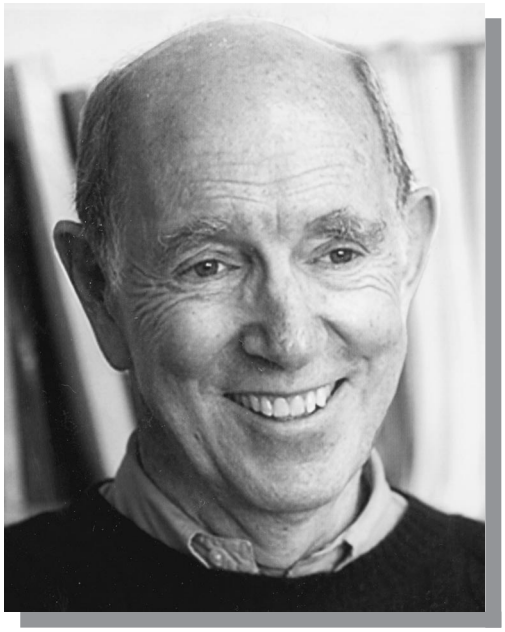

Michael Cole

Award for Distinguished Contributions to the International Advancement of Psychology

Citation

"For his significant and influential contributions to understanding the cultural and historical construction of human life and the implications of collective social practices for human development. Michael Cole is a preeminent international researcher and theorist whose leadership in the study of cognition and cultural context combines traditional approaches and findings of psychology and ethnographic methods. Through ingenious, cross-cultural, and ethnographically grounded analyses, he has shown how literacy, schooling, and development are related to learning in everyday life in and out of schools. He has also developed model educational activities designed to promote the intellectual and social development of children, particularly those who are poor, marginalized, and underserved across the United States, Mexico, Australia, and Russia."

Biography

Michael Cole began his college career at Oberlin College but completed his undergraduate work at the University of California, Los Angeles. At the suggestion of his advisors, Edward Carterette and Richard Atkinson, he attended graduate school at Indiana University, where he received a doctoral degree in mathematical learning theory under the direction of William Estes.

Cole's involvement in international psychology began as an unintended consequence of going to Indiana University, which had recently become the headquarters for a newly initiated academic exchange program with the (then) Soviet Union. He fulfilled his foreign language requirements in psychology by studying Russian, and after completing his doctoral degree in 1962, he spent a postdoctoral year in Moscow under the direction of Alexander Luria. His first international publications grew out of a collaboration with one of Luria's students, Nina Korzh (e.g., Cole & Korzh, 1966).

Shortly after returning from Moscow, he was invited to go to Liberia as a consultant to John Gay, a faculty member at a small Episcopal college, to investigate what appeared to be unusual difficulties that local children and youth experienced in school mathematics. Fascinated by the apparent contradiction between people's competence in everyday activities involving quantification and monetary exchange on the one hand, and their difficulty in dealing with various tests of simple quantitative abilities derived from common psychological tests on the other, he embarked on a decades-long investigation of the role of culture in cognitive development.

He and his colleagues soon discovered that if one begins analyzing questions such as those about the role of culture in the development of mathematical skills, it is most appropriate to start with tasks that are part of the everyday repertoire of the people involved. Hence, when asked to estimate amounts of rice in a bowl, Liberian rice farmers, children, and adults performed more adequately than residents of New Haven, Connecticut, and Yale undergraduates (Gay & Cole, 1967).

At the same time that Cole was beginning to become involved in questions of culture, he became editor of the translation journal, *Soviet Psychology,* and published a handbook of selected writings by Soviet psychologists (Cole & Maltzman, 1969). These two interests began to converge because Alexander Luria had conducted cross-cultural research in Central Asia in the 1930s that had never been properly analyzed and published. During the latter half of the 1960s, he visited and corresponded with Luria, seeking both to induce him to publish the results of his early cross-cultural research and to understand the broader theoretical perspective that had motivated Luria's research. He translated Luria's first article-length publication of this research (Luria, 1971) and wrote a preface to the translation of his book-length monograph on this research (Cole, 1976). Of equal importance, he became involved in editing and publishing a book of essays by Lev Vygotsky (Cole, John-Steiner, Scribner, & Souberman, 1978) and Luria's autobiography (Cole & Levitin, 2005). These projects gave him a much firmer grasp of Luria's overall theoretical approach to the role of culture in human mental life. Henceforth, his own work could be considered an attempt to blend the insights of the cultural–historical activity theory approach to psychology that Vygotsky, Luria, and their colleagues developed in the 1920s–1930s and

Vol. 61, No. 8, 902–917 DOI: 10.1037/0003-066X.61.8.902

his own cultural-context approach, which used everyday activity as the starting point for analysis of cultural contributions to learning and thinking and was based on a combination of his training as an experimental learning theorist and Anglo-American cultural anthropology (Cole, 1988, 1996; Cole, Gay, Glick, & Sharp, 1971; Scribner & Cole, 1981).

In the early 1970s, he had the opportunity to extend cross-cultural work conducted in Liberia to rural parts of the Yucatan in Mexico, where several decades of government policies had caused children growing up in different villages to be exposed to widely varying amounts of education. This situation allowed him and his colleagues to more effectively investigate the extent to which schooling was associated with changes in mental development. This work produced two major outcomes. First, it demonstrated that schooling, not age, was associated with improved performance on a wide variety of psychological tests used as indicators of cognitive development in American and European developmental–psychological research (Sharp, Cole, & Lave, 1979). Second, it deepened his and his colleagues' suspicion that the psychological tests imported from the industrialized world, because they were drawn from and mimicked the kinds of intellectual tasks that are fundamental to schooling, were entirely inappropriate for making school–nonschool comparisons. These suspicions led to several years of work on the ecological validity of standard psychological experimentation and attempts to determine the extent to which the contents and constraints embodied in psychological tests could be identified. They also led to studies on a variety of everyday activities, including those in school classrooms, the settings from which such tests had been abstracted in the first place (Cole, Dore, Hall, & Dowley, 1978; Newman, Griffin, & Cole, 1989).

During the 1980s, Cole and his colleagues began a program of research on the design and implementation of educational activities for children in nonschool settings. On the one hand, this work served as a test of the cultural–historical activity-based ideas that had grown up in their research—were the theoretical principles powerful enough to be used to design for children's development? On the other hand, they served as a medium for an entirely new kind of international collaboration that had broad implications for his work in general.

Cole and his colleagues' specially organized activity was designed to be conducted during the after-school hours. It brought together undergraduates from the university with children from the community in a variety of community-based organizations, including youth clubs, churches, libraries, and schools that were open to after-school programs. An unusual feature of this line of work at the time was that it made use of microprocessors and their telecommunication potential to link children in different geographical locales. These links provided extra sources of intellectual stimulation by encouraging joint activity related to both the activities children shared in common and the different circumstances of their everyday lives. Cole and his colleagues called this activity the Fifth Dimension.

The implementation of this line of research coincided with an initiative by the Carnegie Foundation to engage Soviet educational psychologists in a program of joint research on the role of computers and new information technologies in education. At the time, Cole had been, for several years, the commissioner in psychology for a program of academic exchanges between the American Council of Learned Societies and the Academy of Sciences of the Union of Soviet Socialist Republics. Beginning in December 1985 and continuing into the early 1990s, Cole and his colleagues were privileged to conduct a program of research on how psychologists from two countries that were teetering on the brink of catastrophic conflict would find ways to cooperate. They used as their vehicle their work with the Fifth Dimension.

The Fifth Dimension activity system, because it was designed on the basis of principles derived from cultural–historical psychology, proved to be an admirable medium for this new form of collaborative research. First, it allowed Cole and his American colleagues a legitimate reason for ongoing interactions with Soviet colleagues who had their own view of how to use cultural–historical activity theory for the design of educational activities with children. Because it was about children and their development in educational contexts, it could legitimately be considered a problem of common interest to scholars in the two countries, both of whom were (and are) struggling with understanding the role of new technologies in children's learning and development. Because it involved computer networks, it allowed scholars in the two countries frequent enough access to each other—using communication facilities that eventually became the Internet and then the World Wide Web and supplemented by periodic visits—to make it plausible that progress on the issues could be made.

Eventually, this project, which was located in the Institute of Psychology of the Academy of Sciences in Moscow, broadened out from its focus on children (although research on this topic continued) to an exploration of how to introduce the model of collaborative joint research into all of the social science institutes of the Soviet (then Russian) Academy. Only small parts of this research have been published in journals (Nissim-Sabat, Cole, & Belyaeva, 1997), but a monograph-length report was prepared and is available on the Internet (http://lchc/Histarch/velham.html).

At present, Cole continues to engage in all of these and other related international projects. The local and the global have become so fused in his everyday practices that only the mixture of languages being spoken seems to differentiate them. A lot has changed since the day he chose

psychology as a profession and set out for graduate school—both for him and for the world.

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Internationalism in Psychology: We Need It Now More Than Ever

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A concern with international cooperation in psychology has been present since the beginnings of psychology as a science. In this article, the author traces the development of international cooperation from the late 19th century to the present day to document the interesting ways in which the forms of collaboration have always been related to the large social and political contexts of which they are a part.

Editor's Note
Michael Cole received the Award for Distinguished Contributions to the International Advancement of Psychology. Award winners are invited to deliver an award address at the APA's annual convention. A version of this award address was delivered at the 114th annual meeting, held August 10–13, 2006, in New Orleans, Louisiana. Articles based on award addresses are reviewed, but they differ from unsolicited articles in that they are expressions of the winners' reflections on their work and their views of the field.

This historical material is followed by an autobiographical account of the author's own involvement in cooperative international research to illustrate how such activity can play out. A few selected collaborative enterprises are then described to emphasize the point that psychology stands to benefit enormously from continued international cooperation, especially in times of international conflict such as that being experienced at the present time.

Keywords: international cooperation, cross-cultural research, socio-political context

When a person is recognized for lifetime achievements in an area of scientific endeavor, this indicates that the person so recognized has probably lived long enough to have experienced personally a meaningful fraction of the history of the topic at hand. Consequently, such a person has the opportunity to examine the topic both in the long *duree* of the science and its historical context, and in the not so long *duree* of a single scientific career.

My topic is the involvement of psychologists in international research and collaboration. I begin by providing a brief history of international collaborations among psychologists living in different countries. I then summarize my own international experience, which began at a pivotal time in this history. I end by describing a little of the current state of internationalism in psychology in order to reflect on the special significance of international collaboration at this particular juncture in the truly long *duree* of world history.

Internationalism in Psychology Since the Beginning: The Broad Picture

What is often referred to as scientific psychology was born almost simultaneously in America and several countries in Europe. Although textbooks credit Wilhelm Wundt with founding the first experimental laboratory in Leipzig in 1879, the new approach to the study of the mind was not really the province of any one person or country. For example, at almost the same time, William James was encouraging his students to conduct experiments at Harvard; Francis Galton in England was initiating the first applications of intelligence tests; and Vladimir Bekhterev opened a laboratory in Kazan that explored a wide variety of topics including mechanisms of learning, alcoholism, social behavior, and psychopathology (Cole, 1996).

Almost simultaneously, a proposal was made by a Polish scholar, Julian Ochorowicz, for initiating an international organization to bring psychologists from different countries together around a broad range of potential common topics (Nicolas & Söderlund, 2005). According to Nicolas and Söderlund (2005), Ochorowicz pointed out that psychology had changed a great deal in 50 years and that it was getting closer to the natural sciences. He suggested that it would require collective effort to progress rapidly, with widespread collaboration, and the continuous exchange of observations, information, and experiments. He insisted on the necessity for mutual knowledge and respect as this was the best way to lay to rest polemics that were often just misunderstandings and confusions of terminology (Nicolas & Söderlund, 2005, p. 395).

Eight years later, the first International Congress of Psychology was held in Paris and has continued to be held every few years since, with the exception of the years of the two World Wars in the first half of the 20th century. However, until well after the end of World War II, the international composition of the congress was dominated by Europe and the United States, reflecting their dominance on the world stage. At the first congress, all but 1 of the 27 members of its committees were from Northern Europe (the exception was William James). At the 6th congress in 1909, Euro-Americans were joined by a delegate from Cuba, and in the following congress in 1923, a Japanese delegate was included. But leadership of the organization remained firmly in Euro-American hands. These countries dominate participation and management of the congress to this day, despite the fact that psychologists from approximately 100 countries currently participate (Rosenzweig, Holtzman, Sabourin, & Bélanger, 2000).

Over this same period, English replaced French as the dominant language of congress presentations, with the mix of other languages depending on the country in which the congress was held. This trend paralleled the growing influence of the United States as a leader in world affairs, as well as in psychology.

When the view of the origins of psychology is broadened to include the state of world history as the field was emerging, the domination of Europe and North America in international psychological relations is not difficult to understand. The first congress coincided with the period during which European colonialism reached its high watermark. Several events marked this coincidence. For example, in 1884, 15 European powers (joined by the United States) gathered in Berlin to complete the "scramble for Africa." Colonial powers were officially enjoined to defend and administer the areas they occupied if they wanted other countries to recognize their claims. By 1900, roughly 90% of Africa, 99% of Polynesia, 56% of Asia, and 27% of the Americas were under European colonial rule (and this does not count either Australia or the United States; Sapan, 1906).

The United States, in fact, had joined the process of colonial expansion, most notably in the Philippine Islands. On December 21st, 1898, President William McKinley published a proclamation asserting American sovereignty over the Philippines. He referred to his policy of colonial control as "benevolent assimilationism." It is estimated that more than 250,000 Filipinos, mostly noncombatants, were killed in the ensuing insurrections. In 1901, resistance to

U.S. occupation was made a capital crime by the United States Congress (Blount, 1913).

The political, economic, and military domination of the colonized countries by the colonizing countries was accompanied by a parallel set of beliefs concerning the relative psychological status of the colonizers and the colonized. As part of a long tradition of thought traceable back to the 15th- and 16th-century age of exploration, European scholars, to whom the origins of academic psychology are attributed, assumed varying forms of the idea that non-European, "primitive adults" were psychologically developed only to the level of European children (see Jahoda, 1999, for a summary of these views). To give an idea of how widespread and durable this idea was, H. G. Wells, in his enormously influential treatment of world history could write,

> Primitive man probably thought very much as a child thinks, that is to say in a series of imaginative pictures. He conjured up images or images presented themselves to his mind, and he acted in accordance with the emotions they aroused. So a child or an uneducated person does today. (Wells, 1923, p. 56)

Although similar views were still plentiful in the aftermath of World War II, the process of decolonialization accelerated, and as it did, the number of countries contributing to what became known as the International Union of Psychological Sciences (IUPsyS) increased. IUPsyS officially came into being at the 13th International Congress of Psychology in Stockholm in 1951. Eleven member countries were at the Stockholm meeting, and 9 additional countries joined later that year. By 2005, there were 70 member countries and the organization reported on its Web page that additional members could soon be expected from Africa, Asia, Eastern Europe, and South America (History of the IUPsyS, 2005).

In addition to a marked increase in the numbers of countries represented, the functions of IUPsyS have steadily expanded beyond the goal of organizing scientific meetings to the promotion of international contacts of many kinds. These include sponsoring and implementing research projects that foster international and professional cooperation and the creation of the International Network of Psychology and the Developing World, the Healthnet network, the International Project on Psychological Dimensions of Global Change, the International Network on the Young Child and the Family, the Human Perceptions and Behaviour in Sustainable Water Use project, the Compilation and Comparative Analysis of National Psychological Codes of Ethics, and many more (for more examples, see http://aix1.uottawa.ca/~pritchie/introduction.html). In addition, a myriad of workshops and training activities have been organized in and around congress sessions and in response to special events (such as the Southeast Asian tsunami of 2004).

The American Psychological Association has played an active role in IUPsyS for many years. In 1944, it created the Committee on International Relations in Psychology to advise the association on the rehabilitation of psychological laboratories and libraries in post–World War II Europe, and subsequently the committee took on a broad range of responsibilities for developing contacts between psychologists in the United States and their colleagues abroad (see http://www.apa.org/international/cirphistory.html).

In short, it is evident that a great deal of international cooperation has been going on at an institutional level for more than a century and that the United States has been active in support of these activities. The shape of this activity has reflected the shape of international relations as a whole. For example, if one looks at the major international journals in psychology, it is evident that while English is their linguistic medium and native English speakers represent a preponderance of the authors, authorship has become increasingly diverse, and multiple authorship from people living in different countries is increasing. Moreover, IUPsyS has paid attention to its own history, sponsoring books and papers that recuperate the history of psychology in different parts of the world (e.g., a special issue of the *International Journal of Psychology,* Volume 36, Number 6, 2001).

All of these events, including the fact that the most recent International Congress of Psychology was held in Beijing and that the history and current status of Chinese psychology has been the subject of special articles and an entire handbook (Bond, 1996), indicate that internationalism in psychology has grown enormously over its 125-year history to become more global and more inclusive.

International Collaboration in Psychology: A Personal History

My sketch of the growth of psychologists' internationalism in broad strokes glosses over a myriad of important complexities that those engaged in such institutional work are certainly more competent to address than I. Rather than seek to write a more nuanced account based on secondhand information, I turn now to a totally different level of analysis: my own experience in developing international collaborations in psychology in my own historical context. International congresses play an insignificant role in this story. I have attended only one such congress, and as I recount below, that one occasion came about as a by-product of an ongoing collaboration in which I traded my labor at the congress for access to a unique historical archive of research materials. Some institutional work and organization of meetings, and even a period on the executive board of the Committee on International Relations in Psychology, are part of this history, but they are secondary. The focus,

instead, has always been on joint, collaborative research: its benefits, its charms, and its difficulties.

In addition to analysis at the institutional macrolevel and the individual microlevel, it is also important to consider the contributions of the larger social context to the unfolding of events. Certainly the international context was essential in enabling my own involvement in international collaboration.

I was born just before World War II, so I was only entering my teens when IUPsyS was founded. On the international scene, these years were marked by Cold War competition that broke out in an undeclared hot war in Korea that claimed a close family friend and created a politically charged atmosphere in the United States, where fear of communism kept a tight clamp on political dissent. Racial segregation still reigned in the southern United States, and those who were Jewish could not buy homes in the town where my university is currently located. It was also a time of increasing prosperity in the United States fueled first by the war effort and then by the heavy involvement of the United States in the rebuilding of Western Europe and Japan, as well as maintenance of a wartime economy as a deterrent to the Soviet Union and its allies.

The first member of my family to attend college, I entered graduate school in the same year that the (then) Soviet Union launched the first orbiting satellite. My individual life trajectory and these global events came together at Indiana University. At the time, the Department of Psychology at Indiana University demanded that all doctoral candidates pass two foreign language exams and complete a minor outside of psychology. I had taken enough French to pass one language exam but did not know another. Because I was working with William Estes, I embarked on a mathematics minor, but I have no talent as a mathematician, and I struggled with the required courses.

Then a totally serendipitous configuration of events changed my career course. Stopping by the humanities building to meet my wife, I saw a mimeographed announcement of a new scholarship program for students who wished to go to the Soviet Union for postdoctoral work. Indiana University, it turned out, was the headquarters for a newly initiated exchange agreement with the Soviet Union. The program, sponsored by the Ford Foundation, offered 10% more scholarship money than I was receiving, and it contained two provisos that solved my immediate need for a second foreign language and a minor I could pass: I had to take Russian and a set of courses in Soviet Area studies.

Such a program of studies also promised to solve another, more personal problem. My wife and I had a strong desire to venture out and see other parts of the world, yet we had no family money that would permit us to travel without working. We had thought about seeking a postdoctoral fellowship in England, but that seemed insufficiently venturesome, and while our knowledge of French could probably have been improved to make a fellowship in France possible, I (in my gross ignorance) thought there was little in French psychology that would make a postdoctoral fellowship there of any interest. But Russia! That was an entirely different story! Russia was the home of Ivan Pavlov, whose theories were foundational to the form of learning theory that was my specialty in graduate school. And Russia was *the* other—the place that was supposed to be everything that the United States was not. *That* seemed like an adventure worth pursuing.

So pursue it I did. I applied for and received a Ford Foundation scholarship. My wife and I began taking Russian during the academic year and in summer institutes. Seeking a scholar in the Soviet Union who might be interesting to work with and who might agree to take me on, I encountered an article by Alexander Luria and Olga Vinogradova (1959) on semantic reflexes. That work opened before me the possibility of using the learning theory I was studying at Indiana University to conduct research on such complex human psychological processes as the development of word meaning, in addition to understanding how rats learn to run down mazes and how undergraduates decide among events with different probabilities of occurrence. I wrote to Luria, and much to my surprise he wrote back. Yes, if I were selected to come on the exchange program, he would be glad to be my advisor. And yes, he would be pleased to supervise research on semantic reflexes. The year was 1962. I leave it to the reader to recall the international context.

I have written about the time I spent in Moscow and my relationship to Alexander Luria elsewhere, so I do not pursue the matter in detail here (Cole, 1984; Cole & Levitin, 2005). Two points seem worth emphasizing in this context. First, Luria immediately integrated me into his laboratory, where few spoke English and where I was simply a part of a research team. He was not particularly interested in semantic reflexes at the time, but he found a way for me to pursue my interests in a setting that ensured I would become proficient in Russian and would absorb the ethos of everyday research practices as part of a collective. Second, he was the first to intrigue me with the possibility of conducting scientific, psychological research on cultural differences in cognitive development.

Just before we left Moscow for the United States in the early summer of 1963, my wife and I were invited to tea at the Lurias' apartment. For whatever reason, that afternoon Luria went over to a giant cupboard in his study and withdrew a cardboard folder within which was a set of yellowing research notes. They were records of research he had conducted in Central Asia in the early 1930s.

I do not recall much of what he told us about this expedition. I found it rather odd that this man whom I knew primarily as a neuropsychologist had once conducted cross-

cultural research. But one bit of data stuck in my mind, the response of an Uzbek peasant when presented a simple syllogism:

> All the bears in Siberia are white. My friend, Ivan, went to Siberia where he saw a bear. What color was the bear?

"How should I know what color the bear is?" replied the peasant. "I have never been to Siberia. Ivan is your friend. Why not ask him what color the bear is?"

We chuckled over this response and soon returned to our conversation with our wives. But the chain of serendipitous events set off by my chance encounter with the announcement of a fellowship to learn Russian and go to the Soviet Union had only begun to unwind. There was no way to obtain employment from Moscow, and Bill Estes kindly arranged for me to obtain postdoctoral support at Stanford University, where he had moved at the same time that I was on my way to Russia. My wife and I took up residence in Palo Alto, California, where she continued to pursue the career in journalism that she had begun in Moscow, and I began to write up my thesis and take on a new project applying my background in mathematical learning theory to analyzing data from animals running through mazes. My only connection with Moscow was through editing the translation of the *Journal of Soviet Psychology* and editing a handbook of Soviet psychology—my way of repaying Luria for his kindness. The experience of living and working in Moscow was a wonderful education in life, but nothing in particular about Soviet psychology attracted my lasting interest.

That situation changed in a completely unanticipatable way. One afternoon in early December 1963, I received a phone call at home from Patrick Suppes, the director of the institute where I was working. Pat wanted to know if I might be available, on short notice, to fly to Liberia, West Africa. He wanted me to consult with an expatriate mathematician who was investigating why his Liberian students seemed to have extraordinary difficulty learning mathematics. It turned out that the "new mathematics" movement had spread to Africa, and the expatriate mathematician, John Gay, was skeptical about the efficacy of this, or more traditional approaches to mathematics education, in producing mathematically competent high school graduates.

I accepted the invitation, of course. I knew nothing about how to learn or teach mathematics, but Pat assured me that it was only my skills in learning theory and experimentation that were needed. Others would worry about issues of pedagogy. Two weeks later, my arm punctured with the appropriate collection of inoculations, I found myself riding up a dusty, bumpy road through rubber plantations to a small Episcopal college, where I met John Gay and where my career investigating the role of culture in cognitive development began.

As promised, I was not the only academic to consult with John, but I was the only one for whom the experience grew into a lifetime preoccupation. Our early research focused on trying to understand the difficulties that children manifested in school by looking to the everyday activities in which local Liberians, who made their living principally by rice farming, engaged in arithmetic practices of some kind. We experimented with a wide variety of tasks, many of them designed to model local practices, others using tasks typical of cognitive psychology at that time. One of our principal findings was that when we modeled cognitive tasks on indigenous activities where practices we had identified as arithmetical were in evidence (e.g., measuring quantities of rice), local people actually displayed more mathematical ability than people from the United States. But when the materials or procedures had no local counterparts, local people experienced significant difficulties (Gay & Cole, 1967).

On the basis of preliminary reports of our research, we were encouraged to apply for a larger grant to follow up on its implications beyond the topic of mathematics education. It was in preparing for this grant that I remembered Luria's story about the Uzbeki peasant. Now international collaboration took yet another turn.

I wrote to Luria asking him about the research he had conducted in Central Asia more than 30 years earlier. Why had he done it? What had happened to it? Where had he published it? Luria wrote back that he had subsequently become involved in other lines of research and had never fully analyzed his data. He made me a proposal. If I would come to Moscow for the summer of 1966 and bring a supply of reprints of modern work on cross-cultural psychology, he would meet with me daily to go over the data collected in Central Asia. In return, he asked that I help with organizational work for the upcoming International Congress of Psychology and that I help edit the conference materials in English. Hence, I made my one and only appearance at an International Congress of Psychology.

I remember the congress itself only as a whirl of arrangement making and crisis management as Moscow struggled to accommodate its many visitors in the style to which they were accustomed. The deeper significance of those months was the merging of my own first steps in cross-cultural research with a dawning realization of the broad theoretical reasons for Luria's trip to Uzbekistan. Underpinning both the neuropsychological research that I had been involved with four years earlier and the amusing story about syllogistic reasoning was a form of theory more encompassing than anything I had imagined possible—a bio-social-cultural-historical approach to understanding human nature. Luria's theory assumed that both the structure and functioning of the human brain depend crucially on the sociocultural medium in which the brain has

developed in the course of phylogeny and within which individual human brains develop in ontogeny.

For the next decade, my cross-cultural research and my studies of cultural–historical psychology became ever more complexly imbricated. The next generation of research in Liberia focused on the relationship between schooling and cognitive development (Cole, Gay, Glick, & Sharp, 1971). This line of endeavor failed in its attempt to find international partners in the sense of native Liberians with whom I could engage as coresearchers. While college students were essential to the research enterprise, they were not and did not seek to become professional psychologists, and the few Liberians with degrees in anthropology or psychology with whom we worked were too absorbed in government posts to devote themselves to the work in a sustained way. Instead, I found research collaborations among other North Americans and Europeans who were themselves engaged in cross-cultural research. The same fate befell our cross-cultural research in the Yucatan, where I made contact with Mexican psychologists, but they were preoccupied with their own projects, and it was Americans who joined in to form productive collaborations (Sharp, Cole, & Lave, 1979).

My attempts at collaboration with Soviet psychologists were different, but for a long while little more satisfying. I tried to arrange for Peeter Tulviste, a leading Estonian psychologist who was a student of Luria's, to join our team in Liberia, but such an arrangement was impossible even for Luria to organize. Luria and I continued to cooperate on what was now a combination of projects. I translated the first, tentative publication of his cross-cultural research in the *International Journal of Psychology* and helped to arrange for the publication in the United States of the book about this work (Luria, 1976).[1] I also edited the translations of his two case studies of exceptional individuals (one with an unusual memory, one with a traumatic brain injury) and coedited a set of essays by his colleague and mentor, Lev Vygotsky, as well as his own autobiography (Luria, 1979; Vygotsky, 1978). But Luria died while the Cold War was still intense, and with his death, my substantive collaboration with Russian psychologists came to an end for several years.

In the mid-1970s another serendipitous event ensured that my initial international collaborations with Russian colleagues would not entirely wither and die. The American Council of Learned Societies and the National Academy of Sciences initiated an exchange program with the Soviet Academy of Sciences focused in the social sciences. My mentor, and now colleague, Bill Estes, was named the first commissioner in psychology. Bill asked me to assist him. Together with other colleagues, we went to Moscow on a voyage of discovery to find people and projects that could serve as the subjects of joint research activities. These efforts met with only partial success. Our Soviet counterpart chose to use his position of leadership in the exchange program to restrict drastically the participation of psychologists who were not working at his psychology institute in the Soviet Academy of Science. While some people were of interest to Bill and our American colleagues, many of those Russians whose work attracted the interest of American psychologists were systematically excluded from interactions; when the Soviets made a return trip, only members of that single institute were allowed to make the journey. Under the circumstances, Bill asked if I would take over as commissioner. I did so in part because I could use the existing exchange program, despite its restrictions, to renew and build on old ties and to exploit my knowledge of the Soviet system to make the program more inclusive.

Then began a different kind of international collaboration that was more typical of the workings of institutionalized psychology. Meetings were held alternately in each country. A few researchers went on exchange visits of a few months during which they conducted research that in some cases actually resulted in joint publications (from different institutions in the United States but always with members of the Soviet Psychology Institute within the National Academy of Sciences). Books of collected papers were published in the Soviet Union and to a lesser extent in journals in the United States (Belyaeva & Michaels, 1985; Brushlinsky, 1983; Michaels & Cazden, 1988; Rubtsov & Martin, 1991). It was not a satisfying arrangement from either side's perspective, but it kept at least a trickle of nonvituperative interaction going in the name of the international norms of science.

In the summer of 1980, my involvement in international collaborations broadened. I went to Japan to lecture under the auspices of the Japanese Psychological Association. My host, Kunio Wakai, then at the University of Kobe School of Education, cleverly arranged for me to give a *series* of lectures on culture and development to groups whose specialties included psychology, cognitive science, and a number of other social science disciplines. I assumed that each lecture would be for a different audience because they were often held in different places, but I soon discovered that there was considerable overlap in the audience for each successive talk. Moreover, the same individual was sometimes participating as an educational psychologist, sometimes a developmental psychologist, sometimes a cognitive scientist. Some worked in schools of engineering or primate research centers, others in departments of psychology and education. Despite their diversity, there was relatively little evidence that one or another person could not

[1] Two brief reports about this project appeared in *Science* in the early 1930s, but little of substance was reported.

enter equally into the conversation, because they were not considered expert enough.

To be able to make sense to this complex audience, I began attempting to frame my lectures so that they might be relevant to the recurrent scientific concerns that I was hearing, if only dimly understanding. At my lectures I awaited questions with anticipation, struggling to understand my interlocutors just as they had been struggling to understand me. To avoid boring my audience, I was forced to go more and more deeply into my work and its implications, an educational experience in itself.

During this series of communicative encounters, I experienced a growing sense of the depths in the viewpoints of my Japanese hosts. Prior to my visit to Japan, I was extremely unclear about what, precisely, they found interesting in my work. I knew that I was expected to discuss cross-cultural research. But why? Was I "cultural enrichment," or did my Japanese colleagues have some deeper interest in the cross-cultural enterprise? How did my work fit their theories and styles of research?

To the limited extent permitted by the etiquette of such situations (I was, after all, supposed to do most of the talking), I contrived to have my hosts answer questions about their possible interest in my work. What set of questions organize *their* work? Which American scholars do they use the most in their thinking? How do they apply American ideas to their observations and experiments?

I learned about several pervasive concerns: How can we organize the lives of young children to make them intellectually effective in a modern economy? What distortions does an IQ-based meritocracy introduce into Japanese culture? How can the children of the tens of thousands of Japanese individuals who live abroad for a while be helped over the difficulties of readjusting to Japan? What is the changing role of technology in the process of development? Overriding these concerns and perhaps organizing them in some sense, was a deep interest in what it means to be Japanese.

Fortuitously, near the end of my stay, a group of American cognitive scientists—headed by my colleague Donald Norman, who was then at the University of California, San Diego—arrived for a meeting that included the same people with whom I had been developing a multifaceted conversation. The discussion again broadened as both I and the Japanese colleagues with whom I had been communicating reoriented to consider the topic of culture and cognitive development with respect to the concerns of American cognitive science. Culture was not, at the time, a conspicuous topic in cognitive science, so again important intellectual bridges needed to be built, not only with my Japanese hosts but with my American colleagues—and in the same setting. These unusual social circumstances forced me to rethink, reformulate, and constantly refine my ideas.

By the time I left Japan, I had come to believe that Japanese psychologists have a great deal to contribute to the world's knowledge of human nature and the mechanisms by which it changes. The breadth and depth of their ideas far exceeded what I had been able to glean from their publications in English (I could not judge those in Japanese), where the basic technological and theoretical frameworks were modeled on Western, primarily American, academic psychology. Their unique historical experience had given them a special vantage point on problems of common interest to social scientists, but conventions of scientific communication and institutional organization seemed systematically to block their insights from my view.

Acting on this belief, I began to organize for more intensive exchanges: Several Japanese colleagues came to my laboratory for extended stays, and we organized a series of working meetings on a variety of topics of common concern, some of which took place in the United States and some in Japan (Cole & Hall, 1981).

It did not take me long to discover that my Japanese colleagues were very sophisticated about the process of international exchange. At the first conference we held following my return from Japan, Hiroshi Azuma, who had been a graduate student in the United States in the 1950s, and who had been conducting a comparative research project with Robert Hess on the influence of maternal communicative styles and attitudes on their children's development for a decade, wrote a thoughtful article about the conditions for fruitful collaborative research on cultural differences. His experience had taught him, he wrote, that having commensurate funding from both sides of any comparative study is

> the best, if not the only, way to conduct a fair cross-cultural study. . . . If one group depends financially on the other, the party that finances the study and therefore is accountable to the funding agency will have a much stronger influence upon the decision making at various stages. . . . If one group is paying for the work, the study will turn out to be practically mono-cultural in all phases except that the data will be collected from two cultures, unless the concerned people are extremely careful and sensitive. (Azuma, 1981, p. 23)

Needless to say, Azuma (1981) was speaking to the heart of the difficulties of the cross-cultural research that I had carried out in Liberia, an issue to which I return below. But even the Azuma–Hess project was not free of instructive difficulties. Owing to limitations on time and space, I want to give an example from Azuma's talk of the kind of insights that such reciprocal collaborations can produce.

The Azuma–Hess basic research plan called for mothers to interact with their children around two tasks: a scripted teaching task in which the mother was taught about sorting some blocks and then asked to teach her child to do so, and a free play situation in which mother and child were left alone at a table with a set of constructive materials.

The use of these two tasks was arrived at after long negotiations in which the American researchers wanted to use the block sorting task that had been used in prior research and was structured in a way that was reliably codable, but the Japanese felt that the task was somehow awkward, so they preferred the free play situation as a context for studying mother–child interactions.

When the international research team analyzed the results, they found that the block task produced a clear picture of the influence of maternal communication styles on children's behavior *for the American sample,* but exactly the opposite results were obtained for the free play situation, which produced clearer results *for the Japanese sample*. Azuma (1981) wrote,

> If we had not been working together on equal grounds and did not have mutual respect for each other's intuitions as mature researchers, perhaps we would have ended up discarding free play since its coding was less objective. In doing so, we would have lost a set of predictor variables which turn out to be very suggestive for cross-cultural comparison. (p. 25)

This same project unexpectedly alerted the Japanese researchers to heterogeneity among Japanese individuals with different life experiences. Most of the participants in their sample were educated women; years of education correlated strongly with attitudes about socialization that were similar to the modal American pattern. But a part of the sample came from a rural area where education levels were low. The Japanese researchers discovered that in this latter population, mothers generally worked in the fields and children were left with grandparents to be looked after. Consequently, the entire experience of being left alone to interact with their children was awkward, creating what Azuma (1981) referred to as a *strange situation*. He ended his talk by commenting,

> This was a bitter lesson which taught us that we have to study our sub-cultures with the modesty of foreigners. Being a Japan born Japanese does not guarantee a fair knowledge of our culture, and being a well-trained researcher can mean a well-westernized mind. (Azuma, 1981, p. 25)

Other papers from this and subsequent conferences made a profound impression on me. Over the years, all of us who participated in these interactions came to have a far deeper understanding of the culturally conditioned intuitions we had about questions of human psychology.

International Collaboration in Conditions of Bilateral Conflict

I wish now to show how I came to use Azuma's (1981) cautions about international collaboration as a set of benchmarks for planning, implementing, and evaluating research among psychologists in different countries. I also revisit the special problem of cross-cultural research conducted in nonindustrialized countries where fiscal equality is precluded.

In the latter half of the 1980s, when Mikhail Gorbachev promulgated his policy of *glasnost*, a new opportunity arose to renew research ties with the Russians that went beyond formal exchanges to involve genuine joint research. Under the auspices of the Carnegie Corporation of New York, and acting in my role as commissioner in psychology for the American Council of Learned Societies, I was permitted to initiate a program of joint research with Alexandra Belyaeva, a psycholinguist who worked in the Academy of Sciences Institute of Psychology (and thus was officially sanctioned to conduct research with American members of the joint psychology commission). Belyaeva had played hostess to exchange scholars and had done an exemplary job of making sure that their research visits were productive.

Our challenge was to conduct serious research despite the conflictual relations between our two countries. If either side were seen to be receiving an unfair advantage, the project would fail. Of course, it would also fail, if we failed to learn anything of value.

We chose as the object of our research optimal ways of organizing educational activities for children using microcomputers. This topic was clearly of great interest within both countries. In the United States, the advent of the PC had caused a flurry of interest in educational uses of computers, and PC use was increasing rapidly every year. Soviet psychologists were also interested in computer use, of course, but they had virtually no access to them because their computer industry lagged far behind that of the United States and because, owing to a high level of Cold War tensions, the United States had an embargo on shipping even very low-end PCs to the Soviet Union.

In addition, it was clear that no real progress could be made in joint research if it were to progress at the pace of the science exchange of previous decades. My laboratory had already been engaged in the use of computer networks to link children using computers in different parts of the United States, and it seemed obvious to us that we needed to make computer networking a part of the research plan, but this idea also faced formidable barriers. The use of computer networking was even more restricted in the Soviet Union than was the availability of computers suitable for educational purposes, in effect being restricted to the defense establishment. Only two connections that we knew of could be made by computer network from Moscow and Europe: one via Vienna, Austria, and the other via Helsinki, Finland. Both of those connections ran through a single portal in Moscow in a building under armed guard where the single stream of e-mail was monitored at all times by security personnel.

The system we worked out for creating the possibility of reciprocally beneficial research consisted of several interlock-

ing elements. First, we agreed to focus our research on elementary-age school children using computer software, some of it designed in the United States and some designed in the Soviet Union, all of which could be purchased on the open market. Second, we created groups of interested researchers in both countries who would participate in the work or consult on the use of programs and pedagogical procedures they had designed that would, insofar as technology made it possible, be implemented in both countries. With support from the vice president of the Soviet Academy of Sciences, Evgenii Velikhov, who was Gorbachev's science advisor at the time, Belyaeva and I were permitted to set up an e-mail link between Moscow and the United States for use by psychologists studying optimal ways of organizing educational activities for children using microcomputers. Not only were we able to arrange for Belyaeva and myself to interact in this manner and for the children to interact around common projects (including discussion of computer game strategies, imagining each other's worlds, even worrying about the threats of war that each country faced in the course of the project), but a group of Russian developmental psychologists outside of the Academy of Sciences was permitted to conduct a "joint seminar" on issues of human development and technological mediation with a number of counterparts in the United States.

In addition to American and Russian psychologists' involvement, we succeeded in involving an American software company (The Learning Company) so that one of their educational software programs could be modified using principles suggested by our Soviet colleagues and produced in identical versions in Russian and English (for a description, see Griffin, Belyaeva, & Soldatova, 1993).

I do not want to give the impression that this work proceeded smoothly. There were severe difficulties of coordination and collaboration within and between both the American and Soviet research groups. Within groups, different members sought to promote their own approaches, often at the expense of others. Between groups, participation in communication and collaboration was impeded by the continuing need for Belyaeva and her research group to be the human conduit through which e-mail was funneled going to and from Moscow, a situation that understandably irritated colleagues on both sides. Provision of funds for the Russian researchers was a constant threat to reciprocity. Despite these difficulties, we succeeded in creating joint activities among children and among developmental and educational psychologists that continued for several years and led to a second project with broader political and social implications.

Once we had established that we could, albeit in restricted hothouse circumstances, arrange for the joint study of computer-mediated educational activities for children and exchange ideas about how such activities are best organized according to our local theories and practical circumstances, Belyaeva and I undertook a new project. This time we focused on the use of the Internet for scientific exchange that moved beyond our project to incorporate all researchers at the Psychology Institute in the Academy of Sciences.

This expanded project began by arranging for any psychologist at the Psychology Institute in Moscow to be able to contact a partner abroad and to propose a plan of joint discussion aimed at future joint research. If they did not know of any potential partner outside the Soviet Union, we offered to contact people working in their area of interest. A public terminal with technical help was set up to make such activities possible. We asked only that users keep us apprised of their progress in finding partners and the scientific topics around which they decided to collaborate. Despite a good deal of suspicion about our motives and the possibility for serious legal repercussions among Soviet psychologists, this activity did result in new forms of Soviet–American collaboration in psychology (e.g., Tarabrina et al., 2001) and expanded markedly over the course of several months. On the basis of this progress, we next opened up this same opportunity to all the social science institutes within the Soviet academy, and again over time contacts expanded, albeit unevenly.

Only the part of this research program that involved psychologists has been published (see Nissim-Sabat, Cole, & Belyaeva, 1997), but a report describing the overall project was written and made available online (see *The Velikhov–Hamburg Project 1985–1994,* n.d.). When the Soviet Union ceased to exist in 1991, our efforts were rendered moot, but we had made our point: If people honestly set out to engage in collaborative, reciprocal, international research, even in situations fraught with conflict and potential for misunderstanding, such research is possible. I am no longer engaged in this line of research, but Belyaeva continues the work we initiated with a special emphasis on building civic institutions in Russia and its newly independent neighbors. This work embodies the ideas that Azuma annunciated more than two decades earlier.

Internationalism in Psychology Today

Against this background, I return to considering how the lessons of the past can be applied to confront current circumstances. What have psychologists learned from their mistakes? First, I consider the form of internationalism in psychology represented by traditional cross-cultural research, where it is virtually impossible to obtain equal financing of participants on the two sides, and where, de facto, one side has few or no psychologists with whom to set up international collaborations. Second, I turn to contemporary efforts at long-term collaborative research among (more or less) equal partners.

Reconsidering Cross-Cultural Research Among Countries With Unequal Resources

The form of international research where psychologists from a wealthy, industrialized country go to a nonindustrialized, poor country as a means of furthering their own understanding of the universality or social–cultural restrictiveness of psychological principles is still frequent. It seems that at the end of the 20th century, psychologists began to appreciate the urgency felt by anthropologists at the end of the 19th century: People whose hunter–gatherer way of life approximates that of all human beings 20,000 years ago are rapidly disappearing. Consequently, there appears to be special urgency in seeking such people out as a means of understanding as full a range as possible of cultural adaptations to human life circumstances and the psychological characteristics of the people who lead such lives. But how can this be done with the care and sensitivity that Azuma has warned are so necessary? How can psychologists avoid research in this tradition that is effectively "mono-cultural in all phases except that the data will be collected from two cultures" (Azuma, 1981, p. 23)?

This is a problem with which psychologists have struggled since the earliest cross-cultural expeditions at the end of the 19th century, a problem that has resulted in a large, sometimes acrimonious, and as-yet inconclusive literature (for a variety of relatively contemporary views, see Berry, Poortinga, & Pandey, 1997; Cole, 1996). At the start of cross-cultural scientific psychology, despite their efforts to be careful and sensitive in their studies of sensory processes among the people of the Torres Straits, the cross-cultural research of W. H. R. Rivers (1901) and his colleagues was submitted to devastating logical and empirical criticism by E. H. Titchener (1916). In Titchener's words, their conclusions were null and void. The mere fact that the raison d'être of cross-cultural research requires that the psychologist present equivalent tasks to people who are not assigned to experimental conditions at random compromises foundational presuppositions of psychologists' key experimental method.

Bowing to these problems, some have sought to promote investigation of indigenous psychologies, sets of ideas that have not been transplanted from another region (what anthropologists would refer to as *ethnotheories*). (For a well-informed summary, see Sinha, 1997). International collaboration in psychology using this approach requires psychologists to take seriously local views of matters one considers to be psychological, even if the people one works with have not attended school or obtained a professional degree. This approach has revealed a wide range of ideas about the organization of psychological processes in different cultures. However, there is no agreement about how to determine the commensurability necessary to generalize such knowledge in order to test the theoretical propositions involved, nor does it avoid the problem of relying on indigenous informants who may themselves be marginalized members of their societies.[2]

While there is no logic-tight solution to the problem of conducting psychological research under conditions of gross inequalities of power and the right to define what constitutes a legitimate set of questions and procedures, those who continue to seek a general science of human nature through international research have put together an extensive toolkit of methods to deal more adequately with the attendant problems. To begin with, many psychologists, realizing that they lack sufficient knowledge of a local cultural milieu to assure appropriate use of stimuli and procedures, have entered into collaborations with anthropologists whose deeper knowledge of local cultures could be drawn on in their efforts to be careful and sensitive.

There seems little doubt that some egregious errors have been avoided in this manner, but it is the rare anthropologist who would claim native competence in another culture, and there is a considerable literature within anthropology concerning the fact that those who become the informants that provide information about their culture are themselves likely to be unrepresentative of the population as a whole simply because they are willing to interact extensively with outsiders (Labov, 1972).[3] Further measures are clearly necessary.

During the ensuing 120 years or more since cross-cultural research began, ongoing efforts have been made to conduct such research in a manner that is sufficiently sensitive and careful to overcome the problems inherent in this enterprise. From early on, it was recognized that simple importation of psychological tasks from the psychologist's country of origin was insufficient. At a minimum, the standard instructions had to be given orally in the local language, and care had to be taken that the physical form of the tasks was not alien (photographs are not psychologically equivalent to objects). But eventually it was realized that the very form of social interaction that makes it possible to assemble and instantiate psychological tasks presupposes familiarity with the forms of discourse involved and their appropriateness (e.g., that an older man could interact with a younger woman or girl or could ask known-answer questions of an informant).

John Berry, who has long been concerned with the issue of cross-cultural comparisons, has proposed that psychologists elaborate procedures for moving iteratively between

[2] Shweder (2003) made the point, also noted by Azuma (1981), that often the indigenous experts to whom Western scholars turn have been educated in the West, so that they have acquired the very blinders that contemporary psychologists seek to remove.

[3] Labov (1972) wrote, "the student of his own intuitions, producing both data and theory in a language abstracted from every social context, is the ultimate lame" (p. 292).

tasks imported from the outside to tasks as constructed from the inside and back again as a way of trying to bridge the uncertainties that arise from combining cultural variation with unequal knowledge and power to decide what is and is not an appropriately comparable task (Berry, 1989). This in-principle solution has, however, remained largely in principle.

In light of these complexities, it is also now well accepted that even when great care is taken to draw on indigenous materials and modes of social interaction, conclusions based simply on average differences in performance on a single or small set of tasks defined by the initiating psychologist are basically worthless as a means of gaining insight into the sources of the performance differences across cultural groups. Instead, it is necessary, at a minimum, to focus on cases where local people perform adequately or exceptionally in some versions of a task, yet fail to deal effectively with other parts, in a manner that can be theoretically related to some relevant cultural variations. For example, Peter Gordon (2004) studied the ability of Pirahã adults, hunter–gatherers who live in a remote part of the Amazonian jungle, to match quantities of objects varying in number from 1 to 10 under conditions that varied with respect to their cognitive complexity. The Pirahã were of special interest in this case because their vocabulary for number was of the *one, two, many* variety, and even the words for *one* and *two* were not well differentiated. Gordon found that for simple one-to-one matching tasks, performance was more or less accurate. This provided evidence that the Pirahã participants understood the task. But when the target display was presented only briefly or when it required some kind of spatial transformation to arrive at a correct number, performance decreased markedly. Gordon interpreted this result as providing evidence for the importance of language in thought. This interpretation was supported and extended by Pica, Lemer, Izard, and Dehaene (2004) among another small Amazonian group. These people also experienced difficulty dealing with apparently simple tasks involving enumeration when the quantities exceeded 4 or 5, but seemed able to cope with tasks involving geometric figures for which verbal encoding appeared unnecessary.

A related approach, a naïve version of which my colleagues and I adopted in our early work in Liberia, has been to conduct research in which some of the tasks are modeled on the indigenous practices of one group, some are modeled on the indigenous tasks of a contrast group, and then both tasks are given to both groups and the results are compared. In a sophisticated use of this approach, Robert Serpell (1979) studied the ability of children in Zambia and Scotland to create replicas of model figures drawn on a piece of paper (a stick figure human, a car, a cup and saucer, etc.). When asked to reproduce the figures using pencil and paper, the Scottish children outperformed the Zambian children, but when asked to reproduce the figures using wire (a mode of representation common in Zambia at the time), the Zambian children outperformed the Scottish children, and there were no average differences in overall performance. A similar result was reported recently by Greenfield (2004). Such results lead one to inquire, for example, whether there are indigenous practices among the Pirahã for which they have a well developed vocabulary, but for which adults in other countries (e.g., the United States) do not. Would it be possible to add further evidence concerning the role of language in thought by showing that in such cases, it would be the North Americans who experienced difficulties?

In my estimation, there has been no diminution in the need to heed Azuma's (1981) cautions to be careful and sensitive when conducting research in conditions such as those described here. There has been progress in this form of international research in the past 100 years, but the methodological difficulties encountered by psychologists of prior generations run deep.

International Collaboration Among Equal Partners

While not trouble free, the situation of international collaboration among scholars from countries with well-developed systems of higher education and funds to support their own contributions to research has clearly improved in recent decades. The dominance of international psychology by a few European countries in the late 19th century was followed by the dominance by American psychology following World War II, but there are many signs that American dominance is being replaced by a more equally balanced, if imperfect, set of relations among psychologists from the countries that make up IUPsyS. There is clear evidence of such a rebalancing in the collaborations reported by Azuma (1981). One can find similar evidence in the appearance of publications highlighting the psychological traditions of different countries that 30 years ago, were in subordinate positions that made it difficult for them to have their voices heard. This tendency is nicely articulated in the introduction to a handbook of Chinese psychology:

> Over the past three decades a ground swell of research activity has been gathering momentum. . . . This increase was initiated by overseas Chinese in North America, carried forward by psychologists in the colonial institutions of Hong Kong, given indigenous flavor by Taiwanese psychologists trained mostly in North America, and is now being passed on to mainland Chinese psychologists through professional contacts with foreign colleagues, both Chinese and non-Chinese. (Bond, 1996, p. xviii)

As a consequence of this energetic scholarship, the discipline of psychology now has a counterweight to the dominance of the field by the West. Chinese culture has the necessary age, coherence, and difference from Western

traditions to provide a litmus test to presumptions of universality that tend to characterize psychology done in the mainstream.

Similar statements can be made for a number of national traditions of psychology. As a consequence, a polyvocal international psychology appears to be within reach. Simultaneously, a large number of comparative research projects of a scope unimaginable a generation ago have become prominent on the international scene. These projects became possible thanks to the advent of high-speed air travel and even higher speed communications systems. They have come to seem urgent because of the increased interdependence of peoples living in different parts of the globe, people who perceive common international problems mixed in among their local and national concerns.

Two examples must suffice. The first is the Trends in International Mathematics and Science Study (TIMSS), a massive comparative study of school performance that began in 1994–1995 and gathered data in more than 40 countries at five grade levels (the third, fourth, seventh, and eighth grades and the final year of secondary school). Students were tested in mathematics and science; extensive information about the teaching and learning processes in local classrooms was collected from students, teachers, and school principals. Additional in situ data were collected for more than half a million students, and questionnaires were given to thousands of teachers and school principals. The fourth replication of this effort is due to begin in 2007.

Psychologists have played a key role in this effort as scholars seek to study the variations in classroom practices and their associated belief systems that account for national differences (Hiebert et al., 2005). The results are not especially gratifying for U.S. psychologists, who noted that a constellation of features focus U.S. teacher and student attention on lower level mathematics skills at the expense of conceptual development. The researchers argued that these results not only describe what is actually happening inside U.S. classrooms in a way that counters overly simple current debates about pedagogy, but also suggest ways to introduce practices that U.S. teachers can accept while balancing a skill emphasis with higher order mathematical problem solving.

A second example is a recent multinational comparison of the TV watching habits of children and a variety of associated developmental outcomes (Götz, Lemesh, Aidman, & Moon, 2005). For example, the children, 7–8-year-olds from Germany, Israel, South Korea, and the United States displayed clear differences in the way they incorporated media content into their imaginative worlds. German children displayed a high level of interest in, and concern for, animals and a relatively high level of ecological awareness. Israeli children's drawings and interviews revealed their heightened awareness of living in a society with a high level of civil strife focused around ethnic and religious differences. The Korean children reflected concern with the pressure put on them to perform well in school and fascination with computers, which played an unusually large role in their fantasy worlds. The American children were unusual for the high level of media content focused on consumer goods and for dreams of personal power. These international differences were complemented by evidence of the ingenious ways that the children selectively appropriate what they encounter and reweave it into ways of imagining the world that make them the agents of their own lives.

Conclusion

As one who has championed the value of international collaborations in psychology for the past 40 years, I find a good deal to be optimistic about in the current level of international interaction. It appears that in many ways we are approaching the goals annunciated by Julian Ochorowicz when he urged the foundation of an international congress some 125 years ago. Cross-cultural researchers have become far more aware of the pitfalls in this research, and professional international interactions have increased enormously. However, there are also good reasons to be concerned that progress in making psychology a truly international enterprise is failing to keep up with the pace of globalization and with the increased danger that international misunderstanding will result in a cataclysmic conflict that will dwarf the world wars of the 20th century. While progress has been made in bringing the entire range of nations into the forum of international psychology, the rapid escalation of religious fundamentalism and international conflict, combined with the increased power of many countries to visit massive destruction on others, are phenomena that urgently need to be discussed by all professionals who deal with the human sciences. Contrary to the beliefs of many, science has not replaced religion as a mode of understanding either the physical or the human world. It is little wonder, then, that the serious study of religion has undergone quantum changes in the past decade (Boyer, 2003; Paloutzian & Park, 2005).

In addition, science has not provided convincing guidance in dealing with the looming ecological crisis spurred by globalization and the rapid industrialization of what used to be referred to as the *third world.* In the name of economic prosperity, it appears that humanity is eating itself, so to speak, out of house and home. Here the knowledge of indigenous people discovered in careful cross-cultural research may prove valuable, if it can attract attention. Atran, Medin, and Ross (2005) have reported that a Mayan group living in rural Guatemala displayed what they referred to as *ecological thinking* when presented with the task of classifying a variety of local fauna and flora, whereas American college students classify these

same items taxonomically. This same group of rural farmers had developed methods of sustainable agriculture, while neighboring groups had not. They also reported that Native American children living in rural Wisconsin developed an ecological mode of thinking, whereas middle-class, urban U.S. children did not.

Psychologists cannot solve the world's myriad problems. However, through taking cultural variations seriously and engaging psychologists, members of other professions, and lay people from different societies in the effort to address critical problems confronting all of humankind, they may be able to contribute to the solution of *common* problems that must be addressed if humanity is to survive.

The means to global reconciliation based on common respect and the means to global destruction are both omnipresent. Those who abrogate the responsibility to help lead humankind down the first path should keep in mind that it is to their own progeny that they will have to answer.

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