The Rise and Fall of a Bad Idea: A Research Narrative

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This narrative details a research process from the development of hypotheses from qualitative data through the testing of those hypotheses in large scale quantitative survey research. Discussed are the value issues involved with the conceptual framing of research questions to be congruent with the social work paradigm.

Consumers of social work research – readers of reports, journal articles, books, and the like –get a sanitized product that seems to be the result of a linear scientific process. It appears that the authors established a research question based on gaps in previous research, converted the question into measurable hypotheses and variables, then tested the hypothesis through the application of specific universal rules of research. In fact, research results emerge out of a messy, iterative, sometimes irrational process. The cliché is that no one should be exposed to what goes into the production of sausages or legislation; the same can be said for the production of research articles.

I still remember my shock when, as a beginning doctoral student, I was first exposed to the reality of the development and publication of social work knowledge. In our first-year socializing seminar, in which prominent social work research faculty were invited to informally discuss their work, we were informed that a successful researcher begins with a table of whatever statistically significant results were derived from the empirical process, then works forward and backwards from that. In other words – in a startling departure from what we were being taught at the same time in our research classes – the literature review, the research question, the underlying theory, and certainly the conclusions and discussion were developed only after the statistically significant results were clear. In another course, an eminent sociology professor referred to the text surrounding the Findings section as "dither" – a packaging task designed to make the article ready for consumption.

The above is an extreme view. In reality, of course, researchers bring to their work a knowledge of the issues in the literature of the field; the research questions and instruments are constructed based on that knowledge, and variables are chosen and measured based on prior judgments as to what is important and constructive to the field. Nonetheless, the finished product of the research process does obscure the messy reality of the process.

At the least, there are numerous unanticipated instances where researchers need to guess at what should be clear choices. What about the survey respondents who insist on placing their marks between two of the five offered responses? And skip pattern responses that make no sense - the respondents who indicate that they are not a member of a union, then go on to report a high level of union activity? The meaning of barely audible audio tapes? Did he say he "did" or "didn't" mean to hit that child? Fuzziness in the sampling frame - the new hire who began the job as an intern, but was not officially hired until later?

A larger issue is the representativeness of samples. Here we hope that readers will not dwell on our brief acknowledgments that our respondents (whatever the response rate) surely differ from our non-respondents in important or unknown ways, or, in qualitative studies, that our small sample of convenience may have little in common with larger populations of interest.

Another major area of fuzziness in social work research is the process of model building. Questionnaires are packed to the maximum possible length with measures of any variable that previous research, theory, or simple hunch indicate might conceivably have a statistical relationship with the outcome. The variables are then analytically reduced to those that, in fact, have such a relationship and packaged as a predictive model of the outcome. Even the most by-the-book researcher will not hesitate to abandon prior hypotheses to adopt post hoc theories that provide a context for the statistically significant results actually obtained. Failures to disprove the null hypothesis do not get published. Nor would any successful researcher hesitate to re-frame a set of conclusions to make them more appropriate for a given journal.

This narrative seeks to elucidate some of the messy research processes that lie behind all of the crisp and clear research articles that appear in our journals. In examining a specific empirical sequence from my own career that is more typical than unique, I talk about some of the decisions faced in the real research process, and some of the ethical dilemmas that inform these decisions. In addition, this is a story of a dead end in the research process, a frequent outcome of our endeavors that never see the light of publication.

The Inductive/Deductive Cycle

This narrative focuses on an instance of qualitative data collection, theory generation, and quantitative theory testing. One of the principles taught in our social work research classes is the inductive/deductive cycle of scientific reasoning. The presentation of this process, which is fortunately quite amenable to clear and instructive diagrams, is a staple of our research texts (e.g., Monette, Sullivan, & DeJong, 2005, p. 29; Rubin & Babbie, 2005, p. 52). Theories, or hypotheses, about the relationships in the world among some set of related concepts are developed through observation of the world, exploratory qualitative research, unanticipated conclusions from quantitative research, or pure thought. These hypotheses are then tested through more explanatory quantitative research. The results of these tests, in turn, become material for the development of further testable hypotheses. One of the assets of this conceptual presentation of the development of social knowledge is its integration of qualitative (exploratory, theory-generating) methods with quantitative (explanatory, theory-testing) methods, appropriately emphasizing the roles and strengths of each approach. Tashakkori and Teddlie (1998) categorize this combination of qualitative and quantitative methods as a "sequential mixed method design" (p. 46).

The inductive/deductive research cycle has some validity as a macro picture of the scientific research process of an entire field of endeavor; as ideas are tried, bad ones are discarded and good ones are kept and built upon, even though the disappearance of some ideas and the persistence of others may owe at least as much to political and cultural factors as to purely scientific ones. The trial-and-error approach is less accurate as a description of the work of individual researchers, mostly due to the specializations chosen by researchers. Most social-science researchers are specialized by method, which limits their participation to only one aspect of the entire inductive/deductive process. In addition, we become committed to a specific theory, measurement issue, or narrow field of inquiry. As a result of these specializations, most

researchers do not engage in full cycles of the inductive/deductive process. This brings a looseness to the macro research system as some researchers generate theories, which may or may not be chosen for testing by other researchers, while some researchers report on (successful only) tests of specific theories with little thought given to generating further hypotheses for exploration.

The Retention of Public Child Welfare Workers

The research topic underlying this narrative is the retention on the job of public child welfare workers, especially Title IV-E stipended workers. A statewide shortage of social workers is being experienced and is expected to get worse (O'Neill, 2000). Public child welfare is facing its own acute shortage of social work personnel. Statewide there were 6500 public child welfare positions funded for FY 2000/01, yet there was a need for twice that many to meet minimum standards and three times that many to meet ideal standards (American Humane Association, 2000). Turnover has chronically been a problem in public child welfare (Council of Family and Child Caring Agencies, 1989; Helfgott, 1991) and a number of studies have explored retention in child welfare (e.g., Fryer, Miyoshi & Thomas, 1989; Rycraft, 1994; Vinokur-Kaplan, Jayaratne, & Chess, 1994). Title IV-E, with the dual goals of increasing the number of workers entering public child welfare while increasing the level of education of these workers, represents a significant investment in the education and training of professional child welfare workers. In California, 200 M.S.W. graduates per year are added to the pool of child welfare workers (California Social Work Education Center, 1999). However, the retention of workers who received stipends through Title IV-E has itself become an issue (Dickinson & Perry, 2002; Jones & Okamura, 2000). In Los Angeles County, about half of stipended workers leave DCFS within two years of beginning work (Furman, 1998), a rate similar to that of non-stipended new hires. In San Diego, though, IV-E workers were more likely (89% retention) than other workers to remain on the job (Jones & Okamura, 2000), and the one statewide study to date found a 76% retention rate (Dickinson & Perry, 2002).

Generating Hypotheses

Leaving my doctoral program as an "expert" on welfare reform, I quickly re-cast myself as a researcher in the field of child welfare because at that time money and jobs were available through Title-IV funds. I was hired by a local school of social work to examine the effectiveness of their Title IV-E stipend program. As there was no specific mandate to the research, we began with a general discussion among program staff about the direction of the research and quickly chose to focus on an examination of the factors that impede or contribute to the retention of these workers. We established a longitudinal approach, annually interviewing in depth cohorts of IV-E students, initially as they entered the required internship program (could be first or second year of the M.S.W. program), and subsequently as they graduated, on their first-year work anniversary as they confronted the choice of continuing employment past the mandated one-year commitment, and annually thereafter whether or not they remained with the local DCS. Apart from our guiding research question of identifying the factors that influence remaining on the job, our approach was entirely inductive (Glaser & Strauss, 1967; Strauss, 1987). Though we structured the questionnaires around obvious general areas – family, work experience, M.S.W. education, etc. – we anticipated that the relevant factors would emerge from our interviews. Since our first round of interviews were with students who had not yet begun their employment, our

questions addressed issues of preparation for a career in child welfare. We were then able to trace these themes through the early stages of their child welfare careers.

Interviews were conducted face to face at the internship or at the office, written down on the spot, then immediately typed by the interviewer. Interviews were conducted by me and by a number of doctoral students. We paid careful attention to possible interviewer differences. As a group, we analyzed sets of interviews among a cohort at a point in time. Interviews were coded based on the interview guide, while researchers remained open to emerging topics. The list of codes developed over the time of the project. All coded text was moved to separate files per code to form a data matrix by topic. The text within each file was reduced to derive a summary of each substantive category. Categories among each category were derived from the data. Categories across respondents were examined to identify patterns of responses for subjects (Miles & Huberman, 1994).

Over time our pool of subjects became large enough (68 beginning work with DCS) that we were able to do some simple statistical comparisons. To accomplish this, nominal variables were developed from the qualitative categories; interviews were re-visited and coded quantitatively (Boyatzis, 1998).

The data on the lives of students prior to beginning social work school revealed the presence of a certain path to social work as a profession. The interns began their paths toward social work by perceiving a social need in their immediate environment that had a direct impact on them. They then became directly involved with the relief of these needs through personal responses, volunteer work, or paid paraprofessional human service work. These positive experiences led to the search for further education to increase their capacity to address these needs. In this search they were led to social work either by supportive families or through contact with professional social workers.

The initial exposure to social needs was categorized into either direct or indirect exposure. Direct exposure was further categorized into personal problems (those that occurred directly to the subjects) family problems (those that occurred within the families of the subjects) and community problems (those that occurred in the communities in which the subjects were raised.) Indirect exposure was through a volunteer or work setting; these were older students, approaching social work as a second career.

We saw that these categories related with views among students of their upcoming child welfare professions. Students with personal or family problems took a wait-and-see attitude toward the job. Those students with a background of exposure to community problems reported an intention to stay on the job and to pursue administrative and policy positions. Students with an indirect exposure to social problems expressed the strongest determination to remain on the job.

As it turned out, these categories of early exposure to social problems were related to remaining on the job but not necessarily as predicted by the students. Those students who reported direct early exposure to social problems, especially personal problems, were more likely to remain on the job. The greater the exposure, the greater the likelihood of remaining on the job. Of those who reported a history of personal problems, 68% remained on the job after one year compared with 46% of those who did not report such a history. After totaling the number of reported problems per subject across categories, 39% of those with no problems remained on the job after one year, 52% of those with one or two problems, and 70% of those with three or more problems.

These conclusions, of course, remained suggestive. Statistically, given the small sample

size, the validity problems of variable development and the lack of any multi-variate analysis (work experience, age, and satisfaction with pay and benefits were also strong predictors of retention), there was not a strong foundation here. Nonetheless, given our lengthy and in-depth involvement with these data, we were certain that something was going on here.

Needless to say, this conclusion – that those social workers with troubled backgrounds were more likely to remain on the job than those without – was completely unexpected and quite troubling. In and of itself, this was not an acceptable conclusion. The immediate association was between troubled workers and troubled organizations, an association which came quickly to mind among those many educators and workers who had become cynical about the possibility of a satisfying career in public child welfare. As one colleague said, "Of course, these people stay; they're sick and DCS is a sick organization."

For this reason, we were not willing to report this or even talk much about it until we could frame it in an acceptable manner. It is interesting that the need to re-frame this conclusion in a more acceptable manner was immediately apparent to us; there was no question but that this was our next task. We could not proceed with the proximate understanding of this perceived relationship.

We went back to the data to look more carefully at the sequence from exposure to childhood problems to coping on the job. We saw that these students had experienced certain types of responses to these problem experiences. They reported overcoming and transcending these problems with the help of community networks, supportive families, strong adult role models (usually mothers), personal therapy, and church attendance. We came to understand the concept from a strengths-based perspective. These people, through their childhood trials, developed strengths that could be converted into tactics of survival in a difficult bureaucracy. I hypothesized that while many people suffer in childhood, those that go on to successfully cope with these problems and overcome them are the ones who go into the field of social work.

We went on in subsequent interviews to examine the behaviors of graduates who remained on the job and were professionally satisfied in public child welfare. Among these behaviors were an acceptance of personal responsibility, flexibility in ways of understanding themselves in relation to their jobs, assertiveness in changing their work conditions and the agency, and an ability to establish large networks of support and communication among peers and other supervisors.

We had the beginnings of a grounded theory. Exposure to personal family problems leads to actively addressing those problems, which leads to involvement in social work, as well as to increased ability to deal with difficult, ambiguous, and contradictory settings such as large bureaucracies. In this way we can have a positive understanding of how and why social workers with traumatic backgrounds may be more likely to remain on their child-welfare job. The example used was the member of a family with an alcoholic father (a situation of many of our respondents) who was compelled to develop in childhood alternative methods of communicating and developing sources of information and support. This translates into the ability in a large bureaucracy to bypass formal communication channels.

Developing Quantitative Measures

The next step in my academic career was as a full-time grant-funded lecturer at another social work school. Though my primary research had gone in a different direction, working with M.S.W. students on their theses afforded me the opportunity to pursue some

of the concepts pertaining to the paths that social workers take to their careers. It is a truism that social workers are likely to have come from problematic childhoods, and there is a body of literature that supports this. It has been found that the families of social workers are more likely to include unhappily married parents, members with emotional problems (Rompf & Royse, 1994), victims of violence (Black, Jeffreys, & Hartley, 1993; Russel, Gill, Coyne & Woody, 1993), the physically ill, and deaths among family members (Black et al., 1993) than students pursuing degrees in English (Rompf & Royse, 1994), guidance and counseling, education (Russel et al, 1993), or business (Black et al., 1993; Russel et al, 1993). Social work students are more likely than other students to have been victims of child abuse or neglect, or to have had abuse occur in their families of origin (Black et al., 1993; Rompf & Royse, 1994; Russel et al., 1993). The predominant finding among these studies is the prevalence of alcohol and drug abuse among the families of these students (Black et al, 1993,; Hawkins & Hawkins, 1996; Marsh, 1988; Rompf & Royse, 1994; Russel et al, 1993). Though Hawkins and Hawkins (1996) establish a relationship between past contact with alcoholic families and present mental health problems among social work students, little in these studies connect problematic childhoods with adult concerns. There is little to indicate the relationship between early traumatic experiences and the choice of social work as a career, and little about differences between social workers with past problems and those without past problems.

Together with a group of M.S.W. thesis students, I put together a study comparing 79 entering M.S.W. students with 37 entering M.B.A. students (overall response rate of only 45%) to replicate studies comparing social work students with others, to develop items to cover the range of problems identified earlier – personal, family and community – to measure the degree to which they sought help, and to connect these experiences with choice of career. The sensitive items about past personal and family problems were incorporated into the overall flow of a long survey, and there were no missing data problems with these items.

Social work students were more likely to have experienced personal problems than were business students, especially emotional problems, and more likely to have experienced problems, especially emotional and marital difficulties, in their families of origin than were business students. There were no differences in the rates of experiences of community problems. There was some progress in developing measures. We were able to see which items were meaningful or ambiguous and which achieved a sufficient number of positive responses. And we were able to develop factors which indicated that typology of personal/family/community may not be the best way to organize the items. For example, we derived a family violence scale, a death and illness scale, and an emotional problems scale, each of which included items from both family and personal sections.

It seemed to me that there was an important gap in the grounded theory relating childhood problems to job retention. If positive responses to past difficult situations led to adaptive behaviors on the job, these past experiences should have led to the development of personality traits which could be measured but not observed through behaviors. To this end, we included measures of locus of control (Lefcourt, 1991), but found no correlations between this measure and the occurrence of past problems.

Testing the Theory

Next career stop was a tenure-track position at another social work school. This put me in the middle of a new collaborative project to measure the retention of Title IV-E students from all local social work schools.

The issue of retention continued to matter, especially because of the ongoing investment of funds in the Title IV-E stipend program, and continued to be a source of research funds. Now it seemed there would be an opportunity to quantitatively test some elements of my grounded theory. In the context of an ongoing longitudinal study, we put together a model to test predictors of retention. I proposed adding a shortened version of my items to the model. This proposal was vehemently rejected by our collaborative research group for ethical reasons.

The objections, along with my responses, were:

Objection: We should not include an untested non-standardized item.

Response: How will we move knowledge along if we are limited to already validated measures?

Objection: People won't answer these questions.

Response: They answer questions like this all the time, especially among professional populations. In organizational research, questions addressing issues such as the quality of supervision available are in fact more dangerous to respondents than items about personal lives. In my experience it is queries about salary that are most likely, among all the difficult questions we ask of subjects, to go unanswered.

Objection: People shouldn't have to answer these questions. We don't have the right to pry.

Response: It's accepted practice in social work research to address personal issues where relevant. As in any study, respondents were made aware of their right to not participate and to decline to answer particular questions.

Objection: DCS will use this information against applicants.

Response: DCS already uses a personality test to screen applicants. It's not reasonable that they would add a program to recruit those with a history of personal problems.

Objection: Conclusions from this theoretical approach will not be useful. Conclusions about issues such as salary, workload, quality of supervision, etc. can be used to actually improve the situation for both workers and the agency. But how can insights about the past experiences of workers improve the situation?

Response: There is considerable merit to this argument. Conclusions of this nature will be less immediately useful. However, there may be some benefit from identifying adaptive job behaviors that lead to increased retention. In addition, there could be some larger benefit derived through the insight that it is the positive response of social workers to childhood difficulties that is beneficial on the job.

Objection: Connecting past personal problems with present job behavior violates the value base of social work by putting the blame on the individual for such things as the advisability of remaining on a job. We should not look at the individual to explain behavior that occurs in a social and political context. It makes more sense and is more useful to look at the system itself, rather than at individuals.

Response: This last argument was profound, caused me much introspection, and led to a number of productive conversations with both doctoral and Master's students involved with the research. I was disturbed by the way that the practical exigencies of the research process funneled us toward a focus on the individual, in particular on the weaknesses of individuals. Deficits are easier to measure and identify among individuals than are strengths. Also, individuals are much easier subjects to work with than are organizations, communities, or policies. I became painfully aware that my research had fallen prey to the same value/practical dilemma that plagues social work as a profession. In spite of our professed intentions, we focus on the

problems of individuals rather than the deficiencies of systems because, for the most part, it is simply easier. Individuals are accessible, understandable, and changeable, compared to systems.

Another way to look at the situation was in terms of the classic reductionist dilemma, always a problem for social science research, which examines individuals in contexts. In my research, I had stumbled across a generalization about individuals and was now applying this generalization about individuals to a phenomenon of an organization. Is high turnover an attribute of organizations, or is it the sum of individual decisions? Are we concerned with why individuals choose to leave a job, or with why agencies have high or low turnover rates? This reductionist problem is indeed both an intellectual and an ethical dilemma.

In my own internal struggle, I was able to marshal two arguments (one noble; one self-serving) in my defense. The first was articulated by a friend: One has an ethical obligation as a researcher to pursue interesting and potentially important intellectual issues in spite of the above considerations. It is a violation of the ethic of the objective pursuit of knowledge for a researcher to abandon a potentially fruitful line of inquiry because of speculated potential misuse of the information by others, because there may be no immediate apparent usefulness, or even because of possible violations of the value preferences of the profession. Though this argument was comforting, it was the second argument that was persuasive. I thought I was onto something interesting here, with the potential to propel my career forward. In spite of my sympathy for the motives behind some of the above arguments, I saw nothing unethical enough to prevent me from continuing.

I was able to create a situation where I could quantitatively test the proposed relationship between childhood problems and job retention, by successfully responding to another RFP to study the retention of child welfare workers. This statewide study complemented the above local study in a number of ways. While more restricted in scope time wise, it broadened the geographical scope by inclusion of a statewide sample. This allowed us to include local economic conditions as predictors of turnover. The hypothetical model included personal attributes, organizational factors, and local economic factors. Thus, the research question directly addressed the issue of the appropriate level of intervention for turnover. Perhaps it's neither an individual or organizational question, but rather a factor of the local job market, salaries, and cost of living. Included in the questionnaire were my items about personal past experiences. There were no objections from this group of collaborators; no one noticed anything inappropriate at all.

Potential subjects for the study were all new public child-welfare workers hired in the state between April 2000 and April 2001. Survey data collection began in July 2001 and continued through April 2002. Over 1700 surveys were mailed. About 10% of these were inappropriate, e.g., went to individuals who were not child-welfare workers or who were not hired during our time period. With 519 active cases in the final data file, this is a response rate of approximately 34%.

A factor analysis on eleven items addressing specific childhood problems resulted in three scales that roughly captured our original distinction among community, family, and personal problems. Male, minority, and lower SES respondents were more likely to come from a background of community problems. Divorced respondents were more likely to report personal problems. Those workers who reported high levels of childhood personal problems reported low values on a number of organizational variables, including peer support and job satisfaction, and high levels of role conflict. These findings directly contradicted my hypothesis that these

childhood experiences would translate into bureaucratic survival skills. More to the point, there were no differences on these childhood problem measures between those who left the job and those who stayed.

Discussion

There are several aspects of this narrative to be highlighted. The first is simply as an example of the process of research with its many dead ends, often with conclusions that never see publication. The second is how the research that is carried out is dependent on the financial and institutional support that is available at a particular time. Few of us are in a position to directly pursue the research questions that capture our imagination. Rather, we respond to available RFPs and attempt to integrate our own research questions with the funded mandates. Third, research is a social, collaborative process. Though the research question discussed in this narrative was of interest only to me, the research process was always shaped, if not determined, through interaction with colleagues.

Most important, however, are the ethical and value issues that needed to be resolved as part of the inductive/deductive process. The strongest lesson for me was in the way that we could not proceed even to publicly discuss our initial findings until we had conceptually re-framed these findings in a manner consistent with the values of the social work profession. The notion that "sick" workers are best suited to survive in "sick" organizations was not a possibility to be seriously considered. Our re-framing was driven not by considerations of validity, but by the requirement for our research to fit with the prevailing intellectual and value paradigms of our profession. In doing research we accept without question results that we expect from our data – results congruent with our professional paradigms – while we diligently search for measurement or analysis errors to account for our unexpected or unwanted conclusions, or are led to a re-interpretation of the conclusions. It is a sobering thought that the conclusions of social science research (perhaps even basic research) are always nested in the value paradigms of their professions, if not explicitly than through the kind of self-censorship in which we engaged in this project.

If the above can be seen as driving the conclusions toward a positive congruence with the values of social work, the second striking aspect of our experience on this project, as was pointed out by our critics, drove the research in a manner not congruent with social work values. This was the way that the course of the research tended toward a focus on individual problems. It is the value of social work that our research include considerations of macro settings and macro factors, yet it has been my experience, with this project and others, that the course of least resistance tends to lead us to micro measures and interpretations. On one hand, this is a failure on our part to properly frame our research questions. The research question here was, "What are the predictors of worker retention?" rather than "What are the characteristics of agencies that have a high rate of retention?" On the other hand, it is because of the advantages of measuring phenomena at the micro level. Passing out questionnaires is social work's default research method. To answer the second question would require a substantial number of independent child welfare agencies, as well as agency-level measures that are not simply aggregates of individual responses.

My idea of the relationship between negative childhood experiences and turnover on the job turned out to be a bad idea. Now it is a discarded idea, never to appear in a model of turnover. Was my time wasted? No. How can knowledge proceed if we aren't free to pursue new ideas? Though it is a peculiarity of our research system that the lack of a relationship between early life experiences and

worker retention will not be published. Anyway, it's the difficulties and barriers overcome during the messy research process that ultimately provide the sustenance and the pleasure. Research, like life, is a process, not a destination.

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