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"In memory of Michael Turvey"

Michael Turvey: A Personal Tribute

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ABBREVIATIONS

CESPA Center for the Ecological Study of Perception and Action

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ABSTRACT
Michael Turvey revolutionized the study of perception and action. In this personal tribute, I summarize his most important contributions from my perspective. The contributions start with information-processing psychology and then turn to ecological and dynamical-systems research. My work, like that of many others, was significantly affected by Turvey's insights and charisma. I review studies of my own that particularly bore the mark of Turvey and his colleagues.

KEYWORDS: Ecological psychology | Dynamic systems | Information processing psychology | Coordination | Touch

On August 14, 2023, soon after I learned of the passing of Michael Turvey via a message from the Society of Experimental Psychologists (an honorary society to which Mike and I belonged), I posted the following note:

> *To hear a talk by Mike Turvey was to be dazzled. He was the most eloquent, talented speaker I've ever heard. I had the privilege of hearing him give many talks and was amazed and inspired by every one of them. His papers, too, were spell-binding, not just because of the power and depth of his writing, but also, more importantly, because of what he had to say. He took Gibson's ideas and extended them in profound ways to domains such as haptics and the control of physical action, the area I decided to pursue, partly based on Turvey's work. Going further, Turvey spearheaded the dynamical systems approach in psychology and related fields, leading many who had never thought about perception, cognition, and action in those terms to do so. He changed the field and established a tradition that opened the way for others who would become major figures in their own right – thanks, I believe, to his input. We know so much more than we would have if he had not brought his remarkable energy and intellect to the problems he chose to explore. Mike received many awards, including a Guggenheim Fellowship, but the two awards that I think speak most resoundingly to the sustained impact of his work were his receipt in 1974 of the APA Award for Distinguished Scientific Early Career Contributions to Psychology, and his receipt in 2011 of SEP's Norman Anderson Lifetime Achievement Award. How remarkable! What an amazing person! We were so lucky to have been in his circle.*

My comments were similar to many others that appeared on the SEP listserv. We had lost a special scientist, colleague, and friend.

In this article, I want to amplify on my tribute to Michael Turvey. The way I can do this most effectively, I think, is by discussing the ways he influenced me personally. This way of speaking may seem self-centered, but I want to write from the heart, as it were. The path I pursued in my career changed so dramatically as a result of Mike Turvey's influence that it would be disingenuous for me to write in any other way. Given that a major theme of Mike's work was affordances, it is fitting to focus on what Mike afforded me. Mike's impact on me was typical of his impact on many people.

FIRST MEETING

In the Spring of 1976, I was on a kind of sabbatical from graduate school. At the time, I was a PhD student at Stanford and had gotten very interested in perception and action. I felt that the research I was seeing in cognitive psychology (my specialty area) was more removed from everyday life than I would have liked. My adviser, Gordon Bower, a brilliant and colorful memory researcher who would go on to win the National Medal of Science, freely admitted that he knew little about perception and action. Because I was being supported by an NSF Graduate Fellowship which allowed me to work on my own, Gordon allowed me to go off on and explore perception and action where those topics were being pursued more vigorously. He allowed me to spend the Spring of 1976 in the Boston area, where a number of top-flight perception-and-action researchers were busy at work – among them, Emilio Bizzi at MIT and James Lackner at Brandeis.

While Gordon Bower didn't know much about perception and action, Stanford University wasn't entirely bereft of expertise in these areas. Roger Shepard, best known for his work on mental imagery and multidimensional scaling (and also someone who would win the National Medal of Science), knew a lot about perception, and he mused a bit about action. A well-known example from his writings was his speculation that a dog holding a stick in its mouth while facing a fence with an irregularly spaced gap might mentally rotate the stick to determine which head orientation would let the dog pass through the gap (Cooper & Shepard, 1984). Whether the dog actually engaged in such mental rotation is unknown, of course, though neural recordings of monkeys trained to redirect their hands from initially displayed targets to targets shifted 90 degrees away suggest a neural analog of mental rotation; the direction of the population vector of the motor-cortex neurons of the monkeys turned continuously over time at a rate not so different from that inferred from human reaction times (Georgopoulos et al., 1989).

Roger Shepard, who co-mentored me from his faculty perch in the Stanford Psychology Department (like the one occupied by Gordon Bower), advised me on an experiment I designed to test a hypothesis I might not have thought to test were it not for Turvey. The hypothesis was that visually perceived acceleration is perceived directly; the alternative hypothesis was that acceleration is computed by going through one or more perceptual steps. Acceleration is, of course, the second time derivative of position, and velocity is the first time derivative of position. It was possible, therefore, that acceleration is perceived only after position is perceived or only after velocity is perceived. The alternative, as already mentioned, was that acceleration is perceived without those intermediate steps.

The question of whether perception is direct or indirect interested me because Mike Turvey and his colleagues had helped bring that issue to the fore, building on the work of James Gibson (1950); also see Gibson (1979), Michaels and Carello (1981), and Wagman and Blau (2019). It is fair to say that if Mike Turvey and his colleagues had not pushed this issue, I might not have picked up on it.

I set up an experiment in which I showed people a smoothly moving object that slid along a linear air track (borrowed from the Stanford Physics Department). The object was pulled with a thread over a pulley, and a load was attached to the thread. The participant's task was to press a button when the object passed behind a mark on a screen blocking sight of the moving object. I varied the time and distance of object visibility as well as the time and distance of object invisibility. Based on linear regression analysis, I concluded that acceleration is directly perceived (Rosenbaum, 1975).

Because I was living in Cambridge, Massachusetts after the acceleration paper appeared, Mike Turvey invited me to visit him and his group at the University of Connecticut in nearby Storrs. He showed me around and brought me to a lab meeting where he asked me to talk about the acceleration work. This presentation was, in effect, my first research talk; I lost track of the date of the talk and who was in attendance. What I do remember clearly, however, was that Turvey had amazing energy, friendliness, and openness – and, no less, he had a remarkably statesmanlike demeanor and beautifully tailored clothes. With his deep, resonant voice and distinguished English accent, he was as charismatic as he was brilliant. He also had an impressive group around him. I sensed that the group at UConn would do great things, and I was gratified to be welcomed by them as warmly as I was.

INFORMATION PROCESSING

The next year (1977) I got my PhD from Stanford and joined the Human Information Processing Research at Bell Laboratories, Murray Hill, New Jersey. My department head was Saul Sternberg, a cognitive psychologist who had revolutionized the field by adding reaction times to memory research (Sternberg, 1966, 1969). Rather than only recording accuracy of memory, Sternberg devised a task – what came to be called the Sternberg memory scanning task – where, in each trial, participants memorized a short list and then indicated whether items shown one at a time were on the list. The lists were short enough to be within the typical span of memory, so recognition accuracy was near ceiling. Consequently, the data of interest were the reaction times to decide whether the test items were in the memorized lists.

Sternberg devised alterative models of how the test item would be compared to the stored list. The models were rendered as flowchart models of the kind used in computer science. The mind-as-computer metaphor was popular at the time, and Sternberg's work helped make it so because it offered a workable procedure for distinguishing between or among the possible flow-chart models. The models were distinguished with respect to whether memory search was serial or parallel on one hand, and limited-capacity or not limited-capacity on the other. These became core issues in the information-processing tradition.

Sternberg's Human Information Processing Research had another member who was as established as Sternberg himself. That person was George Sperling. (Sperling and Sternberg were about 20 years my senior.) Back when Sperling was a graduate student, he had discovered iconic memory (Sperling, 1960), which he conceived as a short-term buffer which holds information in a raw tenuous form. Information degrades quickly from the buffer but has a chance of being converted to more permanent form if it is categorized or recognized. Sperling used the term *iconic memory* to refer to this memory store. His work comprised a cornerstone of information-processing psychology, as did Sternberg's.

In 1973, in the heyday of information-processing psychology, Turvey published a paper in *Psychological Review* called **"On peripheral and central processes in vision: Inferences from an information-processing analysis of masking with patterned stimuli"**. Masking was one the main methods for erasing stimuli from iconic memory. Because iconic memory was thought to hold stimulus records in raw sensory (visual) form, a visual stimulus presented "atop" the icon could overwrite the representation, provided the mask was shown at a time and in a form that interfered with the representation. If the mask was patterned one way or another, changes in reports about the just-presented stimuli could shed light on the peripheral (bottom-up) and central (top-down) influences on the icon.

Turvey's work on this topic (Turvey, 1973) was exhaustive and brilliant. So too was his co-authored paper announcing the discovery of an auditory analog of Sperling's visual iconic memory. Darwin, Turvey, and Crowder (1972) used a technique similar to Sperling's (the partial report method) and obtained evidence for echoic memory, as they called it. For this body of work, Turvey received the APA Award for Distinguished Scientific Early Career Contributions to Psychology in 1974. This was the first time the prize was given. Some thought it was created *for* Turvey.

ECOLOGICAL PSYCHOLOGY AND DYNAMICAL SYSTEMS

Not one to rest on his laurels or to fall into complacency about the adequacy of his earlier work, Turvey (1977) published another paper in *Psychological Review* in which he repudiated the information-processing approach. In this new article, titled "Contrasting orientations to the theory of visual information processing," Turvey embraced Gibson's view of perception with its emphasis on direct perception.

The fact that Turvey rejected the prevailing (information-processing) approach made waves in the broader community and was certainly noticed in Sternberg's department, which, arguably, was the center of that approach. I was aware of the foment, of course, for my own work was rooted in the information-processing model, though I had already felt some strains of discontent, leading me to test the directness of visual perception in one context (acceleration) and focus on the control of physical action (Rosenbaum, 1980, 1983), a subject which had gotten relatively little attention by cognitive psychologists given their adherence to a symbol-based rather than body-based view of intelligence.

In 1977, the same year that Turvey published his second *Psychological Review* article, he also published "Preliminaries to a theory of action with reference to vision," which appeared in a book edited by Robert Shaw and John Bransford, *Perceiving, Acting, and Knowing*. Here Turvey expanded his endorsement of Gibson's emphasis on affordances and the inextricability of perceiving, acting, and knowing.

Later, putting this commitment into action, so to speak, Turvey co-founded the Center for the Ecological Study of Perception and Action (CESPA) at the University of Connecticut. CESPA served and has continued to serve as the intellectual hub of Gibsonian research worldwide (https://cespa.uconn.edu/). Relatedly, it became a core location for the pursuit of the dynamical-systems perspective in the study of perception and action.

Dynamical systems are ones that change over time in ways that can be characterized with equations that express the state of the system at one time as a function of the state of the same system at the preceding time. Determining what that function is for a putative system is the goal of the research.

Turvey and others at CESPA sought to extend the dynamical-systems approach to perception, action, and cognition. Core to their endeavor was avoidance of loans on intelligence. The idea was that no inner executive or homunculus could be posited to explain how behavior emerges. Any appeal to such an inner being, explicit or implicit, would beg the question of how the behavior came into being in the first place. I took this view to heart and wrote a book aimed at dispelling the homunculus from the mind and brain (Rosenbaum, 2014).

From the broad perspective pursued at CESPA emerged a slew of new findings, breaking new ground where little relevant work had been done before. One line built on pioneering research by the German biologist, Erich von Holst, who had studied the coordination of extremities in fish and humans (von Holst, 1939). von Holst showed that fish fins tend to couple and that human arms do as well. Later, in the wake of this early work, Turvey and others embarked on theoretical (Kelso, Holt, Kugler, & Turvey, 1980; Kugler, Kelso, & Turvey, 1980; Kugler & Turvey, 1987) and empirical work on coordination. The empirical work included a simple preparation in which people swung two handheld pendulums as they wished (e.g., Rosenblum & Turvey, 1988; Treffner & Turvey, 1996). In other lab situations, people rocked in sight of one another (Richardson, Marsh, Isenhower, Goodman & Schmidt, 2007). Coupling between the hands was observed and so was coupling between the people, suggesting rich patterning reflecting both established *mechanical* relations, as described centuries before by the Dutch physicist Christiaan Huygens, and newly found *informational* relations. The work was published both in physics journals and in psychology journals, including *American Psychologist* (Turvey, 1990), the flagship journal of the American Psychological Association. Such interdisciplinary coverage had little precedent.

Another line of work also had strong ties to physics; this work concerned dynamic touch. The main empirical question was this: When one wields an object without seeing it, can one discern its physical properties? More specifically, can the length of the wielded object (a handheld rod) be perceived through touch alone? The answer was resoundingly "yes" (Carello and Turvey, 2004). By grasping a rod at one end and moving it freely, participants in the experiments could get haptic information about the length of the rod even if it was unseen (and unheard, with no part of it impacting another surface). This body of work, which included other tasks as well, was another significant advance for the ecological/dynamical-systems approach.

PERSONAL IMPACT

Many other findings could be adduced here. I have touched on just two lines of work (coordination and dynamic touch) because they had the biggest effects on me.

I would now like to say more about work of mine that reflected ways Turvey influenced me. My personal experience may be of interest not because I had special connections with Brazil (though I have had the privilege of visiting Brazil to give lectures in several cities and have long known a number of Brazilian researchers), but because, ironically, I have remained on the fence about the strongest versions of the ecological and dynamical-systems claims. Arguably, my tribute may be more credible because I have often been openly skeptical about the approach.

One way I expressed my skepticism was to publish an article in *Motor Control* called "Is dynamical systems modeling just curve fitting?" (Rosenbaum, 1998). Saying that the dynamical systems approach is nothing but curve fitting is not a generous thing to say, of course, but I titled the article as I did to lead up to a conclusion that was a play on words. The word "just" can mean "only" or "justified." The latter meaning was the one I advocated. I concluded that, because dynamical-system regimes are expressed as equations that get fitted to data, there is a sense in which the approach resembles curve fitting. Evaluating arbitrary models by finding empirical parameters that provide best fits to data is, indeed, mere curve fitting, but allowing only certain classes of models, such as those admissible in the dynamical-systems approach, is not. What is most important about the dynamical systems approach, I argued, was that the models under consideration were not arbitrary and that, most especially, the models considered, and the phenomena to which they correspond, had been seldom studied before in psychology, kinesiology, and related fields.

Another context in which I expressed some (hopefully healthy) skepticism about the dynamical systems approach was in regard to a coupled-oscillator model of timing stability. Yamanishi, Kawato, and Suzuki (1980) had offered a two-coupled oscillator model for coordinated bimanual finger tapping. These authors had participants tap their two index fingers in time with series of tones, one mapped to the left finger and one mapped to the right. Holding the frequency of taps constant, Yamanishi, Kawato, and Suzuki varied the required phase lag. They obtained a striking "seagull" pattern where the standard deviations of the produced intervals dipped at phase lags that were integral multiples of π but grew more the larger the rectified difference of the phase lag from those special values.

Yamanishi, Kawato, and Suzuki accounted for these results with a nonlinear coupled-oscillator model, but I endeavored to show that their data, along with other well-established timing results such as Weber's Law for timing variability (Ivry & Hazeltine, 1995) and the tendency for production of polyrhythms to revert to simpler frequency ratios as driving frequencies increase (Peper, Beek, & van Wieringen, 1995) could be accounted for with a linear delay-line model (Rosenbaum, 1998, 2002). The main idea was that noise would grow with the length of the delay line. The novel idea was that coincidence of signals arriving at nodes in the network could mark events associated with the input lines.

The model was first described at a meeting of New England Sequencing Timing. This is a meeting which has been held every year since the early 1990's and has been hosted, most recently, by Ed Large at the University of Connecticut; Turvey was a regular attender and contributor to the meeting. A chapter co-authored by him appeared in a volume of papers based on the first meetings (e.g., Amazeen, Amazeen, & Turvey, 1998), and that volume is also where my delay-line model was first described in print (Rosenbaum, 1998).

After the appearance of that volume, I elaborated the model and was surprised to see that it actually predicted short-term memory limits. The limits arose by virtue of the statistical properties of the linear system being postulated. Simultaneous neural inputs registering events of varying complexity could only be resolved up to some degree of complexity owing to the buildup of noise with more and more complex inputs (Rosenbaum, 2002). To my great excitement, then, established principles of statistics, embodied in the algebra of expectations, led to memory capacity limitations. There was no need to assert, post hoc, that there are memory limits due to the putative size of the "box" in which information is held or the level of attention that must be marshalled to maintain information in short-term memory (Engle, 2002).

The foregoing work might not have been done without Turvey's influence, direct or indirect. Other projects from my lab came about this way, too. Here I will mention a few, all aimed at advancing the ecological approach, and all, appropriately enough, based on everyday observations.

One observation was made at Grand Central Station in New York City (Rosenbaum, 2009). There I noticed that people descending the staircase to the main concourse were especially likely to glance downward about three or four steps from the bottom of the staircase. This observation was later confirmed at the Student Union Building of Penn State University using video recordings and careful coding by naïve judges of hundreds of people walking down the main staircase of the building.

Seeing the phenomenon replicated, I proposed that the prevalence of downward glances near the bottom of the staircase was due to the sudden disappearance of steps from the bottom of the visual field. Prior to that event, images of individual stairs arose and disappeared in the lower visual field, coming into view and being replenished for as long the supply of new (lower) stairs held out. When the renewal stopped, there was a kind of visual catastrophe. The staircase disappeared. That event triggered a downward glance, at least by hypothesis. Glancing downward allowed for quick reconnoitering, and looking back up after the brief glance allowed for preview of the remaining walk down to and into the landing.

I likened the visual trigger to the one proposed by David Lee in his account of diving gannets retracting their wings (Lee and Reddish, 1981). However, in the case of the stair descenders, the trigger was far less subtle; it was, literally, the sudden disappearance of structured visual input from below.

I suggested that after that last glance was made, walkers relied on memory-updating to track where they were in space. The latter component was decidedly different from anything David Lee would have proposed, but I felt it was appropriate to invoke memory plus direct perception because multi-level activity like this characterizes intelligent behavior (Rosenbaum, 2022, 2024). Mike Turvey was, of course, aware of and intent on understanding this interaction as well (e.g., Holden, van Orden, & Turvey, 2009; Lukatela & Turvey, 1998; Shockley & Turvey, 2006).

Another observation from my lab, which again reflected my adherence to and admiration of the ecological approach, might not have happened without the personal impact of Mike Turvey. In a restaurant, I saw a waiter take hold of an upside-down glass before flipping it over to pour water into the glass and then set it down. The waiter grabbed the glass with his thumb down rather than thumb up. This odd behavior caught my eye, for one would never grab a glass with the thumb down if one were going to drink from it, say. I surmised that the thumb-down position reflected tuning of action to forthcoming task demands. Grasping an upside-down glass with the thumb down let the waiter be in a comfortable and easy-to-control posture when later pouring water into the glass and then setting the glass down. The waiter had learned to anticipate this challenge and adjust his behavior accordingly.

Laboratory tasks modeled on the waiter's task confirmed the effect. University students (most of whom had not worked as waiters) did the same thing. A large number of studies conducted in my lab and others around the world established the breadth and limits of this *end-state com*fort effect, as it came to be called (Rosenbaum et al., 2012, 2014). Comparative psychological work showed that the planning capacities underlying this effect were in place more than 80 million years ago, judging from the appearance of the effect in lemurs (Chapman, Weiss, & Rosenbaum, 2010).

Another everyday observation was made in a "very real-world context" – namely, my home bathroom. One day I happened to grab a toilet plunger standing at a relatively high position prior to bring it down to the floor. I noticed that I grabbed the plunger high. Later, when I grabbed the same plunger at the same high position in order to bring it to an even higher position, I grabbed it lower. In both cases, I adjusted the initial grasp to ensure a comfortable, easy-to-control final hand-and-arm position. Much as the waiter and I had grabbed the glass in ways that afforded control after object *rotation*, the analogous thing was happening for object *translation* with the plunger. Grabbing the plunger low before raising it, or grabbing the plunger high before bringing it down, let the arm and trunk occupy midrange joint positions at the ends of the translations. This *grasp-height* effect, as it came to be called, was confirmed in laboratory tasks (Cohen & Rosenbaum, 2004; Jovanovic & Schwarzer, 2017; Weigelt, Cohen, & Rosenbaum, 2007).

GENEROSITY

Typically, a tribute to a great scientist concerns their brilliance and creativity, but when such a scientist also displays unusual generosity, that deserves mention, too. Michael Turvey and his partner, Claudia Carello, were incredibly generous to all those they worked with; the welcome mat they provided was wide indeed.

In my own case, I already mentioned that I was invited to give a talk to Turvey's lab early in my career. Many years later, I was invited to give a lecture about my work on object manipulation (described in the last section). I presented the research in the Geraldine Pellecchia Memorial Lecture Series on Cognition and Coordination at the University of Connecticut, Storrs (October 2008). That I was invited was meaningful given how often I had challenged the claims of the ecological/dynamical-systems approach. I was made to feel welcome, however. Clearly what mattered was the science.

I will end this tribute with a rather silly point about my visit to UConn for that lecture. I stayed at Mike and Claudia's home the night after my talk. After dinner and just when all the guests were about to descend to the famous pub in the basement for conversations that would go on late in the night, I excused myself and went to bed at my usual 8 pm. There was some good-natured laughter at what a killjoy I was, but folks there knew or were told that I was an extreme early bird.

The conversation that night went on without me, but that is the way it always must be in the end. Our conversations must now go on without Mike Turvey. Those continuing conversations and the ones to come after us will be enriched immeasurably by all he did.

AUTHOR NOTES

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