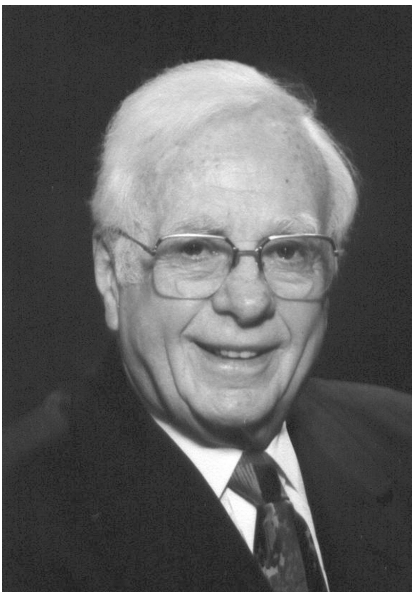
The Fruits of Professional Interdependence for Enriching a Career



by Robert R. Blake

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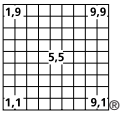

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The Fruits of Professional Interdependence for Enriching a Career

by Robert R. Blake

The happiest day in my professional life came in the fall of 1987. Jane Mouton and I had just learned that we were both to be inducted into the Human Resource Development Hall of Fame on December 9. The gratification was made doubly meaningful because of the simultaneous induction; in other words, a recognition that, whatever contribution had been made, it had been made as a team, not as two separate individuals. That gave validity to the operating premise of our entire joint career.

This moment of great fulfillment was all too soon followed by ultimate sorrow. The ceremony was scheduled in New York, immediately upon our return from a trip to India, where we addressed the International Congress of Training and Development, and then to Athens, where we were scheduled for client activity. The presentation in Delhi went quite well, but at this point a difficulty arose. Jane complained of abdominal pains and, as they grew worse, it was determined she should be hospitalized. She decided to cut the trip short and returned to Austin in late November. I continued to fulfill our commitments, phoning her daily in order to stay apprised of the latest events. Though she remained hospitalized, Jane claimed to be making progress and even thought she might be able to rejoin me in New York for the Hall of Fame ceremony. She died quite suddenly, two days prior to this event, on December 7, 1987.

This tragedy symbolizes the end of a significant part of my career. Jane and I were partners, working hand in hand for 36 years. Together we formulated the Managerial Grid®[1], the conceptual framework of which is contained in a book that has already exceeded sales of two million copies, and is available in sixteen languages. We also published *Synergogy*[2], a book that outlines a radical solution to many of the chronic problems facing teachers and educators today. These were only two of a long line of other books—38 in number—all mutually co-authored by us. Our major effort, however, involved the creation and

development of Scientific Methods, Inc. and the leadership we provided that has sustained it for three decades. For all of these reasons, this autobiography can only be written by weaving the centrally important fact of our joint cooperation into the story which follows.

My Family

I was born in January, 1918 in Brookline, Massachusetts, and lived in Massachusetts until I was thirteen. My family consisted of four children, three boys and a girl. I was the second in line, with an older brother, followed by my sister, and finally a younger brother. We all attended college but I was the only one who maintained my academic interests and went on to complete graduate work. My two brothers were engineers, both finding careers with DuPont. My sister became a nurse and later went into nursing administration.

My mother and father were both critical influences in shaping my future career; my mother, particularly so, as she had enduring academic ambitions. When I showed an interest in learning, she was my number one advocate and supporter. However, I trace the issue of my interest in conceptual learning primarily to my father. He was a graduate of Harvard at both the bachelor and advanced levels. Throughout his life he maintained an abiding interest in intellectual matters, particularly as they related to world developments. He and I maintained a constant dialogue throughout our lives as to the deeper meaning of political events, considered not so much from an ideological point of view as from the standpoint of conceptual assumptions underlying political and other forms of leadership. I believe it was from him that I gained my interest in the conceptual issues of leadership.

The Depression arrived with full fury, and my family moved to Tennessee. My father became responsible for one of the major forest development projects of that era. There were no schools within fifty miles, so I attended a resident high school in Crossville, a small, closely knit school, with only nine in my graduating class.

As I look back on it, my family was one of those in the last generation of tightly knit American families. In one way, the Depression illustrated this point. The Depression provided numerous illustrations of the conditions under which family cohesion permitted its members to persist and retain their integrity. It also taught me the impermanence of material wealth and the durability of conceptual commitments and

academic values. The former could disappear over night; the latter endured a lifetime, to be built upon not only for personal enhancement but for social value and applied utility as well.

Those aspiring toward a college education in my area turned their focus to Black Mountain College in North Carolina, Berea College in Kentucky, or Berry Schools in Georgia. All catered in one way or another to poor students of the Appalachian region. I was accepted in 1936 at Berea, and that became my life for the next four years.

Berea was unusual in every respect, and it continues many of its traditions even today. There was no tuition; rather, everyone worked in the college labor program. Life was simple: no cars, no smoking, no furs, no extravagances. It is still the kind of college where a student can invest him or herself in learning as fully as desired and with minimum interruption. I chose to major in psychology and philosophy. Both disciplines seemed central for understanding life, its institutions, and how they operate—business, government, education, religion, medicine.

In anticipation of graduation in 1940, I applied for graduate scholarships at twelve schools. Six were applications in philosophy; six in psychology. I received a DuPont Fellowship at the University of Virginia and was able to finish a Master's Degree before World War II. I also got married and moved to Virginia, with the clear recognition, however, that this arrangement was to be short lived.

Several key influences upon me from my college period derive from Charles Darwin, Sigmund Freud, Kurt Lewin, and J. L. Moreno. Darwin represented, for me, the perfect scholar: investigating, investigating, investigating; seeking to establish his theory of evolution in the most complete way possible before bringing it to the attention of the world for its scrutiny. He provided such a powerful model for me that his influence on my career is without question. Though I was early to learn and commit myself to "pure" laboratory research resting on preplanned experimental designs, later on natural or field research provided me a model of investigation that paralleled the university laboratory. "Natural" observations were to become my model in the psychoanalytic group therapy studies at Tavistock, and after my university period it became my model for our own fieldwork over the world.

Jane shared my admiration for Darwin. As opportunities presented themselves, Jane and I replicated Darwin's travels that led to his fundamental discoveries. We went to Terra Del Fuego, the turbulent waters below the tip of South America created by the joining of the Atlantic

and Pacific. We sailed those waters to gauge what he must have been up against on that voyage, particularly in view of the fact that Darwin suffered from seasickness. In order to do his research, Darwin served on the Beagle, where its captain sailed the vessel back and forth, back and forth in pursuit of the overriding mission of accurate cartographic mapping. As we sailed these same rough waters, we empathized with Darwin's plight. We continued on to the top of the Andes, where Darwin had been so impressed by the "contradiction" of finding sea shells high in the mountains at an altitude of 15,000 feet and, of course, we went to the Galapagos Islands to see for ourselves something of Darwin's frame of reference as he formulated the theory of natural evolution. To complete our studies of Darwin, we journeyed in England to the town of Downs where he lived in his later years; his house is now a private museum.

Much the same applies to the work of Freud. While I am reluctant to embrace his theory of the unconscious, my respect for him has steadily increased through the years as I have found myself more able to appreciate the depth and hiddenness of the phenomena he sought to explain.

Several projects provided something of an opportunity to study Freud's personal life in Vienna, which I did. I located an historically accurate novel about his life, told in chronological sequence. It reveals what Freud did, where, and with whom, and I used it in its entirety as a tour guide. This allowed me to follow many of his walks, e.g., to "accompany" him from his home to the hospital. It gave me a feel for the environment in which he had operated.

Kurt Lewin pioneered a new kind of social psychology research in his investigations related to leadership climates as well as being responsible for numerous other contributions. His work influenced me by demonstrating that complex interactions are as readily subject to rigorous, controlled investigation through planned designs as are more fixed and stable phenomena of the kind studied in one cause/one effect designs. This meant that many opportunities for expanding the various areas of social psychology research could now be evaluated by such means and methods.

The advent of World War II brought an end to my youth, and from 1940 forward, every life event proved to be of serious import. While recreation and social enjoyment did not recede into the background, what did expand was my sense of purpose. Service in the Air Force during World War II permitted me to continue my conceptual and aca-

demic interest in psychology, but now it concerned applied problems of crew selection for pilots, navigators, and bombardiers for the bombers of World War II. It is probable that my long-term interest in group dynamics originated at this point.

My service in the Air Force was concentrated in a Psychological Research Unit. This was an important experience for me in terms of its impact on my professional career because I was thrown together with 500 or more others in the same and other related fields such as sociology and anthropology. It was a marvelously broadening experience for a person like me who had spent four years in the simple environment of Berea College just a short time before.

I was discharged in 1945 in San Antonio, Texas. Since the closest university was the University of Texas in Austin, I found my way there and began teaching as an instructor, finishing my Ph.D in 1947. I continued as an assistant professor, associate, and finally full professor before resigning to form Scientific Methods, Inc. in 1964.

Two children were born during this period: Brooks, in 1954, currently a ranch foreman on a four-thousand acre spread just outside of Austin; and my daughter, in 1957. Cary has just completed Graduate School after receiving a Bachelor's Degree in psychology, but her focus is now on music, art, and media through which therapy with children may be enhanced.

My Two Careers

My professional life has extended over two quite distinct careers. One involved teaching and research at the University of Texas in Austin. The second, of almost equal length, started with the founding of our company called Scientific Methods, Inc., initially developed as a commercial venture for delivering behavioral science applications to business on a worldwide basis. We currently have offices in over forty countries, the most recent to be opened being in Thailand and the Soviet Union.

The University Years

The university years extended from 1947 to 1964. I concentrated in Social Psychology. It was during these years that research and applications in this area of endeavor were at their most creative and innovative stage. Much of what is taken today as the basis for good, sound practice is based on research findings and conclusions from that era.

In 1949, I received a post-doctoral Fulbright Scholarship that took me to the University of Reading and the Tavistock Clinic in London. This 18-month stint proved to be a major turning point in my life, primarily by virtue of that unique period in time and the nature of the colleagues with whom I came to work. It was during this period that the United Kingdom had enacted medical legislation that embarked the nation on an era of socialized medicine. This included the Tavistock Clinic. As a direct result of this, the staff was able to utilize a considerable portion of its work time for research purposes. Because it was rigorously psychoanalytic in its practice, my participation offered me a dramatic contrast between psychoanalytic thinking and academic psychology. It closed the artificial separation between cognitive social psychology and underlying dynamic motivations that energize behavior.

The London experience put me in contact with Wilfred Bion, author of the famous series of articles, *Working in Groups*; John Bowlby, who became this generation's world famous child psychiatrist and father of *attachment theory*; and Eric Trist, who is known for his work with sociotechnical systems analysis. Most important to me personally, however, was Henry Ezriel, the leading investigator of psychoanalytic group therapy and the person with whom I worked day-in and day-out in a co-therapist role in his groups. This was the bridge that provided the needed connection between cognitive social psychology and psychoanalytic group dynamics.

About this time there was a sharp de-evaluation of British currency. As a result I had to live on a shoestring, with enough, however, for frugal travel to Europe and an occasional weekend in Paris. I found my work and study so engaging that a 12- to 14-hour day became the rule.

My return to the States and to my university career can best be understood by following it along two tracks: the first concerned our research and development and the second our professional collaboration with colleagues and peers.

After Tavistock, rather than returning to the University of Texas, I accepted a research appointment in the Department of Social Relations at Harvard University, where I worked closely with Jerry Bruner and also spent many hours at the Psychology Clinic with Henry Murray of *Apperception Test* fame. Because my credentials had included rigorous experimental research during my graduate school days and with a continuation in the interim years and with numerous publications, I enjoyed accessibility to Memorial Hall.

Memorial Hall was the Department of Experimental Psychology at Harvard during this period of time. The split between psychology and social relations already existed but it was the creation of these two separate departments by Harvard's academia that made it official. Thereafter, it was common knowledge that the "rigorous" people resided in Memorial Hall while those who looked on psychology in a somewhat "looser" way were housed in the Department of Social Relations.

On occasion, when I visited Memorial Hall, I would run into B.F. Skinner, who seemed to find some degree of interest in my background. This formed the basis of our association and, as a result, we talked from time to time. Sometimes we would chat while he was attending his pigeons. These pigeons lived in boxes in the basement of Memorial Hall and Skinner, anxious to ascertain their response, would visit the boxes to "check them out." This effort was, of course, unnecessary as a counter maintained constant surveillance over the number of "pigeon pecks" and these were mechanically recorded. Nonetheless, Skinner avidly enjoyed monitoring this process and I would often find him standing with one foot on the box, doing his own mental arithmetic as to pigeon behavior. It was during these times that we often engaged in an inoffensive but provocative banter about psychology and social relations.

One short anecdote provides the full flavor of these exchanges I shared with Skinner. A ping pong table had been set up and the pigeons trained to peck the ping pong ball back and forth according to a prearranged schedule. That pigeons could be taught such a "social" game as ping pong via an individual reinforcement schedule and then put together in such a way as to respond to each other, not in point of fact but rather according to its own schedules of reinforcement, was regarded as an outstanding contribution for demonstrating that social relations, at least in conventional terms, don't exist. The ultimate communication was of a sign posted on the ping pong table entitled "Social Relations1."

There was a poignancy in all of this, as it served to demonstrate the fundamental cleavage that existed between experimental psychology and social relations at this time. Still, it was in a disguised form which, on the one hand, taught a lesson but, on the other, was sufficiently playful so that no one felt the need to lodge any serious objections.

Another significant development in my career involved the National Training Laboratories. While in London I learned that NTL

was being established in Bethel, Maine for the purpose of studying group behavior. One of my first objectives upon returning to the U.S. was to arrange to spend a summer there to learn more about this. That became the beginning of a ten-year, every-summer session for me, which proved to be a rich and rewarding experience in the context of the academic discipline following the psychoanalytic group therapy I had experienced at Tavistock.

Lee Bradford, one of the founders of NTL, was a unique administrative leader who did much to solve the problem of how to transfer behavioral science knowledge to applied use by "normal" people. He "protected" the T-Group by keeping it at the forefront of behavioral learning and sensitivity training for the period from NTL's inception until his retirement.

I served on summer faculties at Bethel and as a member of the NTL Board of Trustees for a decade. These provided yet another collaboration from which I learned much, mostly centered on Lee's efforts to maintain an action-oriented faculty comprised of many conflicting elements. The major polarity related to the role of theory vs. common sense, or even clinical insight, in seeking to learn more about social processes created in part by an individual's own conduct. Some faculty members saw theory as a comprehensive framework within which process phenomena could be identified and understood and in this way serve as the basis for generalization. Others saw theory as an effort to intellectualize, i.e., a way of deviating from the learning objective of using an activity to learn more about oneself.

This cleavage was never resolved during my tenure at NTL. However, efforts in pursuit of its resolution might be regarded as the core of Jane's and my life work. And, I think satisfaction was felt that we indeed achieved this objective.

Finally I was faced with a basic decision. The University of Texas' impatience with my frequent leaves made it necessary for me to decide where I might best spend my career. My wife, a native Tennessean, cast her vote for the South and a warmer climate. This tipped the scales in the direction of the University of Texas where I ended up concentrating the remainder of my academic work.

In 1952 I returned to Texas and it was then that I first met Jane Mouton as one of my students. She was enrolled in the Social Psychology Doctoral program, having previously finished a Master's Degree in Mathematics at Florida State University. Jane immediately became a teaching assistant and, from that point forward, we cooperat-

ed in every pursuit. Jane received her Ph.D. in 1954 and joined the University faculty as an assistant professor. She joined me in attending the Bethel summer sessions in order to experience the T-Group training methodology then in use. This turned out to be a critical turning point for her in the area of social and group dynamics.

The following year Jane and I conducted a laboratory course to introduce T-Groups to the University. This was a major decision because it made possible our engagement in research on fundamentals of change as they relate to learning social psychology theory, reinforced by a T-Group experience. Furthermore, it led to a book jointly authored by us and published in 1961 entitled *Group Dynamics: Key to Decision Making*[3].

Another "fall out" from these early NTL years was the development of a close acquaintance with Dr. Herbert Shepard, an early Bethel staff member but simultaneously employed by Exxon. This contact led to Jane and me conducting something approximating a ten-year-long experiment within the Exxon Corporation. More on this is to be said later.

As noted, previously, Jane and I had been conducting experiments in our Social Psychology Laboratory. These involved organizing students into T-Groups and then, at various points in time, arranging a competition between T-Groups as the basis for measuring team effectiveness. These experiments proved provocative and resulted in numerous publications. Several of these articles came to the attention of Exxon management. It seemed to them that this research bore some significance for reviewing basic interdepartmental and union-management attitudes.

The combination of my close friendship with Herb Shepard and the interest that Exxon had in our research findings resulted in Jane and me being invited to Baton Rouge to conduct a series of two-week seminars. The Baton Rouge Refinery was thought of at that time as being the avant-garde management development center within the Exxon system. Their readiness to try new things that might have significant implications for management and institutional effectiveness was indeed striking. Several hundred managers participated in this program.

A further experiment was built into the arrangement. The mornings were devoted to a T-Group type experience. The afternoons were to be applied to management development through case study discussions led by Harvard Business School professors. The intention of this experiment was to make possible a systematic comparison of the char-

acter of learning possible from each of the methodologies as well as from both in combination.

This experiment was eventually abandoned following the fourth seminar as a result of the evaluations by Exxon personnel. They came to the conclusion that case studies were simply not absorbing enough when compared to a T-Group experience and they therefore recommended dropping them from use. The seminars which followed, therefore, concentrated on an expanded study of behavioral theories, particularly power and authority in experiential terms and reinforced by theory. This led to the formulation of the Managerial Grid and to further intergroup study of the win-lose dynamics such as those found between departments, unions, headquarters and subsidiaries, and so on. The T-Group continued to be the centerpiece of this learning.

Jane and I accumulated a wealth of data during this time, particularly on the intergroup problem, which in turn led to the publication in 1964 of *Managing Intergroup Conflict in Industry*[4].

This introduction to group dynamics with a significant number of Exxon managers in attendance was judged to be highly successful, particularly as it brought many new and fresh perspectives to bear on how managers looked at recurrent and chronic problems as well as crisis situations and how to resolve them.

The head of the Bayway Refinery in New Jersey attended one of these sessions as a manifestation of his general interest in management development. As he and I became well acquainted, he made it known that he wanted to do something within his own refinery that might go beyond what had been attempted at Baton Rouge. As a result, I agreed to spend two years on a full-time basis at Bayway. The objective of the effort was, "to see how effective an organization might become were it to concentrate its full human resources on the objective of excellence."

The Bayway project closely followed the completion of the one at Baton Rouge. It allowed Jane and myself, and also Herb Shepard to some degree, to reexamine what had been attempted at Baton Rouge and to answer the question, "What should we now do or what would we do differently, given this opportunity of a fresh beginning?" Several aspects were included. One was to concentrate on the Managerial Grid as the centerpiece of learning about the options for exercising leadership when working with and through others. Another was that the seminars would no longer be T-Groups conducted by trainers in a hands-on way. Rather, they would be instrumented, i.e., the synergogic learning theory published by Jane and myself later on as Synergogy[5].They were

attended on a diagonal-slice basis, i.e., with people from all levels of management participating but with no direct boss and subordinate relationship on the same team.

The entire management component of Bayway participated and this stimulated many applications consistent with the learning. While the seminars themselves were highly successful, the hoped-for applications were slow in coming and some simply did not appear. This was disturbing to us because it suggested that the limits of development had been met, and they were insufficient to justify the effort expended. However, they also permitted an alternative conclusion. We hypothesized that the culture of most work teams is so deeply embedded that individual team members in and of themselves are unable to markedly change it, even though each may recognize the obstacles to team effectiveness that prevent them from being as effective as they might be. This led to the discovery of *Team Development*, later spoken of as Team Building. As a result, the effort that followed the basic seminar became an off-site session of each actual "corporate family," starting at the top. People were now instructed to diagnose within their own work team insights and feelings based on open and candid critique. This provided a basis of shared understanding regarding what had been done in the past that should no longer be continued and what might be done and should be done in the future that had not been a part of the past. This had a highly significant impact on team, and organization, effectiveness.

Other areas of application centered on the character of intergroup problem solving, involving such issues as turf and territory conflicts, such as those that occur between union and management, and problems of prerogative and "ownership," such as those between divisions, i.e., engineering and maintenance, manufacturing and marketing, etc. These applications also yielded gains in productivity for the entire organization.

A particularly significant event occurred in the top team development phase. When this group convened, it came to the conclusion that it spent entirely too much time reporting to the top man what was transpiring in each of the operating departments. Traditionally, the boss interrogated each member one after the other. The rationale for doing so was that it kept everyone on his toes. Furthermore, it was justified in the name of good mentoring. As a result of this, however, team members spent an inordinate amount of time being briefed by their own direct reports as to what was going on so that they would be in a position to accurately defend their performance within the top team. Rather

than being involved with the real business of running the company and a future corporate vision, this group of top executives busied itself with keeping track of the nit-picky details of day-to-day operations.

The upshot from this activity was important to the future of OD. This top team decided it was imperative that they stop doing what they had been doing. They determined that the appropriate business of a top team did not lie in a detailed analysis of each minute activity within the departments. Rather, there were other significant matters and many currently missed opportunities in which this top group should be engaged that held far more promise for the long-term health of the organization. This meant, however, that the power/authority dynamics must undergo a dramatic shift, with each member solving the problems of over-centralization that had been created by the need to be knowledgeable of the small details. In other words, the problem resided not only in the top team; it had cascaded down through the organization, resulting in each level being concerned with decisions that should be made at one or two levels below. Authority had been pulled up to the top with levels below unable or unwilling to exercise initiative. In some ways the organization had become paralyzed, waiting on a nod from the top man before action could be taken.

At this point the top team decided to set a date two months out at which time the problem of centralization/delegation was to be resolved, thus freeing the top for new and more important activity. Once this action had been taken, it brought into focus an entirely new aspect of organization development. With over-centralization abandoned, the top team discovered they had nothing to do. There were no projects in which to engage that held any degree of meaning. Top team members had little experience or skill in doing anything other than managing the ongoing operation, fire-fighting, and reacting to crisis. This led to the advent of Ideal Strategic Corporate Modeling, the Phase`4 activity in Grid Organization Development; an important event in integrating sound behavioral dynamics with true effectiveness on the corporate side of decision making.

As this period of time played itself out, I constantly made notes as to the dilemmas I confronted. Upon returning to my apartment in the evenings, I would dictate these daily experiences and then send them to Jane, who remained at the University of Texas. Upon my return to Texas for the occasional weekend, we would take this opportunity to discuss my notes further and to design intervention strategies for the coming week. As a result, Jane became very involved in this project

and joined me at the Bayway Refinery for purposes of systematizing the Ideal Strategic Modeling phase of this OD effort. Eventually our accumulated notes told a very significant story, which truly became the advent of Organization Development as a systematic activity. This was published in *Diary of an OD Man*[6] by Jane and myself in 1976.

The Research Phase

The vast majority of my university period was devoted to the investigation and study of social-psychological problems from an experiment-based point of view. The character of this research can be evaluated in the appended bibliography, which identifies some 150 journal articles, almost all published jointly by Jane and myself and sometimes including other parties as well. The nature of this research centered on leadership behavior on the one hand and conformity and deviation dynamics on the other.

These two topics—leadership and conformity—became the focal points of the business we embarked on for the next twenty-five years. As I now look back, I can see how fundamental it all was to what came later. Perhaps the primary reason for this is that formulating researchable hypotheses and then, through experimental designs, testing their validity proved to be a very sobering experience. We discovered that the way things work in everyday life rarely conform with initial hypotheses as formulated. To us, this was a startling revelation. We learned the importance of expanding the time to make possible more rigorous formulation rather than just accepting common sense explanations or what appeared to be reasonable and taking that at face value. It compelled us to engage in microscopic examination of the phenomena at hand while at the same time maintaining a macroscopic perspective; that is, seeing the problem within the class of problems of which it was but one representative.

While some professors delved only in the research angle, Jane and I were members of that smaller group that also loved to write. The danger in writing, of course, is that it submits one's thinking to the evaluation and criticism of others. This is a difficult hurdle for many to cross, but Jane and I seemed never to have a difficulty with this. We valued the enlightenment provided by constructive discourse with other professionals.

The manner in which Jane and I wrote is noteworthy as it is a bit unusual. Someone recently asked me to describe our approach. Their logic-based query was, "How did the two of you write? Did you do Chapters 1, 2, and 3, and then did she take Chapters 4, 5, and 6? Did

you then pass them back and forth? Did you take the lead, or did she? How did you combine effort?"

This question, which in fact reveals how many jointly authored articles and books are produced, could not have been further from the mark. Rarely did either of us compose a piece of isolated writing to be handed to the other for criticism and review. Rather, we sat together at a large writing desk, long enough that both of us could sit side by side. Then we discussed, analyzed, formulated, and finally wrote, but always simultaneously and together. Sometimes, after things had been framed to a point where they seemed to provide an acceptable formulation, I would dictate to Jane. Or, vice versa. Even this description fails to capture the full character of our work; it is difficult to convey in words the spontaneity with which we interacted. We always felt free to interrupt the other midway through a sentence in order to express the thought more clearly or to reshape it and restate it in a different way. So interdependent were our thought processes that more often than not one spoke the words that resided in the other's mind. It was a total union of effort.

Working with Others

Jane and I had every desire to keep expanding the frame of reference within which we were thinking and analyzing problems. In order to accomplish this end, we engaged in a series of joint projects with other professors, several of whom hold particular significance.

In 1949, I conceived of a symposium to be conducted one meeting per month over the entire academic year at the University of Texas consisting of nine different distinguished persons, one presentation per month. Several of these in particular merit further comment as they directly or indirectly influenced our thinking and later work. Alfred Korzybski, founder of the Institute of General Semantics, was invited to conduct one of these sessions. His contribution to my career is significant because of the fundamental character of General Semantics for understanding human thought. Only now, almost sixty years after the publication of his seminal work, *Science and Sanity*, in 1933, is he being accorded the respect that his contribution deserves.

Two illustrations provide an indication of Korzybski's thinking. One is the dictum, "The map is not the territory," indicating that human perception of a phenomenon is not equivalent with or identical to the phenomenon itself. Therefore, it becomes imperative to understand human thought processes as they reflect how human experience comes to be represented in a "map" which represents an event rather than providing a true replica of it. It is our *interpretation* of an event rather than

the event itself. Furthermore, your interpretation of an event may differ vastly from my own interpretation of the very same event.

A second contribution by Korzybski is the central emphasis placed upon processes of generalization from the concrete to the abstract, showing how abstractions necessarily omit features of the phenomenon itself. And, the higher the order of this abstraction, the greater the omission of selective character of details of that phenomenon.

The legacy of Korzybski's thinking resides in a book organized by myself and Glenn Ramsey as an outgrowth of the 1949-1950 Clinical Psychology Symposium held at the University of Texas at Austin. The book is entitled *Perception: An Approach to Personality*[7].The distinctive aspect of Korzybski's contribution lies in the fact that he had completed the writing of his particular paper but he died eight days prior to its delivery. A close associate of Korzybski, Charlotte Schuchardt, read the paper in his behalf as originally prepared. It was a memorable occasion for all who attended, hearing described posthumously the scientific conclusions that this man had reached. This particular chapter, "The Role of Languages in the Perceptual Processes," became a classic in the field of philosophy, and more specifically among General Semanticists. It continues to be published and republished in different languages; the most recent being in Spanish, which has just been released.

The other members who took part in this Symposium constitute an interesting group in the light of modern history for they turn out to have become illustrious contributors in the field of psychology. Included among these were Carl Rogers, world famous for his formulation of client-centered therapy; Norman Cameron, an important contributor to clinical and psychiatric thinking; Else Frenkel-Brunswik, a key contributor to the justly famous *Authoritarian Personality*; Jerome Bruner, one of the leading spokesmen of social psychology; Frank Beach, a leading biological level investigator of psychological phenomena. Ernest Hilgard, President of the American Psychological Association and famous author of a classic elementary psychology text, and Urie Bronfenbrenner, another world renowned child psychologist.

Although Jane arrived after Korzybski's death, she later became quite involved in General Semantics and was an ardent student and brilliant interpreter of how these processes could enrich our own formulations.

Another colleague who greatly influenced our thinking was Harry Helson, a world respected psychophysicist, whose justly famous

Adaptation Level Theory has stood the test of time and in many respects has led to a reformulation of psycho-physical theory. For those who knew Helson, his interests knew no bounds, spreading across all fields of human endeavor. He has, however, been most noted for his contribution to psycho-physical quantification.

Helson's interest extended into social psychology and it is in this context that he, Jane, and I began our collaborative efforts. Together we published several articles demonstrating how Adaptation Level Theory serves to unravel the influences that determine human social conduct. Stated simplistically, the theory identifies three sources of influence on any human outcome. These can be identified as stimulus, or the focal event being attended to; background, those antecedent experiences that relate to the focal event; and residual, the enduring and more or less fixed responses operable in a person and reinforced over time that relate to the phenomena being studied. Helson accorded great significance to the second of these sources of influence, background. As a result, our experiments were ones in which we kept the stimulus constant but varied the background factor. Two illustrations provide an understanding of how this was accomplished.

In the first, students were invited to participate in a number of experiments. In each case the stimulus remained constant while the background was changed in systematic terms. For example, in one condition, students learned that if they volunteered for an experiment, the obligation of one examination would be waived. In some cases students knew this in advance; in other cases not. In still other examples, volunteering brought no particular gain; and so on. In other words, by systematically varying background, we learned that the rate of volunteering could readily be shifted. In one condition, over 80% volunteered, whereas in another, less than 20% did so.

In a second experiment, concerned with donation as an act of contribution, those to whom the request was made received a standard invitation. Under one background condition, the names of donors were added to a blank list attached to a clipboard; that is, as yet no one had been credited with the act of donation. In this case, as well as others, subjects could readily observe the clipboard. Under another circumstance, subjects were able to note that ten people had already made donations. In a third, 25 could be seen as having contributed. Therefore, the background element varied in terms of whether it indicated number of donors, who those donors were, how much donors had given, etc. We found that the subject was greatly affected by these vari-

ations, thus demonstrating that the same stimulus can produce different behaviors as a function of the background conditions under which the stimulus is experienced.

In summary, as a result of this collaboration, Harry and I became close friends. I found him one of the most stimulating associates of my career because of the character of his thought, the range of his interests, and his dedication of effort.

A third colleague who exerted great influence on my professional development was Muzafer Sherif. I knew Sherif in a formal way from the earliest years. Therefore, in the Fifties it seemed to Jane and me that it would provide a mutually stimulating experience for the three of us to work together. I arranged for him to come to the University of Texas as a visiting professor, which he did, during the years 1955-56.

Many of Sherif's influences are of memorable importance, but two stand out in particular. One of these is his work on intergroup conflict, perhaps best known in the field as the Robbers Cave study. This consisted of an investigation of how two groups of young boys behaved toward one another on first and then upon repeated contact when neither group was previously aware of the presence of the other group within their same "psychological space." The space in this study was the Robbers Cave State Park in Oklahoma where the youngsters were encamped for a two-week summer vacation.

This experiment had just been completed when Sherif arrived in Austin. As related to Jane and myself, it had the effect of stimulating our long-term interest in this field of research and endeavor. Sherif established that conflict is the "natural" reaction of the members of a group when they come in contact with the members of another group under the conditions described above. The deeper aspect of this experiment arose in the opportunities afforded for studying strategies of de-escalation. Sherif found only one condition that held the promise of significantly contributing to de-escalation; that is, creating conditions under which the youngsters experienced a superordinate goal. Only then were previously held competitive attitudes diminished because of the higher stakes of cooperation with each side collaborating with the other for the purpose of superordinate goal achievement. That is, the groups came off of their fixed positions when there was more to gain by virtue of letting go than by holding on to vested interests.

A significant limitation in the Sherif approach to superordinate goals lies in how these goals come into existence. In the experiment,

he found it necessary to contrive conditions that made superordinate goal achievement imperative to the youngsters. While this is an acceptable basis for experimental work, in real life it is not common nor is it desirable for someone on the outside of the circumstance to mastermind conditions that make superordinate goal achievement desirable and/or inevitable. This, however, is the only limitation as we see it in an otherwise brilliant experiment.

Over time, Jane and I sought to rectify this limitation and I believe have successfully done so. This is true at least to the point where amount of cooperation needed from any two contending groups at the beginning of superordinate goal formation is only a commitment to sit down and explore such a possibility. This extension of the Sherif work is currently being published in a *Festschrift* volume honoring Sherif's many unique contributions to the field of social psychology.

It seemed to us that Sherif had come to the heart of the matter of many of the conflicts that operate within organized society: union-management conflicts; those between departments or divisions of companies; tensions between headquarters and subsidiaries; and even the cleavages that exist between nations. Because of its importance, I arranged for Muzafer Sherif to attend sessions in some of our later work in Exxon, and he was much admired by managers from that organization for his conceptual analysis, spirited thinking, and dedication to grappling constructively with problems with which they, too, found themselves entangled.

The other significant aspect of Sherif's impact on my thinking transpired during a seminar jointly conducted by him, Jane, and myself and attended by approximately 30 graduate students. This particular seminar turned out to be one of the most stimulating of graduate courses available at the University of Texas during that time. The title of the course was "Properties of Groups," and the objective was to identify the necessary and sufficient variables for providing an inclusive framework to analyze the properties of any group. We were able to identify several of these variables which lie at the very heart of organization development. One is power and authority in that no group exists without differences and variations in the strength of influence exerted by its individual members. Another is norms and standards; that is, the traditions, precedents, and past practices groups come to accept as second-nature. A third is morale and cohesion, or the extent to which people feel drawn to the group; and, finally, goals and objectives as the identified purposes for which the group exists and toward which it strives.

Since that time we have found it necessary to add only three additional variables to complete our formulation. One of these is structure and differentiation; that is, the extent to which the roles and responsibilities of individual members are made detailed and explicit. Another is feedback and critique, or the ability of a group to make use of the reactions of its members to current and past conditions in order to determine how action should be taken in the future. The third is context; the location of the group in the larger context. These seven variables characterize how a group functions and lie at the center of our approach to organization development and team building.

A fourth colleague who falls in the category of those who have significantly influenced my professional career is Jacob L. Moreno. I met Moreno—founder of sociometry, psychodrama, and action-oriented psychotherapy—during my Tavistock period.

Moreno's primary stimulation came from his hands-on approach to seeking solutions to problems of life. Currently, psychodrama is probably the best-known of his contributions, but sociometry is another seminal area, more of which will be revealed through future investigation. The same is true of a number of other areas of endeavor for Moreno.

My memories of Moreno are reflected in numerous aspects but a key one of these involved his psychodramatic sessions in which problems, personally damaging to the effectiveness of people or their spirit, were subjected to in situ re-characterization. I attended many of these Friday night sessions with him in the Fifties when I found myself frequently in New York working with Exxon on the Bayway project. The actors were called upon to portray the problem they were experiencing and then, by degrees, a stage of supporting actors was set in place. The problem was reenacted. Finally, in a third part of the psychodramatic session, the issues involved could be dealt with in a more rational manner.

Moreno's insights were not only unique, they were brilliant. Many participants felt they acquired an entirely new perspective for evaluating the dilemma they confronted by virtue of participation. For others, it was only a beginning as they embarked on the process of resolving the internal emotional struggles that would need to be resolved before they could successfully grapple with a problem only recently confronted or recognized. In either case it often proved to be the return to prior effectiveness and healthy spirits.

To illustrate the spontaneity involved in this work, I can recount one session in which I was asked to participate. Two new parents were

having trouble with an unruly infant and found themselves locked in conflict as to the better approach to dealing with the problem. To reenact the situation, such an infant was required to recreate the crying and wailing in order for each parent to demonstrate his or her preferred solution. A child's bed was contrived and I became the infant in question. The learning for me was dual. Not only did I gain insight into how readily one can portray such a role, but I also gained a great appreciation for the plight experienced by parents who are unable to cope with such a situation and who, by virtue of turning their frustration toward one another, find themselves engaged in battle. One or the other parent was finally able to deal with me in a way that proved satisfactory and I quickly went to sleep to the great relief of all involved.

As I analyze these several colleagues and seek to identify what they held in common that might have appealed to me, I certainly find no immediate answer in terms of the subject matter. Helson, the rigorous psychophysicist, and Moreno, the spontaneous interventionist, stand at opposite poles. What was the appeal, then, of their thinking that caught my attention? It seems that what I admired most in each of these persons is in some way related to how they went about formulating concepts rather than the specific area of application to which these concepts had relevance. Helson's background factors are very similar to Sherif's frame of reference concept developed in the early Thirties. Korzybski's "Map is not the territory" is in many ways a restatement of what these investigators set out to demonstrate.

A second aspect of these four to which I was strongly drawn was the spirit of their dedication. Each lived his professional life to the fullest and I think found fulfillment in his respective professional work.

The Business Years

The business years might be dated from 1964, when Scientific Methods was registered to the present. I served as President from that time forward until 1984, when I vacated my position, passing it to Jane. I became Chairman, an activity which I continue to this day.

The founding of Scientific Methods, Inc. as a business entity can almost be described as a casual occurrence, responding to empirical need rather than entrepreneurial initiative. What actually happened was that I needed funds (somewhere in the neighborhood of a couple of thousand dollars) to carry on the Baytown work, primarily from the

standpoint of printing and employing clerical helpers to facilitate our effort. The money could not properly be accepted by the University of Texas since it was not intended for research but had a specific non-university purpose behind it. Therefore, it was necessary as well as wise to establish a corporate shell that could receive and distribute the money independently of the public university body. We did this and it proved to be an immediate solution to the problem at hand. It is interesting to note that about this same time we were conducting experiments in which the query was whether or not the ideas inherent in the Managerial Grid and our approach to team building and organization development were more or less unique to an oil company setting and therefore restricted to its use or whether these ideas could be generated across the various categories of business.

As described earlier, the Exxon experiments coincided with the University period. In the beginning, they were activities of the summer months; this created no problem in terms of my participation. Once the Bayway project got underway, however, it called for a commitment of two years, and I arranged two successive leaves of absence in order to engage in the project. However, prior to this, I had taken an almost two-year leave of absence for the Fulbright work at the Tavistock Clinic, and it seemed to me that my outside interests were far outweighing my teaching career.

I finally came to terms with this problem when invited to conduct the third Exxon experiment, this time at Baytown in Texas. This was to be a totally absorbing project because these three experiments in the succession of a decade had provided a basis of building one on the other and thereby coming to a point of understanding of organization dynamics that would otherwise not have been subject to achievement.

During the Baytown project I received an invitation to carry on the same kind of work within Exxon, but now in the Far East. This too was a turning point. I decided to terminate my professorship at the University of Texas and to use Scientific Methods, Inc. as the vehicle for carrying on this applied work.

We soon found ourselves being called upon by banks, pharmaceutical companies, mining companies, chemical industries, high tech companies, service companies, private as well as non-profit enterprise and public agencies, etc. In responding to these invitations, it was not difficult to conclude that the ideas involved in our approach were quite applicable to a variety of settings and unique to no single area of pursuit. Coupled with application projects in Canada and Great Britain,

we soon recognized that the concepts and methodology not only found congeniality in the American culture but held equal interest on a broader scale. In time, this took on an international scope as it led to projects in Japan, Indonesia, Saudi Arabia, Iran, western Europe, South Africa, Venezuela, Brazil, and Uruguay. All these influences were in the process of convergence, demonstrating to us the wisdom of founding Scientific Methods, Inc. and developing it as a worldwide company. The most recent additions are the Soviet Union, where this particular segment of writing is being composed, and in Korea, where an earlier section was in process some months ago. Most recently, a seminar was concluded in Thailand, the first in that nation, and, once again, the concepts were fully embraced. In fact, participants drew a number of parallels between the 9,9 Grid style and the Thai "ideal model" of living; itself heavily centered in Buddhism.

From this widespread degree of generalization, we concluded that effectiveness in mobilizing human resources for productive outcomes is independent of the political system within which problems exist, the religion, the unique culture, national history, and other specialized factors. Fundamental effectiveness in any setting or among any people calls for the same insights into how to work with and through others, regardless of who the others are or what their specific life experiences have been.

Though I greatly enjoyed the application opportunities afforded by the founding of Scientific Methods, Inc., I readily acknowledge that I did not find great pleasure in being responsible for conducting the business. There is something of a contradiction here; that is, taking pleasure from helping others to be effective in conducting their own businesses while finding little personal pleasure in conducting my own. I can only account for such a discrepancy in one respect. It is that being responsible for application projects, which has afforded the opportunity over the years of gathering much data, has enabled me to write in order to crystallize the learnings as they developed, and has offered the possibility of rich and engaging discussions with those in the academic and business world alike. All this has provided full satisfaction for my research and development interest. On the other hand, preparing an annual tax report for the company or spending time in thinking through how it might best be structured or organized were not problems with which I cared to deal. In this sense, Scientific Methods, Inc. may have suffered over the years in terms of its own growth and development by my predilection for research, but I suspect there is a

certain inevitability here. With so many opportunities, choice becomes necessary and, as choices reflect one's basic values, my choices clearly reveal mine.

The Origin of the Managerial Grid®

Given our bent for theory-based formulation of hypotheses, predictions, and experimental tests, it is not surprising that Jane and I tried to create a conceptual framework within which to analyze concrete problems. This resulted in the development of the Managerial Grid.

It happened this way. In the beginning of the Bayway, New Jersey project in Exxon, I immediately attached myself to the top team in the company. Whenever his top team met, I met with it. Often I accompanied one of the members of this team back to his home department in order to see how he reported to his own people what had gone on in these top team meetings. For a period of some two or three months, I continued to attend these sessions, never participating, not even as a process facilitator, but merely being present as a silent observer. My goal was to learn as much as I could about the team and its individual members, its polarities, interpersonal conflicts, and operating difficulties. No experience could have been a richer source of raw material for our future work.

It seemed to me that the problem was rooted in how concepts of power and authority are employed as a means of integrating the available human resources in order to achieve productive outcomes. I was aware of the major leadership theories of the time. In my own estimation, Lewin's autocratic, democratic, and laissez-faire formulation was in the right direction but as yet it was too undeveloped to provide the precision and understanding necessary for really knowing what was going on. In our own work, the laissez-faire style appeared to be absent among this top group of managers, as did a "pure" autocratic style. Furthermore, the democratic approach didn't seem to provide an effective characterization for the process as I observed it. Though McGregor's *The Human Side of Enterprise* had not yet been published, I was aware of Theory X and Theory Y from my many personal contacts with McGregor at NTL and elsewhere. X was too strong a formulation to characterize the give-and-take that occurred within this key group; yet Y was not strong enough. The lack of fit was not a matter of degree; it felt much more to have its origins in the hard extreme of X and the soft, non-conflict facing components of Y.

We were also aware of Fleishman's work on *structure* and *consideration.*I saw this theory as a more complex one which afforded a greater number of options. Therefore Jane and I made a concerted and determined effort to test its utility. As has been shown by the previously studied theories, the Fleishman approach, particularly when viewed from the perspective of the methods of quantification used, separates and isolates the two variables. Structure means just what the label designates: at the "high" extreme, it means telling a subordinate what to do, when to do it, how it should be done, etc.—all extremely rational, logical, and one-way information. On the other hand, consideration at the high end indicates the giving of rewards to subordinates for doing what they have been instructed to do. This entire theory had all the hallmarks of paternalism. The high point of intersection (high structure-high consideration) constitutes a theory which in many respects is "destructive" of involvement, commitment, dedication, and particularly of creative or innovative thinking about how problems might be solved in a better way.

None of these "worked" in helping us to better understand at a conceptual level what was taking place in terms of group dynamics.

On the occasional weekend when I would return to Texas, Jane and I took the opportunity to review whatever dilemma I was presently confronting. In truth, this was a continuous review because, as already indicated, upon the completion of each work day, I dictated as fully and comprehensively as I could what had transpired. If the top team had a meeting, I provided a blow-by-blow account of the details. As a result, although Jane continued her work at the University, she was able to acquire vicariously something which directly paralleled my own participative experience.

As Jane and I persisted in our efforts, we slowly came to realize that the dimensions of the Fleishman graph were the problem. They were behavior dimensions, thus causing the reductionistic problem as described above, i.e., the creation of two isolated variables followed by an effort to add some amount of each in order to gain an accurate picture of leadership.

As we pondered this problem, we came to realize that the dimensions needed for an effective description of operational conduct are attitudinal variables, not behavioral variables. Thus, we identified a horizontal axis, "Concern for Production," reflecting an underlying attitude toward achieving results. We designated the vertical axis as "Concern for People," meaning the character of thought and feeling

one experiences in exercising leadership while working with and through others. We expressed differences in the magnitude of concern on a scale of 1 to 9. This enabled us to talk about the character of leadership that might be manifest when different degrees of the two concerns are observed to be operating in the leadership conduct of any individual.

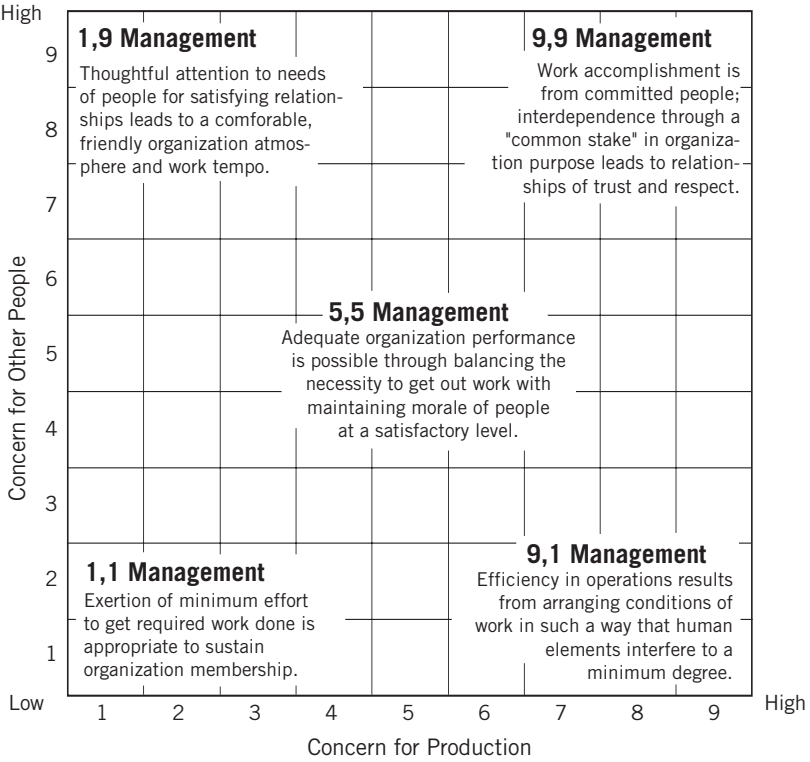
Though we had pondered the dilemma of how to conceptualize leadership for many months, even years, the decisions that resulted in the Grid itself all took place in a very short span of time, perhaps some 30 minutes. These arrangements permitted us to say, "What would 9,1 leadership be like?" Immediately we could answer, "A boss who has a very high concern for getting results simultaneously present with a very low concern for people would certainly be expected to use people as tools of production rather than as human resources who themselves are capable of contributing to problem solving." The same could be done for 1,9, 1,1, 5,5, and 9,9, and a number of variations on these five basic styles. We had no difficulty in saying, "That style fits him to a T," or, of another person, "His basic thinking is in the 1,9 corner," or, "She constantly pushes for intermediate solutions, seeks a compromise, even though it's obvious that no one is ever truly satisfied."

This describes the origin of the Managerial Grid as it fell into place sometime late in 1957 or in 1958. We immediately started writing to crystallize our thinking, using this as the basis for further observation, which in turn led to greater refinement. By 1960, feeling ourselves on solid ground, we wrote the book that was published as *The Managerial Grid*[8] in 1964. Seven years of intensive work had served to shape it, and we now felt assured that the theory had reached a point of stabilization.

Two later editions of the Grid have been published, one in 1978 and one in 1985[9]. A new book was released in 1991, not as a fourth edition, but rather as a comprehensive rendition of the theory. This book is entitled *Leadership Dilemmas—Grid Solutions*[10].

While this latest book is an entirely rewritten statement of the Managerial Grid, in no way does it alter the fundamental character of the original 1964 book. The dimensions as originally formulated continue to be sound. The use of a numerical system of expression has proven equally useful. Centering upon conflict in boss-subordinate relations remains fundamental. In fact, so central is this element of behavior that it has been elevated to a position of greater importance than in previous editions. In all other respects, though, the structure of thinking of the original book has stood the test of time.

The Managerial Grid®*



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What the new book does offer is some significant extensions of previous thinking. The most important is the expanded treatment of a third variable concerned with *motivation* as central to the core of thinking about leadership. This third dimension runs through each Grid style, from a plus end which characterizes what the person is striving to accomplish, through a neutral zone, which intersects with the Grid surface, to a negative pole, which designates what the person is seeking to avoid in his or her leadership conduct. As an illustration, 9,1+ is the "desire to control, master, and dominate the situation," and 9,1- is

"fear of failure." The motivational dimension provides a further degree of clarification for the motivational situation being experienced by a person managing in a 9,1, 5,5, or 9,9 way, etc. It opens up new possibilities for research that hold great promise.

A second major change in *Leadership Dilemmas—Grid Solutions*[11] is the introduction and expansion of paternalism and opportunism as two major Grid styles, making a total of seven. Paternalism and Opportunism were included in the original 1964 version of the book, but only alluded to in paragraph descriptions. As we have learned more and as the world has progressed, we have come to see these two theories as having far greater importance than previously thought. For this reason, they have been elevated to a comparable status with the original five Grid styles. Facades, also presented in first book, needs renewed emphasis in the fifth, but that was not recognized in writing the fourth rendition.

A third major shift lies in the means of illustration provided to the reader to aid in the concretizing of the concepts. The book is presented as a story which portrays each of the major Grid styles as it is characterized by one member of a work team, including its leader. This team and its members are introduced in the beginning of the book and carried through each of the chapters. The final chapters of the book provide a culmination of the story through a demonstration of Grid Organization Development as we would guide it through an organization.

There are some that say the Grid has become the unofficial leadership theory of the day. It is true that in the early years many attempts were made to pinpoint presumed weaknesses or limitations. As each of these appeared, Jane and I reexamined the underlying thinking of the Grid to determine in our own minds whether or not a fundamental limitation had been identified. Fortunately, we have never had to introduce a correction in what was presented in the 1964 edition by virtue of these ongoing efforts.

One frequently-alluded-to limitation, however, is worthy of comment. It is claimed that the Grid does not deal with situations that are different from one another. The argument is, "How might a person be 9,9 in two very different situations?" Our answer is illustrated by taking a 9,9 example. A 9,9 approach to exercising leadership, for instance, is applied in a far different way with a 50-year-old seasoned executive than it is with an 18-year-old new hire. The differences, however, are tactical in nature and relate to the situation itself. They are not differences in the style of exercising leadership—9,9 remains the con-

stant strategy. The 50-year-old application is obviously different than the tactic most pertinent for dealing with an 18-year-old newcomer. For example, in the former case, goals may be set that extend over a year, or two, or five. With an 18-year-old, no shift is made in the manner in which goals are set; that stays the same. The time frame, however, varies; it may be a day, a week, or a month. In other words, the 9,9 principle remains constant; it is the 9,9 tactic that varies, depending on the specific situation. The 50-year-old has a wealth of experience and can engage in goal setting over an extended period, whereas the 18-year-old has a very limited experience. If asked to set goals beyond one's time or range of experience, it would become a futile effort. Thus, the Grid is a strategic approach that fits all situations, but each of the applications are situationally unique. The situationalist criticism of the Grid claims that style should vary with the situation but this is simply not justified by the facts. Nor is it consistent with sound principles of human behavior.

The Grid formulation has enjoyed worldwide interest and continues to do so on an ever expanding basis. The importance of this from an autobiography point of view is that it has enabled us to study and learn much about the exercise of power and authority across many different cultures, nationalities, religions, and economic systems.

Our conclusion is that human problem solving is not susceptible to distinctiveness based on any of the above factors. When two or more people are engaged in problem solving, the critical issue becomes that of relying upon effective human inquiry as to the nature of the problem and the facts surrounding it; open, clear, and candid advocacy of points of view between people who are engaged in collaboration; conflict solving by confrontation and resolution of differences rather than relying on other means of getting around the conflict problem; the full reliance on feedback and critique as a means for ensuring that interpersonal processes are healthy and sound and that progress toward resolution is being achieved. None of these can be characterized as American, European, or Japanese. Nor can they be called Christian, Jewish, Moslem, or Buddhist. They have no unique racial character. Nor do they belong to a single ideology—free enterprise, communism, or socialism.

These processes of human problem solving are deeper than any of the above considerations. They are inherent in solution seeking. They constitute the raw materials from which scientific methodology has been shaped.

It seemed to Jane and myself that the conclusion formulated here is truly of significant proportions because it leads to the implication that only when human problems of collaboration themselves are formulated in terms that permit their resolution is it possible for genuine progress to be experienced.

Other Book Level Formulations

Over the years Jane and I sought to create a comprehensive statement of what we see as important in the future unfolding of Organization Development. Apart from the Grid, I would include the following.

Synergogy[12]

Synergogy is a statement of the learning methodology on which Grid OD is based. But Synergogy is much more than that. It is a way of rearranging learning conditions so that learners can be proactively engaged in their own learning while helping one another learn. It constitutes the single most radical formulation currently available in terms of providing an alternative to pedagogic training of children and adults alike.

When it is realized that pedagogy is a method of education premised upon paternalistic concepts, it can be seen how its use as a learning methodology creates dependent learners rather than people who are motivated to actively master the subjects being taught.

Synergogy has been resisted in educational circles, however, and a brief explanation is useful. Teachers have been extensively interviewed regarding their reactions to Synergogy. They are positive toward it as long as the teacher stays in charge of the learning process and is free to intervene at any point where clarification is needed or intervention seems appropriate. But it is just at these points that learners can learn the most by learning to overcome their own barriers to joint effectiveness by helping one another understand the concept or helping to diagnose the problem they as a team are facing which impedes progress. This places responsibility for learning in the students' hands, however, and removes that source of gratification from a pedagogically minded teacher. It is a difficult transformation for any purveyor of knowledge or skill to make.

Change by Design

In earlier editions, this major change model has been called *Building a Dynamic Corporation Through Grid Organization Development*[13] and *Corporate Excellence Through Grid Organization Development*[14]. *Change by Design*[15] is the latest edition of our work concerned with Organization Development. This book describes the principles, main phases, and probable outcomes available to any organization that engages in a full and comprehensive approach to Organization Development.

Consultation[16]

Consultation represents the fullest treatment of consultation strategies and interventions yet available. It covers all recognizable units of change, focal issues involved in an intervention, and the dynamic aspect of behavior which is involved. This book has become a manual utilized by consultants who need the stimulation of seeing alternative ways of going about offering service to a client. Even more importantly, it confronts the issue of the consultant dealing with the inclusive unit of change rather than simply locking in on individuals as the inevitable recipients.

These four books present the main outlines of our thinking about human behavior in organized settings and how it might be strengthened and made more effective.

Language and Expression Developments

At the beginning of Organization Development, there was a limited vocabulary for talking about organization phenomena. Jane and I found a number of words helpful in expressing what we saw to be important. None of these enjoyed currency at the time, but all are in use today.

Organization Development

Organization Development is, of course, the first term that comes to mind. During the decade of the Fifties, there was literally no conception of organization development as it is now understood. Restructuring was one means by which organizations might be reshaped, but this involved levels of hierarchy, reporting relationships, etc.—the "mechanical" side of organization. In addition, there was

some sense of individual development, i.e., sending seasoned top executives and upper level managers off to prestigious business schools. But here, too, the point of concentration was on the mechanics of thinking about business rather than the more vital consideration of mobilizing human resources and bringing them more fully to bear on productive purposes of the organization.

We coined Organization Development to express the "organization" culture point of emphasis. What we meant by this term was the development of the organization as a whole entity. We were not concerned with some isolated application, a piece of the organization as might be implied by "organizational," but rather with the organization as an intact and whole system, itself a primary entity that could be dealt with in its entirety, albeit through a sequence of development actions rather than some inclusive, simultaneously occurring event.

Organization Development seems now to have taken hold as standard language in the field.

Style

There was no ready shorthand when we began for clearly identifying the unique properties for how a person exercises leadership. Calling it "leadership behavior" didn't solve the problem. Trait was unacceptable for a number of reasons. Finally, we settled on the word "style."

I have maintained some reservations about the use of this word that derive from its fashion industry usage. The word itself carries the notion of being "in" style or "out" of style, indicating that style is a preference that changes from time to time. We made no such assumption when we used this word in the context of leadership, as we see strong leadership, i.e., the 9,9 style, enduring over time and place and not subject to the whims of change. The 9,9 leadership style is the single best approach to solving the leadership dilemma. It has demonstrated value in promoting productivity, creativity, satisfaction, and health and, in this sense, it can be regarded as unbounded by time.

Backup

An empirical observation is that even the strongest leaders may occasionally shift their style of leadership and fall back on another approach for solving a dilemma. The word "backup" provides a basis for understanding something of the unstable character of leadership,

i.e., when one preferred style fails to produce the needed consequences, then a person may shift to another approach. These shifts into a backup style provide a means for understanding why people are not rigidly consistent in their actions but rather seek to adapt to barriers and resistances which they seem incapable of overcoming in any other manner.

Win-Lose

One of the interesting terms that has had an enduring and expanded significance since we first introduced it is the concept of "win-lose." Win-lose became the shorthand phrase we used for characterizing the aftermath of interaction between two groups in the research experiments we conducted where one group was positioned to win at the expense of the other.

This afforded an opportunity to study the consequences associated with victory and defeat. This research has been published elsewhere and need not be commented upon further, except to point out that many applications and extensions of win-lose have been created since that time.

Team and Team Development

Another point of emphasis in the field of human resource development strategies is "Team," emphasizing the unifying communality of several engaged in common pursuit. Developing a team's capacity for mutual problem solving, i.e., Team Development, as contrasted with aiding each individual isolated from the other to be more effective is another original contribution. The team is considered to be the least common dominator; seeing members on an isolated, one-by-one basis constitutes an unacceptable degree of reductionism.

Since the original use of these words, a number of others have served as substitutes, such as the phrase, "Team Building." We have reservations about Team Building because of the mechanical implications in the word "building," i.e., a block at a time, as contrasted with the more organic concept of integrated change contained within the word "development."

Conflict Solving

As we more and more centered our efforts upon conflict as the primary dynamic in human affairs, we felt the need to modify this word

in a manner that might express what we were seeking to realize, i.e., the solving of conflict. Thus, conflict solving became our way of expressing the important aspects that constitute the objective of studying such tensions in the first place.

Solution Selling

This same thinking was applied by us in trying to solve problems associated with the dynamics of selling. It seemed to us that what a person is actually buying in a sales relationship is a solution to his or her problem, whatever that problem might be. Therefore, if the salesperson is oriented to this aspect of the relationship, he or she can learn the skills of offering the customer solutions rather than simply describing various items or services for consideration.

Synergogy

As mentioned earlier, we attached great importance to the learning methodology that provides the delivery system enabling users to learn and use the various approaches that are embedded within Grid development. This methodology lies at the opposite end of the spectrum from traditional pedagogy and bypasses andragogic facilitation by placing the responsibility for learning in the hands of the learners themselves.

Synergogy came about in response to the need for a better and more acceptable educational delivery system than either pedagogy or andragogy could offer. Originally referred to as Instrumented Team Learning, this contrived word conveys the basic idea: "working together," from the Greek synergos and agogus, emphasizing "teacher." Synergogy thus refers to "working or learning together for shared understanding."

The need for a radical solution to the breakdown of education now confronting the nation goes far beyond simply helping teachers become better informed on their subject(s), or better communicators, or better disciplinarians. These are all symptoms of some deeper lying malaise in the classroom.

As Jane and I saw it, what has broken down is the concept of authority which is embedded within the classroom model. Adults and young people alike have repudiated authority in its many forms, yet the fundamental model of pedagogy and its variants rests on acceptance by students of the authority-obedience paradigm. Only by a complete shift

of responsibility onto learners themselves is it possible to arouse motivation to learn. This is done only partially by andragogy, when the expert maintains control of group process.

Synergogy approaches the educational setting from an entirely different perspective. The knowledge base is in books, instruments, and life experience. Learning the requisite model is through individual measurement followed by team testing *after* teams have reached agreement on best answers to set multiple choice and other question formats. Keys are then distributed. Individual and team performances are assessed, with competition between teams for "best," "next best," etc. in terms of learning improvement based on initial individual prework understanding measured against ultimate team insight.

Responsibility is centered within the learning team since no teacher or andragogic facilitator "tells" or helps the teams in their pursuit of learning excellence. Rather, a post scoring critique of team process, also instrument-guided, aids team members to evaluate what they need to do to strengthen team performance for greater or sustained success in the future.

Synergogy is uniformly embraced by learners as a more exhilarating activity than pedagogy, and one that stimulates more positive attitudes toward the learning itself. It has been applied all over the world with comparable enthusiasm. We saw it and I continue to see it as a fundamental solution to creating new and entirely different relations between the subject matter to be learned, the learner and the "teacher." I have no reservations whatsoever to this conclusion, but I should point out that it tends to stimulate great reluctance and reservations on the part of teachers. The explanation for this is that it removes the teacher from the authority-obedience equation and *appears* to lessen teacher control. However, upon fuller examination, it frees the teacher to create learning materials which is a far more demanding and rewarding use of his or her knowledge and skills. Many more students can be educated and in a far more effective and enduring way.

Postlude

This story is being completed, Christmas 1990, three years after Jane's tragedy, which also terminated almost four decades of my career. Since I had never worked alone for any substantial period, I seriously pondered my next move, which leads to the more recent part of this story.

Jane had an understudy during the last fifteen years, someone to whom she and we turned for help whenever we were dealing with a particularly complex or difficult project. This person is Anne McCanse who participated in many of our leading edge projects, including membership in the UAL cockpit project and the nuclear control room design.

Anne stepped in to help me bring to a close some of the projects Jane and I had underway. Rather than simply drawing these to conclusion, however, a new collaboration began to unfold with many unanticipated synergistic results.

Anne's teamwork differs in significant ways from the manner in which Jane and I worked together. Oftentimes Anne and I deliberate a course of action to verbal agreement, and then one of us creates a first draft. We then continue to work it through a conceptual and editorial phase, fine tuning it until completion.

As members of a profit making company, all of this must take place within the context of client work. This has involved extensive travel, taking us to Korea, Australia, the Philippines, Italy, Greece, Brazil, Venezuela, Thailand, and the USSR, as well as to several locations in the US during this same period. We have managed to write two more books during this time, several articles, and numerous projects still in the drafting stage.

The writing of this autobiography reflects this same model of our interdependence.

A main reason for writing a personal story of this sort is to learn from it. For me this lesson involves how human effectiveness emerges and how it might be enhanced. The learnings that seem of greater importance include the following.

1. Conceptual formulations, presented in written form to stimulate public scrutiny, constitute the main opportunity for making enduring contributions.

2. The synergies possible from effective teamwork provide access to much creativity that otherwise would be lost or not even recognized.

3. Intellectual teamwork based on openness and candor, confronting and resolving conflicts, extensive reliance on feedback and critique can further substantially the elimination of false assumptions, thereby further increasing the quality of contributions.

4. Continuity of teamwork is possible even though memberships may shift.
5. Professional teamwork need not be limited to a few people working together over long periods of time. Much synergy is possible in terms of more limited collaborations, as is suggested in my work with Sherif, Helson, Bradford, and Moreno.
6. Similar gains are possible from "joining" with historical figures through taking their assumptions and testing them as the premise of one's own actions, as in my regard for Darwin, Lewin, Freud, and Korzybski.
7. Satisfaction from effort comes far more from the processes inherent in teamwork than in its products or its achievements.

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