

# Five Proposal for Reforming (Especially Quantitative) Journal Article Publishing Practices in the Sociology of Religion toward Improving the Quality, Value, and Cumulativeness of Our Scholarship <sup>1</sup>

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How might the sociology of religion benefit from adjusting some of its journal article publishing standards and practices? This paper builds on five presuppositions about the purpose and nature of good sociology in order to advance five specific proposals for changes in how we write, review, and make use of journal articles in the field. Those proposals concern the publishing of descriptive empirical research notes, meta-analysis review essays, detailed methodological information, arguments about causation, and analytical emphases on the substantive power of variables rather than mere statistical significance. The purpose and expected result of these proposed publishing adjustments is to increase the quality, value and cumulative nature of the scholarship produced in the sociology of religion.

The long-term trajectory of the field of the sociology of religion, when viewed over the past decades, is, I believe, one of increasing strength, methodological and theoretical sophistication, and the contribution of valuable and interesting knowledge both in academia and public life. At different times, the sociology of religion has perhaps gotten stuck in various intellectual ruts or lost a strong sense of direction (see Smith 2008). But the field's many creative scholars and good graduate students completing new doctorates in the field prevent it from remain anything like stagnant or disoriented for very long.

Part of the gains in quality and amount of scholarship in the sociology of religion over past decades has been the result of a helpful self-reflexivity in the field. That itself may be partly the result of a residual sense of marginality or inferiority in relation to other, more dominant fields in sociology; and partly as the result of thoughtful scholars in the field simply trying to think clearly about worthwhile agendas, priorities, and concerns. Whatever the causes, the sociology of religion has benefitted from repeated reflections by various of its leaders about its problems and promising future directions (see, for example, Levitt et al. 2010; Smilde 2010; Sherkat and Ellison 1999; Smith 2008; Smith and Woodberry 2001). In most cases, those self-reflexive exercises taking stock of the state of the field have focused on matters of theoretical and empirical substance, addressing issues such as how the field can expand its geographic scope of vision, diversify the religions it studies, integrate new theoretical perspectives, better measure key concepts, and so on. Those focuses and observations have been good and valuable, in my view. But I also think we can do more along different lines to improve the overall quality and value of our scholarship, and to better achieving the cumulative nature of scientific knowledge that ought to result from our collective scholarly endeavors.

As sociologists reflecting sociologically on the scientific production of academic knowledge, we know that social structures and institutional routines of production and dissemination are crucial in forming the long-term practices, character, quality, and contribution of any field or discipline. There is more than one way to produce and distribute scholarly knowledge. And different ways of doing it can produce diverse long-term results. I think journal publishing in the sociology of religion functions quite well overall. I am particularly impressed by the leadership of editors and editorial boards in this regard. Nevertheless, I would like to suggest in what follows that, at least in some ways, we are stuck in certain institutional routines of scholarly publishing that reduce the overall quality, value, and cumulativeness of our scholarship, at least compared to what I think is our potential.

If so, there may be an irony here. What I mean is that many sociologists are in the discipline of sociology in the first place because they believe in the emancipating or humanizing effect of grasping the socially-constructed nature of much of the world we live in. Because humans have socially constructed much of our world in the first place, sociologists often believe that people, empowered by the sociological imagination, might reconstruct it differently and better. Oddly, however, we sociologists often treat our own professional, institutional systems of knowledge production and distribution as immutable. Those systems can almost seem like fixed facts of nature which individual scholars, trying to build their careers, must take as givens and simply work with, through, and around — however problematic they can sometimes be. I propose here that we step back, take stock of some of the arguably problematic consequences of certain of our scholarly publishing practices and conventions, and consider reconstructing some of them to more effectively serve our shared goals and interests. Unlike many recent worthwhile reflections on the state of our field, in what follows I do not focus on the *substantive* content of the material studied and theories employed in our studies. Instead, I address certain concerns about the *processes, standards, and conventions of our academic journal publishing* — the expectations, evaluations, and dissemination of our scholarship promulgated in journal formats — which I think matter for the long-term quality, value, and cumulativeness of our field’s scholarship. Some of what follows speaks to both qualitative and quantitative (or what I might prefer to call “intensive” and “extensive”) forms of analysis, but I especially emphasize proposals for quantitative articles, since I think quantitative publishing standards could use an extra dose of rethinking.

## Five Premises

The following analysis and proposals are based on five important premises. One is that our field, the sociology of religion, does not exist simply to provide enjoyable academic careers to scholars who conduct research in it, which, lo and behold, are best built by working the publishing system, however it functions, simply to add publication lines to curriculum vitas. Careers are a *means*, viewed from the perspective of our field. The proper *end* toward which the means of our careers ought to work is the production of bodies of social scientific knowledge about religious social worlds that are important, interesting, and useful for both the larger project of social science and for the self-reflexive, real-life social world which we study.

Second, social science requires the skillful combining of both humanistic and naturalistic science approaches to understanding human social life. Good sociology is necessarily both interpretive and explanatory. It seeks both meaningful understanding and causal explanation, each necessarily informing the other. Therefore, qualitative and quantitative concerns and methods of observation, *verstehen*, interpretation, sampling, measurement, representation, inference, and causal attribution are all essential aspects of the sociological project. Concerns with possible ways to improve processes of journal publishing need therefore to address both large-N and small-N studies, both “extensive” and “intensive” research processes.

My third working premise is that rigor in methodological details is very important. We do need to avoid becoming mindless “bean counters.” At the same times, methodological corner-cutting and collective problem-denial damage the value, importance, and legitimacy of our scholarship. It can only help us as a field to identify areas of methodological weakness in our publishing systems and to reform them.

Fourth, I presume that — although our long-term disciplinary goal is causal explanation — much of the value that sociology has to contribute to the world more proximately consists of sheer, accurate, rich description of social life. Excellent description is also the necessary precondition of good causal explanation. Answering the basic questions, *What exists in social life and how does it work?* is a crucial aspect of any sociologist’s scholarship, which we should not devalue but, when done well, rather appreciate, encourage, and reward. Not only can we not explain until we have something interesting, important, and accurately described *to* explain, but our eventual explanatory insights, if they turn out to be any good, must be built upon an intimate familiarity with the described reality that needs explaining. And that requires a great deal of prior descriptive empirical investigation.

The fifth premise underwriting what follows is that social science, like all science, seeks to develop bodies of generalizable knowledge about different domains and processes of reality. (Again, “social science” by my account necessarily includes qualitative, interpretive, ethnographic scholarship, not merely quantitative work.) Ideographic descriptions of particular cases are extremely valuable, as I have suggested and argue below. But stopping with mere descriptions of particularities is social-scientifically underachieving. While always appreciating that, we also need to continually move beyond that by together building coherent bodies of understanding and knowledge that generalize across specific cases to similar types or categories of cases. That is part of what makes our scholarship social *scientific*, and not merely anecdotal, historical, or journalistic. This drive toward general knowledge is inescapably a collective project that requires the thoughtfully bringing together the contribution of many scholars over time in a cumulative, generalizing, theorizing process. My own belief is that the positivist empiricist approach to this generalizing theoretical work has failed and necessarily always will fail — which helps to explain the push-back of much of the anti-scientific, hermeneutical, interpretive, and qualitative in sociology. Our truly general sociological knowledge will, I am convinced, never take the form of “if A then (more likely) B,” as positivist empiricists would have it, because what is patterned or generalizable about human social rarely works that way, especially over time. Rather, following a critical realist approach, we should be looking instead for what is sociologically generalizable by focusing on understanding the recurrent patterns of operative *social causal mechanisms* that make the social world operate often with the continuity, repetition, and explicability that it does. What we can generalize about, in other words, are common and recurrent causal mechanisms that apply in diverse analytical settings, not positivist “if A then B” covering law statements that supposedly apply across different populations (for a fuller, more detailed argument for this view, see Smith 2010).

### **Specific Proposals**

With these five premises in mind, I next advance five specific proposals for the reformation of some of the standards and conventions of our scholarly journal publishing, which I believe will, if institutionalized, improve the long-term value, importance, and cumulative generalizability of our scholarship, relative to current practices and standards. My five proposals are as follows:

1. *Scholarly journals should routinely solicit and publish numerous descriptive empirical research notes in every published issue.* Nearly everything that is published in scholarly journals today (other than book reviews) takes the form of “The Article.” Articles are expected to be fairly long. Not only must they normally analyze empirical data and present believable findings. They must also be well situated in an important developing literature and, importantly here, must contain and advance significant “Theory.” All good articles are required to be theoretically well developed, to make a “theoretical contribution,” to themselves build up general theoretical knowledge. Hence, a standard convention of journal reviews of paper submissions is for reviewers to demand that submitting authors “strengthen the theoretical contributions” of their papers. One of the standard items on the checklists of evaluation criteria in the official report forms to be filled out by reviewers, standard forms designed to help editors decide whether papers merits publication, concerns the quality or originality of the submission’s “theoretical contribution.” My argument here is that, while many good articles do need their theoretical contributions strengthened, this conventional expectation that *every* publishable piece of scholarship be “theoretically significant” is unnecessary and counter-productive.

Let’s be honest. A lot of the “theoretically significant contributions” that our standard practices force journal article authors to develop are a waste of words and journal space. Not all of them. Sometimes journal articles make fantastically stimulating and important theoretical contributions — but not always, or perhaps even often. My observation is that many article authors start off with a finding of analytical empirical value — hit upon in their fieldwork, by crunching survey statistics, or some other kind of empirical exploration — which does address a significant scholarly question or concern. Usually they can situate their empirical finding(s) in a relevant “literature.” And what they have found, even though it is sometimes merely informative empirical description, can often contribute meaningfully to our broader knowledge of the contours and operations of the social world of religion. Yet our taken-for-granted scholarly conventions unnecessarily force all such authors to significantly add length to their papers by beefing up the (alleged) “theoretical significance” of their findings, which are presumed to *have* to exist for those findings to have any value. The assumption seems to be that every new empirical finding in every published journal piece must be matched by some publication-worthy theoretical insight, innovation, or other theory contribution. I think this is ridiculous. On the one hand, our conventions expect far too much from every publishable piece of scholarship. On the other hand, assuming that each published article makes a significant theoretical contribution itself, we far too often neglect the important task of

consolidating the larger knowledge gained by examining years worth of published journal articles focused on the same area into generalized, empirically-based, theoretical knowledge.

Here is a scenario that I have all too often seen befall far too many scholars in a variety of fields in sociology. The result of round one of a journal review instructs authors to “strengthen the theoretical significance” of their papers — decision: revise and resubmit. Round two of the same review then objects that the theoretical claims of the revised papers are not adequately supported by the data and findings — decision: rejection. Something is wrong there. Another common scenario: Round one tells the author to beef up their paper’s “theory” — decision: revise and resubmit. The author, however, knows full well that the kind of “theory” the reviewers want requires an unwarranted, sometimes even speculative stretch beyond what their data and findings actually support. But driven by the career imperative to get published one way or another, the author does his or her best to spin some kind of theoretical story that hopefully, fingers-crossed, satisfies the reviewers as making a real “theoretical contribution.” Sometimes that fails. But, when it succeeds, round two satisfies the overreaching reviewers — decision: acceptance and publication. An extra couple of pages have thus been added to the article that in fact may make very little real theoretical contribution that will stick in any literature over time — extra pages that, when added up across articles, crowd out the potentially valuable empirical contributions of other authors whose papers are likely rejected because they do not pack what appears to reviewers to be enough theoretical umph.

What is going on here? Many empirically-focused papers which I think probably merit publication simply cannot sustain the burden of generating the kind of “theoretically significant contribution” that we typically expect of The Article. And we should not make them do so as a condition of publication. That simply excludes from publication papers that do make limited but real *empirical* contributions. It also forces article authors to spin stories and make claims that their findings may not justify, thus cramming into the pages of our journals too many theoretical pseudo-contributions. Often “Prior Research” sections of articles, which set up their alleged theoretical contributions, are unnecessarily elaborate; and “Discussion” sections are often needlessly extended, discursive, speculative, and only loosely connected to empirical findings.

Imagine a stone-bridge builder who demands not only that all of the quarried stones that together compose the bridge fit together into a single bridge structure, but one who also demands that each and every quarried stone used to build the bridge is itself actually carved into the shape of miniature bridges. That is

something like what we expect of The Article in our journals. Rather than taking a big-picture, architectonic view of our sociological task, which expects our collective scholarship to coalesce over time into valid, reliable, generalizable social scientific knowledge, we instead demand that each and every article itself and on its own make a significant theoretical scholarly contribution. And all too often, as I'll argue below, what we end up with on the whole is not a single, well-constructed bridge that we together have helped build, but rather a valley strewn with myriad individual stones that are more or less well carved to look like miniature bridges but are not particularly well put together into anything larger or more useful.

What ought we to do then? We need to acknowledge that there is more than one way to make a “significant contribution” in sociological scholarship. I propose that journals create different sections in each issue separating “Empirical Research Notes” from “Articles,” and that they encourage the writing and submission of both kinds. Submissions should be required to label themselves as either research notes or articles. I propose that research notes be required to succinctly situate their findings in an existing literature and to be very clear about their methods, but not be expected to explain in more than one paragraph the broader theoretical significance of their empirical findings — clarity and parsimony should be premium values for research notes. Some journal reviewers will need to be educated to review research notes differently from full articles, bringing different standards to bear in their evaluations. Journal readers should also learn to appreciate the longer-term value not only of theoretically significant articles but also of empirical research notes which help to build up bodies of qualitative and quantitative descriptive evidence that in time can spur conceptual insights and underwrite theoretical elaborations based on meta-analyses of many of them. Others who evaluate careers — such as department and disciplinary colleagues, department Chairs, Deans, and letter writers — will need to become better educated about the issues addressed in my argument here, learn how to both appreciate the contributions of descriptive research notes, and learn to distinguish articles' genuine theoretical contributions from pseudo-contributions.

What will be some of the likely consequences of this reform? Graduate students will be more likely to learn early how to conduct and publish solid empirically-focused research as a first step of their ongoing training process, without the daunting burden of having to make an alleged theoretical contribution from their first written paper. Paper authors will be free to make honest claims about conclusions and implications which their data and findings actually support, without being systematically forced to make up theoretical stories

masquerading as “contributions” which their findings may not necessarily justify. This will raise the bar on what counts as a genuine theoretical contribution in published articles, forcing authors to make them well or not at all, thereby increasing the overall quality of articles published. This approach should also have the enriching effect of producing many more empirical descriptions of religious social worlds for all readers to consider and reflect upon theoretically. I, for one, would love to read and would benefit greatly by reading a lot more short research notes contributed by many different scholars in various parts of the world describing qualitative and quantitative empirical findings about significant aspects of religion’s social life, without a lot of obligatory and often useless theoretical setup and discussion attached. All readers would then have a lot more empirical grist for the theoretical mill with which to work, increasing for everyone the overall input of and exposure to a greater variety of kinds and cases of empirical scholarly evidence toward achieving our longer-term scientific aims (again, broadly conceived). All in all, I believe this change — the encouraging and honoring of the simple descriptive, empirical, scholarly research note — would contribute significantly to enhancing the value, importance, and generalizability of the results of our collective scholarly work.

*2. Scholarly journals should encourage and more frequently publish synthetic meta-analyses and literature reviews of accumulated published research notes and articles.* If our larger goal is not simply to publish a lot of articles and so build individual careers, but rather more importantly to collectively develop over time generalizable, empirically-based, causal-mechanisms-focused, theoretical knowledge about specific domains and processes in social life, then we need to invest more into writing review articles that occasionally examine and synthesize relevant bodies of published literature to summarize “state of the art” knowledge and advances in our field. These should not be published too often, since it takes time between them to accumulate enough relevant publications to justify new review articles of them. But I think these should be published more often than they currently are. Much of the real value of any given publication only becomes evident when it is put together alongside many other related publications and is assessed for its relative descriptive or theoretical contribution to a larger project of building general knowledge. Examples of such synthetic and sometimes creative review articles exist — I think, for instance, of Warner (1993), Chaves and Gorski (2001), Cadge and Ecklund (2007), Emerson and Hartman (2006), Regnerus (2003), and Woodberry and Smith (1998), among

others. But I am convinced that we could use many more of these that address different important focuses in our field.

One important thing that good review articles do for their fields is to provide big-picture evaluations of what over many previous years has been valuable, what has gone wrong, what has not added up, and what seems to hold most promise for future research. Good review articles are much more effective at this than the usually-ignored throw-away “for future research” ideas often stuck into the end of many published journal articles. Good review articles can help to terminate unfruitful lines of research, the problems with which individual scholars involved in them might not be well positioned to see. They can expose patterns of success and limitations in research programs, steering our limited resources into more productive directions. Good review articles can also sometimes suggest highly fruitful syntheses of disparate works (even sometimes including work from outside a field or discipline, such as bringing good but largely unknown historical work to bear on sociological concerns), which can spur new research agendas and programs. Review articles can also consolidate and summarize the key findings in certain subfields, reducing the need for scholars thereafter to rehearse the same observations and arguments in subsequent published literature reviews in their articles. Review articles can be extremely helpful to young graduate students who are trying to get up to speed as new scholars in the field and discipline. Finally, good review articles can remind us of just how little we often know in certain areas of inquiry, helping to motivate further, well-informed research.

The standard mentality today is to think that review articles belong in *Annual Reviews*. The latter serves a useful function. But I see no reason why *Annual Reviews* should essentially own the review article genre in sociology, and perhaps particularly in the sociology of religion. I think the standard mentality fails to stimulate or encourage the number and variety of review articles from which we as a field might profit from having written and published. So I think that journals in the sociology of religion should encourage the publication of useful, well-timed, meta-analysis review articles on various important questions in the field. Such review articles should be greatly valued by all scholars in the field and beyond, for their larger mission-serving constructive function. Done well, they should also enhance the accessibility of the findings of the sociology of religion for interested scholars in other fields and disciplines, by reducing the costs to “outsiders” of search and assimilation of the best of what we have learned over the years. That itself should build bridges from, increase interchange between, and raise the visibility and relevance of religion in other fields and disciplines. There is no reason to wait for

individual scholars to volunteer to publish chapters in an *Annual Reviews* volume. The editors and editorial boards of our field's journals should proactively foster possibilities and rewards for the writing and publishing of high quality review articles.

3. *Journal editors, paper reviewers, and readers should require of submitted and published research notes and articles a greater amount of specific information about their particular data and methodologies.* I am not a picayune methods geek, but as a reviewer of papers submitted to journals I am often disappointed and sometimes appalled by the lack of specific information provided about the data and methods employed in their analyses. For example, I recently reviewed a quantitative-analysis paper that in its one-paragraph methods section merely named the survey dataset it was analyzing. No information beyond that was provided, not even the survey's N, not to mention a reference to a separate methodological report, codebook, or other detailed description of the dataset used. It goes without saying that authors should report up-front the number of cases analyzed. But here I am mostly talking about other, equally if not more important methodological information that should be expected and required to be reported as a matter of course.

No research note or article based on survey data, for instance, should be published that does not clearly report the survey's response rate and address possible systematic non-response biases involved and how the analysis addresses those possible biases. Everyone evaluating the merit of the paper and reliability of its findings deserves to know basic but extremely important data facts, like what proportion of all of the respondents who were sampled actually completed the survey. All too often, authors simply report the final survey N, or perhaps the rate of those contacted (not sampled) that completed the survey. That is unacceptable, since the reader is left with no idea of what kind of possible non-response biases might be reflected in the data. Sometimes margins-of-error are reported, but that too is inadequate. Further, even when survey response rates are reported, unless they are remarkably high or already widely established as acceptable (as with the GSS, at least for now), authors should be required to explicitly address possible non-response bias issues. Are there measured or unmeasured factors that we have good reason to believe may associate both with the dependent variable(s) being analyzed and with systematic non-responses to the survey? If so, the data may be in trouble. It is unacceptable simply to push such possible problems into a closet and shut the door by failing explicitly to address such issues. They

need to be laid on the table and explained. There *are* ways to address non-response bias challenges, but they need to be addressed, not ignored (see, e.g., Smith, Pearce, and Denton 2008).

Another unacceptable attempt to address this problem is when authors simply state that “calculated weights were applied to correct for respondent unevenness” or some such claim. Applying weights per se often does nothing to correct for systematic non-response biases (even if they may helpfully adjust for proportions in descriptive statistics purporting to represent some population). Potentially applying such weights could make the non-response biases *even worse*. Weights only potentially work when they correct for the factors associated with the systematic non-response. To rely on statistical weights to boost a low number of, say, racial minorities in a survey sample accomplishes nothing if the systematic non-response bias actually associates not with racial minority status but rather with differences in, say, likelihood of joining voluntary organizations, when your dependent variable is also associated with voluntary organization joining. We should not allow ourselves to pretend that the simple application of weights, which are usually calculated simply to adjust sizes of a few under-represented demographic groups, automatically fixes systematic non-response biases which can be driven by a variety of different biasing factors. At the very least, we must be made aware of what possible non-response biases in any given sample might be.

The larger problem, of course, is that survey response rates have been falling significantly for a long time now. What used to be an unacceptably low response rate at the beginning of my faculty career is now considered enviably if not impossibly high. In short, our professional standards have slipped a lot as social reality has become harder to work with. Nobody wants survey research to go up in smoke because respondents cannot distinguish real social research from telemarketers and so are increasingly inaccessible and unwilling to cooperate. But we simply do not address the challenges noted here honestly by not reporting response rates, not addressing possible systematic non-response biases in the data, and not explaining to readers how and why those biases have been confirmed not to exist or have otherwise been addressed.

Yet another cause for possible significant errors in our scholarly findings are unaccounted for missing data. I regularly review papers conducting large-N analyses which (completely apart from questions about original response rates) drop huge numbers of cases during analyses due to missing data in variables used. The methods sections in many of these papers merely breezily mention in passing that a certain number of cases, oftentimes amounting to 20-30 percent of all sampled cases, have been dropped due to missing data. No such

paper should ever be published that does not first carefully explain which variables contained significant missing data and whether or not their being dropped threatens the integrity of the analysis, and findings; or which does a solid job of imputing missing data. Again, the temptation is to pretend that all is copacetic, when in fact the readers and perhaps even the authors have no idea whether all is well. Journal reviewers and editors simply must raise the bar on their expectations for the standard reporting of crucial information about data and methods that may affect their quality and reliability.

These sorts of data and methods problems are not only relevant for quantitative analyses. Similar or analogous data and methodological information should be required reporting for all qualitative and experimental analyses as well. Tendencies toward slippage and lack of information are sometimes evident here too. Paper authors sometimes claim, for example, to have conducted “ethnographies,” when really they have mere done some limited participant observation. And sometimes authors assert that they have conducted a “participant observation” study, when in fact they have only conducted some interviews, studied some printed literature, and taken a few notes on a field setting. But all of the same fundamental research design questions apply to qualitative studies that apply to quantitative research. Who or what is the study about? What does it claim to represent? Specifically how and why were certain subjects or cases selected or sampled? Who failed to show up in the data, why, and what might that say about possible problems in the data? What are the key analytical distinctions or comparisons involved in the analysis? Do the actual data in fact speak to the research question? Can the evidence support the findings and conclusions? And so on. All scholarship needs to come clean on answers to those kinds of questions.

In the end, it is always better to admit that we know nothing at all than to claim to know something that is actually wrong. Sociology’s equivalent to the medical profession’s non-maleficence injunction derived from the Hippocratic Oath (“*First, do no harm*”) must be for us: “*First, report no mistaken or distorted findings as facts.*” Minimum data-quality thresholds exist below which data are best never collected, much less analyzed and published on. All scholars should know this. Yet it is the job of journal reviewers and editors in the last resort to guard the gate on such quality standards, especially in face of today’s highly competitive career-building pressures to get published as quickly and often as possible. In order to guard the gate well, authors of both research notes and full articles should be expected as a matter of standard routine to report and explain concisely but in sufficient detail information about their data collection, manipulation, and analysis, about their

methodological procedures from beginning to end. In the case of quantitative analyses, authors should also — before jumping ahead to multivariate analyses — normally present first tables describing their variables and showing simple but meaningful, bivariate correlations or cross-tabulations of interest. Data and methods are not minor technicalities that we should impatiently rush past in order to get to our analyses and findings. They are crucial disciplinary matters which help validate the very worth of our analyses and findings.

4. *Scholarly journal publications should be both more careful and more bold and confident about claims concerning causation and causal inferences and explanations in social life.* Many sociologists seem to operate with bad consciences when it comes to causation. The problem is this. Most sociologists view their job, whether they state it exactly this way or not, as essentially providing explanations for why things happen in the social world the way they did or do — and explanations of these kinds inescapably involve identifying the causes of outcomes. Yet most sociologists are aware of the methodological problems with identifying real causal operations and so with making causal claims. Correlation does not equal causation. Rarely do we enjoy clear counterfactuals in analytical situations, which in theory isolate specific causal variables producing outcomes net of other possible confounding and complicating factors. The social world always operates as an “open system” — not as the “closed systems” created by some natural science experiments — making it extremely difficult if not impossible to control for the many forces and variables that can shape real-life outcomes. Sociological datasets often do not contain all of the measures and variables we really need to construct the right analytical models and arguments. And there is always the nagging possibility, in qualitative as well as quantitative analyses, that some unmeasured factor or missing variable or unobserved comparison lurks just beyond one’s analysis which would “explain away” one’s significant associations and causal attributions. So claiming to have identified factors or mechanisms that actually cause outcomes of interest seems risky if not impossible. Yet that in the end is what sociology exists to do.

Rather than completely abandoning causal explanation, as one option, or throwing caution to the wind and making reckless causal claims, as another option, many sociologists seem to simply split the difference. Usually that is done by deploying ambiguous and slippery terminology. Instead of saying that X “causes” Y, sociologists write things like X “leads to” Y, X “increases” Y, X “predicts” Y, or other similarly evasive locutions which neither clearly claim nor deny causal explanation. But that is a problem. Either causes are real or they are

not. Either we can identify some of them or we cannot. We need to fish or cut bait. When it comes to causation, we should not claim to do things that we cannot do, but neither should we shrink back from making claims that we are reasonably entitled to make. If we need to qualify or hedge our causal claims, then let us do that explicitly, not by reliance of obfuscating phrases such as “leads to,” “increases,” and “predicts.”

One problem here is the influence of armies of semi-enlightened journal reviewers who have not clearly thought through the philosophical matter of causation and so too often instinctively reply to papers with knee-jerk predictability that no author may *ever* use the language of causes or causation in his or her paper. That is silly. If all we are really able to do in sociology is establish apparent correlations that say nothing about causation, then it is time to pack up our disciplinary bags and go get our MBAs and law degrees. Science is ultimately about identifying recurrent causal mechanisms and forces (including, in sociology, cultural meanings and reasons) that produce identifiable types of outcomes. That is what we must be shooting for. The problem on the other side, of course, is some sloppy authors who unthinkingly let causal terms and claims slip into their writing without good warrant. That too is unacceptable.

I am not here able to explicate the kind of broad philosophical and methodological understanding of causation and its identification in social life that is needed to make full sense of the causal issue in question in the present discussion (but see Bennett 2008; Gorski 2009; Porpora 2007, 2008). I have, however, done that in soon-to-be published book, which provides the background for my case here (Smith 2010). Suffice it for present purposes to say that causal claims in journal publications need simultaneously to be more circumspect in not claiming more than is analytically justified, but at the same time should be more bold in asserting themselves causally when they are arguably justified. Journal reviewers and editors should continue to caution authors when they make questionable causal statements. But they have no reason to *a priori* rule causal claims out of bounds.

Part of the crucial issue here concerns the need to shift our attention away from positivist empiricism’s “if A then (more likely) B” covering law propositions, the pursuit of which for generalized knowledge fails. Instead, we need to follow the critical realist lead, toward a primary concern with understanding social causal mechanisms. As long as we think our job is to identify regular associations between events operating on the “surface” of reality, we will never solve our current problems concerning causality. But if as critical realists we can affirm that causes are real and that reality involves “levels” that are not all observable (Smith 2010), then we will have repositioned ourselves to make proper sense of causes and causal claims in our scholarship. Only

critical realism, I am convinced — not positivist empiricism, not postmodern deconstruction, not even hermeneutical interpretivism by itself — provides a coherently reasoned way out of our current causal bad conscience and quagmire of practices on the matter of causal explanation.

One of the keys to making sense of our current causality problem, in all of this, is to realize that causation can never, ever be established by our empirical evidence. It just can't. Causes are real, when they exist and are operative. They have ontological being and do operation in the real world, though our empirical methods to nail them down, when understood in empiricist terms, are inadequate to "prove" that. Still, we can know about causes by (stated simplistically for present purposes) (1) bringing our personal knowledge about the world to bear on (2) methodologically careful observations of their patterned consequences and results, as (3) interpreted by our best theoretical accounts of how reality operates and (4) continually examined in various cases and contexts. David Hume was essentially right: we can never directly observe causes. But still we *can* see causes *at work* and their *results*. And we can use our philosophically-well-formed minds operating with the benefit of personal knowledge to oftentimes adequately theoretically understand the operation of the real causes which tend to produce certain characteristic outcomes or results. But it takes critical realism to put all of those pieces together coherently. In short, causal understanding and explanation is not impossible, not beyond the capacity of human knowledge. But we have to know what we are looking for, how to look for it, and what it is like when we see causes at work producing results. In some ways it is easier and in other ways much more difficult that we often suppose. Again, this matter raises discussions of greater depth and complexity than we can engage here (but see Smith 2010). For present purposes, I wish to put on the table as a proposal for collegial consideration the idea that we need not and cannot avoid the shared pursuit of real causal understanding, knowledge, and claims-making, even though that pursuit is not easy or simple and even though some sloppy and misinformed scholarship has been irresponsible on the issue of causation at both extremes. In the end, journal reviewers and editors need to raise the bar on standards regarding causal claims, denying those that do not make it over the bar but also unapologetically accepting those which do. To accomplish that well, I think, will require a stronger meta-theoretical grounding in critical realism.

5. *Scholarly journal publications should pay less attention to statistical significance and more attention to the actual causal force, power, or effects that variables appear to exert on outcomes.* Much of quantitative

sociology — indeed, of quantitative social science generally, from what I can tell — has suffered a kind of “faith conversion” to a mindless devotion to the “cult of statistical significance” (Ziliak and McClosky 2009). Far and wide, it is routinely pretended that simply because a variable in a multiple regression analysis is statistically significant that it automatically is “important” and helps to explain something that actually matters (at times the obsession with  $p < .05$ , entitling us to place an \* next to a coefficient or odds ratio, seems like elementary school children eager to see teachers paste colored stars on their graded homework). But that is bunk, the fetishizing of the asterisk. I suspect, in fact, that in very many cases, many statistically significant variables in journal publications are actually virtually irrelevant for explaining anything about their dependent variables. And if so, then many of our supposedly statistically-authorized published claims are empty. Everybody can of course cite the introduction-to-research-methods mantra, “statistical significance does not equal substantive significance.” But then almost everyone ignores it. In far too many cases of standard practice, what matters in the end, in terms of telling a convincing statistical story to reviewers and readers, is simply that the right variables are “significant” — even if in reality their significance is in fact substantively insignificant.

Slippages in the interpretations of findings in published articles routinely happen, from noting initially that variables are “statistically significant” to the much more ambitious claims that variables “matter” and are “important.” In standard practice, the more asterisks next to one’s coefficients or odds ratios that can be shown in a table, the more successful, it is commonly presumed, are the publication’s analyses and explanations. But to repeat an observation I first heard from Harvard’s Chris Winship (see Smith 2010: 283), ultimately, the only thing that statistical significance tells us is how large our sample sizes are. With large enough sample sizes, nearly all observed differences, however minute, are statistically significant. With samples that are too small, many variables that actually do matter and may explain a lot in outcomes remain insignificant. When all is said and done, I think, this cult of statistical significance leads to one of the weakest aspects of our published quantitative scholarship.

In response, journal reviewers, editors, and readers need to shift the criteria of importance and persuasion and raise the bar on convincing statistical evidence by de-centering statistical significance and instead focusing attention on the relative *power* of variables and the actual *magnitudes* of difference they make in outcomes. The standard question should not be whether a variable is statistically significant. The standard question should be what meaningful difference the variable actually appears to make in any outcome. If a

variable, particularly in a large dataset, is statistically significant but makes little difference, then it simply should not matter. If a variable in a small-N dataset or dummy variable is demonstrably important in the meaningful difference it appears to make in the outcome yet remains shy of statistical significance at the (totally arbitrary)  $p < .05$  level, then it should still count in the publication's carefully-explicated explanation. In short, we should have to use stronger evidence and more brainpower to make our cases about "what matters" than the mere noting of asterisks.

There are various ways to do this. Showing important bivariate cross-tabulations before getting to multivariate analyses is often helpful. In some cases, procedures such as multi-classification analysis (MCA) can show bivariate crosstabs net of the possible effects of other control variables. Simply paying closer attention to changes in R-squares across models can tell us about the relative importance of particular variables. In other cases, showing predicted probabilities can demonstrate the real difference in effect that a significant variable makes. These and no doubt other statistical means for displaying the actual importance of variables, and not merely their statistical significance, are readily available to journal authors. They and what they can tell us simply need to be prioritized as matters of importance, not treated as optional considerations in the presentation of findings and claims about effects. We need, in the end, to develop mindsets and practices which view statistical significance not as the last word but rather as only one provisional tool among others to help determine what really matters in quantitative analyses: the identification of variables that actually make important or powerful differences in outcomes. That is a matter of associational strength and causal force and effect, not simply statistical significance.

### **Summary and Conclusion**

We in the field of the sociology of religion should be mostly happy with the many valuable scholarly contributions resulting from our current practices of journal publishing, and particularly happy with the good editors and editorial boards who exercise leadership in that endeavor. But there are certain areas in our journal publishing practices that I think can and should be reformed and improved. We ought to do much more to encourage and reward the publication of short, informative, descriptive, empirical research notes that bear no burden themselves to make a supposed theoretical contribution. We should take a more proactively cumulative and synthetic approach to the building of generalizable knowledge in the field, encouraging the writing and

publishing of more integrative, meta-analysis review articles. We should raise the bar on our expectations about the standard reporting of specific data and methodology information in every publication submitted and published, so that authors are forced to come clean on crucial methodological matters, such as sampling procedures, response rates, systematic non-response biases, missing data, and so on. We should also do a better job on the key issue of causation and causal claims in published work — by compelling authors in their writing to be more definite about when they are and are not making causal claims, to reasonably defend those claims when they are made, and (on critical realist grounds) to not shrink back from making causal claims when they are reasonably justified. Finally, we should clean up our acts in quantitative analysis on the matter of identifying variables that actually “matter,” by deflating the relative importance of statistical significance per se and instead moving to the center of attention analytical demonstrations of variables’ actual strength, force, power, and effects on outcomes in question. These five proposals are reasonable, realistic, and, if implemented, would have the result, I believe, of significantly enhancing the quality, value, legitimacy, and cumulative character of our scholarly knowledge and understanding.

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