PSYCHONOMIC SOCIETY KEYNOTE ADDRESS

How a cognitive psychologist came to seek universal laws

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My early fascination with geometry and physics and, later, with perception and imagination inspired a hope that fundamental phenomena of psychology, like those of physics, might approximate universal laws. Ensuing research led me to the following candidates, formulated in terms of distances along shortest paths in abstract representational spaces: Generalization probability decreases exponentially and discrimination time reciprocally with distance. Time to determine the identity of shapes and, provisionally, relation between musical tones or keys increases linearly with distance. Invariance of the laws is achieved by constructing the representational spaces from psychological rather than physical data (using multidimensional scaling) and from considerations of geometry, group theory, and symmetry. Universality of the laws is suggested by their behavioral approximation in cognitively advanced species and by theoretical considerations of optimality. Just possibly, not only physics but also psychology can aspire to laws that ultimately reflect mathematical constraints, such as those of group theory and symmetry, and, so, are both universal and nonarbitrary.

The object of all science, whether natural science or psychology, is to coordinate our experiences and to bring them into a logical system.

(Einstein, 1923a, p. 2)

[But] the initial hypotheses become steadily more abstract and remote from experience.

(Einstein, 1934/1954, p. 282)

To the extent that my particular approach to psychological science may be distinctive, I think it may be so in

three principal respects. The first is my predilection for applying mathematical ideas that are often of a more geometrical or spatial character than are the probabilistic or statistical concepts that have generally been deemed most appropriate for the behavioral and social sciences. The second is my unwillingness to be satisfied with any proposed psychological principle whose sole justification is that it fits all the available empirical evidence—whether behavioral or neurophysiological. I crave, in addition, a reason that that behavioral principle (or that associated neural structure) should have the particular form that it does, rather than some other. The third is my belief that if, as I fervently hope, psychological principles are not merely arbitrary, some may be shown to have arisen as accommodations to universal features of the world. If this is so, we might aspire to a science of the mind that, like the physical and mathematical sciences, has universal laws.

The possibility of a psychological science of the kind I envision may be viewed with skepticism, if not, indeed, incredulity. Those outside the field, on hearing the term *mathematical psychology*, often react with the raised eyebrows reserved for oxymora. Some physical scientists subscribe to opinions like that of an astrophysicist member of the National Academy of Sciences whom I heard declare that whereas the physical sciences deal with quantitative measurements and objective facts, the social sciences are limited merely to “expressions of subjective feelings.” Even quantitatively oriented psychologists may assume that because human behavior is inherently less predictable than the behavior of deterministic physical systems, the kinds of mathematics applicable to psy-

I am honored to follow William K. Estes, who gave the first of these newly instituted keynote addresses at the 2002 meeting of the Psychonomic Society (see Estes, 2002). The mathematical theory of learning that Estes put forward at the middle of the last century was a significant inspiration for my own ensuing attempts to construct mathematical theories for fundamental psychological phenomena. This article is based on the keynote address I presented at the November, 2002 meeting of the Psychonomic Society in Kansas City. It is expanded here to include material on discrimination time, music cognition, and how capabilities for mental transformation may make possible the discovery of physical laws—topics that I did not have time to cover in that address but that (in preparing this written version) seemed to help to complete the picture of my approach to psychology. I thank the Governing Board of the Psychonomic Society and its Chair, Stephen Palmer, for inviting me to present the address, David Balota for guidance in my preparation of the manuscript, and William Estes for his many thoughtful suggestions for improving the manuscript. Finally, I gratefully acknowledge the National Science Foundation's support of the research that my co-workers and I have pursued on mental representations and psychological laws during the 35 years following my move to academia in 1966. The opinions and conclusions expressed in this article are my own, however, and do not necessarily represent those of the National Science Foundation (or, indeed, of any of my former co-workers). Correspondence concerning this article should be addressed to R. N. Shepard, 13805 E. Langtry Lane, Tucson, AZ 85747-9637 (e-mail: rshepard@cox.net).

chological phenomena are, at best, probabilistic or statistical.

All such views—perpetuated by a misconceived dichotomy between the so-called “exact” and “inexact” sciences—are mistaken. Human beings and all other animals are themselves physical systems and, as such, operate in compliance with physical laws. Physical systems do, of course, differ widely in the predictability of their behaviors. But these differences in predictability are not determined by whether the behaviors are those investigated by physicists or by psychologists. Rather, they are determined by the precision with which the would-be predictor knows the values and the principles of interaction of the controlling variables (and by the sensitivity of those variables to initial conditions).

Even for a physicist, the future positions of a leaf dropping from a tree on a gusty autumn day are quite unpredictable in the absence of precise knowledge of the shifting and turbulent air currents and of the complex shape of the particular leaf with which those currents interact. But the future positions of a heavy ball released from the top of an evacuated cylinder are highly predictable because, here, the single controlling force of gravity is constant and precisely known.

Likewise, for a psychologist, the future path of the ball that has just arrived in a basketball player's hands is quite unpredictable. That player is highly trained and motivated to keep members of the opposing team guessing as to whether the ball will go left to one teammate, right to another teammate, up toward the basket, or down, to be dribbled forward, backward, left, or right. As a consequence, the processes within that player's nervous system—although deterministic—may be so delicately poised as to be tipped toward the precipitation of a particular one of these macroscopic actions by micromechanical (perhaps chaotic or thermal) events inaccessible to all external observers and even to the player about to take the action.

But, when that player, having been fouled, stands at the free-throw line, the future path of the ball is highly predictable. The trajectory is then governed primarily by the goal of launching the ball on that precise ballistic trajectory—out of all possible trajectories within the basketball pavilion—that will carry it through the small and distant hoop. Moreover, for modeling the player's hand-eye coordination performance, a statistical calculus may be less appropriate than a more deterministic and spatial, differential calculus such as that governing the guidance of missiles or, most simply, the descent of the heavy ball in the evacuated cylinder.

Of course, although humans and other animals are, at one level of description, physical systems, the behavioral or mental principles of interest to the psychologist emerge at a quite different level of description. Of late, psychology and cognitive neuroscience have been dominated by a preoccupation with brain imaging studies. Moreover, since long before, scientists from other fields and non-scientists alike have widely assumed that an objective understanding of the mind can be gained only through a

study of the physical brain. But none of the empirical data I shall cite and none of the mental laws I shall propose arose from or depend on any knowledge about the brain. The history of the relations between the psychological sciences and the neurosciences indicates, rather, that a recognition of the problems that a behaving organism confronts in the world is needed in order to know what to look for in the brain.

I have chosen three general lines of my own work—namely, those on mental transformations, on generalization and discrimination, and on music cognition—to illustrate how abstract spatial conceptualizations of problems posed by the world have led me to candidates for universal mental structures and laws. From there, I shall venture some implications for an understanding of how scientific laws are discovered in general, and for the prospects for a science of universal mental laws in particular. But, first, it may help to review the circuitous odyssey through many quite different fields that led me to my peculiarly geometrical approach.

Childhood and Early School Years¹

Beginning in early childhood, I would spend many solitary hours happily tinkering with old clockworks, telephones, and parts of other appliances that I found in a vacant-lot junk pile or in the attic of my San Jose grandparents' barn and, also, drawing and designing things geometrical and mechanical. These interests continued into my high school years, when I was building robots and other electromechanical devices and constructing models of regular polyhedra and projections of the four-dimensional hypercube. At the same time, I was discovering and devouring books from the library by Sir Arthur Eddington, Sir James Jeans, Eric Temple Bell, and others, on relativity, cosmology, and mathematics. But my lack of attention to school subjects was a concern of my parents, teachers, and school counselors, from my having to repeat kindergarten all the way up through my ignominious freshman year at Stanford.

Undergraduate Years at Stanford

At the end of my second quarter at Stanford, my cumulative grade point average had fallen to within one point of precipitating my permanent dismissal. During my ensuing two-quarter obligatory leave, I worked at the Stanford Research Institute (now SRI International) as a chemistry lab assistant to Dr. Elizabeth Roboz (who was later to become Einstein's daughter-in-law). Several other scientists at SRI, taking a special interest in me, persuaded me that if I wanted to do creative work in science, I should complete my formal schooling. So I did return to Stanford. But I also continued my independent geometrical explorations. (These now concerned the local connection statistics of homogeneous topological networks that rendered them the discrete analogues of continuous two- or three-dimensional spaces with global curvatures that are zero, like the plane; positive, like the surface of a sphere; or negative, like a saddle shape.)

I was strongly attracted by the four-dimensional non-Euclidean Minkowskian and curved-space (Riemannian) geometries of special and general relativity. But I feared that my failure to have obtained a grounding in the full spectrum of relevant mathematical fields precluded a successful career in mathematics or theoretical physics. I was also deterred by the thought that those disciplines were unlikely to provide a window on the inner phenomena of perception, imagery, and dreams—phenomena that figured prominently in my own creative thinking, and that were coming to fascinate me as much as the external phenomena of physics.²

At different times during my ensuing undergraduate years, I entertained the possibility of devoting myself to a career in each of many different fields—including, in addition to math and physics, engineering, biology, art, music, creative writing and, especially, toward the end, philosophy. But I was feeling an increasing need to settle on a field in which the mental phenomena that now so intrigued me might be conceptualized in terms of mathematics—or even, I hoped, geometry (which had been characterized as “the most ancient branch of physics” by Einstein, 1923b, p. 28, and as its “noblest branch” by Harvard mathematician W. F. Osgood—see Taylor, 1984, p. 607).

One day, my father, sensing my growing angst, surprised me by asking whether I had considered psychology. This was a field that could hardly be further from his own fields—originally of mining engineering and now of metallurgy. Nevertheless, he observed that psychology would seem to be the field that most explicitly offered a scientific approach to the mental. I was struck by the thought that the only son of the metallurgist might become—as indeed I have (despite an ensuing period of behaviorist indoctrination)—a *mentalurgist*! I did then register for Psych 1. But, as I feared, I found little in this course that appeared susceptible to the kinds of theories that I so admired in physics and, certainly, nothing that looked geometrical. Still, in desperation, I obtained permission to register, as the only undergraduate, in the graduate-level course Sensation and Perception. Although offered by Donald Taylor, the same professor who had taught the disappointing Psych 1, this course did turn out to be closer to my interests in perceptual phenomena and also, it seemed, more susceptible to mathematical formulation.

At the end of one class, Taylor mentioned an as yet unpublished Stanford doctoral dissertation. In it, Fred Attneave, using stimuli that varied on perceptually distinct dimensions (such as size, shape, and color), found evidence that both subjective judgments of the similarities among the stimuli and objective frequencies of their actual confusions during paired-associates learning were inconsistent with a Euclidean representation. Evidently, the “psychological distance” between stimuli did not approximate the square root of the sum of the squares of their separations on the component perceptual dimensions dictated by the Pythagorean theorem for Euclidean right triangles. Instead, the data implicated a non-Euclidean metric in which the psychological distance approximated

the simple sum of those component separations (Attneave, 1950). New vistas opened before me, and I proceeded to apply for graduate study in psychology.

Early Graduate School Years at Yale

I accepted an assistantship at Yale, largely on the recommendation of Taylor (who would much later move to Yale himself). But, on my arrival there in the fall of 1951, I found that in my assistantship, I was to condition rats to begin running in a wheel within 5.8 sec of the onset of a light to avoid an electric shock so intense that (to my own great distress) it caused the rats to jump, squeal, and, often, to urinate or defecate. I was to record the latency at which each rat began running following the onset of the light. After 60 trials in which a rat successfully avoided the shock, I was to disconnect the shock and record the latency of running during the next 30 extinction trials.

The experiment had been designed to test a prediction of Yale’s renowned stimulus–response (S–R) learning theorist Clark L. Hull, then a frail, bent, and reclusive presence in his basement lab and, as it turned out, in the final year of his life. According to Hull’s theory, the latency of such a conditioned response should gradually increase when, with the absence of the punishing shock, the accumulated habit strength of the response drained away like fluid from a leaking drum. Contrary to this prediction, however, I noticed that any one rat’s latencies of wheel running did not increase in the absence of shock. Instead, each rat continued responding to the light with undiminished alacrity until, on some unpredictable trial, it simply refrained from running. (Because different rats desisted after different numbers of shock-free trials, a plot showing a gradual increase in median latency for the whole group of rats would not be representative of the behavior of any one rat.)

To provide a quantitative representation typical of the behavior of individual rats, I took it upon myself to plot the mean latencies of the wheel-running response on shock-free trials as a function of number of trials since the last shock. The result is shown in Figure 1A. For each rat, these trials were the equivalent of extinction trials in that no shock was received. Because longer runs of shock-avoiding trials were rarer than shorter runs, sample sizes decreased with length of run of shock-avoiding trials and (as can be seen) variability of latency increased. Nevertheless (as can also be seen), there was no systematic increase in latency of response.

When a rat that had thus been responding with short latencies finally did delay its response, it did so discontinuously, refraining from running at all until the shock at 5.8 sec caused it immediately to resume running to terminate the shock. The resumption of running following a shock and the cessation of running that brought on the next shock were equally discontinuous. I was able to show this in the single plot shown in Figure 1B by linearly stretching or shrinking (for each rat) each inter-shock interval so that it fit between the same plotted

Figure 1. Rats' mean latencies of wheel running following the onset of the conditioned stimulus plotted (A) as a function of successive trials after the last shock for those responses that occurred within 5.8 sec and, thus, avoided the shock, and (B) within successive sevenths of the intervals between shocks. From "A Funny Thing Happened on the Way to the Formulation: How I Came to Frame Mental Laws in Abstract Spaces," by R. N. Shepard, 2003. In R. J. Sternberg (Ed.), *Psychologists Defying the Crowd*, pp. 215–237. Washington, DC: American Psychological Association. Copyright 2003 by the American Psychological Association. (Adapted with permission.)

shock trial endpoints. Within each of seven equally spaced time slots between these endpoints, I plotted the mean of the latencies of all the responses within that time slot. With the resulting larger sample sizes, the mean within-slot latencies were all very stable—and absolutely flat throughout the shock-free interval, as can be seen in the figure.

It did not look to me as though habit strength gradually decreased in the absence of the shock. Rather, it looked as though the rats were doing just what I would have done in their unhappy situation. It was as if the rats had formed the *hypothesis* that the light signaled the imminence of another shock that could only be prevented by immediately running in the wheel. But, after continuing to run without any recurrence of the shock, a rat would eventually risk a test of its hypothesis about the connection between light and shock by not running. If the shock occurred, the rat would immediately resume running to terminate the shock. But if the shock did not occur, the exhausted rat, welcoming the apparent break in the connection between light and shock, would refrain from further running. I also plotted median latencies for shock-free trials just during extinction—but backward in time, beginning with the last trial on which each rat responded. The median response latencies were again flat, manifesting no systematic increase until each rat was no longer responding.

Although intrigued by this suggestion of a cognitive process of hypothesis testing in the lowly rat, I could not take the results of my analyses to the reclusive Hull himself. His longtime secretary, Ruth Hays, had allowed

those of us in Neal Miller's learning seminar to read the manuscript of Hull's about-to-be-published final book, *A Behavior System* (Hull, 1952). But she was adamant that absolutely no problem concerning Hull's theory be brought to his attention lest the shock precipitate in the failing Hull a final cardiac arrest.

I later learned that nearly 20 years earlier, Isadore Krechevsky (who later shortened his name to Krech) had published evidence of "hypotheses" in rats—albeit during discrimination learning (Krechevsky, 1932a, 1932b). Yet, the two animal learning researchers on the Yale faculty to whom I did show the results of my analyses found my results incomprehensible and suggested that my outcome-contingent ways of averaging the data were suspect. I filed my plots away and never ventured to publish them until over 50 years later (Shepard, 2003). I also declined, from that time on, to undertake any further experiments on subjects unable to provide informed consent.

A decade later, such discontinuities in learning performance were being confirmed—by means of just such outcome-contingent averaging of data—by Bower and Trabasso (1963, 1964), Levine (1966), Levine, Miller, and Steinmeyer (1967), Restle (1962), and others, including, in a somewhat different way, myself (Shepard, 1963, 1966a). These experiments were conducted on humans rather than on rats, however, and most of them were based on the new, mathematically elegant hypothesis and one-element stimulus-sampling models. This striking new evidence both benefited from and contributed to the then gathering shift from the more "thoughtless" or "hy-

draulic" explanations of behavior to theories of the internal processing of information.

Later Graduate School Years at Yale

After leaving the rat lab, I asked Carl Hovland to be my dissertation advisor. Although Hovland had done his own doctoral research under Hull, he was the one senior member of Yale's experimental psychology faculty who worked primarily with human subjects and who seemed open to theoretical ideas that differed from the S-R behaviorist approach then prevailing at Yale. Indeed, anticipating the soon-to-flourish cognitive revolution, Hovland was already exploring an information-theoretic approach to human concept learning (Hovland, 1952).³

While at Yale, I received inspiration from two young visiting colloquium speakers. One was William Estes, whose elegant stimulus-sampling theory of learning (Estes, 1950) came as a breath of fresh air. In addition to laying the groundwork for the one-element models of discontinuous learning just referred to, Estes showed how empirically testable functional relations could be derived, mathematically, from hypothetical but simple and well motivated (if abstractly formulated) elementary internal processes. This was a notable departure from Hull's practice of postulating such functional relations after empirically fitting curves of theoretically unmotivated functional form to animal data. Yet, when I confessed to Estes that I, too, aspired to a mathematical approach, he counseled me that from his experience, psychologists were not yet ready for mathematically formulated theories. Estes's own dissertation advisor, B. F. Skinner, in commenting on Hull's theory building, had earlier written "A science of behavior," because its problems are not of the same sort, "cannot be closely patterned after geometry or Newtonian mechanics" (Skinner, 1938, p. 437). Much later, when Skinner and I were both at Harvard, he liked to refer to mathematical models as "paper dolls." I am grateful that neither Estes nor I heeded Estes's own warning about the field's lack of receptivity to mathematical formulations.⁴

The other inspirational colloquium speaker was George A. Miller. He presented his information-theoretic analyses of the extensive data that he and Patricia Nicely had collected on the rates at which listeners confused 16 consonant phonemes with each other under 17 conditions of noise and filtering (Miller & Nicely, 1955). The challenge of trying to discover the dimensions of perceptual processing implied by this extraordinarily rich set of data strengthened my resolve—originally stimulated by the dissertation of Attneave (1950)—to find a way of extracting spatial representations of stimuli from similarity, confusion, and generalization data. Moreover, when I and then my Bell Labs associates later succeeded in developing quite general computer methods of doing this by multidimensional scaling, tree-fitting, and clustering, the data of Miller and Nicely proved to be the single most useful set of test data (Shepard, 1972, 1980, 1988a).

I was now convinced that the problem of generalization was the most fundamental problem confronting learning theory. Because we never encounter exactly the same total situation twice, no theory of learning can be complete without a law governing how what is learned in one situation generalizes to another. Hull, recognizing the need for a law of generalization, had earlier persuaded Hovland to determine the form of the function relating generalization to stimulus difference in his 1936 doctoral dissertation. Hovland attempted this by measuring humans' generalized galvanic skin responses (GSRs) to tones differing in frequency from a training tone associated with (mild) electric shock (Hovland, 1937). The averaged GSR strengths fell off with difference in frequency of tone in an apparently concave-upward manner suggestive of an exponential decay function. Accordingly, Hull then added an exponential-decay generalization function as Postulate 5 in his *Principles of Behavior* (Hull, 1943). But GSR data are notoriously variable, and Spence (1937), who had his own theoretical reasons for preferring a convex-upward function, argued that Hovland's (1937) data were too noisy to conclude whether the generalization function was concave or convex, let alone specifically exponential.

For me, there was a more fundamental problem. Even if stable measures of generalization could be found, generalization functions could not in general be monotonic, let alone invariant, when plotted on a physical dimension. There is greater generalization (1) between tones separated in frequency by an octave than between tones separated by somewhat less than an octave, (2) between spectral hues of the shortest and longest visible wavelengths (violet and red, respectively) than between either of these and a hue of an intermediate wavelength (e.g., green), and (3) between the vertical and horizontal orientations of a rectangle than between these and a 45° oblique orientation (Shepard, 1962b, 1965, 1987).

In my 1955 doctoral dissertation, with the supportive encouragement and suggestions of two then Yale assistant professors, Robert Abelson and Burton Rosner, I developed a method for recovering a spatial representation of stimuli from generalization data themselves, without reference to any physical dimensions. Provisionally assuming that the generalization function might be exponential, I applied the inverse, logarithmic transformation to generalization errors recorded during paired-associates learning (which proved to be much more stable than GSR data). I then showed that a type of multidimensional scaling could be applied to these transformed data to yield a spatial representation of the stimuli (and also of the responses). The goodness of fit of the original data to an exponential function of the distances in the recovered spatial representation yielded the first reliable evidence that generalization does in fact fall off exponentially with psychological distance (Shepard, 1955, 1957, 1958a, 1958b).

Although I passed the oral examination on my dissertation, I subsequently learned that one of my examiners, Fred Sheffield (another Yale animal learning researcher),

had expressed the view that “this sort of thing should not be encouraged.” I had previously heard Sheffield opine that psychology was advanced not by constructing mathematical theories but by designing experiments in such a way that the final report could be written in advance of collecting the data, leaving only spaces to fill in the appropriate statistics and associated *p* values. I could only wonder how Einstein would have arrived at the theory of relativity by following such advice.

Years at the Bell Telephone Laboratories

In 1958, after three postdoctoral associateship years (one at the Naval Research Laboratory and two with George Miller at Harvard), I served for eight years on the technical staff of the Bell Laboratories in Murray Hill, New Jersey. Inspired, in part, by a summer workshop conducted by Allen Newell and Herbert Simon on their new computer simulation approach to cognitive processes, I planned to use Bell Labs’ computer facilities to simulate the evolutionary formation of perceptual–cognitive mechanisms. But then the executive director of the large division that included my department, the eminent engineer John R. Pierce, invited me to have lunch with him. No sooner had I mentioned the emerging field of artificial intelligence than Pierce, known for his sharp mind and outspoken opinions, rocked back in his chair and exclaimed, “Ah yes, AI. There’s no holding it up; it keeps hitting new lows!”

Because this was my first real job and I now had a family to support, I decided to use Bell Labs’ computer facilities to work, instead, toward new ways to extract spatial representations from similarity and generalization data—ways that were not possible with the mechanical desk calculators that had been available back at Yale. This led to the development of the methods now known as *nonmetric multidimensional scaling*—first by me (Shepard, 1962a, 1962b) and then, with improvements, by my Bell Labs mathematical colleague Joseph Kruskal (1964a, 1964b). Using only qualitative (i.e., ordinal) properties of the data, these methods yield quantitative (fully metric) spatial representations (Shepard, 1966b). Thus did I attempt to achieve in psychology what had been characterized as “the very essence of science from the time of Anaximenes to the present day”—namely, “quality . . . reduced to quantity” (Sambursky, 1956, p. 11). Moreover, these new methods provided for solutions in non-Euclidean as well as in Euclidean spaces (Cunningham, & Shepard, 1974; Kruskal, 1964a, 1964b; Shepard, 1980, 1991; Shepard & Carroll, 1966). These and related data-analytic methods—including *additive clustering* (Shepard & Arabie, 1979) and, especially, the INDSCAL type of multidimensional scaling developed by my Bell Labs colleagues Carroll and Chang (1970)—have proved to be useful in the behavioral, social, cognitive, and neurosciences. They have also been essential in my own demonstrations of what I termed the *second-order isomorphism* of perception and imagery (see, e.g., Shepard & Chipman, 1970; Shepard & Cooper, 1992;

Shepard, Kilpatrick, & Cunningham, 1975) and in the research I shall review here on mental transformations, generalization and discrimination, and music cognition.

Still, a nagging feeling that none of the basic research I was doing at the Bell Labs would ever earn or save money for the Bell System goaded me into undertaking one experiment of more immediate practical relevance. My research assistant and I had people look up and dial seven-digit phone numbers in either of two ways. One was the usual way; the other was a reversed order in which the less familiar four-digit line number appeared to the left and was to be dialed before (rather than after) the generally more familiar and memorable three-digit central office code. We found that this reversal of order cut both the lookup-plus-dialing time and the frequency of wrong numbers approximately by half (Shepard & Sheenan, 1965). Such a change could have eased the life of the customers and saved the Bell System enormous amounts of money. But when I circulated a draft of our report to the applied and engineering divisions, the feedback I received was to go back to my “ivory tower.” The Bell system was too big to make such a change, I was told. So, I returned to my basic research and, from then on, accepted my paychecks without guilt.

A Shift to Academia and to a Bolder Vision for Psychological Science

It was during my ensuing faculty years that I began to devote sustained thought to the possibility that mental principles may have arisen as accommodations to universal physical or abstract mathematical features of the world. I had admired the elegant axiomatic approach to the psychological taken by R. Duncan Luce (1959a), including, in particular, his mathematical analysis of “the possible psychophysical laws” (Luce, 1959b). Now, during a short (1966–1968) stint on the Harvard faculty, at the other end of the hall from the single-minded psychophysicist S. Smith Stevens, I felt a need to achieve clarity for myself about whether a particular form of the psychophysical function might be advantageous for individuals behaving in the world (see Shepard, 1978b, 1981b). My mission gradually evolved into its present, broader form during my 34-year tenure at Stanford—principally through new investigations into mental transformations and, once again, into my thesis topic of generalization. I came to suspect that a nonarbitrary basis for universal mental principles had been underestimated for two reasons.

First, the relevant features of the world, being both universal and abstract, leave no contrasting background from which they might stand out to catch our attention. For example, all of our collective evolutionary history and individual learning experiences have occurred in a three-dimensional Euclidean space. Much as fish that have never inhabited a nonaquatic medium, neither we nor our ancestors have lived in a four-dimensional space. Some humans know that four-dimensional Euclidean space affords the possibilities of transformations not available in

our familiar three-dimensional space—such as rigid rotation (about a plane!) of a left-hand glove into a right-hand glove—but, presumably, through abstract mathematical reasoning rather than through direct experience-based intuition.

Second, any accommodating mental structures are likely to have become so deeply internalized, implicit, and automatic in their operation as to be largely inaccessible to our conscious awareness. Two early examples struck me as illustrations of the power of the spatial intuitions with which we are unknowingly endowed. In a talk at Newell and Simon's computer simulation workshop in 1958, Marvin Minsky remarked that in comparison with symbolic logic, the proof trees for plane geometry are so ramified that it is only by virtue of our spatial intuitions that we are able to prove even elementary theorems of Euclidean geometry. Similarly, Donald Michie, while visiting me at Bell Labs, described a game in which two players alternately draw single counters from a set of nine bearing the numbers 1 through 9. The first player to obtain three counters whose numbers sum to 15 wins. People are very slow to master this game. Yet, it is isomorphic to the trivial but spatially presented game of tic-tac-toe. This can be seen from the existence of a 3×3 magic square with the numbers 1 through 9 assigned to the nine cells in such a way that (for example) the top, middle, and bottom rows contain 8-1-6, 3-5-7, and 4-9-2, in that order. In this square, the three numbers in each row, each column, and each diagonal (and only these) sum to 15.

But, if the constraints of the particular space in which we have lived have always been externally available to us, what advantage would we have gained by internalizing those constraints? This question was raised most recently and insistently in the articles and commentaries on my work in a special issue of *Behavioral and Brain Sciences* (2001) by Barlow (pp. 602–607), Kubovy and Epstein (pp. 618–625), Schwartz (pp. 626–628), Todorović (pp. 641–651), Bertamini (pp. 655–657), and Jacobs, Runeson, and Andersson (pp. 679–680). I responded with both empirical findings and theoretical arguments (see Shepard, 2001, pp. 712–729, 736–742, particularly pp. 714–715, 723–724). Perhaps most relevant here is the adaptive value conferred by being able to simulate, mentally, the simplest and, hence, swiftest structure-preserving transformation that will carry one represented object into congruence with another in the absence of any external support for the particular path taken by that transformation. Thus, the just considered game of tic-tac-toe is child's play because the number of alternatives to be considered is radically reduced by the implicitly recognized symmetry in which any patterns of Xs and Os that can be transformed into each other by rotations or reflections of the square are equivalent.

Investigations of Mental Transformations

My investigations of mental transformations had a sudden beginning shortly after I arrived at Stanford. As

I was drifting toward wakefulness early in the morning of November 16, 1968, I experienced a spontaneous hypnopompic image of three-dimensional objects majestically turning in space. Even before rising from bed, I had mentally worked out the design of the first experiment (Shepard & Metzler, 1971) in what was to become a long-continuing series, first on mental rotation and then on related phenomena of apparent motion (Shepard, 1984; Shepard & Cooper, 1982). I now describe the two cases of apparent motion and mental rotation and, then, the third case of still more freely imagined spatial transformations. This is their order of increasing independence from external support. I believe this also to be the order of their evolutionary emergence. Mechanisms that originally evolved in the service of perception may have gradually been liberated to aid in the anticipation of future states of the world and, then, in the planning of actions likely to increase the probability of favorable future states.

Apparent motion is the most automatic and perceptual of these three cases. In it, the display of an object in one position is suddenly replaced by a display of an object of the same shape but in a different position in space. The resulting visual experience is that a single enduring object has suddenly moved from one position to the other and has done so over a definite connecting path. Under appropriately general conditions, the path over which the motion is experienced is the most direct and simplest one, as specified by the kinematic geometry of three-dimensional Euclidean space and the symmetry group of the object (Carlton & Shepard, 1990a, 1990b; Foster, 1975; McBeath & Shepard, 1989). If the two displayed positions differ in orientation, this kinematically simplest path is generally curved. In agreement with the experienced continuity of the motion, when the two positions are displayed in ongoing alternation, the minimum duration of the display of each position yielding experienced preservation of object structure increases linearly with the length of the path over which the motion is experienced (Farrell & Shepard, 1981; Shepard & Judd, 1976; Shepard & Zare, 1983). Moreover, the same time-distance relation evidently holds in other modalities, such as the tactile and the auditory, and, in the latter case, on the dimensions of both physical location and pitch (e.g., Jones, 1976; Lakatos & Shepard, 1997a, 1997b; Shepard, 1981c, pp. 318–320).

Mental rotation represents a more voluntary and cognitively effortful case. In it, the time to determine that two displayed objects are of the same shape similarly increases linearly with the extent of the kinematically simplest rigid motion required to bring one object into the orientation of the other in three-dimensional space. Here, too, this simplest motion is affected by any symmetries of the object. Despite the absence of external support, the object is sequentially represented in positions corresponding to successive points along this particular simplest connecting path in the abstract space of possible positions. This is demonstrated by how the

speed of the (accurate) discriminative response to a visual probe presented during the mental operation systematically varies as a function of the time and relative orientation of the presented probe.

Much of the work on mental rotation may be familiar to most cognitive psychologists, and the first decade of that work (and the related work on apparent motion) was covered in the book *Mental Images and Their Transformations* (Shepard & Cooper, 1982). Still, as an experiment providing particularly clear evidence that the internal process represents the structure of a complex object in successively more rotated orientations—and does so during the absence of any corresponding external stimulus—I here describe the results of the elegant and quantitatively precise experiment by my former research collaborator Lynn Cooper (1976).⁵

The participants in this experiment had already served in other experiments of Cooper and were highly familiar with the eight random polygons that she had originally constructed for her dissertation experiments. As a result, each participant's speed of mental rotation had already been determined for these stimuli. At the beginning of each trial, one of these familiar random polygons was displayed to the participant in its standard, original train-

ing orientation, centered within a circular field (as illustrated in Figure 2B). Immediately upon the replacement of this polygon by a blank field, the subject was to imagine the just presented shape rotating in a clockwise direction within that circular field. Then, at some unpredictable time, a test probe consisting of either the same standard polygon or its mirror image was presented within the same circular field at an unpredictable orientation. The participant's task was to push a right- or a left-hand button as quickly as possible to indicate whether the test probe was the standard or the reflected version of the polygon, respectively, regardless of its presented orientation.

The results are summarized in Figure 2. When the probe was presented at an orientation in which each participant should (according to that participant's previously determined rate of mental rotation) be representing that particular polygon at the time of presentation of the test probe, responses were uniformly accurate and fast. As is shown in Figure 2A, these averaged just over half a second, regardless of the absolute orientation of that test figure in the circular field. When the probe was presented in an orientation that differed from the orientation in which a participant was expected to be representing the polygon at the time of test, however, discriminative

Figure 2. Mean time during mental rotation of a random polygon to classify a test probe as the standard or a reflected version of that polygon, plotted (A) as a function of the absolute angular orientation of the probe, when it was presented in the orientation through which the subject was predicted to be mentally passing at the time of test, and (B) as a function of the angular departure of that probe from the orientation through which the subject was predicted to be mentally passing at the time of test. From "Demonstration of a Mental Analog of an External Rotation," by L. A. Cooper, 1976, *Perception & Psychophysics*, 19, 296–302. Copyright 1976 by the Psychonomic Society. (Adapted with permission.)

reaction time increased linearly with the angle of such departure. As can be seen in Figure 2B, this time, which was a little over 500 msec for 0° departure, reached about 800 msec for the maximum possible departure of 180° . It was as if the participant were mentally representing the polygon in orientations successively more rotated in a clockwise direction. Then, when the probe figure appeared in some other orientation, the mental rotation had to continue on—or back up—to the orientation of that externally presented test stimulus in order for the participant to determine whether the test figure matched the internally represented polygon or was its reflected version.

The leftmost point, for the case of a 0° departure from the expected orientation in Figure 2B, is simply the average of all the points in Figure 2A. The open circles among those points are for intermediate test orientations that differed by multiples of 30° from any orientations in which these polygons had previously been presented to these participants. Nevertheless, relative to the virtually horizontal line fitted to the points in Figure 2A, the reaction times for these novel test orientations were no greater than those for the probes in the previously experienced orientations (which had been confined to 60° steps around the circle). Such results provide support for our claim that mental rotation is an *analogue process* in which something is literally rotating. But what is rotating is very abstract. It is, specifically, the orientation in the external space of the blank circular field in which a test stimulus—if it were to be presented—would yield a fast and accurate discriminative response. I would hope that the astrophysicist whom I quoted near the beginning of this article would allow that these data are not mere nonobjective and nonquantitative “expressions of subjective feelings.”

Freely imagined spatial transformations, as in creative thinking and problem solving, generally go on with still less external constraint or support. While lying in bed with eyes closed, one might imagine how a large table could be maneuvered through a narrow doorway. Accounts of many mathematicians, scientists, engineers, inventors, architects, and sculptors have remarked on the importance of spatial visualization in their creative work (Shepard, 1978a, pp. 134–159). Einstein, when asked about the mental processes that led to his revolutionary discoveries, spoke of “visualizing . . . effects, consequences, possibilities” (quoted in Wertheimer, 1945, p. 184) by means of “images which can be ‘voluntarily’ reproduced and combined” (quoted in Hadamard, 1945, p. 142). I suggest that these nonverbal processes are of essentially the same kind as the processes of mental rotation that my co-workers and I quantitatively investigated in an extremely purified form under the highly controlled conditions of the psychological laboratory.

Implicit representation of possible spatial transformations may play a significant role even in simple object perception, in which maximal external support might seem to be available. Any twisted and distorted shape will be per-

ceived as a cube, as long as its edges and corners (no matter how wildly curving and nonorthogonal) project, on the (stationary) observer’s retina, an image that is identical to that projected by a true cube. Followers of Egon Brunswik might argue that the normal cube, rather than any of these distorted variants, is experienced because straight lines and right-angled corners are statistically more common in our environment. I believe that the reason is deeper than this. I believe that it depends on our implicit appreciation of the consequences of potential spatial transformations. For, if the object were actually twisted and distorted in the way described, then the probability that a freely mobile observer would be so positioned that the distorted object yields exactly the same retinal projection as the true cube would be not just low, but zero. This account of the matter is, I suggest, more deterministic and Gibsonian than it is probabilistic and Brunswikian.

Implicit representation of possible spatial transformations may also play a significant role in our appreciation of symmetries in objects and visual patterns (Palmer, 1982, 1983; Shepard, 1981c). The arts and crafts of all cultures of the world favor symmetrical and repeating patterns, and we are exquisitely sensitive to small departures from symmetry. Now, symmetry is defined, mathematically, in terms of invariance of pattern under transformation. Thus, we may immediately, if unconsciously, “see” that a horizontally repeating pattern would be unchanged by certain translations, that a bilaterally symmetric shape would be unchanged by a reflection, and that a circle would be unchanged by any rotation. Indeed, the perception of shape, generally, may derive from an appreciation of the object’s degrees of approximation to self-similarity under all possible rigid transformations (Shepard, 1981c, pp. 315, 326; 1982b, p. 60; 1988b, p. 95), or even nonrigid transformations (Leyton, 1992). Furthermore, as I shall note later, our intuitions about symmetry may have played the crucial role in the thought experiments leading to some of the most important insights of physical science and moral philosophy.

Our implicit spatial competence may in some way also underlie various other cognitive functions or developments that might at first not seem at all spatial. Examples that I have at various times discussed include memory organization and retrieval (Shepard, 1966a, p. 203; 2001, p. 726), comparison and judgment processes (Shepard, 1981c, pp. 329–330; also see Dixon & Just, 1978), language origin, syntax, semantics, and metaphor (Shepard, 1975, pp. 113–116; 1982b, pp. 61–65), and something to which I shall return shortly—namely, music cognition (Shepard, 1982a).

Investigations of Generalization

In the second line of work I have chosen for illustration here, I returned to what had struck me decades earlier at Yale as the most fundamental of psychological laws—namely, a law of generalization. I now applied multidimensional scaling to generalization data from

Figure 3. Simple exponential-decay functions fitted to generalization data that are plotted against the interstimulus distances obtained by applying multidimensional scaling (MDS) to those data. (A) Humans' generalization data from the original study of Attneave (1950) for triangles differing in size and shape, with distances from the MDS solution obtained for these by Shepard (1958a). (B) Humans' generalization errors while learning nine responses to nine circles of different sizes (McGuire, 1954), with distances from the MDS solution obtained for these by Shepard (1962b). (C) Pigeons' generalization data obtained by Guttman and Kalish (1956) for colors of different wavelengths, with distances from the MDS solution obtained for these data by Shepard (1965). (D) Humans' similarity ratings obtained by Ekman (1954) for colors of different spectral hues, with distances from the MDS solution obtained for these data by Shepard (1962b). All four figures adapted from Shepard (1987), "Toward a Universal Law of Generalization for Psychological Science." *Science*, 237, 1317–1323. Copyright 1987 by the American Association for the Advancement of Science. (Adapted with permission.)

many experiments on both human and animal learning in which a great variety of visual and auditory stimuli were used. I found that the probability with which a response that had been learned to one stimulus was made to another stimulus uniformly fell off in approximation to an exponential decay function of distance between those stimuli in the recovered representational space.

Here, I show 4 of the 12 examples presented in Shepard (1987). Figures 3A and 3B, respectively, are the results of my analysis (Shepard, 1958a, Figure 2) of data from Attneave's (1950) original study and the results of my

analysis (Shepard, 1962b, Figure 10) of the generalization errors made by humans while they learned a different response to each of nine circles of different sizes in an experiment by McGuire (1954—see Shepard, 1958b, Table 1). Figure 3C comes from my analysis (Shepard, 1965, Figure 2) of data collected by Guttman and Kalish (1956) on pigeons' generalization of a pecking response, originally learned (by each pigeon) to a particular wavelength of light, when later tested with other wavelengths. Figure 3D comes from my analysis (Shepard, 1962b, Figure 14) of data collected by Ekman (1954) on hu-

mans' judgments of similarities between colors that also differed on the hue continuum. I justify inclusion of data of the latter type on the basis of my assumption that experienced similarity and behavioral probability of generalization arise from the same distance-based representation of similarity. As can be seen, the data are approximated (especially for the human data, in panels A, B, and D) by the simple exponential decay function of distance in representational space. This function is forced to pass through the generalization value 1 at zero distance, and fitted by adjustment of a single slope parameter, which is expected to depend on the particular animal and set of stimuli tested.

In order to put the exponential function forward as a *universal* law, however, I needed to identify something about the world in which any behaving organism might evolve that would favor the emergence of a law of just this form. There is one fact about the world that seemed most relevant and universal. Biologically significant objects belong to distinct basic kinds—each with its own consequences for an organism. I argued that generalization is not merely a failure of discrimination, but a cognitive act of inductive inference. An individual who has found a particular object to have a significant consequence and who now encounters a new object that is discriminably different from that earlier, consequential object needs an estimate of the probability that this new object has the potential for the same consequence. On essentially Bayesian grounds, I argued that the estimation of this probability requires a summation over all hypotheses about which objects might be of the kind having that consequence or, in the case of a continuous space of possible objects, it requires an integration over all such hypotheses. For convenience I refer here to such computations as Bayesian summations and integrations.

Following my geometrical inclination, I supposed that a consequential subset would correspond, in the abstract representational space of objects, to a suitable (in the continuous case, connected) *consequential region*. Intuitively, it seemed clear that points representing more similar objects and, hence, lying closer together in the representational space would jointly fall within a greater number of the potentially consequential regions. I found that the Bayesian integration with priors corresponding to minimum knowledge (i.e., to maximum entropy) does indeed yield specifically the exponential decay function (Shepard, 1987). This result was soon extended, by Stuart Russell, then at Stanford, to stimuli that differed in the presence or absence of discrete features. Russell showed that for such stimuli, summation over candidate subsets yields a function that falls off exponentially with a distance between stimuli, defined in terms of the proportion of such features that are not shared by the stimuli (Russell, 1988).

I found that the theory of generalization also explains the non-Euclidean results of Attneave (1950), which had originally tipped me toward a career in psychological re-

search. The theory predicts that the abstract representational space is (locally) approximately Euclidean only to the degree that the extensions of the consequential regions along different directions in the space are positively correlated. Only then will the exponential arising from the Bayesian integration drop off in different directions in ways that approximately yield the elliptical equal-generalization contours (or surfaces) consistent with the Euclidean metric. This case seems appropriate for *integral dimensions* such as those of lightness and saturation for colors (Shepard, 1958b, 1962b, 1991). If, however, the extensions of the consequential regions along different dimensions are assumed to be uncorrelated, the slope of the computed exponential fall-off varies in a different way with directions in the space. This case, which seems more appropriate for *separable dimensions* such as those of color and shape (Attneave, 1950) or of size and orientation (Shepard, 1964a), yields equal-generalization contours that approximate the diamond shape corresponding to the so-called "city-block" metric implicated by Attneave (see Myung & Shepard, 1996; Shepard, 1987, 1991).

The fundamentally Bayesian approach to generalization briefly described here is being extended to the formulation of additional fundamental laws of inductive inference by Joshua Tenenbaum and Thomas Griffiths (2001a, 2001b). John Anderson too has demonstrated the power of a Bayesian approach to learning and categorization. He was also particularly effective in advocating the idea that principles governing human cognitive processes represent adaptations to general features of the world (Anderson, 1990, 1991).

Investigations of Discrimination

The theory of generalization is, as I noted, a theory of cognitive inference, not a theory of failure of sensory discrimination. It is based on an individual's uncertainty about what region in representational space corresponds to the set of objects harboring some consequence. To the extent that these stimuli are not fully discriminable, there will be additional uncertainty about what point in the space corresponds to a given stimulus. Generally, in this case, the function relating probability of response to distance between stimuli is expected to become inflected and convex upward in the vicinity of the training stimulus (cf. Nosofsky, 1986; Shepard, 1958a, 1986). (Indeed, Figure 3C may manifest some convexity of this sort.)

As was originally demonstrated by Henmon (1906), however, the time required to discriminate two stimuli, like probability of generalization, falls off with inter-stimulus difference according to a concave upward function. But discrimination time does not fall off exponentially. In my 1981 presidential address to the experimental division of the American Psychological Association (Shepard, 1981a), I presented a dozen sets of discrimination time data obtained by different researchers. In each of these sets, this fall-off with distance could be ap-

Figure 4. Reciprocal functions (with positive additive constants) fitted to humans' discriminative reaction times (in milliseconds) plotted against corresponding interstimulus distances. (A) Disjunctive reaction times to pairs of lines differing in length in the original experiment reported by Henmon (1906), with reciprocal function fitted by Shepard (1981a). (B) Discrimination times to indicate which of two circles differing in size was the larger, with fitted reciprocal function, from the experiment reported by Curtis, Paulos, and Rule (1973). (C) Discrimination times to indicate which of two arrays of dots contained the greater number of dots (Buckley & Gillman, 1974), with reciprocal function fitted by Shepard (1981a). (D) Average discrimination times to indicate, for each presented two-digit number between 31 and 99 (except 65), whether it was larger or smaller than 65, plotted as a function of the absolute numerical difference between the presented number and the comparison number, 65. I computed the plotted averages, from data kindly supplied to me by Stanislas Dehaene, from an experiment reported in Dehaene, Dupoux, and Mehler (1990). The smooth curve is the reciprocal function I then fitted to the plotted points. Figure 4B is adapted from Curtis, Paulos, and Rule (1973). Copyright 1973 by the American Psychological Association. (Adapted with permission.)

proximated, rather, by the reciprocal of distance (i.e., by distance raised to the -1 power) plus an additive constant yielding a positive asymptote corresponding to the minimum possible discrimination time. A reciprocal function of this form had earlier been proposed, on somewhat different grounds, by Curtis, Paulos, and Rule (1973). Unlike the exponential, this function increases without bound as interstimulus difference approaches zero. (For extremely small differences, however, a subject will respond earlier than predicted by this function as a result

of random neuronal noise or simply to terminate the trial.)

Here, I present the results obtained from four experiments on discrimination time (all plotted in milliseconds). Multidimensional scaling was not essential to determining the distances in these cases. The stimuli varied along a single physical dimension in such a way that physically defined distances might reasonably be presumed to approximate distances in the appropriate representational space. Figure 4A is based on the times orig-

inally measured by Henmon (1906) for discrimination of lines of different lengths. Figure 4B is the already mentioned results of Curtis et al. (1973) for discrimination of circles of different sizes. Figure 4C is based on the times measured by Buckley and Gillman (1974) to indicate, for each presented pair of dot arrays, which array contained the greater number of dots. In each of these three examples, a reciprocal function with a fitted (positive) asymptote appears to provide an approximation to the chronometric data.

I include the fourth example, presented in Figure 4D, to show that the dimension with respect to which distances are mentally represented need not be physically instantiated. Beginning with Moyer and Landauer (1967), a number of experiments have shown that the time to decide whether one number is larger or smaller than another decreases as distance between those numbers on an underlying number line increases, even though the numbers are presented only in the symbolic form of Arabic numerals—whether as single digits (see, e.g., Buckley & Gillman, 1974; Parkman, 1971; Shepard et al., 1975, pp. 131–133) or as two-digit numbers (e.g., Dehaene, Dupoux, & Mehler, 1990; Hinrichs, Yurko, & Hu, 1981).

In Figure 4D, I replotted data from an experiment by Dehaene in which subjects pressed a left- or right-hand button to indicate, as quickly as possible, whether a presented two-digit number (from 31 to 99, omitting 65) was, respectively, greater or less than the fixed comparison number 65 (Dehaene, Dupoux, & Mehler, 1990). The points are averaged for groups of four consecutive numbers (except close to zero, where, because the curve rises so steeply, the averaging was within smaller groups of two and, finally, of one), and then for such groups equally distant below and above 65. Although there is evidence that symbolic and other factors influence decision time, I take such results to provide compelling evidence that humans compare mentally represented analog magnitudes corresponding to the symbolic numbers much as if they were comparing physically presented lengths of lines or sizes of circles.

As before, however, a proposal of universality requires a theoretical justification for why the law should approximate this reciprocal form. I had already argued that the exponential law of generalization arose as an optimum cognitive strategy in the world, and not merely as a failure of discrimination. I now turned around and asked whether the proposed reciprocal law of discrimination time might itself have arisen because it too is in some way optimal. In our earlier investigations of the time needed to determine that two superficially dissimilar stimuli nevertheless correspond to the same external object in different spatial orientations, my students and I found that decision time increases linearly with the extent of the kinematically simplest transformation that would bring the object from one orientation into the other. For that case, I argued that actually performing such a transformation mentally was the most efficient strategy for establishing structural identity. Establishing

nonidentity presents a different problem. I, among others, noted that in detecting a more subtle difference between two stimuli, any being with limited processing resources is expected to require more fine-grained and, hence, more time-consuming processing.

Building directly on the already developed cognitive theory of generalization, I supposed that any difference might make a difference. Even highly similar stimuli, if at all different, could potentially correspond to objects of different kinds and, so, harbor distinct consequences. I accordingly invoked the same consequential regions and their associated maximum-entropy priors that I had introduced for generalization. I made the simplest additional assumption that each stimulus elicits internal representational events corresponding to the potentially consequential regions containing that stimulus with probabilities, per unit time, proportional to these priors. I assumed that the discriminative reaction is precipitated by the first such event corresponding to a region that includes either of the stimuli but not both. Integration over all potential regions then yielded an expected discrimination time that does indeed decrease with distance in accordance with a reciprocal function plus an additive constant—curves of the form fitted to the discriminative reaction times plotted in Figure 4 (Shepard, 1987, p. 1322). This does not constitute proof that the reciprocal discrimination-time function is optimal in the world. (Such a proof might require a precise specification of the form assumed for the processing limitations of a finite being.) But the reciprocal fall-off, with distance, of discrimination time, in addition to fitting the data, does have the intuitively expected qualitative form and, of course, has (for me) an appealingly simple relation to the theory of generalization.

Investigations of Music Cognition

The evolutionary emergence of mental structures specific to the generation and appreciation of music presents a challenge not posed by the evolutionary emergence of generalization, discrimination, or mental transformations. The abilities both to discriminate stimuli and to determine that they may nevertheless correspond to objects of the same kind, with the same potential consequence, are crucial for survival and must be possessed in some form, however rudimentary, by all animals capable of learning. Similarly, a capability for determining as quickly as possible that two successive images are of the very same object undoubtedly confers some benefit. I believe it to be represented—at least in the more perceptual form of apparent motion—in many visually and spatially proficient animals. But musical ability does not seem essential for survival. At most, as is perhaps suggested by the case of songbirds or whales, the lack of such ability might put one at some disadvantage in competing for a mate and, hence, in perpetuating one's genes. Apart from the debatable cases of birds and whales, what we recognize as music seems (at least on this planet) to be the exclusive province of humans. Even among humans, moreover, innate musical ability varies apprecia-

Figure 5. Musical listeners' ratings of how well each of 12 chromatic probe tones in an octave fits a preceding major-key context, and durations of corresponding tones in an actual piece of music. (A) Broken line: ratings for the context of the diatonic scale (Krumhansl & Shepard, 1979); solid heavy line: ratings for a chord cadence (Krumhansl & Kessler, 1982). Solid thin line: total duration of tones of the same 12 pitch classes in Schubert's op. 94, D780 (Krumhansl, 1985). Copyrights 1979 (for Krumhansl & Shepard, 1979) and 1982 (for Krumhansl & Kessler, 1982) by the American Psychological Association. (Adapted with permission.) The profile for Schubert is from Krumhansl (1985). Copyright 1985 by Sigma Xi. (Adapted with permission.) (B) The same data, but with the 12 pitch classes of the probe tones permuted from their order around the chroma circle into their order around the circle of fifths.

bly. The question therefore arises as to whether, and in what sense, there may be universals of music cognition.

At the outset of our investigations into music cognition, Carol Krumhansl and I introduced a probe tone method (Krumhansl & Shepard, 1979) that has proved capable of yielding stable and reproducible evidence about the representation of tonal hierarchies (for many subsequent examples, see Krumhansl, 1990). With this method, we presented various brief musical contexts consisting, for example, of a musical scale, a chord sequence, or a short excerpt from an actual piece of music (consistent, in each case, with a particular musical key). Following each presentation of such a context, we presented a single probe tone of some pitch, and listeners rated (usually on a seven-point scale) how well that probe tone fit in with the preceding context.

In Figure 5A, averaged ratings are plotted for probe tones presented at each of the successive steps of the chromatic scale within an octave determined by the musical key of the preceding context. [These probes correspond to the unison, U, taken as the tonic tone of the key, and (ascending in pitch) the minor and major seconds, m2 and M2, the minor and major thirds, m3 and M3, the perfect fourth, P4, the tritone, T, and so on up the chromatic scale.] The broken line shows the results for the more musical listeners of our original study (Krumhansl & Shepard, 1979), in which the context was the successive tones of the major diatonic scale (named *do*, *re*, *mi*,

fa, *sol*, *la*, and *ti*). The heavy solid line shows the results from a later study by Krumhansl and Kessler (1982), using richer musical contexts of chord cadences. The hierarchy of tonal functions specified by music theory emerges clearly in the resulting profiles. The highest ratings are uniformly given to the tonic tone of the key defined by the context (and to the tonic tone an octave above the unison, although that is not shown here). The next highest ratings are given to the dominant (or perfect fifth above the tonic). The next highest ratings are given to the other tones belonging to that key (the subdominant, mediant, and so on). Finally, the lowest ratings are uniformly given to all the tones not in the key defined by the presented context (the musical *accidentals*—which, in the key of C Major, would correspond to the black keys on a piano).

These subjective ratings agree remarkably well with objectively measured attributes of actual pieces of music (Krumhansl, 1990, pp. 68, 72). As one example (from Krumhansl, 1985), the solid thin line plots the total durations in numbers of musical beats (as tabulated by Hughes, 1977) of the corresponding notes in the first of Schubert's *Moments Musicaux*, op. 94, D780.

In Figure 5B, the data from Figure 5A are replotted, but with the probe tones permuted from their chromatic order of ascending pitch to their order around the circle of fifths (as is listed along the x-axis). According to music theory (as well as to the spatial and group-theoretic struc-

tures soon to be discussed), the circle of fifths is more relevant to musical relations of tonality and key. The formerly irregular sawtooth profiles now take on a simpler U shape such that all the probe tones not in the key defined by the context are grouped together at the bottom of the U. (The rating profiles for probe tones following minor key contexts also show the same bottom-of-U grouping of tones not included in the natural minor.)

The rating profiles shown in Figure 5 were obtained from musically inclined listeners. Whenever we tested unselected populations, we found large individual differences. The rating profiles obtained from listeners reporting less interest or background in music do not show the sharp, sawtooth pattern of Figure 5A or the strikingly orderly dependence on the circle of fifths of Figure 5B. Instead, the profiles of the less musical listeners show (in addition to a degree of octave equivalence) mainly a monotonic decrease in rating with distance along the chromatic scale from the tonic tone of the context (Krumhansl & Shepard, 1979). Evidently, whereas musical listeners pick up structural features such as tonal hierarchies keyed to the circle of fifths, less musical listeners may pick up only pitch height and octave equivalence. This is the case even in the Western world, where diatonic music is ubiquitous.

The question of the universality of tonal hierarchies led me to undertake, in collaboration with two students (Christa Hansen and Edward Kessler), my one and only cross-cultural experiment. In reading up for that study, I was struck by the preoccupation of most ethnomusicologists with the concrete and particular. Those studying the music indigenous to Indonesia emphasized that the pelog and slendro musical scales of Java and Bali not only differ completely from the Western diatonic major and minor scales but also differ, in specific tuning of the gamelan, from one village to the next. To understand such music, they said, one must immerse oneself in the music and culture of a particular village. But, as a cognitive psychologist, I wanted to know what might be common to all human cultures. Hansen (who was then undertaking field work in Indonesia), with the help of her husband, Mervyn Dennehy (who was fluent in the three dialects of Bali), made the rare find of a remote Balinese village whose residents had never been exposed to Western diatonic music. Yet the ratings these villagers gave to (simplified) probe-tone tests using gamelan contexts (which Kessler and I had devised back at Stanford) provided evidence that a subset of these villagers, just as a musically inclined subset of Western listeners, was sensitive to similar tonal hierarchies (Kessler, Hansen, & Shepard, 1984).

Along with other cognitively oriented music researchers (including Burns, 1999, Carterette & Kendall, 1999, and Dowling, 1978), we have noted that the musical scales of all cultures, despite their differences, have in common certain abstract features. This holds for the pelog and slendro scales of Bali (Kessler et al., 1984, pp. 139–140) and for the different scales of North India

(Krumhansl, 1990, chap. 10). Fundamental is the usual selection of either five or seven principal tones arranged, in the same way within each octave, to achieve a certain uneven spacing in pitch. This uneven spacing is reflected in the unevenness of the sawtooth pattern in Figure 5A and in the corresponding uneven spacing of the black keys on a piano. Moreover, this selection and uneven spacing is not arbitrary. It achieves the uniquely best satisfaction of each of two independent conditions.

The first condition arises from physical acoustics. Its recognition can be traced back to what has been deemed “the first application of mathematical description to physical quantity”—namely, the “enunciation of the laws of musical harmony” by Pythagoras (Gillispie, 1958, p. 63). It has been widely recognized and progressively refined during the last century and a half, beginning with Helmholtz (1862/1954). It requires that the frequency ratios and the approximations to overtone coincidences among the selected tones be such that combinations of these tones, when played together, achieve blends that are physically smooth and, hence, pleasingly consonant to the ear (see, e.g., Hutchinson & Knopoff, 1978; Kameoka & Kuriyagawa, 1969). Recently, Schwartz, Howe, and Purves (2003) have obtained striking evidence that the universals of tone selection and spacing correspond to statistical characteristics common to the speaking voices of all humans, regardless of language. Of course, this does not entail universality of musical structures for all musical beings wherever they may have evolved—unless these statistical characteristics of the human voice reflect (as I am inclined to suggest) universals of physical systems capable of generating suitably complex sound signals in general.

The second condition arises from abstract group-theoretic considerations. These have been identified only more recently and are still not widely appreciated. They concern neither frequency ratios (except the fundamental octave relation) nor anything about overtones. Instead, they place constraints on the merely ordinal relations among pitch intervals necessary to achieve important cognitive-musical objectives (Balzano, 1980; Shepard, 1982a). Of fundamental importance is the establishment of a fixed and readily remembered pitch framework. This is the reason for the unequal spacing of the (seven or five) selected tones. (In the example of the Western C-Major scale, the seven correspond to the white keys on the piano and the five to the black keys of the complementary pentatonic scale.) If, instead, the selected tones were equally spaced, each would have exactly the same pitch relations to all other tones. Such a spacing fails to provide the fixed, anchoring reference frame required for the establishment of the tonic and hierarchy of tonal functions with respect to which there can be melodic and harmonic tension, motion, and resolution. The selection of the unequally spaced subset (of five or seven tones) from an equally spaced larger set (of 12 chromatic tones in Western music) additionally provides for motion, at a higher level, through modulations between keys (Balzano, 1980;

Krumhansl, 1990; Krumhansl & Kessler, 1982; Shepard, 1982a; Shepard & Jordan, 1984).

These two sets of conditions—namely, the physical conditions that determine relations of sensory consonance among tones and the group-theoretic conditions that determine which abstract structures furnish a cognitive framework supporting the full tonal hierarchy and the sense of location and motion of tones and keys in space—share only the assignment of central importance to the octave. Remarkably, in view of their otherwise complete independence, they both specify as optimal the same division of the octave into the 12 chromatic tones and, also, the same type of selection from these tones of the subset defining a given key.

For all divisions of the octave into any number of equally spaced tones, analysis of physical properties such as the extent to which consonant frequency ratios are achieved yields good approximations for 12, 19, 31, 41, 53, . . . tones per octave. But the group-theoretic analysis yields results satisfying the specified cognitive criteria only for 12, 20, 30, 42, 56, . . . tones per octave (see Balzano, 1980). Notice that the only one of these divisions that satisfies both the more sensory, acoustic conditions and the more cognitive, group-theoretic conditions is the division into 12 tones per octave (which corresponds, in the group-theoretic analysis, to the cyclic group $C_{12} = C_3 \times C_4$). The selection of no more than 7 tones to define a given key may also be the only choice favored by either analysis, alone, that is compatible with Miller's (1956) "seven-plus-or-minus-two" limitation of human memory.

It seems almost miraculous that these two fundamentally different sets of (acoustic and group-theoretic) conditions converge in specifying the same musical structure. As just one consequence, consider the musical fifth, which is (after the octave) the most important interval in music theory, the most prevalent in world musics, and the most prominent in the rating profiles shown in Figure 5. In the acoustic analysis, the perfect fifth is unique because it achieves (after the octave) the simplest (3:2) frequency ratio and the least dissonance-causing beating of mismatched overtones. But, in the group-theoretic analysis, the fifth is unique for an entirely different reason. Beyond the semitone, which (on iteration) generates the chromatic scale (laid out on the x -axis of Figure 5A), the fifth is the only ascending interval that generates (on iteration) each of these 12 tones before repeating. In doing so, it produces the very different circle of fifths (laid out on the x -axis of Figure 5B).

All of the preceding has focused on the cognitive structures underlying just one of the two most fundamental components of music—namely, pitch. There is, however, reason to believe that similar cognitive structures underlie the other most fundamental component: time. The periodicities and, hence, frequency ratios of the (horizontal) rhythmic patterns in time are analogous to the periodicities and, hence, frequency ratios of the (vertical) patterns in pitch, but on a hugely expanded

temporal scale. Moreover, the same group-theoretic arguments apply to rhythmic structures as to tonal structures. It may therefore be, as argued by Jeffrey Pressing (1983), that there is a cognitive isomorphism between, for example, the rhythmic structures of West African music and the diatonic system of pitch in Western European music.

My provisional conclusions from these investigations into music cognition are summarized in the following points: (1) Very general principles of perception and cognition apply in the domain of music just as they do in other domains. Between musical objects (tones, chords, melodies, keys, rhythms, styles, etc.), I propose that generalization probability falls off exponentially and discrimination time falls off reciprocally with distance in the appropriate representational space. On the other hand, time to achieve a full mental connection between those objects (as parts of the same melodic stream, chord cadence, etc.) increases linearly with the least-time transformational path between them in the appropriate space. (2) The perception and cognition specifically of music is, however, subject to large individual differences among humans in those components of the representational space corresponding to higher order cognitive structures, such as the circle of fifths, tonal hierarchies, and structural relations among keys or modes. (3) Despite differences among individuals within any one culture, sensitivity to these higher order structures is manifested by the more musical individuals within cultures having such diverse musics as those of Bali, West Africa, and Western Europe. (4) Because these higher order structures are determined by physical and abstract mathematical universals, I conjecture that they may be universally approximated in all sufficiently advanced musical beings.

The Necessarily Abstract Character of Mental Universals

The investigations I have reviewed here on mental transformations, on generalization and discrimination, and on music cognition indicate how in psychology, just as in physics, universality is attained by framing the formulations at a sufficiently abstract level. If we conceive of stimuli, objects, or situations as points in an abstract representational space, then two fundamental types of geometrical properties specifiable for those points are distances between the points and connected subsets of the points. These are just the two properties from which I derived the empirically established forms of two candidate universal laws:

Law of generalization: The probability that an encountered stimulus has the potential for the consequence associated with a previous stimulus, despite their perceived difference, decreases exponentially with the distance between them in their representational space.

Law of discriminative reaction time: The time required to determine that two stimuli are not identical,

despite their perceived similarity, decreases (toward its asymptote) as a reciprocal function of the distance between them in their representational space.

These laws of generalization and of discriminative reaction time extend beyond pairs of stimuli. In the case of categorization learning and performance, generalization and discrimination time both fall off according to the same sorts of (exponential and reciprocal) functions of distance of a stimulus from the category boundary in the representational space (Shepard, 1981a). (Concerning dependence of reaction time on distance from category boundary, see, e.g., De Rosa & Morin, 1970; Levy, Goldberg, & Schmid, 1980. Concerning generalization in categorization, see Nosofsky, 1987, 1989, 1992; Shepard & Chang, 1963; Shepard & Kannappan, 1991; Tenenbaum & Griffiths, 2001a, 2001b.)

Another equally fundamental type of property specifiable for any two points in a representational space is the most direct path connecting those points in that space. This is just the property from which I have derived the empirically confirmed form of my candidates for the following universal laws of mental transformations:

Law of spatial transformation: The time required to establish that two stimuli, despite their apparent dissimilarity, correspond to the same object or to objects of the same shape increases linearly with the shortest path along a geodesic connecting the stimulus points in the representational space of the object's possible positions.

Law of musical transformation: The time required to assimilate two musical tones to the same melody or to pitch-shift one's tonal schema following a modulation between musical keys increases linearly with the shortest connecting path in the appropriate representational space.

The law of spatial transformation is intended to cover the cases of both apparent motion and imagined transformations. Indeed, both this law and that of musical transformation can be regarded as instances of a single, more abstract law of mental transformation. Such a more abstract law might serve as a quantitative specification for a general principle of "normalization of irrelevant dimensions in stimulus comparison" proposed by Dixon and Just (1978).

In some of these cases, such as that of musical transformation, the law of transformation admittedly has, as yet, less extensive empirical support. The case of perceiving the relation between tones is, however, quite parallel to that for visual apparent motion (Jones, 1976; Shepard, 1981c, pp. 318–321; 1984, p. 427; Van Noorden, 1975, p. 48). Thus, coherence of melodies requires that most melodic intervals be small and, also, that additional time be allowed for the larger intervals (see Figure 8 and ensuing discussion of "melodic motion" in Huron, 2001; concerning shift of the tonal schema in the space of musical keys, see, especially, Krumhansl & Kessler, 1982).

Of course, all of the four principal laws just listed require other supporting abstract principles and structures. To avoid technical details, I mention only the most relevant here.

In the theory of generalization, I propose that the local form of the metric of the abstract representational space has been determined by the degree of correlation (over evolutionary history) of the extensions of consequential regions along the dimensions of that space. This, I propose, is what distinguishes what have been termed *integral* and *separable* relations between dimensions (Garner, 1974; Shepard, 1991; see also Shepard, 1964a). In the way that I indicated earlier, the shapes of the equal-generalization contours produced by the Bayesian integration implicate a spatial metric that locally is approximately Euclidean if these extensions have been highly correlated. But the shapes of the equal-generalization contours produced by this integration implicate a metric that deviates from the Euclidean (in the manner of a Minkowski r -metric with $r < 2$) to the extent that these extensions have been uncorrelated (Myung & Shepard, 1996; Shepard, 1987, 1991). (The same theory provides an explanation for a striking difference in classification learning that depends on whether the dimensions of the stimuli are integral or separable—as shown in the experiments of Garner, 1974; Nosofsky, 1986; Shepard & Chang, 1963; Shepard, Hovland, & Jenkins, 1961.)

In the theory of spatial transformations, the global structure of the representational space is determined by the object's symmetries and degrees of approximation to such symmetries. Specifically, the space of distinguishable orientations of an asymmetric object is a curved six-dimensional manifold [which is isomorphic to the semi-direct product of the translation group, R^3 , and the rotation group, $SO(3)$; Carlton & Shepard, 1990a]. But, to the extent that the object approximates some symmetry, this space becomes more convoluted as determined by the symmetry group of the object $[S(O)]$ and the degree of approximation to that symmetry (as set forth in Carlton & Shepard, 1990b, and, in simplified form, in Shepard, 1981c, 1994).

In the case of transformations in music cognition, the appropriate abstract representational spaces arise as group-theoretic products of components generated by iteration of fundamental musical intervals (Balzano, 1980). Thus, the space of musical keys has a circular component (the already mentioned circle of fifths) such that among either major or minor keys, the most closely related keys are neighbors around that circle. Correspondingly, the most common transitions from one such key to another result from the smallest angles of rotation in the space (Shepard, 1982a). Depending on the circumstances of the test, other circular components also emerge. Multi-dimensional scaling applied to probe-tone ratings yielded toroidal configurations in which one circular component is the circle of fifths (laid out linearly along the x -axis in Figure 4B). When the context of a major diatonic scale was presented to listeners of diverse musical abilities

(Krumhansl & Shepard, 1979), the second circular component that emerged (Shepard, 1982a) was the *chroma circle* (first realized in concrete, physical form by Shepard, 1964b, and laid out linearly along the *x*-axis in Figure 5A). When richer contexts of both major and minor chord cadences were presented to musical listeners (Krumhansl & Kessler, 1982), yet another circular component emerged that brought major thirds together and achieved proximity between any key of one mode (major or minor) and its relative and parallel keys of the opposite mode (minor or major, respectively). In this representation, probable transitions between keys of both modes (major and minor) in Western diatonic music correspond to nearest neighbor transitions between points falling on a toroidal surface in a four-dimensional embedding space (Krumhansl & Kessler, 1982).

The Sense in Which Cognitive Principles and Structures May Be Universal

Now, when I put forward as “universal” a psychological law or structure, I am not claiming that the behavior of any given organism—let alone every living organism—behaves in conformity with that law or an associated abstract spatial or group-theoretic structure. Although I expect the exponential law of generalization to be approximated quite widely among animals, I regard it as unlikely that an earthworm appreciates music or is capable of mental rotation, let alone mental rotation yielding a decision time that increases linearly with angular difference. Rather, I am proposing that as organisms evolve more and more powerful capabilities for representing the external world, the principles governing their cognitive representations and, hence, their behavior will increasingly approximate what would be optimum in that world.

At any given evolutionary stage, including that currently attained by humankind, the approximation is very likely imperfect and perhaps unevenly distributed in the population. This is particularly likely to be true for capabilities that are not crucial for an individual’s survival and reproduction. Music cognition may be a case in point. In many other domains, cognitive process may amount to a collection of heuristics (Gigerenzer, Todd, & the ABC Research Group, 1999; Newell, Shaw, & Simon, 1958) or a utilitarian “bag of tricks” (Ramachandran, 1990) that is not guaranteed to yield the optimum result in all situations but, for typically occurring situations, may “satisfice” (Simon, 1956, 1990). If any selective pressure for still better representation continues, however, I know of no impenetrable barrier preventing such collections of heuristics from evolving still closer approximations to theoretically optimum principles for still wider ranges of situations. Surely, we already have advanced quite a way beyond the earthworm.

But what about the widely cited evidence that even we humans fall far short of optimum performance on various cognitive tasks? Examples include simple logical deduction (see, e.g., Wason & Johnson-Laird, 1972; Woodworth & Sells, 1935), probabilistic inference (e.g., Tversky & Kahneman, 1974, 1981, 1983), and intuitive physics

(e.g., McCloskey, 1983; McCloskey, Caramazza, & Green, 1980). Concerning any result that might be taken as evidence against a theory—including a theory about internalization, levels of abstraction, and universality—one should ask whether the experimental conditions are those that are appropriate according to the theory itself. Three points are particularly relevant here.

The first point is that the abstract paper-and-pencil tasks typically used in the experiments just cited bear little resemblance to the concrete, perceptually experienced, real-life challenges that confronted our Pleistocene ancestors. There would have been little selective pressure favoring the cognitive skills needed for the swift and accurate solution of such abstract paper-and-pencil problems. Indeed, tests of the same general kinds of knowledge by means of perceptually more concrete, realistic, or meaningful tasks have usually led to better performance (see, e.g., Cosmides & Tooby, 1997; Kaiser, Proffitt, & Anderson, 1985; Kaiser, Proffitt, Whelan, & Hecht, 1992). (Of course, some humans are now able to solve such abstract problems by paper-and-pencil calculation, but they do so by means of explicit mathematical principles of reasoning slowly developed over the last few millennia and transmitted culturally rather than by means of individually available intuition.) Similarly, people’s behavior relative to objects in space—even when they are blindfolded—reveals more precise knowledge of spatial relations than they are able to express verbally (see, e.g., Milner & Goodale, 1998; Philbeck, Loomis, & Beall, 1997).

The second point is closely related to the first. The basic perceptual skills that actually were important to our ancestors throughout the Pleistocene evidently do operate in close approximation to optimum principles such as those of Bayesian inference (see, e.g., Geisler & Kersten, 2002; Knill & Richards, 1996; Weiss, Simoncelli, & Adelson, 2002) and color constancy (see, e.g., Maloney & Wandell, 1986; Shepard, 1990b, 1994, pp. 16–22). In contrast with our effortful and blundering struggles with the abstract paper-and-pencil tasks, we have much less conscious awareness of these perceptual processes of more ancient provenance. Through natural selection, the latter processes have become so deeply internalized and automatic that conscious, deliberative intervention would only interfere with their swift, accurate, and reliable operation.

The third point is apt to be overlooked until one acknowledges a terrestrially unprecedented evolutionary development that has occurred with the emergence of a capacity for abstract thought and for language in the human line. This is the development of an ability to set aside immediate self-interest and, dispassionately and hypothetically, (1) to imagine or simulate different alternatives within a potentially large but well-defined domain or space and (2) to evaluate those alternatives with respect to some explicitly specified criterion. I have referred to this development as the *step to rationality* (Shepard, 2001, p. 741).

Admittedly, this capacity may not yet be fully or widely developed even in the human line, and in our efforts to exercise it, we often depend on the support of ex-

ternal aids—yes, including paper and pencil and, more recently, computers. Still, it is this new, rational capacity that has enabled a precious few human individuals to make their transformative contributions to science as well as to other disciplines—including musical composition and moral philosophy. (In the latter connection, I have argued that a recognition of the nature of this rational capacity also makes possible a reconciliation of free will and determinism, which have for so long seemed incompatible; Shepard, 2001, p. 747.) Finally, and of more immediate relevance here, only by developing explicit and correct formulations of symbolic logic, probability theory, and theoretical physics are we now able to determine the correct answers to the paper-and-pencil tests on which we otherwise fail so miserably. Only thus are we provided with a benchmark against which to evaluate how closely our innate perceptual or cognitive functions approach optimality.

How Cognitive Principles and Structures May Underlie Our Capacity for Scientific Discovery

In my Paul Fitts Lectures at the University of Michigan in 1993 and, more extensively, in my William James Lectures at Harvard in the fall of 1994, I explored the possibility that the cognitive principles of generalization and mental transformation considered here may have made possible some of the most far-reaching theoretical advances in science. A book largely based on these lectures is still in preparation, alas. But I have recently described how the capacity for mental transformations, which I believe to underlie our deep intuitions about symmetry, may help to explain the power of thought experiments in the discovery of such physical laws as Archimedes's law of the lever, Galileo's mass-independence law for falling bodies, and Newton's action-and-equal-reaction law of motion (Shepard, 2001, pp. 742–743). I showed how these laws can be established without carrying out any experiment physically, by using the single principle that the physics of a situation is unchanged by any permutation of equivalent objects.

For modern theoretical physicists, of course, symmetries are fundamental and pervasive. The mathematician Emmy Noether proved that every continuous symmetry implies a conservation law and vice versa. And, as remarked by the Nobel Laureate codeveloper of the standard theory of particle physics, Steven Weinberg (1992, p. 212), "it is principles of symmetry that dictate the *dramatis personae* . . . on the quantum stage." In my response to the commentators in the 2001 special issue of *Behavioral and Brain Sciences*, I also indicated how the symmetry of invariance under permutation can be seen to underlie the "golden rule" and the essential idea behind the highly influential theory of justice of John Rawls (1971). Going well beyond the realm of science, then, symmetry may also be crucial to the formulation of a rational grounding for universal principles of ethics (Shepard, 2001, pp. 744–748).

In physics, the principle of *least action* is perhaps the most fundamental symmetry principle of all. Helmholtz

(1886) judged it to be with "high probability . . . the universal law governing all processes in nature," and Planck (whose quantum of action is the cornerstone of quantum mechanics) wrote that "Einstein's theory of relativity . . . has shown that it occupies the highest position among physical laws" (Planck, 1925/1993, pp. 69, 80). With Feynman's culminating path-integral extension of it to quantum mechanics (Feynman & Hibbs, 1965), the principle of least action was seen to be, itself, a symmetry principle. It has been described as the "democracy of histories" in that all possible histories of motion are treated, symmetrically, as "equal under the law." (In the curved four-dimensional space-time manifold of general relativity, incidentally, least action entails the geodesic paths of motion that suggested to me the idea that mental transformations might traverse least-time geodesics in the curved six-dimensional space of possible positions of an object.)

I may be foolhardy to challenge the prevailing empiricist world view with the claim that a law of physics might be discoverable by thought alone, or to justify this claim with the suggestion that what is generally regarded as an empirical fact might be a mathematical necessity. But, just as I hope that psychological laws are not arbitrary, I hope that physical laws are not arbitrary. Otherwise, in arguing that psychological laws have emerged partly as accommodations to physical universals (and not just to mathematical universals), I would have succeeded merely in pushing arbitrariness in psychology back to arbitrariness in physics. There are, however, two alternatives to the possibility that the ultimate laws of nature are merely arbitrary or capricious.

The first alternative is that those laws constitute the only mathematically self-consistent set. Such an alternative has been contemplated by several theoretical physicists, including the late particle physicist and cosmologist David Schramm (see Lightman & Brawer, 1990, p. 450). Similarly, Einstein wrote: "In a certain sense, pure thought can grasp reality, as the ancients dreamed" (Einstein, 1934/1954, p. 274) and is reported to have wondered aloud whether "God had any choice." Much earlier, Leibniz (who closely followed Maupertuis as one of the two earliest articulators of the least-action principle) expressed the related idea that the universe is unique in realizing the richest set of actualities by the simplest possible means. (As a visualizable analogy to Leibniz's idea, consider that one simple recursive formula generates the inexhaustible fractal complexity of the Mandelbrot set, beautifully illustrated in Peitgen & Richter, 1986.)

The second alternative is that the universe in which we reside is but one of infinitely many universes and that within the whole infinite set, everything mathematically possible somewhere becomes a physical reality, as conjectured by the physicist Max Tegmark (1998). If universes endlessly spawn other universes, perhaps in one of the ways envisioned by Andrei Linde (1994), Lee Smolin (1997), or Tegmark, symmetry may be broken differently in the birth of each new universe, yielding different laws of physics (perhaps corresponding to such symmetry groups

as $SU(5)$, $SU(4) \times U(1)$, or $SU(3) \times SU(2) \times U(1)$, as Linde suggested). Only in a tiny fraction of these universes would the resulting laws of physics be compatible with the emergence of life and, hence, of mind. The laws in these rare life-engendering universes would therefore not be arbitrary. In accordance with the anthropic cosmological principle, then, even under a cosmology in which all possibilities somewhere become actualities, mental laws—which can reflect, among physical laws, only those that are life engendering—will also not be arbitrary.

Prospects for Psychological Science

I hasten to emphasize that my preceding discussion concerned discovery of certain laws of physics. I am not advocating that physicists—let alone psychologists—abandon the laboratory for an armchair. Despite our halting steps toward rationality, we still have only partial access to only partially internalized wisdom about universals of the world. On looking back, I find that most of my own successful experiments (such as those on mental rotation, apparent motion, and generalization) were ones for which I had a pretty clear idea of what the results would be before I even began collecting the data. But I was surely guided toward thinking of these experiments, and of how they should be set up, by my preceding experiences in the psychology laboratory.

This admission is not, however, inconsistent with my claim that if formulated at the appropriate level of abstraction, psychological laws may partake of the universality of the laws of mathematics and physics. Granted, the non-Euclidean, curved, and high-dimensional spaces and group-theoretic structures posited here are foreign both to what we directly experience and to any folk psychology. One might well wonder whether such abstract spaces and structures and their associated principles have any real existence or relevance for human life, experience, or behavior. In closing, I offer two justifications for believing that they do.

First, these abstractions, like the equally nonintuitive abstractions of theoretical physics, have yielded empirically confirmed predictions. As one example, the theory of mental transformations specifies that the representational subspace corresponding to polygons that have a fixed, intermediate degree of central symmetry but that vary through 360° in orientation is a curve that both lies on a torus and forms the edge of a Möbius strip in a four-dimensional embedding space. Yet, when reaction time data from human subjects presented with these polygons were submitted to multidimensional scaling (of the INDSCAL type developed by my former Bell Labs co-workers Carroll and Chang, 1970), the points representing these polygons fell on exactly the predicted curve in the four-dimensional solution (Shepard & Farrell, 1985).

Likewise, in the case of music cognition, the acoustic, geometric, and group-theoretic analyses mentioned here, as well as analyses by music theorists (e.g., Werts, 1983), dictate that musical tones, at one level, and musical keys, at another, have spatial representations that (depending

on circumstances) are circular, helical, or toroidal in embedding spaces of (respectively) two, three, or four dimensions. Here, application of multidimensional scaling to similarity measures based on human listeners' data have yielded just the predicted circular, helical, and toroidal configurations (Krumhansl, 1990; Krumhansl & Kessler, 1982; Shepard, 1982a, pp. 310, 323; Ueda & Ohgushi, 1987).

Second, although the experiments yielding data that fit these highly abstract mathematical theories are obtained under highly controlled laboratory conditions having little resemblance to the complex situations we deal with in everyday life, I claim that this is just the way in which fundamental science advances. The physicist confirms general and precise quantitative laws, such as the law of falling bodies, not by observing the descent of leaves on a gusty autumn day but by timing the descent of a heavy ball in an evacuated cylinder. Moreover, as the theory strives for greater and greater simplicity and generality, it necessarily becomes, as noted by Einstein (1934/1954, p. 282) "steadily more abstract and remote from experience." Thus, in Einstein's eloquent words (quoted in Holton, 1973, p. 377), the scientist, like the artist, erects a "simplified and lucid image of the world," lifting into it "the center of gravity of his emotional life."

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NOTES

1. This historical section is a shortened version of the corresponding section of my chapter "A Funny Thing Happened on the Way to the Formulation: How I Came to Frame Mental Laws in Abstract Spaces" (Shepard, 2003—in *Psychologists Defying the Crowd*, edited by Robert J. Sternberg), copyright 2003 by the American Psychological Association. Adapted with permission.

2. For more about such inner phenomena, including my own, see Shepard (1978a, pp. 167-183; 1990a, pp. 13-40).

3. For further reminiscences about Hovland, see Shepard (1998, 2000).

4. For more on my view of Estes's trailblazing work, see Shepard (1992).

5. Cooper's 1976 article is reproduced, in a slightly edited form, in Shepard and Cooper (1982, chap. 7, pp. 159-170).

(Manuscript received April 4, 2003;
revision accepted for publication June 25, 2003.)