/problems. So, for example, last year, all undergraduate practice "research" done in classes had to go through IRB review. As you might expect, this increased (exponentially), the work of all those associated with the research and the review of research. This year, there has been another change, and instructors of undergraduate research classes only have to send summary information to the IRB.

Finally, I asked Lynn Smith-Lovin to address some questions raised in Marcel C. LaFollette's book, Stealing Into Print: Fraud, Plagiarism, and Misconduct in Scientific Publishing. This book addresses ethical issues, some that I had never seriously considered. The fact that I was unaware of some of these problems/controversies made me think that I could do a much better job as a researcher and mentor if I did examine them. So I asked Lynn to consider problems and potential solutions. For example, LaFollette mentions that concern about fraud has resulted in some journals hiring "outside consultants." JAMA, for example, employs statisticians who supplement reviewers' comments. Peer review processes are not usually (maybe never) designed to determine authenticity. Should they be?

Apparently, there are no rules that generally cover peer review, and there are very different norms in different disciplines. So, I also asked Lynn to address the general area of conflict of interest, or conflict among perspectives and how such conflict affects reviewer choice. LaFollette also mentions that referees obviously receive manuscripts that are not yet published and there are quite a few examples of such referees taking material or delaying publication. What can be done to prevent such misconduct? (One suggestion is to make reviewers identities known.) Lynn's experience seems to make her less alarmist than I was after reading the book; for that I am relieved!

In our next newsletter, we will feature the ASA meetings to be held in Chicago. Remember that this summer newsletter will be electronic. If you should want a hard copy, feel free to contact me directly.

SECTION MEMBERSHIP

Mathew O. Hunt
Northeastern University
mohunt@lynx.neu.e

Thus far, the Membership Committee has undertaken two primary efforts to sustain, and hopefully expand, section membership. First, in the Fall, we contacted all current section members and encouraged them to renew their membership, and to reach out to interested colleagues and graduate students in an effort to bring new members into the section. This wave of recruitment included a suggestion that current members "sponsor" interested graduate students by covering their $5 membership fee. Second, in January, we contacted all current ASA members who have expressed an interest in social psychology (on their applications), but who are not currently section members. Our letter to these persons encouraged them to join the section, and emphasized the benefits of section membership and the fact that the number of ASA sessions devoted so social psychology is driven by membership counts. Current plans for our final major recruitment effort involve contacting 20001 ASA members who either (1) renewed their ASA membership for 2002 and let their social psychology section membership lapse, or, (2) have not yet renewed their ASA or section memberships for 2002.

2002 Election Slate

For chair-elect (vote for 1)
Guillermina Jasso, New York University
John Skvoretz, University of South Carolina

For Council (vote for 2)
Peter Callero, Western Oregon University
Cathryn Johnson, Emory University
Noah Mark, Stanford University
Dawn Robinson, University of Iowa

Graduate Student Representative (vote for 1)
Blane DaSilva, University of South Carolina
Mark Konty, University of Arizona

Ethics in Journal Reviewing: General Strategies from a Former Editor

Lynn Smith-Lovin, University of Arizona, *smithlov@U.Arizona.edu*

Before addressing any of the issues raised by LaFollette's book, I want to clarify that the responses that I give here don't reflect any specific policies that Linda Molm and I used when we co-edited *Social Psychology Quarterly*, or that I am using now as Associate Editor at *Self & Identity*. In most cases the responses indicate my own personal opinions; in a few instances, they are rough guidelines that Linda and I evolved through conversations about specific problems that came up during our editorship. (She might well interpret or apply principles differently.)

LaFollette mentions a number of concerns about peer reviews: that they often are ineffective at revealing fraud, that they might involve conflicts of interest, that reviewers might misuse material to which they gain access through the review process, and that reviewers might not be neutral judges of the material (e.g., when there is competition or hostility between researchers). She mentions some mechanisms as potential solutions– hiring outside consultants for some manuscripts, having randomly selected authors authenticate their results by submitting data, and having reviewers' identities known to the authors.

First, I would say that many of the problems that LaFollette mentions seem (to me at least) quite rare in the social sciences. While there have been some famous cases of fraud or questionable reporting of data analysis, my sense is that these are very much the exception rather than a consistent problem. While reviewers may not be able to catch all cases of questionable practice, I think they are generally quite good at raising such questions. The skilled practitioners in an area are sensitive to what procedures, data patterns and interpretations are likely to be valid within a given paradigm of research. When something looks odd to them, they call it into question, and often request revisions that explore the pattern further. It's hard for me to imagine that a consultant could do any better. When there's an esoteric statistical or methodological problem to assess, I might choose an expert reviewer who doesn't know much about the rest of the content, and ask him or her to concentrate primarily on that specific domain in his or her review. The ASA Publications Committee discussed having authors archive their data after publication a number of years ago (to allow more general checking and replication). It abandoned the idea after considering its complexities: it places an enormous burden on both authors and the archiving organization. Even NSF's requirement for data sharing after a grant is difficult to enforce.

Problems involving conflict of interest, competition among research programs and the inappropriate diffusion of knowledge through the review process are much more common. These are things that we all have to deal with every day in our professional lives. As an editor, I tried to avoid assigning reviewers with conflicts if possible; if I didn't know about a conflict, I expected authors to tell me about possible conflicts (to avoid even the appearance of bias) and to refuse the review if they felt they could not be fair. I was often happy to get a review from someone who declared an appearance of conflict: it often contained useful information, and I could interpret it in the light of the connection that the reviewer had told me about. In the case of competition or hostility, the case is a little different. In the situation where a paper criticizes another research program, I usually wanted to know how those who were criticized would interpret the critique. I would, of course, interpret the review from the opposing side with that fact in mind; it certainly didn't have a veto on publication. In the case of persistent, outright hostility, one usually wouldn't solicit a review from the "other side," primarily because you already knew what it would say. In the case where you *know* that someone is going to hate paper X for reason Y, there's no point in wasting the reviewer's time...you already know the information that they will provide.

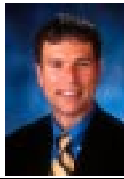
I think the biggest ethical problem is actually the last issue mentioned here: the illegitimate use of information that we gain through reading prepublication papers and grant proposals. It is effectively impossible to cleanse one's mind of information gleamed from reading papers under review. Using this material to forward your own work is natural. But when it is (a) unacknowledged, or (b) results in incorrect credit for the insights, it's a real problem. I know of at least two cases (neither involving *SPQ*) where someone read a prepublication paper, and effectively published the same thing themselves first. I know of several other cases where grant proposals which were not funded (and therefore not in the public domain) were discussed and their ideas disseminated against explicit peer-review norms. A more subtle case is one in which we use a paper that we reviewed to help us supplement a literature search or a general theoretical framing, without acknowledging the contribution that the paper has made to our thinking. Sometimes reviewers write editors to ask if the author will agree to be contacted (so that permission to cite can be requested). But it's a real problem, and one that we need to be careful about. I personally don't think that making authors known to reviewers would be a good idea. It would provide a minor check on the illegitimate use of information, but would add many more problems about interpersonal concerns. Psychology and several other disciplines do it as a matter of course, however, so it's a change that we could make.

A final issue that LaFollette raises is whether or not every paper has a "right" to a peer review. At ASA journals, we generally err on the side of more peer reviewing. At SPQ, we only rejected without review papers that lacked the basic form of an academic article (e.g., references). This problem is getting bigger as journal web pages attract more inappropriate submissions. In general, I think that many more papers should be rejected without peer review– it would save a great deal of reviewer and office staff time. Privately published journals often use more discretion, and it doesn't hurt their inclusiveness. I can't remember a single case where a paper with which I *wished* I didn't have to waste reviewers' time ever came back with even one non-reject review. Some papers are clearly outside the acceptable range, even based on a quick scan. But it's probably better to be safe than sorry. As long as the wonderful reviewers with which we worked were willing to provide their service to the discipline, we took the conservative path. I hope that one day sociology will generate so much research that we'll have to take a more hard-hearted approach. But for now, those reject reviews probably provide some useful feedback for authors who aren't quite getting what it takes to write a publishable paper.

In summary, I guess I'd say that the system works much better than LaFollette's issues would make one think. There are a few rough edges, mostly in areas where norms are applied behind the scenes (e.g., use of information from reviewed pieces, data cleaning, overlapping publications from the same data). But the problems are subtle, and probably don't affect the overall operation of the system much. I think this is mostly a system that's not broken, and doesn't need fixing.

Teaching Corner

Michael Lovaglia
University of Iowa
lovaglia@blue.weeg.uiowa.edu



Asking students to keep a journal in conjunction with the study of symbolic interaction focuses their observation and writing in ways that are immediately relevant to them. Encouraging students to keep a personal journal is a traditional pedagogical tool to develop clear writing and detailed observation. Many students, however, have difficulty starting a journal, floundering about what to write. Journal entries focused through the lens of symbolic interaction solve that problem and bring abstract sociological theory to bear on students' personal lives.

The connection between symbolic interaction and personal growth starts with the ability to take another person's point of view. When I began teaching, a passage in *Social Psychology* by Michener, DeLamater and Schwartz (1990) provided the link. They suggest that socially competent individuals "learn to see themselves and their own actions as if through others' eyes" (p. 87), in part by using symbols for inner thought. With the concept of reflexive behavior and the self as both the source and object of behavior, self-directed personal growth becomes possible. Writing can both spur and record that growth.

Introducing my students to keeping a journal starts with a Point of View (POV) Assignment in two parts. Students first write a page or two about a personal problem, dilemma, or confusing social situation that they would like to resolve. I encourage as much detail as possible, details being the data on which to build understanding. Students choose problems that range from everyday roommate hassles to serious conflicts over a friend's life-threatening drug abuse. Because they write about a social situation important to them that they have experienced, they usually describe the situation well, but almost invariably from the writer's POV only. I assign the second part of the POV assignment when students receive my encouraging comments (but no grade) on the first part. The second part of the POV assignment asks students to describe the same situation from the other person's POV. They try to imagine themselves as the other person. What is that person thinking and feeling? What might the student's speech and behavior have meant to the other person? The analogy to a screenplay can be useful. A story told from one person's POV can be quite different from a story of the same incident told from another person's POV. A few students are adept at taking the role of another, but most struggle. It takes practice.

Keeping a journal allows students to practice taking the role of another. They take a few minutes every day to think about a social encounter, describing in detail what was said and what happened. They then practice describing how the other person might have seen the same social encounter. They can try different explanations for the other person's behavior, and then use their detailed observations to test alternative theories. With practice, social situations that had seemed baffling become not only explicable but also manageable. In a few weeks, students can look back through their journal to see their social competence grow.

Confidentiality and feedback are important issues in a personal journal assignment because the subject matter is important to students' lives. I promise not to talk with other students and faculty about students' personal writing and encourage them to use loose-leaf paper to write dated journal entries. That way, students can submit their work but easily withhold any entry they would rather keep private. A journal kept in a computer file is also easily edited for submission. Perhaps the best part for me is feeling that I have really gotten to know my students. Moreover, from my comments, students feel that they have gotten to know me and that we share in a common endeavor, the construction of a social reality with the potential to benefit everyone.

Graduate Student Profile

Anna Johansson
Texas A&M University
a-johansson@tamu.edu

Anna Johansson just received her Ph.D. at Texas A&M University. Anna came to sociology from a business background. Her undergraduate degree is in business analysis and she spent 5 years working in sales and management before returning to graduate school. It was in the business world that she developed an interest in group behavior and social psychology. It was a world that could have benefited from a sociological understanding of status, hierarchy, legitimacy and incentive structures. As part of her curriculum she crossed disciplines to supplement her coursework with courses from psychology and management. In fact, it was the case studies such as the Challenger disaster, and the Air Florida crash into the Potomac that she encountered in her organizational psychology classes that inspired her to search for sociological explanations in group decision-making.

While she has worked most closely with Jane Sell and participated in research in cooperation and social dilemmas, her own research interests have focused on processes of legitimation. She has sought to build on existing theories and extend knowledge of the multilevel process by comparing the relative strength of sources of legitimation and their impact on group decision-making, specifically group routines. She obviously draws heavily on structural social psychology but also crosses into her other area, complex organizations, to address the concepts of *legitimacy* and *routines* and to challenge theories of decision-making in organizational theory. (She is grateful to NSF for funding her research in this area.) She enjoys the intellectual challenge of theory construction but keeps an eye toward application.

Statement: When I left my corporate job to begin graduate school in sociology, I had to take an undergraduate theory course since I did not have a sociology degree. One of the first books I read in sociology was Randall Collins *Sociological Insight: An Introduction to Non-obvious Sociology.* I couldn't believe what I was experiencing. The social world that I had once struggled to understand became so clear. For me, the decision to find an applied job after finishing my degree was not a difficult one. My challenge will be to show decision-makers that not only is sociological theory not esoteric, but that it is highly useful. My thesis advisor, Allen Haney at the University of Houston, used to call it "doing sociology." I am going to "do sociology" and hopefully do some good.

Theory and Research Column

Louis N. Gray
Washington State University
grayln@earthlink.net

Cost Equalization: Choice and Interaction

Many approaches have been used to address questions about human choice, both individually and within interactive settings. While there are many differences in their detail and mathematical expression, a large number of them have a common feature: equalization, at asymptote, of the costs incurred through behavioral allocation. Thus, for any pair of alternatives 1 and 2 in a choice situation, asymptotic choice proportions should conform to the equation

$$b_1 \text{ (costs} \mid 1 \text{ chosen)} = b_2 \text{ (costs} \mid 2 \text{ chosen)},$$

where b_1 and b_2 represent the proportion of trials on which alternative 1 or alternative 2 is chosen. Instead of either maximizing rewards or minimizing costs, actors are expected to choose so as to equalize or balance the costs they experience. The proportional allocation of behaviors is such that the difference between the costs associated with the behaviors approaches zero. The marginal costs are approximately the same for each alternative.

While costs are most often thought of as failure to receive reward (negative punishment), they can also include the energy expended by engaging in a behavior or positive punishment subsequent to a behavior. Like rewards, costs can also emerge from the frame an actor brings to a choice situation. An actor's framing can result in an observable consequence of behavior functioning as a reward, a cost, or a neutral event (Tversky and Kahneman 1981).

One of the advantages of the cost-equalization approach is that it suggests a dynamic: The alternative chosen at a specific choice point will be that which, in terms of the current state of the actor, has been experientially the least costly. If it is possible to identify an actor's framing or control it through experimental manipulation, then relatively simple dynamic models of choice can be generated that accurately describe behavior.

While simple choice situations can be understood as cost-equalizations, application to interactive settings is more challenging. For example, interaction in task-oriented groups should, according to Bales (1955), require behavioral choices in the socioemotional (expressive) area of activity as well as in the task (instrumental) area. Behavioral choices that advance the task-area performance of a group may entail costs in the socioemotional area that produce tensions and inhibit continued group functioning. In order for groups to endure, mechanisms are required that simultaneously adjust behaviors in both areas and for all participants.

One implication of this viewpoint is that interaction in social systems may involve simultaneous equalization processes across different kinds of costs and different dimensions of group behavior. Since each kind of cost and each dimension of behavior is likely to have an effect on other kinds of costs and other dimensions of behavior, the cost-equalization equations are likely to be interactive and resist closed-form solutions. It is also possible that as social systems get larger, the equations have no unique solution sets and/or behave chaotically.

While computer simulations seem a convenient way to investigate systems of this sort, there are some empirical difficulties that need to be addressed if we are to apply our results to real social systems.

The most important of these is the identification of costs themselves and the frames that actors employ. Though we often assume that actors in small groups share perceptions and evaluations of the events that occur, that need not be the case. To the extent that actors employ different frames, predictions regarding behavior can be expected to vary and, of course, actors' experiences may influence their frames.

In addition to examining the simultaneous equalization of behavioral dimensions, then, we also need to develop a theoretical approach that incorporates frames and the processes that influence adoption of a frame. Since the adoption of a frame in complicated social settings is likely to involve both behavioral experiences and cognitive processing, greater theoretical integration across these areas is also required.

My feeling is that something akin to cost-equalization can be extended in these directions. For example, what costs are entailed in the adoption of alternative frames for a given social situation? If it is assumed that a cost-equalization can be used to predict the relative frequency of the employment of alternative frames, and if the alternative frames produce different behavioral predictions, then it should be possible test hypotheses regarding frame adoption. If two alternative frames produce different levels of cost there should be a move in the direction of equalizing those costs, if that is possible.

The cost-equalization approach extends well beyond applications to individual choice. Its application to actors in interaction situations involving multiple behavioral dimensions and alternative frames of the situation provides an avenue of approach that may prove fruitful. At the very least it forces us to incorporate known principles of individual choice into the kinds of social situations that are of interest to social psychologists.

Bales, Robert F. 1955. "The Equilibrium Problem in Small Groups." Pp. 449-490 in *Small Groups: Studies in Social Interaction*, edited by A. P. Hare, E. F. Borgatta, and R. F. Bales. New York: Alfred A. Knopf.

Tversky, A. and D. Kahneman. 1981. "The Framing of Decisions and the Psychology of Choice." *Science* 211:453-458.

Group Process Meetings!
Thursday, August 15th
Chicago, Illinois

IRB: Formulations and Reformulations

Karen A. Hegtvedt
Emory University
khegtve@emory.edu

Nearly 14 years ago I attended my first meeting. A group of about six faculty members, along with the associate dean of the graduate school,met in a comfortably appointed conference room in the administration building. The dean removed her watch and called the meeting to order. After about 20 minutes of friendly exchange, we concluded that our colleagues applying for federal money to support research involving children were doing what they could to inform and protect their research participants. The dean alerted us that we would meet again next semester, if necessary, and then dismissed us. Such concise, convivial, and infrequent meetings characterized my experience during the first ten years of my service on the Arts and Sciences Institutional Review Board (IRB). I liked the committee assignment — we actually got something done in our sessions — and thus did not mind my enduring tenure (I was still one of the more junior members of the committee during those years). Little did I know that my committee assignment carried an auspicious burden: ensuring fulfillment of the abstract values of respect, beneficence, and justice in research. Indeed, these are the ethical principles put forth as the basis for federal regulations pertaining to the protection of human research subjects and first published in the 1979 "Belmont Report," produced by the National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research. I first learned about the Belmont report about four years ago when our university brought in a consultant to train Arts and Sciences IRB members to deal with a variety of issues, ranging from bureaucratic procedures to meet federal regulations to more conceptual concerns with definitions of research and human subjects. We learned from the specialist that, while our committee had not been doing anything wrong on what we did review, we simply had not been reviewing enough. Non-federally funded research, unfunded research, even that which posed no obvious risks nor involved no vulnerable population, fell under our auspices. Thus began our mission to educate our colleagues on the now more inclusive purview of our review process and to create a more obvious paper trail of what we did review. In doing so, we satisfied federal regulations and found our committee undergoing institutional isomorphism — we began to recommend phraseology in consent forms much like those demanded by our medical school IRB. Such isomorphism assured our medical school colleagues and deans that arts and sciences research was in compliance.

Our brief, infrequent meetings have become memories. The amiable atmosphere remains (and we serve lunch), but the meetings are longer and (gasp) monthly—even during the summer. And I am no longer the junior member of the committee. While I have not served as many years as my colleagues from Political Science and Psychology, I have the dubious honor of chairing the committee now. In the two years since I assumed the chair, Emory University has altered the administrative procedures affecting the functioning of the Arts and Sciences IRB and its purview. These recent changes have stimulated presentations to a variety of different constituents. As a consequence, I have had the opportunity to muse repeatedly, both privately and publically—on the emergence of institutional change with regard to the protection of human subjects (which we now call "human research participants" to ensure political correctness). So, why have these changes emerged, not only at my university but elsewhere as well?

As documented in Protecting Study Volunteers in Research by Dunn and Chadwick (1999), a number of past incidents raised concerns with the treatment of humans in research, e.g., radiation experiments, the Tuskegee study of untreated syphilis in African American males, Milgram's study of obedience to authority. Such studies gave rise to regulations focusing on the establishment of Institutional Review Boards. They are not, however, responsible for the increased surveillance procedures common in universities now. From what I can discern, three interrelated reasons underlie the new era: increased federal dollars to research, egregious failures to protect human research participants (e.g., deaths), and institutional disregard of requests made by federal monitors. Institutional receipt of federal funding obligates the recipient to comply with federal regulations as well as to allow federal agencies to monitor the use and spending of those funds. IRB records may be subject to auditing by federal authorities for the purpose of determining whether standards of informed consent and protection of rights and confidentiality have been met. Failure to conform to standards may result in the suspension of federal funding. Egregious failures to protect research participants are an obvious form of noncompliance. The recent research-related death of a participant in a clinical trail at Johns Hopkins University accentuates both faults in the review process and the principal investigator's dereliction of responsibility in the employment of only approved procedures. Like in many realms of social behavior, when something terrible happens, authorities try to create rules to prevent future negative events (think about new airport security procedures). Moreover, federal authorities expect compliance. Disagreement coupled with arrogance only fuels the feds' ardor in tracking down noncompliance.

To avoid audits, or at least to ensure that, if audited, no incidents of noncompliance will emerge, universities have become more rigorous in their review of research involving human participants. The code of federal regulations (Title 45, part 46 on the protection of human subjects) sets minimal guidelines for review. CFR 46.101.b actually exempts from the policy very low risk research (i. e., risks no greater than encountered in ordinary life) in which participants' responses are anonymous and are held confidentially. Thus, technically, many studies conducted by social scientists (and humanists) need not go through IRB review. I understand, however, that the Office of Human Research Protection (OHRP) looks dimly upon institutions that do not have procedures to review even such low risk research. Most universities go beyond the minimal federal regulations. Because the federal authorities allow much discretion in creating review procedures, there is a great deal of variation in intensity of review across campuses.

What we had not been reviewing in the first ten years of my IRB service generally fell into the "exempt" category. Most social science departments had internal committees to review such research, but the Arts and Sciences IRB never saw it. After the consultant's visit with out committee, we asked departments to forward these exempt protocols to the IRB office for filing. About two years ago, new university procedures went into effect requiring the IRB chair to review exempt protocols. In addition, the committee refined the meaning of "exempt" to distinguish between the very low risk research in which respondents can not

See IRB, page 7

IRB (continued from page 6)

be identified and the research in which indirect links to identity may be made owing to written consent or audio/videotaping, and risks, though low, stem from breach of confidentiality rather than physical or emotional harm. The latter falls into the "expedited" review category, which the chair oversees. Research protocols getting full board review remain those that involve vulnerable populations (e.g., children, prisoners, the mentally incapacitated), risks higher than encountered in everyday life, deception, and alteration in the substance of informed consent and/or waiver or written consent and/or waiver of written consent (if the research does not otherwise fall into the exempt category)-regardless of funding source.

So, in the past two years as chair of the Arts and Sciences committee, I have reviewed far more protocols than ever before (which means I write many letters asking for changes or indicating approval). The committee has seen its work load increase because of the review of unfounded or non-federally funded research. And, we have faced the challenge of bringing in the humanists. As a colleague in Art History once said to me, " We didn't think the IRB rules applied to us." Although the federal guidelines are a bit vague in terms of their definition of research ("…systematic investigation…designed to develop or contribute to generalizable knowledge."), they are clear about what constitutes a human subject ("…a living individual about whom an investigator…obtains data through intervention or interaction…"). Thus humanistic research involving interviews with artists, recording the life histories of civil rights activists, or linguistic analyses of taped everyday conversations constitutes collection of data through interaction and falls under the guidelines of the IRB. And, it does not matter who is doing the research—faculty member, under- or graduate student, staff member. All must be reviewed.

Review, however, does not mean that a certain project can not be done or a certain methodology cannot be used. It is the task of the chair and the committee to examine investigator's arguments regarding the benefits of the research in view of the deviations from typical, clear-cut informed consent procedures. For example, in many instances of ethnographic research or research in the developing world, it would be inappropriate to present a participant with a long consent form to read, let alone sign. The principle investigator needs to justify his or her research strategy and to demonstrate his or her awareness of the potential negative consequences of the research and how to protect the participants from such risks. Some funding agencies now require that investigators become "certified," meaning that they successfully pass a test regarding protection of human research participants and the minimization of research risks. Emory, like many institutions, has such a certification procedure (which at this point remains geared to clinical researchers—a touchy issue with social scientists and humanists alike), although as we have learned from experience, passing the test does not guarantee sensitivity to ethical issues in research. But, perhaps it is a start.

In the review process, IRB members assess the extent to which the research methodology protects individuals' rights and minimizes participants' risks. It is beyond our purview to comment on the research question, the nature and the strength of the methodology, the wording of questions or manipulations, etc., except as they affect participants' welfare. (We do, admittedly, catch grammatical and spelling errors!) We ensure respect for the participant by focusing on elements and comprehensibility of informed consent (information on the purpose of the study and its procedures, statements of voluntariness, confidentiality, risks and benefits, and contact persons), beneficence by assessing the risks in view of the benefits, and justice by examining the criteria for inclusion and exclusion in the study as well as undue benefits or burdens for particular populations.

As of this year, the Arts and Sciences IRB is no longer a stand-alone committee. The university has folded together administratively the medical school's IRB and that of the Arts and Sciences (more institutional isomorphism). The new university-wide IRB has five committees, four of which handle invasive, medical research and one of which focuses on the traditional concerns of the Arts and Sciences IRB. Re-christened the Social, Humanist, and Behavioral IRB, we are charged with reviewing research from all units of campus that involve non-invasive, non-medical investigations. On the one hand, our purview has expanded yet again. On the other hand, we must vigorously enforce our autonomy from the medical committees. By doing so we safeguard our position that the best means to protect human research participants requires flexibility; to effect respect, beneficence, and justice in research depends upon the meaning of the research question in a particular context, given a particular methodology.

As I have tried to stress, the IRB exists to uphold important ethical values with regard to the treatment of human research participants. On the practical level, however, what it boils down to is more paperwork. I have even heard some contend that having to go through the review process is a violation of academic freedom. (Technically, is is not such a violation because as an employee of a university receiving federal funding, one must comply with federal regulations; moreover, the right to privacy of the research participant rests in more fundamental laws than does academic freedom, which is a customary feature of institutions of higher education but not a legally codified one.) As one grumbles over completing the paperwork, keep in mind why IRBs have been established and why procedures have grown more stringent of late. And remember, the more IRB applications one does, the easier they are to complete. And the better investigators get at completing them, the easier the job will be for IRB chairs and their members. Perhaps the 20 minute meetings will no longer be just a memory.

Call for Papers

Special Issue of Social Psychology on Race, Racism, and Discrimination
Edited by Lawrence D. Bobo

Scholarly engagement with the "problem of race" has never been more vigorous and theoretically rich. A special opportunity thus exists to extend our current knowledge base in particularly innovative ways and to better systematize and integrate this rapidly proliferating body of scholarship. The past two decades brought forth several fertile lines of research. Within the survey-based literature these developments include the controversy over symbolic racism/racial resentment theory versus group conflict/group position theory versus political ideology and conservatism theories; a more explicit concern with social stratification beliefs and causal accounts of racial and ethnic inequality; the emergence of new theoretical frameworks such as social dominance theory; renewed attention to the contact hypothesis; and the resurgence of interest in contextual analyses. Within the experimental literature these developments include such topics as the implicit, automatic, and unconscious effects of stereotypes; the pervasive influence of affect; and the impact of cultural stereotypes and of status organizing processes on actual performance and achievement outcomes. More qualitative studies filled in major gaps in our knowledge about the micro-processes of discrimination and on how racism emerges and is re-constituted in everyday interaction. In addition, there has been some but not sufficient growth in work based on either multi-racial/multi-ethnic samples; examining both dominant and subordinate group processes; and merging research methodologies or working at different levels of analysis.

It is the distinctive aim of this special issue to highlight work that transcends single methodological traditions. Focused but synthetic theoretical papers are welcome. We are especially interested in new empirical research that combines data on both dominant and subordinated groups or that is comparative in scope. Research that reflects a cross-fertilization of methods is particularly welcome such as combinations of in-depth or qualitative interviews and surveys; survey-based experimentation; media content analysis and attitude outcomes; participant observation/ethnography and discourse analysis; and multi-level or hierarchical modeling approaches.

The deadline for submitting papers is June 15, 2002. The usual ASA requirements for submissions apply (see "Notice to Contributors" in this journal). Please send four copies, the submission fee, and a cover letter indicating submission to the special issue to the regular editor of SPQ, Cecilia Ridgeway. Send one copy of the paper to special issue editor, Lawrence D. Bobo, Department of Sociology, Harvard University, 33 Kirkland Street, William James Hall, Cambridge, MA. 02138. Prospective authors may communicate with the special issue editor about the appropriateness of their papers (bobo@wjh.harvard.edu). Encouragement to submit will not, of course, have any implication for the ultimate acceptance of the paper.

Name:________________________________Address____

____

e-mail__________________________

____ I am an ASA member and want to joint the Social Psychology Section. Enclosed is a check for $12.00 for section dues this year ($5.00 for students). Make checks payable to the **American Sociological Association.**

____ I am not an ASA member but am interested in joining the Section. Please send me information about membership in the ASA.
Mail to: Membership Services, American Sociological Association, 1307 New York Avenue NW, Suite 700 Washington DC 20005-4701