

Qualitative Methods

Newsletter of the
American Political Science Association
Organized Section on Qualitative Methods

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Letter from the Editor

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The *Qualitative Methods* newsletter is one year old! It seems like only yesterday . . .

Volume 1, Issue No. 1 (Spring 2003) focused on the practical task of teaching a qualitative methods course. It featured retrospective evaluations by a number of instructors and an extensive canvas of qualmeth syllabi and textbooks. Volume 1, Issue No. 2 (Fall 2003) focused on interpretivist approaches, and included contributions from Robert Adcock, David Dessler, Clifford Geertz, David Laitin, Laura Stoker, Dvora Yanow, and myself. (Following protocol, past issues will be available to non-section members one year after publication. Thus, Volume 1, Issue No. 1 is now available on-line on the CQRM web site at <http://www.asu.edu/clas/polisci/cqrm/QualitativeMethodsAPSA.html>. Volume 1, Issue No. 2 will become available this Fall.)

The current issue takes on two challenging topics, field research and content/discourse analysis. The field research symposium, with contributions from Marc Howard, Evan Lieberman and Julia Lynch, is the culmination of a series of seminars led by the authors at APSA and IQRM meetings over the past several years. It commits to print, for the first time, the practical, hands-on wisdom imparted in these valuable sessions.

The second symposium, organized by Yoshiko Herrera and Bear Braumoeller, addresses comparisons and contrasts between two research methods directed to the analysis of textual evidence – content analysis and discourse analysis. Since the material of politics is often textual in nature, it behooves us to consider these methodological issues carefully. What does it mean to analyze a text? What assumptions are required? What is the most appropriate method(s)?

Plans for future issues of the newsletter are beginning to take shape. If all goes as planned, the Fall 2004 issue will include three symposia. The first, on Qualitative Comparative Analysis (QCA), will include contributions from Andrew Bennett, James Mahoney, Charles Ragin, Benoit Rihoux, and Jason Seawright. The second, on the application of economic models to the study of politics, will include a lead essay by Sanjay Reddy, along with various responses. The third, on the

pitfalls of qualitative methods, will include a lead essay by Gerardo Munck, together with several commentaries.

The newsletter has been, and will continue to be, responsive to section members' suggestions and contributions. We continue to look for suggestions for future symposia topics. We also welcome short responses to already-published symposia, which will either be printed in the newsletter or posted on the newsletter's web site. The newsletter is also looking for broad state-of-the-field book reviews to publish in later issues. As always, I solicit your suggestions for published work to include in our annual Book Notes and Article Notes, which will appear regularly in the Fall issue.

In other news, the third annual Institute for Qualitative Research Methods (IQRM) was held at Arizona State University from January 5-16. The 2004 institute hosted 93 attendees – the largest group to date. IQRM's parent, the Consortium for Qualitative Research Methods (CQRM), is now supported by over 20 subscribing institutions, as well as by a generous grant from the National Science Foundation.

The Qualitative Methods section continues to grow. Membership has now topped six hundred, qualifying us as the tenth largest of APSA thirty-five organized sections. Consonant with its increasing size, the section will be sponsoring twelve sections at Chicago this September. (For a full list of panels and roundtables, see page 39.) Please note that our panel allocation for APSA 2005 will depend in large part on attendance at our panels in Chicago. So, please come to as many panels as you can.

By way of conclusion, let me take this opportunity to thank you for supporting the newsletter with your contributions and your annual section dues. I also want to acknowledge the generous support of CQRM and Boston University in under-writing our production and publication costs.

Please feel free to circulate this issue of the newsletter among friends and associates. You might also encourage them to join the Qualitative Methods section!

Symposium: Field Research

Between 2001 and 2004, the three authors of this symposium presented a "short course" on the topic of managing field research to a variety of audiences, including at the annual meetings of the APSA in 2001, 2002, and 2003, as well as at the Institute for Qualitative Research Methods in January 2003 and 2004.¹ This series of brief articles highlights the "strategies" we have discussed during those courses. Our contributions are primarily intended to be useful for doctoral students preparing for field research. These suggestions are largely based on our own experiences with field research: Howard in Russia and Germany (1997-99)²; Lieberman in Brazil and South Africa (1997-99)³; and Lynch in Italy and the Netherlands (1998-2000), as well as conversations with various colleagues, and suggestions and comments from our various short courses.

Introduction: The Promise and Pitfalls of Field Research

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Overseas field research can be an extremely valuable tool for gathering data and theoretical insights for political science research, particularly in the sub-fields of comparative politics and international relations. In addition to making what Brady, Collier and Seawright (2003: 9) describe as "data-set observations," which generate scores on independent and dependent variables, field researchers may be particularly well suited to make "causal process observations," which shed light on "context, process, and mechanism, provid(ing) insight into the relationships among the explanatory variables, and between these variables and the dependent variable." Learning about a particular society and polity from close range allows scholars to carry out "reality checks" for various theories through active engagement of people, places, and contexts. Scholars engaged in intensive field research may generate new theoretical insights based on fresh views of social and political processes which have not been previously captured by other social scientists. They may also use field research as an opportunity to determine if their research will have any broader relevance for

the societies under investigation.

Along with such promise, field research presents a host of practical and logistical problems which make it a risky endeavor in terms of potential costs of time and money. Because there are few formal courses in field research for political science graduate students, information about how to address standard and recurring problems is exchanged anecdotally and unevenly. Our articles for this newsletter are intended to begin to fill a gap in methods training by focusing on the problem of self-management in field research-based projects and by offering a set of extremely practical observations and suggestions, with few pretensions of discussing high-level theoretical issues. Undoubtedly, our suggestions are grounded in our own views of what constitutes "good" and "doable" social science research: a middle ground between barefoot empiricism and highly deductive hypothesis-testing. Along these lines, we view fieldwork as a research strategy that seeks to accomplish two key objectives: It is both an opportunity to collect data in order to explore specific hypotheses, and a strategy for inductively deriving new propositions and new understandings about various social and political processes.

In our articles, we discuss how to address the challenges of gathering the right amount of data, in a usable form, in a reasonable amount of time, at an affordable cost, and without losing one's sanity. There are no panaceas, and field research is inevitably a highly personal process not amenable to any cookie-cutter approach. Nonetheless, we believe that through

conscious self-management, data collection in field research can be carried out more efficiently and effectively. By highlighting potential tradeoffs of various strategies, we hope that scholars will make informed decisions about logistical and other concerns that can have far-reaching consequences for the quality of research that is eventually produced, given time and financial constraints.

It is important to note that in this series of articles, we do not discuss specific techniques of research design, causal inference, survey design, interviewing, historical research methods, participant observation, ethnography, etc., because much has been written on this elsewhere.⁴ Rather, we are concerned with the issues that may come between such components of a research design and actual research execution. Our goal is largely to raise issues, and to suggest tradeoffs in various approaches that can help to mitigate or to avoid such practical problems. Moreover, we are not the first to write on logistical and practical concerns. For example, the contributions to the edited volume by Devereux and Hoddinott (1993) provide a series of important discussions about local-level field research that may be of interest to political scientists working at the village level and/or working on household surveys.

Similarly, Barrett and Cason (1997) also discuss many of the practical and logistical challenges associated with field research, particularly through the use of anecdotes and tips from various scholars. Indeed, many of our suggestions resonate with the advice proffered in these volumes, but our intent is to present a unified management strategy, updated for current trends in political science, and available technological tools.

It is our conviction that field research-based projects present significant management dilemmas. Such projects—especially when carried out as part of one’s doctoral research—are typically multi-year, multi-method endeavors that cost tens of thousands of dollars. Even if one only spends a few months in the field, the time span from grant application to the completion of analysis and writing is rarely less than a year. In other fields and professions, related work tends to be supported with much greater institutional resources. Social science, however, emphasizes scholarship carried out by lone individuals—particularly at the stage of doctoral research—and resources are more limited. As a result one is forced to take on the roles of project manager, administrator, budget comptroller, and data archivist, in addition to being a researcher/scholar. In the later stages of one’s career, this may be less true, as a proven track record can help one to gain access to greater institutional resources for field research, but especially for graduate students, and even for most scholars, self-administration is a central part of field research.

We suggest a set of “ideal” strategies for dealing with very practical issues that may only be achievable in part, but we hope that they may be useful for others in managing their projects. Lieberman discusses a set of strategies useful for preparing for field research; Howard discusses strategies associated with obtaining and recording data; and Lynch discusses the need to keep track of progress while in the field in order to ensure that one is gathering the correct data, and

within an acceptable time frame.

Notes

¹In 2002, Lauren Morris McLean and Benjamin Read co-taught the course with Howard and Lynch.

²Now published as Marc Morje Howard. 2003. *The Weakness of Civil Society in Post-Communist Europe*. Cambridge, UK; New York: Cambridge University Press.

³Now published as Evan Lieberman. 2003. *Race and Regionalism in the Politics of Taxation in Brazil and South Africa*. Cambridge, UK; New York: Cambridge University Press.

⁴Indeed, other disciplines and sub-disciplines within political science have had more to say on these topics than scholars in comparative politics and international relations. These works are far too numerous and extensive to list here.

Preparing for Field Research

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Preparations for field research go well beyond the tasks of obtaining funding and purchasing a plane ticket. In order to maximize the benefits of one’s time abroad, a great deal of advance planning is necessary and should be carried out for at least six months prior to an extended trip. Given personal, professional, and diplomatic constraints, time in the field is finite and valuable, and there are several strategies that can be used to increase the likelihood of returning home with the desired data and insights, which were the objective of the journey in the first place.

Don’t Save for the Field What Can Be Done at Home

Because field research is an iterative process in which new findings lead to new inquiries, it is impossible to prepare for everything. But, to the extent possible, all research that can be done at one’s home institution should be carried out prior to departure in order to improve the quality of the work that gets done in the field. For example, extended time searching internet resources while in the field—often at quite slow dial-up speeds—is a true waste of precious field time, since such work obviously could have been completed prior to departure. Depending on the countries being studied, an enormous amount of information may be available on the internet, and it is often easier to gather (relatively recent) newspapers, government documents, official statistics, many survey data sets, and other information on the internet from an American university connection. Similarly, many foreign documents from the pre-internet era are available in American research libraries. Importantly, the names and contact information of individuals at various organizations may be available on the internet, and to the extent possible, it is useful to try to gather all of this information, and to begin contacting people to arrange meetings or to learn how to obtain materials even be-

fore arriving at the field destination. Similarly, it may be possible to conduct electronic searches of foreign libraries while still at home, which would allow one to arrive at a library or archive with a prioritized set of requests. There is no special value in sitting at a computer terminal in a foreign country, if the same work could be done at home.

As more and more information becomes available to us through advanced communications technologies, it is useful to keep a focus on what will be available only through in-person contact. This may include the specific attitudes and histories of individuals within a country (to be gathered through interviews), special primary sources (to be gathered in libraries or archives), or nuanced information about norms and language (gathered through everyday contact with individuals and/or through more deliberate ethnographic approaches) that may shed light on other sources of data. For scholars working on areas that have been thinly researched, and that are “unwired,” it will be necessary to spend more time in the field, as a much larger proportion of essential data will not be available from alternative sources.

Timing and Duration of Field Research

In preparing for field research it is important to reflect upon the ideal duration and timing of travel in conjunction with one’s stage of research, including the state of theoretical debate around the question at hand, as well as one’s own empirical knowledge and local skill-set. Field research trips may serve various purposes and it is useful to keep these in mind. One class of field research is the “fishing expedition,” which involves the search for a specific outcome of interest or puzzle to explain, or attempts to identify methodologically and logistically sound sites for research. On such a trip, the scholar may work to identify professional and personal contacts, to become familiar with the language and culture, and/or to develop a researchable project to be executed during a future trip. As is well known, funding agencies tend to be far more amenable to proposals that have already completed such initial research. Even at this less structured end of the continuum, preparations for interviews, meetings, and/or archival visits are critical in order to engage the types of conversations and observations that can lead to new ideas.

By contrast, a field research trip may be extremely focused, and the central objective may be to gather very specific forms of data about well-defined variables and observational units. At the extreme, one may manage and/or execute a closed-ended survey project, which involves the execution of a tightly formulated survey instrument in a foreign location.

In between, many or even most political science field research trips are neither completely open-ended nor completely closed-ended. To borrow the phrase from the literature on interviewing techniques, such field research tends to be “semi-structured.” The goals of such trips include the identification of specific data items, through the systematic execution of a research design, in addition to an open-ended component of the trip, in which leads developed during early

stages of the research are pursued during later stages.

Particularly in this last instance, scholars must decide whether to carry out their field research as one long trip, or as a series of short stays. The benefits of a single, long trip include the continuity of logistical arrangements, not needing to finance additional overseas plane trips, greater opportunities for integration within various communities, and less pressure to make every day “count.” A long stay—which in political science tends to be a year or more—may be desirable for all sorts of personal and long-term professional development reasons, including developing language skills and contacts. On the other hand, shorter trips may be less disruptive to one’s life back home, and may prove more efficient if one prepares an extremely tight schedule. In the latter case, advance planning is particularly crucial, because one wants to walk off the plane with a very clear schedule of meetings and tasks in order to focus on “content” for the duration of one’s stay.

It is true that many grants, such as those from the Social Science Research Council or Fulbright, are offered with the expectation that scholars will spend 9 or 12 months in a country, but this does not imply that one should spend every minute of that time gathering new data. As Lynch discusses, a good portion of that time needs to be spent on analysis and perhaps even writing.

For multi-country (or multi-site) studies, it may not be necessary to spend the same amount of time in each place, even if each location counts equally as a “case” in one’s analysis. (In fact, all three contributors to this symposium carried out research in two different countries for our doctoral dissertations, and we each spent varying amounts of time in our respective field sites.) It may make more sense to spend more time in places with greater logistical challenges, where one’s own language skills and/or personal contacts are weaker, where there is a less extensive secondary literature, or where one hopes to develop lasting contacts for the future. Moreover, one may find during the process of gathering data for the first country of a multi-country study that many of the types of “mistakes” and dead-ends that tend to be encountered during the earlier stages of research can be avoided in field research trips to additional countries, reducing the total time needed for executing one’s research design.

Translate a Research Design into a “To Get” List

Field research can be likened to a giant shopping expedition for various types of data. Like other shopping trips, preparation can ensure that most of the needed and/or desired items will be acquired. Of course, exposure to a wider selection of items when one arrives at the store, or the discovery that certain items are no longer in stock or are over-priced can force a complete rethinking of the week’s menu. While the uncertainties of the market suggest the need to be flexible, without any forethought about one’s needs, it is all too easy to return home with many delicious items that looked great at the store, only to discover that key items went unpurchased simply because they were forgotten/not anticipated, or because of lack of time and/or money to purchase them.

If this analogy holds, we think it is extremely useful to convert one's research design into a very extensive and highly operational "to get" or "to do" list. In most cases, a research design that might have been submitted as a dissertation prospectus or funding proposal says something like "Interview top 40 business elites" or "Examine 1970s archives." One needs to move from such generalities and get very, very specific, breaking down every component of the research design into discrete items of data that can be gathered and that can be stored away in a retrievable "data container" that will be readily accessible at a later time. This means identifying, to the extent possible, each person that would be desirable for an interview or meeting (even if identified only by title, and not specific name), every place to visit, and when, in order to begin to organize one's schedule and travel within a country. To the extent possible, it is desirable to arrange interviews and other types of meetings and library visits in such a way that each day's activities are confined to a manageable area.

The more specific and more concrete one's "to get" list is before leaving for the field, the more successful the time in the field is likely to be. There is a big difference between a plan to interview "a mid-level government bureaucrat in capital city," and a plan to interview "Mary Rodriguez, director of human resources, department of finance, in Santiago, Chile on April 7 at 2pm; Call to confirm day before with her assistant." It can take weeks and months to track down the names of appropriate people to meet and to confirm appointment dates, and to the extent that this can be done via electronic mail, regular mail, or phone prior to departure, one's daily life in the field will be much more productively oriented towards content, rather than more mundane logistical work.

In a similar manner, if the use of archives or libraries is demanded by one's research design, it is worth estimating the amount of time that will be required to carry out the anticipated research. It may be useful to ask about the extent of the materials available on a particular subject, and if they are available on-site or if they will be ordered. In addition, it is worth inquiring about any special rules or regulations about the use of computers and pens, or the need for letters of introduction or affiliation. More generally, various "official-looking" letters of introduction, written on university letterhead, can come in handy as a passkey to get through bureaucratic snafus.

There are larger psychic rewards to making a thorough "to get" list: It can help one to envision actual completion of one's research. In the absence of a relatively finite list, one's research project can become an ever-expanding black hole that will make it impossible to experience completion. This list can help one to measure progress during long periods without much feedback (though, as Lynch discusses, soliciting feedback should be an important component of the field research experience).

Moreover, if the "to get" list appears truly overwhelming prior to departure for field research given available time and resources, then one's research design is clearly in need of revision, and it is better to learn this sooner rather than later. Just as problematic as gathering insufficient information is the threat of data overload. It may be tempting to endlessly

gather information in a "scorched earth" strategy in the absence of a plan for what's actually needed. Careful planning will facilitate the appropriate acquisition of materials.

Develop an Information Management System

Even relatively short field research expeditions can generate vast quantities of information, which are likely to arrive in different forms and shapes:

- One may conduct *interviews*, and wind up with dozens of cassette tapes and hundreds of pages of notes and transcriptions from the taped interviews.
- One may carry out *archival* research, and arrive home with hundreds of pages of handwritten or typed notes, and perhaps boxes of copies.
- One is likely to stumble upon useful new *secondary sources* that were not discovered or available prior to your departure.
- One may obtain various *data sets*—either in printed form or, hopefully, in electronic form. These may be surveys others have conducted, national accounts data, or other collections of data that may be useful.
- In the course of casual conversations with people, reading the newspaper, and simply looking around, one is likely to make a set of *observations* that may shed light on one's analyses of the place/society/polity under investigation. Whether one is doing rigorous participant observation, or simply taking note of something that appeared interesting or revealing, such observations can later be extremely valuable in demonstrating the validity of one's more systematic analyses.
- Finally, most field researchers wind up with loads of—for lack of a better label—*miscellaneous documents*. These are brochures, song lyrics, annual reports, and various other printed matter or ephemera that may be useful that come one's way during the course of one's stay. Some of these may turn out to be useful as office decorations, but oftentimes, random stuff helps to capture exactly the point one wants to make in a way that could not be gleaned from a survey, and such documents are exactly why field research can be a gold mine of revealing data.

While it may be exciting to think about these various sources of field-based data, one should anticipate that by conducting field research, an enormous volume of physical and electronic documents will be amassed. As a result, it is extremely important to develop a thorough *information management system*. Frankly, there is no point in going to all the trouble of finding data, if such data are impossible to retrieve when needed.

Every time a report is obtained, an interview conducted, or an archive combed, there should be a clear place for this information to be stored. There is simply nothing worse than returning from the field with loads of unorganized boxes marked "dissertation" or "research project." It will take at least twice as long to sort through those boxes, and much more likely, one will simply never open most of them, rendering the fieldwork itself pointless. In my own case, a great many documents went un-filed from my doctoral field research, and even after publishing my dissertation as a book, those

documents have remained largely unexamined! The point is not that every item of data gathered needs to be incorporated into one's analysis, but that such non-incorporation should be a willful decision and not simply a product of disorganization.

The development of an information management system may involve a variety of strategies, but I believe it is useful to:

1. Make parallel electronic and physical filing systems. That is, develop a logic for sorting all items and documents that can be applied both to hard copies and objects as well as to one's computer hard drive. For example, create main folders or boxes with the top-level categories of "administration" (for letters, itineraries, etc.), "data" (for specific observations and information gathered), "analysis" (for outlines and summaries of the data) and "papers/output" (to store chapters and conference papers).

2. Keep track of every data item with a master index, preferably by using a bibliographic software package, such as Endnote. You will thank yourself later when it comes time to actually create a bibliography if you enter this information religiously along the way.

3. Keep track of contacts in a systematic manner. Most scholars will contact dozens, if not hundreds, of individuals while in the field. While this information may be easily stored in a notebook, there are several more "high-tech" strategies that are likely to be more useful. By creating a spreadsheet or other database, one can quickly search and sort contacts, helping to manage one's schedule and keeping track of leads for interviews. Also, one can back up this very important information quite easily—the loss of such information would be devastating. When an interview has been completed, most word processors contain "mail merge" functions that can facilitate sending out thank-you notes later. Indeed, this is an important norm to follow given that many people in foreign countries spend an inordinate amount of time with American researchers with no tangible reward.

At a minimum, the following information ought to be gathered and stored:

- Name
- Address
- All relevant phone and fax numbers, email addresses
- Names/numbers of assistants
- Dates when contact was made
- Information about messages exchanged
- Information about when and how to follow up
- Date of interview/meeting
- Anecdotes/comments about the contact
- Information about need for post-interview follow-up
- Whether or not a thank-you letter was sent

The contacts one makes in the field may be useful not just for this project, but for future work, and it is a good idea to keep track of as much anecdotal information as possible; for example, by systematically recording which items various individuals promised to send the field researcher or that the field researcher may have promised to send to them, one can ensure that these transactions actually take place. Also, it is nice

to recall, was this person helpful? Friendly? Would you want to contact them again? Such information can prove quite valuable for future research projects. Depending upon the nature of one's contacts, and agreed-upon human subjects protocols, it may be necessary to password-protect such information in order to maintain confidentiality and/or anonymity.

Use Technology to your Advantage

Given the pace of technological change, any recommendations for electronic tools are likely to become dated in short order, but I will make a few suggestions nonetheless.

First off, at this point in time, a laptop is basically essential for field research, and it goes without saying that there is no better insurance policy than backing up. It is imperative to bring some type of backup solution to the field—which may mean sending files back home electronically, and making CDs or Zip disks or using USB keys, which can store quite a bit of information—but don't pack those in the same bag as the laptop, so that if the bag is lost or stolen you won't have lost everything.

Second, where service is available, a cellular phone is extremely useful for making and confirming appointments, and for safety. While international cell phones are available in the United States, the call rates tend to be geared towards business travelers (i.e., extremely expensive), and one winds up with a U.S. phone number, forcing people to make international calls in order to reach you. Instead, it is best to wait until arriving in the country of destination, and to buy a cell phone that will allow the use of pre-paid phone cards—which is increasingly an option in the many countries that use GSM technology. In recent years, prices have become extremely reasonable. Again, it is always important to remember that one's time is valuable, and any opportunity to confirm meetings before arriving to an empty office—where your contact forgot about your meeting or was called away at the last minute—will be critical to productivity and sanity.

To the extent that one's budget allows, other electronic items can be very useful and may save you either time or money in the long run. Personal digital assistants (PDAs) are increasingly inexpensive, and allow for quite a bit of portability with contact lists and other information that you may want with you at all times. It is now possible to purchase collapsible keyboards designed to work with PDAs, which can be quite useful for going into libraries to take notes rather than taking around a bulkier laptop, which might be more vulnerable to being stolen and is likely to have much shorter battery life. Portable printers and scanners can also be very useful as you set up a mobile office in libraries and other places.

A good-quality tape recorder or digital voice recorder may be necessary for recording interviews, but can be very useful even if you're not. It's difficult to write or type when on the road, and it's nice to be able to quickly record any ideas or observations you may have. It is important to test these out before departing, and to keep checking that they are working well, because few things are more frustrating than finding out later that notes and interviews are incomprehensible because the sound was garbled. A benefit of digital voice recorders is

that one can easily create backups and duplicate copies of the recording, including those to be used for transcription.

Budgeting and Prioritizing Time and Resources

Finally, in the area of preparations, it is critical to take one's "to get" list, and to start setting priorities for what needs to be done in the field even before leaving. Given the various personal and real-world snafus that one is likely to encounter, chances are that one's initial "to get" list can turn out to be overly ambitious, even when one thinks one is being realistic. As a result, it is important to start out knowing what is top priority and what is simply "gravy" when it comes to gathering data.

Quite understandably, most funders require a budget as part of the grant-writing process, and as a result, most scholars will have constructed some type of budget well in advance of departure for the field. However, such budgets are generally constructed in very general form, with a dated exchange rate and purchasing power. (As someone who regularly conducts research in South Africa, where the purchasing power of the U.S. has fluctuated wildly in the past four years, this is quite a significant issue.) As a result, it is very important to update one's budget with more realistic estimates of spending needs based on a detailed "to get" list. One should not feel guilty if exchange rate windfalls allow one more spending power than originally planned (because on the flip side, most funders will not revise grants upwards for exchange rate punishments). In the face of hard budgetary constraints, the question for the researcher involves selection of a life strategy:

- Will you take buses or taxis?
- Will you hire a research assistant?
- Will you pay someone to transcribe your interviews?
- Will you allow time and money for exploratory travel to consider novel hunches or to gather materials for a future project?

Each item of the "to get" list implies a budgetary estimate in terms of time and money. Once tallied up, one may find that the time and money required is more than you have. If so, there are two choices:

- Revise the research design to a manageable—i.e., completable—project, given your budget constraints
- Revise the budget constraint, either by obtaining additional resources from funders or by using personal resources. In most cases, the first option is probably the better one at least initially!

Obtaining and Recording Data

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The main priority of field research—whatever the topic or country involved—is to acquire data. In doing so, or in attempting to do so, researchers are bound to face all kinds of questions, conundrums, obstacles, and roadblocks that will inevitably arise and complicate their research. In this article, I will present some strategies that can help researchers make informed decisions about how to deal with the difficult challenges and trade-offs of field research, so that they can obtain and record data in a way that best suits their particular projects and constraints. My purpose is not, however, to provide "set" answers that will apply to everyone, since each person's project, situation, and personality will, of course, vary quite widely. In fact, in order to be successful, field researchers need to be creative, crafty, and able to adapt to unforeseen situations—indeed, that is the very beauty and fun of fieldwork. In other words, while there is no simple "formula" for how to obtain data in the field, my main recommendation is for researchers to think through the potential problems and trade-offs in advance, and during the *early* stages of fieldwork, so that they will make choices that are wiser, more efficient, and more practical in the long-run.

Set Concrete and Realistic Goals

When conducting fieldwork, any researcher is bound to face numerous and often quite tempting distractions. These may include getting "settled" into domestic and social routines, becoming acclimated to a new research environment, and trying to figure out how best to implement one's social scientific research design within a complex real-world setting. Some adjustment period is of course understandable—particularly for scholars who intend to spend a full year in the field, and who may be struggling with a foreign language—but upon arriving in the field, it is important to develop some kind of an organized research plan from the very beginning. Of course, all people have different *styles* of organization, but the main thing is to know what is in store for the next day (or week), so that, as much as possible, one can avoid the scenario of not knowing what to do next. This does not mean, of course, that researchers should *always* be working, but when they are working, it is important to stay focused on the next steps.

In his article, Lieberman presented the idea of a master "to get" list for any research project. But, of course, one cannot work on everything at once on a daily basis. Rather, researchers need to break down their research objectives into smaller, more concrete, and specific tasks that they will then tackle on a daily or weekly basis. For research involving interviews, for example, one should plan out a schedule for writing letters to potential interviewees, following up (whether by phone, fax, or email), scheduling the interviews over a certain period of time, and of course drafting the questions or questionnaire. These types of seemingly basic tasks may take days, if not weeks, to

accomplish.

The best way to keep track of what will soon become very complicated planning is by maintaining “to do” lists of one’s schedule and upcoming tasks. Depending on personal style, some people may prefer daily, weekly, or biweekly lists. It does not necessarily matter if the list is stored in an electronic organizer or scribbled on a little piece of paper—but a personal organizer certainly allows for better back-up and safe-keeping, in case those scraps of paper get misplaced or lost! Either way, the main point is that researchers should wake up each day with a plan for what they are going to do—for that particular day and for the days ahead. Given how easy it is to lose track of things, and to get distracted by a new environment, a “to do” list will help to provide focus and enhance productivity.

It is also important to allow time for logistics and planning, something that few people really consider at first when constructing a schedule. Even if a researcher has followed all of Lieberman’s suggestions for preparations before leaving, there will certainly be a great deal of new, and unpredictable, logistical hassles on the ground, as well. These may include following up with people and institutions upon arrival, adapting to conditions and situations that were far different from what one expected, or having to devote much time and energy to negotiating one’s way into particular data sources. It is important to recognize that some logistical frustrations will arise, and to actually plan this into a daily or weekly schedule—rather than panic later on, when one obstacle or another inevitably leads to delays. There will always be set-backs and problems, but a little bit of foresight and planning can help to avoid or mitigate major logistical disasters.

Finally, although I am focusing on methodological (not personal or social) elements of fieldwork, on the topic of time management, I should also mention the importance of allowing for some down time and relaxation. Too many people get burned out, and stress themselves out, without necessarily being more efficient. In many cases, one can actually learn more about another country by relaxing there a bit, and that “down” time may even lead to some valuable research insights. And the friendships and networks that develop during the fieldwork experience will probably help to improve any given project, not to mention other projects down the road.

Anticipate Everyday Trade-Offs

As anybody who has conducted research abroad knows, research money can disappear very quickly. One should therefore think about some trade-offs at the early stages of fieldwork. Although I cannot provide generic answers, here are some questions to consider:

- For a project that includes some form of archival research, how will information be collected? Photocopying, scanning, and old-fashioned note-taking all have distinct advantages and disadvantages. Photocopying is, of course, very accurate, but it can also be extremely time-consuming and costly, and the paper produced can be quite heavy (and therefore expensive to ship home). Scanning is also very accurate, and it is certainly less costly and heavy than copying, but both of these technologies can lead to “information overload,”

where the researcher just keeps copying or scanning, with the hope that the information will be useful down the road (or with the fear that something valuable might be missed). And of course note-taking (whether on a computer or with pen and paper) can be time-consuming at first, but if the person is well-organized, it can be very efficient. Either way, researchers would be well-served by deciding early on which technique will work best for them, given their personal style and the nature of their research project. Moreover, as Lynch discusses in her article, whatever method is chosen, it is essential to record one’s initial reactions to the documents, since these can be just as valuable as the material itself.

- For research that includes interviews, one trade-off to consider right away is whether or not to tape record them. Researchers need to weigh their need for completeness and accuracy against the possibility that a tape recorder will make a respondent uneasy, too cautious, or speak like a bland propaganda statement. Tape recording may be more appropriate for interviews with ordinary citizens—who have much less to hide or fear—than for elite interviewing, but either way researchers should be forewarned that listening to the tapes can be quite unwieldy and time-consuming, and transcription costs are often extremely high. In my case, for my doctoral research, I did record and transcribe my interviews (which were with ordinary citizens), since I drew upon a number of the exact quotations of my respondents, and this analysis constitutes an essential part of my book. But I would suspect that for most political science dissertations, the costs of recording and transcribing may not be worth the payoff.

- Many researchers consider hiring a research assistant while in the field. This may be a strange feeling for graduate students, who are often RAs themselves, but as the head of a major research project, it might actually be entirely reasonable (and cost- and time-efficient) to invest in some help during fieldwork. It will obviously depend on the location of the field research, since in some countries the cost of hiring an assistant may be prohibitive. But in other locations, it might be a plausible option that is worth considering, and an RA could help with a number of possible tasks, such as helping to translate interview questions or archival documents, helping to arrange appointments, providing an entrée into a local community, or tabulating and coding data for a content analysis. If one does decide to hire an RA while in the field (or, for that matter, as a professor later on in one’s career), it is important to define the RA’s tasks very clearly, to maintain regular contact with that person, and to oversee his or her work and progress closely. Otherwise, if the results are of poor quality, all of that work, time, and money may be wasted. Although I personally had a very good experience with an RA in Russia, I know of many others who have had more mixed results.

- Sub-contracting a survey is a relatively high-cost endeavor, which most people only do if they have a separate grant for it, but it may not be prohibitively expensive, especially for those who would consider buying a question or several questions off of a weekly or monthly “Omnibus” survey. In fact, many survey organizations that conduct polling for newspapers and businesses actually have special prices for

academic research. In my case, thanks to a small grant from the NSF, I was able to subcontract a series of questions on a survey (at “negotiated” rates—especially in Russia), the results of which provided multi-method complementarity, and which eventually constituted an important part of my book.

Develop a Strategy for Gaining Access to Data Sources

In these articles, we are not focusing on the specifics of interviewing techniques or archival research—for which there are already larger, well-developed literatures on which to draw—but I do want to emphasize here the fact that it takes a tremendous amount of work simply to gain access to one’s data sources. This applies whether a researcher is trying to schedule and carry out interviews with elites, to make ordinary citizens feel comfortable telling their life stories, to get a librarian at an archive to grant special access to restricted and sensitive documents, or to make an arrangement with a survey institute to ask a question on a survey.

In order to develop specific techniques for any of these types of tasks, I would strongly recommend talking to other experienced scholars about their own experiences, in order to receive advice and tips that will fit with both a particular research project and the location of the fieldwork. Much of this can be done before actually leaving for the field, which can also be helpful over the longer term, as one integrates into a network of scholars who work on the same countries or region. But researchers should also seek out other colleagues and journalists who are on-site, and who may have valuable suggestions and insights.

It is also important to think about appropriate ways to sequence one’s research in order to get the most out of each source. For example, it often makes sense to speak with low-level bureaucrats before high-level bureaucrats, in order to avoid asking silly questions of the individuals who possess information that cannot be gotten elsewhere, and who are likely to offer researchers only a very limited time to meet. Moreover, it is usually a good strategy to plow through printed materials before interviewing too many people, again, because one may find that many of one’s questions can be answered ahead of time, and it is easier to probe more deeply with interview subjects given prior information about background. Given the time constraints that interview respondents have, it is best to avoid wasting their time asking questions that could easily be answered with other sources.

Overall, a major challenge of field research is showing people why it is that they ought to be providing their time and help. Many people—whether elite or ordinary citizen interview respondents, archivists, or employees of an institute—are extremely generous and helpful, in ways that often exceed a researcher’s expectations. But it is important to remember that making them want to help takes a fair amount of work and effort, and it is critical to the success of the overall project.

Consider Interacting with Local Institutions and Scholars

One of the most valuable and rewarding elements of fieldwork can come from interactions with local scholars and institutions. But there are definitely both advantages and drawbacks

to these interactions, and it is important for each researcher to think through what will work best in a given context.

If possible, it helps to become affiliated with a local research institute. This often allows photocopying and printing privileges, and sometimes office space, but also the opportunity to become part of an organization and intellectual community, to discuss and develop one’s ideas and sources with other scholars, and even perhaps to acquire some added local status while in the field. And in most cases, foreign institutions will be very happy to have a visiting researcher come for a year, especially if it does not cost them anything financially.

There are also some potential downsides, however, such as the expectation that the researcher will come regularly to the office, and perhaps also read and comment on the work of others, which could be distracting and time-consuming. Another potential drawback of an affiliation—which could be relevant in certain contexts—is the risk of losing one’s neutrality by becoming affiliated with a partisan institute, or one with a certain reputation. In other words, researchers should not *automatically* rush to become affiliated just for the sake of it, but rather only if they think it will really help, which it often does.

Field researchers will also want to consider some other potentially tempting offers that may come up, and to think about whether certain opportunities will be useful for them. They may, for example, be asked to give a formal presentation about their work, to teach a class, to write an article while in the field, or even to do some outside consulting. These can obviously be tremendous opportunities that are both very flattering and gratifying, but it is important for researchers to make sure that they do not get too side-tracked by them. In some situations, it could also be problematic for a researcher to broadcast her views and analyses too widely, at the risk of future respondents already forming an opinion about her research, before she has had a chance to hear their own views. Moreover, if a researcher makes a presentation, writes an article, or undertakes some service that is far from his topic and expertise, he may lose valuable time. In fact, some people get so wrapped up in these types of activities that they never leave the field! In any case, the main point is to know in advance that these kinds of situations may well come up, and to think through what is, and is not, worth undertaking.

Be Prepared for Uncomfortable Situations

Fieldwork is usually a positive experience, but many field researchers do encounter some uncomfortable situations at one point or another, and it is best to be prepared for them, as much as possible. A researcher’s gender and race can obviously influence how people perceive and interact with her, in any number of ways, depending on the context. But by thinking through how such situations might come about—particularly by talking to other colleagues who have field research experience in the same or similar countries and contexts—and considering what would be an appropriate response, one can help to minimize their effect on one’s research, as well as the unpleasantness itself. I am not suggesting that researchers should have a scripted or prepared response, but rather that

they learn to expect the unexpected in potentially awkward or difficult situations.

For those who do encounter sexist, racist, or otherwise offensive behavior while conducting their research, what colleagues who have encountered these types of situations suggest is that the most important thing is not to get overwhelmed, not to get rattled, and certainly not to let it alter one's self-worth and self-perception as a professional. At some point the researcher will have to make a judgment call. For example, if a female researcher experiences some form of sexual harassment, or is subjected to offensive remarks, should she walk out of the interview? Or how much should someone relax his principles by keeping quiet (or nodding his head) in an interview with a neo-fascist? I cannot tell people how to react if they are confronted with an uncomfortable situation, but researchers should know that these things do happen—and, unfortunately, not infrequently—and they should try to think through their possible reactions ahead of time, so that they do not later regret how they responded.

Act Like a Professional Researcher

A final point about obtaining and recording data, which is particularly important for graduate students, is that field researchers should think of themselves as professionals. Graduate students are often used to feeling low on the totem pole, but while in the field, any field researcher should hold his head up high, and remember that he is a scholar who is going to write a potentially important piece of research.

This also means that researchers should dress and act appropriately. Attire and inter-personal behavior are often more formal and reserved in other cultures, and a field researcher may make a better impression—and probably be more successful in getting a repeat interview or access to restricted archives—by presenting herself more professionally than she might in the U.S. Not only will it make other people more inclined to cooperate, but it will hopefully also remind that researcher that she is there for a reason, with a job to do.

Even in difficult situations, fieldwork is a challenge that is extremely rewarding. The data-gathering stage is probably the most fun and inspiring part of a research project, and while much of it requires “rolling with the punches” and “expecting the unexpected,” a little bit of strategic planning and foresight can go a long way.

Tracking Progress While in the Field

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Evaluating realistically the progress of research while in the field can be, from an intellectual and emotional standpoint, one of the most difficult aspects of managing fieldwork. How does one know if one is getting the data one needs to answer one's questions, especially if one's understanding of what the right questions are keeps changing as one gathers more informa-

tion? Many field researchers find themselves thinking uncomfortable thoughts as their time in the field wears on: Why is it taking me so long to figure things out? What if I never get the information I need? Am I working hard enough? Why on earth did I start this project? Why on earth would anyone want to know the answer? Should I just chuck it all in and write for a travel magazine?

These uncomfortable questions about one's place in the cosmos may be good for the soul, but they can wreak havoc in the field. In the name of efficiency, if not personal enlightenment, I offer some strategies for suppressing the internal demons: (1) have a plan for digesting data as they come in, (2) periodically assess progress towards the goal of a completed piece of scholarship, and (3) gather only the data that one needs. Digesting data and assessing progress help in the almost inevitable moment of crisis that occurs when researchers become convinced that their research designs and research questions just aren't working. The first part of this contribution describes some ways to keep track of the status of a research agenda while one is in the thick of field research. The second part lays out some strategies for retooling on the fly if necessary, and the final segment offers some thoughts on figuring out how and when to stop collecting data.

Digesting While You Collect

One's research designs typically tell a lot about the data that one needs to collect, and some about how one will evaluate the sum total of the information that one has gathered once one has it all in hand. What they don't do is instruct us how to evaluate smaller pieces of the puzzle as information trickles in. I have found that digesting data as it comes in, rather than simply collecting it, is a crucial first step that enables us to evaluate our progress while in the field, and to rework a research agenda on the fly when necessary.

Digesting data helps break down the information one has gathered into usable “nutrients” that can then be used to build an argument. It also aids in condensing data into a more readily accessible form, just as Readers Digest does with the world's great literature (well, maybe that's not such a compelling analogy for scholarship, but...). One can think about digesting data as a way of justifying every piece of information that one collects. For every chunk of data collected, one needs to be able to answer two questions: Why am I gathering this information? And what is it telling me that I didn't know before? There are three main steps involved in thoroughly digesting data gathered in the field: recording one's reactions to the data, placing the information in the context of the overarching research question, and synthesizing occasionally for an audience.

Recording your Reactions

Fieldwork involves holding conversations, real or imagined, with our sources. Recording our half of these conversations—our reactions to what our sources are telling us—is the first step in digesting. Reading notes, interview notes, ethnographic notes, field journals, and literal recordings of our own voices are some of the many forms that these recorded observations

may take. What is important to note here is that what one is recording is not the data presented by the source in question (an interview, a document, a slice of daily life), but our own reactions to these data.

An example may help clarify: Say I've just found a document I've been trying to get access to for a couple of weeks. It's a section from a 1954 Italian government report on poverty. I don't have time to read the whole thing right now, but I've made a photocopy of the relevant sections and stuck it in a folder. Right there in the archives, I begin the task of digesting by recalling the task on my list of research priorities that this document is related to, and scribbling myself a note on the cover of the folder: "photocopied because I need to know whether the government was aware of the problem of poverty among children in the early post-war period. This is important for my project because I need to know whether the government was consciously directing more welfare dollars towards the elderly than towards children because they thought the elderly needed it more."

That evening, as I am putting my day's work to bed, I make a note in my field diary of what this document (or it could have been a sentence, or a facial expression, or a poster observed on the street) tells me about my research question. I note that the government publication "had 3 big sections on issues related to the problem of poverty among young people, and only one brief one on poverty among the elderly." I record my reaction to this information. What does it make me think about my hypotheses? What new or unexpected issues does it bring up? How does it fit in with information I've received from other sources? (Just to finish off this example,) I note "I was really surprised to see this, because I expected that there was more welfare spending on the elderly because of a perception among policy-makers that the elderly were really worse off, and younger people could better take care of themselves."

I have not yet read the document or taken notes on the actual contents. None of the notes I've made to myself are particularly profound, and the whole process took less than five minutes. But the payoff is large. When I am at leisure to read the full document I will remember why I collected it in the first place, and what I expected to find. And in the mean time, I know where I stand with the task on my "to do" list that is concerned with collecting documents that will allow me to evaluate government priorities with respect to the impoverished elderly and the young.

Placing the Data in Context

I recommend taking time out from data collection periodically—after each interview, after a day's work, at the end of each week, and/or at the completion of each research task on the to-do list—to record and evaluate what one has learned *in terms of the overarching goals of the research project*. When a researcher ties the data that he has gathered into the larger framework of his project, it serves as a reminder of where he is going, and keeps him abreast of the progress he is making.

One needn't write whole chapters or articles while one is

in the field — though of course that is a tremendously productive way of synthesizing material, and I encourage the attempt even when it seems impossible. Writing to digest can take many other forms. It does not have to be for public consumption, and it can take place before one has read or transcribed or cross-tabulated every datum in the document, interview, dataset, etc. When transcribing interview notes from notebook to computer, for example, one might develop a system for recording how and why statements by respondents fit in to the three categories of "justificatory schemes" that you expect to discuss in the three chapters of your book. Another kind of writing to digest is making careful notes about the potential relevance of each source, even if it is impossible to read every page of photocopies from an archive before it goes into a box to ship back home. Notes should serve as a reminder of why each piece in a growing archive was collected. For example, the researcher might record that a particular article from a trade publication was photocopied because it illustrated how the industry association's leadership perceived the government's introduction of a new water safety regulation. She might go on to remind herself that this is important in the larger framework of her thesis because it contradicts the lobbying efforts of key individual textile manufacturers, which in turn is important because... Ultimately, notes on each small piece of the research should be tied logically into the overarching research question.

Synthesizing for an Audience

Keeping up with the not-for-public-consumption tasks above helps us to keep track of and reward data collection as it occurs in the field. But without some more sustained synthetic and analytic attention to the information coming in, fieldwork can stall. I find particularly helpful the discipline of periodically producing a somewhat more formal summary of our intellectual progress on the journey towards a completed project.

One digesting technique that we've found useful is to write weekly or bi-weekly briefs describing the work we've carried out in that period, and summarizing the conclusions that we've drawn, even if only tentatively, from the information we've gathered so far. These briefs might take the form of an electronic field work journal or an email archive of periodic messages to a colleague, advisor, or favorite pet. While the analysis should be as disciplined as if it were for public consumption, there need not be any actual exchange of information with another human being. Some researchers may find it more helpful to write up these findings in a more formal way, for a real audience like a dissertation chair, funding agency, or colloquium. Whatever method is chosen, and I suggest experimenting with a few, writing while in the field is the key to digesting gathered data. And digesting is the key to being able to assess the progress of ongoing work, in order to retool if necessary.

Assessing Progress Periodically

As the researcher begins to accumulate and digest the pieces of information on her "to get" list, she may discover that each

piece fits neatly into her research design, and that every week she spends in the field brings her one step closer to answering in a definitive way her major research question. Alternatively, she may come to believe that all of her careful work generating falsifiable hypotheses and testable implications has led her nowhere, and that she is asking the wrong question entirely. Regardless of the methodological approach favored by any particular researcher, the reality of fieldwork usually lies somewhere in between the two extremes of completely predictable deductive research and completely disorderly induction. No one should go into the field without some kind of expectations about how the research will proceed. That said, the world is a messy place, and that messiness is why it is so important that political scientists continue to do field research as well as use more formal analytic techniques like game theoretic and econometric modeling. The messiness of the real world also means that one of the major challenges of field research is figuring out how to retool a project on the fly, often without backup from colleagues or advisors.

How does one know if a research agenda needs retooling in the field? Our experience suggests that many dissertation students, in particular, worry too much about the progress of their data collection and the state of their research agendas while they are in the field. As a general rule, I would suggest that if a researcher, guided by a “to get” list, is making progress towards gathering the data needed to make some judgments about the initial hypotheses or questions posed by the research agenda, chances are good that the project is on track.

Inevitably some tasks on the “to do” list take much longer than anticipated, just as others that seemed likely to be very complex turn out to have easy answers. Fieldwork often follows the 90/10 rule, where 90 per cent of the time, effort and other resources are expended gathering 10 per cent of the data. The feeling of “wasting time” that often occurs as a result can be discouraging. I believe that the best insurance against this feeling is to identify data needs clearly in a “to get” list and stick with the job of fulfilling these needs.

Respondents, archivists, local experts and the like are often key sources not only of data, but of much-needed feedback on the progress of a research project. Field researchers wisely rely on locals for periodic reality-checks. Unfortunately, this feedback can be discouraging if local sources convey the message that the researcher is addressing the problem in the wrong way, or addressing the wrong problem. Clearly, field researchers should not routinely ignore what their respondents are telling them—if that were the case, why bother to go to the field? But it’s worth remembering that one’s perspective as a political scientist is likely to be different from respondents’ perspectives as politicians, policy makers, citizens, etc. And even local political scientists may, for reasons associated with the discipline’s intellectual trajectory outside of the United States, place less emphasis than many American political scientists would on things like the discovery of causal mechanisms, falsifiability, or even empirical research per se. So discouraging feedback from local sources in the field should not be taken as proof positive that a research project is in trouble.

Almost all field researchers experience moments of crisis,

when they come to believe that their project is fundamentally flawed. In some cases this is because data collection seems to be taking too long, or because feedback from local sources suggests a mistaken research strategy. These moments of crisis seem to us to be quite natural responses to the problem of incomplete information. One cannot know what the answers to one’s research questions are until one has collected all the data; but one needs to assess the state of the project periodically in order to make sure that one is still on track. In the absence of any real yardstick for measuring progress before the job is done, many researchers fall back on the only information they have: that the job of data collection seems fraught with inefficiency, and that there are people out there who think that the approach is flawed. Again, I think that the best insurance against these moments of crisis is to bring your own yardstick with you, in the form of detailed “to get” lists. When incoming data are appropriately digested, so the researcher knows what she has and still needs, then there exists a solid basis for making decisions about where the research agenda stands at any given point in time.

Periodic re-evaluation of progress towards a completed research question may reveal problem areas. Key data may be persistently unavailable. The data that are available may contain surprises, suggesting that a researcher’s original hunches or hypotheses were wrong. The researcher may come to believe that he or she is testing the wrong hypotheses, or even asking the wrong research question. All of these contingencies may suggest the need to rethink a research agenda on the fly. The question most researchers struggle with is how to interpret these disturbing revelations. When is it appropriate to stay the course, when is it necessary to shift directions, and when does the situation actually require switching horses in midstream?

When to Retool

There are no hard-and-fast rules. (Some general guidelines are summarized in Table 1.) As I indicated before, most researchers experience moments when they are certain that abandoning ship is the only sensible way to proceed, and in many cases this is simply not true. For example, if key pieces of data seem unavailable, researchers may be able to address the problem by triangulating with a creative use of multiple sources and methods. If this is not possible, a researcher might decide to plan a return trip to the field to fill remaining data needs, or find a way to do without a particular piece of information altogether. It is only when multiple, central hypotheses become untestable due to unavailable data that it would likely become necessary to consider radical changes to a research program.

Similarly, even unpleasant surprises emerging out of fieldwork need not signal the premature death of a research agenda. Even if the hypotheses laid out in a research design turn out to be unsupportable, researchers can often find a way to make these unexpected results work to their advantage. Before concluding from surprising data that the original research question was so mistaken in its framing that there is nothing to be gained from pursuing it further, researchers should consider the many ways in which a surprising result may be useful.

Table 1: General Guidelines for Field Research

What seems to be the problem?	Why is this happening?	Possible fixes	When to jump ship
I can't get the data I need to answer my question.	<p>You've asked an unanswerable question—check for falsifiability of all key hypotheses <i>before</i> you leave for the field.</p> <p>You've run into problems with access to people or documents; there are records of what you're looking for.</p>	<p>Try multiple sources methods. <i>Triangulate.</i> Come back later if you can. Do without that piece of information.</p>	If <i>multiple, central</i> hypotheses are untestable given the limits of the data you can get your hands on, you may need to consider changing your emphasis or your topic.
I'm surprised by what my data are telling me.	<p>Your original guesses and hunches are wrong.</p> <p>Your respondents or other sources are not giving you the full story.</p>	Find a way to make this into a "good" surprise; something that illuminates a larger theoretical debate. Is this a case of someone else's hypotheses holding up under unexpected circumstances? Of the common wisdom not prevailing? Of an unexpected application of a different literature from the one you relied on in constructing your project?	If what you've learned tells you that you were so mistaken in your framing of your research question that there's simply nothing to be gained from pursuing it further, consider re-tooling.
I'm testing the wrong hypotheses.	The theoretical literature was out of date, not attuned to the realities on the ground in your corner of the world, or otherwise inapplicable.	Come up with new hypotheses and ways to test them.	This shouldn't be a deal-breaker unless you have discovered the problem at the last possible moment and there is no way for you to go back to the field or get more information remotely.
My research question is the wrong question to be asking.	<p>You're bored of your topic, and something else looks more appealing.</p> <p>Too much time has elapsed between conceiving the project and getting into the field, and your issue no longer seems important.</p> <p>Your time in the field has convinced you that life on the ground does not reflect the theoretical issues your question relates to.</p>	<p>Go back to your research design and remind yourself of why you picked this project: what is its import for political science theory, what attracted you to it personally?</p> <p>Link up to a different theoretical literature that seems more relevant given what you've learned about your topic.</p> <p>Take a historical view.</p>	<p>Better to shift the emphasis than to abandon your project altogether.</p> <p>Major changes only if you and your network of feedback-givers agree.</p>

Does it illuminate a larger theoretical debate? Is it a case of someone else's hypothesis holding up under unexpected circumstances? Of the common wisdom being overturned? Of an unexpected application of a literature not considered in the original research proposal?

Researchers may also become convinced in the course of fieldwork that they are testing the wrong hypotheses. The theoretical literature on which the research proposal was based may have been out of date, not attuned to realities on the ground in a particular field site, or otherwise inapplicable. The researcher may have made an informed and intelligent but nevertheless wrong guess about a particular political dynamic under investigation. Under most circumstances, it is a simple matter even in the midst of fieldwork to come up with new hypotheses that seem more reasonable, and with new ways to test them. If the problem is discovered at the very last moment and there is no way to go back to the field or to get more information remotely, a more radical change in research strategy may be necessary. But these occurrences are rare when researchers are diligent about prioritizing their data collection needs, digesting data as it comes in, and reassessing progress periodically.

The most disheartening moment for many field researchers is that moment when they fear that the very research question motivating their field work is fundamentally misguided. I believe that this moment is almost inevitable. Good field research involves observing the often inexact fit between theories and facts on the ground. Theories often reflect ideal types, while real places are the "cases" that force us to update underlying assumptions and think about theory in new ways as one interacts with sources and data. In and of itself, this inexact fit between theory and data should not be a cause for concern. If the research question truly seems to be the wrong question to be asking under the circumstances, in the most extreme cases a project may need to be fundamentally rethought. More often, relatively minor adjustments to the research question can restore relevance.

There are other less profound, and probably more common, reasons why a research topic may come to seem irrelevant or misguided. When funding cycles or qualifying exams cause a long period of time to elapse between the initial conception of a research agenda and its execution on the ground, very topical research may come to seem less relevant. It is wise to consider at an early stage how to protect a research topic from obsolescence, particularly if it is related to very current events. In the field, it may be necessary to take a more historical view, or link up to a different set of theoretical issues if a topic that was once topical now seems passé. Researchers should also expect to confront periods of boredom with a major research project — periods when any topic other than the one at hand seems more compelling. When this happens, it may be helpful to go back to the original grant proposal, dissertation prospectus, or the like to remind oneself of the theoretical, substantive, and/or normative motivations driving the project.

Most research projects do evolve over the course of time spent in the field, in response to data limitations, new informa-

tion, a changing political environment on the ground, and the iterative process of data collection and re-evaluation of theory that characterizes life in the field. Before making any major changes in a research agenda, though, I suggest soliciting input from an intellectual support network consisting of colleagues, collaborators, and/or advisors. In the midst of fieldwork, when researchers become steeped in on-the-ground understandings of a problem, it is easy to become overwhelmed with empirical detail and lose theoretical focus. An intellectual support network can often help researchers re-envision the forest during periods when trees are likely to dominate the landscape.

Solicit Input

The diminishing cost and increasing ease of staying in contact with "home base" (via email or phone) while in the field may make it feasible, depending on where one is working, to maintain up-to-the-minute contact with dissertation advisors, collaborators, or colleagues. One of the nicest things about being in the field, I think, is getting away for a while from the familiar modes of thinking that dominate our normal day-to-day circles of colleagues. Still, there will likely be times when researchers would like some feedback from advisors or colleagues on how to move forward. How much contact is the right amount? What kinds of contact are likely to be most useful?

I believe that for dissertation-writers, in particular, contact with advisors should foster the independence of the researcher, giving her permission to make her own judgments about the information she's uncovering in the field. A dissertation-writer's contact with her advisors should make her feel like she is the world's leading expert on her topic (which she often is), and should place high value on the information she has uncovered through her research. At the same time, both graduate students and more experienced researchers often want and need feedback on how to reassess the direction of a project based on the new information they've uncovered. A researcher can help elicit this kind of useful feedback by summarizing for an audience what he has learned so far, and the impact that he perceives this information to have on the research agenda as a whole. When asking for help on what to do next, his audience then has real data to work with, and a strong sense of how the researcher thinks his argument will be impacted by the information gleaned from fieldwork.

Local academics, particularly political scientists, can provide the perfect mix of country expertise and analytic ability that scholars at home may lack. But as we've noted, political science means different things in different places. As a result, relying too extensively on local contacts for "external validation" of the progress of research may be problematic, since local expectations about what constitutes good scholarship may differ quite substantially from mainstream American political science. Researchers should be wary of automatically trusting a local expert's judgment more than their own simply since the local is local. Still, informed locals can be an invaluable source of advice and support, perfectly positioned to remind the researcher that there really is a there there. I encour-

age researchers to seek them out and make them a part of their intellectual support network.

Avoid Data Overload

The final piece of advice for field workers that we'd like to offer is how to figure out when to stop collecting data. Some researchers stop collecting data when a field site becomes dangerous, or when their health fails. Most call it quits when the money or the visa runs out. We'd like to encourage researchers to adopt a different rule of thumb: Stop gathering data when you have what you need.

This seemingly simple piece of advice can be difficult to follow in practice. A few concrete suggestions are in order. First, keep track of data collection priorities, digest incoming data regularly, and keep an up-to-date "to get" list. When sources begin to repeat themselves—when you've heard or read the same thing three times and you believe it to be accurate—it's time to move on. (Only if the claim being made is highly controversial would more supporting evidence be necessary. One additional mitigating circumstance suggests itself: if during survey research in the field there is an opportunity to do more than simply attach questions to an omnibus, it pays to map out and try to fill data requirements for several research projects at once.) If you find yourself with extra time in the field after all of your data needs have been met, resist the urge to gather more. Spend the time digesting what you have more thoroughly, or better yet, begin writing up your results.

The perils of gathering too much data are real, and underappreciated by most researchers. For those on a tight budget, it pays to remember that getting data back home is expensive, particularly in paper form. Thoroughly digested data, on the other hand, weigh less! In addition to being expensive, shipping masses of undigested data back home can seriously delay the beginning of the write-up phase, as time must be devoted to reading, digesting, and organizing.

Few researchers actually gather every piece of information they need in a single trip to the field. But it can be difficult to know exactly what is missing until write-up begins. Researchers whose schedule and budget allow for return trips to the field will benefit from pausing after an initial round of data collection to take stock of real persistent data needs. But all researchers will be better able to resist the temptation to gather too much data if they act as if they will be returning to the field. Most research projects end up requiring less in the way of actual data than originally planned for. Even if a return trip seems necessary but is not possible, careful maintenance of contact addresses and phone numbers (as well as diligent writing of thank-you notes to survey respondents, interviewees, librarians, archivists, etc.) will often make it possible to access additional information remotely. Finally, remember that each individual research project, flawed and incomplete as it may be, makes up part of a larger research agenda that in its entirety can reflect a more complete view of the world.

Symposium: Discourse and Content Analysis

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This symposium grew out of our own interests in content analysis (CA), discourse analysis (DA), and the diverse epistemological and methodological issues that a comparison of the two might raise.¹ In particular, the similar goals of the two techniques made us wonder whether some amalgamation of the two might produce a method that could incorporate the major strengths of each—or whether, conversely, their superficial similarities might mask an insurmountable ontological divide. When John Gerring raised the possibility of a symposium on the subject for this newsletter, therefore, we were intrigued by the possibilities. We took the opportunity to do what, in our opinion, symposium editors should do: assemble a group of smart people and give them free rein to write about whatever they find interesting about their areas of expertise. We suggested as a starting point the general question of how discourse and content analysis are similar and how they differ; as expected, discussions along these lines led our contributors to a number of interesting additional topics, insights, ques-

tions, and (of course) disagreements.

Most of the contributors agree that discourse and content analysis differ in significant ways. The real question is the degree to which they differ—indeed, whether they are even comparable at all. We begin the symposium with a contribution by Cynthia Hardy, Bill Harley, and Nelson Phillips, who very concisely outline the two methods, their differences and potential for overlap. The next three contributions—by Neta Crawford, Will Lowe, and Mark Laffey and Jutta Weldes—discuss discourse analysis (Crawford, Laffey and Weldes) and content analysis (Lowe) separately and in greater detail. The final three contributions, by Ted Hopf, Kimberly Neuendorf, and Karin Fierke, more explicitly contrast the two methods. Some of the contributors employ an ideal-typical analysis in contrasting the differences between the two methods, but all of the contributors note that both DA and CA can be done using a variety of techniques, and some of the contributors even go so far as to outline specific techniques and innovations (e.g. Crawford, Lowe, and Laffey and Weldes).

To some extent the question of whether the methods are comparable is answered by four contributions that explicitly do so (Hardy et al., Hopf, Neuendorf, and Fierke). Hardy et al., for example, compare the two techniques across twelve dimensions. But beyond basic comparability, the question of how much overlap there actually is between the methods remains debatable. After presenting rather stark differences between

the two methods, Hardy et al. come around to arguing that actually there can be a mixture between the two, and they outline possibilities for overlap in Table 2. Similarly, in making the case for stating the assumptions behind content analysis, Lowe points out that doing so is important both for its own sake (because we can't, or shouldn't, pretend that we don't have any) and because if assumptions are made explicit they can then be relaxed to fit particular research circumstances. One could apply this principle to the adjustment of CA assumptions toward DA assumptions outlined by Hardy et al. A slightly different take on the issue of overlap comes from Neuendorf, who argues for using qualitative and quantitative methods together, and while not necessarily arguing for a hybrid of DA and CA, suggests how the two can be complementary. Laffey and Weldes, however, argue more strongly than some of the others that, although there are some superficial similarities in technique, DA and CA are oriented toward different research goals: in particular, DA is fundamentally concerned with power relations and the situatedness of the meaning of language, both of which are outside the bailiwick of content analysis.

As this initial summary of the comparison of discourse and content analysis suggests, there was some disagreement on not just the definitions of discourse and content analysis, but the definition of discourse itself, and for that matter, the definition of language and text, as well as the related concept of practice. Laffey and Weldes provide a definition of discourse as the structures and practices that are used to construct meaning in the world. Similarly, Crawford argues that discourse is "the content and construction of meaning and the organization of knowledge in a particular realm." These definitions are remarkably unlike Lowe's definition of discourse as a "probabilistic content analysis model" or in other words, "a theory of what is more or less likely to be said, and of what the conceptual elements are that generate and constrain these possibilities." There is some overlap here insofar as Laffey and Weldes argue that particular discursive practices and structures make certain representations possible; one could reasonably ask whether for what practitioners of DA understand as "conditions of possibility," their CA colleagues might reasonably substitute "necessary conditions," or "some X s.t. $\Pr(Y|\neg X)=0$ "—an underlying conceptual construct (X) without which the expression of a given word or phrase (Y) is impossible. Arguably, the first formulation is constitutive—in that Laffey and Weldes emphasize that discourse is not just a particular collection of words, but a constitutive set of structures and practices, that do not merely reflect thoughts or realities, but rather structure and constitute them—and the second is causal, but from a methodological point of view it only matters whether the empirical implications are the same, and in this case, they seem to be.²

Another way of thinking about the definition of discourse is to enquire about the meaning of language; this issue was raised by Fierke, as well as by Neuendorf and Laffey and Weldes. Is language simply a reflection of reality (be it objective or intersubjective), or is language itself constitutive of reality? Fierke argues that in the former ontology is separate

from epistemology, whereas in the latter they are connected because the way that one comes to know the world determines what that world is, at least from the point of view of theory. The discussion of competing definitions and comparability of discourse and content analysis thus far has already raised several deeper questions that arise out of or inform these discussions. The first is the issue of how to understand the relationship among ontology, epistemology, and methodology; in other words, what are the connections among the nature of reality, the ways in which we come to know it, and the tools we use to do so? Are these connections logically necessary or merely habitual? Second is the question of whether and, if so, how discourse and content analysis fit into positivism and the scientific method. A third question concerns the issue of how to accommodate, methodologically, the dynamism of socially constructed meanings and changing realities. Finally, a fourth question concerns the role of power relations, both in our subject matter and in our analyses.

Ontology, Epistemology, and Methodology

Issues of fundamentally different ontologies and epistemologies arise often. This issue famously informed Friedrich Kratochwil and John Gerard Ruggie's (1986) critique of regime analysis, namely, that the (positivist) epistemology of regime analysis fundamentally contradicted the (intersubjective) ontology of regimes themselves. The basic issue was that "the different approaches construe the social world differently—just as Newtonian mechanics and quantum mechanics do in the physical world" (Ruggie [1998, 86]; see also Kratochwil and Ruggie [1986, 764-66]). At the same time, other analyses suggest strongly that there is no necessary link between ontology and epistemology or method: even Clifford Geertz demonstrated the link between the Balinese cockfight and Balinese culture more generally, in part, via a statistical analysis of the structure of wagering (1973, pp. 429-30). The question of the compatibility of CA and DA often hinges on whether or not intersubjective processes are thought to produce objective empirical "footprints."

Every one of the contributions to this symposium has commented on the relationship between ontology, epistemology, and methodology, and some, like Fierke's, have further delineated methods from methodology, treating methods as discrete techniques and methodology as the combination of methods with positions on epistemological and ontological questions. The starting point of this debate is the issue of objectivity, and the related position on ontology. Whereas for many analysts using content analysis, the idea of a fixed and objective reality is acceptable, the embrace of the intersubjective construction and interpretation of reality is a core assumption of discourse analysis. Indeed, the analysis of subjectivity and mediation is one of the primary goals of discourse analysis, and is embodied in the attention to context.

The concept of context, or the situatedness of knowledge, suggests a second aspect in the ontology/epistemology/methodology debate, namely the relationship between epistemological and ontological positions, already alluded to

above in the discussion of the definition of language. As Neuendorf points out, the question of epistemology for content analysis is relatively straightforward: the positivist scientific method is how we know things. In contrast, Fierke argues that the necessity of understanding context in discourse analysis stems from a refusal to separate ontology and epistemology: what we know is not separable from the method in which we came to know it. Laffey and Weldes, as well as Crawford and Hopf, also argue that discourse analysis unites epistemology to ontology in that DA asks how we came to know the representations (words, phrases, language, gestures, etc.) that we claim constitute reality. Similarly, Neuendorf also argues that for discourse analysis one needs the subjective interjections of the analyst; quoting Phillips and Hardy (2002, p. 83), she writes that “there are no unmediated data.” Fierke, in contrast, calls into question the common perception that DA is subjective: she claims that it is in fact potentially more objective than CA because it is less dependent on categories pre-chosen by the analyst, and thus subject to his or her interpretation. Some scholars see the mediation of data as something that must be acknowledged but minimized as much as possible. On the other hand some argue that researchers should explicitly engage in some types of mediation in order to advance their understanding of the object of study.

This position on the inseparability of ontology and epistemology brings up the third question regarding the relationship of methods or methodology to epistemological and ontological positions, to wit: Can methodology be separated from epistemology and ontology, and if so, does one come first, or is one determinative of the other? In the specific context of DA and CA, does using a particular method determine what is knowable? Lowe, paraphrasing Alexander Wendt, argues that methodology underdetermines epistemology—in other words, the methods we use do not solely determine what we can know. Others, notably Laffey and Weldes, are not convinced that methodology can be separated from epistemology. Hopf provides a somewhat different perspective on this debate in arguing that “epistemology and ontology trump methodology.” By this he means that rather than solving the question of what reality is and what is knowable via better methods, we must acknowledge the limits of our certainty about what is knowable and what reality is. In other words, researchers must be circumspect in their claims, regardless of their methods.

Another way to think about points raised in this ontological, epistemological and methodological debate is to acknowledge that theoretical content is often derived from the methods we use. While some might hope that the choice of methods could be “theory-free,” meaning that methodological choices would not impose particular theories on researchers, in reality, theories are often based on a set of implicit assumptions derived from the methodology with which the researcher is most familiar. That is, rather than hypotheses being based on the researcher’s hunches about what makes the world go around, certain methods in themselves may make particular theories likely to be the focus of analysis. Of course some methodological techniques were designed with certain

classes of theories in mind, and so in some cases the relationship may be intentional. But nevertheless, the debate over the relationship of ontological, epistemological and methodological positions reminds us that we should be aware of the potential theoretical content of the methods we use.

Positivism and Scientific Methodology

Related of course to the question of the relationship between epistemology, ontology, and methodology in discourse and content analysis is the issue of how discourse analysis and content analysis are related to positivism and scientific methodology. In particular, a question that comes up in several contributions to the symposium is whether CA and DA are positivist. Some contributors, e.g. Hardy et al., Lowe, and Neuendorf, argue that content analysis either is positivist or can be usefully constructed as a positivist inference process. Indeed, Lowe’s objective is not to argue in favor of positivism, because he seems to take for granted the utility of positivist methods for social science. Rather, his task is to consider the relationship between CA and positive quantitative analysis, and argue that CA should be part of the quantitative fold. It is intriguing to note that Lowe considers CA to be currently outside of statistical mainstream—although he argues persuasively that it should not be—while others in the symposium consider CA to be orthodox statistical positivism. Moreover, if CA is, or is to become, part of mainstream quantitative analysis, is DA already part of, or should it be part of, mainstream qualitative analysis? These corollary questions were not addressed by the contributors but are worthy of further consideration.

Another question raised by the association of CA with positivism is whether the association is ironclad. Hardy et al., after positing CA as positivist, outline ways in which CA and DA methods can be mixed so as to call into question its positivist assumptions. But if we agree that CA is not necessarily positivist, one wonders whether the corollary assertion (that DA is necessarily non-positivist) also holds. While several contributors have given examples of ways in which CA may be done in a non-positivist manner, it is interesting to consider whether DA is fundamentally non-positivist (a position espoused, at least implicitly, by some of the contributors) or whether some versions of DA might plausibly be “positivizable.” As there are already many existing varieties of both DA and CA, these questions are answerable in different ways, depending on the type of DA or CA that one chooses to use.

Related to the question of positivism is the question of whether CA or DA are necessarily quantitative or formal. While the immediate response might be to simply dichotomize CA as quantitative and formal and DA as non-quantitative and non-formal, consideration of actual techniques suggests room for overlap. For example, Hardy et al. point out similarities between the two techniques, and Laffey and Weldes also acknowledge this point, although they argue the similarity is only superficial compared to the different goals of, as well as assumptions behind, CA and DA research.

The quantitatively inclined will find that attempting to

parse the sorts of relationships described by DA into formal statistical terms produces interesting thought experiments. For example, Laffey and Weldes describe discourses as “sets of rules that both enable practices and are reproduced and/or transformed by them.” The statement that rules enable practices would seem only to eliminate the possibility of certain practices in the absence of certain rules, not to make any predictions about the frequency of those practices in their presence. The production and reproduction of rules and practices would suggest severe and possibly intractable endogeneity issues: error terms in time-series models with multiple dependent variables are rather complex—some, for example, incorporate multiple separate error terms to capture different sources of unexplained variation (e.g., contemporaneous shocks). The transformation of rules, moreover, suggests either that the relationship between rules and practices is not constant over time or that it is contingent upon some additional, unspecified factor. Despite the simplicity of the initial formulation, therefore, the associated statistical specification would have to be highly sophisticated in order to counteract a variety of threats to inference. These threats are no less present in qualitative analysis, of course. Moreover, some elements common to both kinds of analysis are given dramatically different interpretations by the contributors: whereas Lowe considers CA in terms of a probabilistic model, Hopf argues that one of the attributes of DA is that it specifically directs attention to absences and anomalies—phenomena most likely ignored or relegated to the error term in a probabilistic model. The point here is that, even if no quantified data will ever exist, constructing quantitative models of the processes described by qualitative methodologists might be a useful exercise: qualitative analysts could be made aware of threats to inference that they had not considered, and quantitative analysts might begin to produce methods that more closely capture the kinds of processes common to DA research.

The role of the analyst, while central to the issue of subjectivity, may also be a place for overlap between CA and DA. Crawford notes that DA requires many choices on the part of the analyst, especially regarding the limits of the discourse. Indeed, the boundaries of the discourse, or the object of study, for those engaged in discourse analysis is not clearly and externally delineated. Hopf argues that DA in fact assumes an “open social system,” in the sense that there are overlapping webs of meaning with no obvious starting or end points of analysis. But despite the implicit goal of most statistical techniques to minimize the role of the researcher, it is also the case that any type of quantitative analysis, including CA, requires the analyst to make choices about the limits of what is or is not included in a model or data set. These choices matter substantially in the process of extracting meaning from text, regardless of the method used.

Indeed, given the necessity of scholarly understanding of a subject, even for CA, it is interesting to ask whether it is even possible, except in the crudest and most mindless ways, to do CA without some level of implicit DA as well. Neuendorf argues that one needs to do some DA before CA, in order to

come up with coding guidelines, but one can push the question further to ask if DA and CA are inseparable or if there is an ordering in which one should come first. Another point of consideration is to what extent one can use CA techniques (methods) within discourse analysis methodology, or vice versa, i.e. DA techniques (methods) in a CA or positivist methodology. The contributors are split on these questions: Some argue that the methods can be used together or in a hybrid form, but others disagree.

Finally, the question of positivism and scientific methodology raises the issue of how both CA and DA address the concepts of replication and validity. Neuendorf argues that DA is more concerned with validity, while CA focuses more on reliability; others see less of a distinction in this regard. Lowe addresses this issue for CA, while Crawford makes the case that DA can be both rigorous and attentive to replication and validity issues. Hopf agrees with Crawford with regard to scientific rigor, validity and replicability and refuses to concede science to either positivism or CA, arguing instead that DA can be used to generate theories and test hypotheses in a scientific manner.

Change and Timeframes of Analysis

A third set of questions that this symposium has raised has to do with time—notably, the question of how to address change over time as well as the appropriate timeframes of analyses. The issue of change over time is not unrelated to the earlier issue of ontological position: can reality be taken as fixed or is it fundamentally fluid? While realists and positivists would acknowledge that reality changes, the question is whether analysis of reality requires a constantly dynamic model—and, if so, whether the parameters of such a model can even be taken to be fixed. Laffey and Weldes suggest that attention to change over time is one difference between DA and CA, with DA being attentive to fluidity in meanings while CA assumes a static conception of reality; Fierke argues that this distinction may be “too stark.”

Moreover, while there may be difference in the choices that researchers make regarding this issue, it is clear that both DA and CA must nevertheless at some point posit enough stability to be able to acknowledge a baseline from which change can be measured. For example, to the extent that DA is interested in the construction of meaning, presumably the set of relevant meanings changes from one particular state to some other state; how else but by capturing meanings at multiple points in time can one claim that meanings have been constructed or changed? Thus, beyond the ontological difference, the difference in attention to the fluidity or reality or meanings may be a matter of emphasis rather than a substantive difference between DA and CA.

A second way in which DA and CA differ with regard to time, however, concerns the timeframe of the analysis of data. Whereas with CA the timeframe for each bit of data is relatively constant, as Crawford points out, in DA the researcher must “extend the time frame”—that is, investigate where the beliefs or ideas came from and how they changed, rather than just accepting them as they are at a particular time. This

tracing of individual elements of an argument to different periods of time seems to be more common in DA than CA, but one could imagine adapting CA methods to accommodate this concern.

Power

The last issue to consider in the difference between DA and CA is the way that each addresses issues of power and hierarchy. The way in which power relations structure, constrain, and produce systems of meaning is a fundamental concern of DA. Laffey and Weldes' concept of interpellation specifically addresses this through the investigation of subject positions, i.e. identities and power hierarchies. Similarly, in outlining DA methodology, Crawford argues that researchers must identify specific beliefs of dominant actors for a particular context. All other contributors to the DA discussion similarly note the importance of power considerations in DA. This concern should be acknowledged as a core contribution of DA, but we may still question whether power is exclusively the concern of DA, or whether power considerations could be integrated into CA and other types of qualitative or quantitative methodologies.

Conclusion

It is clear, by virtue of their detailed responses to our unstructured initial query, that many of our contributors have thought quite a bit about the questions of the fundamental natures of CA and DA and which relationships might exist between them. We are happy to be able to offer their collected thoughts on the subject in the hopes that they will enlighten, provoke, and produce further discussion

Notes

¹ We are grateful to Karin Fierke, Will Lowe, and Jutta Weldes for comments on an earlier draft. Errors of fact or interpretation remain our own.

² When faced with the prospect of rendering the kinds of statements about the world that DA produces in statistical terms, one might reasonably wonder what the point of such an exercise would be. There are, we think, two answers. The first, simply, is to permit generalization from a representative sample to a larger population. The second, elaborated below, is to take advantage of a substantial statistical literature on threats to inference, many of which might very well apply across methods.

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Discourse Analysis and Content Analysis: Two Solitudes?

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In this essay, we outline the key features of discourse analysis, contrast it with content analysis, and then consider the extent to which these two methods can be seen as either complementary to, or in conflict with, each other. Our underlying premise is pluralist in that while we recognize that these two methods are based in very different philosophical camps and play very different roles in social science research, we also believe that they can be seen as complementary and even mutually supportive in the exploration of social reality. Furthermore, given the recent "linguistic turn" in social science and the related increasing interest in the study of texts of various kinds, the contrast between these two methods provides a particularly useful context in which to discuss assumptions about the nature of language and the role of linguistic methods in social research.

Discourse Analysis

Discourse analysis is a methodology for analyzing social phenomena that is qualitative, interpretive, and constructionist. It explores how the socially produced ideas and objects that populate the world were created and are held in place. It not only embodies a set of techniques for conducting structured, qualitative investigations of texts, but also a set of assumptions concerning the constructive effects of language (Burman & Parker, 1993). Discourse analysis differs from other qualitative methodologies that try to understand the meaning of social reality for actors (e.g. Geertz, 1977) in that it endeavors to uncover the way in which that reality was produced. So, while it shares a concern with the meaningfulness of social life, discourse analysis provides a more profound interrogation of the precarious status of meaning. Where other qualitative methodologies work to understand or interpret social reality as it exists, discourse analysis tries to uncover the way that reality is produced (Hardy, 2001; Phillips & Hardy, 2002).

Discourse analysis also presupposes that it is impossible to strip discourse from its broader context (Fairclough, 1995). Discourses have no inherent meaning in themselves and, to understand their constructive effects, researchers must

locate them historically and socially. The meanings of any discourse are “created, supported, and contested through the production, dissemination, and consumption of texts; and emanate from interactions between the social groups and the complex societal structures in which the discourse is embedded” (Hardy, 2001: 28).

Discourse analysis is thus more than a method: it is a methodology (Wood & Kroger, 2001) based on two primary assumptions. First, discourse analysis is founded on a strong social constructivist epistemology. Social reality is not something that we uncover, but something that we actively create through meaningful interaction. The study of the social thus becomes the study of how the objects and concepts that populate social reality come into being (Phillips, Lawrence & Hardy, forthcoming).

Second, discourse analysis grows out of the belief that meaning, and hence social reality, arise out of interrelated bodies of texts – called discourses – that bring new ideas, objects and practices into the world. For example, the discourse of strategy has introduced a series of new management practices over the last fifty years (Knights and Morgan, 1991); the postwar discourse of human rights has brought about the contemporary idea of a refugee with rights to asylum (Phillips and Hardy, 1997); and the discourse of AIDS has empowered groups of patient-activists (Maguire et al., 2001). Discourses are thus “concrete” in that they produce a material reality in the practices that they invoke. Accordingly, a discourse is defined as a system of texts that brings objects into being (Parker, 1992). From this perspective, social science becomes the study of the development of discourses that support the myriad of ideas that make social reality meaningful. And, since discourses are embodied in texts (Chalaby, 1996), discourse analysis involves the systematic study of texts to find evidence of their meaning and how this meaning translates into a social reality (Phillips & Hardy, 2002).

Highlighting Similarity; Recognizing Difference

Content analysis, as it is traditionally employed, differs from discourse analysis quite profoundly even though it is similarly concerned with the analysis of texts. Most importantly, it adopts a positivistic approach – the fundamental activity is hypothesis testing using statistical analysis (Schwandt, 2001). At a practical level, it involves the development of analytical categories that are used to construct a coding frame that is then applied to textual data. Content analysis as a mode of textual analysis is characterized by a concern with being objective, systematic, and quantitative (Kassarjian, 2001: 9): objective in the sense that the analytic categories are defined so precisely that different coders may apply them and obtain the same results; systematic in the sense that clear rules are used to include or exclude content or analytic categories; and quantified in the sense that the results of content analysis are amenable to statistical analysis. Underlying this concern is the belief that the meaning of the text is constant and can be known precisely and consistently by different researchers as long as they utilize rigorous and correct analytical procedures (Silverman, 2001). Content analysis is the study of the text

itself not of its relation to its context, the intentions of the producer of the text, or the reaction of the intended audience.

While discourse analysis and content analysis are both interested in exploring social reality, the two methods differ fundamentally in their assumptions about the nature of that reality and of the role of language in particular. Where discourse analysis highlights the precarious nature of meaning and focuses on exploring its shifting and contested nature, content analysis assumes a consistency of meaning that allows for occurrences of words (or other, larger units of text) to be assumed equivalent and counted. Where discourse analysis focuses on the relation between text and context, content analysis focuses on the text abstracted from its contexts. On the surface, the difference between the two methods could not be more stark (see Table 1). While discourse analysis is concerned with the development of meaning and in how it changes over time, content analysis assumes a consistency of meaning that allows counting and coding. Where discourse analysts see change and flux, content analysts look for consistency and stability.

It is, however, worth pointing out that there are forms of content analysis that look much more like discourse analysis (Gephart, 1993). More qualitative forms of content analysis that do not assume highly stable meanings of words but, rather, include a sensitivity to the usage of words and the context in which they are used are compatible with discourse analysis and can, in fact, be used within a broad discourse analytic methodology in the analysis of social reality. In Table 2 we provide an indication of how content analysis might be used in a way that is compatible with discourse analysis. As one moves from simple counting to more complex interpretation, the two forms of analysis become increasingly compatible, although at the expense of positivist objectives. For content analysis to form part of a discourse analytic methodology, it is necessary to weaken the assumption that meaning is stable enough to be counted in an objective sense. From a discourse analytic perspective, all textual analysis is an exercise in interpretation and while clear exposition of the methods used to arrive at a particular interpretation is a hallmark of good research, it cannot remove the necessity for interpretation. With this proviso, content analysis can, through its focus on being systematic and quantitative, play a potentially useful role in expanding our understanding of the role of discourse in constructing the social.

Conclusion

In conclusion, while discourse analysis and content analysis come from very different philosophical bases, they can be complementary. Traditionally, the differences mean that they provide alternative perspectives on the role of language in social studies. In this regard, they are complementary in terms of what they reveal despite conflicting ontology and epistemology, which is most easily seen in the focus in content analysis on reliability and validity, contrasting sharply with the focus on the interpretive accuracy and reflexive examination that characterizes discourse analysis. More interpretive versions of content analysis also complement discourse analy-

Table 1: Differences between Discourse Analysis and Content Analysis

	Discourse Analysis	Content Analysis
Ontology	Constructionist - assumes that reality is socially constructed	Realist - assumes that an independent reality exists
Epistemology	Meaning is fluid and constructs reality in ways that can be posited through the use of interpretive methods	Meaning is fixed and reflects reality in ways that can be ascertained through the use of scientific methods
Data Source	Textual meaning, usually in relation to other texts, as well as practices of production, dissemination, and consumption	Textual content in comparison to other texts, example over time
Method	Qualitative (although can involve counting)	Quantitative
Categories	Exploration of how participants actively construct categories	Analytical categories taken for granted and data allocated to them
Inductive/Deductive	Inductive	Deductive
Subjectivity/Objectivity	Subjective	Objective
Role of context	Can only understand texts in discursive context.	Does not necessarily link text to context
Reliability	Formal measures of reliability are not a factor although coding is still justified according to academic norms; differences in interpretation are not a problem and may, in fact, be a source of data	Formal measures of intercoder reliability are crucial for measurement purposes; differences in interpretation are problematic and risk nullifying any results
Validity	Validity in the form of "performativity" i.e., demonstrating a plausible case that patterns in the meaning of texts are constitutive of reality in some way.	Validity is in the form of accuracy and precision i.e., demonstrating that patterns in the content of texts are accurately measured and reflect reality
Reflexivity	Necessarily high - author is part of the process whereby meaning is constructed.	Not necessarily high - author simply reports on objective findings.

Table 2: Using Content Analysis within a Discourse Analytic Approach

Dealing with Meaning	There is no inherent meaning in the text; meanings are constructed in a particular context; and the author, consumer, and researcher all play a role. There is no way to separate meaning from context and any attempt to count must deal with the precarious nature of meaning.
Dealing with Categories	Categories emerge from the data. However, existing empirical research and theoretical work provide ideas for what to look for and the research question provides an initial simple frame.
Dealing with Technique	The categories that emerge from the data allow for coding schemes involving counting occurrences of meanings in the text. Analysis is an interactive process of working back and forth between the texts and the categories.
Dealing with Context	The analysis must locate the meaning of the text in relation to a social context and to other texts and discourses.
Dealing with Reliability	The results are reliable to the degree that they are understandable and plausible to others i.e. does the researcher explain how s/he came up with the analysis in a way that the reader can make sense of?
Dealing with Validity	The results are valid to the degree that they show how patterns in the meaning of texts are constitutive of reality.
Dealing with Reflexivity	To what extent does the analysis take into account the role that the author plays in making meaning? Does the analysis show different ways in which this meaning might be consumed? Is the analysis sensitive to the way the patterns are identified and explained.

sis in that they may be usefully combined in a single study: the more structured and formal forms of discourse analysis are compatible with the more interpretive forms of content analysis. Research is, from this perspective, an exercise in creative interpretation that seeks to show how reality is constructed through texts that embody discourses; in this regard, content analysis provides an important way to demonstrate these performative links that lie at the heart of discourse analysis.

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Understanding Discourse: A Method of Ethical Argument Analysis*

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In 1862 Bismark said, "The great questions of the age are not settled by speeches and majority votes . . . but by iron and blood. (Quoted in Shulze, 1998: 140)" While beautifully evocative, Bismark's reasoning raises more questions than his formulation answers. What are the great questions of an age? How do those preoccupations arise? If political argument is meaningless, or nearly so, why do actors engage in it? And if some issue is settled by force, what led individuals and nations to sacrifice their blood and treasure, their sons and daughters? Realists generally say that one of two factors typically explains the preoccupations of an age and the resort to force; humans are motivated by either material interests or the drive for the power necessary to secure their interests. We need look no deeper.

Yet there are obviously cases where actors disagree about their "interests," don't know their interests, or act contrary to a wish to enhance their power. For example, realists would have predicted that Great Britain keep its preeminent position as the world's largest slave trader in the 18th and 19th Century; yet the British ended their own participation in the trade in 1807 and spent millions in treasure and thousands of lives in blood over the next decades to suppress the trans-Atlantic slave trade. How did the slave trade and slavery, once taken for granted as good, just, virtuous and right for *both* master and slave, become stigmatized and eventually abhorred as illegitimate and human institutions? Such questions are about the meanings individuals and groups attach to practices and how those meanings change. Discourse analysis can help uncover the meanings that make the "great questions of an age" and underpin the dominant relations of power. Discourse and argument analysis can also help us understand how those meanings, and the social practices associated with them, change.

Aims and Varieties of Discourse Analysis

Discourse analysis assumes that discourse — the content and construction of meaning and the organization of knowledge in a particular realm — is central to social and political life. Discourses set the terms of intelligibility of thought, speech, and action. To understand discourses then is to understand the underlying logic of the social and political organization of a particular arena and to recognize that this arrangement and the structures of power and meaning underpinning it are not natural, but socially constructed. For example, contemporary western science is a discourse which assumes certain facts about the physical world and how we should come to both know it and manipulate it. That under-

standing has evolved, and is still evolving, while its content and evolution is related to other discourses in the social and political world. For example, gender and racial discourses intersect with western scientific discourse. Further, the discourse of western science privileges some practices and actors, legitimizing them, and delegitimizes other practices and actors. For example, alchemists are derided and physicists are exalted.

Discourse analysis, which can help us decipher the underlying meaning, deep assumptions, and relations of power that are supported by and constructed through a discourse, can be done many different ways depending on the type of discourse to be understood and the purpose of the analysis. Aristotle (1991) was an early scholar of argumentation and rhetoric. Hayward Alker (1996), building on Aristotle and others, offers examples of how to model narratives, arguments and fairy tales. Karen Litfin shows how scientific discourses “do not solve environmental problems — they merely offer alternative interpretive lenses through which problems can be viewed, lenses that lend themselves to certain policy solutions” (1994, 194). Roxanne Doty (1996) examines how “representations” structure relations between North and South. Karin Fierke (1998) uses Wittgenstein to analyze the language games of international politics and in particular how discourse by non-state actors can change relations of power among states. Space does not allow discussion of other approaches taken for example by Hopf (2002), Weldes (2003), and the authors in Weldes, et al. (1999). For a recent survey of critical discourse analysis and its techniques see Wodak and Meyer (2001). And as Laffey and Weldes argue in this issue, discourse analysis need not be restricted to the analysis of written texts.

Argument Analysis

Meanings are constructed over time within and across cultures and so also are political arguments made and political issues decided over some duration. Thus, anyone seeking to understand how certain interpretations of the world became dominant, how other views were submerged or erased, and how new meanings took hold must examine some slice of the discourse prior to and co-terminus with the question they are interested in. In other words, the analyst must make choices about the kind of discourse they will focus on and the boundaries of the discourse — both temporal and genre — that they will examine.

If one is interested, for example, in how particular nuclear arms control questions were understood by participants one might engage in argument analysis of a discrete debate or formal argument. For example, Homer-Dixon and Karapin (1989) use graphical argument analysis to articulate and expose the “warrants” and data for claims by interlocutors during the “window of vulnerability” debate. Their method is suitable for explicating the architecture, if not the deeper meaning, of the logic of claims and how attacks might affect the strength of an argument. Alternatively, Duffy, Federking and Tucker used “dialogical analysis” to understand US-Soviet arms control negotiations which they test by showing that “certain

‘action theorems’ follow logically from the contents of belief inventories. (1998: 272)”

But sometimes the arguments of interest are not fully captured by formal and discrete debates among a small set of interlocutors. For example, in my recent work, I asked why slavery and colonialism ended when those practices were, arguably, still profitable. In other words, the realist and materialist explanations for the change in these longstanding practices were inapplicable. Why did the dominant beliefs about these practices change? While there were certainly discrete debates — such as the disputation between Bartolome de las Casas and Juan Gines Sepulveda in 1550 — the arguments about these practices occurred over five centuries, and involved many actors. I therefore needed to find a way to assess not only the logic or pragmatics of discrete debates, but to understand the content of informal ethical and practical arguments and to evaluate their causal importance. To do so, I developed a method of informal argument analysis to understand the underlying beliefs, political purchase, and persuasiveness of informal political arguments about slavery and colonialism that occurred over a several hundred year period.

The method of argument analysis of informal ethical arguments, as I developed it, occurs in five steps. First, having identified a problem or issue area, analysts seek to identify the purpose of particular arguments that are being used in efforts to maintain or challenge a practice. Analysts must then specify the argument’s role. Whether arguments are intended to facilitate deliberation, reframe the issues, persuade others, or do all of these things, may be inferred from what the speaker says and by the location (forum) where the arguments are made. In the transition from established behavioral norms to new norms, there are likely to be periods of confusion and uncertainty. With two or more conflicting (and perhaps nearly equally legitimate) prescriptive normative beliefs on the table, expectations will be uncertain, coordination will be more difficult, and the sense of approval or disapproval associated with certain practices may be in flux. It is at these points when ethical arguments may be the most prolific and explicit, as interlocutors strive to be clear and persuasive in their attempts to maintain an existing practice or establish a new mode of behavior.

Second, one must identify the specific beliefs that are held by dominant actors and that are at work in a particular political context. As Jonson and Toulmin note, “Each discipline has its special field of debate, within which people of experience share *kanoi topoi* (‘commonplaces’) — that is, bodies of experience that underlie the forms of argument that guide deliberation and discussion in the particular field. (1988: 74)” The goal is to find the *topoi* (starting point) of the arguments actors used to uphold or change practices and the background of pre-existing beliefs that interlocutors presupposed in making their arguments.

Third, informal argument analysis, as distinct from the analysis of discrete debates, expands the time horizon and asks where immediate and background beliefs came from and why and how they changed. Analysis of political arguments must thus be context sensitive, looking for the deeper beliefs

that are the starting points and background assumptions without which the arguments would be unintelligible. This entails tracing the process and examining the content of decisionmaking over long periods of time within particular historical and cultural contexts. The focus is on the articulation, content, contestation, and flow of arguments.

Fourth, informal argument analysis may attempt to show how and why some beliefs and arguments won out over others and ultimately why certain policies were chosen. In practice this means tracing whether and how the ethical arguments put forward succeeded in changing the terms of debate and whether an ethical argument meant to overturn a practice was able to denormalize, delegitimize, change actors' conceptions of possibility and their interests, alter the balance of political power, and have its normative beliefs institutionalized. This also entails looking at the grounds for change in the support for conformity and receptivity to new arguments. Informal analysis of ethical arguments thus emphasizes the content and process of arguments — the words used (and not used), appeals actors make to dominant (unquestioned) beliefs and other normative beliefs, claims about legitimacy, and the use of evidence. This method focuses on how the arguments develop over long periods of time, in particular social settings, including definition and redefinition of the problem (meta-arguments or framing), and the evolution of the features in the argument that are taken for granted or contested.

Fifth, the results of informal argument analysis ought to be compared with other plausible explanations for behaviors to see whether the arguments are important causally. There are several "tests" for the causal significance of ethical argument. 1) temporal ordering — normative beliefs and ethical arguments should be given as a justification for the behavior before or simultaneous to a behavior change, not after; 2) after an ethical argument succeeds, one would expect a (not necessarily universal) congruence between the normative beliefs that underpinned the ethical arguments and the behavior; 3) the relevant normative beliefs should be used in arguments about correct behavior and those who use those arguments are not ignored or mocked; 4) when the prescriptions for behavior implied by the ethical argument are not adhered to, those who do not adhere to the standards of normative belief attempt to justify their (non-normal) behavior on ethical or practical grounds (these actors thus acknowledge that they are norm violators and make an argument about why their violation was good or necessary); 5) the normative belief is linked with other normative beliefs, becoming part of the arguments used to advance these other norms. For example, anti-slavery, human rights, and self-determination beliefs should be discussed with each norm's reasoning being used to legitimize the other norms.¹ The new norms become part of what is seen to be a web of interrelated discourse.

Two harder tests of the role of normative belief and ethical argument are: 6) the presence and use of international sanctions by the majority of the international community to change the behavior of those who violate the normative pre-

scriptions or those who support such norm violators. Finally, 7) ethical arguments may be viewed as causally important whether and to the extent that actors with incentives to violate normative prescriptions act counter to their "interests" and follow the new normative prescriptions, or to the extent that actors re-frame their interests in light of coming to hold new normative beliefs. For the last test to be valid three conditions should hold: states (or rather the influential elites that shape government policies) and other actors should "know" their interests (or at least believe they do); actors should not have been compelled by other (non-normative) circumstances, such as a change in their ability to pursue their interests; and some more efficient solution for achieving the same ends, while not technically violating the normative prescriptions that followed from ethical arguments, was not found. This "interest" test should not be seen as assuming a dichotomy between the normative and the self-interested behavior of actors. Ethical arguments may be used to change actors conceptions of their interests, and successful ethical arguments may alter the political situation to the point where it changes the material capabilities of actors. Rather, this test focuses our attention on the crucial relation between the ideational and material.

Challenges of Discourse and Argument Analysis

No matter what method of discourse analysis one chooses, there are numerous challenges to doing discourse analysis. The first challenge, and perhaps the most daunting encountered by any scholar, is identifying the bounds of relevant discourse. As Roxanne Doty notes, "discourse delineates the terms of intelligibility whereby a particular 'reality' can be known and acted upon. When we speak of a discourse we may be referring to a particular group of texts, but also importantly to the social practices to which those texts are inextricably linked. . . . a discourse is inherently open ended and incomplete. . . . Any fixing of a discourse and the identities that are constructed by it, then, can only ever be of a partial nature. (1996: 6)" Thus, discourse analysis involves making hard choices of the extent and limits of analysis. Which leads to the next set of problems, those of interpretation and reliability, which are dealt with differently by the various approaches noted above. In other words, our analysis may not only be so large as to be unwieldy and overwhelming, but it is also necessarily partial and subject to dispute by others. There is not space here to discuss the various ways scholars who employ discourse analysis tackle these challenges. Suffice it to say however, that nearly all the scholars I have mentioned have given explicit attention to these questions.

There are two other elements of discourse analysis which there is also insufficient space to discuss here. Scholars who engage in discourse analysis must have a thorough understanding of the context of the discourse they are analyzing — modes of production, class structure, political formations — in order to situate their analysis and explain relationships. And those who engage in discourse analysis should be empathetic. Specifically, while an unreflective belief in the

discourse one is analyzing is actually unhelpful, a certain degree of empathy — the cognitive and emotional apprehension the world from another perspective — can sharpen the analysis. Indeed, a well developed sense of empathy would probably be a useful asset for many forms of both quantitative and qualitative analysis.

Notes

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¹ Even if normative beliefs and ethical arguments pass all of these “tests,” we still cannot *prove* causality. However, passing all or several of these tests make it more *likely* that normative belief and ethical argument had a causal role.

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Content Analysis and its Place in the (Methodological) Scheme of Things

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In this article I’m going to argue two points. First, content analysis should not be seen as fundamentally different to any other type of quantitative method available to political scientists. And second, that political methodologists need to integrate content analysis into their current statistical machinery.

The first point is descriptive, and I’ll defend it by drawing some detailed analogies between existing methods, e.g. cross-tabs and regression, and seemingly unrelated content analytic techniques. At the high level, this will involve sketching out a probabilistic framework for understanding content analysis in general. More specifically, I’ll try to show that content analytic techniques often make most sense if they are understood as implementations of particular statistical models. These models and their assumptions are seldom made explicit, so uncovering them will have three desirable effects. First, and most obviously, knowing what assumptions your methods presume allows you to recognize when applying them is likely to be appropriate. Concretely, this helps answer the question: “should I use content analysis for this?” Second, a good understanding of assumptions makes it easier to relax or change them individually, in response to substantive needs. Finally, since the assumptions are explicitly probabilistic in nature, the methods can be integrated into standard statistical theory.

Philosophically, the purpose of constructing a probability model for existing content analytic practice is not description but explication. Content analysts may seldom work with a probability model in mind when doing research, but reconstructing a model is a way of explaining why, and under what conditions what analysts do makes sense.

A class of probability models for content analysis

Arguably, the methodologist’s standard statistical tool is the linear regression model. At the most basic level, a regression relates some observable phenomena, the x-s, to other observable phenomena, the y-s, and assumes that x-s are measured without error, and that conditional on x values, y-s are randomly distributed around a mean value deterministically related to x. This is not a framework immediately well-suited to understanding content analysis, since although words and phrases are observed, their *content* is only inferred. For a more useful model approach we must look to psychology, to the classical literature on intelligence and individual differences that developed factor analysis. Content analyses, by

which I mean the standard dictionary-based methods embodied in e.g. Diction (Hart 1997) or VBPro (Miller, 1997) are best understood as *latent variable models* (Everitt, 1984), of which factor analysis is one instance.¹ Structural equation modeling (e.g. LISREL) and Bayesian networks (Pearl 2000) are two more powerful examples.

In the simplest form of latent variable model *unobserved* variables, which we can still usefully call x , give rise to observable effects, the y -s, which are randomly distributed around a mean value deterministically related to x . In other words, this is a regression where x is not observed. As in ordinary regression analysis we focus on the conditional distribution (or likelihood function) $p(y|x)$. *Inference* in a latent variable model involves inverting this distribution to obtain a probability distribution $p(x|y)$ over values of x when a particular y is observed.

To begin the analogy, x describes the content of document, and y measures its observable features e.g. its words and phrases and how often they occur. Content analysis specifies the mapping from x -s to y -s by building a dictionary of words and phrases. The dictionary states how a particular underlying concept or content is expressed in words and phrases. It is a mapping from particular content to observables. We then infer the content of a new document by inverting this mapping to get its probable content.

The conditional distribution $p(y|x)$ describes how a determinate but unobserved concept or content x gives rise to different (random) choices of words and phrases.²

In classical latent variable models, x is inferred by assigning it a prior distribution $p(x)$, and using Bayes theorem to find $p(x|y)$:

$$p(x|y) = p(y|x)p(x)/p(y)$$

Psychologists can often make strong assumptions about x , e.g. many psychological abilities are Normally distributed in the population. In content analysis this is not usually possible, so $p(x)$ may be taken to be flat. In this case $p(x|y)$ is proportional to $p(y|x)$.

Probability models and assumptions

To take a concrete example: Pennebaker's Linguistic Inquiry and Word Count (LIWC) content dictionary³ (Pennebaker and King, 1999) has an entry for the concept *insight*, containing the following list of words:

accept, acknowledge, adjust, admit ...

If there are K of these words, and we assume for simplicity that *insight* is the only content category in the dictionary, then one explicit probability model might look like this:

$p(y | x=insight)$ is a Multinomial distribution with probability values of $1/K$ for each of the words in the list, and 0 for every other word.

Since all conditional distributions express a recipe for generating data, in regression as well as latent variable models, we can think of the Multinomial $p(y|x)$ in the same way: Whenever the author wants to express *insight*, she picks a

word from this list randomly and inserts it into the text. And if there are several categories, then the recipe for generating a whole text is:

- 1) pick a content category,
- 2) pick a word in that list at random,
- 3) write down the word,
- 4) go to 1.

These text generation assumptions listed above are seldom stated as such, perhaps because they are quite implausible when revealed to the light, but the success of content analytic methods are a credit to their practical applicability.

The relevant assumptions here are then:

- *conditional independence*: word choice is conditionally independent given the content category. This is the random generation part.
- *irrelevance of syntax*: all non-content related factors that structure a text are noise,
- *equal category probability*: each category is as likely as any other.

Working the analogy

Returning to the single category case, the quantity $p(y | x=insight)$ can be used to rank any new document according to how much of the LIWC concept *_insight_* is expressed in it using Bayes theorem.⁴ This model is an explication because $p(y | x=insight)$ will rank any new documents (with words y) in the same order as simply counting up the number of times each word in the LIWC entry occurs.⁵ It is in this sense that it is useful to think of this as the implicit probability model underlying content analysis: *If* the generating mechanism for text is as described above and the assumptions are fulfilled, *then* constructing dictionaries as lists and counting occurrences in documents will be the best way to infer content.

With assumptions in view then, we can begin to change them. Some immediate changes would be: to change the *equal category probability* assumption on the basis of data. We expect different genres to have different probabilities of expressing categories in same dictionary. And we expect that not all *insight* terms have an equal chance of occurring in a document e.g. because authors avoid repetition or because some terms are more formal or forceful than others. Finally, content is certainly not the only force determining how words occur, so we might model the others e.g. by adding grammatical constraints, or introducing serial dependencies, perhaps using n -gram models borrowed from computational linguistics. It might also be useful to think of content analysis categories in the same way as linguists think of parts of speech - as the unobserved variable that determines whether 'bank' is a something a river has, or something a plane does - and use a Hidden Markov Model (Rabiner, 1989) tagger to assign content in a way that relaxes the conditional independence assumption.

All these suggestions take the content model beyond anything that would be effectively implemented with a list of categories and word lists. But that is exactly the point. Knowing the assumptions we have been making allows us to ad-

vance beyond them, and reconstructing them probabilistically allows us to leverage existing statistical methods.

Model specifics aside, reconstructing content analysis in a probabilistic framework provides another useful opportunity. Any probabilistic content analysis model implicitly defines a discourse, in the sense that such a model is a theory of what is more or less likely to be said, and of what the conceptual elements are that generate and constrain these possibilities. There is certainly more to discourse than simple conditions of lexical possibility, but the non-verbal structures that discourse and content analysts use words to investigate cannot completely float free of their texts and leave language choice unconstrained.⁶ Consequently, texts might therefore be usefully compared under several models, and questions like “has this discourse fundamentally changed?” addressed as “is this new text probable under my old model, or is it better thought of as generated by this new one?” These new questions will be recognizable to political methodologists as the familiar and well-studied problems of model selection.

Integrating content analysis

If content analysis really can be understood as a implicit form of statistical inference, then there is no reason not to bring it into the existing methodological fold. Aside from making good scientific sense, there are sociological advantages to doing so. Content analysis has rather negative associations in some political methodology circles, but “methods are not epistemologies” (Wendt, 1999), and there is nothing about content analytic methodology that decides epistemological position. Thus although this article has attempted an explication that emphasizes consistency with positivist inference standards, another explication might reconstruct content analytic practice as quite a different exercise. But that is the nature, and utility of explication.

If the broadly positivist path described above is pursued, then one way to demonstrate the continuity of content analysis with the familiar toolbox of statistical methods is simply to go ahead and integrate the two, so proving it can be done. This article is a first sketch of how we might start. The advantage of integration to more traditional methodology is a much needed broadening of outlook, and crucially, a set of ways to deal systematically with text. The advantages to content analysts are ready access to highly articulated theory and long collective experience of data analysis in many forms. The advantages to both sides seem too great to turn down.

Notes

¹ Two excellent sources of information on contemporary content analysis methods, available software, and approaches are <http://www.car.ua.edu> and <http://www.content-analysis.de>.

² ‘Random’ here is used in its statistical sense to denote anything unrelated to the mechanism being characterized. In an information extraction task, ‘who did what to whom’ would be x and the noise in y would cover stylistic variations, whereas in a rhetorical study of the same text, the style itself would be x, and ‘who did what to whom’ merely noise. In both cases,

necessary grammatical structure would be noise, since one cannot simply throw down words in any order to communicate content.

³ A content dictionary in this context means a mapping a set of words or phrases to one word; the one word is the label of a substantive category and the set describes the words or phrases that indicate the tokening of the category in text. As an example, for the substantive category, *death* (category 59), the Linguistic Inquiry and Word Count (LIWC) dictionary maps the word set {ashes, burial*, buried, bury, casket*, cemet*, coffin*, cremat*, dead death*, decay*, decease*, deteriorat*, die, died, dies, drown*, dying, fatal, funeral*, grave*, grief, griev*, kill*, mortal*, mourn*, murder* suicid*,terminat*} to LIWC category 59. The asterisks are “wild-card” characters telling the program to treat “cremating,” “cremated” and “cremate,” as all matching cremat*, and thus all mapping to category 59. For the substantive category, *insight* (category #22), LIWC lists 117 terms in the word set.

⁴ And if there is more than one content category it can for each word provide an estimate of the probability that any particular word expresses each of the available categories. This is explicitly built into Benoit, Laver and Garry’s (2003) work on Wordscores.

⁵ Actually it will do better since it will correct for the document length.

⁶ To assume that they can is a form of Cartesian skepticism that is as immune to reassurance as it is to empirical argument.

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Methodological Reflections on Discourse Analysis

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Part of the difficulty in establishing methods for discourse analysis (DA) is that DA is not singular. Distinct forms of analysis are collected under this label, invoking different understandings of discourse, drawing on different disciplines and canons, and specifying different methodologies. DA is most commonly understood as referring to language. Analyses thus often deploy the term discourse to refer to 'extended samples of spoken dialogue' (Fairclough, 1992: 3) such as conversations and so prompt analysis of, among other things, turn taking or the structure of conversational openings and closings (e.g., Atkinson and Drew, 1979; Coulthard, 1992).

For us, discourse is not equivalent to language. Instead, we define discourse as structure and practices. As structure, discourses are 'sociocultural resources used by people' – and which use them – 'in the construction of meaning about their world and their activities' (Ó Tuathail and Agnew, 1992: 192-3). As practice, they are structures of meaning-in-use. This conception of discourse implies that:

- Discourses are sets of rules that both enable practices and are reproduced and/or transformed by them. In examining a discourse, we examine 'a group of rules' that define not the dumb existence of a reality, nor the canonical use of a vocabulary, but the ordering of objects' (Foucault, 1972: 49).
- Discourses manifest themselves in both linguistic and non-linguistic practices (Laclau and Mouffe, 1987: 82-4). The study of discourse therefore cannot be limited to the study of texts or language narrowly defined (Neumann, 2002).
- Discourses are productive. They produce subjects, objects, and the relations among them. They produce truth as well, stipulating the criteria according to which claims are judged. DA thus highlights the mutual implication of power and knowledge.
- Discourses are always implicated in institutions, broadly conceived. They circulate through and around – sometimes reinforcing, sometimes challenging, sometimes participating in or being expressed through, sometimes completely ignored or marginalised by – sites of institutionalised power.
- Discourses are inherently political. They are about the production and distribution of power, and struggles

over knowledge, interests, identity and the social relations they enable or undermine.

Method

As Milliken observes, there is disagreement amongst discourse analysts over whether to do 'methods' at all (1999: 226-7). Like her, we do not believe that attention to method and rigor necessarily entails the sort of 'scientism' against which many discourse analysts define themselves. In methodological terms, we understand DA to entail the retroduction of a discourse through the empirical analysis of its realization in practices (Laffey and Weldes, 1997: 210).¹ That is, DA reasons backward to establish structure from its empirical manifestations. It asks what the conditions of possibility are of this or that particular discursive production. At the same time, it also examines how discourses are naturalised in such a way as to become common sense, the 'regime of the "taken-for-granted"' (Hall, 1985: 105). 'Method' in this context thus refers to the conceptual apparatus and empirical procedures used to make possible this retroduction.²

Two concepts that help us to get at these related aspects of discourse are articulation and interpellation.³ Articulation itself has two dimensions. First, it refers to the practice of creating and temporarily fixing meaning through the contingent connection of signifying elements, whether narrowly linguistic or broadly semiotic. Through articulation, different terms, symbols and meanings come to connote one another and thereby to be welded into associative chains (Hall, 1985: 104). Second, articulation refers to the connection of these meanings to institutions and social relations. The notion of articulation implies that these connections are socially constructed, historically contingent, and therefore require a great deal of ideological labor to establish and maintain. At the same time, it means that articulations can be broken, making rearticulation an ever-present – if more or less difficult – possibility.

Interpellation refers to a dual process whereby subject-positions are created and concrete individuals are 'hailed' into or interpellated by them (Althusser, 1971: 174). That is, interpellation means, first, that specific identities are created when social relations are depicted. Different representations of the world entail different identities – they make sense from or presuppose a certain interpretive position – which in turn carry with them different ways of functioning in the world, are located within different power relations, and make possible different interests. Second, in a successful interpellation concrete individuals come to identify with these subject-positions. Once they identify with them, the representations in which they appear make sense and the power relations and interests entailed in them are naturalized. An important condition of such naturalization is the practical adequacy of representations to the social realities people face. As a result, the representations appear to be common sense, to reflect 'the way the world really is.'

How then, armed with these concepts, does DA proceed empirically?⁴ Articulation can be investigated through a series of analytical steps. These might entail, first, investigat-

ing representational practices. The main signifying elements of the discourse, whether linguistic or non-linguistic, must be identified. Early neoliberal discourses like Thatcherism, for example, were constructed out of discursive elements such as 'free markets', 'big government', and the like (Hall, 1988: 39; Peck and Tickell, 2002). Chains of connotation among these signifying elements might include the linking of 'unemployment' to 'welfare state' to 'big government' and in turn to 'de-regulation' and 'privatization' in order to make markets 'free' and 'flexible', for example, or the linking of patriarchal notions of the 'solid English citizen' to 'free markets' through 'working-class respectability' (Hall, 1988: 50). Within such chains it is usually possible to identify nodes where several different connotative chains come together (Laclau and Mouffe, 1985). Signs are always multi-accentual, in the sense that different social interests can be refracted through particular signs – such as 'freedom' or 'democracy' or 'respectability' for instance – in different ways, with diverse ideological effects (Purvis and Hunt, 1993). It is for this reason that such nodes themselves become sites of struggle, as overlapping and competing discourses seek authoritatively to define what is real, true or possible.

Further analysis requires discovering the articulations of these representations with, and their sedimentations in, institutions. Some discourses are more powerful than others because they are articulated to, and partake of, institutional power. Thatcherism, for instance, was first articulated to think tanks like the neo-liberal Institute for Economic Affairs and the Centre for Policy Studies, then to the Conservative party, and gradually to media outlets like *The Sun* and *The Mail* (Hall, 1988: 46-7). Investigating articulations also involves examining power/knowledge relations as these are the mechanisms that obscure or naturalize relations of power. The 'ideological effects of representations are not internal to the representations themselves but are closely bound up with the contexts in which they are deployed' (Laffey and Weldes, 1997: 211). DA thus necessarily entails considering not only representational practices but also social relations. This involves examining, for instance, the normalizing effects of Thatcher's splendidly ideological claim that 'there is no alternative' to the free market, despite the existence of alternatives, and the curious fact that the size and penetration of the state into society has actually increased under neoliberalism (Brenner and Theodore, 2002)

Interpellation, also, is investigated through a series of analytical steps. Crucial is the discovery of the subject positions – identities of subjects and objects and their position relative to others (e.g., Doty, 1993: 306) – constructed in the discourse. U.S. cold war discourse, for instance, constituted 'the U.S.' in opposition to 'the Soviet Union'. The analysis of predication – the linking of qualities, literally predicates, to subjects and objects – tells us what meanings attach to them. Predicates attached to the U.S. thus included freedom, honesty and openness, democracy and defensive strength. The Soviet Union, in contrast, was a slave state, duplicitous and secretive, despotic and aggressive. The oppositional subject positions, that is, followed from the predication.

A crucial question in the investigation of interpellation is 'who speaks', that is, which subject position authorises the discourse? A good example here is the striking 'we' of U.S. political discourse (Weldes, 1999: 105-107). This 'we' is a 'shifty shifter' (Schwichtenberg, 1984: 305) that facilitates interpellation: it is a referentially ambiguous pronoun that allows authorship to slip between and among 'we, U.S. decision makers,' 'we, the U.S. state,' and 'we, the U.S. public.' Identifying a shifty shifter exposes a mechanism that helps to construct a subject position – 'we, the U.S.' – while simultaneously welding disparate audiences into a single identity, creating common sense by hailing concrete individuals into that identity, and legitimating the argument in which the identity participates. 'Of course, "we, the U.S." – freedom loving democrats – must combat totalitarianism wherever "we" find it.' In turn, not only must such a subject position make sense, i.e., be meaningful, it must also make it possible to negotiate the world. Interpellation thus gets at the question of practical adequacy. It is this 'making sense' of interpellation – in both senses – that generates common sense – the moment of ideological closure, of normalization and naturalization, when those hailed by the discourse say 'yes, of course.'

Examining processes of interpellation and the question 'who speaks?', also highlights other power relations. 'Power/knowledge' practices (Foucault, 1980) privilege some actors and voices while marginalizing others. One can investigate which subjects are privileged over others, for example, and how this manifests itself in both linguistic and non-linguistic practices. As Schram shows in his analysis of welfare discourse, 'top-down managerial discourse constructs poverty and welfare statistics in ways that emphasize the state's managerial concerns at the expense of the concerns of the poor' (1995: 77). The result is a neoliberal shift from welfare as the solution to poverty to welfare dependency as itself the problem. In the process, the issue of poverty is attributed to a marginal group – and thus itself marginalised – rather than being seen as a persistent structural feature of capitalist societies.

Power and Politics

DA is always about power and politics because it examines the conditions of possibility for practices, linguistic and otherwise. As such, it exposes the ideological labour that goes into producing meaning and the ideological effects of particular structures of meaning-in-use. The discursive structures that make meaning possible are the 'stakes, par excellence, of political struggle, the inextricably theoretical and practical struggle for power to preserve or transform the social world by preserving or transforming the categories by which it is perceived' and enacted (Bourdieu, 1985: 729). Because discursive practices entail power relations, they become sites of contestation and struggle.

None of this is accessible through content analysis (CA). The emphasis in CA on patterns in documents, on identifying content units (words, themes, stories and the like) and their clustering, does offer a preliminary way of accessing processes akin to articulation. There is also a superficial similarity be-

tween the CA emphasis on messages and our focus on interpellation. But these similarities disappear once we discard the mistaken assumption that DA and CA both examine ‘language’ (cf. Neuendorf, this issue). The emphasis in DA is discursive practices and the structures – linguistic and non-linguistic – that enable them. At stake is the discursive structures that make certain kinds of representations and practices possible and – for many – plausible, not the mere frequency of particular words or their patterns in the representations themselves. To repeat, DA is about power and politics, not language. The one is not reducible to the other.

Notes

¹ The limits of a discourse are those boundaries ‘when particular representations of the world seem “unintelligible,” “irrational,” “meaningless,” or “ungraspable” in and through the symbolic resources offered by’ the discourse itself (Muppidi, 1999: 124-5).

² There are divergent methods even for investigating discourse defined as structures of meaning-in-use, including computational modelling (e.g., Hudson, 1991), predicate analysis (e.g., Milliken, 2001), metaphorical analysis (e.g., Lakoff and Johnson, 1980) or the approach we discuss here. They can be used in combination.

³ For more detail see Althusser, 1971; Laclau, 1979; Hall, 1985; Weldes, 1996, 1999.

⁴ In our view, DA methods are replicable and subject to intersubjective checking (see also Crawford, Fierke, and Hopf, this issue).

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Discourse and Content Analysis: Some Fundamental Incompatibilities

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Let me begin with a caveat. My version of discourse analysis that unfolds below is as much a normative ideal in my own mind, as much as a description of how it actually is or has been done in practice, although I find the Hardy, Harley and Phillips's (HHP) definition a very comfortable fit. Therefore, it may come off like "already existing socialism" came off to residents of the late Soviet Union. Moreover, my understanding of what content analysis is and does is doubtlessly biased in the opposite direction, some vague memories of graduate school reading assignments that purported to be using that method supplemented by the contributions to this symposium. I apologize for both biases, but hope you will not stopreading just yet! What follows is an enumeration of those aspects of DA that I find fundamentally incompatible with CA, conventionally understood. (Though HHP's surfacing of a more qualitative CA softens some of these differences considerably) But then, perhaps surprisingly, I claim that DA is in fact much more "scientific" than either practitioners of DA or CA acknowledge.

Seven important differences:

- DA is a theory of power politics
- DA assumes an open social system
- DA assumes social, not natural, kinds
- DA places texts in intersubjective context
- DA treats anomalies and absences as evidence, too
- DA is about daily social practices, a social text, not just a written one
- DA assumes at least the partial autonomy of language

From the outset, it must be acknowledged that DA is in fact a political theory as much as a method of inquiry. As Crawford and HHP explicate here, DA assumes that language is a medium within which prevailing power relations are articulated. The use of discourse reproduces these predominant configurations of power, and disrupting and challenging them is one of the central features of what we call politics. This means that the meaning of any given text for DA often points to some underlying political problem or question. I do not think that the latter come up very often as topics of inquiry in CA.

DA recognizes that discourses reside in open social systems. (Ricoeur 1984, 135) To put it another way, it is impossible to control for "omitted variable bias." (King, Keohane, Verba 1994, 172) The meaning of any conversation or family outing is not fixed; it depends on the contexts in which a par-

ticular text is being performed. Not only is meaning dependent on the particular text in which a practice is located, but also on the relationship of that text to others. DA therefore assumes intertextuality, the relationship among texts. (Greenblatt 1990) And since it is impossible ever to specify with certainty where the texts stop, so to speak, it is an open social system, and statements about meaning must therefore be moderated accordingly. Neuendorf appropriately warns against "drawing generalized conclusions about one's qualitative findings." (Neuendorf 2004)

This lack of certainty about ultimate meaning is in part rooted in the assumption that the variables of interest are socially constructed in a particular intersubjective space, and so may not be present elsewhere, or at a different time. In other words, meanings and categorizations are social, not natural, kinds. They are products of human agency, not of God or Nature. So, these meanings stand mostly in relation to each other, rather than in relationship to an objective reality. Without the objectivist anchor, natural kinds are impossible, and so, generalizations with high levels of scientific certainty are, too. (Little 1991, 190-9)

Since meanings are bounded by context, DA does not expect a word like "market" to mean the same thing whenever and wherever it is uttered. Different meanings across different contexts is a hard reality for CA to assimilate into its "a priori coding instrumentation." (Neuendorf 2004) Instead statements and actions are always within a broader text that give them intersubjective meaning, and this single text itself is in meaning-giving and meaning-taking relationships with other texts. This necessary intertextuality implies not only that the meaning of a "variable" cannot just be assigned by a putatively objective observer, but also that this meaning cannot be assumed to be the same across time and place, KKV's "unit homogeneity" problem. (KKV 1994, 93-4)

DA does not pretend to solve the complex of problems here: open social systems, social kinds, omitted variable bias, and unit *heterogeneity*. Instead, it recognizes them as insuperable problems requiring not the application of yet more methodological techniques, but rather the admission that knowledge claims about social phenomena must simply be less ambitious. In other words, epistemology and ontology trump methodology. The result is strong claims, but within more bounded historical and temporal domains. For example, the context for Weldes's discursive analysis of the Cuban missile crisis is the Kennedy Administration in 1962; she does not make any claims about American foreign policy before or after that crisis. (Weldes 1999) Kier's account of French and British military doctrine before World War II relies on the discourses that predominated within the French and British military institutions in the 1930s. She does not make claims about militaries in the world in general, or even the French or British militaries after the war. (Kier 1997) DA recognizes its epistemological differences with CA, but does not try to overcome them, but instead bases more modest claims as a consequence of them.

DA also assumes that two kinds of phenomena matter that CA might not regard as so critical: absences and anoma-

lies. The assumption in DA is that if women never appear as engineers, race car drivers, presidents, or prime ministers, this is evidence of how daily lived reality is being socially constructed in a particular context. It is unclear to me how CA would theorize absent variables, given its emphasis on “a priori coding instrumentation.” Second, and related, in a predominant discourse of women absent in high status and high paying careers, the anomalous presence of a woman bank robber, for example, would be highly interesting to DA. It would perhaps be treated as the possible emergence of a counter-hegemonic discourse on gender, and would prompt more attention to see if other kinds of “subversive” practices were present in the text. CA would perhaps more likely treat anomalies as noise, unexplained variance, or part of the error term.

DA is about more than words or written texts. It is also about the daily conduct in which each of us engages to make our way in the social world. (Berger and Luckmann 1966, Bourdieu 1990, and Geertz 1973) While DA much too often neglects this ethnographic part of the picture, concentrating far too much on the written and spoken word, CA seemingly ignores it altogether. When a white woman walking down the street shifts her handbag to the other side of her body upon spying an approaching pair of young black men, she is discursively constructing race relations in that context. She has said not a word, and there is no written record of the event. But that simple action produces as much meaning as a few choice words. But I confess that this pushes DA beyond where it usually goes, as well.

Finally, DA usually assumes at least the partial autonomy of language, such that actors are themselves, in part, constituted by the daily practices in which they participate. (Doty 1983) CA, on the other hand, takes the speakers as given. This is reminiscent of the old debate between neorealists and regime theorists about international institutions. Are the latter just product, or are they productive? Do we just use language to express our preferences, or does language itself help constitute those very preferences in the first place?

A recent example springs to mind. I observed to a colleague of mine how surprised I was that the speakers’ series she directed was entitled Statesman’s Forum. She laughed and assured me there were no women leaders from the part of the world for which she was responsible. In this innocuous exchange is revealed her liberal confidence that language merely reflects reality and my assumption that language is helping create reality.

Carol Cohn, while a postdoctoral scholar at Harvard, discovered the power of the predominant discourse on nuclear weapons, such that some of the very concepts she would have liked to introduce into the conversation, peace, for example, were selected against by the prevailing discourse of deterrence and warfighting. (Cohn 1987) Notice I use the phrase selected against, and not prevented. After all Cohn was not physically restrained from talking about peace, but rather if she wanted to be a participant in the conversation, a speaker who would have continued legitimacy, authority, and credibility, she could not talk about peace in a way meaningful

to her. She, instead, had to assimilate herself to the discourse of post-exchange ratios, hard target kill capabilities, circular error probables, drawdown curves, and the like. Here is discursive power at work; here is the partial autonomy of language at work. Here is an obstacle liberal communicative action theorists have to overcome to make their version of an idealized speech situation work. Here is a problem with CA. How would a content analyst code Cohn’s use of words that she herself in fact has not autonomously chosen? One might suggest that the important theorizing here should be done about the discursive structure that is “causing” the language that has to be chosen to participate in the conversation, not the words themselves. I guess this is a bottom line of sorts: would CA have recovered what words mean during a conversation between Carol Cohn and her post-doc colleagues? I think my provisional answer is no, because finding the social or discursive structure that is already shaping the conversation is not part of the focus of CA, conventionally understood.

This discussion of the partial autonomy of language relates to the issue of DA as a kind of political theory, a theory of power politics. But it also points out the implicit political theory that undergirds CA: liberalism. For CA to work, the actors must be assumed to be sovereign autonomous independent individuals choosing precisely what they want to say, as if what they want to say is not a social product itself recoverable in discourse. Perfect intentionality, if you will, assumed by CA, is theorized by DA.

Having enumerated these seven bedrock differences between DA and CA, my methodological conclusion might be unexpected. But I argue that there is no reason for discourse analysts not to adhere to some common forms of research design and social science methodology. Hans-Georg Gadamer has written that the natural and human sciences are separated *not* “by a difference in method, but a difference in the aims of knowledge.” (Gadamer 1987, 340) Look, for example, at Crawford’s list of seven ways to determine whether her own interpretations of the discourse on slavery over 150 years are superior to candidate alternative explanations. All discourse analysts should act with such methodological self-consciousness, with an eye to both the replicability and competitive validity of their findings. The predominance of a discourse may very well be related to the underlying configuration of political and social power that is discovered through the discursive analysis. At that point an account of the determinants of that particular configuration could permit a set of “predictions” about future sustainability and change both possible, and falsifiable, in principle. Possible determinants could be derived from available theories of institutionalism, coalitional and electoral politics, and social and economic structure. The point is that discourse analysis need not be merely the “raw material appendage” of putatively more scientific approaches to the study of politics. It can hold its own as a generator of theory and testable hypotheses derived therefrom.

I think the problem with HHP’s characterization of DA is that it is only concerned with change, rather than constancy. In fact, yes, in principle, meanings are always in flux, but this

does not mean that they are not fixed through power arrangements institutionalized in various ways at various historical junctures, such that it is possible to theorize about replicable patterns of social conduct over time. We need to weigh equally DA's political theory of language and subjectivity and its theory of discursive power embodied in normally constrained subjects.

I wonder as well about HHP's validity measures of DA and CA. I would have said that CA has behavioral implications: these beliefs imply these actions. And I would go on to try to get DA to make similar predictions based on its analysis of the politics and institutions of discursive reproduction in the domain of interest.

In sum, I think that DA can maintain its fundamental differences with CA while simultaneously being far more methodologically rigorous, and so participate far more vigorously and equally with putatively more scientific methods of analysis in fashioning accounts of the social world.

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Content Analysis: A Contrast and Complement to Discourse Analysis

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According to experts on discourse analysis, texts are not individually meaningful (Phillips & Hardy, 2002, p. 4). This notion strikes at the heart of a primary commonality between discourse analysis (DA) and content analysis (CA). Both are concerned with drawing conclusions about some aspect of human communication from a carefully selected set of messages. How they do so is rather different, but ultimately their findings can fit together quite nicely, providing a good example of triangulation of methods, a highly desirable situation.

Content Analysis

Content analysis is a summarizing, quantitative analysis of messages that relies on the scientific method, including an observance of the standards of objectivity/inter-subjectivity¹, a priori design, reliability, validity, generalizability (with probability sampling from a defined population of messages), replicability, and hypothesis testing (Neuendorf, 2002). It belongs to the "family" of quantitative methods to which survey research also belongs—indeed, some researchers hold that CA is in fact a subset of survey research (Neuendorf, 2002). Both surveys and CAs measure variables as they "naturally" occur (as opposed to the manipulation of independent variables in the true experiment); their sole difference is CA's focus on a message component as the unit of data collection or analysis. CA is not limited as to the type of message-centric variables that may be measured or the context in which the messages are created or presented. Moreover, contrary to some popular misconceptions, CA is not limited to a mechanical counting of manifest characteristics. Such counting is indeed often the "bread and butter" of CA, but there's much more to the meal.

CA is often quite rich, and offers the possibility of tapping complex, latent constructs. Rogan and Hammer's (1995) study of authentic crisis negotiations is a great example of how deep meaning may be extracted via CA. In their study, "message affect" was measured via five adjuncts of language intensity derived from qualitative and experimental work—obscure words, general metaphors, profanity and sex, death statements, and expanded qualifiers.

Their sample was too small and non-random to achieve generalizability, but their findings were striking—the profiles for negotiator/perpetrator message affect were different for

negotiation processes with successful vs. unsuccessful outcomes. The analysis technique and findings might prove useful not only in a practical, predictive sense for negotiation practitioners, but also in providing a baseline for further analysis of deep meanings of negotiation profiles. The “markers” of significant shifts in the relational roles of negotiators and perpetrators located via CA might be used in further study by DA analysts. Thus, CA may provide identification of the “pragmatic” contextual cues of crisis communication, while DA provides a more nuanced interpretation of their meaning.

CA is not limited to an analysis of words. As far back as the seminal Payne Fund Studies on the content and effects of movies on American youth (Charters, 1933; Dale, 1935), the stylistics, images, and behaviors of characters in moving image content have been systematically analyzed via CA. Concern over the effects of violent television in the 1960s and 1970s brought renewed commitment to such CA studies, spurring the development of CA schemes to measure such nonverbals as anti-social, pro-social, gender-typed, family role, and occupational role behaviors (e.g., Gerbner & Gross, 1976; Greenberg, 1980). The analysis of written or transcribed spoken words, a subset of content analysis, is called text analysis. Its computer-aided form (now supported by more than 20 soft-wares) is called CATA (computer-aided text analysis), a fast-growing segment of the CA literature.

CA is limited to a focus on messages. A simple inference from such messages to source motivations or receiver effects is, some have argued, not warranted. But to take full advantage of its findings, CA may be linked with source and/or receiver data, providing core evidence for a full model of the communication process (in what has been called the “integrative model of content analysis”; Neuendorf, 2002). For example, Naccarato and Neuendorf (1998) analyzed a wide variety of textual and graphical characteristics of business-to-business print ads, and then statistically linked those characteristics with four measures of readership and recall for the ads, derived from a commercial magazine readership survey. And Hertog and Fan (1995) paired their CA of news stories about AIDS transmission in major newspapers and news magazines with findings from independent public opinion polls. They found news coverage to predict public opinion at a later point in time; public opinion did not predict subsequent news content. In both cases, the bigger picture of message effects was generated by a combining of systematic CAs and quantitative data from “external” studies. And, both examples show the potential of CA procedures—particularly when linked with other data sources—to discover particular patterns of control, both in terms of information flow and message impact on receivers. Such dynamics seem central to the fundamental concern of DA with power and hierarchy.

DA and CA Compared

The range of substantive topics appropriate to DA is also generally appropriate to CA. Moreover, the various “sources of data” (types of messages/texts) can also serve as the foci of CA analyses. Both methodologies have embraced the use of computers for particular tasks, although in both

cases, their application generally seems to be a case of old wine in newer, faster bottles. In considering both DA and CA, there is a common bottom line—“There are no unmediated data” (Phillips & Hardy, p. 83). Those using DA attempt to fully disclose their mediation (through rich discussion of all “backgroundings”—assumptions, epistemologies, etc.), while those using CA attempt to minimize their mediation (through adherence to the scientific method, including an aim toward intersubjectivity, if not objectivity).

The overriding importance for DA of validity, and the relative lack of concern with reliability (Phillips & Hardy, pp. 79-80), is a core dissimilarity between the two methodologies. For CA, reliability is paramount—in fact, measures that do not achieve an acceptable level of reliability ought to be dropped from further analysis. Further, replicability is clearly not a focus of DA, while it remains an additional important standard for CA.

Thus, in CA, measurement is via a coding scheme that is written out in great detail, with an accompanying coding form (or a set of dictionaries (word/concept lists) if the analysis is strictly of written text). In all cases the coding instrumentation is established *a priori*, and the goal is to create a coding plan that is so carefully defined that virtually anyone, with sufficient training, can serve as a reliable coder. This contrasts sharply with DA, for which the researcher serves as the measurement instrument. Hence, the measures and analysis are highly dependent on the expertise and orientations of the researcher(s). Fittingly, DA has been characterized as “techniques *plus* perspective/assumptions” (i.e., method plus epistemology). With CA, the epistemology is a given—an endorsement of the scientific method.

DA and CA Used Together

As noted above, triangulation of methods—i.e., approaching a research question from multiple methodological stances—is the ideal. When the findings agree, the conclusions of the researchers are strengthened multi-fold (Gray & Densten, 1998). Unfortunately, few studies have combined CA and qualitative message analysis. One example is Miller, Wiley, Fung, and Liang’s study (1997) of storytelling in Taiwanese and European American families, which combined in-home ethnographic fieldwork with content-analytic coding of audio and video recordings of naturally occurring talk in the home. They concluded (1) that personal storytelling operates as an important socializing practice for children ages 2-6 in widely different cultures, (2) with Chinese families more likely to use storytelling to convey moral and social standards, and European American families more likely to employ stories for entertainment and affirmation. The former (1) was a conclusion that could be drawn from the in-home observation, while the latter (2) was a finding derived from the systematic CA coding of the recordings.

DA and CA seem a good fit for such triangulation, although not for precise replication. DA provides a rich source of contextual data, and provides a “big picture” of a realm of communication activity, ostensibly leaving no stone unturned in a consideration of all critical messages. Such a diverse

collection is not generally conducted in CA. DA is therefore more likely to allow the discovery of the variety and texture of communication. From the standpoint of a content analyst, DA gives a multitude of “clues” that go beyond the typical preparation (via literature review and “immersion” in a message pool) for a CA.

For example, Hardy and Phillips’ (1999) study of the discourse concerning the Canadian refugee system uses the full power of DA to establish a network of meaning. They examined legislation and the statements of government officials and NGO leaders and others to analyze the institutional field of the refugee. Plus, they used editorial cartoons to explore the societal-level discourse. Their analysis of cartoons uncovered prototypical portrayals of refugees and of the refugee system itself that would most likely go undiscovered via CA. But a future CA could make use of the findings to effect a more broad-based study of cartoons, creating a realistic, summary picture of the “message pool” available to various publics at various periods in time. Additionally, a CA adds the pedigree of reliability—an assurance that the findings are not entirely the product of one analyst’s opinion.

Conversely, a CA may serve as a stimulant to the conduct of a DA. For example, one unexpected finding of Smith’s (1999) study of women’s portrayals in U.S. commercial film was that films with more females in creative control (i.e., writing, directing, producing) presented more gender-typed portrayals of women.² This cries for a follow-up, and DA seems uniquely suited. A DA could assemble a fuller investigation of the network of discourse surrounding this trend, in order to begin to answer questions of how and why this phenomenon occurs. A variety of framings are probable within the institutional curve—e.g., it’s possible that the female film executives do not even perceive the portrayals as gender-typed. Here, CA provides the “clue” as to a critical pattern in message content that deserves a more in-depth look.

More generally, qualitative and quantitative investigations should routinely be used together. It is wrongheaded to proceed on any quantitative study without considering various conceptual definitions derived from the reflexive processes of qualitative research; it is equally wrongheaded to draw generalized conclusions about one’s qualitative findings without adding quantitative evidence on the prevalence and patterns of message occurrence.

A Final Observation

Perhaps the most compelling—and startling—macro-level observation one can make from an examination and comparison of the two methods is in fact a social constructivist one. The *discourse* concerning DA reveals the approach to be one of inspection, introspection, and primacy of cognitive activity, with emphasis on reflection, discussion, and debate, while the discourse of CA is one of a more “industrial” milieu, with emphasis on production, output, and broad-based generalization. These framings correspond to views of DA as constructivist (with evident concern for the precision and validity of description and identification by the observer) and of CA as intersubjective (with evident concern for the shared

understanding of the research assumptions, process, and findings). It would be interesting to trace the roots of their creating such discourses; to situate the contexts of their training and identify the assumptions of their pedagogical origins. More importantly, it seems that a dialogue among DA and CA researchers might be well served to consider such discursive contrasts, and to consider how the approaches and advantages of the two techniques are complementary.

Notes

¹ This distinction between objectivity and intersubjectivity is an intriguing and important one. Some researchers (e.g., Babbie, 1986) have acknowledged the unattainable nature of true objectivity in measurement, and have opted instead for a *goal of intersubjectivity*—i.e., such clear and publicly proclaimed assumptions and methods as to assure fully shared meaning among researchers.

² The operationalizations of gender-typing were derived primarily from a host of qualitative investigations, most of them critical-cultural.

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World or Worlds? The Analysis of Content and Discourse

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There are many approaches to discourse analysis and I would be reluctant to try to capture its essence. Perhaps the most familiar use in International Relations has emphasised the construction of identity and difference.¹ However, scholars such as Crawford have focused on argument analysis,² Duffy, Frederking and Tucker on language games,³ as have I in a somewhat different way⁴; Milliken on the sequencing of moves in foreign policy interactions,⁵ Sylvan and Majeski on the construction of foreign policy choices,⁶ Alker on an ethnomethodological/dramaturgical approach to studying how people play iterated Prisoner's Dilemma games⁷ and Weldes on the discursive construction of the Cuban Missile Crisis, problematizing its status as a 'crisis.'⁸ Not all of these scholars would refer to themselves as discourse analysts, but they are all in different ways dealing with the analysis of texts. As Milliken rightly points out, there is a stream of thinking among discourse analysts in international relations that rigor and systematic method should be avoided given their association with positivist method.⁹ That discourse analysis, in contrast to the rigor and formal methods of content analysis, is relativistic, interpretive and a bit woolly minded is an assumption that is often shared by advocates and critics alike. This difference is often implied in the distinction between formal and informal.

I would like to argue that this dichotomy should be drawn into question. The difference between discourse analysis and content analysis has less to do with the degree of formality in the method per se than the methodological foundations on which the methods rest. I thus begin with a distinction between methodology and method. Methodology refers to those basic assumptions about the world we study, which are prior to the specific techniques adopted by the scholar undertaking research.¹⁰ Methodology includes both ontology and epistemology; the question of whether they can be separated is at the heart of the methodological difference. My analysis revolves around a contrast of two ideal types, which are oversimplified to amplify the difference, recognizing that both traditions involve a diversity of approaches and assumptions. I will focus on four issues: the relationship between language and world; the question of stability vs. change: coding vs. interpretation; and the meaning of formality.

World or Worlds

Both content analysis and discourse analysis revolve around the analysis of texts. However, they each assume a different understanding of the relationship between word and world. Do we assume that language functions as a label for discrete objects or subjects in the world or that it is constitutive of the world?¹¹ In the former case, the nature of being (ontology) is separate from the way of knowing (epistemology). The existence of objects or subjects is distinct from the labels we attach to them. On a very basic level, the ability to treat words as discrete categories is a necessary point of departure for their quantification. Arguably counting individual words requires their isolation from a context. More sophisticated forms of content analysis do attempt to cluster categories, to explore further entailments of a word, and overcome context-related deficiencies.¹² However, for the sake of contrast, we can say that the emphasis on quantification goes hand in hand with an assumption, building on the tradition of logical positivism, that language mirrors objects in the world.

By contrast, discourse analysis is more geared to an examination of the embeddedness of words in patterns of relationship. From this angle, the nature of being (ontology) cannot be separated from ways of knowing (epistemology). This is not to deny the obvious point that the material world exists independent of or prior to human society. It does, however presuppose that this material world has been dramatically altered by human interaction with it.¹³ A tree branch, aside from being part of a tree, may be used as a weapon or formed into any number of human artifacts from chairs, to baseball bats, to totem poles to a beam in the structure of a house. Once the material object, that is, the tree branch, is shaped into a specific form, it has a place within a particular type of social context, where it has meaning in relation to other objects (e.g. chairs and types of rooms or totem poles and religious rites), particular uses (e.g. to sit on or dance around, respectively), and is part of a language or grammar (e.g. of homes or religion). Humans not only interact with nature, thereby transforming it, but with each other, forming, in the process, different types of culturally and historically specific practices and institutions that are also rule governed. The point is that if, in the first case, treating words as labels serves their quantification, in the second, treating them as systems of relationships requires an analysis of the grammars which constitute particular worlds. While in the first case, the world is assumed to exist as an objective place, populated by discrete objects, in the second, we are dealing with worlds plural. The subjects, objects and practices constituting the world of 16th century witchhunts, 18th century slave trading or 21st century terrorism are historically and culturally specific. The assumption of multiple possible worlds, rather than a single world to be discovered, is linked to other assumptions.

Discovering Content vs. Mapping Change

Other contributions to this issue have made a distinction between the assumption of a stable world and meaning on the

part of content analysts, and a world of meaning in flux on the part of discourse analysts. Some discourse analysts would agree with this contrast. Based on my work, I would argue that it is too stark. There are periods when meanings become relatively fixed, and not least the Cold War. However, there are also periods of change, when relationships and practices are transformed. Content analysis, given its assumption of a fixed world, is more conducive to the analysis of continuity. Discourse analysis can deal with either periods of continuity or change, although the underlying assumption of multiple worlds is most evident when mapping the process by which worlds change. Most of the examples of argument analysis, cited by Crawford in this issue, are examples of attempts to map the transition between worlds. In her own work, she looks at the transformation of a world in which slavery was legitimate to one in which it became an illegitimate practice. My own work on the end of the Cold War mapped the transition from a world in which any kind of disarmament was considered irrational, and the Cold War was assumed to be a more or less permanent feature of international relations, to one in which disarmament, and a whole range of other previously unthinkable practices, became possible.¹⁴ If the end is to map a process of world changing, then it stands to reason that the method cannot begin with the coding of categories chosen by the analyst, which then provide a template for searching texts. It is through the searching of texts plural that one identifies the process by which the central categories and entailments of one world relate to the transformation of that world as a whole. This happens on several levels. Arguments, as arguments, are part of the process of contestation. But this does not mean language is completely in flux. In fact, a process of contestation is often constituted in a shared language, which is a reflection of belonging to a shared world, albeit one undergoing change.

The following example of the politicization of nuclear deterrence in the 1980s makes this point.¹⁵ Actors occupying very different political positions drew on a shared grammar of prisons in the process of contestation. Each of the parties articulated a distinct moral argument about nuclear deterrence, but they relied on a shared language in doing so. The key object was deterrence as a prison. The question was the form of action to be taken in relation to that prison: staying inside, escaping, being liberated or destroying the structure.

NATO: The prison cannot be escaped: we should maintain deterrence because it is a blessing that has prevented war in Europe for the last forty years.¹⁶

REAGAN: The prison can be escaped: we should develop technologies that will allow us to find security in ways other than threatening mutual destruction.¹⁷

PEACE MOVEMENTS: We need to be liberated from the prison: we are trapped in a way of thinking that is imprisoning us and keeping us from thinking about alternative solutions.¹⁸

GORBACHEV: The guillotine should be destroyed: we can develop a form of international politics based on civilized norms, but need to destroy our reliance on

deterrence and balance of power thinking.¹⁹

Once the shared components of a political language are identified it becomes possible to trace a transition, from one language game to another. I use the word language game rather than discourse to emphasize that language and practice - not incidentally relating to material objects such as nuclear weapons - are interwoven²⁰. The structure of the change and the patterns are to be found in the context itself, in the meanings given by the actors to their world. If the analyst begins by pre-selecting a range of codes the richness of the context and change within it are likely to be lost. In this case, the change was also evident in a shared grammar of foundations (which were being maintained, repaired, dismantled or undermined by different actors) and intimate relations (involving marriage, divorce, emancipation, etc). What was accepted at an earlier point in time (that deterrence was a prison from which we could not escape, and a foundation of stability separating two alliance families) became the object of contestation. This provided the basis for mapping the transition from the relatively stable world of the Cold War, and the division of Europe, to a new world of a Europe transcending East and West.

Objective Quantification vs. Subjective Interpretation

That the scholar who codes categories is engaged in objective quantification is often contrasted with claims that discourse analysts bring subjective interpretation or their own perceptions to the reading of texts. However, building on the example above, I would argue that the centrality of a shared language at a site of contestation draws the latter into question. It is no more subjective to identify a grammar based, for instance, on a system of relationships between prisons, liberation, escape or destruction, than to quantify the number of times a word or cluster of words occurs in texts relating to deterrence. Of course, individuals do interpret single texts in a variety of ways, and debates over the significance of this have a long history in political theory, focusing not least on the relationship between context, text and individual. My point is that the distinction between objective quantification and subjective interpretation begins to blur when the analysis covers a large number of texts, which would seem to be necessary in making any claims about an area such as international relations. The above study involved some six hundred texts. The patterns by which subjects, objects and practices were configured, and altered had very little to do with subjective interpretation on the part of the analyst or the subjects of analysis. The analysis focused on a shared language and grammars that frequently recurred across texts from different types of actors.

A single word can have any range of meanings when placed in a different context. A journal referee once pointed out that the word 'romance' in Russian had a whole range of different meanings than in English. However, when the word 'romance' is embedded in a cluster of other categories, such as 'unrequited love', a relationship between a strong masculine actor and a weak feminine one (a frequent correspondence in Russian public texts regarding the relationship be-

tween Russia and NATO in the immediate aftermath of the Cold War), the cluster of words in and of itself establishes the context.²¹ The analyst may exercise some interpretation in focusing in on this particular cluster, as opposed to another, but the words themselves belong to a grammar of categories, which we would all recognize as belonging to intimate relationships, drawn on in this case to give meaning to the hierarchical relationship between post-Cold War NATO and Russia. The appearance and frequency of a particular grammar is not down to the interpretation of the analyst. These patterns emerge across texts and are discovered by the analyst. The danger of going to any particular set of texts with a range of predetermined categories, for the purposes of coding, is that the world of analysis is limited from the start by the choices, and thus arguably the interpretation, of the analyst.

The Meaning of Formal Analysis

Quantification is *easier* if single words are quantified rather than patterns of relationships between subjects, objects and practices within a world. Many content analysts have embraced the greater complexity of analyzing clusters of relationships and are therefore engaging in practices that are not dissimilar to the one described above. The question is what is gained or lost in the translation into numbers. What is gained is credibility within a world of science that values quantification over other forms of analysis and one in which the word formal has acquired the meaning of using quantification. What is potentially lost is the very human, social and political processes by which actors call the world around them into question. If formal is taken to mean systematic and rigorous analysis, then there are numerous examples of scholars engaged in discourse analysis who, with this definition, can be considered to be formal analysts. This redefinition may seem problematic in so far as the term formal analysis is derived from a scientific grammar, which includes categories of measurement, replication and quantification, which have been acquired from the natural sciences. However, the obvious difference between the work of the natural scientist and the social scientist is that content analysis *cannot* be a method of the former. The natural scientist has a freedom to impose meaning on the natural world. The social scientist does not because the subjects of analysis are meaning creating creatures. We thus inevitably are involved in negotiation with the subjects of study and cannot hope to provide an accurate representation of their world if we simply impose our own categories. We also have a greater responsibility to recognize that we too are situated in a social world, constrained by relations of power and meaning, which are defined within historical and cultural contexts. Critical theorists have long expressed a concern that the assumption of a stable and universal world serves to legitimize existing structures of power and meaning. It is this difference between the desire to know and understand a stable world vs. a changing world that most distinguishes the two approaches. Quantification is more complicated in the latter case, but not impossible. However, in either case there remains a question of the threshold at which a pattern can be claimed as a pattern.

Notes

¹ Roxanne Lynne Doty, 'Foreign Policy as Social Construction: A Post-Positivist Analysis of U.S. Counterinsurgency Policy in the Philippines,' *International Studies Quarterly* 37(30), pp. 297-320; and *Imperial Encounters: The Politics of Representation in North-South Relations* (Minneapolis: University of Minnesota Press, 1996); Iver Neumann, *Uses of the Other: 'The East' in European Identity Formation* (Minneapolis: University of Minnesota Press, 1999).

² Neta C. Crawford, *Argument and Change in World Politics: Ethics, Decolonization, and Humanitarian Intervention* (Cambridge: Cambridge University Press, 2002).

³ G. Duffy, B. Frederking and Seth A. Tucker, 'Language Games: Dialogical Analyses of INF Negotiations,' *International Studies Quarterly* 42(2) 1998, pp. 271-294.

⁴ K.M. Fierke, *Changing Games, Changing Strategies: Critical Investigations in Security* (Manchester: Manchester University Press, 1998).

⁵ Jennifer Milliken, *The Social Construction of the Korean War: Conflict and its Possibilities* (Manchester: Manchester University Press, 2001).

⁶ Stephen Majeski and David Sylvan, 'How Foreign Policy Recommendations are Put Together,' *International Interactions*, 25 (4) pp. 301-332.

⁷ Hayward Alker, *Rediscoveries and Reformulations: Humanistic Methodologies for International Studies* (Cambridge: Cambridge University Press, 1996). See also: Hayward Alker and David Sylvan, 'Some Contributions of Discourse Analysis in Political Science,' *Kosmopolis* 24 (3), pp. 5-25.

⁸ Jutta Weldes, *Constructing National Interests: The US and the Cuban Missile Crisis* (Minneapolis: University of Minnesota Press, 1999).

⁹ Jennifer Milliken, 'Discourse Study: Bringing Rigor to Critical Theory,' pp.136-159, in: K.M. Fierke and Knud Erik Jorgensen, *Constructing International Relations: The Next Generation* (Armonk, NY: M.E. Sharpe, 2001), p. 136. She points in particular to the arguments of Ashley, Walker and Campbell.

¹⁰ This distinction is also made by Cynthia Hardy, Bill Harley and Nelson Phillips, 'Discourse Analysis and Content Analysis: Two Solitudes?' (This issue)

¹¹ This issue is examined in greater depth in: K. Fierke, 'Links Across the Abyss: Language and Logic in International Relations,' *International Studies Quarterly*(2002) 46, 331-354.

¹² See the section on Quantitative Content Analysis, in Rawi Abdelal, Yoshiko M. Herrera, Alastair Iain Johnston, and Rose McDermott, 'Identity as a Variable, Unpublished paper, May 10, 2003, p. 17

¹³ This discussion is adapted from: K.Fierke, 'Cause or Constitution,' Chapter One, *Diplomatic Interventions: Conflict and Change in a Globalising World* (forthcoming, Palgrave Press)

¹⁴ K.M. Fierke, *Changing Games, Changing Strategies: Critical Investigations in Security* (Manchester: Manchester University Press, 1998).

¹⁵ This is taken from: K.M. Fierke, 'Constructing an Ethical Foreign Policy: Analysis and Practice from Below,' in: K.Smith and M.Light, *Ethics and Foreign Policy* (Cambridge: Cambridge University Press, 2001)

¹⁶ W. Hofmann, 'Is NATO's Defence Policy Facing a Crisis?' *NATO Review* 32 : 2 (1984).

¹⁷ Ronald Reagan, 'The Geneva Summit,' *Current Policy* 766 (1985).

¹⁸ Mary Kaldor, 'Liberating Ourselves from Cold War Ideologies,' *END Journal* 10 (1984).

¹⁹ Mikhail Gorbachev, 'Gorbachev Talks to Moscow Peace Forum,' *Current Digest of the Soviet Press* 39: 7 (1987).

²⁰ Ludwig Wittgenstein, *Philosophical Investigations* (Oxford: Basil Blackwell, 1958), par. 7.

²¹ See: K.M. Fierke, Besting the West: Russia's Machiavellian Strategy, *International Feminist Journal of Politics*, 1: 3 (1999), pp. 403-434.

Announcement: APSA 2004 Panels

1. Roundtable on American Politics: Views from American Political Development

Chair: Howard L. Reiter

Participants: Daniel P. Carpenter, Ira Katznelson, Karen Orren, and Paul Pierson

2. Causal Explanations and Case Studies: World War I and the End of the Cold War

Chair: Gary Goertz

Richard Ned Lebow, "Contingency, Catalysts and International System Change."

Stephen G. Brooks and William C. Wohlforth, "Power, Globalization, and the End of the Cold War: Reevaluating a Landmark Case for Ideas."

Paul W. Schroeder, "Embedded Counterfactuals and World War I as an Unavoidable War."

Robert D. English, "Power, Ideas, and New evidence on the Cold War's End."

Jack S. Levy, "Necessary Conditions in Case Studies: Preferences, Constraints, and Choices in July 1914."

Discussant: James Mahoney

3. The Analysis of Political Regimes

Chair: Scott Mainwaring

Gerardo L. Munck, "Political Regimes in the Post-Cold War Era."

Steven Levitsky and Lucan A. Way, "The Dynamics of Competitive Authoritarianism: Explaining Hybrid Change in the Post-Cold War Era."

Jose Antonio Cheibub and Jennifer Gandhi, "Classifying Political Regimes: An Extension and an Update."

Scott Mainwaring and Anibal Perez-Linan, "Studying Latin American Regimes, 1945-99."

Discussant: Susan C. Stokes

4. Alternative Pedagogies of Qualitative Research

Chair: Cecelia Lynch

Hayward R. Alker, "Pedagogical Principles of Methodological Pluralism."

Raymond D. Duvall, "Pedagogies of Critical Social Theory."
David Sylvan, "In the Field: Qualitative Methods, Discipline and Practice."

Discussant: Alexander Wendt

5. Roundtable on Mixed Methods in Post-Communist Russia

Chair: Mitchell A. Orenstein

Participants: Anna M. Grzymala-Busse, Keith A. Darden, Jason Wittenberg, Yoshiko M. Herrera, and David D. Laitin

6. Analyzing Necessary and Sufficient Causes: Applications and Issues

Chair: David Collier

Gary Goertz, "Assessing the Importance of Necessary or Sufficient Conditions in Fuzzy-set Social Science."

James Mahoney, "Explaining the Great Reversal in Spanish America: Statistical Analysis vs. Fuzzy-Set Analysis."

Jason Seawright, "Qualitative Comparative Analysis vis-a-vis Regression: Comparing the Underlying Assumptions."

Gunnar Grendstad, "A Boolean Approach to Party Preference: A Five-Country Study."

Discussants: Jonathan N. Katz and David Collier

7. Roundtable on Uses of Language in Constructivist International Relations

Chair: Audie Klotz

Participants: Jeffrey T. Checkel, Roxanne Doty, Matthew J. Hoffmann, Cecelia Lynch and Iver B. Neumann

8. QCA/FS: The State of the Art and Future Prospects

Chair: Benoit Rihoux

Badredine Arfi, "Aggregating Causality in Multiple-Level Theories: A Linguistic Fuzzy Logic Approach."

Sakura Yamasaki, "Social Network Analysis and Qualitative Comparative Analysis: An Exploratory Study."

Carsten Schneider and Claudius Wagemann, "The Fuzzy-Set/QCA Two-Step Approach to Middle-Range Theories."

Benoit Rihoux, "On the Use, Misuse, and Good Use of QCA: State of the Art and Prospects."

Discussant: Andrew Bennett

9. Philosophies of Qualitative Sociopolitical Inquiry

Chair: Janice Bially Mattern

Gavan Duffy and Evelyn Goh, "Testing Sincerity: Henry Kissinger's Opening Encounter With the Chinese Leadership."

Michael J. Shapiro, "Political Theory versus The Fetishism of Method."

Patrick Thaddeus Jackson, "Language, Legitimation, and the American Empire."

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