2 Self-Organization of Behavior: The Basic Picture

Im Anfang war die Tat! ("In the beginning was the deed.")
—J. W. von Goethe

SOME HISTORICAL REMARKS ABOUT THE SCIENCE OF PSYCHOLOGY

Science always requires a language, and the science of psychology is no exception. But psychology is in a tricky position, scientifically speaking. The reason is that, according to Webster's dictionary, it must do double duty as the science of mind and of behavior. Even if we leave the brain out of psychology, which seems a bit counterproductive, the language of mind (percepts, images, thoughts, feelings, etc.) is very different from that of behavior.

It's a strange definition of psychology that contains the fundamental problem of psychology: what is the relation between mind and behavior? Even when we bring the brain back in, the best we seem to be able to do is *correlate* its physicochemical and physiological activities with different aspects of experience. Strangely enough, in the contemporary cognitive and brain sciences, any empirically adequate language of description for the two different domains of brain and cognition seems to suffice. Perhaps this is why correlations between the two are often less than compelling: after all, *something* must be going on in the nervous system when we perceive and act, think, learn, and make choices. That we should find some kind of correlation is hardly surprising.

From my point of view, not only does the presence of correlations fall far short of explanation, but also we seem to be correlating apples and oranges. Thus, with few exceptions, the science of psychology, broadly conceived here to include the cognitive and brain sciences, tacitly assumes that the physical and the mental are independent, irreconcilable categories. To abandon such an assumption must surely seem reckless. For me, however, the greatest drawback to understanding the mind-body problem is the very absence of a common vocabulary and theoretical framework within which to couch mental, brain, and behavioral events. Without commensurate description, how is it possible to see the interconnections? And, without a common conceptual language to reconcile the mental and the physical, how can psychology be called a science?

In this chapter I will show, through the use of a specific example and its detailed analysis, that the concepts and language introduced in chapter 1 are precisely what psychology needs. But first, as a context within which to embed these new ideas and results, let's briefly consider some of the shortcomings of other approaches.

Behaviorism

When we look at the history of psychology it's easy enough to speculate why the dictionary includes both mind and behavior in the definition: scientific psychology is a litany attesting to the continual tension between the two. B. F. Skinner, for example, appropriated the term *behaviorism* for a science of behavior, yet limited his analysis of behavior to the consequences produced on the environment.[1] An astonishing fact about behaviorism was that it did not actually deal with behavior or action, but only the results or *outcomes* of individual acts. The central concept of Skinner's behaviorism, the *operant,* captured nothing about how behavioral actions were organized spatially and temporally. Put another way, behaviorism acknowledged that pigeons can press a lever and rats can run a maze, but it didn't care a hoot about how the lever was pressed or the maze run. It treated the organism as a dimensionless point and ignored the form of behavior produced. Of course, a person may select a particular action based on the consequences of such action, but this tells us nothing about the coordination of action per se. Yet we know that it is this coordination that breaks down in various brain disorders such as Parkinson's disease and Huntington's chorea. And we know that it is important when people speak or walk or play the piano. One of the greatest drawbacks of modern robotic devices is that they lack this flexible coordinative ability.

Perhaps it is because all of us, from early childhood, are so used to coordinating our bodies that the science of mind and behavior (and certainly behaviorism) virtually ignored the problem of coordinated action. By analogy, all of us are familiar with falling, but it took us thousands of years to come up with the notion of *gravitation.*[2] People, as the Gestalt psychologist Wolfgang Köhler noted years ago, tend not to ask questions about phenomena with which they are thoroughly familiar. Such, it seems, has been the lot of coordination as far as most of psychology is concerned. All of us know, in a way, *what* coordination is, but little is known about *how* or *why* it is the way it is. One is reminded of the story about a tourist from the dogstar Sirius who described the most miraculous machine he had ever seen:

> A remarkable machine unlike any other I had seen before was rushing toward me.... It apparently did not have any wheels but nevertheless moved forward with an amazing speed. As I was able to see, its most important part was a pair of powerful elastic rods each one consisting of several segments.... Each

rod moved along a complex curved arch and suddenly made a soft contact with the ground. Then it looked as if lightning ran along the rod from the top to the bottom, the rod straightened and lifted off the ground with a powerful, resilient push and rushed upwards again.... As I was told, the machine consisted of more than two hundred engines of different size and power, each one playing its own particular role. The controlling center is on the top of the machine, where electrical devices are located that automatically adjust and harmonize the work of the hundreds of motors.[3]

Maybe we should be more like this tourist to see coordinated action as the miracle it is.

Ethology

The ultimate aim of the field of *ethology*, the naturalistic study of behavior, was to describe actions in terms of patterns of muscle activity.[4] Yet the overwhelming amount of detail combined with difficulties in recognizing and classifying relevant chunks of behavior has proved to be an enormous barrier to understanding. This is an oversimplification, of course, and there are notable exceptions, such as the seminal work of Erich von Holst in the 1930s that we will discuss later. Recently, ethologists such as Ilan Golani[5] advocated using a movement notation scheme devised by Eshkol and Wachman (E-W) to choreograph dance sequences.

Like a musical script, the E-W system provides a permanent record that allows for the reconstruction of behavioral actions. Without going into all the details, it treats the body as a set of limb segments, the movements of which are described relative to an imaginary sphere centered, say, at the carrying joint. This sphere, like your friendly globe of the world, is marked by coordinates analogous to lines of longitude and latitude. Thus the values of the two coordinates can be used to specify the position of the limb. A nice feature of the idea is that the coordinates can be defined with reference to a sphere centered on the joint of a particular limb, a partner involved in the movement or some fixed reference point in the environment, thus affording a description of movement in terms of an individual actor, one actor relative to another, or, indeed, one actors' body relative to some outside event, such as orchestral music (figure 2.1).

The E-W scheme has been used to describe everything from the social behavior of jackals and Tasmanian devils (small, ferocious, carnivorous marsupials that inhabit Van Diemen's land), to the ritualized behavior of geese and aggressive interactions between Australian magpies. One of the most impressive applications is by Golani and John Fentress,[6] who examined the ontogeny of facial grooming in mice. They were able to show how grooming develops from a small set of simple, stereotyped movements into the rich and precise repertoire of adult mice.

More recently, Golani in collaboration with David Wolgin and Philip Teitelbaum[7] used the scheme to analyze recovery of function after lesions to the rat's brain, as well as the behavioral effect of drugs. Their results suggested

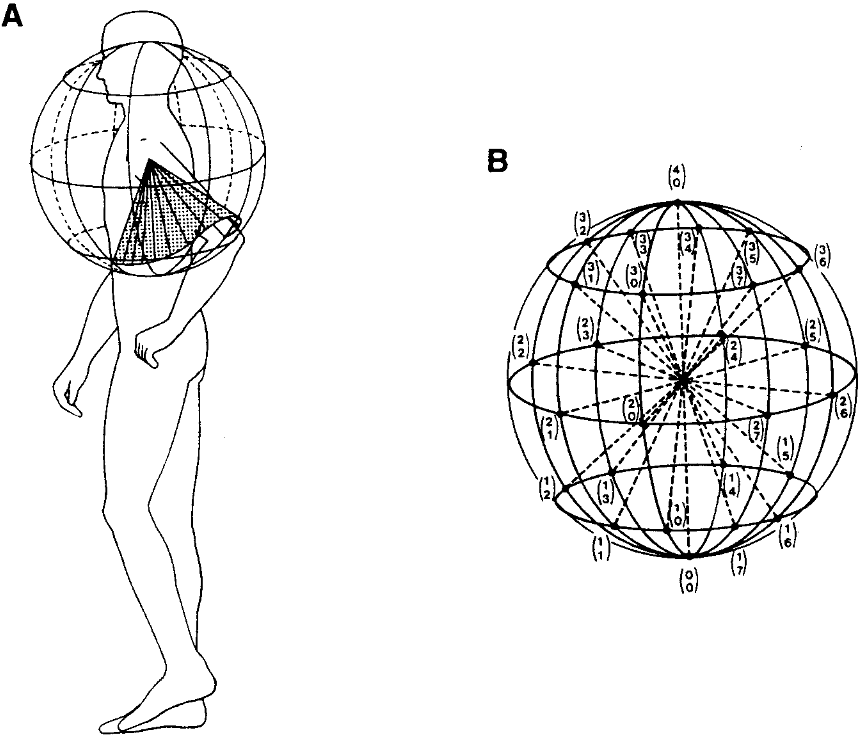

Figure 2.1 (A) The Eshkol-Wachman (E-W) scheme showing a sphere centered at the shoulder joint. The path of the elbow is traced on the surface of the sphere. (B) The sphere, like a geographer's globe, is marked by coordinate lines analogous to latitude and longitude. Positions on the surface of the sphere are specified by two coordinates. The E-W notation allows movement of a limb to be described by its initial position, its final position, and the trajectory from one to the other.

similarities between the way an adult animal *recovers* from brain damage and the way a young intact animal *develops* certain exploratory behaviors, a version of the ontogeny recapitulates phylogeny theme.

From misguided or nonexistent descriptions of behavior in terms of outcomes or results of action, the E-W system substitutes a formal movement notation scheme for the description of behavior. Although it allows for objective and accurate description, it is not at all motivated by theoretical considerations or even the context within which action occurs. Any other ingenious measurement system could do the job just as well, if not better. Nevertheless, the E-W system seems the best tool that ethologists have at the moment to describe naturally occurring behavior.

My view is that accurate description is not enough for a science of behavior, whether of brains or people. Necessary perhaps, but not sufficient. I doubt very much that naturally occurring behaviors are the place to find laws of behavioral and neurological organization. Rather, most naturalistic behavior is simply too complicated to yield fundamental principles. The latter, after all, are hidden from us and it takes, I believe, either special strategies or pure serendipity (of the Archimedes in the bathtub kind) to reveal them. Relatedly, description and explanation are obviously not the same. Explanation demands theory and a coupling of theory to experiment. No matter how refined a formal description of behavior is (e.g., dance notation), there is no guarantee (indeed it seems highly unlikely) that a purely formal approach will provide any deep insights into the organization of behavior. In fairness, Skinner had a theory of behavior, but by ignoring behavior, he threw the baby out with the bathwater. The ethologists refined the measurement of behavior, but the returns, in terms of theoretical insights or understanding, have been modest to say the least.

Cognitive Psychology

So much for psychology as the science of *behavior*. What about psychology as the science of mind? Noam Chomsky, the MIT linguist and activist, is known to have disliked the term "behavioral science." For him it suggested a far from subtle shift of emphasis toward behavioral evidence itself and away from the abstract mental structures that such evidence might illuminate. Chomsky's concern for human language as a subject of study in its own right, his concept of linguistic competence, an internalized system of rules that determines both the phonetic structure of a sentence and its semantic content, bolstered the emergence of modern cognitive science.[8] Chomsky's abstraction away from conditions of language use ("performance") to the study of formal rules of structures, and his generally mentalistic, antibehavioristic stance, were aimed at shifting the science of psychology away from behavior and back to mind.

Of course, these days the characterization of mental life is dictated by a machine metaphor: the brain is viewed by many as a sophisticated computer whose software is the mind. Laymen and scientists alike are prone to describing almost any activity as involving "information processing." There are detractors, however. The philosopher John Searle recently argued that the brain, as an organ, does not process information by some imaginary computational rule-following any more than the gut does![9] Certainly one can *model* some of the functions of the brain on a computer as we do, say, with the weather, but that should not make us believe that the brain, any more than the weather, is a computer. Yet many, in my view, take the machine metaphor far too literally.

In one of my main fields of research, the control and coordination of movement, the computer metaphor has predominated for years.[10] It's easy to see why. Actions must be precisely ordered spatially and temporally. Order, it seems obvious, must be *imposed* somehow on the motor elements.[11] But how? The machine perspective says that order originates from a central program that elicits instructions to select the correct muscles and contract and relax them at the right time. "Just so," as Rudyard Kipling might say. Another artifact familiar to proponents of the machine perspective is the servomechanism. This is good when you want to regulate some property (e.g., limb position, temperature) using feedback. A template or reference level compares the feedback it receives with its own value, and based on this comparison, emits orders to an output device to eliminate any error.

Programs, reference levels, and set points feature heavily in explanations of intelligent behavior, but where do they come from? How, for example, does a given reference signal attain its constancy? If a reference signal at one level is simply the output of another servomechanism at a higher level, this leads to what philosophers term an "infinite regress," or, as Daniel Dennett would say, "a loan on intelligence" that somehow has to be repaid.[12] Any time we posit an entity such as a reference level or a program and endow it with content, we mortgage scientific understanding. The loan can be repaid only when these "phantom users" are vanquished. From the present point of view, it is best not to use artifactual constructs at all. Computers and servomechanisms are not natural systems but artifacts whose characteristics are not especially relevant to understanding living things. Supplanting artifactual machine views of mind and action with the language of dynamical systems and the concepts of self-organization may be easier said than done, but that is the journey we embark on here.

In short, Chomsky and others before and after him tore apart content and process. Chomsky's nearly entire emphasis on the competence part of his performance-competence distinction of linguistic behavior is now pursued to the extreme by program theorists who see the brain as the programmer and the body as a mere slave. The thesis here, however, is that psychology might be better off if it tried to explain the richness of behavior of living things in terms of self-organization, which does not require science to take out a loan on intelligence.

In self-organizing systems, contents and representations emerge from the systemic tendency of open, nonequilibrium systems to form patterns. As we noted in chapter 1, and as will become more and more apparent as we proceed, a lot of action—quite fancy, complicated behavior—can emerge from some relatively primitive arrangements given the presence of nonlinearities. That is, intelligent behavior may arise without intelligent agents—a priori programs and reference levels—that act intelligently.

We will need neither the formal measurement schemes of ethology nor the formal machine vocabulary of cognitive science. Instead, we will emphasize the necessary and sufficient conditions for the emergence of dynamic patterns in a complex system, like an animal with a nervous system immersed in a contextually rich environment.

Gestalt Psychology

Before leaving psychology (which we never really do, since this book is largely about a science of psychology), I should mention two approaches to which I am far more sympathetic than those discussed thus far. I will say more about them later when we consider perceiving as a self-organized process, but mention them here for the sake of closure, even though they are only loosely related to the central topic of this chapter. Both views, nevertheless, are intimately related to the idea of self-organization, but in ways that in my view are quite complementary. Both are antagonistic to the machine stance.

I refer first to the Gestalt theorist Wolfgang Köhler,[13] who viewed psychological processes as the dynamic outcome of external constraints provided by environmental stimulation and internal constraints of brain structure and function. No programmable machine metaphor for Köhler. Instead, macroscopically organized brain states were deemed the relevant stuff of mental life. The latter cannot be observed at the micro level of individual neurons, nor can they be derived by exclusive scrutiny of the microscopic elements. According to Gestalt theory, only at the molar level of description will correspondences be found between mental life and brain states.

Gestalt psychologists of the 1930s and 1940s insisted on the primacy of the language of physics, albeit extended appropriately to include organizational and dynamic aspects of mind. The perceptual process, for example, had to be understood as a result of autonomous creation of order in the perceptual system itself. Köhler's field-theoretical model of perception viewed the brain not as a complex network of many different interacting neurons working together, but as a homogeneous conductor akin to a container full of water. This view was, I'm afraid, hopelessly wrong. What was *not* wrong in my opinion was Köhler's emphasis on order formation, his adherence to the methodology of natural science, and his insistence that physical or physiological explanation be paired with the reality of phenomenal experience.

Scholars such as William Epstein and Gary Hatfield in the US, and Michael Stadler and Peter Kruse in Germany recently reappraised the Gestalt program.[14] Epstein and Hatfield quite correctly, I think, note that neither the technological nor the conceptual tools available to Köhler and his school were up to the task they set themselves. Brain imaging techniques were nonexistent, and the physics of open, nonequilibrium systems had not yet appeared on stage. The German scientists, although perhaps not entirely unbiased (forgiveably so), argued that Gestalt theory anticipated some of the concepts of complex, nonlinear systems presented in chapter 1. Fairness dictates, however, that we recognize that the latter were in no way inspired by Gestalt theory. Nevertheless, I am quite sure that the founders of Gestalt theory would be positively disposed to efforts to establish that brain and overt behavior follow natural laws of self-organization.

Ecological Psychology

Of course, it's not only the nervous system of people and animals that is potentially subject to laws of self-organization. Consider, as the perceptual psychologist James Gibson did, how we drive an automobile.[15] For Gibson

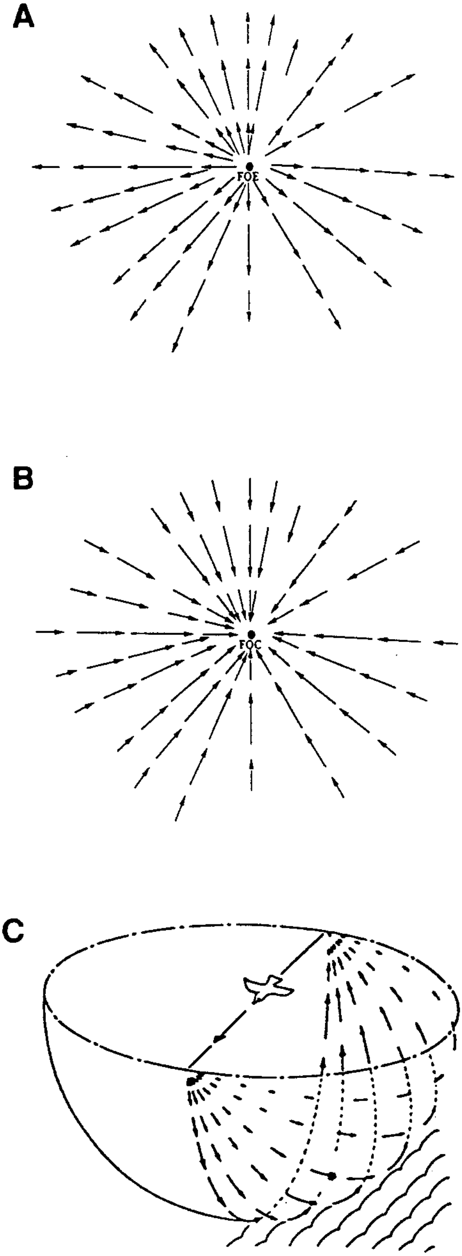

Figure 2.2 (A) Optic flow relative to the focus of expansion (FOE). (B) Optic flow relative to the focus of contraction (FOC). (C) Optic flow for a bird flying in a straight line. (From reference 15.)

and his followers, including Michael Turvey, Peter Kugler, and Robert Shaw, an essential construct is the *optical flowfield* that specifies properties about the car's motion in relation to the environment. This flow, like a fluid, spreads out as an object is approaching us or we are moving toward a surface (figure 2.2).

From the rate of divergence of optical flow it is possible to detect a simple parameter, tau (τ), that specifies time to contact.[16] How we slow down or speed up the car is determined by how we move in relation to the optic flowfield. Moving forward on a straight line produces radial expansion of the flowfield, moving backward radial contraction. Ask yourself how gannets (the large seabirds that plunge dive in such places as the Firth of Forth in bonny Scotland) or your regular housefly "know" when to close their wings or extend their legs as they approach a surface. In all these and many other cases such as long jumping, catching a fly ball, running over rough terrain, and driving a car, timing is controlled in a direct fashion by using τ.

Of course, this is a much longer and more detailed story than I want to pursue here (see chapter 7). Gibson's essential point is that information must be meaningful and specific to the control and coordination requirements of action. Rather than grounding perceptual theory on brain states or as computational rules that generate three-dimensional forms from two-dimensional images on the retina, the Gibsonian program asks how structured energy distributions are lawfully related to the environments and actions of animals. Note that the flowfield and the information it contains are *independent* of the particular visual system that occupies the moving point of information. That's why the gannet, the fly, Carl Lewis (the Olympic long jumper), and Juan Fangio (for many years the world's top race car driver) all use the same macroscopic optical quantity to guide their action.

Perhaps then, this teasing quote from Gibson's 1979 book (published after his death), *The Ecological Approach to Visual Perception,* is not entirely out of place, setting the stage for what is to come:

> The rules that govern behavior are not like laws enforced by an authority or decisions made by a commander; behavior is regular without being regulated. The question is how this can be. (p. 225)

What? No deus ex machina? No skeleton in the elevator? No élan vital? No entelechy? How can this be?

ARE ACTIONS SELF-ORGANIZED? IF SO, HOW?

Here's the basic two-pronged problem. The human body is a complex system in at least two senses. On the one hand, it contains roughly 10^2 joints, 10^3 muscles, 10^3 cell types, and 10^{14} neurons and neuronal connections. As Otto Rössler once said, finding a low dimension within the dynamics of such a high-dimensional system is almost a miracle.[17] On the other hand, the human body is multifunctional and behaviorally complex. When I speak and chew, for example, I use the same set of anatomical components, albeit in different ways, to accomplish two different functions. Sometimes, against the wishes of my sainted mother, I do both at the same time. Next time you watch a film, observe the rapidly flowing and shifting scene of sound and motion. Where does one event begin and another end? Where are the boundaries separating the flow of events? When the voice lowers, when the eyes look askance, when the face flushes, when the head turns aside, when the hands fidget, what does all this have to do with what is being said?[18] Referring back to the first chapter, the joint challenges of *compositional complexity* and *pattern complexity* seem to confront us again with a vengeance.

The great Russian physiologist Nikolai Bernstein (1896–1966) proposed an early solution to these problems.[19] He lived in the Soviet Union during the time that Pavlov's views were considered the only ideologically correct explanation of higher brain functions. But he was dead against the notion that the function of the brain could be understood in terms of combinations of conditioned reflexes. One of his chief insights was to define the problem of coordinated action as a problem of mastering the many redundant degrees of freedom in a movement; that is, of reducing the number of independent variables to be controlled. For Bernstein, the large number of *potential* degrees of freedom *precluded* the possibility that each is controlled individually at every point in time. How, he asked himself, does coordination arise in a system with so many degrees of freedom? How do we take a multivariable system and control it with just one or a few parameters?

The Synergy Concept

The resolution to this problem offered by the Bernstein school contained two related parts. The first was to propose that the individual variables are organized into larger groupings called linkages or synergies. During a movement, the internal degrees of freedom are not controlled directly but are constrained to relate among themselves in a relatively fixed and autonomous fashion. Imagine driving a car or a truck that had a separate steering mechanism for each wheel instead of a single steering mechanism for all the wheels. Tough, to say the least! Joining the components into a collective unit, however, allows the collective to be controlled as if it had fewer degrees of freedom than make up its parts, thus greatly simplifying control. Peter Greene, a computer scientist-mathematician who did much to promote the work of the Bernstein school in the US in the early 1970s, likened this idea to an army general saying, "Take hill eight," with the many subordinate layers of the military (subsystems) carrying out the executive command.[20]

For the Russian scientists, later led by the eminent mathematician Israel Gelfand, synergies constituted a dictionary of movements in which the efforts of the muscles were the letters of the language, and synergies combined these letters into words, the number of which was much less than the number of combinations of letters. For Gelfand and colleagues, the language of synergies was not just the external language of movements but also the internal language of the nervous system during the control of movement.[21]

The second, absolutely crucial aspect of the synergy concept is that it was hypothesized to be *function* or *task specific*. The notion of synergy is actually an old one, but earlier ideas associated synergies with *reflexes*. The reflex, in fact, was considered the basic building block of behavior. As one of its greatest advocates, Charles Sherrington, said in the early 1900s "simple reflexes are ever combined into greater unitary harmonies, actions which in their sequence one upon another constitute in their continuity what might be termed the 'behavior' of the individual as a whole."[22] Shades of Isaac Newton. For Bernstein, the reflex didn't contribute to the solution to the coordination problem. Instead, it was part of the problem. The reflex was just another piece that had to be somehow glued together with other pieces; fancy words like "great unitary harmonies" weren't much of a glue.

Testing the Synergy Hypothesis

Bernstein's hypothesis was not about hard-wired anatomical units; rather, synergies were proposed to be functional units, flexibly and temporarily assembled in a task-specific fashion. How might this hypothesis be tested? A stringent test would be to perturb the synergy by challenging only one of its members. If the organization is really a synergy, then all the other functionally related members should readjust immediately and spontaneously to preserve the functional goal.

A good deal of research has gone into identifying these functional synergies in such tasks as maintaining upright posture, walking, and grasping an object.[23] The experimental examples I like best involve coordinated movements of both arms and the production of speech. I like them because, in the first case, they are so simple you can do them yourself sitting in an armchair. In the case of speech, they are technically very difficult to do but are well worth the effort. Both experiments helped put the synergy hypothesis on solid ground.

Pick two targets, one for each hand, that are a different distance away. The task is to reach for them when given a "go" signal. It is well known that the movement time for a single limb depends on the distance the limb has to move and the precision requirements of the target. What happens when the two hands must move very different distances to targets whose precision requirements also differ? This question came up in a graduate seminar I taught in 1977. I well remember one of my students, Dan Southard, using two pieces of curtain material as targets and showing that the limbs reach both targets practically simultaneously. In other words, the brain coordinates both limbs as a *single* functional unit. This is revealed only when you do high-speed film analysis of the movements,[24] which in those days was extremely time consuming. I use the word "functional" because obviously the nervous system does not *have* to control both limbs as a single unit. Only under the task requirement to do two things at once does it create a functional synergy out of its myriad participating elements.

Speech, of course, is a complex system par excellence. The production of a single syllable requires the interaction among a large number of neuromuscular elements spatially distributed at respiratory, laryngeal, and oral levels, all of which operate on very different time scales. We breathe in and out roughly once every 4 seconds, the larynx vibrates at a fundamental frequency of about 100 times a second, and the fastest we can move our tongues voluntarily is about 10 repetitions a second. Yet somehow despite (or maybe because of) these complications the sound emerges as a distinctive and well-formed pattern. For a baby to say "ba" requires the precise coordination of approximately thirty-six muscles. The brain must have some way to compress all this information into something relevant.

Betty Tuller, Carol Fowler, Eric Bateson, and I considered speech to be a prime candidate for testing the synergy hypothesis.[25] But how to test it? First we had to construct a device to perturb an important speech articulator. We chose the jaw, in part because it moves up and down (and sometimes sideways) for all kinds of sounds, and earlier work by John Folkins and James Abbs showed that it was possible to perturb the jaw and obtain interesting results.[26] The very fact of "pipe-block" speech suggests that some kind of synergizing process is going on. Freeze the jaw's motion by putting a pipe in your mouth, and you still don't disrupt speech. (Pipe and cigar smokers do it all the time: a functional analogue to the stiff upper lip). However, one might argue that a lot of learning goes into producing speech with a pipe in your mouth. The strongest test of the synergy hypothesis would be to perturb a person's jaw suddenly during speech and see if other remote members of the putative synergy spontaneously compensated *the very first time* the perturbation was applied. Remember, the synergy concept refers to a functionally, not mechanically, linked assembly of parts. Any remote responses observed should be specifically related to the speech sound actually being produced.

We used infrared light sensors to transduce movements of the lips and jaw. Fine wire electrodes were inserted into speech muscles such as the genioglossus, a major tongue muscle, to monitor electromyographic (EMG) activity. With the help of Milt Lazanski, an orthodontist, I designed a special jaw prosthesis made of titanium for the two subjects. Gaps for missing teeth due to old rugby injuries enhanced the ability to set the prosthesis firmly into the subject's mouth.

The results were stunning. When we suddenly halted the jaw for a few milliseconds as it was raising toward the final [b] in [b æ b] (rhymes with lab), the upper and lower lips compensated immediately so as to produce the [b] but no compensation was observed in the tongue. Conversely, when we applied the same jaw perturbation during the final [z] in the utterance [b æ z], rapid and increased *tongue* muscle activity was observed exactly appropriate for achieving the tongue-palate configuration for a fricative sound, but no active lip compensation.

In short, the form of cooperation we observed in the speech ensemble was not rigid and sterotypic; rather, it was flexible, fast, and adapted precisely to accomplish the task. The many components of the articulatory apparatus always cooperated in such a way as to preserve the speaker's intent. The functional synergy, as it were, revealed.

FROM SYNERGIES TO SYNERGETICS

Synergies correspond to some kind of collective organization that is neurally based. They simplify *control*, or, as Bernstein would have it, they render control of a complex multivariable system possible. But how are synergies formed? What principles govern their assembly? Bernstein saw coordination as the *organization* of the control of the motor apparatus. Just as the theoretical concepts of self-organized pattern-formation in open systems were not available to the Gestaltists, so it was with the Bernsteinians. Nevertheless, both groups looked to the future possibilities of "antientropic processes" in open systems to account for autonomous order formation in perception and action. In his last book, *The Coordination and Regulation of Movements* (Oxford: Pergamon, 1969), Bernstein foresaw the end of the honeymoon between the sciences of cybernetics (servomechanisms and the like) and physiology, and Köhler intuited that the general systemic tendency toward equilibrium of inanimate matter (linear thermodynamics) was not really applicable to the organism.

Michael Turvey, long an advocate of Bernstein's approach to motor control and Gibson's ecological approach to visual perception, summed up the research conducted in the context of Bernstein's formulation of the degrees of freedom problem as the *first major round* of theorizing and experimentation on coordination.[27]. The second major round revolves around some of the questions that I have raised above. In a sense, round 2 really started with a pair of papers published in 1980 by Peter Kugler, Turvey, and me. More concretely, it began with a rather vague and unsubstantiated claim, namely, that a functional synergy is a *dissipative structure* "that expresses a (marginally) stable steady state maintained by a flux of energy, that is, by metabolic processes that degrade more free energy than the drift toward equilibrium."[28]

A somewhat controversial Nobel Prize (aren't they all) had just been awarded in 1977 to Ilya Prigogine for his theory that, as a system is driven away from thermodynamic equilibrium, it may become unstable and then evolve new, coherent, dissipative structures. As I mentioned earlier, Hermann Haken introduced the term synergetics in the late 60s to describe an entire interdisciplinary field dealing with cooperative phenomena far from equilibrium. (Synergetics, by the way, is not a cult, but rather Haken's theory of how pattern-formation phenomena that arise in different contexts and disciplines are related, e.g., in the laser, chemical reactions, and fluid dynamics.)

If I may digress just a bit, the approaches of Haken and Prigogine (and for that matter, Rene Thom's catastrophe theory) are actually very different, even though they've often been bundled together in popular treatments. Prigogine's original theory was heavily weighted toward equations of a thermodynamic character that describe the behavior of ensemble averaged macroscopic quantities. Haken's work, from its very beginning, always included an essential role for, and explicit treatment of, microscopically generated fluctuations. This is also where it deviates from Thom's completely deterministic theory.[29] Fluctuations, as we will see, turn out to be quite crucial, both conceptually and methodologically, to our understanding of self-organization in living things.

Kugler and Turvey[30] stress the relationship between the stability and reproducibility of oscillatory movements and dissipative structures. One of their major goals is "to explain the *characteristic quantities* (emphasis theirs) of a rhythmic behavior—for example, its *period, amplitude* and *energy* per cycle (emphasis mine) [which] cannot be rationalized by neural considerations alone" (p. 4). In a series of experiments in which subjects oscillated a pair of hand-held pendulums whose length and mass could be independently varied, they and their colleagues discovered a number of fascinating relationships between these "characteristic quantities." For example, the pendulum period, over variations in the masses and lengths of pendulums, was proportional to mass to the 0.06 power and to length to the 0.47 power. They were able to match these empirical results with a model in which the characteristic frequency was that of the free, undamped motion of the pendulum with a spring attached a short distance away from the pendulum's axis of rotation. The spring represents muscles and tendons that elastically store and release mechanical energy. In their words, the wrist-pendulum system is a "macroscopic mechanical abstraction ... in which ... the only forces at work in the abstraction are the gravitational force (F_g) and an elastic force (F_k)" (p. 178).

A rather amazing feature of this macroscopic mechanical abstraction is that it helps explain certain features of quadruped locomotion, specifically the limb frequencies of animals (large and small) moving about the Serengeti plains. When plotted against limb length or mass, the stepping frequency, from Thompson's gazelle to the black rhinoceros, falls on three straight lines, one for each locomotory mode. Kugler and Turvey's pendulum-with-spring model, which represents the joint effects of gravity's tendency to return the limb to its equilibrium position and the spring's stiffness or restoring torque, fits the data remarkably well. In fact, for each animal cruising across the Serengeti they found that the ratio of spring torque to gravity's restoring torque was unity for walking, six in trotting, and nine in cantering. As Turvey elegantly points out, there is universality to the design of locomotion, a particular exploitation of nature's laws.[31]

But which laws are we talking about? Everything that has been empirically established thus far in hand-held pendulum studies appears to be consistent with Newtonian physics. The limbs of an animal (or person) behave like an inverted pendulum coupled to a spring. Colin Pennycuick, who collected the Serengeti locomotion data, uses them to support the following claim:

> [A]t the intermediate scales of biology, Newtonian physics still works as well as it ever did. The reason is that Isaac Newton was himself a medium-sized animal, and naturally discovered laws that work best over the range of scales which he could perceive directly.... Biology occupies that range of scale in which Newtonian mechanics can account for physical processes to a level of precision appreciably higher than that to which most biologists are accustomed. In biology, if not in physics, Newton still rules.[32]

From the point of view of self-organization in complex systems in which dynamic instabilities play a central role, nothing could be farther from the truth. In the context of *coordination* in living systems, appropriate observables are not usually provided by Newtonian mechanics but have to be discovered. The Kugler-Turvey research program stressed scaling laws among (averaged) physical quantities such as mass, length, and frequency of oscillatory movement, and produced some important results. But such quantities tell us nothing about how the limbs are coordinated (e.g., in a walk as opposed to a trot or gallop), or the principles of neuromotor organization through which such coordinative modes spontaneously arise, stabilize, and change. Entirely new quantities are necessary to capture the coordination of living things as a self-organized phenomenon. Dynamic instabilities, long at the core of pattern formation in open nonequilibrium systems, provide a way to find them. In summary, if coordinated action is based on functional synergies and if functional synergies are indeed self-organized, most if not all of the criterial features of self-organized, synergetic systems—multistability, bifurcations, symmetry breaking fluctuations, etcetera—should be found in behavior itself. How, then, do we go from a potentially fruitful analogy to experiments at the bench? How can behavior be understood as a consequence of self-organizing processes? Obviously we have to find an experimental model system that may give some of these ideas a concrete and precise meaning. Such a model system should be simple and accessible, yet still retain the essentials of the coordination problem. A tall order indeed.

REQUIREMENTS OF A THEORY OF SELF-ORGANIZED BEHAVIOR

Theory is a good thing but a good experiment lasts forever.
—Peter Leonidovich Kapitsa

The front page of the January 1993 issue of the American Psychological Association's *Monitor* contained the following headline:"Chaos, chaos everywhere is what the theorists think." According to the article, psychologists picked up on the chaos idea in the early 1980s and "have been applying it with a vengeance ... to both hard core scientific aspects of psychology and clinical psychology, including both family and individual therapy."

I am not going to comment on the rhetoric surrounding the buzzword chaos and how it provides a more holistic view of human life, except to say, chaos of what? What are the relevant variables that are supposed to exhibit chaotic dynamics? What are the control parameters? And how do we find them in complex living systems where many variables can be measured, but not all are relevant? Certainly, if we are so inclined we can use the word chaos to explain everything, but how do we find the nonlinear equations of motion, whether continuous or discrete, in the first place? What is the x in nonlinear equations of the type $\dot{x} = f(x, \lambda)$, the derivative of a variable x with respect to time is a function of x and a parameter, λ? What are the attractors? What does the bifurcation diagram look like? Are these concepts and mathematical tools even relevant? How does one establish them, even in a single case?

All the hype about chaos and fractals tends to sweep these questions under the rug while everyone admires the nice pictures. Don't get me wrong, I like chaos and fractals. Some of my best friends do this stuff. I also like numerical simulation and computer graphics—couldn't do without them, in fact. They allow you to see inside a mathematical theory. But, as a scientist, I want to know what these pictures represent; I especially want to know that the mathematical equations represent (some small portion of) reality. There has to be some connection between mathematical formulae and the phenomena we are trying to understand. Without this connection, as the popular song goes, we're "p___ing in the wind." Establishing a connection between theory and experiment is one of the canons of science that the "chaos, chaos everywhere" crowd seems to ignore.

Once Again—Dynamic Instabilities

Unlike the fluid patterns and chemical reactions described in chapter 1, or Haken's famous laser example where the microscopic level of molecules or atoms is well-defined, in biology and psychology the path from the microscopic dynamics (e.g., the brain with 10^{14} neurons and neuronal connections) to collective order parameters for macroscopic behavior is not readily accessible to theoretical analysis. So how might the spontaneous formation of pattern—self-organization—be studied? What kind of dynamical law gives rise to the self-organization of behavior? The answers to these questions are rooted in the notion of *instability of motion.*

What's so special about instabilities? First, they provide a special entry point because they allow a clear distinction between one pattern of behavior and another. Instabilities demarcate behavioral patterns, thereby enabling us to identify the dimension on which pattern change occurs, the so-called collective variable or order parameter concept of synergetics. As I mentioned earlier, very many observables may, in principle, contribute to a description of behavior even if observation is restricted to a single level. If we study a system only in the linear range of its operation where change is smooth, it's difficult if not impossible to determine which variables are essential and which are not. Most scientists know about nonlinearity and usually try to avoid it. Here we actually exploit qualitative change, a nonlinear instability, to identify collective variables, the implication being that because these variables change chaotic dynamics? What are the control parameters? And how do we find them in complex living systems where many variables can be measured, but not all are relevant? Certainly, if we are so inclined we can use the word chaos to explain everything, but how do we find the nonlinear equations of motion, whether continuous or discrete, in the first place? What is the x in nonlinear equations of the type $\dot{x} = f(x, \lambda)$, the derivative of a variable x with respect to time is a function of x and a parameter, λ? What are the attractors? What does the bifurcation diagram look like? Are these concepts and mathematical tools even relevant? How does one establish them, even in a single case?

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Second, instabilities open a path into theoretical modeling of the collective variable dynamics. In other words, they help us find the equations of motion. The idea is to map observed patterns onto attractors of the collective variable. Instabilities, as we have seen, are created by control parameters that move the system through its collective states. Candidate control parameters have to be found, and instabilities offer a way to find them. Collective variables and control parameters are the yin and yang of the entire approach, separate but intimately related. You don't really know you have a control parameter unless its variation causes qualitative change; qualitative change is necessary to identify collective variables unambiguously.

Third, instabilities provide a means to evaluate predictions about the nonlinear, collective variable dynamics near crisis or critical points. Two predicted features of synergetics concern *critical fluctuations* and *critical slowing down*. In the former, values of collective variables undergo large fluctuations as instability is approached. Fluctuation enhancement, in fact, may be said to *anticipate* an upcoming pattern change. Critical slowing down refers to the ability of the system to recover from a perturbation as it nears a critical point. This recovery process takes longer and longer the closer the system is to a critical state. Measurement of the time it takes to return to some observed state—local relaxation time—is an important index of *stability* and its loss when patterns spontaneously form.

Finally, on a more conceptual level, instabilities are hypothesized to be one of the generic mechanisms for flexible switching among multiple attractive states; that is, for entering and exiting patterns of behavior. Thus, although transitions may be realized or instantiated in a multitude of ways on many different levels, the generic mechanism of instability is universal to all of them.

To summarize briefly, we have tried to rationalize instabilities on both methodological and conceptual grounds as a fundamental mechanism underlying self-organization. All that remains now is to establish their existence in human brain and behavior, specifically, in the experimental laboratory.

The Phase Transition Story

I must admit that how the next sequence of events unfolded is still a bit of a mystery to me. It's a strange mixture of intuition and serendipity. Or, as Louis Pasteur was purported to say, luck favors a prepared mind. The background is this. I was aware through reading Haken's work that when macroscopic patterns of behavior change qualitatively, the dynamics of the entire system may be dominated by one or a few order parameters: when rolling motion starts in Bénard cells there is an enormous compression of information. I was aware also of Schrödinger's order-order transition principle as his proposed new physical principle of biological organization. So some rather vague form was circulating in my mind. But how to create an experimental way to study these ideas so that they might no longer be vague but mathematically exact? To *want* the rules of behavior to be self-organized is one thing, but finding a means to realize one's desires is another issue entirely.

One intriguing idea was that gait transitions—when an animal shifts from, say, a trot to a gallop—might be analogous to the simplest form of self-organization known in physics, namely, the nonequilibrium phase transitions analyzed by Haken. Unfortunately, no one had studied gait transitions in this way, and it was quite impossible to conceive of doing the experiments at Haskins Laboratories, which is world famous for its research on speech, not animal locomotion. Imagine, then, the following scene.

It is the winter of 1980 and I'm sitting at my desk in my solitary cubicle late at night. Suddenly from the dark recesses of the mind an image from an ad for the Yellow Pages crops up: "Let your fingers do the walking" To my amazement I was able to create a "quadruped" composed of the index and middle fingers of each hand. By alternating the fingers of my hands and synchronizing the middle and index fingers *between* my hands, I was able to generate a "gait" that shifted involuntarily to another "gait" when the overall motion was speeded up. Talk about the spontaneous formation and change of ordered patterns!

On hindsight, the emergence of this idea was itself a kind of phase transition reminiscent of the kind experienced by my favorite sleuth, Philip Trent. As his friend, Dr. Fairman explains: "'What Trent means is to put it quite simply, that a certain concept had planted itself in his subconsciousness, where an association of ideas had taken place which abruptly emerged, quite spontaneously and unsought, in the sphere of consciousness.' The Inspector gazes grimly at the speaker for some moments. Oh! If that's all he means, why couldn't he say so? You *have* relieved my mind. He turned to Trent, 'You had a brain-wave—is that it?'"[33]

The effect was unbelievably compelling, a real party trick, as one reviewer from the journal *Nature* said. I quickly found that the situation could be simplified even further to involve just the two index fingers. Was this a paradigm that perhaps might provide a window into self-organization in biology and behavior, that might take us from a potentially fruitful analogy to experiments at the bench? As my colleague Pier-Giorgio Zanone (whose work with me on learning you'll see more about in chapter 6) is fond of remarking, "I can't believe it!" Neither, frankly, could I. The next step was to establish the reproducibility of the phenomenon in a series of experiments. That's just a first step, of course, but here's the gist of what I did.

A Phase Transition in Human Hand Movements

My original experiments involved rhythmical behavior.[34] There are a lot of good reasons why rhythmical movements are a good place to start. Rhythmical behaviors are ubiquitous in biological systems. Creatures walk, fly, feed, swim, breathe, make love, and so forth. Rhythmical oscillations are archetypes

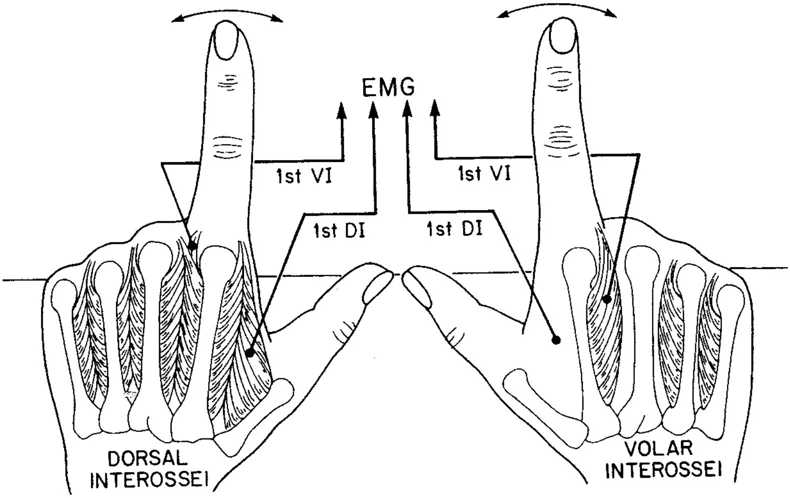

Figure 2.3 One version of the bimanual phase transition paradigm. Subjects move their index fingers rhythmically in the transverse plane with the same frequency for the left and right fingers. The movement is monitored by measuring continuously the position of infrared light-emitting diodes attached to the fingertips. The electromyographic (EMG) activity of the right and left first dorsal interosseus (DI) and the first volar interosseus (VI) muscles are obtained with platinum fine-wire electrodes. (Drawing by C. Carello.)

of time-dependent behavior in nature, just as prevalent in the inanimate world as they are in living organisms. Although they may be quite complicated, we have the deep impression that the principles underlying them should possess a deep simplicity. Ordering or regularity in time is important also for technological devices, including computers.

The task for my subjects, initially colleagues at the lab who wondered what on earth I was up to, was to oscillate their index fingers back and forth with the same frequency of motion in each finger (figure 2.3). Subjects can stably and reproducibly perform two basic patterns, in-phase (homologous muscle groups contracting simultaneously) and antiphase (homologous muscle groups contracting in an alternating fashion). Using a pacing metronome to speed up finger twiddling, oscillation frequency was systematically increased every few seconds from 1.25 cycles per second (Hz) to 3.50 Hz in small steps. Figure 2.4 shows a time series when the subject was instructed to begin moving her fingers in the antiphase mode.

Before going too much further, I should say a bit more about the instructions I gave because they are important. Subjects were required to produce one full cycle of movement with each finger, for each beat of the metronome. Furthermore, if they felt the pattern begin to change, they should not consciously try to prevent it from happening but rather adopt the pattern that was most comfortable under the current conditions. "If the pattern does change," I told them, "don't try to go back to the original pattern but stay in the one that's most comfortable. Above all, try to keep a one-to-one (1 : 1) relationship between your rhythmical motions and the metronome beat." My

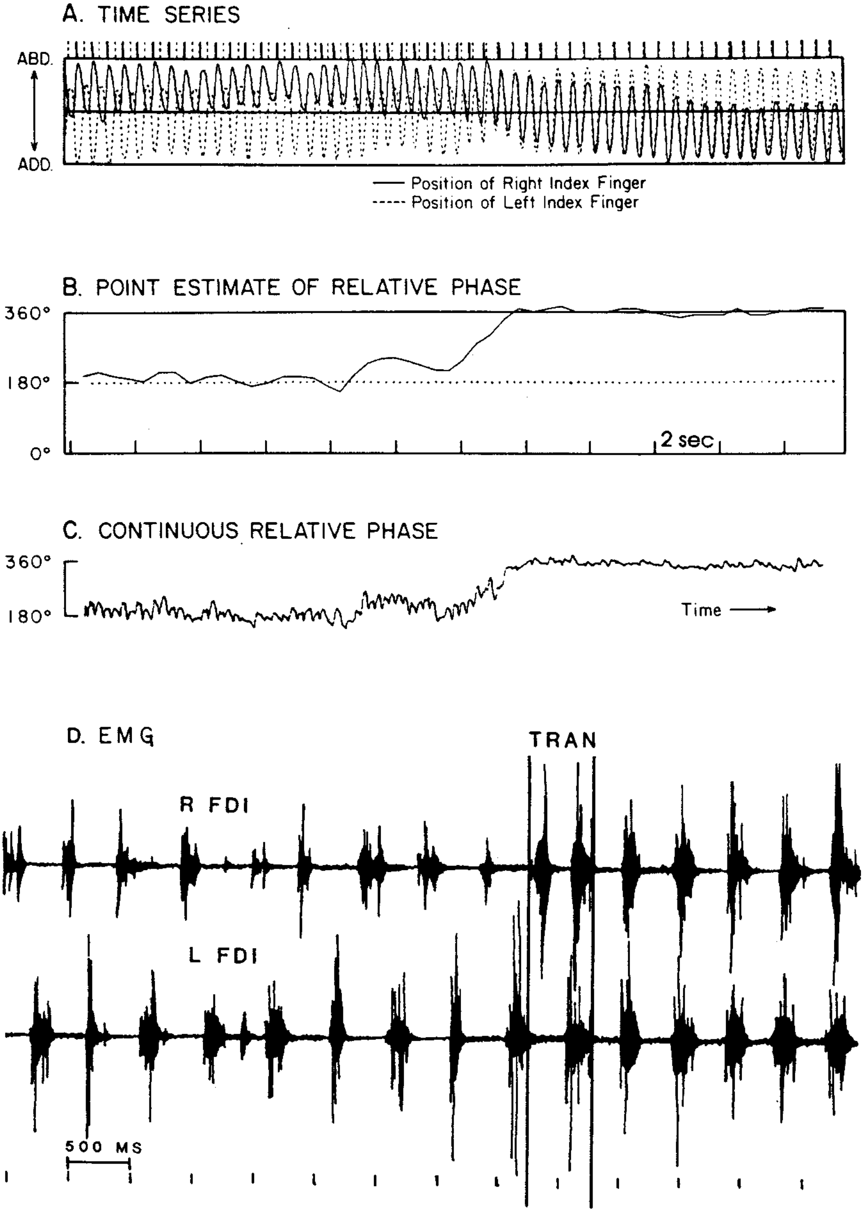

Figure 2.4 (A) The time series of left and right finger position shows the transition from antiphase movement to in-phase movement. From left to right the movement frequency, (*F*), was increased. (B) The point estimate of relative phase (obtained from the relative position of the left finger's peak extension in the right finger's cycle) changes from fluctuating around 180 degrees to fluctuating around 360 degrees. (C) A more refined measure of relative phase is the continuous estimate, obtained from the difference of the individual finger's phases that were calculated from the phase plane $(x, \dot{x})$ trajectory. (D) The EMG record of left and right first DI muscles also shows the change in phasing.

reasons for all this will become clearer later on when we consider the role of volition or intentionality in the self-organization of behavior (chapter 5). For now, it's important to establish the mechanisms underlying involuntary or spontaneous pattern formation and change.

As figure 2.4 clearly reveals, around a certain frequency of movement (the critical region), subjects spontaneously switch from the antiphase parallel motion of the fingers to an in-phase symmetrical pattern. No such switching, however, occurs when the subjects start in the in-phase mode. They stay there throughout the entire frequency range. Thus, while people can produce two stable patterns at low frequencies, only one pattern remains stable as frequency is scaled beyond a critical point.

I devised a way to monitor the transition behavior by calculating the phase relationship between the two fingers. A *point estimate* of relative phase is the latency of one finger with respect to the other finger's cycle time or period, determined from its peak-to-peak displacement. When the latency, t, of one finger, (x_L), is divided by the period, T, of the other (x_R) and multiplied by 360 degrees, we obtain the relative phase in degrees (figure 2.5A). This measure evaluates coordination at only one point in each cycle. I also made a *continuous*

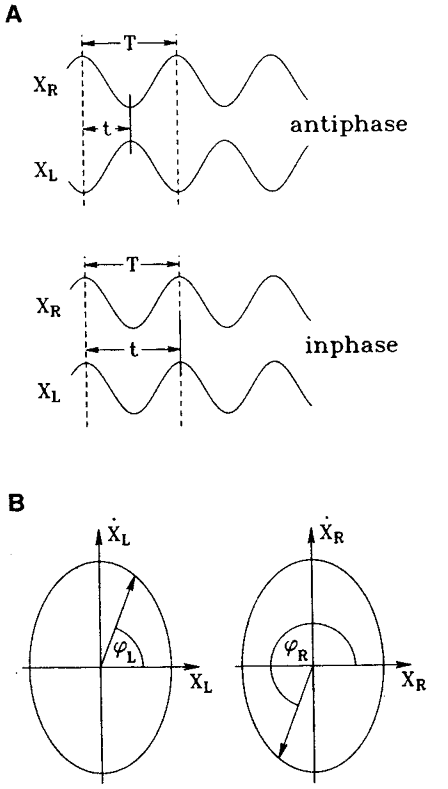

Figure 2.5 (A) Calculation of relative phase as a point estimate from two time series. (B) Calculation of the continuous relative phase from phase plane trajectories (see text).

estimate of relative phase by calculating the relative phase at the sampling rate of 200 times a second. Figure 2.5B shows how this was done. The *phase plane trajectory* (a plot of each finger's velocity, $\dot{x}$, versus its position, x) of each finger is shown. Normalizing the finger oscillations to the unit circle, the phases, ϕ_L and ϕ_R, of the fingers are obtained simply from the arctangent, $(\dot{x}/x)$, if x is the normalized position. The continuous relative phase is then just the difference $(\phi_L - \phi_R)$ between these individual phases at every sample. In figure 2.4 we see that the relative phase fluctuates before the transition and stabilizes thereafter. Amazing!

I first formally reported the result at a major meeting of experimental psychologists in the United States, the Psychonomic Society, in 1981. My talk wasn't very well attended. In those days, self-organized phase transitions in psychology were hardly in vogue. A little later, in March 1982, Arnold Mandell and Gene Yates invited me to a conference they were organizing at Ray Kroc's ranch (the late Ray Kroc of McDonald's hamburger fame) in Santa Barbara called "Nonlinearities in Brain Function." With all the hoopla about chaos in the brain—not to speak of other body parts—in the last few years, Mandell and Yates are seldom mentioned. In my opinion, history will reveal them as visionaries, far ahead of their time.

I will never forget the Kroc conference. It was one of the intellectual highlights of my life. Arriving at the ranch after a bus journey through the Santa Barbara hills (during which Mandell, referring to the latest work in nonlinear dynamical systems, told me excitedly, "You ain't seen nothin' yet!"), we quenched our thirst at a huge sideboard containing individual dispensers of every drink imaginable. This Irishman's dream.

But when it came to proposing theoretical models of my phase transition experiments, the well, so to speak, was dry. And this well included some of the top theoretical physicists and applied mathematicians (as well as neurobiologists) in the world. For example, I roomed with a young theoretical physicist from Los Alamos, Doyne Farmer, who was later featured prominently in James Gleick's book *Chaos*. Farmer was and is right at the forefront of the nonlinear dynamics business, and a brilliant teacher to boot. I felt embarrassed to show my little toy model borrowed from catastrophe theory. Little did I know at the time that the picture I'd formed was a reasonably good guess, but hopelessly wrong in detail. It was only an image, a vague analogy, at best. Here are my notes from 1982, word for word, describing this crude picture which was resurrected from my files (figure 2.6):

> Think of an asymmetric potential well; choose the initial condition by applying a "force"(?) favoring the left-hand well [the antiphase pattern]. As this potential system is scaled(?) the right hand well [the in-phase pattern] becomes strongly favored, i.e., the depth of the right-hand well relative to the left is increased. When the left-hand well is somewhat flat, the system is particularly influenceable such that any increase in "dissipative noise" will effect a shift into the right-hand well (a favored mode).

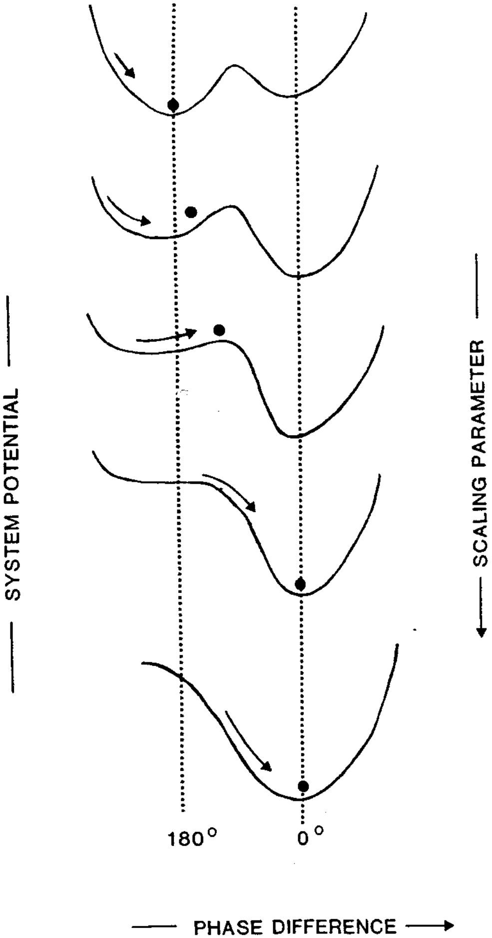

Figure 2.6 The original phase transition model presented by the author at the Kroc Foundation conference on "Nonlinearities in Brain Function."

Brackets are added to clarify the meaning of left- and right-hand wells. The question marks are in the original notes and reflect my uncertainty about which words to use. More sophisticated alternatives to figure 2.6 were suggested to me at the Kroc meeting—Niemark bifurcations, forced Duffings, and the like. Although I didn't fully understand them at the time, these turned out to be wrong too.

One person who was unable to attend the Kroc conference due to illness was the theoretical physicist Hermann Haken. Although I didn't know it (or him), he was just the person I was looking for.

To cut a long story short, after reading a draft of my experimental paper that I had sent to him for comments, Haken invited me to come to Stuttgart in the summer of 1983 to work with him and his co-workers on a theoretical model of phase transitions in human hand movements. From that point on, a strategy evolved in which perceptual-motor coordination was viewed no longer as a fairly peripheral (to some) topic of study in its own right, but as a window into biological self-organization. The mystery is that none of this would have happened had I not imagined my fingers as walking.

From Phenomena to Theory

To recap, the main features of my experiment were fourfold. First was the presence of only two stable coordination patterns between the hands. Which one was observed was a function of the initial conditions, meaning how subjects were instructed to move their hands at the beginning of the experiment. The fact that humans can stably produce, without a lot of learning (see chapter 6), only two simple coordination patterns between the hands remains for me an absolutely amazing fact. A complex system of muscles, tendons, and joints interacting with a much more complex system composed of literally billions of neurons appears to behave like a pair of coupled oscillators. A truly synergetic effect! Later, Betty Tuller and I showed that even skilled musicians and people who have had the two halves of their brain surgically separated to control epileptic seizures are still strongly attracted to these two basic patterns.[35] That is not to say that other timing patterns are impossible; only that people have a great deal of difficulty producing them. Second was the abrupt transition from one pattern to the other at a critical movement frequency. Third was the result that beyond the transition, only the symmetrical pattern was stable. Fourth, when cycling frequency was reduced, subjects did not spontaneously return to the initially prepared antisymmetrical pattern but stayed in the symmetrical one.

Now that we are in possession of the main facts, the next step is to identify candidate collective variables and control parameters. Since the fingers are moving at a common frequency, one candidate order parameter for coordination might be the relative frequency or frequency-ratio between the time-varying components. But frequency-related measures are inadequate because they refer to events occurring in an individual component, not *between* components. As I emphasized before, to understand coordinated behavior as self-organized, new quantities have to be introduced beyond the ones typical of the individual components. Also, we need a variable that captures not only the observed patterns but transitions between them. Only the *phase relation* appears to fulfill these requirements.

Unlike many other possibilities, it is relative phase that reflects the cooperativity among the components and embodies the kind of circular causality typical of synergetic systems. Thus, on the one hand, the interaction of the subsystems (here the individual finger motions) specifies their phase relation, and on the other, phase specifies the ordering in space and time of the individual subsystems. Also, as figure 2.4 shows, the phase relation changes far more slowly than the variables describing the behavior of the individual components that are oscillating to and fro—another typical feature of the collective variable or order parameter concept. But the most important reason why phase is a suitable order parameter is that it changes abruptly at the transition and is only weakly dependent on the prescribed frequency of movement outside the transition region. Since frequency of movement induces a qualitative change in phase, it may be considered an appropriate control parameter.

The final step is to develop a theoretical model that captures the main qualitative features of the data. If we can do that, quantitative predictions may be expected to follow. But extracting a law of coordination from a set of measurements is not so trivial. The big plus here is that we've done a simple experiment that contains many of the desirable features of biological systems that we want to understand, such as stability, flexibility, switching capability, and so on, yet at the same time prunes away many of the real life complications typical of naturally occurring behaviors. Just as Galileo used an inclined plane (which he could manipulate) to understand the free fall of objects (which he could not), so this phase transition situation allows us to understand how coordinated actions are self-organized. Now the aim is to obtain a precise mathematical description of coordination, stripped down to its essentials.

A Brief Digression

I promised myself as well as the reader that I would limit the number of mathematical equations in this book, relegating them to the technical literature. But the main equation describing coordination is about to appear on center stage, and to develop it I need a few elementary concepts from the field of *dissipative dynamical systems*. A dynamical system is simply an equation or set of equations stipulating the evolution in time of some variable, x. In our case we are interested in the temporal evolution of our hypothesized collective variable, relative phase. How does it change from moment to moment as the control parameter varies?

A dynamical system lives in a *phase space* that contains all the possible states of the system and how these evolve in time. A dissipative dynamical system is one whose phase space volume decreases (dissipates) in time. This means that some places (subsets in the phase space) are more preferred than others. These are called *attractors*: no matter what the initial value of x is, the system converges to the attractor as time flows to infinity. For example, if you stretch a spring or displace a damped pendulum, they will eventually wind down and stop at their equilibrium positions. The attractor in each case is a fixed point or simply *point attractor*.

Some people say that point attractors are boring and nonbiological; others say that the only biological systems that contain point attractors are dead ones. That is sheer nonsense from a theoretic modeling point of view, as it ignores the crucial issue of what fixed points refer to. When I talk about fixed points here it will be in the context of collective variable dynamics of some biological system, not some analogy to mechanical springs or pendula. Other kinds of attractors than fixed points also exist, such as limit cycles and chaotic attractors, but we'll discuss them more fully as they emerge in specific examples later on.

An important concept related to the idea of attractors is the *basin of attraction*. For a given attractor, this refers to the region in phase space in which almost all initial conditions converge to the attractor. Several attractors with different basins of attraction may also exist at the same time, a feature called *multistability*. Multistability, the coexistence of several collective states for the same value of the control parameter, is, of course, an essential property of biological dynamics. When a control parameter changes smoothly, the attractor also usually changes smoothly. However, when the parameter passes through a critical point, a qualitative change in the attractor may take place. This phenomenon, as mentioned before, is called a *bifurcation*, the mathematical term used in dynamic systems theory, or *nonequilibrium phase transition*, the term preferred by physicists because it includes the effects of fluctuations.

Finally, when the *direction* in which the control parameter varies is changed, the system may remain in its current state or switch at a later point, thereby exhibiting *hysteresis*. This means that an overlapping region exists where, depending on the direction of parameter change, the system can be in one of several states. As we have stressed, bifurcations and hysteresis are hallmarks of nonlinearity in complex biological systems.

The Haken-Kelso-Bunz Model

What, we asked ourselves, is the layout of attractor states in our hand movement experiments, and how is that layout altered as a putative control parameter is changed? Answers to these questions rest on the nature of the collective variable relative phase, ϕ, which turns out to possess an amazing *symmetry* in both space and time. A symmetry is simply a transformation that leaves the system the same afterward as it was before. What can systems with symmetry do? Imagine your fingers walking again. If you look in the mirror while you do the antiphase and in-phase movements, you can exchange left or right hands and the phase relation does not change. In other words, a *spatial* symmetry exists. Similarly, the hand motions are periodic, repeating at regular intervals in time. If we shift time by one period forward or backward the relative phase stays the same. Periodicity, in other words, constitutes a *temporal* symmetry. The fact is that the *only phase relations possible* under left-right exchange and a phase shift of 2π are in phase ($\phi = 0$) and antiphase ($\phi = \pm\pi$). It almost suspends belief that these silly hand movement experiments reveal the existence of a spatiotemporal symmetry that governs the way individual components (here the fingers) interact in space and time.

Of course, much of the action will come when we break or lower this spatiotemporal symmetry, which nature does all the time! But for now, the task is to postulate the simplest mathematical function that could accommodate space-time symmetry, bistability, and the observed bifurcation diagram in the walking fingers experiments. Let's call this function, V, now known in the literature after Haken, Kelso, and Bunz as the HKB model.[36] Since V is time symmetric (periodic), we can write

$$V(\phi + 2\pi) = V(\phi).$$

Since V is mirror image or space symmetric (left-right exchange) we can write

$$V(\phi) = V(-\phi).$$

The first condition allows us to express a function, V, as a Fourier series. According to Fourier, any periodic function, and indeed many functions normally encountered in physics, can be made up as the sum of simple harmonic components such as sines and cosines. The second condition eliminates sines from the function, since only cosines are invariant when ϕ is replaced with $-\phi$. Any intrinsic left-right asymmetry, of course, requires the inclusion of sines. For now, to accommodate all our observations we need include only the first two terms of the Fourier series:

$$V = -a\cos\phi - b\cos 2\phi,$$

where the minus signs allow us to interpret the function, V, as a landscape with attractor states for positive values of a and b.

The behavior of the system is easy to visualize by identifying ϕ with a black ball moving in an overdamped fashion in the landscape defined by the function, V. By changing the ratio b/a, inversely related to frequency in my experiment, we can travel through the evolving landscape as shown in figure 2.7. When we initially prepare the system in a state illustrated by the black ball in the upper left panel ($\phi = \pm\pi$) and decrease the ratio b/a (equivalent to shortening the period of the rhythmical coordination pattern) we obtain a

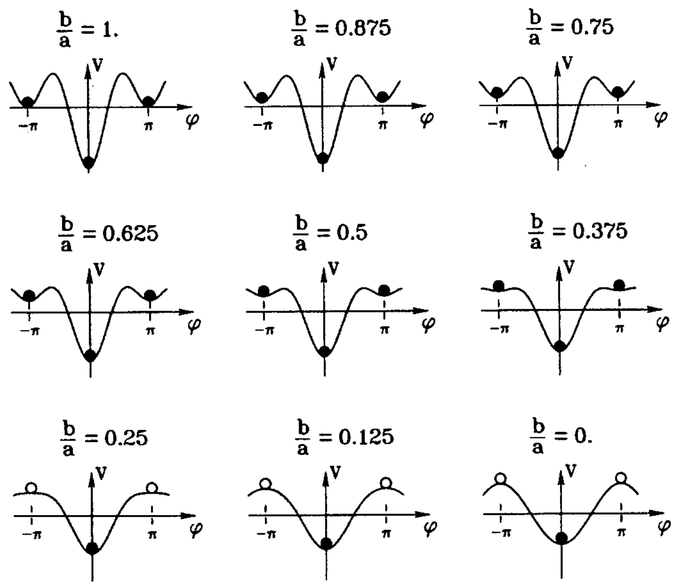

Figure 2.7 The HKB model of coordination. The potential, $V(\phi)$, as the ratio b/a is changed. The little ball illustrates the behavior of the system initially prepared (upper left corner) in the antiphase state. White balls are unstable coordinative states; black balls are stable.

critical value where the ball falls to the lower minimum corresponding to $\phi = 0$.

This means that the hand movements exhibit a transition from the antisymmetrical ($\phi = \pm\pi$) mode into the symmetrical ($\phi = 0$) mode. Notice that when we now reduce the frequency of motion, reversing the direction of parameter change, starting in the lower right portion of figure 2.7 the system will stay in the symmetrical in-phase mode even past the critical point. Theoretically (and experimentally) $\phi = 0$ is the deepest minimum of the function and therefore the most stable coordination pattern. But even more important is that our theory contains the experimentally observed and essentially nonlinear hysteresis effect.

We have built up a theoretical model of the phase transition without any discussion of differential equations. Differential equations arise whenever a law is expressed in terms of variables and their derivatives, or rates of change. Our coordination law may therefore be expressed in terms of the derivative of the collective variable, ϕ, which we denote as $\dot{\phi}$. This is simply the negative derivative of the function, V, with respect to ϕ.

$$\begin{aligned}\dot{\phi} &= -\frac{dV}{d\phi}\\ &= -a\sin\phi - 2b\sin 2\phi.\end{aligned}$$

A beautiful way to intuit this basic coordination law is to plot the derivative of ϕ (called phi dot or $\dot{\phi}$) against ϕ itself for different parameter values. This is called the *vector field* of the relative phase dynamics and is shown in figure 2.8. Note that our coordination law contains stationary patterns or fixed points of ϕ at places where $\dot{\phi}$ is zero and crosses the ϕ-axis. When the slope of $\dot{\phi}$ is negative at the abscissa, the fixed points are *stable* and *attracting*. When the slope is positive, the fixed points are *unstable* and *repelling*. Arrows are drawn in figure 2.8 to indicate the direction of flow. Thick solid lines correspond to stable fixed points; dashed lines represent the unstable fixed points. As one travels from bottom to top in this figure, decreasing the ratio b/a, the stable fixed point at $\phi = \pi$ eventually disappears, leaving only one at $\phi = 0$. The bifurcation, appropriately enough, is called a pitchfork in the jargon of dynamical systems: the stable coordination pattern at $\phi = \pi$ is surrounded by two unstable fixed points delineating its basin of attraction, only to be annihilated at a certain critical point.

Notice again the terribly important fact that there is no one-to-one relation between the parameter value and the coordinative patterns. *Both modes coexist* for the *same* parameter value, necessitating nonlinear models. This is what I meant earlier by the need to formulate a coordination law that is simple, but not too simple. Our elementary coordination law possesses a remarkable symmetry and contains multistability, bifurcation, and hysteresis as primitive behavioral properties.

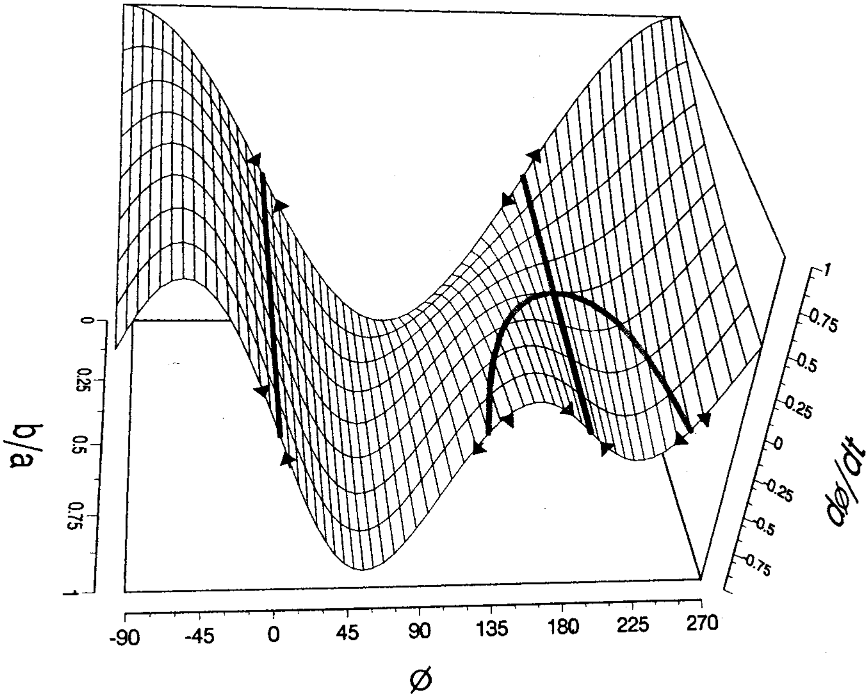

Figure 2.8 The HKB model of coordination expressed as a vector field, arrows indicating the direction of flow (see text for details). Thick solid and lighter dashed lines correspond to attractive and repelling fixed points of the collective variable dynamics. Note the inverse pitchfork bifurcation as the control parameter *b/a* is decreased.

... And Back Again?

All the main features of my experiments—the presence of only two stable relative phase or attractor states between the hands; transitions from one attractor to another at a critical cycling frequency; the existence of only one attractor state beyond the transition; even hysteresis—have been theoretically modeled, but so what? What makes us think that HKB theory is any more than a compact mathematical formulation? If the experiment is a really crucial one, we still have to prove that our approach has primacy over others, notwithstanding that it can describe our results well.[37] I have to admit that one of the main motivations behind these experiments was to counter the then dominant notion of motor programs, which tries to explain switching (an abrupt shift in spatiotemporal order) by a device or a mechanism that contains "switches." This seems a cheap way to do science, kind of like attributing thunder to the Norse god Thor. I have the same problem with ascribing words such as "schizophrenic," "alcoholic," and "depressive" to genes, but that's another book.

The real power of the synergetic theory of self-organization lies in the central concept of stability, which is important because stability can be lost.

That is exactly what happens at nonequilibrium phase transitions where patterns form or change spontaneously with no specific ordering influence from the outside (and no homuncular motor program inside). The hallmark features of such instabilities are, as I mentioned before, a strong enhancement of fluctuations (critical fluctuations) and a large increase in the time it takes the system to relax from a perturbation (critical slowing down). As we will see, our specific theoretical model of hand movements contains these predictions, thus allowing us in principle to transcend mere description.

Critical slowing down is easily intuited from the pictures of the evolving attractor landscape and its corresponding vector field (figures 2.7 and 2.8). In the former, notice how the potential around $\phi = \pm\pi$ deforms, the minimum in question becoming shallower and shallower as the parameter reaches a critical point. If perturbed away from its minimum, the little black ball will relax slowly compared with when the slope around the minimum is steep (top left). Similarly, in figure 2.8 the slope around $\phi = 0$ is greater than the slope near $\phi = \pi$ (180 deg.), which progressively gets shallower and hence less attracting as the parameter approaches criticality. There, the system is poised to change state by just the slightest little nudge.

Critical fluctuations arise because all real systems are subject to random fluctuations of various kinds, such as the environment and the multitude of microscopic components, that produce deviations away from the attractor state. Imagine a soccer team kicking our little black ball entirely at random. When the slope of the hill is steep, the ball can't be kicked very far away from its equilibrium position. When the slope flattens, however, the same magnitude of kick will cause the ball to move much farther away. As a result, near the critical point the attractor state suffers wild critical fluctuations.

I tested these predictions in the bimanual coordination paradigm with the help of John Scholz. I had already noted in the original publications that the phase relation between the limbs became much more variable near the transition, and discussions with Haken encouraged me to look in detail at the fine structure of fluctuations.

Scholz and I measured fluctuations in the two basic coordination patterns as subjects increased movement frequency by calculating the standard deviation from the relative phase time series. Dramatic increases in fluctuations were noted for the antiphase, but not the in-phase pattern before the transition. After the transition, the previously unstable antiphase state (now in-phase) fluctuated at the same low level as the stable in-phase state, a striking confirmation of the prediction.[38]

We tested critical slowing down by applying a little torque pulse to perturb briefly and unexpectedly one of the subject's oscillating fingers. This knocked the fingers away from their established phase relation and allowed us to calculate the time taken to stabilize the phase again at its value before the perturbation. In agreement with theory, we found that as the critical point neared, the relaxation time in the antiphase mode increased while it remained constant or decreased in the in-phase mode. Also we found that perturbations near the critical transition frequency often caused transitions from one mode to the other, exactly what one might expect from a complex dynamical system poised near an instability.[39]

Perhaps I should say a word or two about how to calculate the theoretical model parameters a and b. As we know, the ratio $|b/a|$ in the HKB model corresponds to the control parameter, movement frequency, in the experiment. From another viewpoint, the ratio expresses the relative importance of the phase-attractive states at 0 and $\pm\pi$ (we remind the reader that for $|b/a| > 0.25$, the system is bistable; as the ratio approaches a critical value the antiphase state loses stability and for $|b/a| < 0.25$, the system is monostable at the in-phase state (see again figure 2.7). Therein lies the secret to calculating the parameters of our theoretical model. But to do this, we have to take fluctuations into account explicity. Technically speaking, we have to study the transition behavior by adding a fluctuating force to the HKB model. This means solving the stochastic dynamics of ϕ by transforming it into a Fokker-Planck equation (apologies for the technical jargon). This equation describes the time evolution of the probability distribution for a system described, like the HKB model by a potential, V. Gregor Schöner, an expert in stochastic dynamics, collaborated with Haken and me on this problem.

One outcome of Schöner's analysis was that it allowed us to determine the model parameters, a and b, as well as how strong the noise is in the system, using experimental information on local relaxation time and variability measures of the patterns in the noncritical parameter regime. For example, the relaxation times were predicted as:

$$\tau_{rel,0} = 1/4b + a; \qquad \tau_{rel,\pi} = 1/4b - a,$$

where 0 refers to the in-phase mode and π to the antiphase mode. Similarly, we were able to estimate the noise strength in the system using measures of phase *variability* in the two modes before the onset of the transition.

I should stress that this is not just a parameter-fitting exercise, but rather it allowed us to check the consistency of the entire approach. Moreover, the stochastic theory contained another feature that we were able to examine experimentally, namely, how long the transition should take from antiphase to in-phase, a variable we called the *switching time*. An excellent agreement between the stochastic version of the HKB model and the experimental data was observed, in terms of both the mean switching time and the shape of the distribution of switching times.

Obviously, a lot of mathematical details have been left out of this description. Mathematics, as one wag said, is like sex, better performed in private than in public. However, I did not want to leave the issue of parameter estimation dangling, as if parameters were left freely blowing in the wind (as they sometimes are in theories). A conceptually important result of our analysis is that not just control parameters but fluctuations are instrumental in effecting transitions, probing the stability of coordinated states and pushing the system over the edge from unstable to stable states. Confirmation of theoretical predictions regarding critical fluctuations, critical slowing down, and switching times reveals that the emergence of coordinated behavior may be understood in considerable detail in terms of the physics of nonequilibrium processes. These same effects have now been observed in many other experimental model systems, attesting to the general validity of the theory (see chapters 3 and 4).

On the lighter side, two real world examples of fluctuation phenomena are shown in figure 2.9. One picture, under the headline "Nightmare," shows the

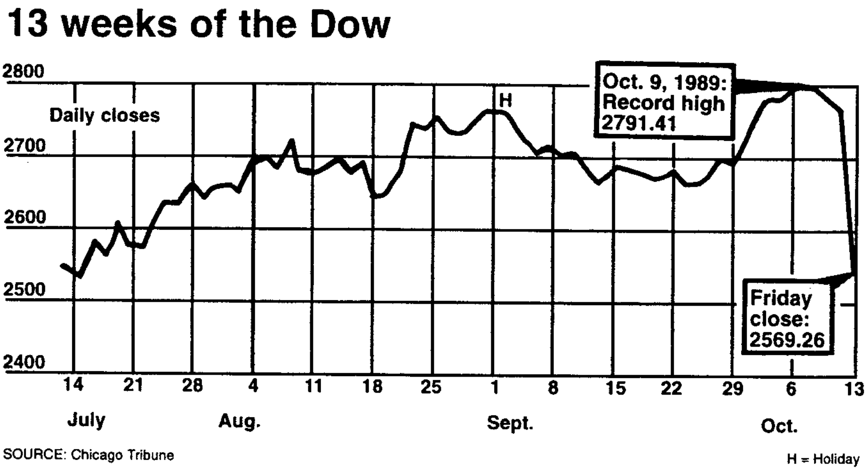

Figure 2.9 Two real world examples of fluctuations: (A) economic (Reprinted with permission from Knight-Ridder Graphics Network), and (B) political. (Copyright © 1992 by the New York Times Company. Reprinted by Permission)

fluctuations in the stockmarket before the big drop of nearly 200 points on October 13, 1989. Are they critical? The other ("Switch, Don't Fight, Mr. Perot," *New York Times*, September 18, 1992) shows the independent candidate for U.S. president, Ross Perot, on the brink, trying to decide whether to stay in the race or not. So what pushed him to stay in?

Relating Levels. I. The Components

A key feature of our approach is to characterize coordinated states in terms of the dynamics of collective variables, in this case, with relative phase as an order parameter. Obviously, it is possible to study the system on yet another level of description, namely, that of the individual limb or finger's dynamic

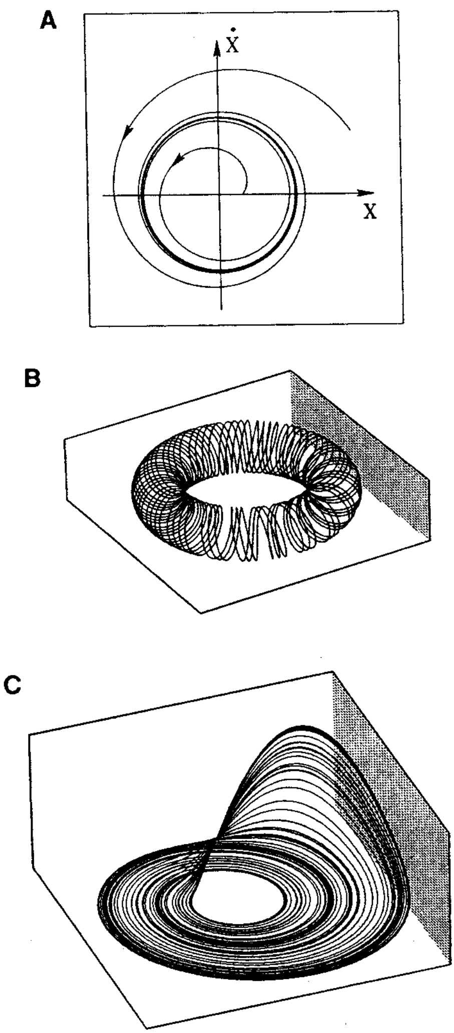

Figure 2.10 (A) Limit cycle attractor. (B) Torus or quasi-periodic attractor. (C) Chaotic attractor.

behavior. Thus we may choose each limb's position, x_i, and velocity, $\dot{x}_i$ ($i = 1, 2$), as collective variables; collective now with respect to the next lower level of description, such as the coordination of neuromuscular activities (see below). The stable and reproducible rhythmic performance of each hand may now be modeled as an attractor in the phase plane, $(x_i, \dot{x}_i)$, in this case a limit cycle. When the hand is on its limit cycle, it oscillates with a certain frequency and amplitude that are functions of parameters only, not of the initial conditions. The stability of this attractor is revealed by the fact that trajectories originating outside the limit cycle spiral inward, whereas trajectories inside spiral outward toward the limit cycle (figure 2.10A).

The stability of the limit cycle and its persistent, self-sustaining character are fundamentally due to a balance between excitation and inhibition (from the nervous system) and dissipation. Dissipation predominates outside the limit cycle, causing the amplitude to decrease; excitation predominates if x_i and $\dot{x}_i$ are small and inside the limit cycle, causing the amplitude to increase.

Bruce Kay, Elliot Saltzman, and I sought kinematic relations that might allow us to identify the form of the limit cycle characterizing each oscillating limb.[41] For example, in studying individual hand movements we found that amplitude of movement decreased monotonically as frequency was experimentally increased. We were able to map this and other observed kinematic relations onto a limit cycle attractor that combined features of the well-known Rayleigh and van der Pol oscillators. In performing this mapping, the concept of stability was once again at the heart of theory. We measured the stability of the attractor using perturbation techniques similar to those described for the coordinated case. Trajectories perturbed away from the limit cycle return more rapidly to strong attractors than weak ones.

Another assumption of limit cycle models is that the oscillation is essentially autonomous. The basic idea is that the phase of an autonomous time-invariant oscillator is marginally stable, unlike that of a driven oscillator that is locked to the driving function. A way to check this is to perturb the hand at different phases of its oscillation and see if and how the phase is reset or shifted. To assess the pattern of phase shift that the rhythm exhibits, we constructed and analyzed *phase-resetting curves* that plot the old phase where the cycle is perturbed against the new phase at which the cycle stabilizes after the perturbation.[42] We found that the oscillation tended to be *phase-advanced* by a perturbation, thereby producing a consistent phase-dependent shift in pattern. From these phase resetting results we concluded that central timing elements in the nervous system responsible for generating rhythms (see chapter 4 and below) are affected by the biomechanical properties (e.g., stiffness, damping) of the limb being controlled.

A final assumption of limit cycle attractors is that they are effectively one dimensional, forming a simple closed curve in phase space. However, real biological systems—and our hand movements are no exception—are not mathematically ideal, perfectly periodic systems. When plotted on the phase plane, a rhythmic movement trajectory appears instead as a band around some average closed curve. But how many degrees of freedom are actually involved? If the variability is due to stochastic noise, it is an infinite number—a daunting prospect. If the band of variability is produced by additional *deterministic sources*, for example, oscillations having frequencies that are incommensurate with the main frequency, the attractor should be m-dimensional, one for each oscillatory process. Topologically, such a *quasi-periodic* attractor is defined by a m-dimensional torus (T^m). Figure 2.10B shows an example for $m = 2$. Bands of variability on the phase plane may also be produced by *deterministic chaotic processes* that exhibit fractional or fractal dimension[43] (figure 2.10C).

Using a computational method[44] that allowed us to estimate the dimensionality of hand movement trajectories directly, we found evidence for two processes, one at a global scale and one at a small scale. The global process was a low-dimensional limit cycle attractor entirely consistent with earlier kinematic results showing stability in the face of perturbation. The small scale process was essentially infinite-dimensional, that is, stochastic noise. Model simulations on a computer confirmed this result. The main point is that once again, although now at the level of the individual components, we can take a complex dynamic behavior and map or encode it onto a dynamical model whose veracity can be checked experimentally. By such methods (and I know how time consuming they are) it is possible to reach an understanding of the coordination dynamics on different levels.

Relating Levels. II. Coupling

What is the relation between the limit cycle attractors of one hand and the phase entrained coordination dynamics of two hands working together? Coordinative and component levels of description can now be related by coupling the latter to create the former. This is not as trivial as it sounds. The simplest kind of coupling is obviously linear, for example, making the coupling a function of the amplitude differences between the oscillators. This doesn't work, at least if the goal is to derive all the features observed at the coordinative level. It turns out that not only do the oscillatory processes have to be nonlinear, so also does the *coupling*. Minimally, coupling functions have to contain terms that are products of individual oscillator amplitudes and velocities, that is, time derivatives. It is important to emphasize that the coupling is quite unspecific with respect to the patterns of coordination that emerge. That is to say, different coupling functions can give rise to the same coordination patterns.[45] Also, *changes* in coordination can be brought about in a variety of ways.

Figures 2.11a through c show *Lissajous curves* of our nonlinearly coupled nonlinear oscillator model of basic coordination. The different widths and slopes of the tracings reflect the phase difference between the oscillators. In each case, the initial conditions of each oscillator are identical, and all that is done in the computer simulation is to increase the intrinsic frequency of

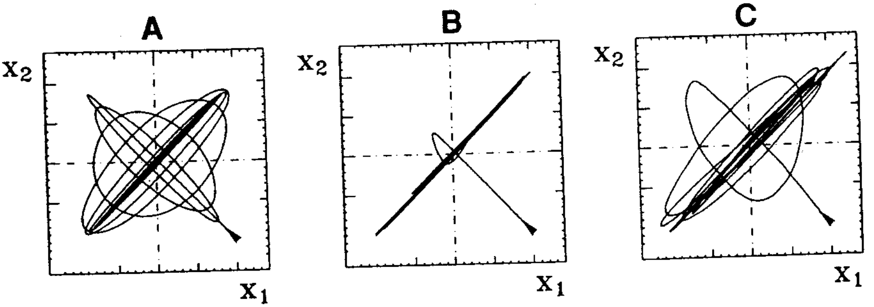

Figure 2.11 Computer simulations of nonlinearly coupled nonlinear oscillators. Arrows indicate the starting antiphase state. The coupling is of the form $\alpha(\dot{x}_1 - \dot{x}_2) + \beta(\dot{x}_1 - \dot{x}_2)(x_1 - x_2)^2$, where x_1 and x_2 refer to the oscillators, and α and β are coupling coefficients. A noise term, $F(t)$, is added to the coupling. (A) Fixed coupling parameters and noise. (B) Increase in coupling strength with noise the same strength as A. (C) Coupling parameters same as A, but noise strength is increased.

oscillation. In figure 2.11a the coupling parameters and the noise level are constant, and a transition occurs as frequency is increased. In figure 2.11b the coupling strength is increased, and the transition occurs almost immediately. A similar result is evident in figure 2.11c, but here the coupling parameters are as in figure 2.11a, and only the noise level parameter is increased.

A nice way to summarize the entire phenomenon is through the torus plots displayed in figure 2.12. The state spaces of the two oscillators ($x_1, \dot{x}_1$ and $x_2, \dot{x}_2$) (figure 2.10A) are plotted against each other. For a rational frequency relationship between the two oscillators, in the present case 1 : 1, the relative phase trajectory is a closed limit cycle and corresponds to a phase- and frequency-locked state. Figure 2.12a shows a stable antiphase limit cycle transiting (figure 2.12b) to a stable in-phase trajectory (figure 2.12c). A flat representation of the torus displays the phase of each oscillator in the interval $(0, 2\pi)$ on horizontal and vertical axes. The constant relative phase between the oscillators is reflected by straight lines.

In the flat representation shown in figure 2.13a the phase relation is π; that is, one oscillator's phase is zero and the other is π. The apparent discontinuity is not real, but is due to the 2π periodicity of the phase. Thus, when you fall off the top edge of the plane in figure 2.13a you reappear at the bottom. Figure 2.13, parts b and c show the transition to the in-phase relationship (zero phase lag between the oscillators).

The conclusions from experiment and theory are inescapable. First, the same behavioral patterns may be obtained from very different kinds and strengths of couplings among the components. In other words, *invariance of function* is guaranteed despite reconfiguration of connections or couplings among component elements. Second, and related, several patterns (here for convenience, two) may be produced by the same set of components and couplings. Such *multifunctionality* is an intrinsic property of the present approach. Third, one can see how difficult it is in complex biological systems to

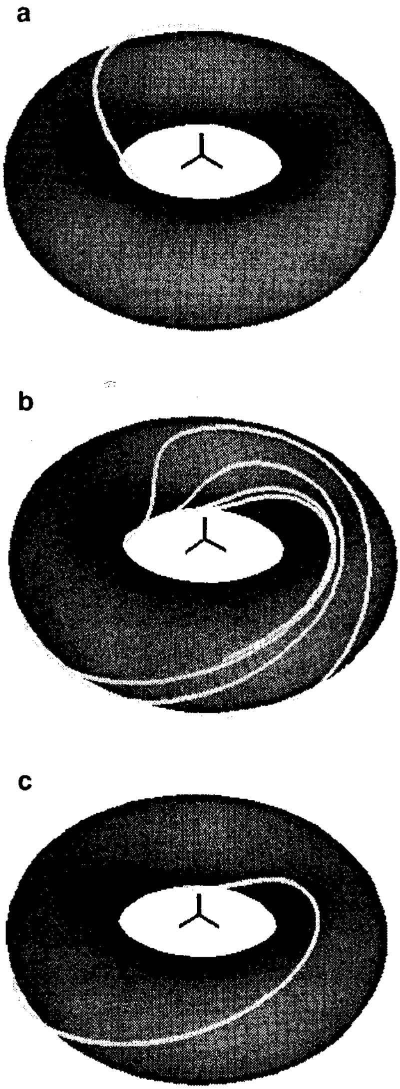

Figure 2.12 Same transition as in figure 2.11 but now displayed on the torus. As coupling is varied a stable antiphase trajectory (a), loses stability (b), and switches to a stable in-phase trajectory (c).

attribute the system's collective or coordinative capabilities to couplings per se. Many mechanisms, both physiological and mathematical, can instantiate or realize the same function. The conspicuous lack of a one-to-one relationship between self-organized coordination patterns and the structures that realize them is a central feature of the present theory, and surely constitutes one of the basic differences between living things and mechanisms or machine.[46]

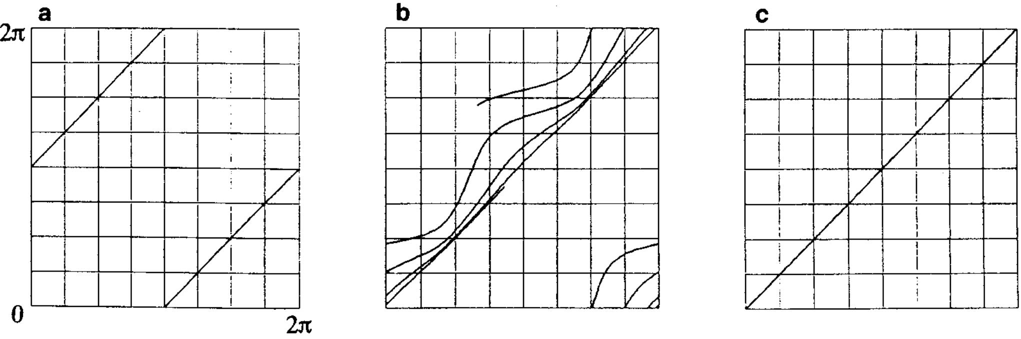

Figure 2.13 Flat representation of the torus displaying the phase of each oscillator on the horizontal and vertical axis. The transition is the same as shown in figure 2.12.

The Tripartite Scheme—Once More with Feeling

It may be possible to carry out this level-independent analysis when we step down to other scales, such as that of neurons and neuronal assemblies (see chapter 8). But for now, let's pull together the main conceptual themes that emerge from the walking fingers example. Figure 2.14 represents the linkages between phenomena and dynamic pattern theory (horizontal mapping) and between levels of description (vertical mapping). Here are the key points to keep in mind.

- A minimum of three levels (the task or goal level as a special kind of boundary constraint, collective variable level, and component level) is required to provide a complete understanding of any *single* level of description.
- Mutability exists among levels. For instance, the component level defined here in terms of nonlinear oscillators may be viewed as a collective variable level for finer-grained descriptions such as the way agonist and antagonist muscles generate kinematic patterns.
- Patterns at all levels are governed by the dynamics of collective variables. In this sense, no single level is any more important or fundamental than any other.
- Boundary constraints, at least in complex biological systems, necessarily mean that the coordination dynamics are context or task dependent. I take this to be another major distinction between the usual conception of physical law (as purely syntactic, nonsemantic statements) and the self-organized, semantically meaningful laws of biological coordination. Order parameters and their dynamics are always *functionally* defined in biological systems. They therefore exist only as meaningful characteristic quantities, unique and specific to tasks.

Reprise

I have demonstrated that simple behavioral patterns and considerable pattern complexity may arise from the process of self-organization, as emergent con-

Phenomena	**Pattern Dynamics**
Boundary constraints	
Initial conditions: e.g., oscillate fingers in a given fashion...	$\phi = 0$ *or* $\pm\pi$
Non-specific parameters: e.g., change frequency, spatial orientation...	b/a
⇕	⇓
Collective variable	
characterizes coordinated states, e.g., relative phase (ϕ)	$V(\phi) = -a\cos(\phi) - b\cos(2\phi)$
⇕	⇑
Components	
nonlinearly coupled oscillators	$\ddot{x}_1 + f(x_1, \dot{x}_1) = G(x_1, \dot{x}_1; x_2, \dot{x}_2)$ $\ddot{x}_2 + f(x_2, \dot{x}_2) = G(x_2, \dot{x}_2; x_1, \dot{x}_1)$

Figure 2.14 The tripartite scheme applied to understanding coordination: the bimanual coordination example (see text for details).

sequences of nonlinear interactions among active components. Ironically, the very aspect of behavior that scientists, especially psychologists and biologists, usually try to avoid—instability of motion—turns out to be the key generic mechanism of self-organization. The very many neurons, muscles, and joints act together in such a way that the entire system acts as a single coherent unit.

The discovery and consequent analysis of phase transitions in human hand movements introduces a new paradigm into biology.[47] It appears also that a comparison of nonequilibrium phase transitions in physics and discontinuities in coordinated action goes beyond mere analogy.[48] Of course, in physics, phase transitions remain objects of concentrated research. But the basic events that we have found in voluntary human hand movements, critical fluctuations and critical slowing down, occur over and over again in nonequilibrium systems, and suggest that the same laws and principles are in operation. The step we have tried to take in this chapter, albeit a baby step, is from the identification and descriptive language of functional synergies in action, to synergetics, a theory of how synergies are created, sustained, and dissolved. This is the conceptual and methodological foundation on which I believe a scientific psychology should be built, a science that bridges mental, brain, and behavioral events.