IMPROVISING AND MUDDLING THROUGH

By

Victor H. Vroom

In recent years I have been involved in the teaching of a course at Yale called Individual and Group Behavior. IGB, as it has been affectionately called by the students, is an experiential course designed to encourage personal reflection as well as mastery of psychological concepts and principles. One of the activities that has historically been a part of that course is a lifeline exercise in which students are asked to draw their lifelines, including points marking birth and death, key choice points and the external events which helped to shape them. While initially resisting the activity, it is customary for a student to derive a great deal from stepping back from the day-to-day events which are the usual focus of attention and reflecting on the large questions of the trails they have taken and of those they have yet to take.

Writing this chapter is a comparable experience for me. It has given me an opportunity to sit back and reflect on what at times seems like an incredibly busy schedule and to attempt to make sense of where I have been and where I am going. The reader is, of course, free to look for his or her own patterns in this and in other chapters which comprise this volume. One conclusion which I have reached from examining the data which follows is the paucity of long-term planning that has characterized my own career. Chance has overwhelmed purpose in influencing the choices with which I have been presented as well as the outcomes of the choices that I have made. What little planning that has occurred has been short range, like the novice chess player who looks at most two steps ahead in choosing the next move.

My Yale colleague, Ed Lindblom has given a name (Lindblom, 1959) to this phenomenon which he feels characterizes much of organizational decision making, despite what normative theorists argue should be the case. He calls it "muddling through." Perhaps I am deceiving myself or I am being unduly modest. Perhaps the astute reader will see deliberateness and the pursuit of long-range goals in the pages to follow. Perhaps in attributing my behavior in large measure to situations with which I have been confronted, I am committing what psychologists have come to call "the fundamental attribution error"the tendency to see the causes of one's own behavior in the environment and the causes of others' behavior as residing within them. On with the tale.

I was born in the heart of the Great Depression. My father, who had been a naval officer during the First World War, was fortunate enough to maintain his employment at the Northern Electric Company in a plant that had been described to me as the Canadian counterpart of the Hawthorne Works. My mother was born in South Africa where her parents had settled after her father retired from the British Army to pursue interests in gold and diamond mining. I was the youngest of three boys; Alan was 12 years older, and Kenneth 5 years older than I.

As a child, I was not conscious of any scholarly ambitions or pretensions. Learning was something that came easily to me but was not something that I pursued with much passion. My passion was rather preserved for music. At about age 10, my mother bought me a $25 clarinet, later to be followed by a $40 saxophone. For the next 4 years, the Vroom household was "treated" to an endless set of squeaks, groans, and then, ultimately, notes and chords. When I was not practicing my instrument, I was listening to recordings of Benny Goodman and Artie Shaw or Johnny Hodges and Charlie Parker. Growing up in the suburbs of Montreal during the "big band era" it was possible to see all of my favorite performers live. I was, typically, the tall kid standing by the bandstand gazing with awe at my heroes effortlessly executing passages to which I could only aspire. My bedroom, in which I practiced as much as 10 hours a day, was adorned with autographs of my favorite performers.

By age 15, I was finally good enough to be invited to join a local dance band called the Blue Knights. We all wore sky-blue blazers and navy blue slacks and played standard arrangements from the big band era. Even today when hearing Stan Kenton's "Intermission Riff," Glen Miller's "String of Pearls," or Tommy Dorsey's "Song of India," I find my thoughts drifting back to life on the bandstand and my fingers revisiting the passages I have played so many times.

It was a glamorous life for a high school sophomore. The other members of the band were all in college or college graduates pursuing successful careers. For the next 3 years, I played at least two or three nights a week in various dance halls and nightclubs in and around Montreal. My studies suffered but not to the point of jeopardizing my high school graduation, which occurred on schedule.

I must confess to not having given much thought to what I would do after high school. My two brothers had graduated from McGill University, and one had gone on for a Ph.D. in chemistry. However, I had never regarded myself or, in fact, been regarded by teachers as "college material." To further complicate the picture, my father took early retirement from the Northern Electric Company and did not have the financial resources to pay for my college education as he had done for my two brothers.

That fact did not upset my greatly since the thing I wanted most was to play full time with a "name" band. For me that meant crossing the border to the States. To be realistic, I felt that I must start with what was called a territory band which moved from city to city in a region of the country, usually in band buses. The ultimate objective, of course, was to end up with a "real" name band as had my idols Oscar Peterson and Maynard Ferguson, both of whom were Montrealers who had successfully made the transition to the United States.

I gave myself the summer after my high school graduation to plan the move to the States. In retrospect I am taken with the incredible naivety with which I approached the entire process. The difficulties in obtaining a work permit or finding a band that needed a 17 year old alto saxophonist who also doubled on clarinet, or even of learning to live away from a home that had nurtured me since birth were never confronted or successfully managed.

By late August of 1949, my father had grown impatient with my lack of progress in career planning and used his local contacts to get me a job as teller in the branch of the Royal Bank of Canada less than a mile away from home. My interview with the manager and observation of the job of teller quickly convinced me that this was not for me. In desperation I sought an alternative and took the advice of a fellow member of the Blue Knights to apply to Sir George Williams College. Situated on the third floor of the YMCA building in Montreal, Sir George, now called Concordia University, had a relatively simple admissions process and, of even greater significance, had a tuition of only $250 a yeara sum that I could easily afford from my musician's income.

Sir George was relatively unique among colleges in one respect -- all incoming students had to take a battery of psychological tests, including the Kuder and the Strong Vocational Interest Tests. The psychologist who presented me with my results informed me that my interests corresponded with two occupational groups -- musicians and psychologists. The latter was a surprise to me. I had only the vaguest of conceptions of what psychology was all about. However, I looked forward to taking a course in that field.

The opportunity to study psychology did not present itself at Sir George. However, the core curriculum was most exciting, and unlike my high school experience, I found myself pouring more and more energy into my studies. To this I credit in large part an opening lecture by Dean Henry Hall who pointed out that a college education was one of the few things that people would pay for but would not necessarily get. In college no one would prod me to learn. What I learned would be up to me.

The source of the impact of Dean Hall's remarks is still a bit hazy to me. Perhaps it was the fact that I was financing my own education; perhaps it was the relative absence of the external controls that had permeated my educational experience to date; perhaps it was the discretion that I was to be accorded as an adult. Most likely it was all three factors that contributed, for the first time in my 17-year history, to behavior that demonstrated "dedicated scholarship."

While I continued to play music at Sir George, I found that my mind was becoming my "principal instrument." Mastery was still the goal, but notes and keys were replaced by ideas and concepts. A common theme, however, was improvisation. I was never content to parrot back the ideas and theories of others. My college lecture notes reveal a persistent pattern of recording the key ideas expressed by the instructor on one side of the page and my personal reactions and thoughts on the other side of the page.

By the end of my first year, scholarship had clearly acquired the status at least equal to that of jazz music. I sought to transfer to McGill University and, to my surprise, was accepted with full credit from my year at Sir George. At McGill I took my first course in psychology from Donald Hebb, who had just completed his landmark work entitled *The Organization of Behavior* (Hebb, 1949). I was impressed both with him and with my exposure to psychology and by the end of my sophomore year had been admitted to a special honors program in that field. There were only three or four of us in that program, and we would meet weekly for lunch with "Hebb" to discuss psychological questions that we found interesting. My fascination at that timewas with philosophy of science, particularly the concept of determinism and its implications for free will, religion, and individual responsibility.

My budding romance with psychology complimented my continuing interest in music. By now the Blue Knights had disbanded but was replaced by larger and more professional orchestras -- this time with five saxophones, five trumpets, five trombones, full rhythm sections, male and female vocalists, and their own arrangements. The exciting, glamorous life of music continued and, in fact, enabled me to not only finance my own college career but also to pay rent to my parents in whose home I continued to live.

It was too good a life to cut short precipitously, and on graduation from McGill I elected to take a master's degree in psychology at that university. At that time McGill had just introduced two parallel tracks for graduate work in psychology. The academic track lead to an MSC and Ph.D. The professional track, which embraced both clinical and industrial psychology, lead to an MPS.Sc (Master of Psychological Science). I chose the latter path with a concentration in industrial psychology, even though I did not even know what industrial psychology was. I knew Edward Webster, who covered that subject, and he seemed like a nice-enough gentleman. Furthermore, industrial psychology seemed to offer possibilities other than academic research and teaching, which appealed to my urbane side.

In studying industrial psychology I was exposed to the works of Joseph Tiffin, Charles Lawshe, and Jay Otis. It seemed to me that I learned all there was to know about psychological testing, techniques of hiring and placement, job analysis, job evaluation, and the technology of merit rating. However, to my dismay I saw little if any connection between the academic psychology on which I had labored so diligently in my undergraduate years and this new field. Industrial psychology seemed more a set of techniques than it did the application of a set of theories and concepts.

One of the ingredients of the McGill program was a set of internships in which students had an opportunity to practice their new profession in the field. My first internship was at Canadair Ltd.an aircraft manufacturer in Montreal. There I worked on the development of weighted application blanks utilizing demographic measures to predict job turnover. I recall being upset that there was no similarity between the items which predicted turnover for Assembly Fitters "A" than for Assembly Fitters "B," and I couldn't figure out the rationale. However, my advisors urged me not to be discouraged. They sought unsuccessfully to convince me that the important thing was not theoretical consistency but the magnitude of r^2 or variance explained.

In the late summer of 1954 after my first year of graduate work, the International Congress of Applied Psychology was held in Montreal. Among the participants in that scientific gathering were Rensis Likert and Carroll Shartle. I began hearing about the University of Michigan's organizational behavior program and the leadership research then underway at Ohio State University. Once again there seemed to be an exciting world across the border in the United States, but this time it was the world of industrial social psychology rather than the world of big band jazz!

During my final year in the master's program at McGill, I interned at the Aluminum Company of Canada in their staff training and research division, This was completely different from Canadair and helped me to form a more complete picture of this new field of which I wanted to be a part. Here I was exposed to the works of Fritz Roethisberger, Carl Rogers, Douglas McGregor, and Paul Lawrence. I had ample opportunity to read in unpublished form the latest research being conducted at Michigan and at Ohio State, and I resolved to pursue the Ph.D. degree. I applied to five schools: Michigan, Illinois, Purdue, Ohio State, and Carnegie Tech. My first choice was Michigan, and I was delighted in March of 1955 to hear that they too wanted me.

Michigan was everything that I might have hoped for in a graduate education. Here at long last was the means of integrating psychological theory with application. I majored in social psychology where I was exposed to such professors at Ted Newcomb, Doc Cartwright, Dan Katz, Jack French, and Helen Peak. At the same time I worked in the Survey Research Center where I was concerned with applying social psychological ideas to understanding how to make organizations more effective. While he had died several years previous, the ideas of Kurt Lewin were very much alive at Michigan, and they had a pervasive influence on my thinking about individual behavior. Specifically, his reminder that behavior is a function of person and environment helped me to integrate in my own mind the social psychological emphasis on situational or environmental determinants and the prior McGill influence on individual differences. I was ready for Lee Cronbach's classic treatise (Cronbach, 1957) on the two disciplines of scientific psychology, which appeared toward the end of my graduate work.

Norman Maier was also a very strong influence on me at Michigan. My introduction to experiential learning occurred while serving as a teaching fellow in his course, Psychology of Human Relations, during my first year. His book, *Psychology in Industry* (Maier, 1955), was used as a text in that course, and his constant attention to the psychological underpinnings of behavior helped to reinforce my belief that the theoretical and the practical could be integrated.

My doctoral dissertation at Michigan, "Some Personality Determinants of the Effects of Participation," dealt with the moderating effects of two personality variables authoritarianism and need for independence on reaction to participation in decision making. It gave tangible expression to my interest in the interaction between person and environmental variables. Sometime after the completion of my dissertation, I received a phone call from Donald Taylor at Yale informing me that my dissertation was to receive an award from the Ford Foundation in its doctoral dissertation competition and would be published as a book by Prentice Hall. Needless to say I was ecstatic, and the red leather bound edition given me by the publisher still occupies a position of honor on my bookshelf.

That recognition encouraged me to pursue a more general formulation of a theory dealing with the interaction of individual differences and situational variables. That formulation came to be called "VIE theory" or "expectancy theory" when it was published several years later in *Work and Motivation* (Vroom, 1964).

While I was at Michigan, my career got another unexpected boost. Norman Maier was invited to write the chapter on industrial social psychology for the 1960 Annual Review of Psychology. He had planned to decline since he was going to be on sabbatical leave in Europe that year. However, he knew that I made a practice of keeping up on everything that was written in the field and asked me to do the literature search so that he could organize it into a chapter on his return. I went beyond the original mandate and wrote the entire chapter. He changed barely a word before sending it off to the editors.

It was around this time that I became aware of the fact that my exchange visitor visa status obligated me to return to Canada for a two-year period. This seemed most unfortunate since at that time there were no jobs for industrial or organizational psychologists in Canada. In desperation I went to an office in the university with responsibility for international students.

Together we devised a plan which they felt had a modest probability of overcoming my predicament. In the past whenever I had returned to Canada on a visit, I had in my possession a document from the university requesting a reissuance of my exchange visitor permit. This time the document referred to a "student visa" instead of "exchange visitor." (A student visa carried with it no such requirement of returning to one's native land.)

It was indeed fortunate that Ann Arbor is relatively close to the Canadian border. I recall spending a long weekend crossing into Canada at each of the border crossing routes and immediately turning around and presenting my document. Three times I received a stern reprimand from the customs official who insisted that I return to Ann Arbor to get the proper document. At the fourth try the customs official took pity on me and issued a new student visa. I was now free to enter the United States job market. I did my best to contain my jubilation until I was safely out of sight of the official lest he change his mind.

After I received my degree at Michigan, I was invited to stay on as a study director in the Survey Research Center and concurrently as a lecturer in the Department of Psychology. It wasn't something that I wanted to do for the rest of my life, but Michigan had been so good to me that I felt I could do far worse. Besides I had formed a small jazz combo called The Intellectuals, which continued to provide an outlet for my musical talents and rambunctious impulses.

I stayed at Michigan for another 2 years. It was a somewhat ambiguous status. On one hand, I was treated like a faculty member and invited to faculty meetings and gave colloquia and the like; on the other hand, I felt a bit like a senior graduate student who had stayed on past his time. In 1960 1 received an offer from the University of Pennsylvania to join their Department of Psychology. At that time Penn's department was in a state of resurgence after many years of inbreeding. The back bone of the department was mathematical psychology with Robert Bush as chairman and people like Luce and Galanter among the full professors. The sole industrial psychologist there was Morris Viteles whose 1932 book was a classic and required reading during my McGill days.

I recall some discussion of whether I might wish a connection with the Wharton School. George Taylor, who was then the Chairman of the Department of Geography and Industry, strongly encouraged me in this direction. I recall being tremendously impressed by his range of contacts in the field of labor management relations -- a field about which I knew remarkably little. However, I had been well indoctrinated by colleagues in psychology at Michigan with a disrespect of business schools which at that time was all too characteristic of faculty in the arts and sciences. I politely turned down the invitation.

While at Penn, I taught large sections of introductory psychology as well as courses in social psychology, industrial psychology, and motivation to both undergraduates and doctoral students. During this time, my major preoccupation was writing *Work and Motivation*. I should point out that I did this without a great deal of support from my Penn colleagues, most of whom felt that attempting to write a major book instead of journal articles was more appropriately a professorial endeavor rather than a task of a first-term assistant professor.

Nonetheless, I had no choice or at least felt I had none. Never before had I pursued any task (except perhaps learning to play the saxophone!) with such diligence and dedication. I spent every evening in the library and was frequently there until closing. I was compulsive about checking every reference and exhaustively surveying every item that could conceivably be relevant to my quest. Unlike my experience at Michigan, I was beginning to feel like an independent scholar. I missed the conviviality and stimulation of a group of colleagues with similar or complimentary interests but relished the freedom which Penn provided me to do my own intellectual work.

One of the casualties of my appointment at the University of Pennsylvania was my part-time musical career. Such "frivolities" were frowned on by my department chair and preempted by my intellectual preoccupations. My saxophone and clarinet became relegated to a distant corner of the basement to gather dust as remnants of discarded boyhood dreams.

In 1963 my three-year contract as assistant professor of psychology was up, and I began to be wooed by several other universities. This time the most attractive alternatives were not in psychology departments but in schools of business or management. Among the chief competitors was Yale, which had Chris Argyris, E. Wight Bakke, Bob Fetter, and Donald Taylor in what was then called the Department of Industrial Administration. Yale did not then have a doctoral program, but one seemed likely to be approved in the near future.

I came very close to going to Yale because it had many attractions but, in the end, decided on Carnegie. The opportunity to work with Dick Cyert, Jim March, Harold Leavitt, and Herbert Simon was just too attractive to refuse. I might point out that I would have chosen Carnegie more quickly had it not been for the requirement that I teach master's students in their program of industrial administration. I was still a psychologist first and foremost and still believed that there was something not quite intellectually pure about the mercenary interests of those seeking a career in business!

My early days at Carnegie were filled with mixed emotions. I found myself surrounded by colleagues in economics, operations research, marketing, and accounting -- each of whom had a different language and set of scholarly pursuits that I found difficult to encompass into my psychological compartments. For the first few months, I kept my office door closed and restricted my conversations with my colleagues to the exploits of the Steelers, the Pirates, or the latest office gossip. Gradually these social encounters acquired intellectual overtones, and by the end of my first year, I began to embrace the interdisciplinary exchanges which then characterized Carnegie's Graduate School of Industrial Administration (GSIA).

One of my most vivid memories of this period was a lunchtime faculty seminar on topology. My interest in this aspect of mathematics had been spurred by an apparent resemblance to some of the ideas of Kurt Lewin. A group of about ten faculty members agreed to buy a major textbook on the subject, and we devoted one lunch hour each week to exploring the subject. Each participant took the responsibility for one class. Now 25 years later, I am painfully aware of the fragility of this degree of open-minded collaboration among the disciplines. In fact the opposite -- veiled hostility and at times open warfare -- seems to be the norm at many schools of management.

My growing interdisciplinary bent was reflected during this period by a collaboration with Ken McCrimmon. We pursued a mammoth project aimed at developing a stochastic model of the careers of managers in the General Electric Company. It was a very large project indeed, the only one I have been involved with based solely on archival data. My principal regret is that only the earliest of our findings were ever published. Ken McCrimmon left to accept a professorship at the University of British Columbia, and the collaboration became too difficult to carry out over such a long distance.

At Carnegie I had a joint appointment between GSIA and psychology and served on the steering committee of both units. However, my major teaching commitments were in GSIA with aspiring or experienced managers. Not too long ago I came across a file containing my lecture notes from one of my early attempts to teach master's students. I read with embarrassment of my rather unsuccessful efforts to elicit their interest in the latest issues in psychological theory or to impress them with the sophistication of new psychological research methods. The lectures that had proven highly effective with Penn Ph.D. students or undergraduate psychology majors seemed to be of little interest to this new audience.

My salvation was found in a return to the experiential teaching methods, particularly role playing which I had learned with Norman Maier during my first year of graduate study. While I rarely use role playing now, I find John Dewey's admonition that people learn by doing is fundamental to sound education regardless of the audience. Along with my adaptation to the managerial classroom went an appreciation of the legitimacy of managerial interests and later a belief that the managerial world represented a rich source of problems for research.

While at Carnegie, I had two students with whom I continue to have a strong intellectual association. One of these was Edward Deci, who received his Ph.D. not from GSIA but from Psychology. One event during the latter stages of Ed's doctoral work is suggestive of the serendipity which characterizes the research process. I had just returned from a research conference on compensation held at General Electric's Crotonville facility. At the conference I was a discussant of a paper given by Leon Festinger in which he argued, based on cognitive dissonance research, for the incompatibility of intrinsic and extrinsic sources motivation sources. As a discussant of Festinger's paper, I argued for a contrary position, which Gordon Allport has termed functional autonomy, i.e. means become ends. One might start pursuing an activity, such as work in the pursuit of external socially mediated gratification, but end up valuing the work for its own sake.

At the conference the debate was spirited and lively, and when I returned to Carnegie, I resolved to look at the evidence in greater detail. I was delighted when Ed Deci took up the project, and he quickly embraced it, ultimately carrying out his doctoral dissertation on the subject. It is a great source of pride that Ed has continued to explore intrinsic motivation and has achieved an international reputation for his work in that area.

While he was at Carnegie, we collaborated on and edited a volume for Penguin Business Series entitled, *Management and Motivation.* In the 20 years following its publication, that book sold about 200,000 copies. This fact led Penguin to try to persuade us to undertake a revision. We accepted, and the revision is in press at the time of this writing.

A second student was Philip Yetton. Phil had come from England to do research on the behavioral theory of the firm. He was surprised to find that very little was going on at Carnegie on that subject despite Cyert and March's book published less than a decade earlier (Cyert and March, 1963).

I had recently authored a chapter in the *Handbook of Social Psychology* (Lindzey and Aronson, 1969) in which I reviewed the empirical evidence relevant to the efficacy of participation and decision making. Below I reproduce a quote from that chapter which foreshadowed much of my subsequent theoretical and empirical work.

The results suggest that allocating problem-solving and decision-making tasks to entire groups, as compared with the leader or manager in charge of the groups, requires a greater investment of man-hours but produces higher acceptance of decisions and a higher probability that the decisions will be executed efficiently. Differences between these two methods in quality of decisions and in elapsed time are inconclusive and probably highly variable. . . . It would be naive to think that group decision making is always more "effective" than autocratic decision making, or vice versa; the relative effectiveness of these two extreme methods depends both on the weights attached to quality, acceptance, and time variables and on differences in amounts of these outcomes resulting from these methods, neither of which is invariant from one situation to another. The critics and proponents of participative management would do well to direct their efforts toward identifying the properties of situations in which different decision-making approaches are effective rather than wholesale condemnation or deification of one approach. (Vroom, 1969, pp. 239-40)

I had used that chapter in a doctoral seminar that I was conducting and expressed to the seminar participants my interest in going beyond the usual "bows" to situational relativity. Phil came to me the next day and expressed interest in the topic, and our collaboration began. For the next year and a half, we drew decision trees and tested them against scenarios which came from our joint and rather limited managerial experience. Since we were somewhat aware of these limitations, we began asking managers to describe decision-making situations in which they were involved, the process they had used in resolving it, and the outcome. Within a year our files were filled with short cases which gave us a substantially broader base for "testing" the efficacy of our decision trees.

Phil had the idea of formalizing the logic implicit in our decision trees, and this spawned the concepts of rules and the related concepts of the feasible set, both of which became important features of the Vroom-Yetton model. It was I who conceived the idea of studying the "decision trees" that managers used. This lead to the idea of a problem seta standardized set of cases taken from our files which I thought, quite naively, would enable us to draw the decision tree used by a given manager. This rather ambitious goal required not only a relatively large number of cases but also the selection of cases in accordance with a multi-factorial experimental design. Such a design would render situational variables, which might be highly correlated in the real world, statistically independent of one another.

About a year after our collaboration began, I was granted a sabbatical leave and decided to spend it at the University of California at Irvine with my old friend Lyman Porter. Phil accompanied me, and we both devoted full time to writing and researching our model. While in California we realized that we needed much more access to data from managers in order to test our emerging notions of a problem set, and I agreed to give several seminars on participative decision making to managers in General Electric and in other organizations. To gain the cooperation of managers, I agreed to give them an individual analysis of their decision-making styles. I made this commitment knowing full well that we did not yet have a technology for carrying out that analysis but confident that a concrete deadline would spur both of us to get it ready in time.

It was during those exciting days in California that we began to realize that the research that we viewed as relevant to basic theoretical issues also had tremendous applied value. My telephone began ringing off the hook from organizations wanting to participate in the research program so that their managers could get feedback. Phil Yetton and I were very clear that we were primarily academics; on the other hand, we wished to have some control over the future development of the educational technology without sacrificing our academic integrity. The options which occurred to us at that time included 1) putting everything that we had done in the public domain or into scientific journals and monographs by which it could be accessed by our colleagues, 2) setting up our own organization which would conduct seminars and further develop the educational technology in a manner somewhat similar to what Robert Blake had done with the managerial grid, or 3) licensing to an existing firm the rights to use the technology. It was at this point that I made what I now view as an unfortunate error - but more of that later.

While putting on one of the early seminars on the model at General Electric's Management Education Center at Crotonville, I encountered Bud Smith of the Kepner-Tregoe organization and communicated some of the enthusiasm that I felt for what we were doing. Bud expressed an interest in these ideas, and negotiations began in a serious way with Kepner-Tregoe resulting in a signed contract with them in April of 1972. My conception of the agreement with Kepner-Tregoe was that the models (including the decision trees, rules, feasible set, problem attributes, and the taxonomy of Al through GII) would be available for all interested parties to use in both teaching or research. Phil Yetton had the same conception. We both believed Bud Smith did too. Consistent with this view we published the model in our book *Leadership and Decision Making*, published by the University of Pittsburgh Press. Kepner-Tregoe was given an exclusive license to the problem set and feedback technology including cases, computer programs, manuals and the like. This exclusive license was subject to Phil's and my unrestricted right to continue to use these materials in our own research, teaching, and consultation.

In 1972 just after signing the contract with Kepner-Tregoe, I decided to leave Carnegie for Yale. Carnegie had been a good place for me for nine years. It had shaped my intellectual development in several important ways. I had entered as a organizational psychologist and left as an organizational social scientist with a passionate interest in management. I came as a person capable of giving a reasonable lecture; I left as a person committed to the process of education.

On the other hand, it seemed time to move on. Carnegie was not the source of interdisciplinary work that it was in the early 60's. While there were still no departments within the school, the creation of a strong group of economists who were committed to theoretical issues within economics foreshadowed similar coalitions within operations research and in organizational behavior. Furthermore, Leavitt and March had moved on to other universities; Cyert was about to become president of the university; and Simon had left the field of organizational behavior more than a decade earlier.

Yale was presented to me as being in transition. Chris Argyris had left for Harvard the previous year; Richard Hackman and Clay Alderfer, both of whom I had tried to recruit at Carnegie, were trying to keep the Department of Administrative Sciences together despite financial and organizational uncertainties.

Despite its mammoth endowment, the University had been losing money. I was asked to chair the department by President Kingman Brewster within weeks of my arrival. It was a prime candidate for budget cuts if not total elimination. The only factors in our favor were constant alumni pressure to establish a business school and, more tangibly, the fact that the University had accepted a large bequest from the Beinecke family to establish at Yale an educational unit to develop future managers.

My role as chair brought with it the responsibility to help shape the manner in which that educational endeavor would be realized. I was appointed to a task force along with the Dean of the Graduate School, the Dean of Yale College and the Director of the Institution of Social and Policy Studies to prepare a report proposing how Yale should address the challenge of management education and research.

I recall disliking intensely this sudden emersion into a strange world of power and politics. I was at a high point in my research production, and academic administration was never something to which I had aspired. I consoled myself with the observation that all parties aspired not to replicate any existing institution but rather to create something that was qualitatively different. This was a rare opportunity to leave a legacy by shaping an institution which could influence management education not only at Yale but throughout the nation.

It would have been completely impossible for me to have continued my research program were it not for a very rewarding partnership with Art Jagoa partnership which continues to this day. Art arrived at Yale as a graduate student at the same time that I did. In fact, he had called me at Carnegie to inquire whether I was planning to leave for Yale, as had been rumored. I told him of my intentions even before I had announced it to my colleagues at both Carnegie and Yale.

During the 4 years in which we worked together at Yale, Art and I carried out a great deal of research on the descriptive aspects of participation in decision makingmost notably, research on differences between sexes in use of participative decision making and hierarchial differences in participative decision making. Perhaps our best piece of work during this period was research on the validity of the Vroom-Yetton model, which we published in the *Journal of Applied Psychology* and which represented a line of research which would be followed by many others in years to come.

My 3 years of administrative travail at Yale was rewarded by the creation of the School of Organization and Management and with the admission of the first class of students in September of 1976. Pending the appointment of the first dean, Bill Donaldson, I had agreed to chair the first board of permanent officers, chair the search committee for a dean, and chair the committee that designed the first curriculum.

In those early days Yale's School of Organization and Management was an exciting place. The students were challenging but a joy to teach; the administrative mechanisms were highly participative and the faculty highly collegial. I recall driving home from the orientation of the entering class in 1976, which we called community building. Clay Alderfer and I had jointly conducted that session which created a set of eight-person groups, each with their own two-person faculty advising team. I recall remarking to Clay that I had never before experienced the tremendous shared level of excitement and enthusiasm that both faculty and students felt about this joint endeavor. Is it conceivable that this level of commitment could be maintained indefinitely? With the appointment of a dean and the institutionalization of many of the norms and practices that I had sought to create in the School of Organization and Management, my administrative role declined; and I devoted my attention to developing and teaching the core course to which I previously referred, Individual and Group Behavior (IGB), and to resuming my research program.

In 1977, another unexpected event occurred that was to shape my subsequent career. One day in June I was sitting in my office when I received an unprecedented phone call from my physician at the Yale Health Plan. He had received my chest X-ray taken the previous day during a routine physical examination. He asked me to report immediately to the X-ray department. Somewhat alarmed, I dropped what I was doing and found myself spending the next 2 hours having my chest X-rayed from all possible angles. Finally the technician told me that I could leave. I refused until I received an explanation of what was going on. When she declined to tell me anything, I burst through a door to find a radiologist surrounded by what seemed like hundreds of pictures of my lungs taken from every conceivable angle. He too refused to give me a diagnosis but referred me to my physician who, unfortunately, had left for the day. When I threatened him with bodily harm unless he told me what was happening, he said to prepare myself for bad news.

The next day my physician confirmed the judgement: the X-rays showed evidence of carcinoma in both lungs and at a very advanced stage. An appointment was arranged two days later with an oncologist who confirmed the diagnosis. He wanted to admit me without delay to the hospital for exploratory surgery. However, lecture commitments of mine and medical commitments of his precluded the scheduling of the operation for a full week.

For the next several days, I contemplated my own mortality for the first time in my 45-year old life. Routine events, such as watching my youngest son pitching in a little league game, carried new meaning when watched for the "last time." I thought of all the things that had seemed important but no longer had significance and of all the things that should have been significant but had been overlooked due to pressure of work and time. In short, I resolved to spend whatever time was left for me pursuing life with a different set of priorities than I had previously.

Finally, the week was over, and I went in for the surgery. Awakening from the operation, I was given the surprising news that the tumors that had almost filled both lungs were not cancerous but rather a disease called sarcoida disease that had been overlooked because it rarely afflicts white males living in the Northeast. While not curable, sarcoid is treatable and has not been of further difficulty.

I describe that event in detail because of its substantial and continuing impact on how I choose to live my life. Even though the fear of imminent death was gone, my resolve to live my life differently did not disappear.

The first effect involved reconsidering a decision made almost 15 years earlierto abandon my musical talents. My saxophone and clarinet were retrieved from deep storage and totally reconditioned. It was discouraging to hear what time had done to my lip. However, practice during the highly restricted time schedule cures all. In this case practice did not make perfect, but it did enable me to begin playing on a casual basis with a variety of groups in the New Haven areaa custom that I continue to this day. It is amazing how therapeutic it is to play an evening of jazz accompanied by a great rhythm section to an appreciative audience. Re-establishing this aspect of my identity did wonders for my sanity and served to re-establish a critical aspect of my identity.

It was in the fall of 1978 that I purchased my first serious sailboat -- a 28-foot sloop called Impulse. I had always been inclined toward sailing, perhaps due to my father's tales of his sailing adventures (and misadventures) while growing up in New Brunswick. I was feeling somewhat guilty over time not spent with my two sons during their early years, and doing cruises seemed like a marvelous way of building the kind of relationship that I felt had been lacking.

Our cruises to places like Newport and Martha's Vineyard were in fact mutually rewarding but somewhat cramped in a 28-foot boat. So a much larger Cal 39 was purchased which my sons affectionately named AI after the leadership style which they professed that I used when at the helm! Since 1980, AI has cruised to the Maine coast, to Chesapeake Bay, to Bermuda and amongst many of the Caribbean islands. On each of these voyages, I have been at the helm accompanied by at least one and, typically, both of my sons.

During the late 1970's and early 1980's, the Vroom-Yetton model was a hot topic for research, and many investigators in the U.S. and abroad sought to determine its validity along with its strengths and limitations. Meanwhile, hundreds upon hundreds of textbooks published the now-familiar decision tree along with the problem attributes and the taxonomy of decision processes. One day a professor who was visiting Yale from the Peoples Republic of China showed me a textbook written in Mandarin and turned to a page in Chinese characters which I could not decipher. However, I did recognize the familiar structure of the time-efficient decision tree.

I must confess to being a bit embarrassed by the magnitude of the impact of this model. When I first wrote *Leadership and Decision Making* with Phil Yetton back in 1973, it had seemed much more like an academic exercise than a guide for practicing or potential managers. Its publication as a research monograph by the University of Pittsburgh Press pointed to the scholarly nature of the work. However, its widespread adoption in the organizational world pointed to the fact that it addressed issues of widespread concern.

By 1983, Art Jago and I became convinced that the Vroom-Yetton model had serious flaws. Our own research on the model's validity along with that of many others convinced us that it had a reasonable batting average but fell far short of the potential of such a model.

What to do about the model's problems was another matter. The VroomYetton model bears some similarity to the Ten Commandments. There are seven rules, each of which takes the form of a "thou shalt not" statement. To have added an eighth rule would have meant that there would be some situations in which no action was possible. In other words, none of the five decision processes would be allowed because each would be contra indicated, albeit in a highly specific and restricted set of circumstances.

Furthermore, there were many situations in which the seven rules were of no use whatsoever since they allowed all five decision processes. In order to reflect what we and others had learned through research, we found it necessary to develop a vastly different model. It required a fundamental change in the old Vroom-Yetton model structure. The concept of rules and the feasible set were eliminated. In their place we substituted functional relationships between problem attributes, decision processes, and the four criteria which had been implicit in the Vroom-Yetton model -- namely, quality, acceptance, time, and development.

Following this realization, there was another period of intense intellectual excitement and exchange. Since Art was now at the University of Houston and I was still at Yale, we could no longer hammer out ideas face-to-face. Instead, phone and Bitnet messages went back and forth on a daily basis. Gradually we evolved a new model based not on seven rules but four mathematical equations. There was little doubt in our minds that these equations would do a far better job of forecasting the actual outcome of various forms and degrees of participation in decision making than the Vroom-Yetton model it replaced. Our concern was that validity and usability were far from perfectly correlated. How could managers be encouraged to solve four equations in order to determine which process to use?

It was Art's thinking that the personal computer and the floppy disk represented the solution. He spent more than a thousand hours writing a computer program which would not only solve the equations but also provide a manager with a highly usable account of the likely consequences of the different degrees and forms of participation in a given situation. Once a person had used the software once or twice, it proved to be almost as fast as using a decision tree and a great deal more accurate.

The computer form for the model enabled us to overcome another limitation of the Vroom-Yetton model. It had permitted only two levels of each problem attribute. Questions which lined the decision tree had to be answered yes or no. Managers told us that their worlds were not dichotomous but rather decorated with "shades of grey." Embracing the computer technology enabled us to allow multiple levels of problem attributes without sacrificing usability.

Once the software problems had been laid to rest, we set about to summarize our research and thinking (which had now spanned 15 years) into a book form. In the summer of 1986, Art and I went out on a cruise on AI to put the finishing touches to our book which we titled, *The New Leadership: Managing Participation in Organizations.*

The foreword to the book was authored by Ben Tregoe, the chairman of Kepner-Tregoe. Kepner-Tregoe had been kept informed of developments of the new model (including the software) and had expressed interest in phasing out the Vroom-Yetton model and replacing it with our new one. They extended contracts to both of us to license our new work. After much deliberation, Art and I decided not to grant them a license since the nature of their proposals as well as information gleaned from people within the firm led us to fear that with such a license Kepner-Tregoe might "shelve" the Vroom-Jago model and continue to use the Vroom-Yetton model they already had, with the possible result that the Vroom-Jago model would be unavailable for ourselves or others. In addition, I had never been happy with the job that Kepner-Tregoe had done either in developing a suitable training package based on the Vroom-Yetton model or in marketing the one that they had developed. The original ideas on which the organization had been founded -- problem analysis and decision analysis -- seemed to me to so dominate the culture as to leave little room for concepts invented by outside scholars.

There was great excitement at the publication of *The New Leadership* in 1988. Just prior to its publication, Art and I founded a little company which we had initially called AI Software after the autocratic term in our model. However, we should have realized that AI also signified artificial intelligence, and that corporate identity subsequently became Leadership Software. We envisioned it as a small, single-product company solely to produce MPO, the software program which Art had written to produce the floppy discs which ran the new model. Art would be the only employee, and the operation would be conducted out of his home in Houston.

This low overhead enabled us to advertise the software complete with manual and vinyl folder in *The New Leadership* at a very reasonable price. This new company was one way in which Art could derive some small economic return for his tremendous investment of time in developing the model and its applications.

My general feeling of new beginnings that characterized 1988 was interrupted violently by a visit paid by Benno Schmidt, Jr., the new, young president of Yaleto the School of Organization and Management in October of that year. In a meeting with faculty, he announced 1) the appointment of a new dean, Michael E. Levine, who would assume office immediately with unprecedented powers (all faculty voting rights granted in the bylaws of the university were to be suspended -- a condition that the university counsel later described to me as similar to martial law); 2) the organizational behavior doctoral program was to be terminated, and all nontenured faculty in that field were to be terminated without review on the expiration of their current contracts; 3) the faculty in operations research were to be transferred to the Faculty of Arts and Sciences.

Later the new dean was to announce other changes. Community building, student participation in admissions, a weekly student/faculty meeting called liaison and the core course in organizational behavior, Individual and Group Behavior, (voted by alumni that year as the most valuable course in the school) were to be terminated. All of these changes were made without any semblance of faculty participation and debate. I was filled with such rage that I was speechless. It seemed to me that all of the things that had made Yale's School of Organization and Management unique in a world of business schools had been eliminated by one administrative act that defied comprehension.

My shock was undoubtedly heightened by the fact that I had been a member of the search committee for the dean. To be sure, that committee had not met in over six months, but our brief discussion of Levine had led me to believe that there was no support whatsoever for his candidacy. Two weeks before the announcement I had heard a rumor of Levine's possible appointment and had lunch with him in which he refused to confirm or deny the rumor. I took it upon myself to interview as many full professors (who constitute the governing board of the school) as I could find to learn their feelings about this possibility. The consensus was striking! Of the twelve professors I interviewed, all but one found Levine totally unacceptable as dean. I then had a long conversation with the president in which I relayed my findings. He thanked me profusely for what I had done, and I left the interaction greatly relieved feeling that an appointment which I felt would have been a disaster had been averted. I returned to my colleagues in a style akin to Chamberlain on his return from Munich in 1939.

The uproar following Benno Schmidt's decision has been chronicled in dozens of publications including *The New York Times, Business Week, Newsweek, and The Wall Street Journal*. What has been less publicized are the long-term effects. As I write these words almost two and one half years later, it is very, very, difficult for me to see any benefits accruing from these changes. The school has been dropped from the top 20 schools in the *Business Week* survey; alumni in the main are disenfranchised, and most have ceased making contributions to their alma mater. Some alumni have even hired airplanes to overfly Yale graduations and the Harvard-Yale football game trailing banners critical of the president, the dean, or both. Current students, virtually none of whom knew the school before the changes, are divided on their support, but many complain about core courses taught by visitors -- a practice brought about by the difficulty of faculty recruiting after the well-publicized events of the past 3 years.

Too often we take for granted the institutional settings in which scholarship takes place. Nurturing of collegiality is a function which is either overlooked or relegated to deans or department chairs. My experience at both Yale and Carnegie Mellon taught me the fragility of these settings, of how quickly a tradition of collaboration could erode or be destroyed altogether. Yale University has a long and impressive past and undoubtedly a long future, one member of the Yale Corporation of my acquaintance has urged taking a long-term view. He counseled that Yale's traditions will enable it to recover from what I see as its current difficulties. I hope that he is right, but there is precedent for an alternative scenario. An editorial in the latest issue of the student newspaper asks how long Yale will tolerate a second-rate imitation of Chicago or Rochester within its ranks.

Less than a year after the changes at Yale a second personal disaster was to strike. A sheriff arrived at my front door to serve notice that Kepner-Tregoe had filed suit in Federal court charging me with copyright infringement and breach of contract. That was followed by a subsequent suit against Art Jago and Leadership Software. The claims made by these suits appear to be that the terms AI . . .GII as well as the problem attributes and their definitions are owned by Kepner-Tregoe and that their use by others, including me, is an infringement of their copyright. Kepner-Tregoe's motives, to me, seem obvious. The Vroom-Jago model threatened to make the Vroom-Yetton model obsolete. It would be hard for Kepner-Tregoe to justify its charges (about $300 per participant for materials alone) and its acclaimed position as market leader in this field if the product it uses was almost 20 years old and Art and I were writing about and lecturing on a vastly improved product which Kepner-Tregoe did not own. Attempts on my part to discuss the matter in a meaningful manner and reach some equitable resolution were totally rebuffed, and I found myself embarked on a course of litigation which would consume most of my time and energy and financial resources for a period of years.

To those who have not been defendants in litigation launched by a large corporation (Kepner-Tregoe was bought by U.S. Fidelity and Guaranty in the middle 1980's), I can describe the experience only with difficulty. Even though you feel certain that the charges against you are without substance, you also know that the financial costs of defending your rights ate beyond comprehension. While there have been some peaks and valleys, I can honestly state that almost half my time has been spent preparing for depositions, attending depositions, searching records, and consulting with the three law firms that are representing my interests. One should add to this the "nonproductive time," the sleepless nights filled with rage or self -questioning which accomplishes nothing but consumes precious energy.

Both events -- the wrenching changes at Yale and the Kepner-Tregoe lawsuit -- have taken their emotional and intellectual toll. I find it impossible to carve out the amount of time and space necessary to write, say, a book like *Work and Motivation, Leadership and Decision--Making,* and *The New Leadership*. Ed Deci and I have just finished a complete revision of our edited book, *Management and Motivation,* but even that edited project was delayed a year and a half beyond its expected deadline by my seeming inability to get my share of the writing done on time.

Even writing this piece which in earlier, less stressful times could have been completed in a week, was written in fits and starts, half an hour here in a hotel room followed by 45 minutes a month later while flying from New York to Atlanta. The most sustained investment of time occurred when I dictated the earliest sections to my wife and soul mate while driving from Connemara to Dublin, Ireland.

In spite of the fragmented nature of the process, I can honestly report that the task was personally worthwhile and at least mildly therapeutic. As my students in the now-defunct course, Individual and Group Behavior, have reported, reflection has been helpful in regaining a perspective on one's life.

And now the tale is over, at least for the present. Hopefully, future years will bring trails that are not only more satisfying but also more conducive to scholarship. Meanwhile it's about time to practice a few chords and scales.

References

Cyert, R.M. and March J.G. *A Behavioral Theory of the Firm.* Englewood Cliffs, NJ: Prentice-Hall, 1963.

Lindblom, Charles E. "The Science of Muddling Through." *Public Administration Review,* 1959, 19, pp. 79-99.

Hebb, D.O. *The Organization of Behavior.* New York, NY: John Wiley and Sons, 1949.

Cronbach, L.J. "The Two Disciplines of Scientific Psychology." *American Psychologist,* 1957, 12, pp. 671-684.

Maier, N.R.F. *Psychology in Industry.* Second Edition. Boston, MA: Houghton Mifflin, 1955.

Vroom, V.H. *Work and Motivation.* New York, NY: John Wiley and Sons, 1964.

Lindzey G. and Aronson, E. (Eds.) *Handbook of Social Psychology.* Vo. 5. Reading, MA: Addison Wesley Publishing Company, 1969.

Victor Harold Vroom

BIBLIOGRAPHY

Books

Vroom, V.H. *Some Personality Determinants of the Effects of Participation*. Englewood Cliffs, NJ: Prentice-Hall,

1960. Reprinted New York, NY:Garland Publishing Company, 1987.

Mann, F.C., Vroom, V.H., and Indik, B.P. *The Productivity of Work Grows.* Ann Arbor, MI: Institute for Social Research, 1963.

Vroom, V.H. *Work and Motivation.* New York, NY: John Wiley and Sons, 1964. Reprinted Malabar, FL: Krieger Publishing Company, 1982.

Vroom, V.H. *Motivation in Management.* New York, NY: American Foundation for Management Research, 1965.

Vroom, V.H. (Ed.) *Methods of Organizational Research.* Pittsburgh, PA:University of Pittsburgh Press, 1967. Reissued as*Organizational Design and Research,* Pittsburgh, PA: University of Pittsburgh Press, 1968.

Vroom, V.H., and Deci, E.L., (Eds.) *Management and Motivation.* London, England: Penguin Books, 1970.

Vroom, V.H., and Yetton, P. *Leadership and Decision- Making.* Pittsburgh, PA: University of Pittsburgh Press, 1973.

Vroom, V.H., and Jago, A.G. *The New Leadership: Managing Participation in Organizations.* Englewood Cliffs, NJ: Prentice-Hall, 1988.

Articles

Vroom, V.H. "Effects of Design on Estimation of Size of Coins," *Canadian Journal of Psychology,* 1957, 11: 83-92.

Vroom, V.H. "Some Personality Determinants of the Effects of Participation," *Journal of Abnormal Social Psychology*, 1959, *59*,322-327. Also in Smelser, N.J. and Smelser, W.J. (Eds.), *Personality and SocialSystems.* New York, NY: John Wiley and Sons, 1963.

Vroom, V.H., and Mann, F.C. "Leader Authoritarianism and Employee Attitudes," *Personnel Psychology*, 1960, 13: 135-140. Also in Karn, H.W., and Gilmer, B. von H. (Eds.), *Readings in Industrial and.Business Psychology.* New York, NY: McGraw-Hill, 1962.

Vroom, V.H. "The Effects of Attitudes on the Perception of Organization Goals," Human Relations, 1960, 11, 229-240.

Vroom, V.H., and Maier, N.R.F. "Industrial Social Psychology," *Annual Review of Psychology*, 1961.

Vroom, V.H. "Projection, Negation and the Self-Concept," *Human Relations*, 1959, 12: 335-344. Also in Vinacke, W.E., Wilson, W.R., and Meredith, G.M. (Eds.), *Dimensions of Social Psychology.* Chicago, IL: Scott-Foresman, 1964.

Vroom, V.H. "Ego Involvement, Job Satisfaction and Job Performance," *Personnel Psychology*, 1962, 15: 159-177.

Vroom, V.H. "Human Relations Research in Industry: Some Things Learned," in Bairstow, F. (Ed.), *Research Frontiers in Industrial Relations,* McGill University, 1962.

Vroom, V.H., and Jones, S.C. "Division of Labor and Performance Under Cooperative and Competitive Conditions," *Journal of Abnormal Social Psychology*, 1964, 68: 313-320.

Vroom, V.H. Some Psychological Aspects of Organizational Control," in Cooper, W., Shelly, M., and Leavitt, H.J., *New Perspectives in Organization Research*. New York, NY: John Wiley and Sons, 1964.

Vroom, V.H. "Employee Attitudes," in Fisk, G. (Ed.), *The Frontiers of Management Psychology.* New York, NY: Harper & Row, Inc., 1964.

Vroom, V.H. "A Comparison of Static and Dynamic Correlational Methods in the Study of Organizations," *Organizational Behavior and Human Performance*, 1966, Vol. 1, No. 1, 55-70.

Vroom, V.H. "Organizational Choice: A Study of Pre- and Post-Decision Making Processes," *Organizational Behavior and Human Performance*, 1966, Vol. 1, No. 2, 212-225.

Vroom, V.H. "The Role of Compensation in Motivating Employees: A Behavioral Scientist's Viewpoint," Paper delivered at the 43rd Annual Conference of the Life Office Management Association, Miami, FL, 1966. Also reprinted in *Business Management*, November, 1966.

Vroom, V.H., and MacCrimmon, K. "Toward a Stochastic Model of Managerial Careers," *Administrative Science Quarterly*, June, 1968.

Vroom, V.H., Grant, L., and Cotton, T. "The Consequences of Social Interaction in Group Problem-Solving," *Organizational Behavior and Human Performance*, 1969, Vol. 4, No. 1, 77-95.

Vroom, V.H. Industrial Social Psychology," in Lindzey, G. and Aronson, E. (Eds.), *Handbook of Social Psychology.* Reading, MA: Addison Wesley Publishing Company, Vol. 5, 1969.

Vroom, V.H. "Some Implications of Research on Learning for Management Development," in Goldberg, W. (Ed.), *Behavioral Approaches to Modern Management*. Vol. 1. Goteborg Studies in Business Administration, Goteborg, Sweden, 1970, 74-88.

Vroom, V.H. "The Relevance of the Behavioral Sciences for Manpower Management," in Goldberg, W. (Ed.), *Behavioral Approaches to Modern Management*, Vol. 1, Goteborg Studies in Business Administration, Goteborg, Sweden.

Vroom, V.H., and Deci, E.L. "The Stability of Post-Decision Dissonance: A Follow-Up Study of the Job Attitudes of Business School Graduates," *Organizational Behavior and Human Performance*, 1971, 6, 36-49.

Vroom, V.H., and Pahl, B. "The Relationship Between Age and Risk Taking Among Managers," *Journal of Appied Psychology*,1971, Vol. 55, No. 5, 399-405.

Vroom, V.H., and Deci, E.L. "Leadership, Social Value, and the Risky Shift," *Repesentative Research in Social Psychology*, 1971, Vol. 2, No. 1.

Campbell, J.P., Cranny, C.J., Lawler, E.E., McKinney, A.C., and Vroom, V.H. "The Changing Role of Industrial Psychology in University Education: A Symposium," *Professional Psychology,* 1971, *2,* 2-22.

Vroom, V.H. "A New Look at Managerial Decision -Making," *Organizational Dynamics,* 1973, Vol. 1, No. 4, 66-80.

Vroom, V.H., and Jago, A.G. "Decision Making as a Social Process: Normative and Descriptive Models of Leader Behavior,"*Decision Sciences,* 1974, Vol. 5, No. 4, 743-769.

Vroom, V.H. "Decision Making and the Leadership Process," *Journal of Contemporary Business,* Autumn 1974, 743-769.

Jago, A.G., and Vroom, V.H. "Perceptions of Leadership Style: Superior and Subordinate Descriptions of Decision Making Behavior," in Hunt, J.G., and Larson, L., *Leadership Frontiers,* Kent State University Press, 1975, 103-120.

Vroom, V.H. "Leadership Revisited," in Cass, E.L., and Zimmer, F.G., *Man and Work in Society.* New York, NY: Van Nostrand Reinhold Co., 1975, 220-234.

Vroom, V.H. "Leadership," in Dunnette, M. (Ed.), *Handbook of Industrial and Organizational Psychology.* Chicago, IL: Rand McNally, 1976.

Vroom. V.H. "Can Leaders Learn to Lead?" *Organizational Dynamics,* Winter 1976, 17-28.

Jago, A.G., and Vroom, V.H. "Hierarchical Level and Leadership Style," *Organizational Behavior and Human Performance*, 1977, 18, 131-145.

Jago, A.G., and Vroom, V.H. "Predicting Leader Behavior from a Measure of Behavioral Intent," *Academy of Management Journal*, 1978, 21, 715-721.

Vroom, V.H., and Jago, A.G. "On the Validity of the Vroom-Yetton Model," *Journal of Appied Psychology,* 1978, 63, 151-162.

Yetton, P., and Vroom, V.H. "The Vroom-Yetton Model of Leadership: An Overview," in King, B.T., Streufert, S.S., and Fiedler, F.E. (Eds.), *Managerial Control and Organizational Democracy.* Washington, DC: Winston & Sons, 1977.

Jago, A.G., and Vroom, V.H. "An Evaluation of Two Alternatives to the Vroom/Yetton Model," *Academy of Management Journal*, *23*, No. 2, June 1980.

Vroom, V.H. "Decision Making in Organizations: A Case Study in Programmatic Research," in Sgro, J. (Ed.), *Virginia Tech Symposium on Applied Behavioral Science,* Vol. 1, Lexington, MA: Lexington Books, 1981, pp. 199-214.

Jago, A.G., and Vroom, V.H. "Sex Differences in the Incidence and Evaluation of Participative Leader Behavior," *Journal of Applied Psychology,* 1982, 67, 776-783.

Vroom, V.H. Leaders and Leadership in Academe," *Review of Higher Education*, 1983, *6*, 367-386.

Vroom. V.H. "Reflections on Leadership and Decision-Making," *Journal General Management,* 1984, 2, 18-36.

Jago, A.G., Ettling, J.T., and Vroom, V.H. "Validating a Revision to the Vroom/Yetton Model: First Evidence." *Proceedings of the 45th Annual Meeting of the Academy of Management,* 1985, 220-223.

Vroom, V.H., and Jago, A.G. "Managing Participation: A Critical Dimension of Leadership," in Pate, L.E. (Guest Ed.), Special Issue of *The Journal of Management Development,* 1988, Vol. 7, No. 5, 32-42.

Jago, A.G., and Vroom, V.H. "Vom Vroom/Yetton-zum Vroom/Jago-Fuhrungsmodell: Neue Ubetlegungen zur Partizipation in Organisationem," *Die Betriebswirtschaft,* 1989, 49, 1, 4-17.

<u>Abstracts. Notes. Short Articles. and Reviews</u>

Vroom, V.H. Forward to *The Acceptance of Human Resource Innovation: Lessons for Management*, by Ellen Ernst Kossek. New York, NY: Quorum Books, 1989.

Vroom, V.H. "A Primer of Leadership," Review of R. Bellows' *Creative Leadership,*" in *Contemporary Psychology,* 1960, 5, 297.

Vroom, V.H. "Some Factors Affecting the Perceived Similarity of Social Objects," *Proceedings of the 16th International Conference of Psychology.* Bonn, Germany, 1960. (Abstract)

Vroom, V.H. Review of Thomas G. Spates', *Human Values Where People Work, The Annals of the American Academy of Political and Social Science,*1961, M, 228.

Vroom, V.H. "Maslow at Play," *Contemporary Psychology,* 1967, L2, (2), 97-98.

Vroom, V.H. Review of Fred E. Fiedler's *Leadership and Management, Contemporary Management: Issues & Viewpoints,* Prentice-Hall, 1972.

Vroom, V.H. "Psychology for Managers," *Contemporary Psychology,* 1973, Vol. 18, No. 8, 391.

Vroom, V.H. "Evocative Theory," *Contemporary Psychology,* 1981, 26., No. 1, 29-30.

Vroom, V.H. "The Essential Attributes of Norman Maier: A Review of Norman Maier's *Psychology in Industry,"* *The Academy of Manaizement Review,* 1989, Vol. 14, No. 1, 104-106.

Vroom, V.H. Preface and Introduction to *Manage People, not Personnel: Motivation and Performance Apppraisal.* Boston, MA: Harvard Business School Press, 1990.

Unpublished (but Widely-Cited) Papers

Vroom, V.H. "The Self-Concept: A Balance Theoretical Treatment." Department of Psychology, University of Pennsylvania, 1963.

Vroom, V.H. "Some Observations on Herzberg's Two-Factor Theory." Paper delivered at APA, September 1967.