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Members of the Control Systems Group receive Closed Loop quarterly. For more information, contact Ed Ford, 10209 N. 56th St., Scottsdale, AZ 85253; phone (602)991-4860.

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From the Editor

Closed Loop begins its fourth year with a new subtitle, *Journal of Living Control Systems,* which replaces *Threads from CSGnet.* I hope that this will signal a shift in the contents away from CSGnet conversations, toward research reports, clinical studies, and review articles. My ultimate goal is a peer-reviewed journal which includes contributions to the science of living control systems written by both Control Systems Group members and non-members. To start moving toward that goal, I need submissions from *you*. Why not send a manuscript *before* I come asking you personally? Consider yourself warned!

This issue contains articles by seven "pioneering" (in my estimation) perceptual control theorists, all of whom have been studying and using (and, in some cases, inventing) perceptual control theory for several years. I asked them to write on the theme "PCT: Looking Back, Looking Forward," and, in my opinion, they have done so quite admirably. Now I am sorry that I didn't ask them to write on "Solving the Problems of the World with PCT" Of course, there's always a next time....

How Perceptual Control Theory Began: A Personal History

Mary A. Powers
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The beginnings of PCT lie in two major developments of the 1920s and 1930s: H. S. Black's concept of negative-feedback control in electronics and Walter B. Cannon's concept of homeostasis in biology. These were brought together in the early 1940s, primarily by Norbert Wiener, a mathematician, Julian Bigelow, an engineer, and Arturo Rosenblueth, a co-worker with Cannon. In 1943, they published the first paper relating engineering control theory to neurophysiology.

Although Wiener called his 1948 book *Cybernetics, or Control and Communication in the Animal and the Machine* and believed in the importance of control theory as a key to explaining some phenomena of living systems, he was far more interested in communication engineering and information theory. This bias was shared by the participants in the 10 Macy Conferences that preceded and followed the 1948 publication of *Cybernetics.* Many of the people who attended these conferences were prominent figures in the life, social, and behavioral sciences, mathematics, physics, and philosophy. Though officially titled "Cybernetics: Circular Causal and Feedback Mechanisms in Biological and Social Systems," the meetings were primarily concerned with issues of information and communication.

Enter Bill Powers, an ex-Navy electronic tech and college physics major in his 20s (hardly the sort of person who got invited to the Macy Conferences) who had then what he has now: an irresistible urge, when confronted with something unfamiliar but interesting, to grab a pencil and a piece of paper and start figuring it out. What was interesting to him in *Cybernetics* was not communication, but rather the idea that the nervous system seemed to be a control system. He thought this was an enormously exciting idea, and he couldn't wait to see where the big scientific guns and gurus would carry it. Because he couldn't wait, he started figuring it out for himself, but he was sure for many years that someone far more competent than he would be coming along with a more extensive and profound analysis. That someone, we now know with 20-20 hindsight, turned out to be himself, 20,30, and now 40 years later.

Why Bill Powers? My purpose here is to suggest a few of the variety of characteristics and circumstances that made him uniquely the person to develop PCT. Since he is a private person, I intend to avoid getting too personal, with the one exception that for the rest of this paper I'm going to call him Bill.

One place to begin is with the satisfaction Bill has always found in figuring out how things work, as mentioned above. This contributed to a professional career at a technical level, with little aspiration to rise beyond the actual hands-on design and construction of control systems and development of computer software into the heady realms of administration and paperwork. And his real career lay elsewhere—since working at the lower levels of an organization means, usually, being able to walk out the door at five o'clock and leave it all behind, the evenings and weekends that others might have spent furthering their professional ambitions were free for PCT.

But sticking to the technical level also meant looking at a lot of emperors and finding them naked. There is a good deal of difference between talking about control systems metaphorically, philosophically, and theoretically, and dealing with them on a practical basis, when you're in there soldering wires and making the damned things work. And Bill made a number of control systems work very nicely indeed.

While this sort of experience contributed to the solidity of the foundations of PCT, PCT at the same time contributed to Bill's successful design of control systems: he would imagine "taking the point of view" of the control system he was designing—if I were this system, what would I be able to perceive, what would I need to perceive, what would "really" be going on? This worked so well that he was convinced he was cheating, fudging over gaps in his technical expertise by using control theory as he was developing it to explain living systems (of course one person's cheating is another person's insight).

Another circumstance fostering Bill's approach to living control systems was his coming of age in the era of analog computers. The digital computer as a metaphor for the workings of the nervous system was immediately more attractive to many than the telephone switchboard it replaced, but in Bill's eyes, it is false at its base. His programs, although digital, are designed to simulate the actual analog functions of the brain, not, as in Artificial Intelligence, to produce brain-like results by whatever means. The contributions his analog models might make to neuroscience have yet to be explored.

While Bill wanted his model to be plausible and workable from the physiological ground up, his main interest was psychology. What he knew of psychology when he began was whatever was taught in undergraduate courses around 1950. Behaviorism held the high ground as far as psychology as a science was concerned. The therapeutic community was largely Freudian, with a dash of humanistic psychology —Carl Rogers and Fritz Perls, and later Abraham Maslow. The treatments available for psychosis were lobotomies and electric shock. Bill was interested in psychology for personal reasons, as almost everyone is, and like many young engineers and other technically inclined people, he discovered what seemed to be a far more fruitful approach in the pages of what for many of us was our favorite magazine, *Astounding Science Fiction.* Many people were drawn to Dianetics because, unlike behaviorism, it didn't try to do away with the mind; in fact, *all* the action was in the mind, accessing and dealing with memories, in a very straightforward and routinized manner. There was an appealing technical flavor to it. Like others who went into Dianetics, Bill got out when L. Ron Hubbard's grandiosity, greed, and paranoia turned off youthful enthusiasm, and when it seemed that this "new science of the mind" was not all that it was cracked up to be.

Soon, the first wave of disillusioned Dianetikers went back to work or school and went on with their lives (I kept running into them at the University of Chicago in the early '50s, and there are four—that I know of—alive and well in the CSG). Bill, who had read *Cybernetics* by that time and thought it to be a much more promising approach than Dianetics had turned out to be, went to work as a medical physicist, and he discovered to his delight that his bosses knew about, used, designed, and could teach him about control systems. This means he did not approach the subject of living systems from the point of view of a control engineer, but rather as a student of control theory, applying what he was learning to both artificial and living systems at the same time. This, I think, is the source of his realization that the reference signal, which in artificial systems is set externally to the system and labeled "input," is, in living systems, internal, and not an externally accessible input at all.

Together with Bob Clark, another physicist, and later Bob McFarland, a psychologist, the first model of hierarchical living control systems was worked out. It was published in 1960 as "A General Feedback Theory of Human Behavior," which presented a six-level hierarchical model. By this time, Bill had left his job and begun graduate work in psychology at Northwestern University, and the association with Clark and McFarland ended. The graduate work ended after one year, done in by total incomprehension on the part of the faculty as to what on earth Bill's master's thesis proposal was about, by wifely financial panic, and by an appealing job offer from the Northwestern astronomy department.

"Feedback theory" was the name of the game for many more years, as a book slowly took shape, was dropped into the wastebasket, was written again, and then again. As this went on, the emphasis shifted from the one immediately obvious component that makes control systems unique, namely feedback, to the overall system of which feedback is a part, and ultimately to that aspect of a living control system that makes it so radically different and so difficult to understand, the control of perception. The only possible way to know what is happening, or what one is doing, or the effects of either on the other, is by perception.

How does a person entirely alone develop a science, without money, a lab, or colleagues? One answer, of course, is that all the equipment was readily at hand. Between children, a dog, a clunky computer, and above all, himself, there was more than enough to observe and think about. The nature of much of that observation was unique, however, and involved a form of introspection in which one does not think about thoughts, but about what one is seeing: What perceptions are necessary to see an object, or movement? From what perceptions does logic emerge, or principles? Thus the six levels of 1960 became nine by 1972, and they now number 11. Bill is the first to admit that the levels he sees are personal, and possibly idiosyncratic, and it is with some dismay that he sees them taken as a final word on the subject, copied down and memorized. But the main point here is that the levels, and much else about PCT, were derived from experience; the theory had to explain not just the performance of subjects, of others, but how the world looks from the only point of view available to anyone, from the inside.

The main thing that Bill has been able to bring to his work, then and now, is a mind with no strings attached except his own initial feeling that control theory could answer some of his questions. It is from that stance that he has read books, taken courses, and otherwise absorbed what was already available in the life sciences. Learning what other people have done has never meant accepting either their premises or their conclusions. As an outsider, he has never had to conform to any particular school of thought or please any particular community of scholars. When confronted with such pressure (as with his master's thesis), he has simply walked away and continued on his own path.

I think it took many years for Bill, and for the other people who have become committed to PCT, to fully realize how radically different control theory is from the rest of the behavioral sciences. There is something about PCT that offends just about every point of view: behaviorists, cognitive scientists, dynamic systems analysts, roboticists, cyberneticists, and even control engineers seem equally unimpressed, or baffled, or annoyed. Well-meaning attempts to integrate control theory into the mainstream have succeeded only in confusing the issue with inaccuracies and gratuitous embellishments. The concept of PCT is expressed as principles which contradict many fundamental assumptions: that behavior is the end point in a chain of events, that

the brain calculates necessary outputs, that the concept of purpose is unnecessary to explain behavior, that reference signals (if they exist at all) can be imposed from outside, that feedback can be given or withheld, that self-regulation is a conscious process only and has nothing to do with homeostasis, and so on, and on.

In 40 years, Bill and his colleagues have developed a rich and comprehensive theory which encompasses and resolves many issues in the behavioral sciences. I will never forget the astonishment, joy, and relief on Bill's face as he looked around at the people gathered at the first CSG meeting in 1985, when he really felt for the first time that control theory was not a lonely and eccentric obsession, but rather a shared enterprise that might, just might, change the behavioral sciences forever. That hope, unfortunately, is still discouragingly far from being fulfilled, but at least it is clear that PCT no longer exclusively depends on the unique life, talents, and circumstances of a single person.

Perceptual Control Theory: Origins, Development, Future

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Preface

The following paper is a condensed summary of my experiences and applications of Perceptual Control Theory from the time I first met Bill Powers, during the period of our collaboration, as well as our initial association with R. L. McFarland. My later separate work with McFarland, Richard J. Robertson, and others is included, as well as a similar condensation of my independent activities through later years.

Having participated in the original development of PCT, I have continued to apply and develop related concepts. During most of these years, I was out of touch with the early group, but I continued to work with these concepts informally on my own. My contact with these early associates appeared to have been completely and permanently broken. Since my employment was unrelated to behavior theory, none of these ideas was written, presented, or published.

Those familiar with PCT can generally infer much of how I would have been applying those concepts and methods. These informal applications continue. I think this is about what all of us expected from the beginning of our association. At that time, none of us thought we had "final answers." I believe this remains our mutual orientation.

I feel that my general success throughout these twists in my activities is due, at least in part, to my familiarity with those ideas now labeled "PCT." Those insights Bill and I shared from the earliest days also continue to provide much of my basic orientation.

Before VA Research Hospital

Perceptual Control Theory began when Bill Powers and I met in the early 1950s. Our physical science backgrounds, interests, and orientations resulted in an "instant fit." We both read science fiction and had been impressed by L. Ron Hubbard's Dianetics. When a friend learned of my interest, he suggested that I contact Bill. At that time, Bill was actively working on the application of Dianetics.

My own orientation was derived from my family, childhood, and schooling. My father, a professor of Classics, a humanist, and a philosopher, was highly verbal—with a general questioning and skeptical orientation. (A great admirer of Socrates.) My older brother and our mutual friends were also intellectually and academically oriented.

In high school, I belatedly discovered that my peers did not share my interests. This roused my curiosity, so I undertook to learn more about their interests.

At the university, this interest developed in the form of participation in the operations of a social fraternity, various clubs, theater, and other organizations. I helped form a discussion group concerned with international affairs.

My interest became more focused when an English composition course required a "source theme." Ambitiously, I decided to write about my opinion of modern psychology. I reviewed several current books: Gestalt, Behaviorism, Psychoanalysis, Extroversion/Introversion, and others. I found little of interest in most of them—my theme was clearly negative. Later, I found I could attend a course in Abnormal Psychology as a non-credit listener without taking the General Psychology course. This included a visit to a mental hospital. On taking the exam (without having bothered with the reading assignments), I would have rated a C grade. This would have been very hard to do in the physics-mathematics-chemistry curriculum I was following. While my interest in behavior continued to increase, my interest in current psychological theories practically disappeared.

Having completed my triple science major, I entered the graduate physics program at the University of Illinois at Urbana. Before completing my Ph.D., I moved to the Columbia Presbyterian Medical Center in New York, where I joined the Radiological Physics Laboratory. Here I continued preparation for my Ph.D. and performed my thesis research: "Absolute Determination of Beta Ray Activity."

During this period, I had an opportunity to attend a class in "Memory and Concentration." The teacher, Bruno Furst, was the author of *Stop Forgetting*. In this course, I learned a great deal about how memory works, and how one can intentionally put it to work for one's own purposes—a course, in my opinion, that should be introduced very early in the teaching/learning process.

When I met Bill, I found he was then involved in activities related to Dianetics. I still have a couple of papers he wrote at that time, as well as some of the Scientology materials then current. We were repelled by the developing transition of Dianetics into Scientology and other less realistic areas. But our mutual orientation and interests gradually developed into an intensive collaboration. It was never clear to me which of us came up with which proposals—it never was of any importance.

Our first experiment was proposed by Bill. It was generally known that a sudden loud noise results in a reduction of electrical skin resistance. It occurred to Bill that the galvanic skin response (GSR) could be connected to form a positive feedback loop. At the time, Bill had access to suitable equipment and space (at the Cancer Hospital) to put this together and try it out.

Bill assembled the equipment, and the first trials showed the beginnings of oscillation. But I (as a subject) soon found I could more or less "ignore" the noise, no matter how loud it went. In today's terms, my higher-order systems were able to modify the sensitivity of some of the systems involved. So we demonstrated—to each other—that the GSR is not a purely automatic response.

In the process of working with this equipment, I was able to learn partial control of my own skin resistance. I can voluntarily reduce it, but then have to wait awhile for it to return to higher levels. I still have this ability.

VA Hospital—Three of Us

In 1954, I learned of an opening at the VA Research Hospital in Chicago. I was able to arrange for Bill to join me, together with an outstanding tool and instrument maker and a secretary. We reported to Therapeutic Radiology, Diagnostic Radiology, and Radioisotopes. We had excellent shop and electronics facilities.

I continued my "outside interests," which in the long run contributed perspective regarding the behavior of "ordinary people." These interests included: (1) observing hospital management (manager's weekly meetings), working with purchasing, personnel, etc.; (2) Health Physics Society (second president of the Midwest Chapter); (3) National Health Physics Symposium; (4) Association of Physicists in Medicine; (5) other physics-related activities—I helped organize Radiation Control, Inc., and was the president of the company during most of its life.

The Physics Unit at the VA Hospital included primary responsibility for overseeing the installation, operation, and safety of a unique Cobalt 60 teletherapy unit. This later led to our learning computer programming. After work, we drove up to the Evanston campus of Northwestern, where there was an IBM 650 that Bill had arranged for us to use. We would have dinner and then spend a long evening working with the computer. (Home around 12:30 or 100!)

Bill and I continued our close collaboration, gradually developing a theory of behavior on which we generally agreed. There were always some points where we were not fully satisfied, and we frequently exchanged ideas. Our exchanges were so frequent and informal, it was rarely clear to me which of us came up with any particular idea.

However, I am sure that Bill first pointed out the relevance of the negative-feedback control concept and examined its history. Further, he suggested the concept of an interlocking hierarchy of simple versions of such control systems. If such systems are successful in their operation, they would always be close to their normal condition, so linearity would be a valid first approximation.

I'm not sure just when, but soon after joining the VA, we met Bob McFarland, a Ph.D. clinical psychologist in the Neuropsychiatric Service. This was the beginning of a three-way collaboration (Bill, me, and McFarland) that lasted until Bill left (1961). McFarland and I continued at the VA through most of 1963. McFarland had other responsibilities and outside activities, so his participation in discussions of theory was limited.

While joining in our discussions, McFarland's primary contribution was finding ways that our work could be presented. During these years, I, and usually all three of us, attended meetings and presented papers. Informal presentations were not uncommon. However, on several occasions we were specifically invited to present papers.

The first major presentation of what came to be called 'PCT" was a symposium arranged by McFarland at the August 1958 meeting of the American Psychological Association. Each of us presented papers and were joined by J. Arbit and C. Van Buskirk. During the preparation, we reviewed each other's papers and offered suggestions, as Bill and I had done from the beginning of our association. This practice continued throughout our collaboration.

Throughout our affiliation with the VA Hospital, we made presentations at meetings and published papers (see the appended lists of meetings and papers).

VA Hospital–Two of Us

After Powers left to pursue his own objectives (1961), McFarland and I continued to work together. I transferred to the Neuropsychiatric Service, working more closely with McFarland. About this time, Dick Robertson became interested in applying these developing ideas to his own interests.

McFarland arranged for me to participate as co-therapist with a group of VA patients. A couple of events that impressed me were these:

One day, Bob announced that I would have to conduct the meeting alone—he had other commitments. I was concerned that I might

"somehow do some harm." Bob pointed out that it is at least as hard to make a mental patient worse as it is to make him or her better, so that I need not be concerned about accidental "errors."

A patient reported repeated problems with another patient (not present) at a water cooler. Suggestions that she try a minor change in the way she spoke to him were immediately and strongly rejected. Indeed, a general characteristic of the group was the rejection of any and all attempts to try something different. This seemed likely to be one result of being locked in some form of internal conflict, while including something that precluded operation of the Reorganizing System. Of course, this is not necessarily true of all mental patients, there are many other possibilities. However, there seemed to be an implied "I am what I am, and that's all I am." Even the least attempt to change was avoided.

A study on the effects of drugs on "laboratory induced anxiety" (conceived by the Chief of the Service) led to my learning more statistics than I had needed for physics. "Statistical inference" was included. I found "non-parametric statistics" quite a different and interesting basis for experimental design. Two papers on computerization resulted from this work.

The first of these was presented at the annual meeting of the Society for Psychophysiological Research in October, 1962. This was held in Denver, which made it convenient for us (McFarland and me) to visit Jay Shurley's Sensory Deprivation Facility in Oklahoma City. The subject floated in a skin-temperature water tank in a dark soundproof room. Excellent design. Both Bob and I tried this out. As I recall it, the conclusion was that after sleeping a bit, the subject's expectations largely determined his experiences. I had the odd experience of the "disappearance" of my left arm from the wrist to above the elbow. It reappeared on the slightest movement, so I concluded that the water temperature matched the skin temperature of this part of my arm so closely that I no longer had any sensory inputs from that area. With muscles relaxed and joints stationary, there were no other signals available.

The second paper was presented at the New York Academy of Sciences Conference on Computers in Medicine and Biology in May, 1963.

The "anxiety" project was the basis of seeking funds for a computerized data gathering and reducing system. Although McFarland had been informed by Central Office that funds would be approved, he learned that the Chief of the Service had rejected them without conferring with either of us. We both resigned in protest.

The Human Systems Institute

In 1963, we formed the Human Systems Institute. This was intended to be a Tax Exempt Organization, but, as I learned much later, we did not know how to get IRS approval—and our attorney was of no help! This was the year that McFarland was the president of the Illinois Psychological Association.

During this period, we had several interesting projects.

For Illinois Bell, we analyzed a management position with a view to providing a training program at some later date. When we reported that this position was obsolete and unnecessary, the training program was dropped. Meanwhile, this produced some income for us.

Another project was computerization of a Career Profile Test for one of Bob's psychologist friends.

In addition, we submitted a grant request in collaboration with IIT Research Institute—an education in grantsmanship!

Also, through his connections, Bob arranged clients for a project in the therapy of adult stutterers. This led to meetings with several speech therapists, as well as a presentation at a national meeting of speech therapists. Interestingly, in the informal meeting afterward, one of their members asked if I had been a stutterer. "Not to my knowledge." He then stated that he was a "cured" stutterer, and that my purely theoretical analysis fitted his experience exactly! Theory confirmed by experience!

In 1965, funds were running out, so HSI had to terminate. I returned to teaching as Associate Professor of Physics at the Chicago Circle Campus of the University of Illinois.

The Mosier Safe Company

In 1968, I improved my economic condition by moving to Cincinnati and joining the Mosier Safe Company, becoming Manager of Applied Research. My primary responsibility was the development of an automated identification system. In this connection, I investigated signatures, voiceprints, and fingerprints, as well as several other concepts. I participated in evaluation of other systems that were offered to Mosier.

In addition, I supervised Computer Applications, Test & Evaluation, Materials Lab, and Special Projects. I brought the heads of these several groups together for discussions of the various projects and related matters. I was surprised to find that some of these people expected me, as "Boss," to "know all the answers." However, they generally seemed pleased that I recognized their capabilities and respected them as individuals. (PCT paid off in terms of general attitude and cooperation.)

Assembly/editing of "Technological Forecasts," written by the engineers of R&D, was also my responsibility.

When I joined Mosier, the management was in the process of implementing the 'Profession of Management Program," produced by some industrial psychologists (names no longer available to me). This

was a very ambitious program, taught first to top management, who then taught it to their subordinates, and so on down the line. This material was based on the concept that management is, somehow, an identifiable skill that can be learned independently of other knowledge and skills. Thanks in part to my PCT-related background, I was able to review this rather extensive material in a couple of months of spare time. It was pretty much a mix of the obvious and the unnecessary. For example, they placed great emphasis on "communication." Sensible enough; but they neglected to indicate when, where, and to whom to communicate about what. It was also pretty clear that an effective manager must know quite a bit in addition to general principles of management. The behavior of the old-timers was as might have been expected: they learned the special language and could recite it when necessary. But they rarely made any application to their previous methods.

I was able to attend additional internal courses in management and finance. It was interesting to see how the accounting was handled in the transfer of income from Mosler to its owner, American Standard.

I was quite surprised at a pricing decision that was made after a very coherent presentation by Mosler's quite competent marketing people. After seeming to understand the survey data and the logic that clearly demonstrated an optimum pricing strategy, the key vice-presidents went for a minimum initial price! And the analysis really wasn't that difficult.

Perhaps the closest I came to direct application of Perceptual Control Theory was in solving a paint matching problem. Here I learned more than I really wanted to know about paint manufacture and application. The problem was that furniture for bank lobbies was manufactured in plants in two different cities. The color depended on several interacting variables. To control each of them in proper balance would have been unmanageable. One of the important variables was adequate stirring of the paint, which was controlled by the individual painters. My solution was to provide sets of reference chips to each painter and to their quality control people. This, of course, was the right concept, but incomplete. Another level of control was needed. Therefore, I required painting a test chip in each production batch. This freshly painted chip was to be compared to the local quality control reference chip, then forwarded to me for final approval. So far as I ever learned, this seemed to have solved the problem.

Is this a two- or three-level hierarchical control system? Where is the test of the controlled variable? As long as the completed units match, there is no disturbance. The controlled variable is revealed only when the completed units fail to match.

Mosler's Central R&D was terminated in 1976.

DFS and Insurance

Being a bit old to find a job in technical management, which would have been my preference, I accepted an opportunity to join "DFS," Diversified Financial Services, Inc., a small company. Max Redlich, the owner, had a background in life and health insurance, as well as the retail furniture business. As a Financial Consultant, he had several clients who were more or less on the edge of bankruptcy. My part was helping with the handwork, accounting, etc. There were several interesting experiences during this period, without a direct relation to PCT, but relating to people—where PCT is always helpful. Later I became a co-owner, and a third person was added. We were working with a computer programmer to develop a program for retail business management. But a key client, refusing to accept our advice, was forced into bankruptcy. Here I learned a good bit about how bankruptcy works: how one can deal with the IRS, how to work with banks in refinancing loans, etc. A PCT background is very helpful in understanding the interactions among people in these sometimes-tense situations.

When we found we had gone too far for a specific client, it became necessary to dissolve the company. Max returned to his initial field, life and health insurance, as well as pension planning and administration. 1 stayed with him to help with the planning and administration.

During this period, I "officially" retired—that is, I started to receive my Social Security benefits. So that I could work with Max as an independent consultant, my wife Mary Ann and I formed an S-Corporation in 1980. Nothing else really changed.

When the insurance agency we were affiliated with suddenly had its General Agent replaced, Max and I, as individuals, both moved to The Lincoln National Life Insurance Company. I was to provide a communication link between the home office and agents having pension clients. To me, it looked like an unnecessary linkage, but then I was new to the agency system. And there were funds available.

I learned a lot of things about the insurance business and had a lot of interesting experiences with insurance people.

The first clue I got to the agency system was when I asked for my job description. The General Agent tried to provide one, but it was clear that he was entirely unacquainted with the concept. It became clear that I was really expected to sell pension plans—but that was never spelled out.

1 found that the agency system is not a hierarchical structure. Rather, the General Agent (who is paid on the basis of the production of his affiliated agents) provides facilities and services to the individual agents. The agency provides forms, advertising, sales materials, etc.—produced by the home office. In some cases, the General Agent charges

the affiliated agents for the use of office space and/or other items. But he gives them no direction beyond basic "training" in the company's requirements and the use of the many forms. The agency might provide additional seminars in sales presentations and methods.

As a form of sales training, the agency brought in a consultant who presented a series of seminars and workshops produced by the Wilson Learning Corporation of Eden Prairie, Minnesota. This series was based on a two-dimensional classification of "Social Styles." I found it generally consistent with PCT, but the basic concept of feedback control was only included by implication. This approach could be considered for revision and, possibly, integration with PCT's higher orders.

During this period, I completed both the Chartered Life Underwriter and the Chartered Financial Consultant courses. These included about a dozen quite respectable college junior- and senior-level one-semester courses covering such topics as accounting, taxation, economics, investments, and other appropriate topics.

While associated with Redlich, I had become fully licensed for sales of life and health insurance, as well as pension plans. I later qualified for sales of plans having investment aspects. I still retain this licensing. In addition, I became a "Registered Representative" with Lincoln National for selling mutual funds. I later discontinued this because of the continuing paperwork required.

I also learned about the selling process—from the agent's viewpoint. Essentially, an insurance agent is a true entrepreneur. And his/her most important asset is at least 200 satisfied clients. While an income is obviously essential, the most effective agents are those who enjoy providing for their clients' desires. Their clients think of them as friends rather than salespersons. The successful agent accepts the prospect's solution and sells him/her the implementation. (If the customer wants a Cadillac, you don't try to talk him/her into a Volkswagen—no matter that it would be much better for him/her.)

However, my personal orientation tended to be too much one of trying to solve the prospect's problem and then selling him or her my solution. At this point, I ceased active sales efforts.

AARP

While I was still involved with insurance, I learned that the AARP (American Association of Retired Persons) includes Chapters. I joined one nearby. Their newsletter was being distributed at meetings instead of being mailed. I joined with others to sell advertising for non-profit-organization postage. With a mailing list of over 200, this helped increase the membership and meeting attendance. I became the Legislative Chairman, reporting activities of both the State

Legislative Committee and the National Legislative Committee. Later, I was appointed to the Ohio State Legislative Committee. In this capacity, I formed a Legislative Council consisting of the presidents and legislative chairmen of the 19 Chapters located in the five counties in the southwest corner of Ohio. We arranged to videotape the monthly meetings for presentation on the public cable system. In this position, as a Registered Lobbyist, I learned a great deal about the details of the legislative process. I also worked with several of the Chapters in conducting Candidate Forums during election season.

In the course of these activities, I had learned about the internal operations of the AARP. I saw the operation of their structure of volunteer leaders as they were guided by a staff of permanent employees. This was very instructive.

In July 1987, after over three years, my appointment to the Ohio State Legislative Committee terminated. For several reasons, I ceased to work further with the AARP organization, but I do retain my membership.

Contact!

Sometime in the fall of '87, the phone rang—and I heard a voice from the past! Bill Powers had found me in Cincinnati!

We exchanged a few letters in which we discussed some of the ideas I had been developing over the years since Bill had left. I was working from Parts I and H of the 1960 publication, and from the ideas McFarland and I had developed. At that time, I had not even heard of Bill's 1973 book, *Behavior: The Control of Perception (BCP).* I was interested in the Orders above Fourth, which were not yet well worked out. From my association with people of highly varied backgrounds, it occurred to me that "Fifth Order," as I had conceived it, could be subdivided into "Modes" corresponding to the various Orders. This was based on classifying the topics of ordinary conversation. The irregular correspondence with Bill did not develop further at that time.

Greg Williams visited me while he was in Cincinnati at Christmas time in 1987. From him, I learned of *BCP* and other events. Greg and I corresponded irregularly for a couple of years. I sent him some of my notes and preliminary drafts, including a discussion of "Fifth Order." He reciprocated with copies of *Continuing the Conversation, Closed Loop,* and various papers. He also sent me a copy of *Living Control Systems: Selected Papers of William T. Powers.* This covered the years 1957 to 1988. Bill sent me a second, autographed copy about the same time. *Behavior: The Control of Perception* arrived from the publisher a bit later.

Through Greg, I learned about CSGnet. I got a modem and connected to the net in September 1991. In December 1992, I posted my first discussion of the "Decision Making Entity" (DME), including an extension of the concept of "Modes of Fifth Order." My contact with the net continues.

Museum

Meanwhile, in July, 1989, I learned that a small museum was being formed. The Archimedes Rotorcraft and V /STOL Museum involved rotary wing aircraft, in which I had long been interested. On joining the group, I found that they assumed that non-profit organizations are automatically tax exempt. From my insurance studies, I knew that this was insufficient. The regional head of IRS Tax Exempt Organizations was more than willing to give us the guidance we needed. He was particularly helpful with the documentation needed to get the official recognition letter. It took about three months instead of the usual 18 to 24 months!

I was a Board Member, wrote the By-laws, and became the Secretary, Treasurer, and Editor/Publisher of our monthly newsletter. While the membership was not large, it was international in scope. The newsletter included historical notes written by a past Director of Flight Dynamics at the Wright-Patterson Air Force Base. He and I both provided technical papers related to the engineering and operation of rotary wing aircraft. A small gift shop was included.

For two successive years I participated, as Financial Chairman, in planning the Annual Convention of the Popular Rotorcraft Association. I managed the publication of the Convention Program, as well as selling local ads for it.

After about 3 years, I resigned all connection with the Museum. It had become clear that more time was needed than I cared to spend on this activity.

Civic Activities

During the latter period of my insurance activities, and continuing through the AARP and museum periods, I became interested in local community organizations. The first of these was the Forest Park Business Association. I found that this group of business people did little long-term planning of the Association's affairs! After being elected to their board, I worked with another Board Member in rewriting the Bylaws. Later, I dropped the Board Membership, but I still continue my membership in the organization.

When I decided to study the City of Forest Park, I became Legislative Chairman of the Business Association. This facilitated my contacts with the City of Forest Park. For the past year, I have attended all scheduled meetings of the Administrative Staff and the City Council, most of the meetings of the several Commissions, and some of the meetings of the Council's Standing Committees.

PCT concepts and methods have been very helpful in working with these people—with no need to try to teach PCT to them. On several occasions, I have helped resolve developing conflicts. "Moving up a level" is always useful—although not always easy! Another helpful approach is emphasis on "Reality"—that is, current perceptions, otherwise known as: "What's happening *now?*"

A recent campaign for election to City Council was most interesting—I knew most of the 13 candidates and found that they campaigned almost exclusively on the basis of their personalities! Virtually no policy or other proposals were discussed! That is, there was very little consideration of lower-level problems and their relation to higher-level policies.

Throughout this period, I found the interactions among personalities at least as interesting and pertinent as considerations of organization and operation. Personalities and their interactions certainly pertain to the higher levels of human systems.

Recently, I became a member of the Civil Service Commission. I joined this Commission because it works closely with the Human Resources Department and the City Manager in personnel-related matters. It is, therefore, a place where PCT is directly pertinent both in personnel decision-making and in conflict resolution.

Future

When one turns to the future, one finds a mixture of projections and hopes—both truly imaginary.

In seeking to reach a larger and more understanding audience, I expect there will be extensions, elaborations, and modifications of the basic theory. The levels of the hierarchy will be studied in more detail and become more meaningful and available for application. While a specialized vocabulary has its place in technical discussions, expressing the concepts in more ordinary terminology will be necessary for more general understanding. As this is accomplished, both the number of participants and the applications of PCT will increase steadily.

In a view of the future, we find applications that are waiting for study and development. These might include the following:

Economics. Here we can expect to find recognition of the multiplicity of peoples' motives. Projections that go beyond static or linear methods will be examined. Situations where decision-makers have incomplete and/or incorrect information will be analyzed. Alternative specifications for the various levels of the hierarchy—especially those above purely mechanical systems—will be proposed.

Sociology. Group behavior where individuals have differing objectives will be analyzed. How cooperation occurs and how conflicts are resolved will be studied. Various forms of organization will be examined from the standpoint of the effects on the individual's freedom.

Education. Improved understanding of the "learning process" will be developed. Many new methods and procedures are being studied—mostly from the standpoint of teaching, rather than learning. PCT will be particularly helpful in these efforts.

Mental Illness and Psychotherapy. To the extent that such problems are the consequence of inadequate (incorrect?) learning, PCT has already been helpful. As PCT becomes better known in the general community, these important matters will become better understood. PCT methods will also assist in distinguishing between organic and functional problems.

In the course of these developments, I expect to find modifications and/or additions to the basic theory. These might include the following:

Emotions. The relation between imagination and emotion will be clarified. The physiological effects of different emotions—friendship, affection, loneliness, aversion, and many others—will be explored. This important area has barely been touched.

Memory. The relation between memory and the hierarchy will be extended to include those memories not directly used for operation of the control systems. How formation of memories can be improved and controlled will be examined, as will how and why availability of memories can be limited, even while their existence is beyond question.

Conflict Theory. This important topic will be extended to include conflicts between organizations as well as internal conflicts between anticipated (imagined) events. This will help clarify the operation of the DME.

Reorganization. Recognition of the special requirements for this critical process will be clarified. The role of intrinsic error in initiating Reorganization will be analyzed. Relations among memories, the planning process, and the operation of the DME will be clarified.

To accomplish all these developments and applications, as well as others, will take much time and effort. It will require multiple participants, mutual understanding, and cooperation. I anticipate and hope for the development of the kind of interactions I enjoyed in working with Bill Powers, Bob McFarland, and others some 30 to 40 years ago—but on a much larger scale! On with the show!

Meetings Attended as a Speaker:

Symposium—American Psychological Association Meeting, Washington, D.C., August 30, 1958. Papers by R. K. Clark, W. T. Powers, J. Arbit, V. Buskirk, and R. L. McFarland.

Symposium on a General Feedback Model of Behavior, All-University Seminar, Urbana, Illinois, November 1958. Papers by R. K. Clark and others.

Symposium in Neural Mechanisms, Information Theory and Behavior, VA Hospital, Battle Creek, Michigan, March 10 & 11, 1960. Papers by R. K. Clark, W. T. Powers, R. L. McFarland, and others.

Northwestern University Society for Neuroelectrokinetics, Evanston, Illinois, December 1960. Paper by R. K. Clark and others.

Westsuburban Psychologist's Association, Moosehart, Illinois, February 1961. Paper by R. K. Clark and others.

Bio-Medical Engineering Colloquium, Northwestern University, Evanston, Illinois, March 30, 1961. Paper by R. K. Clark.

Symposium—American Psychological Association Meeting, New York City, September, 1961. Papers by R. K. Clark (two), R. L. McFarland (two), and R. J. Robertson.

American Academy of Psychotherapists—Seventh Annual Conference, Chicago, Illinois, October, 1962. Invited paper by R. K. Clark.

Cleveland Physics Society, Cleveland, Ohio, November 1962. Invited paper by R. K. Clark.

Society for Psychophysiological Research—Second Annual Meeting, Denver, Colorado, October 13 & 14, 1962. Papers by R. K. Clark and representatives of other research groups.

New York Academy of Sciences—Section of Biological and Medical Sciences, Conference on Computers in Medicine and Biology, New York City, May 1963. Paper by R. K. Clark, R. L. McFarland, and M. Bassan.

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Clark, R. K., McFarland R. L., & Powers, W. T. (1957). A general feedback theory of human behavior. *University of Chicago Counseling Center Discussion Papers, 3(18).*

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Clark, R. K. (1960, March). *A general feedback theory of human behavior.*

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Powers W. T., Clark, R. K., & McFarland, R. L. (1960). A general feedback theory of human behavior: Part I. *Perceptual and Motor Skills, 11,* 71-88.

Powers W. T., Clark, R. K., & McFarland, R. L. (1960). A general feedback theory of human behavior: Part II. *Perceptual and Motor Skills,* 11, 309323.

Clark, It K. (1960, December). *A general theory of human behavior from the viewpoint of physical science.* Paper presented at the meeting of the Northwestern University Society for Neuroelectrokinetics, Evanston, IL.

Clark, R. K. (1%1, March). *Human behavior as an organization of feedback systems.* Paper presented at the Bio-Medical Engineering Colloquium, Northwestern University, Evanston, IL.

Clark, R. K. (1961, September). *A brief overview of general feedback theory.* Paper presented at the meeting of the American Psychological Association, *New* York.

Clark, R. K. (1961, September). *The group therapy process scale and the personal assessment program.* Paper presented at the meeting of the American Psychological Association, New York.

Clark, R. K. (1962). A general theory of human behavior from the viewpoint of physical science. *Newsletter for Research in Psychology, 4(2).*

Clark, R. K., & McFarland, R. L. (1962, October). *How can the scientist help the psychotherapist?* Paper presented at the Seventh Annual Conference of the American Academy of Psychotherapists, Chicago.

Clark, R. K., Chessick, R. D., & McFarland, R. L. (1962). High speed data processing—Compromises and considerations. *Psychophysiology Newsletter, 8(4).*

Clark, R. K. (1962, October). *Feedback system analysis of behavior.* Paper presented at the meeting of the Radiation and Medical Physics Society of Illinois, Chicago.

Clark, R. K. (1962, November). *A "systems oriented" theory of behavior.* Paper presented at the meeting of the Cleveland Physics Society, Cleveland, OH.

Clark, R. K., & McFarland, R. L. (1963). Systems concept of stimulus.

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New York Academy of Sciences, 115, 905914.

Chessick, R. D., McFarland, R. L., Clark, R. K., Hammer, M. & Bassan, M. (1966). The effect of morphine, chlorpromazine, pentobarbital and placebo on "anxiety." *Journal of Nervous and Mental Disease, 141(5).*

The Early Days of Perceptual Control Theory: One Person's View

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Introduction

One of the net losses of modem civilization, it seems to me, is the loss of adventure in contemporary life. To be sure, in our generation we have had astronauts going to the moon, people living on the sea floor, explorers of new life styles, and pioneers in science, but these are uncommon and rare people. I believe that we have lost adventure in the lives of ordinary people, like the pioneers who pushed toward the ever-receding frontier. Their lives were filled with challenges simply in the course of pursuing a livelihood. That frontier is gone—the tangible, geographic one. What we have left are the pinnacles gained by the few explorers in the fields of endeavor that are not yet purely cut and dried.

So it struck me with quite some surprise, recently, that throughout the years while I have been bemoaning the lack of adventure in modem life, I have been on an exciting voyage of discovery without realizing it. While fiddling with the television controller the other night, I happened to stumble on the movie "Columbus." As I watched Columbus journey from place to place begging for a hearing and being put down by most of the smart and powerful of his time, I began to get a feeling of familiarity. I know a captain and a crew who have had a similar experience. Let me tell you about it.

I seem always to have enjoyed taking the historical approach to understanding things in which I am interested. I want to see what follows from what. Thus, it feels natural to try to pin down why it was that, in 1957, when three guys came to the University of Chicago Counseling Center to give an hour's lecture on a new approach in psychology, I was ripe for that one hour to start gears rolling that would give direction to the rest of my life. It happened in our Thursday afternoon open seminar, during my internship. I had been interested in psychology ever since I read through an ancient text by Pillsbury that an uncle had left lying around our house from his college days, in my first or second year of high school. But in graduate school I had been a somewhat indifferent student. In fact, when I took my M.A. in Human Development in 1952, the department suggested that I should probably regard it as a terminal degree. They said I didn't have much ability to conceptualize.

I felt that they might be right. I was aware that I could not keep straight all the distinctions between positive reinforcers of desirable behavior, and negative reinforcers of undesirable behavior, and positive reinforcement of stopping undesirable behavior, and cessation of aversive reinforcement of desirable behavior, and several further refinements. (I still can't keep them straight, but, thank goodness, now I don't have to.) I knew that I got bored and tended to drift away when trying to memorize these concepts and their definitions.

I had found Freud's ideas more stimulating, for the most part, but I noticed eventually that his "explanations" of behavior take the form: The reason A causes B is that when you observe B on many occasions, you usually find A preceding it. In contrast to this, in my physiology courses I was learning explanations for things like blood pressure in terms of stroke volume, heart rate, and resistances in the system. That struck me as more like what I thought "explanation" should mean. I didn't find explanations like that in any of my psychology courses. I had not been doing terribly well in many of them, either; I seemed to keep asking the wrong questions-like, 'What would be happening in the brain when a person is having a given experience?" So I took the advice of the departmental counselor, and my M.A. and I went out into the business world.

However, after a few years of employment as an industrial trainer and, later, job analyst, I realized that I would soon die of boredom at such work. So I talked my way back into the university, partly thanks to the good graces of Carl Rogers. I had taken a number of his courses and had been intrigued by his point that one learned much more about the behavior of an individual by trying to view the world through that person's eyes, rather than by surmising about what was going on inside by observations from outside.

In his theory of personality, Rogers (1959) declared that we all live in a world of our own perceptions. This idea had a profound place in his conception of what therapy is about-that by reflecting a person's message, you will help him or her see more clearly what reality he or she is perceiving (and coping with). This concept might have helped make me receptive, later on, to the idea that behavior is the control of perception. I'm not sure about that; it seems connected to me. However that might be, what I am sure of is my state of excitement after hearing those three guys talk on a "General Feedback Theory of Behavior" on that Thursday afternoon. A year or two later, when I had my Ph.D.

in hand, I took my first job four-fifths time, so as to be able to volunteer one day a week at VA Research Hospital in Chicago, where Bob McFarland and Bob Clark were working with the theory. Their new conceptions were that alluring.

Early Research on a Feedback Theory of Behavior

Once I had begun to learn the theory from Clark and McFarland, they gave me a job running subjects on a gray box with four red lights and four pushbuttons on the front that Bill Powers had left behind when he moved on to study and work at Northwestern University. This box presented a game that the subject had to learn totally through experience and could only win by stumbling onto anticipating the moves of the machine. After running a subject, I would sit and measure the distance between blips on the readout tape and plot the resulting pattern of reaction times on a graph.

The results often showed a neat, descending curve with three reaction-time (RT) plateaus, reflecting the three levels of skill that had to be mastered in a successful performance: (1) the pattern of key-light connections (control of the finger-position configurations); (2) the order of finger pushes (control of the sequence of events); (3) the plateau of negative reaction times where the subject was anticipating the machine (control of the time relationship).

I say that the results "often" showed this neat picture. But not always. The RT patterns of some subjects were simply chaotic—my term for a graph showing no plateaus, in which the RT's appear to be scattered at random. The same phenomenon occurred years later, when I was teaching at Northeastern Illinois University and ran the experiment again, this time on computerized apparatus that Bill Powers instrumented for me. Once again, there was the same mix of neat three-plateau curves and random patterns. But this time, I had obtained verbalizations from the subjects as they went through the task. Certain of them seemed quite significant. I found that only those with the patterned graph could articulate the concept that the way to win the game was to beat (anticipate) the machine. In both the earlier and later programs, the subjects with random patterns fell into two groups. Either they never did win, or if they stumbled accidentally on a win, they could not say how it worked.

The original research was published in-house in Northwestern University Psychiatry Research Papers. I took an illustrative curve from that study for my article on control theory in the Wiley *Encyclopedia of Psychology* (Robertson, 1984). The replication was published in *Perceptual and Motor Skills* (Robertson & Glines, 1985) as a rebuttal to behaviorist Keller's (1958) attempt to discredit Bryan and Harter's

(1899) description of plateaus in learning telegraphic communication. These experiments, conducted over a period of 20 years with subjects of presumably different demographic characteristics and different instrumentation show what I consider a very robust result. The curve taken from the original data for the *Encyclopedia* article and that taken from the later data for the *Perceptual and Motor Skills* article both show similar plateaus and increasing variability before plateau shifts. Despite different overall length of the graphs, these features look similar, as do also the mean RT's per plateau. That suggests to me that we are observing something fundamental about the way people, in general, learn tasks of this sort. First you learn a set of configurations, then the sequence in which these configurations apply, then you play with the sequence in time until it dawns on you to precede the machine. Reaction times decrease at each step: when you know which key punch turns off which part of the display, you can be ready to strike as soon as it comes on; many subjects try to time it to the instant and thus stumble on anticipation by being too fast.

Early Applications: Clinical and Rehabilitation Work

Bob McFarland suggested an explanation of the other phenomenon depicted in the 'Towers Game" experiments: the characteristic increase in RT variance just prior to the drop to the next plateau. He proposed that the subject was experimenting with his/her performance when mastery of the first level did not result in a win. Later on, in Powers' (1973) discussion of reorganization, we have a theoretical explanation. He states: 'The effects of the outputs of the reorganizing system must be such as to change the properties of behavioral systems... as a result, of course, visible behavior would change its character, as would experienced behavior." (page 185)

Powers made the implication that the organism does not know exactly what change must occur. Random excitation caused by the reorganizing system results in various alterations of action. Then the action that begins to bring the desired objective under control becomes the basis of a new control system. The increased variability of RT's prior to the drop to the next plateau certainly appear to be instances of such reorganization outputs. The subjects, in their spontaneous verbalizations while doing the experiment, say things like, "Hmm, I know which key turns out that light, but do they come on in a fixed order or any old order? Hmm, I think they always come on in the same order. Oh, I've got it, they come on in this order" (rapidly extinguishing the machine display in fixed sequence). Their RT's often slow up while they pause to think, then become very fast when they try out their hypotheses.

At the time I began to think about reorganization during the original experiment, I was involved in my professional life as a clinical psychologist in a rehabilitation hospital. I would often have patients with aphasia or other cognitive deficits whom I would try to help rebuild lost capacities. What better occupation in which to experiment with reorganization? I was also learning from Clark and McFarland that they had had some good results in working with veterans by applying the scheme of the control system hierarchy (the original version, Powers, Clark, & McFarland, 1960a, b). Noting a lack of some particular skill in a patient, they would examine the immediately lower orders in his hierarchy and construct drills on weak aspects of the presumed foundation.

Following their lead, I would approach a patient who, for example, could not draw a straight line, by getting him to make any mark on a piece of paper. Then I would get him to draw the mark from one dot on the paper to another immediately adjacent dot, and then move the dots progressively farther apart. Next we would employ the technique to copying letters of his name, eventually making a signature. The idea of reorganization underlying this is to encourage perceiving the task as a sequence of moves across the whole signature, rather than as isolated drawing movements.

Shortly thereafter, I went into Veterans Administration research, where various small successes continued to show the promise of developing and applying "reorganization theory" (if I may be permitted to glorify it with a name). I will cite two of them which illustrate how just the bare bones of a good concept can lead to useful applications. One day, one of my research assistants came running fearfully into the office saying that a veteran, to whom she had tried to administer our 20-odd-page questionnaire, had chased her out of his room, menacingly. A moment later, he walked in cursing about invasion of his privacy and with a few other complaints about the questionnaire. I noticed the large metal plate in the front left portion of his skull and surmised that as he looked at the thick questionnaire, the idea of replying to it as a whole might have felt overwhelming. I said, "Oh, sure, no problem, but you wouldn't mind telling me your birth date here, would you?" (As I indicated the beginning question.) He complied with my request, and I then asked if he minded telling me the next piece of information, and so on, until we had completed the entire questionnaire.

I cite this as an example of how a good concept/theory provides lines of action that would not otherwise occur to a person. In this case, my impression that this man lacked a lot of computing ability in the brain area that is usually attributed with sequence-controlling properties, plus my experience with the usefulness of "order-reductions" in McFarland's and Clark's training efforts, led to the technique of pointing the patient at each question as a separate event and urging his attention away from the task as a whole.

The other story from this era is about the satisfaction that comes from using theory to make sense out of an otherwise puzzling observation. One of the VA patients happened to be a former state table tennis champion. Under his leadership, we soon had a large number of staff and patients involved in a round of ping pang tournaments. During the tournaments, a number of us noted a curious change that would quite regularly occur in the play of a contestant when he began to recognize that he was clearly outmatched. He would first concentrate very hard, then begin to alternate between wild shots and cautious play. It occurred to me that these periods of variability, if we could graph them, would look like the patterns of RT variation preceding a new plateau in the Powers Game experiment. The participants themselves acknowledged this aspect of play as part of their experiments to obtain eventual increases in skill. In this view, what would have seemed a lapse into sloppiness on the part of a losing player took on an opposite significance.

Powers' Book and Further Developments

I left VA research to take a position in the Psychology Department of Northeastern Illinois University. I soon began to offer a seminar to a few select students in which I used Powers, Clark, & McFarland (1960a, b) as the main text, supplemented by notes on my experiences in learning the theory and a few other reprints from Clark and McFarland. After giving the class a few times, I had four students who really grasped the theory and began to use it in constructive ways in their own lives. One of them made what I consider a profound use of the concept of reorganization in clinical work—one that I continue to find invaluable in my own practice of psychotherapy.

She was doing a fieldwork project in a psychosomatic ward of a large general hospital. She was allowed to talk with patients as a kind of supportive "mentor," being a graduate of the ward herself. One day, one of the patients was threatening to withdraw from the program, complaining of severe anxiety, saying, "Nothing seems right, all my ways of thinking about things are up for grabs." My student had a powerful "Aha!" at that point. She said, 'Wait a minute, that is just what you should be experiencing. If you were still reacting in the way you used to, you would be doing what got you here in the first place. The fact that nothing seems right is because you have changed. You are no longer perceiving things as you did, and so your experience does not feel familiar." With this insight, the patient chose to tolerate her unsettled state a while longer and eventually proved a success in the program.

It was at about this time that *Behavior: The Control of Perception* (Powers, 1973) came out. We devoured it eagerly, and it helped us draw several further applications from this experience with the patient on the psychosomatic ward. It occurred to us, after reading Powers' chapter on learning, that this patient's complaint of everything being up for grabs was in a certain sense similar to the variability in performance in the learning experiments and the ping pong tournaments. Having become disillusioned with her old ways of thinking about her experiences, she wavered through various new percepts. In Powers' theory of reorganization, this would be the result of random signals in the existing hierarchy. They would, of course, interfere with the functioning of some of the existing systems. As she began to settle upon new, more effective ways to view her situation, she reported that her anxiety dissolved. We began to form a wholly new idea about anxiety as a result of these observations.

Clinical reports in the field of psychotherapy have frequently noted that patients complain of anxiety as they move deeper into self-examination. Clinicians have typically treated anxiety as a condition to be gotten rid of, rather than as an indication of an underlying process of reorganization. However, it seemed to us that—if Powers's concept of the control systems of a person as a huge interconnected hierarchy were right—then of course when the reorganizing system begins to alter the parameters of some systems in the body, many other systems that interact with them would be plunged into varying degrees of error. It struck us that anxiety could be the name given to that condition. Later on, I ran across other clinicians whose experience had also suggested that anxiety in treatment often appears to be a precursor of major change. I have subsequently had many instances where simply offering this view of anxiety has helped a person to refocus on their desired changes, instead of on the symptoms of anxiety, and to achieve a good outcome.

Another application of PCT in my clinical work has been to encourage patients very firmly to keep stating their goals or objectives as specifically as possible, a method I share with David Goldstein and Ed Ford, although each of us seems to do it in a slightly different way. My favorite procedure is to ask the person repeatedly to state what he or she wants to perceive or experience in literal terms. For example, "So, you want to hear your boss say, 'You're the hardest worker here,' is that right? How close has he come to saying just that? What do you do that gets him to say anything like that?" It continues to surprise me how often a person is looking for a particular feeling but has hardly any idea of the kind of events that must occur for him or her to get that feeling.

Once patients get the idea that the good feelings being sought are closely tied to specific events, they usually take off with the concept, making further applications on their own. I wrote an applied control-theory psychology text for my students in an introductory mental health course, to pass along these observations. It gave a simplified sketch of the theory, showing how it had led to these and other applications. I also offered it to a number of "pop" psychology publishers. I heard from several of them that the first reader or two liked it, but it was always rejected at higher levels. A fair number of my students grasped enough of the main idea to make their own applications, as the patients had done, so it seemed to have served some purpose anyway.

Later Research

Once I had *Behavior: The Control of Perception* as a text, my classes grew slightly larger, and I found some students who were interested in learning more about doing research from the control-theory point of view. We settled upon research on the self as a fertile ground. Carver and Scheier's book (1981) had come out by then. They reviewed many studies in social psychology dealing with various aspects of the self, which they interpreted, more or less, as having aspects of a higher-order control system. Since I had been interested in the self since way back in my days with Carl Rogers, I found their work of considerable interest. However, I wasn't satisfied with the relatively murky views of this concept that one finds in the literature on it. I proposed it as a subject for deeper investigation in one of my first advanced courses in perceptual control theory.

I have an unusually clear (for me, at least) recollection of the progress of that series of discussions. I think it illustrates well the development of a theoretical question through intermediate steps to a research program, so I would like to spell it out in detail. We began by speculating that at least some of the previous concepts of the self in the psychological literature seemed suggestive of control systems, even on the part of writers who had never heard of control theory. A good example is Epstein (1973), 'The Self-Concept Revisited, or a Theory of a Theory." He proposed that the term "self" is used to describe a conception, or theory, that a person develops to explain him- or herself to him- or herself for the purpose (among others) of knowing how to make difficult decisions.

In our discussions, we began to play with the idea that a self could then be thought of as a control system of the highest order. What would it control? We examined Powers's scheme of the learned hierarchy for types of variables controlled at the various levels. Variables like intensity, configuration, relationship, and sequence are, in a certain sense, very concrete. That is, you can construct objective measures for them, as Bill did in constructing tracking experiments where the relationship of "equal" or "in line" can be viewed directly between cursor and target on the screen. Now, what would be the counterpart of that at the level of a system controlling that a person would continue to be the same consistent person? It struck us at some point that we were seeing that in action all of the time. We noted that when we talk about ourselves, a portion of our talk consists of telling each other what kind of person we are. "I am a quiet person," "I am a talkative person," "I am a shy person," etc. These are the kinds of attributes, collections of which some writers called "self image."

At this point, we derived, from the theory presented in *Behavior: The Control of Perception*, an implication that had not been clear in previous studies of the self. In Powers' discussion of how you can determine whether you are observing a control system in action, he described the "test for the controlled variable." If a phenomenon is under feedback control, you will see it corrected back to its prior state if you disturb it. During one of our class discussions, one of the students made the self-image remark, "I am a shy person." I simply said, "No, you're not," as an attempt to apply Powers' test. Her jaw dropped, her eyes widened, and she said, with indignation in her voice, "I certainly am!"

Looking back, I marvel at how much more work it took to incorporate this simple experience into a workable experiment. I proposed at this point to David Goldstein that we work on it. But as traditionally trained psychologists, we seemed to have to go through a series of successive approximations to move from a traditional research format to a rigorous presentation of this original, simple, informal test of the controlled variable. I will describe the history and present the research here, since it is unlikely to be published anywhere else.

We began with a design typical of hundreds of studies on various aspects of the self. We got subjects to describe themselves and their ideal selves on an adjective checklist and then had them estimate where their own scores would fall on a wheel-like circumplex of eight personality factors (sociable, accepting, submissive, assertive, etc.) from Conte & Plutchick (1981). A week later, we gave them a doctored "personality profile report" in which some of the factors they had rated as self-descriptive were affirmed but others were reversed. Our rationale was that the false descriptions would result in a sense of error which they would then take some action to correct. We provided the opportunity to do that by describing the doctored feedback as a new experimental instrument and invited them to correct any attributions they thought the testing had got wrong. We then scored any statements they made to correct "wrong" descriptions as favoring the hypothesis and failure to contradict as against the hypothesis. Anyone familiar with the typical research in this area will recognize that this design follows a very usual format, as for example the study by Frey and Stahlberg (1986) that Runkel (1990) took apart in great detail in his text on psychological research methods.

Table 1 shows the results of three samples of subjects with whom this first design was employed. These results do not support the hypothesis. In the first sample, there were more instances of acceptance of false attributions than corrections. The second sample results were favorable to the hypothesis. In the third sample, there were more instances of correcting undoctored attributions. At that point, we faced a question common to many research projects of this sort. Was the hypothesis (that people would correct falsified self-descriptive attributions) disproved by the results, or was the experiment inadequate to the task? Like many researchers who have invested time and money, we preferred to believe that the method was inadequate.

Table 1. Reactions to receiving false attributions.

Subject sample number	N	Corrections of disturbed categories	Acceptances of false attributions	Corrections of undisturbed attributions	Ambiguous
1	10	9	10	6	5
2	12	13	9	6	8
3	12	8	2	18	8

Note: Each subject responded to three questions, hence frequencies show number of chances to correct; that is, three times the number of subjects.

One member of the research team, a schoolteacher familiar with students like our subjects, speculated that many of them did not have a very robust self concept. We went back to the drawing board, determined to control for a confounding factor such as ego strength. The data were already at hand in unanalysed information that we had gathered during the project. We tallied up the discrepancies for sample 1 between "actual" and "ideal" ratings that the subjects had given themselves on the circumplex measure, then defined a measure that we called "self-knowledge" as the inverse of the total. We split the sample at the median on this measure and cross-tabulated it with the correction data. This time, indeed, we found that the subjects with the higher "self-knowledge" performed according to the hypothesis, as compared with those low on "self-knowledge," as indicated in Table 2.

Table 2. Sample 1 subjects' reactions to receiving false attributions, by high and low self-knowledge groups.

Discrepancy	N	Corrections of disturbed categories	Acceptances of false attributions	Corrections of undisturbed attributions	Ambiguous
high	5	1	5	7	2
low	5	8	4	0	3

Note: High discrepancy is equivalent to low "self-knowledge," and vice versa.

A chi square on these results was significant at the .05 level, and we could presumably have had it published somewhere, had we stopped at this point. But we made the mistake of trying to replicate these results. The data for samples 2 and 3 came out in the opposite direction. The operational hypothesis was thus invalidated. I might note that, in all of these samples, there were contrary instances, and whatever differences were noted were only between group means, a fault that Kunkel (1990) has pointed out in almost all psychological research aimed at investigating properties of human beings *qua* human.

After some intermediate steps which are not worth describing here, we began to see that the concept of testing for a controlled variable calls for an entirely different research design—and for results that should be universal. The first inference we drew from careful thought about PCT was that the instance of disturbance of the self image, and its correction, if any, should be immediate in real time. We had realized that there is no particular justification for assuming that any individual is controlling exactly the same aspect of his or her self image a week later, as compared to the initial selection of adjectives.

We developed a format closely similar to the initial, informal situation from which the inquiry had started. We had subjects work in pairs in which one partner would do a Q-sort self-description with items from the original adjective check list. The other partner had been secretly instructed to read off the first item and say, "Why, no, you're not like that," immediately upon completion of the Q-sort. The complicit partner then wrote down the other's reply. We then had judges score replies like "I am so" as for, and all others as against the hypothesis. Table 3 presents these results.

Table 3. Subjects' replies to contradiction of self image in four samples, using the second design.

Subject sample number	N	Corrections	Non-corrections	Ambiguous
1	8	8	0	0
2	8	7	0	1
3	10	8	1	1
4	9	9	0	0

Summing the results of 35 subjects in 4 samples, we have 32 instances of correcting, one failure to correct, and two unscorable replies. This finding appears considerably more robust than one usually finds in typical research on this topic. However, we were uneasy about the deceptive aspect of the way in which the self image had been disturbed. Therefore, we designed a format in which subjects again made self-descriptions, but this time we asked them to imagine what they would say to someone who looked at their description and said that it was not accurate. As a control, we asked them to do the same with an arbitrary list of neutral adjectives. We had their answers rated by student judges according to whether they objected or not to the aspersion, as well as to the neutral terms. Table 4 shows those results.

Table 4. Written responses to hypothetical disturbance of self-chosen self-descriptive and neutral attributions.

Reactions to relevant statements				Reactions to neutral adjectives			
Subject	Correct	Modify	Accept	Subject	Correct	Modify	Accept
1	1	4	0	1	0	4	1
2	3	2	0	2	0	2	3
3	5	0	0	3	0	3	2
4	0	5	0	4	2	2	1
5	5	0	0	5	2	3	0
6	4	1	0	6	0	0	5
7	4	0	1	7	0	2	3
8	5	0	0	8	0	4	1
Total	27	12	1	Total	4	20	16

This methodology is rather simple and perhaps primitive, but its strength lies, I believe, in that it applied some rigor to something anyone can witness in everyday life. I have repeated the informal experiment now on hundreds of occasions with almost universally consistent results. Anyone else who wishes can do the same. It doesn't require any particular lab set-up or complex instrumentation. All that is required is to wait until a potential subject makes a self-descriptive remark and then contradict or interfere with it in some way and observe the result. I am satisfied that the objective has been achieved. There is almost invariably a strong correction to a disturbance of self-description when a person declares himself or herself to have such and such a characteristic. From that, I conclude that it is feasible to regard the "self" as a type of control system, and the "self image" as a type of controlled variable monitored by such a system.

Toward the Future

I have found it extremely exciting—and I still do—to be "along for the ride" in this paradigm revolution concerning the nature of behavior. I get a thrill when I experience the sense of simplification by seeing an odd collection of psychological "phenomena" as special cases of the same underlying process. For example, some of my most satisfying experiences in working on the textbook (Robertson & Powers, 1990) were insights such as when it occurred to me that "self-fulfilling prophecies," "experimenter bias," and learned helplessness" could all be seen as special cases of control of expectations. (The reference settings in each instance were established in the particular events used to define these "phenomena.") In the literature where they are introduced, they are offered as unique human processes, unrelated to each other.

The psychological literature is full of such cases. It is equivalent to the condition that would have obtained in physics before Isaac Newton. The motions of planetary bodies, apples falling from trees, and cannon balls would have all had to be explained with separate and unrelated "laws of nature." The lack of a unifying theory allows, nay, introduces many false complications into psychology and, at the same time, diverts energy from investigating the true complexity of living processes to the pursuit of many trivial distinctions and measurements.

I must acknowledge, of course, that drawing upon theory to attempt to simplify the underlying structure of phenomena is only the first step in gaining knowledge. Proposed simplifications are speculations that need confirming. That is, they need confirming in those instances where the surface phenomena continue to be interesting after one takes a look at them from a PCT point of view. I suspect that in many instances they will not. At least, I have stopped being amazed to have it pointed out that people regularly act, quite automatically, to bring about experiences that match their expectations—of whatever sort.

I believe, further, that there is a tremendously varied and exciting realm of possibilities for different directions in which to test out, and draw applications from, PCT. The study of some of the lower-order systems is well along in the various tracking experiments done by Powers, Bourbon, Marken, and others. The existence of higher-order systems, postulated by Powers, is to my mind established in the self research results. But there is much to be done in investigations of the intervening levels.

Some of that is already implicit in Bourbon's results with two-person interactions. I don't know if anyone is yet sketching out (or cataloging—is that a more apt term?) the principles and programs that different people implement in doing the tasks. Likewise, it will be very interesting as young investigators construct and test models of how different people choose a strategy for dealing with a task, the mastery of which is unknown to start with. The Powers Game is one type of activity where subjects' choices of strategies for mastery will be amenable to the test for the controlled variable. There obviously are many more.

There also needs to be research on the reorganization phenomenon in all kinds of learning situations. Many fertile questions about how reorganization proceeds have appeared on CSGnet in the past year. I would hope to see some of them instrumented and pursued in the near future.

There are many observational facts in psychology that might well be recast into PCT terms in an approach to find the underlying mechanism. For example, compare Plooij's (1987; 1989a, b) work on developmental sequences with that of Piaget. Piaget presents some excellent step-by-step descriptions of how behavior gradually becomes more complex in many skill dimensions. But his "theory" proposes "explanatory" concepts like "equilibration," which resolve into "it happens because that's how it happens," when analyzed. In contrast, Frans Plooij, also describing some invariant sequences in behavioral development, has related them to Powers' hierarchy of controlled perceptions and has shown how the more complex are combinations of the prior steps in development.

There are many observational facts in developmental, clinical, and social psychology, but very few genuine attempts to propose underlying mechanisms, outside of PCT and the field of psychobiology. In that field, control theory is beginning to be applied, but is hobbled by the kind of misunderstanding of it that Powers has spent so much time pointing out. And certainly, there are many potentially rewarding applications of PCT to follow those being begun by a growing number of us.

For the person who gets personal satisfaction from seeing an unknown shore come into view for the first time, as well as from the company of fellow voyagers, PCT has emotional and intellectual satisfaction to offer. I find it rewarding to interact personally at our small face-to-face PCT meetings with people whose specialties are distant from my own, as well as with those in the same field. I'm glad at such times that PCT hasn't attracted a great horde of people whom I wouldn't be able to know as individuals. I don't enjoy seeing anonymous faces, talking about things about which I have no chance to stop and ask what they really mean. For that reason I haven't attended an American Psychological Association convention in many years. I feel some sadness in knowing that this is already beginning to change in the CSG. When I see Bill Powers laboriously leading a querulous interloper in CSGnet through the careful first steps of understanding how behavior is the control of perception, I often get an impulse to shout, "Save yourself for those who come of their own accord!" And as to the fact that so many well-established people can't be converted, we have already sufficiently understood how PCT already predicted it. Let's not waste any more time about that. On with finding new facts and making new discoveries!

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Confessions of a Non-Pioneer

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The people who can write as legitimate PCT "pioneers" are Bill and Mary Powers and Bob Clark. The fact that Greg Williams includes me in a list of pioneers says much about the past, present, and probable future of PCT; the message is not encouraging. Much of what I have written here reveals the lack of any contribution by me during the early years of PCT. If what I say is of any value, it is probably by way of documenting how long some of the supposedly "new alternatives" and "new objections" to PCT have really been around.

1960-1973

In 1960, when Powers, Clark, and McFarland published their first papers on control system theory (CST) in *Perceptual and Motor Skills,* I was an undergraduate student, changing my major and my institution for the third time. I had completed four courses in psychology at a small private college, all under one Skinnerian radical behaviorist. Since my high school days, I had been interested in studying how people and their environments affect one another. I saw little chance of working on that subject under a devoted rat runner. Seeking broader exposure to psychology and physiology, I transferred to the University of Texas at Austin in 1960. I eventually earned a B.A., then completed a Ph.D. in 1966.

During those years, while the original papers on CST languished in obscurity, I learned a psychology that was a mixture of Hull-Spence behaviorism, the ecological psychology of J. J. and Eleanor Gibson, information theory and its applications in psychology by people like Garner and Hake, classical psychophysics, and the early work on signal detection theory. The "information processing perspective" was in its birth throes.

In physiology, I came across work on control and regulation, but I read much of that material on my own. The physiologists and physiological psychologists with whom I studied presented some material on physiological "feedback," but then the term referred to pathways that descended from higher centers, "feeding back" down the nervous system toward the sensory receptors—feedback was activity feeding back down the lineal causal pathway for behavior, serving some unknown function. I thought there was more to the idea than that but could not work it out. (Incidentally, when they diagrammed a control process, nearly all physiologists used a version of Norbert Wiener's diagrams, in which the system controls its output.)

I learned my psychophysics under Chuck Watson, a young faculty member just graduated from Indiana University, a point of origin of signal detection theory, and under Lloyd Jeffress, one of the authorities on binaural hearing. In Watson's graduate seminar on instrumentation and electronics, I located material on servomechanisms and negative-feedback control systems. There was no hint that such a device might serve as a model for a living system, but I thought it might. I did nothing more with that vague idea at the time. Jeffress was one of the grand figures in my educational experience. I completed my dissertation under him on an obscure topic in auditory signal discrimination. During all that time, while I pursued trivia, Bill Powers, and Mary, worked alone on *Behavior: The Control of Perception.*

I finished my degree during a "seller's market," when new Ph.D.s had many opportunities awaiting them. Like all of Jeffress' students before me, I was expected to go to a decent university where I would become an audition-psychophysics person, teaching, researching, and publishing on those topics for the remainder of my career. I disappointed everyone by going to a university that offered only the B.A. and M.A. in psychology. It was a place where there would be no pressure to publish or perish, which meant I would be free (as free as 12- to 15-hour teaching loads each semester would allow) to pursue my own line of study. After my education in psychology and physiology, I was convinced I had been conned—there was nothing in it that could explain the behavior of real organisms in the real world (apply the PCT interpretation of "real").

By 1973, my students were enduring the results of my personal attempts to fit feedback into the ecological psychology of the Gibsons, and to tie it all together in a mix with information theory and signal detection theory. (All of those poor students. I should have been tarred and feathered!) I was convinced that the environmental and ecological levels and layers described by the Gibsons must represent classes or levels of perception in a person, not objective features of the environment—basic psychophysics and physiology left no other possibility. But I was getting nowhere with working it all out.

I read Bill's 1973 article in *Science*. I can still remember the electric feeling as I devoured it, saying over and over to myself, "Of course! Of course!" He made it all so transparent and simple. When I received the flyer from Aldine announcing "the book," I ordered it immediately. The notes scribbled all over my old copy document my excitement with the book, and my conviction that it was a work of genius, and my determination to do something, no matter how minor it might be, or when it might be, to help "put out the word" about CST. As things turned out, it was quite a while before I did anything other than make CST part of my teaching in an obscure university.

From the first day when I talked about CST in my classes, some students were interested in the theory and excited by its implications for behavioral science. (Of course, many others—and my faculty colleagues—were not at all excited.) David Goldstein joined our faculty to teach courses in developmental psychology and cognitive psychology. He encountered students who kept talking about CST and about how it was related to topics in his classes. In self-defense, he came and asked me what it was all about. During the time from 1973 to 1992, David was the only one of my former colleagues who ever tried to learn what the commotion was really about; most of the others were content to ignore me and take cheap shots at my students. Eventually, David was in trouble running up large phone bills by calling Bill Powers, and he helped bring Bill to our campus for a visit. By then, I had convinced myself that, despite my determination to help, I was equipped to do very little.

In an attempt to learn how to do computer modeling, in the late 1970s I attended an NSF-sponsored short-course on modeling with NDTRAN, a systems dynamics modeling program patterned after the more elaborate and expensive DYNAMO. A few thesis students dared to tackle some modeling problems in CST using NDTRAN. In 1980 and 1981, we went on the road to talk about those projects, first at a meeting of the Society for General Systems Research (SGSR), then at a meeting of the IEEE Society for Man, Cybernetics, and Society. In both places, everyone seemed to think CST was an old-hat version of cybernetics— not their cup of tea. At SGSR, they were interested in entropy, chaos, nonlinear systems, and lots of elaborate verbalisms; at IEEE, they cared about optimization and models of optimal control. CSGnet is not the first place where we have encountered resistance from devotees of those ideas.

Frustrated, in 1982 I went alone to Columbus, Ohio, to a meeting of the American Society for Cybernetics (ASC). I hoped to locate at least a few people who might be interested in CST. When I wandered into the general meeting hall the night before the meeting was to start, I was surprised to see Bill Powers, messing around in a tangle of cables, setting up his homemade computer. At ASC, nobody else was interested in CST. I was dismayed to see that they could so easily reject Bill's ideas without even a hint of a fair hearing or an attempt to understand. Bill was ready to give up on the cybernetics people, whom we both thought ought to be the group most likely to understand CST. I was afraid he might give up altogether—the years of rejection and, for the most part, "going it alone" had taken a toll. In retrospect, I know he would never have quit.

1983-1994

I hit on an idea that I thought might help. It had nothing to do with modeling, and it is probably my one significant contribution to the present state of CST (now known as perceptual control theory, PCT). I knew that Bill and Mary kept a map on their kitchen wall next to the refrigerator, with a pin marking the location of each person who had called or written to ask about or discuss CST. I asked Bill to send me the short list of names that accompanied the pins. I contacted all of them and invited them to attend the 1983 meeting of ASC, in California. Then I contacted Bill Reckmeyer, president of ASC, and told him we needed three sessions on the program. To my amazement, Reckmeyer gave us the sessions. In 1983, a band of seven CST people used smoke and mirrors to create the impression that we were everywhere at the ASC meeting. One evening, at dinner in a Chinese restaurant, we gave Bill a "certificate" from the "off-the-wall group" of control system theorists, commemorating the 10th anniversary of the *Science* article and *Behavior: The Control of Perception.* The next year, Bill Benzon organized the CST contingent at the meeting of ASC in Philadelphia. There, ASC people were "into" poetry, "second-order cybernetics," and autopoeisis—they gagged on CST.

In Philadelphia, we could not all fit in a single elevator. We knew we were really making progress! I flew back to Texas with a good feeling about what was happening. The next year, the CSG held its first meeting, at Kenosha, Wisconsin. I'm sure other "pioneers" have written about the CSG meetings.

We were not finished with ASC. That group had convinced the organizers of the annual Gordon Research Conferences that the ASC brand of cybernetics was scientific and deserved to be the subject of two Gordon Conferences. The first was in Wolfboro, New Hampshire, in 1986. Bill, Mary, and I were the only CST people there; second-order cybernetics, autopoesis, aesthetics, and deconstructionism ruled. At the second conference, in California, there was a formal CST session.

After the California gathering, the Gordon people dropped the ASC from the Gordon Conferences (not, I hasten to add, because of the CST session —the problem was too much emphasis on aesthetics and too little on science).

One evening during the first Gordon Conference, Bill, Mary, and I escaped onto the boat dock and were talking about a paper he had started. We agreed to collaborate on it. It eventually became "Models and Their Worlds," which was rejected several times by legitimate" journals, then published in *Closed Loop* in 1993, seven years after we began our collaboration.

Along the way, Bill gave me copies of some of his programs, on which I hacked around and half-way learned how to do programming and "real" modeling. It seemed obvious that my poor programming might benefit from time spent with Bill, and I planned to spend a week or so visiting him in Northbrook. Greg Williams and Bill Williams learned of my plan and arranged to be in Northbrook at the same time. That was the start of a three-year series of Northbrook "mini-conferences" on CST. During each of them, Dick Robertson and Wayne Hershberger dropped in for awhile. I was always desperate for travel money to attend the mini-conferences. One summer, I submitted a proposal for faculty development money to support a trip to "the laboratory of William T. Powers." In the proposal, I said I would hold down expenses by "sleeping at the laboratory." I didn't tell them the laboratory was in the room behind the kitchen in Northbrook. To keep things "honest," I gave Bill a plaque that proclaims 'The Laboratory of William T. Powers," a place I believe is one of history's great centers of intellectual accomplishment, as I said in the Foreword to Bill's *Living Control Systems II.*

My Students

While I was teaching from 1967-1992, I directed 55 master's theses. After 1973, 14 students dared to complete theses that involved PCT. Some used it as a "perspective" for reinterpreting other work in psychology. Others used it as a source of tasks or behavioral measures for research projects. A few of the hardiest used formal PCT modeling in their theses. No matter the degree to which they used PCT, they all encountered far more flack and nonsense than the typical graduate student in our department—my former colleagues never did appreciate PCT. I had, and always will have, great respect for all of those graduate students, and for the many undergraduates who also faced what was often outright scorn from their peers, who all knew (perhaps with a little help from *my* peers?) that PCT was folly and those who followed it were fools.

Back when I began teaching psychology, I was "safe." Many of my earlier graduate students completed doctoral programs and have become clinicians, research scientists, faculty members and administrators. But in the PCT era, I watched one student after another go off to doctoral programs, wishing they could continue to study PCT—saying they would manage to keep their interest in PCT—then caving in under the pressure to do what they *must* do to survive. During the final few years of my teaching career, I could not accept the error I experienced over seeing students become excited about PCT, then asking me the inevitable question, "Where can I go for a Ph.D.?" At the end, I was watching undergraduates and graduate students simply drop out of psychology altogether, rather than study in a traditional department. Many of those who wanted careers in clinical practice opted for certification in areas other than psychology. I no longer thought it was fair for me to expose students to material that could only end their hopes for professional careers in behavioral science. I left teaching for what looks as though it will be a full-time effort to obtain funding for research on topics other that PCT.

Beyond 1994

Now, 34 years after the first papers on CST-21 years after *Behavior: The Control of Perception* and Bill's article in *Science a* handful of people do PCT modeling. In one way or another, most of us either abandoned, or never pursued, a traditional career path, out of our conviction that PCT is a revolutionary theory. There seems to be no other way to go about this business. Unless others soon pick up the task of PCT modeling, there is very little future for PCT as a science, no matter how many dedicated people use PCT in their applied work. The modelers never had doctoral students who went on to perform modeling. We never had doctoral students, period!

Without more contributors to the modeling, we run the risk of PCT becoming one more among a multitude of "perspectives" or "frameworks," another gloss added to the theories a person held before encountering PCT. There is abundant evidence of that phenomenon on the CSG computer network. There, I see one person after another stake a claim that this, that, or the other theory "says the same things as PCT"; or that PCT is fine, as far as it goes, but that such and such theory is necessary for going further; or that blah theory is more fundamental than PCT and can generate PCT. Time and again, 1 am struck by the fact that those invocations of other theories sound familiar; they are often the same things I heard in the 1960s—the same theories I recognized as part of the immense con job that passed for my education and training in scientific psychology.

In addition to more modelers, we need more people to gather solid empirical evidence to demonstrate and document the phenomenon of control at every level, including social, neurological, biochemical, and applied. We must demonstrate the phenomenon of control, *then* invoke the model, not the other way around. As things stand, even some supporters of PCT show very little interest in empirical work on control—a few even dismiss that work as trivial and say it "adds nothing to our understanding of PCT." With friends like that....

Finally, anyone who suggests that another theory, or an improvement or addition to PCT, is better than any present working version of the PCT model must produce the evidence—a working model that does the job better. Given evidence like that, there is no question about which model works best. That's the only way this game can be played.

My Life as a Control Theorist

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My life as a control theorist began in the spring of 1974 when I was roaming through the library at the University of California at Santa Barbara. I had just completed the requirements for my Ph.D. and was preparing to start what would turn out to be a very pleasant career as a professor of psychology at a small college in Minneapolis called Augsburg College. My major emphasis in graduate school had been the study of human perception; my thesis research addressed a rather arcane question in auditory psychophysics: what is the time and frequency resolution of the auditory system for detecting tonal stimuli? I got the teaching position at Augsburg based on my familiarity with computers rather than auditory psychophysics (the latter not being in great demand at the time). Before leaving beautiful Santa Barbara, I would occasionally roam through the library to see what was new in the psychology section—particularly in the "perceptual psychology" section. It was during one of these tours that I ran across a book by William T. Powers called *Behavior: The Control of Perception (BCP).*

I was attracted to the book by its title. Being a student of perception, I found it downright puzzling. I had thought about perception quite a bit in graduate school, but I would never have thought of it as something to be controlled (whatever that meant). I checked out the book and found that it had something to do with control theory, feedback, and cybernetics. I was immediately impressed by the clarity of the author's presentation; he seemed to know what he was talking about. I was intrigued, and a little frightened; intrigued because what Powers was saying seemed to be relevant to "real" human behavior in a way that all my graduate studies in psychology had not been; frightened because, in a way I could not yet articulate, Powers seemed to be calling into question some things that I took for granted. I probably spent about two hours with *BCP,* but it had made a bigger impression than I then knew.

My encounter with *BCP* was just a vague memory when I ran across it again in 1977—this time in the library at Augsburg College. But now fate would intervene to allow *BCP* to change my life. Various chance factors made it possible for me to learn what we now refer to as perceptual control theory (PCT), the theory of behavior described in *BCP.* I was, at this time, preparing a rebuttal to a talk given by a colleague (and good friend) about the meaning and implications of Skinnerian behaviorism. B. F. Skinner was receiving a great deal of attention after publication of *Beyond Freedom and Dignity.* Discussions of "determinism" vs. "free will" seemed to be all the rage, and the possibilities of "behavioral engineering" were being actively debated. Like most "cognitive psychologists," I thought that there was something wrong with behaviorism—I just couldn't articulate the problem to my satisfaction. I was planning to base the rebuttal to my friend's pro-Skinner talk on Ulric Neisser's discussion of Skinnerianism in his recently published *Cognition and Reality.* Neisser was one of the "biggies" in the then relatively new field of cognitive psychology, and his arguments against behaviorism made sense. But I could tell that these arguments were more like opinions than scientifically based conclusions.

BCP reappeared at this time. I saw its relevance to my planned rebuttal right away, since the discussion of PCT was framed as a critique of behaviorism. *BCP* seemed to provide a scientific (rather than an emotional) alternative to behaviorism that was missing from the cognitive view. After weeks of going back and forth—whether I should base my rebuttal on the conventional "cognitive" view or on the PCT view of behavior (which I still only vaguely understood)—I opted for PCT. This turned out to be a good choice because it started me on the path to learning PCT; it was a bad choice because I had only the vaguest idea what I was talking about—which made for a pretty ineffective rebuttal.

But I quickly lost interest in debating Skinnerians because another fateful development helped me move forward in my understanding of PCT. In early 1978, some friends of mine at Stanford introduced me to the "personal computer." It was love at first sight. I immediately bought an RCA Cosmac computer kit (with 8K of memory, mass storage on a cassette tape, and "hex pad" data input). By 1978, I had convinced Augsburg to buy a couple of what were then considered to be very fancy Apple II computers. At the same time, I (again by chance) ran across an article by Powers in *Psychological Review* ("Quantitative Analysis of Purposive Systems"). A number of tracking experiments were described in that article, and I was able to replicate them fairly easily on the Apple II (using the game paddle as cursor controllers and Apple Basic as the programming language). Shortly after that, I found Powers' series in *BYTE* magazine, which reassured me that I had been doing the experiments correctly and taught me how to do some of the modeling needed to evaluate the results of the experiments.

My experiments with the personal computer helped me understand PCT in a way that would have been impossible (for me) with words

alone. These experiments made it possible for me to understand what "the control of perception" meant; it meant the end of psychology as we knew it. At this time, I was also writing a textbook on experimental psychology and statistics. I was leading a double life: my PCT experiments on the personal computer showed that the basic assumption on which all psychological research is based—the assumption that perceptual input is the cause of behavioral output—is wrong; and I was writing a textbook explaining how to do psychological research based on this assumption. By 1979, I knew that what I was saying in the textbook was wrong. But I finished the book (it was published by Brooks/Cole in 1981 as *Methods in Experimental Psychology)* in order to get tenure (I did) and to show the "right" way to explain the wrong way to study behavior (I believe I succeeded, though the book was not a bestseller). Once the book was finished, my attachment to conventional psychology was finished as well.

What finally "put me over the edge" and convinced me that PCT is a revolutionary new approach to understanding behavior was the apparently trivial (but completely astounding) realization that, in a control loop, the input to the loop is *not* the cause of the output. This can be demonstrated most easily in a compensatory tracking task where you are to keep a cursor aligned with a target. When control is good you are able to keep the cursor almost exactly on target, despite the fact that there are disturbances that would tend to move the cursor away from the target. You keep the cursor on target by moving a control handle appropriately; to the left to keep the cursor from moving off to the right, and to the right to keep it from moving off to the left. Most people looking at a subject performing this task would say that the deviation of the cursor from the target "tells" the subject which way to move the handle in order to keep the cursor on target; deviation of cursor from target is the "stimulus information" that is used by the subject to make the appropriate responses. But Powers showed that there is almost no relationship (correlation) between deviations of cursor from target and movements of the handle that controls the cursor. Yet there is a nearly perfect relationship between the unseen disturbances to the cursor and handle movements. These results seem "magical"—completely contrary to the "input-output" or "cause-effect" model of behavior—yet they are exactly what is predicted by PCT.

I started trying to do experiments to see if I could find a flaw in the PCT view of the tracking situation. After all, manual tracking studies were well known to me and had been done for years; how could anyone have missed this incredibly surprising fact—that inputs don't cause outputs, that what subjects see doesn't determine what they do. It was during this period that I hit on the idea of having the subject do two runs (at different times) with exactly the same disturbance present both times. This was easy to do with the computer. The idea was this: even though the correlation between cursor and handle movements is low, it might be that something about the cursor is still the cause of handle movements; it's just something that is not picked up by the correlation. For example, handle movements might be caused by some function of the cursor movements, or by the cursor movements from some time in the past, or by some odd weighting of several cursor positions, etc. Any of these aspects of cursor movements might be the cause of the handle movements, and if so, they would not show up in a simple correlation between cursor and handle movements. However, they would show up in a correlation between cursor movements on two different trials where essentially the same handle movements had occurred; the cursor movements on these trials would correlate because something about them must be the same if the handle movements were the same. I knew that I could get the subject to make nearly the same handle movements by presenting the same disturbance on two different occasions; the cause-effect model would predict that the cursor movements should also be nearly the same on these two occasions. In fact, they were not the same at all.

I designed several other tests of the input-output model of tracking, and the results were always exactly those predicted by PCT: no effect of perception on behavior; behavior is the control of perception. The basic assumption of experimental psychology—indeed, the basic assumption of all social science is wrong. This was heady stuff. But the excitement was tempered considerably by my growing realization that work on PCT was going to be very lonely indeed. As I began to present the results of my research to other psychologists (in publications, at meetings and seminars), it became increasingly clear that, while psychologists love to talk about scientific revolutions and to call every new theory in psychology "revolutionary," they don't want a real revolution—and you don't get much more revolutionary than PCT. My presentations on PCT were met with polite interest and, sometimes, nodding agreement, but it was clear that no one really wanted to stop what they were doing and start psychology all over again, from scratch.

It was also becoming clear that there were not many psychologists besides myself who were doing research based on PCT. In fact, the only PCT research publications of any quality that I knew of were by Powers himself. So I wrote to Powers in 1979 and went to visit him in 1980 (he was living in Northbrook, Illinois, at the time, relatively close to Minneapolis). Bill turned out to be as brilliant in person as on paper—and a truly wonderful human being too; kind, helpful, humble—surprising qualities in a person who is just about always right about everything. Through Bill, I learned that there were some other scientific psychologists actively interested in PCT. Eventually, we developed a bit of a network of PCT afficionados. With the invaluable assistance of Bill's wife Mary (a very accomplished PCT afficionado herself), this disorganized group of scholars, who shared little more than an interest in PCT, finally got together in one place—a retreat near Kenosha, Wisconsin—for the first meeting of the Control Systems Group.

In 1985, I left teaching and returned with my family to California. I left teaching only because I could not, in good conscience, continue to hypocritically teach a curriculum that had to be taught if students were to learn "psychology." I could have stayed at Augsburg as long as I liked—and I was encouraged to stay—teaching one or two "special" courses a year on control theory. But I felt that this was not fair to the students or to control theory. When I did teach such courses, students wondered why I was teaching a course that challenged everything they were being taught in the other psychology classes; it seemed as if I were engaged in a personal feud with my colleagues. I also found that teaching PCT in the context of the conventional psychology curriculum gave the impression that PCT is a new explanation for the "facts" being learned in the other classes; it took me several years to realize that this is actually not the case—that PCT is a totally new approach to understanding behavior, a new start for psychology. Existing psychological "facts" are not facts at all, from the PCT perspective: they are usually based on statistical data, so they are not true "all of the time" (often not even a good proportion of the time), and they are not true of any individual person, but only of a non-existent "average person." I realized that in order to do PCT properly, one has to stop doing conventional psychology and start doing PCT—period. In 1985, I stopped doing conventional psychology.

Since leaving teaching, I have made my living as a "human factors" engineer by day while continuing my PCT research at night and on weekends—time permitting. I have managed to publish several papers on PCT since leaving teaching, but my interest in publishing in the conventional psychology journals has almost completely evaporated. Not only is it nearly impossible to get past the review process with a PCT paper, there is virtually no response to these papers when they are published. I am no longer surprised or saddened by this response to PCT; it is quite understandable in PCT terms; psychologists can be expected to control for doing psychology in ways that achieve their higher-order goals—which seem to include publications, recognition by peers, tenured faculty positions, and best-selling textbooks. PCT is obviously not a way of doing psychology that will help a psychologist achieve these goals; in fact, PCT is a disturbance to the kind of psychology that does allow psychologists to achieve these goals. Efforts to "convert" psychologists to PCT are no more likely to be successful than efforts to convert believers to atheists (or vice versa). Nevertheless, it is still fun to discuss and argue about PCT concepts, and the most exciting forum for doing this now is on the Internet.

In 1990, a computer network dedicated to discussion of PCT was formed; there are now approximately 120 people in at least five countries participating in this network, known as CSGnet (the Control Systems Group network). This network might not increase the number of "converts" to PCT, but it *will* provide a forum for sharing ideas and results that come out of PCT research and modeling.

I see two important paths for the future development of PCT. One is the scientific path: much more research needs to be done on the basic PCT model. If I were the head of the Living Systems Research Institute, with many graduate students to help with the research, I would have no shortage of projects to suggest. I think it's important to study the control of higher-order variables—sequences, categories, programs, and even principles. I have begun some simple studies of the ability to control sequences and programs. These studies should be perfected and extended, and a start should be made at modeling the perceptual functions involved in the control of these complex perceptual variables. I would also like to perfect methods for monitoring the value of the reference for a controlled variable. It is important to be able to distinguish variation in a controlled variable that results from poor control from variation that is intended. I also think it is important to study intra- and inter-personal conflict in some detail. Conflict is the basic human problem, from a PCT perspective; we have to understand its essentials in order to know how to deal with it.

The second path is therapeutic. PCT implies a specific approach to therapy based on the idea of getting consciousness "above" the level of the internal conflict—to the level of the systems that are setting the incompatible goals. It should be possible to teach and apply this approach to therapy clearly and consistently. We need to develop therapists who can reliably apply the "method of levels" and who can teach it to others. This means that PCT therapists will have to understand the science of PCT at least as well as the scientists understand the therapy. In fact, PCT should break down the barriers between scientific and clinical approaches to psychology. Any person who is able to do PCT therapy should also be able to do at least some basic PCT science, and any person who is able to do PO' science should be able to do some basic PCT therapy. The difference between PCT science and therapy should only be a difference in emphasis, not a difference in scientific integrity or human compassion.

Of course, there are many more directions in which PCT can expand, but I see them all turning around these two poles—the scientific and the therapeutic. Much more needs to be done with modeling complex,

multidimensional control processes; for example, a model hand might be a nice sequel to the Little Man's pointing arm. This kind of modeling will probably be of most interest to those traveling down the scientific path—but those on the therapeutic path would do well to try to understand why such models work. There are also great possibilities for PCT in the realm of social relations; PCT principles should allow people to develop social organizations that allow people to maintain individual control—to the collective benefit of all individuals inside and outside of the organization. This is a "therapeutic" application of PCT that would surely benefit from the scientific modeling of group behavior using PCT.

Perceptual Control Theory: Looking Back, Looking Forward

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Looking Back

I finally became a pioneer in something! I guess this is what happens if you live long enough. Let me put on my coonskin hat and remember.

When I was in graduate school at the University of Connecticut in 1973 or 1974, Michael Turvey alluded to Bill Powers in one of his graduate perception courses, but he couldn't really explain how a control system works which made any sense to us (or him). Later on, in a paper with Carol Fowler, he revealed his lack of understanding in a clear way. I sent Bill a copy of the Turvey and Fowler paper. Bill wrote a very long letter trying to explain where the authors went wrong. They never answered. From the exchange between them, I came to appreciate the difference between substance and style.

Tom Bourbon got me into this! His students kept asking me how Jean Piaget related to Bill Powers, whom I didn't remember hearing about in graduate school (but actually did). I read *Behavior: The Control of Perception* out of self-defense and became addicted. At the time when this happened, I was an assistant professor at the Stephen F. Austin State University in Nacogdoches, Texas, and I taught graduate and undergraduate courses. I also had a part-time private practice of psychology.

At that time, I didn't understand Pa well enough to apply it to doing psychotherapy. But I did see how it related to biofeedback therapy. The paper which I wrote in Wayne Hershberger's book summarizes some of these ideas.

I invited Bill Powers to talk at SFA, and he accepted! The reaction of the other faculty members was really interesting and pretty typical of the reactions we have come to expect. Bill's description of Nacogdoches as "the backwater of the world" still sticks in my mind. (How should we describe Durango?) Somewhere in the official records of the SFA newspaper is an article describing Bill's visit. Unfortunately, the video tape made of this event was lost.

Corresponding with Bill by mail on different topics has kept up my interest in PCT. It takes a long time to understand this PCT stuff! We used to say “two years.” Perhaps with all the new computer demos, books, tapes, etc., it does not take as long. I was amazed by how patient Bill was when explaining PCT ideas. I was impressed by the fact that he seemed to have thought about most of the questions/issues which occurred to me in my field and had come to some conclusions about them. His willingness to think about fields of knowledge far from his own reminds me of the idea of a Renaissance man. These qualities have become obvious to all those participating in CSGnet.

The first meeting of the control theory group which I recall attending was in Philadelphia in 1981. This was actually part of an American Society for Cybernetics meeting. I got to meet Rick Marken, and we had fun trying to get a computer program to do a pursuit-tracking task on my Commodore-64. Rick’s special talents with modeling and his colorful personality were apparent even then. I presented a pursuit-tracking study at that meeting; the research subjects were special education students. The data were analyzed with a transfer function approach devised by Bill. The parameter estimation/modeling approach which we use today did not exist then.

I think I attended all of the annual CSG meetings in Wisconsin except for the last one. The following experiences stand out in my memory:

(1) During one of the meetings, the group name “Control Systems Group” came into use and stuck.

(2) Dick Robertson and I collaborated on applying the PCT approach to looking at the self-image. We explored the idea that the self-image is a systems-level controlled perception. We were not able to get the two papers we wrote on the subject published. One was a research-oriented approach, while the other one was a theoretical integration of the self-concept area. (Dick, should we try *Closed Loop?)*

(3) Ed Ford introduced us to the idea of Quality Time for improving relationships and took us on some very pleasant long walks. I learned about Reality Therapy from him, as well as from Diane Gossen and Perry Good. For a while, it looked as though Bill Powers and Bill Glasser would make a dynamic duo. But....

(4) Dick Robertson, Clark McPhail, Chuck Taylor, and I had some fun times playing tennis during the afternoon breaks.

(5) I started using the Q-Methodology approach as a statistical way to identify a person’s self-image. In fact, we described Bill Powers using this approach and obtained the different ways in which subgroups of people at one meeting viewed him. Bill thought it was an interesting “projective technique.” From Bill’s reactions to this, given his lack of fondness for statistics, I have come to the conclusion that the intensive study of the individual case is the best chance we have of finding “facts.” The question Bill always asks: For what percentage of people

will this statement be true? For what percentage of time will this be true of a person?

For this essay, I analyzed the results of the Q-Methodology study. Bill Power’s self-image and five different subgroups can be compared as follows:

Bill’s self-image	*Group 1 (n = 9)*	*Group 2 (n = 6)*	*Group 3 (n = 7)*	*Group 4 (n = 5)*	*Group 5 (n = 5)*
obstinate (+3)	yes	no	no	no	no
good-natured (+2) yes	yes	yes	yes	yes	
assertive (+2)	yes	yes	yes	yes	yes
quarrelsome (-2)	yes	yes	yes	yes	yes
resigned (-2)	yes	yes	yes	yes	yes
submissive (-3)	yes	no	no	yes	no

From the PCT perspective, the group which knows Bill best is the one which matches his self-image most closely. This translates to 1, then 4, then 2, 3, and 5.

Here, the descriptor “obstinate” was defined by the sentence: “It is difficult to get me to do something I don’t want to do.” The descriptor “submissive” was defined by the sentence: “I do what other people want me to do.”

The results lead one to think about the relationship between a person’s theory and the person’s self-image. There is obvious agreement among the groups in how they perceive Bill, but it seems clear that group 1 knows Bill the best, that is, comes the closest to his self-image.

(6) I started to apply the Q-Methodology approach to therapy cases. One such case was written up and published in *Operant Subjectivity,* the journal of the Q-Methodology people. A more general look at Q-Methodology from a PCT viewpoint was also published in this journal in a second paper.

The publication of the PCT textbook edited by Dick Robertson and Bill Powers provoked me to want to go back to teaching introductory psychology. I taught one course at Glassboro State College as an adjunct. I was allowed to use the Pa book as long as it was supplemented with a “standard” one. The reactions of the students were interesting. They found PCT understandable but challenging. All exams were take-home essays, and I have kept the answers for future reference.

The creation of CSGnet was the brainchild of Gary Cziko. I first heard about it at the CSG meeting which took place in Pennsylvania. The net has done wonders by allowing people scattered all over the country and world with an interest in PCT to talk and learn from each other. It captures the feeling of the CSG meetings in terms of the intellectual stimulation and willingness to listen to people from other fields of interest.

Unfortunately, a combination of practical factors have made it impossible for me to attend the meetings taking place in Durango. I have greatly missed them. Thank God for CSGnet and the telephone!

At one point I had become discouraged that the PCT approach was so hard to apply to therapy cases. I presented a clinical case on CSGnet, after which Bill offered to teach me about the method of levels by applying it to me. This took place over a number of months. It resulted in a more sophisticated methodology for studying self-image than I previously had used and persuaded me that PCT had some unique contributions to make to therapy.

The "method of levels" plays an important role in PCT psychotherapy. It is a way of raising a person's awareness so that he/she can become aware of background experiences (perceptions) to the one which started out the conversation. As a result of attending to background experiences, a person's awareness rises to higher levels of perception.

The method of levels is the process that Bill Powers went through which resulted in the different levels in PCT. The method of levels is a "bottom-to-top" procedure. The therapist starts where the patient's awareness is ("bottom") and, by looking for background experiences, helps to move the patient's awareness higher ("top"). There is no assumption that the specific levels mentioned in Bill Power's books are the ones which will be found for a particular case.

Those who want to see what PCT psychotherapy looks like when the specific levels are used as the basis of therapy should read Ed Ford's *Freedom From Stress.* Ed follows a "top-to-bottom" strategy. He has people identify the important system-level experiences in their life. Then, for each system-level concept, he has them identify the important principle-level perceptions which are the means of achieving it. Then, for each principle-level perception, he has them identify the important program-level perceptions to achieve a given principle-level perception.

For the past several months, Bill Powers has been doing the method of levels with me. We have communicated using e-mail. Here are some of the things I learned as a result of doing the method of levels with Bill. I was the "patient" and Bill was the "therapist." We went through two rounds of the method. One start-off topic was my reaction to the method of levels as I understood it. The second start-off topic was my reaction to the experience of playing tennis. Here's what I learned:

(1) Each statement which the patient makes has potential background experiences which the therapist can ask the patient to address.

For some reason, I used to think that the background experiences would only show up after rather large segments of conversation. Each statement goes into and through the therapist, who is looking for background experiences along with the patient.

When Bill and I were doing the method of levels, the notational convention emerged to put the background material in brackets. (Like this.] Bill started doing this. Then, when I was writing my e-mail post to him, I would put background experiences which I noticed in brackets as I became more sensitized to what a background experience was like.

(2) The background experience feels more like an observation than an inference. I got the best results if I could observe the feeling or thought which was in the background. It did not feel as if I drew a conclusion or made an inference. It felt as if I made an observation. This helped to give me confidence that the background experience was something which was just as real as the topic which started the conversation. Prior to doing this method-of-levels exercise, I had my doubts about the reality of the background experience.

(3) The therapist is much more active in identifying the background experience than I understood to be the case from Bill's general description of the process before we did the exercise. I don't think that, in most cases, the patient will be doing this on his/her own, at least in the beginning. As the process goes on, the patient does become better at identifying background stuff. The therapist identifies a background experience for the patient and asks if the patient wants to address it, or prefers a different topic, or prefers to continue on the same topic.

The length of the therapist's answer makes a big difference. If it is too short, the patient feels alone in the enterprise. If it is too long, the patient is focusing too much on the therapist.

(4) The identification of a background experience feels a lot different than receiving an interpretation. Bill and I wound up calling this more traditional approach to therapy "psychologizing." The result of psychologizing was that I felt annoyed to have to address stuff which seemed to come out of the blue from Bill. When we were following the method of levels, I felt as though I was addressing my stuff. Psychologizing reliably resulted in blocking the flow of the conversation and progress. I am sure that giving interpretations has useful roles in therapy, but I am more aware of the negative side-effects it can have than I was before the exercise.

(5) The method of levels is not as abstract or difficult to do as I had thought. In fact, I observed that really good ordinary conversations sometimes follow the method of levels. One person says something. The other person tunes into the background stuff and addresses it. I no longer believe it is restricted to highly intelligent, verbal adults who are intellectually oriented. In fact, since the therapist meets the patient

wherever the patient's awareness is located, it probably is applicable to any age group for which other verbal therapy approaches would be attempted. The therapist has to be willing and able to adjust to the state of the patient.

(6) There are no fireworks emotionally or intellectually when the level of awareness is raised. The changes feel much more subtle. The patient is not transformed into someone who the patient never was. It is true that the patient might become aware of stuff of which he/she was formerly unaware. As I went up levels, the feeling aspects of the experience seemed to diminish in intensity. At the lower levels, the feelings were stronger and more salient parts of the experience.

(7) I did become aware of an internal conflict. Becoming aware of the conflict did not result in the immediate resolution of the conflict. I did give myself a daily assignment which I carry out to help me resolve the conflict. I did not previously identify this internal conflict. I can see how it has resulted in some significant inconsistencies in the way I am/behave. If this were a real therapy session, the therapist would probably have to spend time helping the patient figure out ways to resolve the conflict, once it was identified.

(8) The end result of the exercise was to start to examine my self-image. When we got to this point, the method of levels was more difficult to apply. It was here that I decided to continue the exploration on my own and that the joint exercise has stopped. I am now applying the self-image exercise procedure, based on PCT ideas, which I presented at the last CSG meeting I attended.

Recently, I have applied a more sophisticated version of the Q-Methodology studies, taking into account some of the criticisms expressed by Bill and other CSG people. Instead of using items consisting of single words drawn from a standardized set, I use sentences unique to the person being studied. And I resurrected the how/why technique, which I had presented at one of the CSG annual meetings, to have the subjects take each sentence and generate meaningfully related sentences. All of the sentences created became the universe from which a smaller set of sentences, about 20 to 50, would be chosen for the sort.

A second innovation is the way I have been selecting "conditions of instruction"—the sorting instructions given to the subject whose self-image I am studying. The conditions of instruction are chosen so as to sample as widely as possible from the different emotionally packed episodes which have been discussed in therapy. For example, I might instruct a patient, "Describe the way you are at the time of your divorce." Or, "Describe the way you are when you are riding your bicycle."

The interesting thing about the in-depth self-image studies I have conducted so far is that multiple self-images have emerged. It might

be "normal" to be multiple. However, at a level "above" the multiple self-images in most people (even people with multiple personalities), there is a single "observer" self with many of the characteristics of the reorganization system. I hope that one of these therapy case studies will see the light of day in a journal.

For the past three years, I have been the Clinical Director in an adolescent residential treatment center in New Jersey. For the first time, I have been able to apply PCT ideas on a wider basis than in private practice or the classroom. From this experience, I am beginning to learn how to "soft-sell" PCT to clinicians who have different viewpoints and to others. I have introduced 'Post-Critical Incident Counseling," which is PCT-based, brief (15 to 30 minutes), and fills the gap between our behavior-modification-based point/status system and the traditional therapies.

Looking Forward

Now I'll exchange my coonskin hat for my herbal tea leaves (no caffeine, please), with which I shall forecast the future with unerring accuracy.

Closed Loop will become a "real" journal. This seems to be happening already. The participants on CSGnet do not seem to be at a loss for words. I see more and more PCT research being done. People of the PCT persuasion will become more and more involved in following their own hunches. They will become less self-conscious and defensive and feel less of a need to persuade others of the merits of PCT. The research will speak for itself and attract others. People from all walks of study will want to publish in the *Journal of Living Control Systems.* PCT will become the equivalent of the universal language in Hesse's Glass Bead Game *(Magister Ludi).*

The PCT approach will be applied at the biochemical level. Advances in genetics research will combine with PCT ideas. A perceptual signal is "a copy of" a reference signal, just as DNA can create copies of itself. Do we have control systems operating in the genome? Bill Powers is already working with one person in this area. It is very exciting!

In the tea leaves, I see a set of neuropyschological tests based on PCT ideas. The levels of the control system hierarchy are calling out for someone to make them into a set of tests and, at the same time, test some of Bill's ideas about levels and relationships among levels.

A PCT research institute will be established.

PCT tasks will be utilized in research studies even by non-PCTers. They will be impressed by the ability to predict performance in tracking tasks. This will become a tool which they will use, and they will relate the performance tasks to all kinds of things which PCTers wouldn't.

PCT methodology will become more accepted and refined. The intensive study of the individual case will become the way to go. Unlike behavior-modification people, PCT people will study controlled perceptions. The methodology of researchers and clinicians will be merged into one new scientific approach when studying living control systems.

I'll write a book on the PCT approach in clinical psychology. Preliminary title: *Everything I Know about Psychology 1 Learned in Kindergarten or after Graduate School.* (There is something in me which wants to see merit in what non-PCTers have done and are doing. This results in my being less pure than some other PCTers. Oh, well!)

New people will take over part of the functions which Bill Powers has been doing all by himself for all of these years. Fortunately for us, Bill has longevity in his family. However, with all of the "young Turks" coming on board, he will gladly let go of some of his functions. His wiseman function will, however, be retained.

The PCT approach will become widely known: I see PCT as being *the* approach of the future. We have a common language in terms of which people from a diversity of fields can talk to each other.

Bill Powers will live to 100+, will give a keynote invited address at an American Psychological Association annual conference, and will be recognized as the one of the greats in psychology. Finally, Bill will receive the recognition he deserves from the old guard. The history of psychology will become divided into pre- and post-PCT—BC and AC, for short.

And we all will live happily ever after. The people of the world, starting with parents, will stop trying to use brute force to control other people. We will all become very sophisticated at peaceful ways of conflict resolution.

Perceptual Control Theory at 40

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As this issue of *Closed Loop* is the first one carrying the subtitle *Journal of Living Control Systems,* readers encountering our approach to this subject for the first time might need an overview of perceptual control theory (PCT) to get started. So this paper will be Yet Another Introduction to PCT. I will slant it, however, toward those coming into to this subject from the physical sciences; the relationship of PCT to physical approaches has been discussed at some length lately on CSGnet.

Rather than just reviewing the history or the principles of PCT, I'll try to develop an argument that leads from conventional views of behavior to the new view that PCT gives us, emphasizing in the end the odd role that organisms, seen through the eyes of PCT, play in a world otherwise dominated by physical laws. The point will be to show that control theory provides us with the germ of a radically new understanding, a break with all traditional theories of behavior—and many new ones as well. The future progress of PCT depends on understanding just how different a view of behavior we get by understanding the logic of control, the logic of a controlling organism's relationship to its environment.

The Etiology of Perceptual Control Theory

All living systems are sensitive to their environments; all act on their environments. This is ancient knowledge. The puzzle presented to the behavioral scientist is only how that sensitivity becomes converted into action. What are the rules, if any?

The most obvious and straightforward scientific approach to this question was realized long ago. In the physical sciences, if you want to know the properties of an assemblage of matter, you apply experimental forces and other influences to the object and observe what it does as a consequence.

In the worlds of physics and chemistry, this is a relatively easy task. Objects tend to be simple and have few properties; they are normally homogeneous or made of simple repeating units. It is not hard to make sure that experimental effects on them are the only effects of any importance. All similar objects made of the same materials behave in essentially the same way, and they will continue to do so no matter how many times an experiment is repeated—in fact, measurements of properties can be almost indefinitely refined by repeating them. A physical or chemical experiment can be clearly described and can be replicated by anyone who wishes to check the results. The reasoning about the meaning of an experiment can be communicated in clear and formal language, and even the reasoning process itself can be made public by being expressed in mathematical terms that anyone can learn. The history of a material object is entirely expressed in its present condition; the path by which it got into that condition is irrelevant, and only the current environment is of any importance in determining what will happen in the future to that piece of matter.

These confidence-building thoughts about the physical-science approach were, of course, tried out on organisms. The results were anything but confidence-building. A behavioral scientist reading the preceding paragraph might well experience mounting despair and envy of the physicist. While it is true that organisms are made of matter and must therefore obey all of the laws of physics and chemistry, it is not true that they are homogeneous or made of simple repeating units. They are, in fact, immensely more complex internally than the objects studied by physicists and chemists. They are too sensitive to their environments for any scientist to be sure of having control of everything important that happens to them. Not only are they sensitive, but they adjust themselves internally to external circumstances. It is not possible to perform the same experiment over and over on an organism to refine measurements of its properties—just imagine giving the same physics test over and over to refine a determination of a student's state of knowledge of physics, or giving a weight-lifting test to an athlete, day after day, to refine measurements of the athlete's strength.

The initial attempts to apply the methods of physical science to organisms were moderately successful at answering questions about perception. When the same methods were extended to the study of behavior, the results were not so encouraging—in comparison with expectations, they could only be called failures. Organisms were so subject to unpredictable influences, it seemed, that extraordinary precautions had to be taken to eliminate unwanted and unpredicted behaviors. This seemed at first to be a technical problem, to be overcome by greater attention to controlling the environment during experiments. As the years went by, however, it became apparent that no amount of attention to detail was enough. Not even the simplest phenomenon, such as a blink of an eye in response to a puff of air, could be made to occur with complete reliability. More complex behaviors simply went

all over the map. The dream of creating "Newton's laws of behavior" was apparently unattainable.

This did not cause a loss of faith in the methods of physics. Most behavioral scientists continued to assume that behavior was created by environmental influences. This assumption led to an attempt to find suggestions of regularity in behavior through statistical means, and then to a conclusion that this was the only possible means of exploring behavior, because behavior is inherently variable. The basic concept was retained: what organisms do is caused by what is done to them by the surrounding environment. But the requirements for formal language, public means of reasoning, ability of anyone to reproduce results, and refinement of measurements by continued experimentation were mostly impracticable. Despite the failure of the physical-science approach, the assumption was that the failure of organisms to behave as predictably as planets was due to technical difficulties, not errors in basic principles. The alternative conclusion, that something was wrong with applying the physical principle of cause and effect to the behavior of organisms, was simply not considered.

This alternative eventually came into play by a roundabout path.

Control theory was invented by engineers of the 1930s trying to build devices that would behave like human beings carrying out a specific kind of task: a control task. Even though the engineers did not realize it (many still do not realize it), the concept of control introduces a new principle, one that denies the basic idea that organisms do what the environment makes them do. While cause and effect still work in control theory as anywhere else, the organization of a control system creates apparent cause-effect dependencies that are different from the actual ones. Part of understanding control processes in organisms is the understanding that conventional cause-effect interpretations can be more misleading than informative.

Organisms are sensitive to their environments, and they act on their environments. The old assumption was that the sensing was the primary process, with the acting following from it. But that is an arbitrary assumption. It is just as plausible to assume that the acting is the primary process, and that the sensing, at least in certain critical regards, follows from the acting. It is even more plausible to say that sensing and acting are processes that go on simultaneously, in continuing streams that can't be clearly separated into cause and effect. This is basically what the inventors of control theory discovered: a type of system in which behavior affects the inputs on which behavior appears to depend. This is the type of system they had to use to imitate the human behavior called controlling.

This discovery led eventually to cybernetics, which endorsed this concept of closed causation without exploring more than its general philosophical implications. Years had to pass before more detailed implications came to light. Still more years had to pass before the basic concepts of control theory could be boiled down to a systematic model of control behavior—now called PCT—that could replace the old systematic cause-effect model based on the approach of physics.

The basic difference between the physical approach and that of control theory is that the physical approach deals with properties of energy and matter, while control theory deals with the properties of *particular organizations* of energy and matter. George Herbert Mead pointed out early in this century that physics doesn't deal with forms, with the entities into which we divide the world of experience. The physicist explores what is the *same* between a horse-cart and an ox-cart. The systems approach is concerned with what is unique to each vehicle, with differences in behavior brought about not by the differing physical or chemical composition of different objects, but by the differing organization of forms made of the same materials differently arranged.

It stands to reason, therefore, that physical laws will have a different significance when seen in the context of an organized system. We can admit that they make the behavior of the system possible, without also admitting that they *explain* the behavior of the system. Physics and chemistry can explain how it is that a neural signal liberates energy that causes a muscle fiber to contract, and how it is that this contraction leads to accelerations, velocities, and positions of limb segments connected to a joint spanned by the muscle. But they can't explain how it is that this signal arises under just these circumstances to reach that particular muscle. Physics and chemistry can't even be applied until the organization is specified. It is at the level of organizational understanding that control theory confronts older conceptions of the organization of behavior in living systems.

The Phenomenon of Control

I am going to avoid semantic arguments about what "control" really is. I will use the term in a particular sense; if others interpret it in a different sense, they will have difficulty following this exposition. I use it in this sense: A system is said to control a variable if it acts on that variable, in the presence of other unpredictable influences of comparable size on the same variable, so as to maintain the variable in an arbitrary state. The "arbitrary state" might mean a state of constancy, or any arbitrary pattern of change. The critical aspect of this definition is that physical influences that normally account entirely for the state of the variable are no longer effective, while the action of the control system causes the variable to behave independently of those other physical influences. When that is true, the variable is called a controlled variable.

The first important fact about control to notice is that the controlled variable is being acted upon by many forces, only one of which is attributable to the control system. The driver of a car can apply a lateral force to the front end of a car by turning the steering wheel. But there are many other influences that create forces acting laterally on the car at the same time: crosswinds, bumps in the road, tilts in the roadbed, unevenly inflated tires, and asymmetries in the aerodynamics of the car's shape, to mention a few.

If we observe, as we commonly do, that the path of the car does not follow strictly from the sum of all of the external forces acting on the car's mass, we can only conclude that it is the driver's contribution that makes the difference. If we see the car moving in a straight line, we can only conclude that the sum of all forces, including the one that the driver can alter, is zero.

So, if any of the external influences is seen to vary, but the path of the car does not vary as Newton's laws and engineering principles would predict, we have to deduce that the driver must be producing a varying force that just cancels the sum of the external forces. Indeed, we can observe the driver continually making adjustments of the steering wheel angle, while the car continues in a straight, or very nearly straight, line.

Likewise, if we observe the car moving along a smooth curve, but we see that the sum of all extraneous forces would tend to make it move along some other path, we can deduce that the varying forces created by the driver add just enough more force in just the right way to produce the curved path. If we see the car moving along a straight expressway, then turning off to take an exit ramp, then making other turns until it ends up parked in a parking lot, we can be quite sure that normal external forces would not have made the car follow just that path (an easily tested assumption). We can be sure that the varying forces created by the driver's motor actions on the steering wheel must have been exactly those necessary to add to the natural forces to create this overall result.

To anyone accustomed to normal physical or engineering analyses of the motions of objects, there must be a jarring note in this account. What is generally done is to observe all of the independent contributing forces and the initial conditions, and then to deduce through physical laws what the resulting motion must be. The driver's steering forces and the external forces due to winds, road tilts, and so forth simply occur as they occur, and the car's path is the outcome.

But here we are speaking as if one of the determining forces, the varying force being generated by the driver, is being adjusted so as to create a *preselected* outcome. Instead of the outcome varying randomly as the unrelated applied forces make it vary, the outcome conforms to some predetermined pattern. One of the causal forces which adds to the other forces continually changes in just the way needed to maintain that pattern. We would appear to be saying, and we are in fact saying, that the outcome we observe is being produced *on purpose.*

The vast majority of behavioral scientists has always rejected this interpretation. When the concepts of PCT were first being developed, this resistance was massive and almost universal (it is considerably less today). To say that outcomes are produced intentionally has seemed to most scientists to call for a reversal of cause and effect, or for giving the future an effect on the present. Many have argued that if all of the causal influences are known, the outcome must be whatever it is, and to call it "intentional" adds no explanatory power. Clearly, the outcome is an effect of converging causes, not a cause of the converging causes. Even if behavior does seem to entail intended outcomes, a scientist must stick to normal cause and effect, and find some other explanation.

There have been centuries of attempts to find some other explanation. But prior to the advent of control theory, all other explanations, we now know, were spurious. Even now there are many who strongly resist admitting that outcomes are indeed intended, and that organisms are the loci of these intentions. This resistance is misplaced, because now we can explain exactly how it is that an outcome can be controlled.

How Control Works

Once again: All living systems are sensitive to their environments; all act on their environments. So far we have talked only about actions and other physical influences on the environment. To see how control works, we must now talk about how organisms sense their environments.

Sensing is a process by which an external variable comes to be represented as a neural (or chemical) signal inside an organism. This looks like normal physical causation, but it is not like most causal processes. There is *amplification* involved. Metabolic processes in an organism maintain the sensing nerve-endings in hair-trigger states of readiness to fire. Only a tiny added stimulus is needed to cause a neural impulse to be generated, and metabolic processes instantly restore the sensor to the brink of firing again. So a small continuing stimulus causes the sensory nerve ending to fire again and again, at a frequency that corresponds to the amount of stimulation. The signals that leave the nerve ending involve the expenditure of many times the energy that causes the sensory ending to fire, nearly all of the energy being supplied from stores within the organism itself.

These neural signals can be further amplified, and eventually they can be routed to effectors such as muscles that provide a final amplification up to levels that can have significant effects on physical

processes in the environment. The result is that organisms can create physical forces of large magnitudes which are produced without any significant reverse effect on the physical variables being sensed. This creates a novel relationship between the organism's output forces and other physical processes.

I remember inventing my first perpetual motion machine, at the age of perhaps 12. I had read that a certain kind of motor could be used either as a motor or as a generator. So I thought of putting fan blades on two of these motors and using one to blow air onto the other, the idea being that the generator would supply the current needed to run the motor while the motor supplied the wind that would run the generator. It took a few more years of education to realize that one has to think of physical processes quantitatively, not just qualitatively. It makes a difference *how much air* can be blown, and *how much* current can be generated, and *how fast* the driving fan can be spun by the available current. High school physics was enough to show me the embarrassing truth: that in physical systems, there are balances that are maintained: balances of forces, of momenta, and of energies. The world studied by physicists is rigorously constrained by these balances, these conservation laws. You can't get any more out of a physical system than goes into it. This is how I and most other people learned to think about physical processes.

This is also true of organisms, of course. No more energy comes out than goes in. But the energy that goes in is of a different form from the energy that comes out it is the chemical energy in food and air, obtained independently of the physical processes involved in behavior, and stored for future conversion into actions. So when an organism, a person, comes across some natural physical process in its environment, it is in a position to throw a monkey-wrench into the machinery by spending some of its store of energy.

Let's switch examples now. Suppose a person sees a fat child and a thin child sitting on opposite ends of a teeter-totter. The end with the fat child on it is, of course, on the ground, and the thin child is high in the air. The upward force of the ground on the fat child's side, plus the upward force from the thin child pressing down on the other end, just equal the fat child's weight. The physical system is in equilibrium.

Now the person places a hand on the thin child's end of the teeter-totter and pushes down, spending a bit of metabolic energy from the last few days' meals and several thousand breaths of air. The thin child descends and the fat child rises. If the amount of downward push follows a certain law, the teeter-totter will end up horizontal and stationary again.

What is the required law? If the force applied is large when the fat child is low and small when the fat child is high, with a continuous transition between the two states, there will be one state in the middle where the force is just right to bring the teeter-totter to the horizontal with all forces in equilibrium. But what could make the force applied by this helpful person follow that law?

Suppose we tried to mechanize this effect. When the fat child's end goes down, a cable pulls a weight at the center of the teeter-totter toward the thin child's end, and vice versa. The history of perpetual motion machines is full of such clever devices. All such devices, however intricate and devious their designs, fail because you can't get more out of a physical system than went into it.

But the person helping balance the teeter-totter is exerting a force of just the right amount without any linkage from the teeter-totter that produces that force. The only link from the teeter-totter to the person is through the person's visual sense, which registers the angle of the teeter-totter as feeble neural signals inside the person's brain. This requires only intercepting some of the light reflected from the physical apparatus and the children, a process that supplies only an infinitesimal amount of energy to the person and exerts no measurable force at all, either way.

The neural signals that now represent the angle of the teeter-totter are further amplified, and they finally enter muscles where the greatest (by far) amplification of all occurs, producing a force that acts downward on the teeter-totter. This force is greatest when the fat child is accelerating upward, smallest when accelerating downward. Stored energy is used by the person in applying the force to the moving teeter-totter. That's vital; none of this could work without the independent source of energy that comes from the eggs and roast beef and peanut butter sandwiches that the person has been eating.

What happens in the end is that the neural signals representing the angle of the teeter-totter come to some particular state representing the horizontal position, and the force applied to the teeter-totter is just the difference in weight of the two children. The physical system is now being maintained in a state far from equilibrium, but if you include the helpful person in the physical system, everything is in equilibrium again: forces, momenta, and energy inputs and outputs.

The factor that determines where this equilibrium will occur is now in the person, not the external physical system. There is some particular condition of the sensory signals that corresponds to the observed equilibrium. If the sensory signals indicate a deviation from this condition, the force will either increase or decrease in the direction that tends to restore the equilibrium. The rule is simple: if the angle slopes downward toward the fat child, increase the force; if upward, decrease it. This rule, which is applied inside the brain of the person, is what determines the equilibrium point.

There's one more factor to consider. The person balancing the teeter-totter might decide to maintain the board at some angle other than horizontal. This amounts to redefining the condition of equilibrium. In a control system model, this is done by providing an adjustable reference signal against which the signal representing angle can be compared. This occurs inside the person's brain. The final amplification of signals that drives the muscles is applied to the *difference* between the reference and sensory signals, so the opposition to even small deviations from equilibrium can be very strong.

With the addition of the variable reference signal, the person can now cause the teeter-totter to behave in any arbitrary way at all, as long as the available muscle forces are large enough and the person doesn't exhaust the stores of metabolic energy. As the reference signal varies, the teeter-totter's angle varies in exact correspondence. It can be made to vary regularly or irregularly, quickly or slowly, with or without a child sitting on either end—or not at all, even though the children climb on and off the board. The angle of the teeter-totter is now completely determined by a reference signal inside the person's brain, and the normal physics of the teeter-totter is totally overridden. The person is inserting extra force, extra momentum, and extra energy—whatever is required to make the desired behavior appear.

This same analysis could have been applied to the driver of the car. The lateral position of the car is represented in the driver's brain as some sort of neural signal. Another neural signal, a reference signal, specifies the lateral position that is to be maintained, and amplification of the difference between the two signals produces muscle forces that act on the car to make its lateral position, as sensed, match the specified position. Varying the reference signal will then cause the lateral position of the car to change in a parallel way, independently of other forces acting on the car. The normal physics of car motion is overridden; external forces lose their determining effects.

Organisms in Control

In the world of physics, there are physical objects linked to each other by properties of the environment and physical laws that cause the behavior of one object to depend on the behavior of other objects. Even in the most complex of physical systems, there is a kind of natural bookkeeping that accounts for all of the interactions. The sum of all forces acting on and inside the system, counting both actions and reactions, is zero. The sum of all changes in energy content, including energy inputs from outside and energy outputs to the outside, is zero. All momenta add up to zero, or at least a constant.

If we want to make one variable in a physical system depend on another one, the normal approach is to establish a physical link. This link connects forces from one object to another object, which involves transfers of energy and momentum and sometimes flows of matter. The new link participates in the balances of the system; it can generate no new energy, and it can create no unbalanced forces. The affected object is in physical equilibrium with the affecting object. If A is pushing on B through the new linkage, then B is pushing back on A with exactly the same force.

An organism is, of course, a physical system subject to all of the same laws and balances. But the organism can create linkages among objects in its environment which, at first glance, seem to violate physical principles.

First, the organism can move about in its environment and dispose itself to create forces on many different objects in many different ways. This means it is in a position to affect objects that are not normally affected by such actions.

Second, the organism can orient its sensors to create internal signals representing many aspects of physical objects around it. The visual sense is particularly potent in this regard: simply by looking in different directions, the organism can create internal signals that stand for the states of objects in many different ways: their position, velocity, size, color, relation to other objects, shape, and so forth. It can do this without affecting those objects in any measurable way.

As a result, an organism can position its muscles and limbs, and its sensory apparatus, in ways that create arbitrary linkages between the objects it can sense and the objects to which it can apply forces. Furthermore, because of the high amplification that takes place inside the organism, this linkage can be made one-way—that is, one object can be made to affect another object without being affected by the reverse path through the same link. There is a violation of the normal energy balance in the physical system, because any normal physical link would require energies, forces, and so on to remain in balance.

The unbalances are made up by the organism from its internal energy stores, and from the way it braces itself against the world as it exerts forces. If we consider the physical environment and the organism as a single system, there is, of course, no violation of any physical principles. The point, however, is that the physical environment linked to an organism can no longer be treated as if no organism were present.

Consider the car and driver again. With no driver in the car, but with the car rolling along the road, physical influences on the car can be calculated according to normal physical principles. From the speed and direction of the wind and the aerodynamic properties of the car, the wind force acting on the car can be calculated. Similarly, forces arising from tilts and bumps and soft tires can be calculated. All of these forces

can be added up, and their effects on the car can be computed. From these forces and the properties of the car and road, the motion of the car can be computed with, in principle, as much exactness as we please.

But now put a driver in the car. Suddenly, the path of the car ceases to follow from the sum of all external forces and the properties of the car and the road. Instead, we find that a new physical linkage has been created. Now when the wind blows and the road tilts, the result is a movement of the steering wheel which *prevents* the car from obeying the physical laws that previously applied.

Even more important, we find that the physical linkage that has been created is *not* between the steering wheel and the wind or the tilt of the road, but between the steering wheel and the lateral position of the car. What the driver is sensing is the *outcome* of all of the applied forces (which now include the effects of turning the steering wheel). The driver watches the visual appearance of the hood of the car against the road ahead and acts to maintain that visual appearance in a specified state (either constant or changing in a specified way). The only thing that gives the car's lateral position an effect on the path of the car is the fact that the driver is sensing that lateral position, internally specifying an intended state for that perception, and producing steering forces based on the difference between what is actually sensed and what is intended to be sensed.

From outside the driver, this critical perceptual linkage is invisible, undetectable in terms of any changes in the physical world. Nothing in the world changes measurably because of being sensed. Nothing in the physical outside world indicates the driver's internal reference signal that specifies the intended state of the perception. As far as any external measurements are concerned, the force that turns the steering wheel has no observable external physical cause. It is an arbitrary force generated for no physically observable reason.

The strangest thing about this force is that after it is added to all of the other independent forces that are applied to the car at the same time, the result is an outcome that is repeatable with great accuracy for long periods of time, even if the external forces change and even if there are changes in the properties of the car and the road. When all of the external forces change, the outcome does not change; instead, the remaining force applied to the car changes in just the way that keeps the outcome the same. The cause changes in order that the effect be preserved.

An organism can attend to any perceivable aspect of the environment. If the forces that the organism can generate are comparable to the external forces that exist at the same time, that aspect of the environment can be made to conform to the organism's intention for it, and to cease behaving as the natural forces on it would otherwise dictate.

The actions of the controlling organism supersede the physical laws that normally govern that part of the environment, in the respect that the organism is controlling.

Conclusions

Organisms are physical systems, and they exist in a physical world. But the laws of physics do not explain their behavior or its effects on the physical world. Organisms force the world around them into highly improbable forms, states of motion, and organization, and they act in a way that keeps normal physical forces from having their normal effects. It is organization, not physics, that explains how they do this.

To understand human behavior in these new terms is to seek a kind of explanation completely different from what behavioral scientists, modeling their approach after physics, have sought. This is what PCT is about, and where its promise for the future lies.

The Control Systems Group is a membership organization which supports the understanding of cybernetic control systems in organisms and their environments: *living control systems.* Academicians, clinicians, and other professionals in several disciplines, including biology, psychology, social work, economics, education, engineering, and philosophy, are members of the Group. Annual meetings have been held since 1985. The CSG Business Office is located at 73 Ridge Pl., CR 510, Durango, CO 81301; phone (303) 247-7986.

The CSG logo shows the generic structure of cybernetic control systems. A Comparator (C) computes the difference between a reference signal (represented by the arrow coming from above) and the output signal from Sensory (S) computation. The resulting difference signal is the input to the Gain generator (G). Disturbances (represented by the black box) alter the Gain generator output on the way to Sensory computation, where the negative-feedback km/ is closed.