THE RESEARCH PROCESS

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In writing this chapter I was surprised at the difference between how work is described publicly and how it actually progresses. In a public seminar or in a request for tenure or promotion, progress appears to follow a logical and consistent path. Such presentations, by their nature, avoid describing the meandering and lurching about that actually takes place. I suspect, however, that a straight line typically cannot be drawn between the past and the present. In my experience, research develops along many paths simultaneously. Projects unfold, growing like vines: Some branches stall or are dead ends, while the main branch continues, perhaps with punctuated progress.

In this chapter I describe work I have done, first with a topical and chronological overview, then with more background on a few projects to give a little of the flavor of how things actually progressed. In the overview I describe research that led up to the Cultural Consensus Model, a theoretical and analytical framework that facilitates the description of cultural beliefs. Then, I describe a project examining illness behavior in Guatemala, where research did not always go according to plan. I also present two research projects where findings were accidental: In the first, we discovered multiple linguistic taxonomies, and in the second, I discovered that condoms may not be as effective in reducing sexually transmitted HIV as commonly assumed.

**OVERVIEW**

My research has focused primarily on the measurement of attitudes and beliefs. Much of my work is descriptive in nature, emphasizing the importance of minimizing researcher bias and prior assumptions when describing the beliefs of others. I have conducted comparative studies between different cultural groups on illness concepts, adolescents’ beliefs about appropriateness of disciplinary punishments, and mothers’ reasons for choosing breast- or bottle-feeding. This work has resulted in the development of a set of procedures, the Cultural Consensus Model, to estimate typical belief patterns and to assess reliability and validity of results.

My work began with a series of papers on disease concepts. In my first study, I examined variation in beliefs on the concepts of contagion, severity, age, and a humoral concept of hot and cold.
medicine in an urban U.S. and in an urban Guatemalan setting. The results showed a strong similarity between U.S. and Guatemalan concepts of illness. In a second study, I focused on the humoral concept of hot and cold medicine in rural and urban settings in Guatemala. In the hot-cold concept of illness, illness is believed to be the result of an imbalance between hot and cold elements in the environment, in the diet, and in the body. The hot-cold theory did not appear to be as important as previous investigators had reported.

These initial studies came from my dissertation. I was excited about new methods I was learning and wanted to apply them to problems in medical anthropology. My work built upon that of Fabrega, D’Andrade et al, and Young in medical anthropology, and extended work on intracultural variation (especially by Romney and colleagues) by applying new techniques. An important aspect of these studies was the inclusion of comparison groups. Only by using two or more illness concepts per site, or two or more samples of informants, can you judge if the pattern you observe is unusual. In the urban Guatemalan sample, the concepts of contagion and severity showed no meaningful intracultural variation in beliefs and thus were highly salient. In contrast, the hot-cold concept showed no meaningful agreement. Additional comparisons were provided by repeating the study in different sites, for example, urban versus rural Guatemala.

My first job took me twenty miles from the department where I got my Ph.D., but much farther in terms of the department I joined. I got my Ph.D. at the University of California, Irvine (in Irvine) and then joined the Department of Pediatrics at the University of California Irvine Medical Center (in Orange). I was surprised to find how much general pediatricians and social scientists had in common in terms of their substantive interests and I began collaborating with my physician colleagues. C. I. Dungy and I studied women’s preferences for breast- or bottle-feeding and D. P. Orr and I studied adolescents’ beliefs about disciplinary punishments. These projects developed from their interests in infant feeding methods and child abuse and my interest in intracultural variation.

My next job, at the University of Pennsylvania, had an even more profound impact on the direction of my research. Again, my main faculty appointment was in the School of Medicine in the Departments of Pediatrics and Internal Medicine (Clinical
Epidemiology Unit). It was at Penn that I consciously decided to pursue two distinct lines of research. The first area of research represents collaborative ties within the medical school. Physicians work far more collaboratively and collaterally than we do in anthropology. Team membership and participation is essential. Thus, a body of my work reflects these ties. My role took different forms; sometimes even a simple statistical consultation could take a project in a new direction. I also began working with students and medical fellows.

The second line of research was the continuation of my work in anthropology. It is probably fair to say that this work was little understood and little appreciated in the School of Medicine. At the time, Kim Romney and I were hot on the trail of the Cultural Consensus Model. This was the period when results from several areas converged (my work on illness concepts, breastfeeding and discipline; Kim Romney’s previous work; and Nunnally’s discussion of reliability theory).

We observed that agreement among respondents was related to shared knowledge, and developed a set of procedures to estimate typical beliefs. At this stage, we were using a simple aggregation of responses to represent the cultural beliefs and comparing each informant’s responses to the aggregation in order to estimate the “cultural knowledge” of individuals. We first tested our idea on the Bernard, Killworth, and Sailer social interaction data and found that informant accuracy could be estimated solely on the basis of reported data.10 In another study, we used the same procedure to model adolescents’ beliefs about discipline and found that students deviating from “consensual beliefs” were four times more likely to experience corporal punishments at home.11

Although the basic tenets of the Cultural Consensus Model were outlined in our first article, Batchelder joined us and formalized the relationships mathematically. Together, we presented a formal model with mathematical derivations and empirical examples.12 The formal model describes the process, at an individual level, of how questions are answered and how identical responses result from shared cultural knowledge. Later, we developed procedures to accommodate different types of response data.13 (A highly technical review of the formal model has also been published.)14

The core problem that the Consensus Model addresses is the appropriateness of aggregating responses across informants in
order to estimate cultural beliefs. If each cultural member has different beliefs, that is, there is no single set of group beliefs, then informant responses should not be aggregated. If, on the other hand, there is concordance among informants, then it is appropriate to combine informant responses and describe the group’s beliefs. The Cultural Consensus Model provides a means for determining if a single, shared belief system exists. If a single belief system is present, then the model provides a way to optimally aggregate responses across informants to estimate the culturally correct answers. The model also provides an estimate of how much each informant knows and shows that small sample sizes are adequate when agreement among respondents is high.

Throughout my work is a concern for ways to improve reliability and validity of data. My initial dissertation work focused on comparisons among individuals, across different aspects of illness, and between different groups of informants to try to describe more accurately what informants think about illness. My collaborative work wound its way through a variety of topics, but retained a continued emphasis on intracultural variation. Changes in jobs were beneficial and exposed me to new people, new problems, and more importantly, new skills.

HEALTH BEHAVIOR
AND MALARIA IN GUATEMALA

During the mid- to late 1980s, I became involved in a project on health care and health care decision-making in Guatemala. While the main project explored “Knowledge, Attitudes, and Treatment-Seeking Behavior for Malaria,” a wide variety of data sets were established, allowing for spin-off projects. Projects included studies of health care actions, folk illnesses, and desirable qualities of village health workers. I include this project because the grant proposal offers a written record of the initial purpose and direction of the project and history shows what we actually did. This example illustrates how the project was conceived, what its original intent was, and how things did not always go according to plan.
In 1983 Trenton Ruebush II of the United States’ Centers for Disease Control, in collaboration with the National Malaria Service of Guatemala, began a large-scale study to evaluate village health workers’ effectiveness in detecting and treating malaria cases. In Guatemala, village health workers treat only malaria; they take blood smears to test for the presence of malaria parasites and administer antimalarial medications to those with malaria or with malaria symptoms. Two separate studies were carried out in six regions. In the first study, the records of the health workers were analyzed to see how many residents utilized them and how many cases of malaria were detected. In the second study, residents in the six regions were surveyed and blood samples were taken to estimate the true prevalence of malaria and utilization of different treatment sources. Results were surprising. Obtaining treatment from a village health worker is free, yet only 20 percent of those with symptoms suggestive of malaria or those who thought they had malaria sought treatment from one. Instead, the majority of villagers used self-treatment strategies spending a day’s wages for a non-curative dose of antimalarials!

The results appeared to be, at a minimum, economically irrational. The Pacific Coast of Guatemala is an agricultural region with cotton and sugarcane plantations. Work is hard and wages are low. Men work six days a week in the fields, doing such activities as clearing sugarcane with machetes, for about two dollars a day. Women tend to work at home shelling corn, carrying firewood, and preparing food. Most families do not have excess financial resources to deal with emergencies, and illnesses that interfere with economic productivity are costly. Why were residents paying a day’s wages for a single shot of antimalarials, when they could get an entire course of pills at no cost from a village health worker?

This is how projects start. They start as questions: Why is this happening? Could we better understand how this works? Trent Ruebush contacted me and others to talk about what might be going on. We met in the fall of 1984 and discussed his findings. Trent is a physician-epidemiologist and the two studies were extremely well done. The confusing results were not due to prob-
lems with the study design or the survey. Together we designed a new project to understand the cultural factors affecting treatment-seeking behavior.

The new project was divided into two phases. The first phase focused on illness in general. What illnesses are there? How are they treated? What sources of treatment or care are there? We planned to model decision-making with vignettes of hypothetical illness scenarios and validate (test) the model with illness case histories. Both retrospective and prospective illness cases were to be collected. In the second phase of the project, efforts would focus directly on malaria. Open-ended interviews would elicit such information as illnesses similar to or confused with malaria, and the symptoms and causes of each; treatments for malaria; and sources of treatments for malaria. Again, treatment-seeking behavior would be modeled and validated using vignettes and malaria case histories.

Decision-Making: Vignettes

We began by examining illness in general. Open-ended, structured interviews were used to obtain information about illnesses, their symptoms, causes, and treatments. We also examined the factors predictive of treatment actions. Vignettes were used to study the effects of specific factors on treatment choices, with hypothetical illness cases. Illness case histories were collected from 250 households randomly sampled from six villages. The vignettes did not work as well as we had hoped, as the model of treatment choice based on vignette responses did not predict treatment actions very well. We also had trouble making predictions based on the illness histories themselves, because the predominant treatment strategy was “self-treatment.”

Vignettes or hypothetical scenarios have been used by researchers to study respondents’ perceptions and/or decision-making while systematically varying specific factors in the scenarios. For example, factors like cost, waiting time, and travel time can be used to study consumer’s preferences for health care availability. Scenarios were created from the different categories in each variable, for example, low cost, long wait, and nearby care versus high cost, short wait, but far away. My first attempt with
vignettes was to try a decision-analytic approach to understand the relative importance of factors affecting rural Guatemalan health care choices. Essentially, we tried to replicate the findings of Young. In a study in Mexico, Young reported being able to correctly classify about 90 percent of treatment actions.

We began with open-ended questions, eliciting different sources for health care. We also asked contrasting questions, comparing pairs of sources of health care, to elicit reasons for selecting one source over another. Our findings, at this stage, paralleled those of Young, namely that severity, financial resources, and prior experience/knowledge seemed most important in choosing a course of treatment action.

We then created vignettes from all possible combinations of those three factors: for example, a serious illness in a family with available financial resources, and prior experience with the illness. With three factors, and two levels (presence/absence) on each, eight vignettes can be created. Informants were presented with each of the eight scenarios and asked, “What would you do?” “Where would you go?” “What would you do if that didn’t work?”

Respondents seemed to have some difficulty in grasping the abstract scenarios. Although the data appeared interpretable (and sensible), we had the feeling that if we questioned their responses (for example by repeating the answer, “You’d go to the doctor?”), that they would change their answers. (Of course, interviewers’ judgements about the reliability of informants’ responses have been shown to be the least reliable of all data!)

We examined the relationship between the factors that were varied in each vignette and the reported health care choices. A hierarchy of rules was drafted to predict individual treatment choices. The rules (the model) were then tested by comparing predicted actions with the actions taken in the illness case histories. House-hold economic resources, severity of the illness, and prior experience (expressed as six classification rules) predicted behavior only 7 percent better than chance did! Not very good!

The following year, we focused more specifically on health care choices for malaria. We tried using vignettes again. This time we had a better understanding of health care choices and designed more “concrete” vignettes. We learned from our previous experience that the categories must be well defined. For example, from the collection of illness case histories we found that the average cost of a doctor visit (including medications) is
equivalent to a week’s (six days) wages, so we made the presence of financial resources equivalent to that (Q20.00). We also found we needed to clarify the age of the children and chose to portray them as about seven years old. Verbal descriptions of illness treatment had “natural” break points so we chose those for the duration of symptoms. Symptoms of about a day are generally treated at home, while a different strategy may be considered for those that persist for three days. This time, we portrayed the vignettes with visual aids.

While we have not yet tested the new model against the malaria cases, it is clear that there are several problems with using vignettes to study decision-making. First, factors used in the scenarios must be clearly defined. Second, operationalizing those factors for the purpose of prediction must also be clearly defined. For example, how do you judge if an illness case is “serious” or not? In Young’s study, he classified cases by his own judgement. In our study we used the informants’ own judgement (when we collected the illness case histories, we asked them whether or not the illness episode was “serious”). Third, what is an “illness case”? We defined a case as an illness episode in a single individual. Young counted each step in treatment as a new case, in essence double- and triple-counting individuals. Finally, it is still unclear if Young’s results were due to a methodological artifact or if our results were due to our own bungling.

Treatment for Malaria

We also wanted to understand local perceptions of different brands of antimalarials, their mode of administration (pill or shot), and appropriate doses. One of the reasons we had started the project in the first place was to understand why villagers tended to purchase (with a day’s wages) a single shot of antimalarials, when they could get a curative dose (in pills) from a village health worker for free. This case illustrates how a well thought-out design sometimes must be modified or abandoned, because it does not work in the field.

We devised a task to explore perceptions about effectiveness. We used two brands of antimalarials: ARALEN® in colorful, sealed cellophane packages with one pill per package and SNEM tablets, the national malaria service tablets, which are
unwrapped, plain, white pills. We compared the two modes: pills and ampules, like those available in pharmacies for injections. Also, since the recommended dose is four tablets (or four ampules), we compared different numbers of pills and ampules. Items were compared, two at a time, and informants were asked which was more effective. For example, we compared (a) two ARALEN tablets with three ARALEN tablets; (b) three ARALEN tablets with three SNEM tablets; and so on.

Choices within brand-type like that in (a) indicated that more pills were considered to be more effective (four > three > two > one). When equivalent doses of different brands were compared, the store-bought drug (Aralen) was perceived to be slightly more effective. As we proceeded, responses followed in a transitive and reasonable progression (like Piagetian tasks), until we started comparing ampules. The pharmaceutical dose in an ampule is the equivalent of a tablet. Thus, if a proper dose of tablets is four pills, four ampules/shots would be needed. However, one ampule was like an ace; it beat one, two, three, and four tablets! When we tried to compare two or more ampules to the tablets, we were told no one would take such a dose! It was too strong!

We abandoned our initial strategy and instead put about a dozen tablets in a little pile next to an ampule and asked, how many pills it would take to produce the same effect as one ampule. We found that most informants thought an ampule was the equivalent of about four tablets. Which is, of course, the dose strategy that they were using. Informants did not know that an ampule was the equivalent of only one tablet. Or perhaps, they thought that the dose from an injection was stronger because the drug was delivered “directly into the blood.”

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Mosquito Bed Nets: Validating Interview Data

An important part of research concerns things that go “wrong” in the field. Things that go wrong can be simple things, like asking someone for directions and getting lost. “Errors” or “failures” can teach you a lot about the topic you are studying and about yourself. Simple errors like getting lost provide a valuable lesson in informant reliability. You won’t have to get lost too many times to learn that you had better ask more than one person for direc-
Similarly, it is important in a field situation, if possible, to verify informants’ responses. In a study of mosquito bed nets, interviews were conducted about the care and use of nets in Northern Guatemala. To check the accuracy of the interview data, a subset of households were selected and observations were performed at different times throughout the day. We began visiting in the late afternoon; all nets were folded up and not in use. We then waited until dark when people were retiring, and sampled more households. It felt intrusive and in fact, one household didn’t answer the door and told us, in not very polite terms, to get lost.

Bored, disappointed, and embarrassed, we wandered over to see how the SNEM (National Malaria Service) team was doing sampling mosquitoes. Some households had been selected for a detailed study of the effectiveness of insecticide-treated bed nets and the entire house was tented with an additional net to capture mosquitoes leaving the house. (The treated nets not only kept mosquitoes away from the person sleeping under the net, but also were supposed to kill mosquitoes that came in contact with the net.) Inside the house, we could see that all the nets were hung correctly, covering the beds. No one was in the beds, which was not a surprise, since two or three men from the SNEM staff were outside the house, but inside the tent, capturing mosquitoes. However, there was an infant sleeping in a hammock, completely exposed. Infants are most vulnerable to malaria, since they have had no prior infection and thus have less tolerance for the parasites.

Finding the infant sleeping in a hammock offers an important lesson on verification. Although some field tasks are uncomfortable, they may be necessary to obtain valid data. If questions had focused solely on “beds” we might have missed hammocks. Furthermore, questions needed to specifically address each family member. Time of day is also an important factor in malaria transmission, because although mosquitoes are out and biting from dusk on, there is a higher proportion of infected mosquitoes biting from midnight until dawn. Thus, we asked a detailed series of questions about family activities, including who all the household members were, where they slept, and what times they went to bed and woke up.
Reinterpretation of Existing Data: Serendipity

Sometimes chance or serendipity plays a role in the direction research takes. Accidental discoveries take many forms, but in general, you must be willing to see the aberrant observation and take advantage of the opportunity to explore it. In the discovery of penicillin and X-rays, several researchers noted “interference” with their main observations, but dismissed the aberrant observations as a nuisance. Sometimes things occur that are not on your main agenda, but prove fruitful when followed up.

Linguistic Taxonomies: Multiple Solutions

While at Penn I had the pleasure of working with an undergraduate student from the engineering school, Charles (Chip) Buchholtz. Kronenfeld and Thomas had recently published a reanalysis of the Salishan language data (originally from Swadesh) using hierarchical clustering. And, as part of an independent study project, Chip was programming some of the clustering algorithms in PASCAL for the IBM-PC for me. Clustering, and specifically hierarchical clustering, is a kind of statistical analysis that estimates taxonomic relations among items (for example, the evolutionary taxonomy of relations among different kinds of animals). Up to that point the programs were only available on the large, mainframe computers. In particular, I wanted single-, complete-, and median-link hierarchical clustering programs. Chip and I reviewed the original papers on these methods, and he began to work.

He came in one day and asked me, “What do I do with ties?”
I said, “What ties?”
“Well, it is possible that an item cannot be clearly classified into one group because it ties with two groups. What do I do with those items?”

Who knew about ties? The current computer programs certainly didn’t warn one about ties. The implication of this finding was that clustering solutions may or may not be unique. A non-unique
solution means that there may be several possible solutions. A careful rereading of some of the classification experts indicated that they knew that solutions could be non-unique (that some methods resulted in a “class of solutions”), but none of the existing computer algorithms indicated anything was amiss. Clearly the computer algorithms were classifying ambiguous items arbitrarily.

Chip and I talked and decided (a) to have our program, when it found a tie, warn the user that there was more than one solution, and (b) to have another program that could reorder items in order to try and find the other solutions. The resulting program is “4M—A Program to do Minimum-, Maximum-, Mean-, and Median-link Clustering.” I think the program is still one of the best available.

Our original intent was to reanalyze the Salish Language data, which we did. Low and behold there was not one solution, as the results of Kronenfeld and Thomas had suggested, but instead there were three taxonomies using the same algorithm they had used! The multiple taxonomies varied in their placement of the Puget Sound dialects. We published our findings in the *American Anthropologist*.21

A similar problem has plagued genetic anthropologists, but on a much larger scale. From mitochondrial DNA, similarity in sequencing patterns can be used to create taxonomies of human groups. From the taxonomic models, anthropologists have hypothesized the source of human origin and migration patterns. Wilson and Cann estimated from a taxonomic model that a single “Eve” existed about two hundred thousand years ago in Africa.22 Recent reanalyses of the mitochondrial data indicate that there are thousands of possible taxonomies of human groups.23

Condom Effectiveness: Just Because It’s in Print Doesn’t Mean It’s True

The value of careful scholarship cannot be overestimated. Sometimes profound discoveries can be made in your library! Not long after I arrived in Galveston, Texas, I considered doing a project on adolescent perceptions and use of condoms. The federal government (NICHHD) had issued a call for grant applications.
Galveston is fairly unique in that it has one main high school, which is about one-third Anglo, one-third Hispanic, and one-third Black. With the spread of human immunodeficiency virus (HIV) and auto-immune deficiency syndrome (AIDS) such a study could have social significance and perhaps precede interventions aimed at increasing use of condoms. And, Galveston offered relatively easy access to a multiethnic sample, including the higher risk minority groups. As I was thinking about whether I wanted to do such a study, and, if so, how I would do it, I began reading the literature.

I began with the contraceptive literature: effectiveness at preventing pregnancy, adolescent pregnancies and use of contraception, and barrier method effectiveness at preventing sexually transmitted diseases (STDs). The effectiveness of different methods for contraception has been well studied: The typical failure rate for condoms is about 12 to 15 percent and the lowest reported failure rate is 4.5 percent. Thus, condoms are considered to be approximately 87 percent effective for contraception. The literature regarding method efficacy at reducing STDs was not as clear.

In 1989 I began checking the citations offered by researchers as “proof” that condoms were effective in reducing the risk of HIV. I couldn’t believe it. People were citing “Letters to the Editor” and not even scientific studies! One letter told of a man who was about to go off on vacation and was interested in how much protection a condom would provide him from STDs, so he and his buddy looked at one condom under an electron microscope! He concluded he would be safe from herpes and thus, probably from HIV. Some researchers examined only two or three condoms! I thought, this can’t be right. This can’t be the proof. There must be something somewhere that shows condoms are effective.

I also looked at the results from human studies. Data from studies of prostitutes or “commercial sex workers” were also being cited as evidence that condoms were effective. However, there is a severe flaw in using these studies as evidence. Specifically, studies based on individuals with multiple sexual partners cannot tell if all the individuals have been exposed to the virus. That is, if prostitutes who use condoms have a low rate of HIV, you cannot tell if the low rate is due to condom use or due to the fact that they have not had sexual relations with someone with HIV. Even worse, careful examination of some of the prostitute data showed a higher rate of HIV positivity with condom use.
The best test of condom effectiveness comes from the unfortunate but “natural” experiment where one individual is infected with HIV and their regular monogamous sexual partner initially is not; where some couples use condoms and some do not; and HIV status is determined by a blood test. Studies meeting this requirement were available for partners of HIV-positive hemophiliacs, recipients of HIV-contaminated transfusions, and intravenous drug users. I located thirteen studies (published prior to July 1990) that met these requirements. Failure rates in the individual studies ranged from 2 percent to 62 percent with only three studies showing results significantly different from a failure rate of 100 percent. By combining data from a series of studies, an estimate can be made that may be more accurate than an estimate based on any single study. Such a meta-analysis indicated that the effectiveness of condoms at reducing heterosexually transmitted HIV was 69 percent.25

Truth is not always valued in science. The condom study was rejected by some of the best journals in medicine and public health. Five journals rejected it over a period of about three years. The original manuscript, however, was unwieldy and detailed all evidence of condom effectiveness at reducing HIV transmission (studies of in vitro, in vivo, and mathematical models). The published version focused on the human (in vivo) studies. Social Science and Medicine published the paper at the same time as the AIDS international meeting in Berlin. My university issued a press release on Monday, June 8. Tuesday, at 7 A.M., I was awakened by a call: “This is NBC in Washington, DC. May we interview you?”

Summary

Research ideas come from a variety of sources and are most often extensions of the work of others. A research project may be an extension of the work of a faculty advisor. Or you may note, by careful reading of an article, a logical or methodological flaw and wish to try a similar, modified study. Or, you may wish to apply a new technique to an old problem. Rarely does an idea occur that is unique and independent, causing a giant leap forward in a field.

Research can also be initiated by reanalyzing existing data. Reanalysis of existing data has many advantages. By walking
through a previous investigator’s steps, a better understanding and sometimes new insight can be gained about how to better study the problem. Often, you will be surprised to find gaping holes in the solution and in knowledge. You may even reach a different conclusion about what the data mean. Reanalysis of existing data also offers an easy way to test new ideas. Of course, an important part of reinterpreting existing data is that the data exist and are available.

Serendipity plays a big part in discoveries. Digressions from the main path in a research project can give birth to a whole new enterprise. My newest and current project is a spin-off of the malaria project in Guatemala. In analyzing the illness case histories, I noticed that there were several cases of the folk illness empacho. Empacho is an illness believed to be caused by food that has somehow become “stuck” in the digestive tract, causing gastrointestinal symptoms. We wrote an article describing empacho in Guatemala: its symptoms, causes, and treatments; and how often it occurs (it occurs as frequently as colds and flu).26 Because published descriptions of empacho confounded variability in beliefs with different methodological approaches, a new study was planned using the same research design to study the beliefs of four geographically dispersed groups of Hispanics. Data were obtained in Guatemala by me, in Guadalajara, Mexico by Roberta Baer, in Edinburg, Texas, on the Mexican border by Robert Trotter, II, and in Hartford, Connecticut by Lee Pachter.27 Baer, Pachter, Trotter, and I are now using the four-site study design to examine beliefs about other illnesses. Who knows where this project will lead, but that’s what makes research fun!

NOTES


SUGGESTED READINGS


