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THE TRIALS AND TRIBULATIONS OF CROSS-CULTURAL RESEARCH

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We shall not cease from exploration
And the end of all our exploring
Will be to arrive where we started
And know the place for the first time.

—T. S. Eliot

Those of us who do cross-cultural research usually have a story of precisely when we began our journey into that global territory. For some, the interest might have developed at a very young age; for others, it might have been happenstance, having stumbled into the field after doing work within one culture for many years. The litany of reasons why we venture into the world of cross-cultural research is also very diverse—for some, it might be to discover general universal principles; for others, it is to uncover the “thick description” of a particular culture (Geertz, 1973); and for still others, it might be to simply understand the complex elephant of what culture is, addressing fundamental issues of human nature.

Although the beginnings of and passions for doing cross-cultural research are varied, researchers ultimately share many common experiences along the bumpy road of cross-cultural research—experiencing many of the joys and delights, on the one hand, and many of the trials, frustrations, and disappointments, on the other. Unfortunately, the restrictions of journal

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space and norms that focus researchers on reporting hypotheses, procedures, and results make it difficult to collectively realize that many of the issues that they encounter along the way are, in fact, common. The conference “Conducting Multinational Research Projects in Organizational Psychology: Challenges and Opportunities,” which took place at Michigan State University in October 2009 was a great success in helping researchers to share their research stories and to collectively make explicit what is often not discussed or implicit across many individuals. Whether you call the occasion an academic conference detailing complex research programs or a collective therapy session, sharing these stories is critical for building institutional knowledge in cultural science, for giving a realistic preview to newcomers entering the field, and for empowering all of us with information that can help facilitate high-quality cross-cultural research.

In this chapter, I detail some of my own experiences conducting cross-cultural research, including, among others, a quantitative study of tightness–looseness across 33 nations (Gelfand, Raver, et al., 2011) and a qualitative study of subjective culture in the Middle East (ME) that I embarked upon across eight nations (Gelfand, 2008). I begin with my own serendipitous entry into the field and place it in a historical context, discussing the intellectual heroes who have had a great influence on my own thinking. I then discuss some lessons learned, accumulated across many studies and from a lot of time spent in the cultural trenches. Because there are many good academic treatments of the issues that arise in the cross-cultural research process (see the volume on methodology in the seminal *Handbook of Cross-Cultural Psychology* [Triandis & Berry, 1980]; see also Cohen, 2007; Gelfand, Raver, & Holcombe Ehrhart, 2002; Matsumoto & van de Vijver, 2011), in this chapter I discuss specific examples of my own work, some published and some unpublished, to more vividly illustrate the issues that one invariably encounters when venturing into cross-cultural research territory.

STUMBLING INTO CROSS-CULTURAL PSYCHOLOGY

My own personal journey into cross-cultural research began, to echo T. S. Eliot (1943), when I was forced to step out of my cultural comfort zone and had the acute realization of just how much had been fundamentally shaped by American culture. I was a junior in college, a sheltered kid from Long Island, when I ventured off to London for a semester. I remember the strange sounds, sights, and smells of the United Kingdom, and being completely overwhelmed—dazed and confused—from experiencing the culture shock that comes with being away from one’s own familiar territory. I vividly remember a phone conversation with my father that was arguably the begin-

ning of my journey of becoming a cross-cultural psychologist. I was telling him about how strange it was that people in my study abroad group would go to Paris, Amsterdam, Scotland, and the like, for just a few days. My father responded, in his Brooklyn accent, "Well, imagine it's like going from New York to Pennsylvania!" That metaphor gave me so much comfort that the very next day I booked a low-budget tour to Egypt. It was just like going from New York to California, I reasoned (much to my father's dismay!). Those travels, and later living on an Israeli Kibbutz, sparked a lifelong passion for understanding the dynamics of culture. I was fascinated with basic questions, such as: How is it that culture shapes the self so profoundly, yet culture is so invisible and taken for granted? How does culture develop, how is it sustained, and how does it change over time? How does culture contribute to misunderstandings and conflict at the individual, organizational, and national levels?

When I was back at Colgate University for my senior year, I was fortunate to find that Carolyn Keating, a cross-cultural psychologist, was teaching a cross-cultural human development class. Keating was a student of Marshall Segall, and it was in that course that I became exposed to their great work from the 1960s in Africa that showed that even basic psychological processes—such as visual perception—are not necessarily universal. At the time, the notion that humans do not vary in fundamental ways of perceiving the physical world—space, size, distance, or color—went largely unquestioned in psychology. Segall, Campbell, and Herskovits (1966) turned this assumption on its head. Taking a largely empiricist and Brunswikian perspective, they argued that people use whatever cues they have learned through their past experience to perceive objects, a process they referred to as *ecological cue validity*. Their research, done across 15 countries, indeed showed that Europeans were much more susceptible to classic illusions, such as the Müller-Lyer illusion and the Sander parallelogram illusion. Aside from these fascinating differences in deep psychological processes, I was intrigued by their explanation of the findings: In explaining such differences, Segall et al. advanced the *carpentered world hypothesis*, which suggests that individuals who experience a lot of rectangular angles in their environment (which is more the case in Western cultures as compared with non-Western cultures) would be more likely to interpret nonrectangular figures as representations of rectangles, thereby exacerbating these types of visual illusions. I found this work—namely, that *culture* is a prime source of experience that causes different habits of inference to arise—to be completely fascinating, and it was my first entrée into the wide world of cross-cultural research.

It was in that course that I also became inspired by work that had been done in the "culture and personality school" in the 1950s and 1960s, and in particular, on how socialization processes and personality factors vary within a particular society (i.e., Benedict, 1946; Mead, 1928) and how culture

shapes, and is shaped by, personality across different societies (B. B. Whiting & Whiting, 1975; J. W. M. Whiting & Child, 1953). I was intrigued by the ecological approach in B. B. Whiting and Whiting's (1975) classic work *The Children of Six Cultures*. Their theory highlighted the role of the physical environment (e.g., climate, terrain), history (e.g., migrations), and maintenance systems (e.g., subsistence patterns, social structure), as important factors that shape children's learning environment, which in turn were thought to affect the development of adult personality (including learned and innate components) and projective expressive systems. Although the theory had a largely deterministic flavor and drew heavily on psychoanalysis, which was of less interest to me, the broad view of culture—and the idea that one could use the scientific method to understand it—was incredibly inspiring to that sheltered kid from Long Island!

After I graduated from college, I was determined to go to graduate school to study culture and psychology, yet it was clear that there were no PhD degrees in this field. In a fateful conversation, Keating recommended that I talk to Richard Brislin, then head of the East–West Center at the University of Hawaii and expert in cross-cultural training, about graduate programs in the field. After listening to my interests, Brislin declared that I should work with Harry Triandis at Illinois. The rest was history.

Triandis was an incredible mentor who influenced my thinking and my approach to science. The sheer breadth and depth of his study of culture—both basic cultural processes and culture's applications to personality, social, and organizational psychology—is forever inspiring to me. His classic early work on the *Analysis of Subjective Culture* (1972) has influenced my thinking to this day. Triandis was highly influenced by the work of Herskovits (1955), who defined *culture* as the human-made part of the environment, consisting of both *physical* elements (e.g., tools, bridges, educational systems, religious institutions) and *subjective* elements (e.g., beliefs, attitudes, norms, values). Triandis set out to further explore subjective culture and was the first to develop methods to systematically identify social psychological constructs, such as categorizations, associations, attitudes, beliefs, expectations, roles, and norms across cultural groups. His work showed that coherent themes cut across these different elements of subjective culture—for example, individualism and collectivism, which we later examined in terms of its vertical and horizontal elements (Triandis & Gelfand, 1998). Another major contribution of the *Analysis of Subjective Culture* was that, similar to the work of B. B. Whiting and J. W. M. Whiting (and later Berry, 1979), it placed the thematic elements of subjective culture into a larger ecological and historical framework. The theoretical framework that was developed included *distal antecedents* (e.g., climate) and *historical events* (e.g., wars), *proximal antecedents* (e.g., occupations, language used, religion), and *immediate antecedents* of

action (which included all the elements listed above), which result in patterns of action. The *Analysis of Subjective Culture* set the stage for large-scale studies on dimensions of culture that took hold in the 1980s and had a strong influence on my later interest in tightness–looseness and the structure of everyday situations. Triandis's work on the dynamics of culture (Trafimow, Triandis, & Goto, 1991) inspired my later interest in culture by situation interactions in the domain of negotiation and conflict (Gelfand & Realo, 1999; Morris & Gelfand, 2004).

Aside from the breadth of his theoretical training, Triandis gave me a big dose of the methodological realities that one confronts in doing cross-cultural research. His admiration for both *emics* (culture-specific elements) and *etics* (culture-general elements), and his insistence on using multiple methods, inspired me to use qualitative interviews, surveys, experiments, and archival methods, among others. He grounded me in the rich history and debates in the field, which I believe are critical to convey to new scholars (Kashima & Gelfand, 2011). Above all, Triandis's optimism, modesty, and good humor helped to bring the human element into science. His philosophy—that it is important to be passionate about one's work, to not take yourself too seriously, and to not be afraid to be controversial—has served as an important reminder to me throughout my career on the bumpy road of cross-cultural research.

VIEWS FROM THE TRENCHES: TRIALS AND TRIBULATIONS OF CROSS-CULTURAL RESEARCH

As a graduate student in the early 1990s, I read many papers about the issues that one confronts when doing cross-cultural research, but these challenges, which seem like distal abstractions as a student, come to life when you begin doing the work. The cross-cultural research process, as I described it (Gelfand, Higgins, et al., 2002), involves a road map wherein one has to make numerous judgment calls, or crucial decisions that need to be made without a hard or fast rule (McGrath, 1982). The research process is also cultural in that the very issues that researchers often seek to study—values, norms, beliefs, assumptions—also infiltrate every stage of the process, from deciding what is a worthy question (which itself is value laden; Gelfand, Leslie, & Fehr, 2008); to creating a multicultural research team (in which cultural intelligence is critical for managing cultural diversity); to designing and implementing a particular research method (which involves unique cultural reactions, differential motivations, and ethical issues); to analyzing, interpreting, and publishing the data. In this respect, methods are infused with culture; they are difficult to separate. As Shweder (1990) once aptly

remarked, "You can't take the stuff out of the psyche and you can't take the psyche out of the stuff" (p. 22). So too is the case with conducting cross-cultural research.

Theory as the Starting Point

Probably the best advice I got when starting to do cross-cultural research was on the importance of having a strong theory. Because of the sheer number of "rival hypotheses" that can explain one's findings (other than culture), having a strong theory provides one with much greater confidence that the results are not due to extraneous factors. *Theory* here refers to both the constructs of interest (What is the construct space? How is it operationalized?) and the relationships between constructs. Cross-cultural research is complex because the theory of the construct itself requires serious consideration, particularly given the risk of "imposing" etic constructs on other cultures (Berry, 1980). Many, including myself, deal with this issue statistically—can one demonstrate that the scales being used have similar factor structures, similar loadings, and are in essence "equivalent" (Berry, Poortinga, Segall, & Dasen, 1992). One of my first research projects on sexual harassment did just this. I used simultaneous factor analysis in multiple samples to demonstrate that the Sexual Experiences Questionnaire (SEQ), which had been developed in the United States, had the same structure in Brazil (Gelfand, Fitzgerald, & Drasgow, 1995). Over time, however, I became skeptical with using this statistical approach to justify the theory of the construct because it assumes that the construct space itself—which has almost always been derived from Western theory and Western samples in the vast majority of cases—has been "mapped" appropriately. From a psychometric perspective, although factor analysis can illustrate whether certain items or dimensions are relevant (are similar) or are contaminated (are not working well in other cultures), or both, it cannot reveal whether important dimensions of a construct have been neglected or omitted (e.g., whether certain dimensions of harassment are novel in Brazil). This is a theoretical issue and requires a deep understanding of the cultural context. I often find myself asking, why would a construct developed in the United States necessarily have the full range of variation that is needed to capture realities of the phenomenon in another context? For example, Ramesh and Gelfand (2010) showed that although job embeddedness was a universal predictor of turnover, family embeddedness was an important (but heretofore neglected) predictor of turnover in India (and even in the United States). Farh and Earley (1997) showed that if one wants to understand organizational citizenship behavior in China, new dimensions (unearthed in interviews about what constitutes the construct) and new measures need to be added to the construct for it to be relevant in China. To be clear, it is not necessarily the case that new dimen-

sions and items need to be added to a construct or measure, but assuming that the construct space “travels” perfectly is problematic.

Theory also guides the choice of samples. For example, in early work that I did on culture and procedural justice, I theorized that voice was much more important in high power distance than low power distance cultures. I selected Turkey (which is high on power distance) and Costa Rica (which is low on power distance) because they are both collectivistic societies; thus, I was trying to, generally speaking, isolate the cultural variable of interest. Our sampling of cultures for a multination study on cultural tightness–looseness was likewise guided by theory of the predictors of the construct. I theorized that population density, history of territorial conflict, resource scarcity, human disease, and natural disasters are predictive of tightness–looseness, and consulted extant archival databases (see the discussion below of the promise and pitfalls of archival databases) to choose nations that reflected substantial variation on these variables (Gelfand, Raver, et al., 2011). In other work (Kashima et al., 1995), we sampled nations so as to have variability on the individual, collective, and relational self to examine culture and gender influences on these constructs. Compared with representative sampling, this approach reflects *theoretical* sampling (Boehnke, Lietz, Schreier, & Wilhelm, 2011) that aims to maximize the variability of nations according to the theory being tested.

Theory also guides measures, whether they are mediators, moderators, or control variables. The importance of unpacking cultural differences has long been discussed in cross-cultural psychology (B. B. Whiting & Whiting, 1975). Merely showing country or other group differences does not elucidate the reasons for the effects, making it critical to try to illuminate the cultural phenomenon explaining the differences found. Although much work has used survey measures of personal values (e.g., of individualism–collectivism) as potential mediators, cultural mediators need not be based on value measures on surveys—they can be based on descriptive norms (Shteynberg, Gelfand, & Kim, 2008; Zou et al., 2009); the structure of situations, roles, and networks (Gelfand, Raver, et al., 2011; Kitayama, Markus, Matsumoto, & Norasakkunkit, 1997; Morris, Podolny, & Ariel, 2000); implicit measures (Kitayama & Karasawa, 1997); and artifacts or cultural products (Morling & Lamoreaux, 2008), among other variables. Unpacking cultural differences is a tricky business, but ultimately theory is critical for identifying potential mechanisms and alternative explanations for country or group differences.

Cultural Legwork

In my experience, compared with doing research on one sample, cross-cultural research takes much longer and requires considerably more legwork before one even launches a study. For example, gaining access to samples and

developing a highly functioning cross-cultural research team are critical for a successful cross-cultural project and require time, resources, cultural skills, a lot of patience, and some degree of pure luck. Building networks of people who are interested in the same questions, committed to the same publication goals, are willing and able to do all that is required to complete the process (translations, piloting, implementing, and interpreting data), and are on the same time frame with the necessary resources is a daunting task. Managing expectations—which can be very different depending on one’s cultural background—is critical from the very start of a research project. A number of basic questions need to be addressed before launching a project. Will all collaborators be authors on the study? (I personally believe this is important, having been influenced on this issue with Triandis.) Is the research question and method appropriate in the local context? How will translations be handled? Who “owns the data?” Are funding agencies involved, and does this have implications for local collaborators? Are there ethical issues that need to be discussed involving the participants, the researchers, or both? For example, in a project in which I have been examining culture and negotiation in the ME, the funding agency (Department of Defense; DoD) clearly wants to be acknowledged on publications that result. Yet for some projects, it became clear that acknowledging the DoD would place collaborators in some countries at a huge reputational risk. Knowing this, I was able to negotiate with the DoD that in some cases we cannot acknowledge the funding agency. Having honest discussions about the goals of the research, where one wants to publish it, authorship, funding, and time frames for the work is critical for the success of a multicultural research team. Of course, “honest discussions” themselves are culturally constructed, and considerable trust and cultural skills are critical for dealing with controversial issues and the invariable relationship, task, and process conflict that can occur in multicultural collaborations. There is the added temptation, in my experience, of dealing with these issues using e-mail discussions, but e-mail is too “lean of a medium” (Daft & Lengel, 1984) to accommodate the complexity of these tasks. In my own work, I try to meet face to face often, talk on Skype (at least you can gauge nonverbals), and communicate frequently to ensure that everyone is fully comfortable with the many issues involved in the research.

Having agreed on the nature of the collaboration, one is then in a position to get the research started. This itself also takes considerable time, energy, and patience. Whereas in my own unicultural work in the lab or field, I can typically get my surveys or experiments ready for testing relatively quickly, implementing cross-cultural research involves many other steps. When using an experiment or survey across cultures, it is critical to discuss and modify the questions, methods, and designs with local collaborators, and critically, to expect this cultural input to change one’s plans.

For example, in a recent collaboration, I created a multicultural team involving collaborators from Egypt, Iraq, Jordan, Lebanon, Pakistan, Turkey, and the United Arab Emirates to understand important subjective culture—values, focal concerns, beliefs, and norms—in the ME region. Because there is very little research on the psychology of culture in the ME, I thought it was critical to use qualitative methods to gain a “thick description” (Geertz, 1973) before developing surveys, experiments, or other methods. I settled on conducting interviews in all of these countries using the methodology pioneered in the *Analysis of Subjective Culture* (Triandis, 1972) as the platform, which involves word associations, antecedents and consequences of relevant constructs, and methods to illuminate situational variation in the constructs. Before developing the interview protocols, I read through numerous books and articles about the region, and then drafted a list of potential constructs to discuss with the ME team. These constructs were then the subject of many discussions within the team, and various questions were dropped as other questions were added. Based on this input, I drafted the next version of the interview protocols, which included interview probes on the psychology of connections (*wasta*), fate, honor (*sharaf, irdh*), face and public image (*wujah*), respect, modesty, dignity, values, trust, negotiation, conflict, revenge, forgiveness and apologies, and collaboration. This next draft was then the subject again of numerous discussions and iterations. After several months, we settled on a final set of protocols to pilot with local focus groups. Although the process was very time consuming, with this extensive cultural legwork I could be much more confident that the interview protocols reflected important focal concerns and would yield valuable “cultural fruit.”

Before even piloting the protocols (which is critical to do before launching the interviews), we had a lot more work to do to get ready to launch the study. First, there was the anguishing process of translation. All protocols were originally written in English and had to be translated into Arabic, Farsi, Turkish, and Urdu. Once translated into the local language, we had different translators back-translate them into English so we could compare the original English version and the back-translated version. Invariably there were many discrepancies, so we then needed to figure out where the differences in translation occurred (was it a problem in the original translation? or was the back-translation the culprit?). In my experience, translations often reveal problems that have a basis in poor English, for example, double-barreled statements, colloquial language, metaphors, and idioms that don't even make sense to English speakers! As a rule of thumb, I have always assumed that the English version will need to be modified in addition to the translated version. Another rule of thumb is that one should estimate the time needed to do translations and then double or even triple this time to have a realistic estimate. Coordinating numerous translations (wherein there

might need to be different changes that are standardized across all materials) takes considerable time. For example, in our study of tightness–looseness across 33 nations (Gelfand, Raver, et al., 2011), our study materials were in 21 languages, and the translation process took a full year to complete.

When one has confidence in the final translated materials, it is time to pilot the methods. Piloting is critical for the quality of cross-cultural research projects (Gelfand, Higgins, et al., 2002). All methods—experiments, interviews, surveys—need to be carefully examined for numerous criteria before implementing the research: Are participants familiar with the tasks, comfortable with them, motivated by them? Do they understand the instructions in the intended way? How do they react to the experimenter, interviewer, or other “epistemic authority” running the study? Are they perceived as ethically acceptable? Is there a problem with using deception if applicable? Is there ample time for the study? Are the incentives appropriate in all cultural groups? Piloting the study before data collection, in my experience, always results in important changes. For example, our pilots of the interview study discussed previously revealed a lot of perceived overlap in the questions that made participants less motivated. As a result of this feedback, we had to further condense and reorganize the protocols. Our pilots also revealed that we needed to spend more time building rapport, particularly in rural areas, to make people more comfortable answering the questions, some of which were highly sensitive.

Many of the pilots that I have conducted illustrate how the methodological choices we make are often laden with Western assumptions that, ironically, can relate to the very questions we are asking. Put differently, the very “stuff” that we’re interested in looking at cross-culturally is found in cultural issues that one encounters in the method. For example, in Gelfand and Realo (1999), we were interested in examining how accountability produces very different effects in negotiation depending on the cultural context. Much research on accountability in the United States in the 1970s and 1980s showed that accountability produces competition in negotiation: In fact, negotiators assume that their constituencies want them to be competitive (Benton & Druckman, 1973; Gruder, 1971). It is not surprising that accountability activated competitive construals and behaviors, and resulted in lower negotiation outcomes for individualistic samples. We reasoned that accountability need not produce competition in all cultures as it does in the individualistic United States; rather, we hypothesized that accountability acts as a norms enforcement mechanism, producing whatever is normative in the cultural environment. To test this, we first conducted research in the United States among Asian Americans and Caucasians and showed that, in fact, among collectivists (Asian participants), accountability activated cooperative construals and behaviors, and resulted in higher negotiation outcomes. These effects were reversed in unaccountable negotiations, when, in effect,

negotiators were released from normative pressures to do what is expected. Interestingly, when we tried to pilot the material in Japan for a follow-up study, we had a very difficult time getting our manipulations of accountability to work. In fact, low accountability situations were seen as having very high accountability, even after numerous pilots, making it difficult for us to run the study in Japan (at least as a scenario study). In retrospect, it is not particularly surprising that getting the manipulations of accountability to be equivalent in the United States and Japan is challenging, given that Japan is a very tight culture with much higher monitoring (Gelfand, Raver, et al., 2011) and thus has higher naturally occurring accountability as compared with the United States. Thus, what is perceived to be a low-accountability situation in one culture may be seen as a high-accountability situation in a culture with a higher base rate, in general, of monitoring. This one example aside, it is critical to ensure that one's manipulations are understood in the same way and have equal strength before data collection, even if the manipulation has worked time and time again in one's own cultural context.

In addition to having equal cognitive comprehension of the task instructions, it is important to ensure that participants in all cultures have equal motivation to perform the task. In other words, comprehension of the task is not enough; it also must be equally engaging across groups. Again, ironically enough, the very nature of the research question that is being addressed can reveal cultural issues in motivation in the method. For example, in Gelfand, Higgins, et al. (2002) we were interested in cross-cultural differences in egocentric biases in negotiation. Much research in the field of negotiation has illustrated that negotiators tend to view their own behaviors as more fair than others (Thompson & Loewenstein, 1992), which leads to more aggressive behavior, less concession making, and ultimately lower outcomes (Babcock & Loewenstein, 1997). In Gelfand, Higgins, et al. (2002), we theorized that serving biases of fairness in negotiation would be consistent with ideals within individualistic cultures, in which the self is served by focusing on one's positive attributes to stand out and be better than others but would be disruptive to ideals in collectivistic cultures, in which the self is served by focusing on one's negative characteristics to blend in and maintain interdependence with others (Heine, Lehman, Markus, & Kitayama, 1999). We first did a number of survey and scenario studies to examine cultural differences in egocentric biases of fairness in conflict in the United States and Japan, and then in our last study, we set out to examine how differences in egocentric biases affect hard negotiation outcomes. We created a simulation that required students to assume rules and to negotiate over four issues. The context for the negotiation, we thought, was very interesting and engaging—namely, a negotiation between two distinguished honor student clubs over space, time, and other issues on which they needed to coordinate. We piloted the task in the

United States, and it was a smashing success. Students reported that it was very motivating and enjoyable. When piloting in Japan, however, we found that the task completely flopped—the students reported being uncomfortable and unmotivated with the issues that in their cultural eyes seemed very atypical. It became clear from discussions that this was a vertical individualistic task and produced negative reactions in Japan. Unbelievably, the very same question that we were interested in (cultural differences in how much people like to stand out and their implications for negotiation in different cultures) became embedded in our research tasks! We went back to the drawing board and developed a new buyer and seller task that proved to be much more motivating and relevant in both contexts (Gelfand, Higgins, et al., 2002). Without this extensive piloting, we would have missed the fact that even though it would have been possible to translate the original honor's club task, and have it be equally comprehensible in both cultures, participants would not have been equally motivated to engage in the simulation in Japan, and our results would be very difficult to interpret.

In other work we (Gelfand, Brett, et al., 2011) have done, pilots and focus groups have often revealed that the methods that we export abroad are far too decontextualized to be understandable and motivating in other cultures. In our interview questions, for example, we initially asked questions such as “Is compromise good or bad?” and later adapted the questions to have tags to reflect the *circumstances* under which it is good or bad. In our interviews, respondents in general in the ME needed to know with whom, about what issues, and in what circumstances we were asking about compromise, negotiation, *wasta*, revenge, forgiveness, apologies, among other constructs. We rarely found that our American counterparts asked for this information. In experiments we have likewise found it important to include much more information in the case materials for the instructions and study materials to make any sense. For example, in our case *At Your Service* (Brett & Gelfand, 2008), we had individuals negotiate either as part of a team or as part of a dyad in the United States and Taiwan. The case involved a relationship between two individuals who were either trying to form a deal to own a restaurant together (i.e., a deal-making context) or were trying to dissolve a relationship that had been subject to many problems (i.e., a disputing context). When we first wrote the case, we included details only about the issues to be resolved. Yet pilot studies in Taiwan revealed that they could not negotiate the case without more information about the people, their histories, their relationship, and so on. Put simply, it was too low context for a high-context culture (Hall, 1976).

Other Western assumptions also sneak into researchers' methods. For example, in the time-as-money Western culture, researchers often assume that participants can read the materials, get into roles, answer questions, and negotiate a case in a remarkably short time. In the same Taiwan–U.S. study (Brett &

Gelfand, 2008) discussed above, we ran the study in 90 minutes, but we found that more than 50% of the Taiwanese participants failed to reach agreement in that time. We found some very interesting results, but to trust the findings, we had to rerun the entire study to ensure that our results were not a methodological artifact of not having enough time to negotiate. Ultimately, the results of our follow-up experiment were identical, giving us confidence in the theory. Nevertheless, in this case, it was clear to us that the same amount of time that is needed in one culture to complete a study might be highly problematic in another.

In summary, the above descriptions and examples make clear that it is important to choose a task collaboratively with all local researchers that will ensure equal familiarity, comprehension, motivation, and ethicality in the study and illuminate any potential problems in the implementation of the task. Either focus groups or pilot analyses should be held in each culture, and the results of these preliminary analyses should be used to make substantive changes in the protocols. In the pilot test or focus groups, I have found it useful to employ comprehension and motivation checks (Berry, 1980) or use judgmental methods with which experts evaluate the stimuli (Segall et al., 1992).

Methodological Tradeoffs and the Importance of Triangulation

McGrath (1982) instilled in me the idea that all methods are flawed and each has strengths and weaknesses. In cross-cultural research, all methods have additional cultural baggage (Gelfand, Higgins, et al., 2002), and many rival hypotheses can threaten one's confidence in the interpretation of cultural differences that are found. Because of this, I have always found that to the greatest extent possible, it is important to see one's theory replicated with more than one method—that is, to see that the results triangulate.

For example, questionnaires or surveys have a number of advantages—they may be less intrusive than other methods (e.g., laboratory experiments, discussed below) and provide the ability to collect data on a wide range of questions at any one time. Cross-cultural challenges to surveys abound, however, including potential differences in motivation, understanding of instructions, validity, reliability, and response sets, making it important to replicate findings with another method. In our study of cultural tightness–looseness (Gelfand, Raver, et al., 2011), we measured the strength of norms and degree of sanctioning across 33 nations with Likert survey measures (e.g., “There are many social norms that people are supposed to abide by in this country”; “In this country, if someone acts in an inappropriate way, others will strongly disapprove”; “People in this country almost always comply with social norms”). Because survey measures can be subject to response sets and a lack of equivalence, it is important to perform procrustes factor analysis in all cultures to examine

the structure of the measure and also check to see whether standardization is required. In addition to these issues, differential motivation to respond (e.g., social desirability) and potential differences in the interpretation of the items also make it critical to gather additional data from other sources in order to provide convergent validity for one's survey measure. For example, we have more confidence in the Likert scale, given that tightness–looseness scores were strongly correlated with nonsurvey measures, including expert ratings on the construct, higher monitoring in society (more police per capita), and more severe punishments (e.g., the death penalty) for crime. The questionnaire measure was also correlated with unobtrusive measures, including the percentage of people who write with their left hand (a very visible indication of being “deviant” from norms) and with greater accuracy of public clocks in cities, indicating a greater concern with order and uniformity. By showing that our tightness–looseness scores were correlated with other indicators (Gelfand, Raver, et al., 2011), rival hypotheses due to response sets or differences in meaning of items, among many other issues, were reduced. In another study that relied on surveys of perceptions of conflict episodes in which we used multidimensional scaling to examine the dimensions on which people perceive conflicts in the United States and Japan (Gelfand et al., 2001), we coupled this method with analyses of newspaper accounts of conflicts in the *New York Times* and Japanese *Yamiuri*. Both analyses illustrated that although cooperation versus competition (or win–lose) frames were universal, U.S. conflicts were perceived to be much more competitive (win oriented) than those in Japan. In this case, cultural documents such as newspapers can help to provide additional confidence that one's theory generalizes beyond one method.

As another example, experiments offer numerous advantages in that they provide a controlled research environment and allow for greater inferences in causal relationships. Laboratory research is also beneficial in that it enables one to assess implicit attitudes in addition to explicit self-reported attitudes. Yet laboratory research, particularly in cross-cultural settings, can present many problems that can reduce confidence in the interpretability of the results. As noted earlier, it is critical that laboratory tasks and procedures are equally understood by and motivating to individuals across different cultures. The very artificiality of the laboratory environment (and the role-playing manipulations that I often use) can be unfamiliar to people outside of the West, making it critical to replicate one's theories with another method. In our research on accountability using laboratory experiments (Gelfand & Realo, 1999) or self-serving biases (Gelfand, Higgins, et al., 2002), for example, it was important to see that our effects were also replicated using scenario-based measures.

Interviews and other qualitative methods are another useful method in cross-cultural research in that they enable one to gain depth on a research question and often afford more of an understanding of emic perspectives that

are especially useful in early stages of cross-cultural research. Interviews are also essential when dealing with illiterate populations. Many difficulties and judgment calls arise, however, when implementing this method. For example, characteristics of the interviewer need to be carefully chosen for their cultural appropriateness (e.g., in the ME, using female interviewers for female samples). The ways in which interviewers gain trust, develop rapport, and probe the participants to answer questions can be very different in different cultures, making it important to try to negotiate a standardized interview process with one's collaborators before data collection. For example, in some cultures, revealing information about oneself is critical for the development of trust, yet in other cultures this would be seen as inappropriate, and a practice is perceived as threatening the objectivity of the data collection. Before our interviews in the United States and ME, we searched the literature for best practices in interviewing (it was not surprising that much of this was derived from interviews with Western samples), and with input from our collaborators, we designed an interview manual, which was discussed, revised, and finalized on the basis of the entire team's input. Other aspects of the interview process, such as tape or video recording, can have very different implications depending on the culture. For example, recording interviews could be highly sensitive in some cultures—particularly in very tight cultures, in which there is a concern about authorities hearing one's responses (e.g., my experience in Iran), making additional safeguards and assurances necessary. For example, in recent work on community negotiations in the United States and Egypt, I was not able to videotape the negotiations, as Egyptians would have considered this too invasive.

Many issues arise in determining how to extract, code, and interpret interview data. For example, in my project, I needed to first transcribe all interviews (which were between 1 and 2 hours per participant) from their native language from audio files to actual text in Arabic, Turkish, Urdu, and English. Each interview produced an average of 15 to 20 pages of actual text. Because I wanted to code the data in a standardized way, I needed to develop a method to reliably extract the answers. It took approximately 6 months to develop a process in which I felt confidence. I first developed a standardized manual for all team members to discuss. For each interview question, the ME and U.S. teams first completed extractions (i.e., the answers to questions) on two to three designated transcripts for reliability purposes. I took the most difficult interviews (those with the most variance in terms of where the answers could be found, some being found right after the questions were asked and others found later in the interview as well). I computed reliability across U.S. and ME collaborators, and after several iterations and resolving several disagreements (and updating our manual), I was able to have trust in the reliability of the process.

This process was far from completed. After all the data were extracted (approximately 1 year later for all questions), I needed to figure out what to do with the extracted data. I again sought to develop a standardized code development process that all collaborators could agree on and that was reliable and valid. I was very concerned at this stage with not viewing the data through our Western glasses and allowing emic concepts and themes to be identified in each country. The process that I created ultimately involved three phases. In the first phase, ME and U.S. teams separately examined the extracted answers for a particular interview question and constructed a list of possible codes or themes for their respective countries. For the construct of honor, for example, 1,769 codes, or an average of 103 codes per question, were generated across the teams! The second phase involved sorting and organizing these codes at a conceptual level by the U.S. team with input from our ME collaborators, as well as with input from extant research. Finally, the third phase involved writing a coding manual that described the code categories in detail and set forth procedures and guidelines for coding. Coding then was done with bilingual individuals with reliability checks. This process was implemented for all questions in the interview protocols. It is not surprising that these steps take a lot of time and resources. And as with other methods, it is important to triangulate interview findings with other methods to gain confidence in the results. For example, much of the interview analyses illustrated that honor and face loss become much more contagious across individuals in the ME than the United States, generally speaking. I am currently in the process of replicating these results with free recall methods, laboratory experiments, and computational models.

Finally, archival databases also have notable strengths in cross-cultural research in that they provide another unobtrusive source of cross-cultural data. Examples include ethnographies, which provide in-depth information about a given culture, and cross-cultural databases on ecological, sociological, economic, or political variables. These sources, however, also have notable weaknesses. Preexisting databases may only be available for a limited number of countries or variables. In addition, databases may label or assess constructs differently across cultures, and as such, comparisons are problematic. In addition, without a developed theory, the use of such sources can result in dustbowl empiricism. I have found it important to try to find convergent evidence when possible to help bolster my confidence in my own research using archival methods. For example, in the study of tightness-looseness (Gelfand, Raver, et al., 2011), I wanted to test the notion that ecological threats (e.g., lack of natural resources) and man-made threats (e.g., population density, human disease) are related to tightness. To assess the extent to which nations are subject to resource scarcity, I collected data on arable land, food production, food supply, and food deprivation from the Food and Agriculture Organization of the United Nations, as well as the percentage of farmland and access to safe water in each country from

Kurian's (2001) world ranking. Data on population density were taken from the *Atlas of World Population History* in the year 1500 but also from the United Nations in 2000. To assess threats due to human disease, I located a number of different sources to triangulate the results. The index of historical prevalence of pathogens was taken from Murray and Schaller's (2010) research, in which they constructed the disease prevalence index based on early epidemiological atlases. The World Health Organization (WHO) provided data for years of life lost to communicable disease and prevalence of tuberculosis—a highly communicable disease. Mortality rates for children under 5 were also gathered from the United Nations. By finding the theoretically expected results with multiple sources, one can have more confidence in the results of archival analyses. In addition, as with other methods, it is comforting to replicate one's results with a completely different method. For example, in recent research, I have been priming ecological conditions (e.g., population density) in the laboratory to examine the theory of tightness–looseness.

In summary, all methods clearly have strengths and weaknesses, and all are very useful, particularly in combination, when doing cross-cultural research. Each method varies considerably in its capacity for gaining depth about the phenomenon (interviews), control and causal inferences (laboratory experiments), unobtrusive nonreactive measures (observations, content analyses), and the ability to have standardized, structured responses (surveys). Above all, each method presents problems for interpreting results across cultures, rendering it essential to replicate with a complementary method when possible.

CONCLUSION

As this chapter has, I hope, illustrated, cross-cultural research is a passion for me and a lifelong journey that brings many joys. It also presents many challenges at all stages of the research process that require many difficult judgment calls that do not necessarily have one right or wrong solution. To cope with the long and bumpy road, I have always relied on the use of theory, much cultural legwork, the wisdom of my collaborators, the triangulation of methods, and patience. Textbooks and journal articles on methodological issues in cross-cultural research can also provide very useful technical advice. It is also instructive, however, to share the stories, dilemmas, and serendipity that are behind the scenes of research lives to more vividly illustrate the issues that are invariably encountered when venturing into cross-cultural research territory. This is both empowering, as many researchers experienced at the Michigan State conference, "Conducting Multinational Research Projects in Organizational Psychology: Challenges and Opportunities," and critical for building institutional knowledge in an ever growing field.

Best Practices Recommendations

- Always have strong theory guiding cross-cultural research. Theory is needed for the construct itself, its relationships, sampling, design, and measures.
- Involve local collaborators in every step of the research process. They know much more than you do!
- Assume there will be considerable cultural legwork in building a multicultural team before data collection.
- Conduct multiple pilots before doing your research, and expect that this cultural input will change your plans. Culture is in the method too!
- Identify rival hypotheses for results before conducting research. Measure them, control for them, or both.
- Use multiple methods to triangulate your findings.
- Attend to equivalence in measurement and response biases when examining your results.
- Be passionate, but don't take yourself too seriously! (Advice from Harry Triandis.)

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