Unresolved Critical Issues

Unresolved Issues in Qualitative Research

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The editors of this special issue asked me to write a 600-word comment on unresolved issues in qualitative research. There are many. There always will be thousands and thousands of words worth, not to mention pictures, tastes, odors, and textures. I’ll mention just one.

It has been fascinating to watch the exponential growth in qualitative/ethnographic research, literature, and software over the last couple of decades. The term “qualitative” is now so promiscuously used that it’s no longer clear what it means on any specific occasion. The only clear meaning is kind of data, qualitative or propositional versus quantitative or numeric. But I’ve never written an ethnography without numbers in it, and positivist colleagues often use propositional data as alternative measures of theoretical priors.

In multidisciplinary projects I’ve worked on, the ambiguity has turned “qualitative” into a dangerous term. Everyone agrees that a qualitative component is useful, interesting, or required by a funder. But once the actual research starts, it turns out that the initial agreement concealed major differences in epistemology and research practice. Serious project conflict surfaces where all was peace and love beforehand. Examples like iterative interview design and discourse-based coding and abductive logic and the like are beyond the limits of this commentary to describe.

Over the last decade or so I’ve tried to straighten all of this out, at least in my own mind. A published version of a lecture I gave, “An Ethnography by Any other Name,” can be downloaded from my website (http://www.ethknoworks.com/). But in recent workshops I’ve started thinking about a more fundamental and historically grounded answer to what this “qualitative” business is actually all about.

Qualitative/quantitative is the eternal return of a question that dates at least as far back as Hegel versus Newton. The question is: Is human social research a different kind of science? The conclusion of some, though far from all, people who call themselves qualitative would be, yes it is. At the human/social level of the scale, the consciousness and sociality of both researched and researcher are a necessary part of the research process. We are not gas molecules.

I use a cartoon now to set up the issue in lectures. It was originally published in the New Yorker in 1925, authored by E.B. White. An elegant-looking mother and daughter are sitting at the dinner table. The mother says “It’s broccoli, dear.” The daughter says, “I say
it’s spinach, and I say the hell with it.” Human consciousness and sociality are required to describe and explain what’s in play here, and the consciousness and sociality of the researcher—or in this case the author, publisher, and reader—need to be part of the story as well.

This I think is what the qualitative/quantitative distinction wants to be about, but it has lost its bearings. Dozens are the towering ancestors who argue that human social research is different from received view science. In addition to Hegel, names like Vico, Dilthey, Tarde, Husserl, and Schutz come to mind. If I were classically educated, I could probably come up with a couple of Greeks and Romans. A Jesuit-trained colleague argued that the Sophists were just misunderstood social constructionists.

The point here isn’t to provide an exhaustive list of names or a reading list. The point is that these are a few of many examples of a substantial and profound intellectual history around the argument that human social research is a different breed of epistemological cat, whether propositions, numbers, or any other kind of information is in play.

Qualitative, in the drug field or anywhere else, needs to reposition itself in this powerful intellectual history and draw some clarity and coherence from the exercise. Qualitative needs some perspective from standing on the shoulders of giants. Never mind that Newton coined that phrase. He wasn’t all bad.

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Qualitative Research Transformations: The Strange Case of the Video Addiction Challenge Tool

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From 1996 to 1999 an extensive European clinical research project was funded under the Fourth Framework Programme of the European Commission, BIOMED II (BMH4-CT96-0688 DG 12-SSMA) (Broekaert, Raes, Kaplan, and Coletti, 1999). The project had the objective of investigating and assessing the treatment and relapse prevention needs of

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emerging drug-dependence groups in Europe. The study brought together a total of 11 universities and research centers from different European countries (Sweden, Norway, Germany, Italy, Spain, Greece, Belgium, Netherlands, France, and Scotland) and 35 residential treatment services, which were, for the most part, therapeutic communities. The project had multiple aims including the mapping the needs of the new kinds of clients at the residential treatment centers (e.g., dual diagnosis clients); the standardizing of psychiatric protocols for relapse prevention; the constructing of a European database of relapse prevention strategies, and the evaluating of the impact of implementing research and clinical instruments into the therapeutic setting. With such a large database of over 1,000 European clients, the quantification of the results was obvious. However, the project also was transformative in its design and utilized qualitative approaches at several key junctures to achieve its objectives. The statistician of the project, Ove Frank, the Professor of Social Statistics at the Stockholm University, continually challenged the project researchers to use statistics in creative ways and not to take the quantitative data for granted without a careful inspection of its underlying categories (Frank et al., 2001). He emphasized that neglected statistical procedures, such as entropy based on the mode measure of central tendency, stood at the interface of the transformation of qualitative data into quantitative data and needed to be included in the analysis along with the conventional methods of applying statistics (Frank, 1998).

This transformative process spawned a number of innovations including the development of a new clinical research instrument, the Videotaped Addiction Challenge Tool (VACT). The strange case of the VACT was indicated by the development of this qualitative tool in the midst of a project that had collected extensive quantitative data. The VACT was designed to provide a psychoeducational assessment of a client that could be used in his or her individual treatment planning. The VACT employed a videotape depicting an “average” life story of a therapeutic community resident. The initial idea of the instrument was to show the videotape to each new client that was admitted to the program in order to stimulate personal discussions about his or her life. This should lead to enhanced conceptualization by providing new and additional qualitative information from the client’s experience. In a later phase of the therapeutic community program, the VACT was used as a tool to initiate in-depth discussions within the treatment groups. The methodology of the development of the VACT has been described extensively: The script of the videotape evolved on the basis of a rigorous qualitative analysis of life stories and the anamnesis file information (Broekaert, 2001; Broekaert et al., 2001). The study of the reliability and validity of the VACT was aided by the computer software program WinMAX 98, which provided a case-oriented quantitative transformation of the initial qualitative data based on the theoretical and methodological principles of Max Weber and Alfred Schutz (Colins, Vandevelde, and Van Hove, 2008).

The strange case of the VACT was not limited to its methodological place in the project. The application of the VACT also led to transformations in the understanding of what characteristics of clients were the most clinically relevant. For instance, while using the VACT in clinical situations, criticism was raised that the instrument had a gender bias and was “woman unfriendly.” The life stories used in the videotapes were heavily male stories and the resulting transformations were not relevant to the females. For this reason, a female version of the VACT was developed in close collaboration with the female residents in De Kiem therapeutic community in Belgium, which functioned in the project as the VACT clinical research development unit. While describing this process, it became clear that the women not only changed the content of the script and the video performance but, being so closely involved in the development process, the methodology to obtain a new version of the VACT was also challenged (Broekaert and De Wilde, 2005). It changed from a classic individualistic qualitative approach into a community participatory approach
with much more accent on collaboration, emancipation, diversity, and inclusion. From a theoretical point of view, the methodology was further transformed from a modernist social constructivism postmodern approach incorporating a denial of the great narrative and an accent on change and uncertainty (Roets, Reinaaert, and Van Hove, 2008).

In considering the VACT, one can hardly ignore the integration of modern and postmodern aspects. To a certain degree the three traditional major paradigms that underpin science (existential-phenomenological, empirical-analytical, and critical social) alternated with each other resulting in a dialectical integration and transformation of the original vision and version of the VACT. This development produced an astonishing insight: Modern and post-modern qualitative approaches can easily be attuned, integrated, and transformed in concert with quantitative methods. This implies that a holistic paradigm underpins the existing scientific paradigms. The unresolved challenge lies in how the diverse qualitative and quantitative research approaches can be harnessed using a transformative scientific research process.

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References


**On Odds Ratios and *Contagium Vivum*: Homo Politicus and the Ethnographer’s Identity**

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In 1993 we received the visit of Mr. Edouard Balladur, the Prime Minister of France. He spent an hour at a field station in the North-East of Paris that was in the heart of the crack scene at that time (more recently areas of great social unrest). During the discussion with the social workers who worked with our research team, he came to understand that almost all crack users had a housing problem and asked, “But why don’t they sleep at home?” After the visit, there was no doubt to us that our “homo politicus” was animated by a strong will for knowledge and comprehension. However, we did not at all envisage that such a will would be accompanied by such ignorance and lack of a basic understanding of the daily life experience of crack cocaine users.

A few years before, in 1987, we were conducting our first ethnographic study of the effects of liberalization on the sale of syringes in French pharmacies (Ingold and Ingold, 1989). At that time we were well aware that our scientific mission was to let the words of the drug users be heard by the policy makers. Our research aimed at not only assessing the impact of this innovative policy but also to provide a realistic evaluation of how and why the

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policy was in fact working. By 1996, we were doing our third study on injecting drug use (IDU) risk behaviors (Ingold and Toussirt, 1996). Our research was tracing the evolutions of the twin HIV and HVC epidemics. Year after year the size of our samples increased: from a little less than 300 in 1987 to almost 2,000 in 1996. But even as our sample sizes grew, our methods remained the same: snowball sampling in three to five large French cities, participant observation with field notes, interviews, and short questionnaires. For us, the definition of a “qualitative” study with large numbers had to be viewed at a strict methodological level. Even with these large numbers, we struggled to avoid losing the sense of our subjects’ field of experience and point of view. As with Ed Preble, the great pioneer of drug ethnography, who published his seminal study in this journal 40 years ago, the use of numbers and percentages did not worry us (Preble and Casey, 1969). We simply saw them as illustrations of a more global interpretation that we were developing from our extensive qualitative data. The results of our studies could never be adequately summarized by quantitative figures alone. The richness and subtle variations provided by our qualitative data drove our attempt to grasp the total impact of the epidemics on our subjects and their communities.

From a political standpoint, our scientific choices failed to recognize a fundamental need of homo politicus for quantitative figures. The imputation of a direct and unmediated link between the idea of a scientific proof of a political argument and a series of numbers that stood for the facts characterized the thinking of homo politicus. The growing size of our samples was attractive to the policy makers as was our data that could prove their political points. But at the same time, these temptations exposed us to the risk of a quantitative drift in our basic research approach. For example, we were sometimes urged to use the statistical methods of logistic regression with their elegant “odds ratios” to represent our findings. But for us the simple use of odds ratios posed a real danger. Reducing our intensive fieldwork to odds ratios could lead to a serious weakening of our ethnographer’s identity. It would have been a convenient and politically expedient for us to abandon the field and the perspectives of our subjects and enter into the disembodied and anonymous analytical zone of odds ratios accounting for particular distributions of variables. This transgression would have replaced the real subject of our research with a virtual one. However, a virtual subject cannot by definition be contaminated by a virus.

For us, who saw ourselves as drug epidemiologists as well as ethnographers, this transgression would have been necessary for us to reject Frascator’s hypothesis, according to which infectious diseases are indeed transmitted by contaminant particles—the “contagium vivum”—and certainly not by miasmas, putrid atmospheres. Girolamo Frascator was a poet who in the 16th century invented the concept of microbial cells that he called “contagium vivum.” In the modern French context in which we conducted our research, rejecting the bleach strategy accompanying the syringe exchange for preventing infection corresponded to the idea that contamination was somehow unavoidable and was caused by the miasmas that had been argued for by Frascator’s contemporary opponents (Ingold, Toussirt, and Jacob, 1994). Likewise, it was difficult to imagine for our own contemporary opponents in France that water could transport a virus from one person to other via an unused syringe, and that simple bleach would be an effective intervention to prevent the spread of the HIV contagium vivum. I seriously doubt whether the discovery of this process of contamination of the HIV contagium vivum could have been done using the virtual subjects constructed by a miasmas represented by the odds ratios of collection of risk factors. Instead, this discovery, like so many in the history of science, had to be achieved by
intensive participant observation of a large number of real IDU subjects in various French cities and independently by ethnographers in other countries.

Regrettably, this 1996 research project became our last funded one. We would experience the homo politicus’ will of ignorance witnessed firsthand 3 years earlier on that fateful day in North-East Paris. The Prime Minister would believe—reading our works since 1987—that he had nothing more to learn from our studies and avoid questioning himself about the dramatic differences between the trends of HIV and HCV epidemics among new populations of drug users. He would accept that preventive strategies targeting IDUs could be inspired from models which had been invented for the gay community and not by the drug ethnographers’ works. That is, models focusing on mucous membranes and neglecting the vectors of contamination (the water for injections). For the gay community, prevention equals the use of condom. But for drug users, prevention is a more complex problem because we have to look at the vectors of contamination, that is to say, blood and water; and the sharing of syringes/cotton/water, etc. He would drastically reduce the budgets for qualitative research and preventive actions among the new French minority groups and their growing number of drug users, who our team of French ethnographers and social work colleagues were beginning to work with. Finally, he would—looking at the obvious and dramatic social needs of this population—convince himself that it was the right choice to leave these drug users to their bitter fate.

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Qualitative drug research has a long and rich history of adaptation to both changing conditions and demands for knowledge (Feldman and Aldrich, 1990; Preble, 1980). The global HIV/AIDS epidemic with the discovery of intravenous drug use and sexual behavior under the influence of drugs as key risk factors for the spread of infection provided new conditions that stimulated the development of qualitative research. The design and implementation of public health interventions demanded new knowledge based on detailed descriptions of the subculture and context of the daily life of drug users from their own point of view. Qualitative research methodologies had the flexibility and adaptability for use in a variety of settings and situations in order to obtain otherwise unavailable data. In the process, new risk behaviors and new risk groups were discovered (Grund, Kaplan, Adriaans, Blanken, and Huisman, 1990; Sterk and Elifson, 2000; Zule and Desmond, 1999).

Short-term exploratory research and rapid assessment studies became increasingly necessary employing the competencies of qualitative researchers (Needle et al., 2003; Power, 1996). The training of “indigenous” field specialists for outreach in order to recruit and collect data became a new field of specialization for qualitative drug researchers (Wiebel, 1993). Methodological innovations to access and sample “hidden populations” were extensively created (Dunlap and Johnson, 1999; Heckathorn, 1997; Lambert, 1990; Sifaneck and Neaigus, 2001; Valdez, 1999; Watters and Biernacki, 1989). These methodologies provided essential tools for the accurate updating of patterns of drug use that could not be detected by existing quantitative data systems for surveillance and monitoring (Maxwell, 1999). Drug epidemiology showed signs of becoming a new “translational” science that synchronized quantitative estimates of the extent of drug use in populations with thick descriptions of the qualitative patterns of the nature of drug use (Agar and Reisinger, 1999).

With the established demand in drug research for qualitative methods stimulated by the HIV/AIDS epidemic come new challenges for the future. The acceptance of qualitative methods, paradoxically, has highlighted a persistent, but still critical unresolved methodological issue that drug research shares with other related fields—the effective and efficient integration of qualitative and quantitative research methods. Creating the necessary synergy and synchronicity of qualitative and quantitative research is not an easy process without costs (Kaplan and Verbraeck, 2001). The integration requires careful and reflective thought that may not be conducive to the haste that is often required in the competitive environment of today’s drug research (Pels, 2003). Reframing the issue in terms of the most recent calls for translational research may have benefit. Translational research presently is changing the way drug research is being done (Gual, 2008; Office of Behavioral and Social Science Research, 2007). By capitalizing on the growing momentum urging drug researchers to think outside their narrow disciplines, impetus can be given to qualitative and quantitative drug researchers to create new designs that integrate their specializations.
Qualitative researchers must make the first step of translating their work for speakers of quantitative research discourse. In response, speakers of quantitative idioms need to work on developing their capacity to listen to the voices presented by qualitative research and then find the appropriate quantitative design equivalents. This translation process does not change the fact that both methods are distinct. Methodological integration occurs in the transactions between quantitative and qualitative researchers in the creation of the procedural elements of a comprehensive research design. Qualitative and quantitative thinking are needed in data analysis as much as in data collection. The analysis of drug use in a population as a whole will still require the awareness of the difference between the validity of statistical relationships and the coherency of the basic social processes that express these valid relationships in different contexts and cultures. Admittedly, the feedback loops involved in this translational process are still not very clear.

General models that have been proposed in the past to link qualitative and quantitative research are largely linear (Miles and Huberman, 1994). These models do not account for the subtle and nonlinear interactions between the two methods at key points of the research process. New areas of investigation, such as drug use and disasters, can testify to the value of translating qualitative research methods into their quantitative equivalents. This research demonstrates how qualitative descriptions of the large variation observed in patterns of drug use in hidden populations after 9/11 can provide material for quantitative researchers to construct interpretations for their statistical findings of the stability of drug prevalence in these populations before and after the man-made disaster (Factor et al., 2002).

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References


The Team Concept in Qualitative Research With Illicit Drug Users

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Team field research is a methodological strategy that has over time gained popularity among social and behavioral drug use(r) researchers. The team concept is not new to scholarly and policy-directed research, but received increased attention since the 1970s with the resurrection of everyday life social theory (e.g., symbolic interaction). Jack Douglas (1976) conceptualized team research as a practical tool for exploring field settings marked by conflict in values, politics, resources, objectives, and viewpoints. In these types of social situations, one cannot trust the ability of a single researcher to capture all the nuances and facets of complex social phenomena. This argument would certainly apply to socially stigmatized and difficult to penetrate phenomena like illicit drug use.

In our study of the early rave movement (Kotarba, 1993), for example, a team stratified by age and social status was invaluable to gaining an understanding of this emerging subculture. A contingent of young sociology graduate students—in their early 20s—was necessary for gaining entry to the rave scene, for appreciating the unique jargon used to communicate within the scene, and for having the capacity to attend rave parties that typically began after midnight and lasted through the early morning. These generationally distributed skills were later called upon when we conducted a study of the more contemporary online discussion and management of club drugs (Kotarba, 2007).

The ideal model of team field research denotes a reflexive division of labor. In terms of horizontal imagery, all team members have the opportunity, if not the responsibility, to engage in all or perhaps most of the team’s tasks. In our ethnographic study of drug-injecting, homeless youth (Kotarba et al., 1991), all team members engaged in interviews, observation, data entry, and analysis, although team members selectively engaged in uniquely skilled tasks, such as computer management and report writing.

In recent years, however, it appears that the ideal team research concept has been increasingly difficult to sustain. As the call for intervention and related clinical skills increased, research project membership has become increasingly fragmented, and organizationally vertical. Instead of field researchers, we have ethnographers, community health outreach workers, interviewers, grant writers, report writers, and so forth. Again, over time, the increase in the amount and complexity of federal funding for field research has created job categories that serve to isolate staff members from each other.

I would argue that the integrated and horizontal team is more important than ever in research on illicit drug use. The shifting demographics of drug use (e.g., constant fluctuations in cigarette and steroid use); rapid change in the illicit drug-supporting subcultures
(e.g., hip hop, country, and athletics); and the changing historical contexts of drug use (e.g., the effects of massive events, such a Hurricane Katrina) all call for a renewed emphasis on the exploratory essence of classic team field research. In our current study of the reformulation of New Orleans drug markets in Houston following Katrina (Dunlap, Johnson, Kotarba, and Fackler, forthcoming), understanding the clash of drug subcultures; the rapid evolution of subculture drug-usage patterns; the largely unexpected integration of a New Orleans gang culture into an essentially nongang-oriented Houston drug scene have been enriched and even made possible by a type of brainstorming, which is made possible by the common conversation among university students, street-wise ethnographers, university research faculty, and NIDA research executives. An orientation to discovery is critical to drug research at a time when, as Douglas (1976) instructed us, drug marketing and usage scenes are especially marked by self-deceptions, misinformation, evasions, and fronts.

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Joseph A. Kotarba is professor and chair of Sociology at the University of Houston. His most recent books are Understanding Society Through Popular Music, coauthored with Phillip Vannini (Routledge, 2009); and Postmodern Existential Sociology, coedited with John M. Johnson (Alta Mira, 2002). Dr. Kotarba has served as principle investigator, researcher, and consultant on NIDA-sponsored studies examining injectable drug use, HIV/AIDS, homeless adolescents, drug use and popular culture, rave parties, and Internet websites focusing on designer drug use. He is the 2009 recipient of the Lifetime Achievement Award of the Society for the Study of Symbolic Interaction.

References

Difficulties in Conducting Ethnographic Research of Drug Use in Japan

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Compared to 10 years ago, it is much easier to obtain soft drugs, such as marijuana and MDMA, in the major cities of Japan. Even methamphetamines are not hard to purchase through underground connections. The amount of confiscated MDMA by police in 2005 was three times more than that in 2002. However, very few ethnographic studies of illicit drug use have been conducted in Japan to describe and understand the routes for obtaining drugs, systems of drug trafficking, patterns of drug use, social and cultural contexts of drug use and treatment, and the impact of drug use on society, health care, the judicial system, and the economy. There are several reasons for the lack of ethnographic studies in drug use in Japan. Most importantly, researchers fear of being arrested while in the field for charges of possible use, possession, selling, and transferring of illicit substances. There is no legal protection for researchers who contact and interact with drug users in private places, build rapport, and ask them to describe their substance use and life experience. Also, there is no legal protection for drug users who agree to be interviewed by researchers. Even with drug users who are serving sentences in jails or prisons, it is very difficult for researchers to get approval from the judicial system to conduct interviews. Why are there so many obstacles and barriers for researchers to conduct ethnographic studies of drug use in Japan?

One reason may be that substance use issues are not considered an important social problem in the Japanese society. For instance, there are very few universities, schools, or even classes which address substance abuse prevention and treatment. However, the biennial National General Population Survey reported that more than 2.3% of the population has used illicit drugs, such as solvents, marijuana, MDMA, and other drugs. Our previous research showed that many Japanese who travel or temporarily reside in foreign countries, such as Japanese tourists in Bangkok, Thailand, and students in Honolulu, San Francisco, and New York City, use illicit drugs.

The substance use treatment issues or policies to regulate illicit drug use come to national attention only after criminal cases involved with substance use were sensationally reported by the Japanese news media. In order to improve the substance abuse prevention and treatment programs, policies to regulate illicit drug use, and criminal-justice systems, it is absolutely necessary to describe and understand the lives of drug users in Japan. This is especially the case for youth who experiment and become dependent on drugs, adults who daily inject methamphetamines, gay men who use poppers for sexual pleasure, and other drug users who become addicted.

We recommend the following to facilitate substance use treatment and prevention research, as well as to change the policies and regulations regarding substance use issues in Japan. First of all, Japanese policy makers need to acknowledge that the campaign against substance use by facilitating fear of addiction to drugs and severe sentences for...
drug-related crimes have limited impact on prevention. Prisons are overcrowded and beyond capacity because the number of prisoners who are serving for drug-related crimes has substantially increased in recent years. Second, appropriate substance abuse prevention and treatment programs must be developed specifically to target certain audiences, and the campaign for substance use prevention must be delivered at multilevels, such as mass media, schools, public service agencies at local and national levels, non-governmental or community organizations, and even business associations (e.g., neighborhood merchant associations). Professional researchers and clinicians in substance abuse prevention and treatment in Japan are mostly in the field of basic or clinical sciences. We need to facilitate collaboration with researchers in the social sciences and public health, such as sociology, psychology, anthropology, criminology, and epidemiology. Above all, the Japanese government needs to increase the budget in substance abuse prevention and treatment research. The government-funded programs must address to nurture new investigators in the field of substance abuse prevention and treatment, and implement effective and culturally appropriate drug use prevention programs, particularly targeting youths and young adults.

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Unresolved Issues in Research on Drugs: The Impact of Illegality on Health and Well-being

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Harry Anslinger started it all—asserting that a pattern of drug use associated with undesirable people was somehow a threat to the “cream of American youth.” The courts’ early opinion on the subject of opium smoking (Chen vs. State of Nevada, 1876) had come down on the side of the individual’s right to practice any kind of private, personal behavior as long as it did not infringe on the rights of other individuals. One pictures an exhausted Chinese railroad worker, chilling out in his tent over a pipe of opium as a meager respite from a grueling day’s work. Events of the time somehow thrust Mr. Chen into the courts to defend his right to smoke opium. Things have gone downhill ever since. The same animus that led authorities in Nevada to prosecute Mr. Chen continues to pervade the official approach to the use of illegal drugs today.

Well-meaning scientists have weighed in on the debate, most often on the side of efforts to control consumption of illegal drugs, but sometimes favoring decriminalization. The evidence is mixed. On one hand, the human debasement that accompanies addiction has become evident to most people, and naturally we would want to strive to protect people from that experience. On the other hand, each major drug in the illegal pharmacopeia has millions of nonaddicted users. How well do we understand what differentiates the addicted from the unaddicted? What proportions are we talking about? In the case of alcohol, we can estimate that up to 10% of ever-exposed users eventually develop addiction. It is more difficult to estimate proportion of impaired users in the total group of ever-users for the illegal drugs, because we have such a difficult time defining denominators. These are always gross estimates based on multiple indicators of variable quality, such as emergency room presentations, treatment enrollments, street convenience samples, and medical examiner records. When it comes to assessing impact of illegal drug use, global indicators seem ill-suited to the purpose.

Street ethnography, where the social scientist and the drug user occupy the same space, appears well-suited to making an assessment of how illegality of street drugs affects users and the people in the users’ immediate social environment. We used that technique in assessing marijuana use among long-term consumers of that drug in Costa Rica, and it told us several things about Cannabis smoking that our hospital tests could not: users have to endure arrests and incarcerations far more than nonusers; they sometimes find it helpful in their jobs, especially dull, repetitive jobs; users have conflicts with the women in their lives over marijuana smoking, because women tend to distrust men who smoke; and the more income the users had, the more they smoked. Participant observation and open-ended interviews yielded those and other results that extended our understanding of health and personal impact. Other examples of street ethnography, from Rosenbaum’s Women on Heroin to Sterk’s Fast Lives, to Bourgois’ In Search of Respect, have likewise contributed

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to our understanding of how drug-using patterns interact with the daily lives of the users. They all suggest the same thing: Even the addicts might be better off if their drugs of choice were obtainable from legal sources.

We can hardly resolve this issue in this limited space, but it needs more attention as we watch public treasure being squandered on failed interdiction policies and incarceration of an unconscionable proportion of our citizenry. Long-term drug ethnographers have accumulated in their experience the necessary facts to make policy statements that refute the polemical assertions of “drug warrior” politicians. Let’s use that knowledge base to best advantage—soon!

THE AUTHOR

J. Bryan Page, Ph.D., is professor and chair of the Department of Anthropology at the University of Miami. He has been engaged in anthropological study of street-based drug use in many different settings during the last three decades. His studies have led to productive collaborations with diverse disciplines, including molecular biology, virology, immunology, ophthalmology, internal medicine, and psychiatry. Each of his studies begins by establishing a perspective on the use of drugs in the natural habitat of the user. From this level of understanding, he builds layers of perspectives and parameters to derive conclusions beyond the reach of a single discipline.

On Future Directions of Qualitative Drug Research: Technologic Advancement and Symbolic Interaction

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As qualitative methodologists engaged in drug research continue to utilize different forms of emerging technology for data collection and analysis, it promises to ultimately guide the field in more secure scientific directions. In addition, as society becomes more technologically dependent, so do the new generations of qualitative researchers. While this can be viewed as a fruitful development, it may be at the expense of the symbolic process of interaction.

Over the course of the last 15 years I have been engaged in various capacities of drug use(r) research from a street/community-based ethnographer to project management on studies involving diverse drug using/selling populations (marijuana/blunt smokers, non/new injectors of heroin, prescription pill and club drug users, etc.). I have seen different forms

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of technology improve many aspects of the research process. Mobile communications technology such as pagers (beepers) and cellular phones both had a very instant and profound impact on the research process. First, pagers made it easier for ethnographers to stay in touch with their respondents and other project staff members. I employed pagers in the field when they were a necessary accoutrement of retail drug sellers, also co-opting the numeric codes used in the drug trade, which facilitated communication. Mobile phones soon replaced pagers, and mobile phones have been replaced by phone/PDAs (Treos, Blackberries, etc.) as mobile communications got more sophisticated, immediate, and precise.

Besides improvements in communications technology, the ability to record field notes and interviews improved with new innovations. Laptop computers and PDAs replaced the traditional handwritten “field notebook.” Analog audio tape recorders gave way to smaller, sleeker, higher-audio quality digital recorders. While digital audio recorders and highly portable and mobile computers may have improved the aesthetic quality of collected data (higher fidelity recordings, word-processed field notes, etc.), they have not improved the ability for the researcher to understand “the meanings” that arise when observing interaction.

When Herbert Blumer wrote his seminal work *Symbolic Interaction: Perspective and Method* (1969), which provided a theoretical framework for the ethnographic method, he articulated the following: “Instead it [symbolic interaction] sees meaning as arising in the process of interaction between people.”

If “meaning” manifests itself in the “process of interaction between people,” what happens to meaning when technology becomes a conduit for the interaction? Does meaning get interpreted differently? Does the “process” of ethnographic research get abstracted through technology ultimately affecting its analysis? As more levels of technology get added to the research repertoire, does the researcher place more levels of distance between themselves and the actual persons and cultures being studied?

If Geertz observed the Balinese cockfight via streaming video, while he would be able to visually observe what transpired, he would not be able to smell the sweat of the crowd, feel the tension and temperature of the air, and observe the peripheral social contexts of interaction—all necessary qualities for adequate thick description and interpretive understanding of cultural meanings.

Other forms of technology which have impacted the methodology are qualitative data analysis software (see Johnson et al. this issue) While this type of software has made it easier “to manage” larger amounts of qualitative data, it has also placed “levels of distance” between the interactive words and the researcher. This type of software can be used to identify, quantify, and categorize key words and phrases, but it cannot decipher nuances of meaning. Also, this software cannot adequately decode words embedded in specific subcultural contexts.

When teaching graduate and undergraduate students the ethnographic method, I speak of ethnography as being as much of an “art” as it being a “science,” where three essential elements are required: access, rapport, and trust. Access to a population is an obvious prerequisite for an ethnographic study, and the success of any ethnographic research relies on the access the project’s ethnographers (and not necessarily the principle investigators) have to a population. Rapport is the second step in the ethnographic process, which is dependent on the interpersonal communication skills of the ethnographers. These skills involve the interpretive use of language and specific subcultural codes, along with an array of nonverbal communication abilities. Trust only develops between the researcher and those being researched when sincere and genuine rapport is established. Trust is a
necessary condition for research participants’ divulgence of truthful information, which is even more vital when studying stigmatized behaviors, such as illicit drug use.

Different forms of technology have aided ethnographic drug researchers in a multitude of ways. These forms include mobile communications: field note, audio, and visual data collection; and qualitative data analysis. Qualitative researchers need to understand the fine line between being assisted by technology and being dependent on it. As more complex technologic possibilities surface, they will never substitute for the traditional skills required to perform ethnographic research including observing and interpreting meanings of interaction, and establishing rapport and trust with research participants. Integrating technology into the ethnographic process will be most successful when the rudiments of studying symbolic interaction are first practiced and mastered.

THE AUTHOR

Stephen J. Sifaneck, Ph.D., has been engaged in a full-time research career (on various NIDA and other government and private-funded grants), which has focused on studying different drug using and criminal/deviant urban populations since completing his doctorate in sociology in 1996. These populations have included users and sellers of marijuana, new injectors and noninjectors of heroin, prescription drug users/smugglers, club drug users, gang members, sex workers, nomadic homeless youth, squatters, and street peddlers. Throughout this tenure, Sifaneck has authored and coauthored over 30 peer-reviewed journal articles and book chapters. His works have been referenced internationally in the United States, Europe, and Asia. In the fall of 2007, Professor Sifaneck was recruited to teach and develop the new Justice Studies Program at Berkeley College in New York City. He presently resides on the Lower East Side of Manhattan, and is a full-time professor at Berkeley College.

Reference
