

The *Sociological Methodologist*

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Contemporary Issues in Causal Inference

by Ken Frank

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This article discusses contemporary issues in causal inference and how they are being addressed, especially by social scientists. While it is intended as a general introduction, many of the issues are controversial. The presentation below reflects my own take and interpretations rather than a broad review of multiple viewpoints.

The fundamental problem of causal inference is often defined by the counterfactual. As an example: I have a headache; I take an aspirin; my headache goes away. Is it because I took the aspirin? It is impossible to know for sure. We could be certain

only if we could have also observed what happened to me if I had not taken the aspirin. But this control condition is impossible to observe for a single individual. It is counterfactual.

Recognizing our inability to observe the counterfactual as a fundamental problem of causal inference at the individual level, scientists typically infer causality by comparing sets of units that received different treatments. A classic example would be a chemist who divides a single solution (composed of molecules) into two parts, one exposed to a treatment and one to serve as a control. Resulting differences are then attributed to the treatment. This example is often considered to be a gold standard for inferring cause.

But even the gold standard requires certain assumptions for making inference. Namely, that the units receiving the treatment and control are homogeneous. But units could be heterogeneous if the original solution is not properly stirred, if the two solutions are exposed to even small differences in conditions not associated with the treatment (such as temperature), or if two solutions have different levels of purity.

Like the chemist, social scientists often analyze sets of units to make causal inferences. But it is precisely when analyzing an aggregate of individuals that causality is uncertain because there may be baseline differences between those who received the treatment and those who received the control, or the treatment may have a different effect for those who received the treatment than for those who received the control.

In order to reduce the baseline differences, social scientists often randomly assign subjects to treatment and control conditions. As sample sizes

increase, randomization reduces differences between treatment and control groups, thus making causal inferences based on differences between the treatment and control group more robust. As a result, randomized control trials (RCTs) are currently considered the gold standard for inference in the social sciences.

But RCTs have two important limitations. First, the experiments on which they are based are often different in important ways from treatment conditions as they naturally occur. For example, in attempting to insure uniformity of treatment, experimenters may educate or engage those who implement the treatment in ways that are unlikely to occur outside the treatment. In education, some evaluators require that the treatment be implemented by 70% or 80% of the teachers in a school, a level of implementation that does not occur for most reforms or innovations.

Second, randomization still requires that people are treated and respond independently of one another. This may not be the case if doctors treat multiple patients. In education, many reforms and innovations are implemented through the coordinated activity of teachers. Therefore, teachers within a school are not independent of one another. Typically, many sources of dependencies are accounted for by carefully defining the units that can be considered independent (e.g., schools) but this can dramatically increase the cost of RCTs.

Given the above limitations of RCTs, social scientists often make causal inferences from observational data or quasi-experimental designs. Examples include analyses of large scale data bases that have shown a relationship between smoking and lung cancer, or between job training and employment outcomes, or between socioeconomic status and achievement.

Causal inferences from observational studies are tenuous relative to those from RCTs because there could be baseline differences between those who received the treatment and control that could cause differences in outcomes. To account for these, social scientists employ a range of statistical tools. First, social scientists can control for a covariate using the general linear model as in ANCOVA. Second, more complex controls might employ an instrument as an alternative measure of assignment

to treatment condition. This may reduce bias in estimation, but requires extra assumptions and has decreased power. Interestingly, a recent meta-analysis [Glazerman, Stephen, Levy, Dan and Myers, David (2003). "Nonexperimental versus Experimental Estimates of Earnings Impacts." *Annals, AAPS* (589): 63-85] found that statistical control for a prior measure better estimated effects later obtained from RCTs than estimates using instrumental variables. Third, social scientists have recently started to approximate the counterfactual by matching those who received the treatment with those who received the control based on propensity to receive the treatment.

Even after employing statistical controls for covariates, there may still be concerns that treatment effects could be attributed to uncontrolled baseline differences between treatment and control groups. Defined in terms of the general linear model, my work quantifies how large the impact of an uncontrolled confounding variable would have to be to invalidate a statistical inference.

The scenario begins when $r_{t,y}$, the observed sample correlation between t (reflecting assignment to treatment condition) and some outcome y , is statistically significant and is used as a basis for causal inference. Now define the impact of a confounding variable on $r_{t,y}$ in terms of $r_{v,y} \times r_{v,t}$, where $r_{v,y}$ is the correlation between an unmeasured covariate, v , and y ; and $r_{v,t}$ is the correlation between v and t . Next define $r^\#$ as a quantitative threshold for making inferences from a correlation representing the relationship between a predictor of interest and an outcome. For example, $r^\#$ can be defined by a correlation that is just statistically significant or by an effect size. Then, (maximizing under the constraint: $\text{impact} = r_{v,y} \times r_{v,t}$):

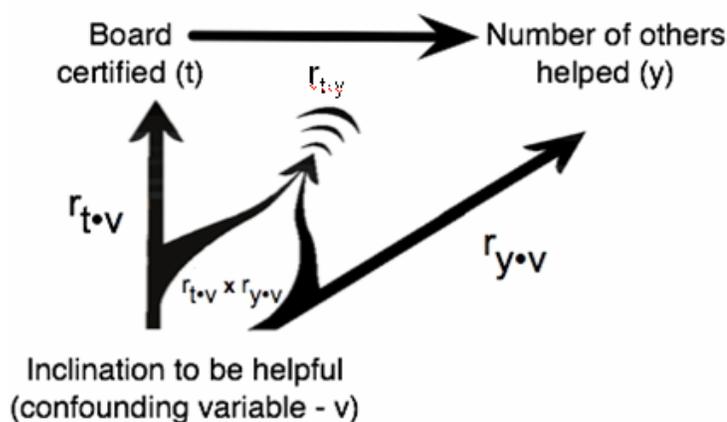
if the impact of an unmeasured confound $>$
 $(r - r^\#) / (1 - r^\#) \rightarrow$ original inference is invalid

if the impact of an unmeasured confound \leq
 $(r - r^\#) / (1 - r^\#) \rightarrow$ original inference is valid.

The example of the impact of a confounding variable in figure 1 applies to an analysis I recently conducted regarding the inference that attaining National Board certification affects the amount of

help a teacher provides to others in her school. Because teachers who are more inclined to be helpful may attain National Board certification, the inference may be invalid. But calculations show that the impact of “inclination to be helpful” would have to be greater than or equal to 0.081 (with each component correlation equal to about 0.28) to invalidate the inference. As a basis of comparison, of the measured covariates, the extent to which a teacher believed leadership would enhance teaching had the strongest impact (.017) on the estimated effect of National Board Certification. Thus the threshold value of .081 shows that the impact of an unmeasured confound would have to be four times greater than the impact of the strongest covariate in our model to invalidate the inference. This suggests the inference that National Board certification affects the amount of help a teacher provides to others in her schools is at least moderately robust with respect to concerns about unmeasured confounding variables. Importantly, robustness indices, as a form of sensitivity analysis, do not alter the initial inference. What they do is to quantify the robustness of the inference to inform scientific debate (for a spreadsheet and sas software for calculating my indices of robustness, and power point and related papers, see <http://www.msu.edu/~kenfrank/research.htm#causal>).

Figure 1: The potential impact of a confounding variable on a regression coefficient



Causal inference is one of the most rapidly growing areas for social science methodologists.

As a result, it is impossible to comprehensively discuss contemporary issues in causal inference in the limited space provided. For more comprehensive or alternative discussions, see work by James Heckman, Charles Manski, Donald Rubin, Paul Holland, Thomas Cook, Steve Raudenbush, Guanglei Hong, Michael Sobel, Philip Dawid, and Paul Rosenbaum. At Michigan State, Jeff Woolridge (Econ) has an excellent discussion of causal inference in his textbook *Econometric Analysis of Cross Section and Panel Data*; Nigel Paneth and Jim Anthony (both in Epidemiology) have written on causal inference especially with respect to experiments; Claudia Holtzman (Epidemiology), teaches a course in causal inference; Daniel Patrick Steel discusses the philosophy of causal inference; and my colleague within education, Barbara Schneider, has written on issues of causal inference and scale-up. Barbara and I are teaching a seminar (CEP991B: section 3) in Spring of 07 on causal inference).

For an excellent introductory and intermediate text, see Shadish, W. R., Cook, T. D., and Campbell, D. T. (2002). Experimental and Quasi-Experimental Designs for Generalized Causal Inference. Boston, NY: Houghton Mifflin. For a starting point on the web, try <http://www.wjh.harvard.edu/~winship/cfa.html>.

“Of Course It Works in Practice, But Will It Work in Theory?” Paradigms, Pragmatism, and Combining Multiple Methods

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This title summarizes the themes in my article for the first issue of the new Journal of Mixed Methods Research (Morgan, 2007). Because that article is 28 pages long, several people have asked for a short summary of the basic argument, and the recent solicitation for newsletter material provides that opportunity.

The essence of the article is an argument for applying Thomas Kuhn’s conception of paradigms to social science research methodology as a field of

study. At the core of Kuhn's description of paradigms (especially as presented in the Postscript to the second edition of his book, 1996) is an emphasis on systems of beliefs and practices that influence how researchers select both the questions they study and the procedures they use to study them. I thus propose to apply Kuhn's ideas to social science research methodology in terms of some of the major questions in our field and the procedures we use to pursue those questions.

Although the concept of paradigms has been at the heart of many of discussions about social science methodology over the past two decades, there is a strong contrast between Kuhn's own views about paradigms and the most widespread version of that concept in social science methodology. This second version of paradigms treats them as fixed combinations of Ontological, Epistemological, and Methodological assumptions, based on a rather traditional approach to the Philosophy of Science (for a less traditional alternative, see Hacking, 2000). Over the past two decades, this version of paradigms has been particularly influential among qualitative researchers, where it initially arose as a way to express the differences between Positivism and Constructivism, and then expanded into a broad-based approach for comparing different approaches to social science research (Guba, 1990; Guba & Lincoln, 2005; Lincoln & Guba, 1985).

To put this discussion into historical context, it is important realize that the advocates for this approach also used it to explain the rising popularity of qualitative research during this period. From this philosophically-oriented perspective, the differences between qualitative and quantitative research were about much more than methods; in addition, these differences raised deeper issues about the nature of social science research itself. Stated in the paradigmatic terms I suggested above, these advocates claimed that the most important questions in social science methodology concerned philosophical differences in how researchers studied the world, and that the proper procedures for studying these issues came from a specific approach from the Philosophy of Science.

There is thus a double irony, because this group of advocates for qualitative research not only used

Kuhn's concept of paradigms as a way to promote their own desire for a paradigm shift in social science research, but also did so by relying on a version of paradigms that was quite different from Kuhn's preferred version of that concept. To cap off the irony, Kuhn's own version of paradigms provides an excellent framework for understanding both this attempt to create a paradigm shift and more recent reactions to those efforts.

Since this philosophically based approach originated more than 20 years ago, it is hardly surprising that later commentators have called attention to problems with it. The most obvious problem is that there is no consensual list of philosophically based research paradigms in the social sciences. Indeed, over the years the number of paradigms included in semi-authoritative versions of such lists has grown from the original pairing of Positivism and Constructivism to a list of at least five possible contenders (Guba and Lincoln, 2005). Although issues concerning which paradigms get recognized and who gets to designate the list of available options cut to core of this approach, these problems have only recently begun to receive attention. As Kuhn would predict, these criticisms started among researchers who felt that this particular existing belief system did not fit their own work. In this case, the questions have come from researchers who advocate both the combining of qualitative and quantitative methods and the use of Pragmatism as a philosophical justification for their approach (e.g., Creswell, 2003; Tashakkori & Teddlie, 2003).

Strangely, Pragmatism was almost never included in the earlier lists of paradigms, despite its close connection with such historic figures in American social science as John Dewey, William James, and George Herbert Mead. The reasons for this exclusion can be traced to the original goal of contrasting the underlying assumptions behind Positivism and Constructivism. More specifically, the preferred distinctions between those paradigms focused on metaphysical connections between Ontological concerns about the nature of reality and Epistemological concerns about the possibility of truth. On the one hand, this concentration on metaphysical abstractions led to caricatures of Positivism as naive realism and Constructivism as

radical relativism. On the other hand, the metaphysical approach also pulled attention away from Pragmatism's emphasis on the connection between Epistemological concerns about what we can know and Methodological concerns about how we would achieve such knowledge.

We thus have arrived, once again, at a point where a group of advocates for a new way of doing research (i.e., combining qualitative and quantitative methods) have also proposed an alternative conceptual framework for social science research (i.e., Pragmatism). Just as the previous debates about the differences between qualitative and quantitative research were about more than just methods, a shift from metaphysical questions about the nature of reality and truth to a Pragmatic approach also raises larger questions about research in the social sciences. In particular, a Pragmatic approach to the research process would focus on both the decisions that researchers make and the outcomes they expect as a result of those decisions. This points to a methodological agenda that emphasizes understanding how social science researchers make decisions about both the questions we ask and the procedures we use to address those questions.

Ultimately, the claims I am making raise the question of whether we are in the midst of another paradigm shift – where Pragmatism would replace the more metaphysical approach to social science research and combining qualitative and quantitative methods would become just as legitimate as using either one separately. Only time will tell, of course. Meanwhile, I can imagine the spirit of Thomas Kuhn looking on with amusement as we struggle to understand both the meaning of his work and its implications for how we pursue our own field of research.

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Thoughts on "Raw Coefficients" in Recent Publications

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In some recent AJSs I have noticed two things about the use of "raw" coefficients. One was a paper which I believe to be excellent that had a coefficient for a well-described measurement of the independent and dependent variable which was reported as .01, with a robust standard error of .01, which was insignificant, and then another, perhaps in another paper, with a coefficient of .01, and a standard error of .01, and it was significant. So one

of them might be .0051 with a standard error of .0149, and be clearly insignificant, and one might be .0149 with a standard error of .0051 and be clearly significant. One of the independent variables was measured in thousands, so if measured in hundreds of thousands it would have three significant digits and no problem. But the point is that if they really thought that they needed to publish the raw coefficients, they should report enough significant digits so you can make some sense of it.

The other paper was, I think, in the last issue, on collective events. The table title said the dependent variables were counts, so I will take them at their word. Since they were doing negative binomial and talked learnedly of "n-lambda" as the thing they were fooling with, I am not completely confident that their predicted "mean" for the community area was a count. Anyhow, of the control variables (which I was trying to figure out so I could interpret their theoretical results and how important they really were) only one had its units of measurement reported. So four out of five such variables were meaningless except for their signs and number of asterisks ("robust asterisks," as a new concept in statistics). The one where the metric was given was "percent black," and it had a coefficient of .07 (so if they used proportion black, they would have had three significant digits--but that's not the point here). So I take it that means that if the mean community had 18 such events (calculated from numbers on a graph, combined with the number of community areas that was the N of the table), then as you go from the mean by 14 points, you would go to 19 community events, I guess. But the rest of the metrics are "an index developed from" answers on the number of your kin and friends living in the neighborhood, and so on, with no sensible metric given. I doubt if the metric was known, or would ever be used again, so actually we don't need it. But if we don't need it enough for them to tell us, why use the raw ones. The original reason for using the raw ones was that the variances may vary from one situation to another, and if our variables are real rather than relative, then we need raw ones. But the variances are built into the model.

It seems to me that if our journals would adopt two editing rules, it would save a lot of heartache.

1. If you use raw coefficients, change the units of your "index" or the number of decimals you print so that there are at least two significant digits in significant coefficients, and their standard errors.

2. If your metrics are so arbitrary that you don't think it worth while to give them in the paper, then use standardized coefficients where you have a continuous variable, and a normalized independent variable if your dependent variable is loglinear, or a count, or a rate or probability, or whatnot.

Centre of Research Excellence: Proposal

by Peter Davis

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You might be interested in a proposal that a group of us have put forward for a Centre of Research Excellence (CoRE) with a strong methodological emphasis (see below). The competition for CoRE status will be fierce, however! On a different tack, are any of you interested in having a symposium or workshop at the coming ASA on the management of research centres in the social sciences? I have attended the Chair's conference for a couple of years, and I thought that something like that for centre directors or similar personnel might be useful. Anybody interested?

1. Title of Centre of Research Excellence

Centre for Advanced Methods and Policy Application in the Social Sciences (CAMPASS)

2. Abstract

The CAMPASS mission is to establish excellence in advanced social science research for the betterment of New Zealanders. Using advanced methods of analysis, we will address questions that are fundamental to New Zealand's economy, society and culture. Advances in the power of computing and the development of analytical frameworks mean that techniques previously confined to the physical sciences can now be applied to complex social science problems. This proposal brings together people and analytical tools from across a range of

disciplines to provide new synergies and advances in social science research. Our research will address the government's priority areas of (1) economic transformation, (2) families young and old and (3) national identity. Its unifying thread will be the challenge of maintaining a balance between social cohesion and dynamic flexibility in the face of global change. Our Centre will develop the potential of advanced statistical and analytical methods to answer questions and solve problems. We will work with Maori, Pacifica and new settler communities to ensure the wider relevance of our research.

Organisationally CAMPASS is a synergy of three existing units – the Social Statistics Research Group (University of Auckland), the Health Services Research Centre (Victoria University of Wellington), and Motu Economic and Public Policy Research Trust. We will bring together researchers from diverse backgrounds including Economics, Engineering Science, Geography, Information Systems, Philosophy, Political Science, Psychology, Public Policy, Sociology and Statistics. While our primary focus is New Zealand, the work of CAMPASS will have implications of international significance.

We seek to map broad trends using systematic and innovative methods. While emphasising fundamental, cutting-edge and world-class research, we have linked our research programme to the Government's higher-level priorities. We are committed to informing policy, engaging with the community and with public and private sectors, and training a new generation of researchers.

3. Summary of Proposal

The Centre for Advanced Methods and Policy Application in the Social Sciences will be based at the University of Auckland, with collaborators at Massey, Victoria, and Canterbury universities. Another key partner will be the Motu Economic and Public Policy Research Trust in Wellington. The originating core of CAMPASS is the Social Statistics Research Group (SSRG) led by Professor Peter Davis at the University of Auckland, which currently holds a number of contracts for research in health policy and social science. CAMPASS will

build on the SSRG base by bringing together a much wider group of researchers from Economics, Engineering Science, Geography, Information Systems, Philosophy, Political Science, Psychology, Public Policy, Sociology and Statistics.

CAMPASS research will address the government's priority areas of (1) economic transformation, (2) families young and old, and (3) national identity. Under 'economic transformation', CAMPASS will focus on five sub-themes: (i) Globally competitive firms; (ii) World-class infrastructure; (iii) Innovative and productive workplaces, underpinned by education, skills and research; (iv) Auckland as an internationally competitive city; and (v) Environmental sustainability. Under 'families young and old' we will undertake a systematic mapping and modelling of different areas of human interaction at the level of the family, individual and community. We see these as forming the micro-foundations of society and are the basic social mechanisms that make society work.

Under 'national identity' we seek to explore the implications of a more diverse society than in the past for social cohesion in New Zealand. Using international datasets to situate our particular circumstances in an international framework, and drawing insights from theories of cultural evolution, we will model how national identity in New Zealand can be expected to evolve, and what may be needed to develop it. We will acquire new software that make possible wide-ranging computer searching for the dynamics of word choices in textual discourses relevant to national identity, scanning widely through documents containing elite discourses as well as popular 'talk' about identity.

CAMPASS will bring to bear advanced methods across all its research themes, taking advantage of developments in power of computing and new theoretical and methodological developments, particularly in modeling how individuals can be expected to behave in certain situations, and testing those models experimentally or by simulation.

Training will be fostered by a range of programmes strengthening quantitative skills, including postgraduate student scholarships, doctoral conferences and symposia, sponsored

conferences and workshops, short courses and publications. The School of Government at Victoria University will play a major role in promoting end-user knowledge and uptake of research. The researchers will use their existing links with government agencies and through teaching programmes to ensure that research findings are widely disseminated and to promote the uptake of research amongst policy makers.

CAMPASS will become a powerhouse of social science research, expertise, and training, a hub of debate and a source of policy options for New Zealand's development. Its research will address questions that are fundamental to New Zealand's economy, society and culture.

New Social Statistics Section

Dr. Michele Haynes, from The University of Queensland Social Research Centre (UQSRC), Australia, is the inaugural Chair of the Social Statistics Section of the Statistical Society of Australia. Michele is a senior statistician in UQSRC, with interests in Bayesian methods, generalized distributions and methods for longitudinal and hierarchical data structures. The Social Statistics Section will foster methodological research in applied social statistics, contribute to the dissemination of new methods to the social science research community and encourage substantive and methodological collaboration between statisticians and social scientists.

Semi-Retirement Announcement

Earl Babbie retired from active teaching in January 2006 but is remaining active as the Emeritus Campbell Professor in Behavioral Sciences at Chapman University. He plans to devote more time to writing.

Website Announcement

by Erin Leahy
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Methodology section members might be interested in this NIH-funded website:

<http://www.4researchers.org>

Presently the site is still under construction but it should be completed soon.

Books and Article Announcements

Forthcoming Book by Duane F. Alwin

Duane F. Alwin. (Forthcoming, June 2007). The Margins of Error: A Study of Reliability in Survey Measurement will be published in the Mathematics and Statistics Series, Survey Research Methods and Sampling section, John Wiley & Sons.

There is a little blurb about the book on the webpage below that describes the book's contents.

<http://www.wiley.com/WileyCDA/WileyTitle/productCd-0470081481.html>

Forthcoming Book by Glenn Firebaugh

Glenn Firebaugh. (Forthcoming, 2007). Seven Rules for Social Research. Princeton University Press. Seven chapters (one for each rule).

The book is intended for graduate students, practicing researchers, and undergraduates doing Honors Theses. The seven rules are:

1. There should be the possibility of surprise in social research.
2. Look for differences that make a difference, and report them.
3. Build reality checks into your research.
4. Replicate where possible.
5. Compare like with like.

6. Use panel data to study individual change and repeated cross-section data to study social change.
7. Let method be the servant, not the master.

Article Publication by Martha Foschi

Foschi, Martha. (2006). "On the Application-Files Design for the Study of Competence and Double Standards." Sociological Focus 39: 115-132.

Forthcoming Book by Lingxin Hao and Daniel Q. Naiman

Hao, Lingxin and Naiman, Daniel Q. (Forthcoming, April 2007). Quantile Regression. Sage Publications.

This is the first book in a two-book series and it establishes the seldom recognized link between inequality studies and quantile regression models.

Key Features:

1. Establishes a natural link between quantile regression and inequality studies in the social sciences
2. Clearly defined terms, simplified empirical equations, illustrative graphs, empirical tables and graphs from examples
3. Computational codes using statistical software popular among social scientists
4. Oriented to empirical research

Book Publication by Paul Voght

Earlier this year I published a text:

Voght, Paul. (2007). Quantitative Research Methods for Professionals. Allyn & Bacon.

The emphasis in this text is to provide workers in the applied social science professions (e.g., criminology, social work, education) with an overview of quantitative methods so that they can read the literature in their fields. The book is introductory in that it is meant to be accessible to those without extensive backgrounds, but it does not

deal only with elementary topics. A good range of advanced topics are covered, such as logistic regression, factor analysis, SEM, and HLM.

Call for Suggestions for Upcoming Book by Paul Voght

Along with my colleague from the University of Plymouth in the U.K., Malcolm Williams, I have contracted with Sage Publications to produce an edited Handbook of Methodological Innovation. This will draw from all traditions and disciplines in the social sciences. We welcome suggestions as to what constitutes a methodological innovation and what should be included in the handbook. Send ideas to wpvoght@ilstu.edu.

Notes from the Editor

I have changed the publication schedule to conform more closely with current practice: Winter (when you receive this) and Summer (late June). Submission deadlines are a few weeks prior to publication, but circumstances mandate flexibility. Despite repeated requests, submissions trickle in very slowly, so I have been postponing publication until there are enough to justify the effort. One alternative is to commission articles, but then this becomes my newsletter, not yours. I hope we can discuss this at the section business meeting in August. In the meantime, you can email suggestions or comments to me at L.raffalovich@albany.edu.

The section web page is here at SUNY-Albany (<http://www.albany.edu/asam>). Please give us feedback. As always, I solicit section news, news about section members, brief essays, and suggestions for process, content, and format.