

# Twenty Years Later, Gibson's Advice Is Still Good

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*This article is part of a series commemorating APS's 25th anniversary in 2013.*



Eleanor Gibson

This year will mark the 20<sup>th</sup> anniversary of a talk by Eleanor J. “Jackie” Gibson that served as the Keynote Address at the 1993 APS Convention. It’s instructive to revisit that talk as we celebrate the early years of APS. It was an insightful commentary on the state of the field at the time by one of our most esteemed scientists, and it provides an opportunity to reflect on her contribution and our field’s progress since.

It’s important to remember that Gibson, an APS William James Fellow, was unusual for her time — a woman of achievement in a field rife with sexism. The lot of women scientists, including psychologists, had not been an easy one. Mary Whiton Calkins, Margaret Floy Washburn, and Christine Ladd-Franklin all had distinguished careers as psychological scientists, but only after mighty struggles just to receive graduate training and degrees. Calkins never received her PhD from Harvard, and Washburn was denied admission to Columbia University, which did not admit women. Yet, Washburn studied under Edward Titchener at Cornell University, where she became the first woman to receive a PhD in psychology. Both Calkins and Washburn went on to become presidents of the American Psychological Association. Christine Ladd-Franklin taught high school mathematics for several years before being allowed to take graduate classes at Johns Hopkins University, and during that time she published nine papers in professional mathematics journals. Nevertheless, Johns Hopkins still would not permit the woman to receive her PhD in either math or psychology. She went on to make substantial contributions to scientific psychology, including a prominent theory of color vision. Johns Hopkins awarded her a PhD when she was 78 years old.

I mention this history to introduce my brief acquaintance with Jackie Gibson. I was a faculty member in the Department of Psychology at the University of South Carolina when I heard that she was spending part of the winter in Columbia, South Carolina. Her son was a faculty member in the medical school

there, and she took that opportunity to escape the Northeastern winters for a brief respite each year. I tracked her down and asked her to talk to my graduate seminar. Of course, I was interested in the science she was doing at the time, but I was particularly interested in what she could say to our budding scientists about what she had gone through during her august career. It took some prodding and rather direct questioning during the seminar to get her to talk about what it was like not to be taken seriously as a scientist in her early career. Gibson, who received her PhD at Yale in 1938, came into the field at a different time than Calkins, Washburn, and Ladd-Franklin. She did her work with Clark Hull, though most psychologists would not draw a direct line between the work of the two famous scientists.

Her husband, James J. Gibson, was hired on the faculty at Cornell in 1949, but because of anti-nepotism rules at Cornell, she could not be employed as a faculty member. She told marvelous stories about her work as a research associate in the lab of H. S. Liddell, who was famous at the time for his work using classical conditioning to study experimental neurosis in sheep. Jackie hated working with the sheep, but persisted. She eventually was allowed to have a lab in psychology, where she did pioneering work on infant development. She did her great early work without the benefit of a faculty position, but a mere six years before she was elected to the National Academy of Sciences, Cornell gave her a tenured professorship. A year after her election to the NAS, she was given an endowed chair. When I asked her about her work over the 15-year period when she had no faculty position, she alluded to "A Room of One's Own," the essay by Virginia Woolf about educational and social limits placed on women: The comparison to her lab situation was obvious. Gibson was awarded the National Medal of Science in 1992 and gave the talk at the fledgling APS's annual meeting in 1993.

Her keynote address came toward the end of a long and distinguished career and reflects a bit of disappointment at where the field seemed to be heading. She spoke of the glory days of grand theorizing by her mentor Clark Hull and by Edward C. Tolman, among others. She described the giddy optimism of being a young psychologist when there was hope of a grand unifying theory of mind and behavior, much like the search for a unified field theory in physics. She expressed her disappointment about the field fracturing into ever-narrowing lines of research, and about the lack of communication among the scientists in these new and emerging niches.

Lest she be thought of as a Cassandra, she did offer some suggestions for the future. (And recall that this was 20 years ago, so we have some recent history to compare with her prescriptions and hopes.) She proposed three general strictures for the future: (1) avoid dualism, (2) avoid reductionism, and (3) quit thinking of nature and nurture as two ends of a continuum.

Much of her concern about dualism reflects the criticism of many theories of mind and behavior that require a homunculus, or "ghost in the machine." While the remaining radical behaviorists still rant at this criticism of cognitive approaches to mind and behavior, I think most researchers and thinkers today are aware that mind and behavior are materialist and must be derived ultimately from brain action. As I tell my introductory psychology students, "The mind is what the brain does." Ignoring the homunculus problem lets us think and talk about one of the hallmarks of development that Gibson discussed later in her talk: agency, or cognitive control. It is clear that such agency emanates from the brain. Not knowing the specific brain matter that leads to that agency should not prevent us from talking about the huge issue of cognitive control, which seems so important in cold as well as hot cognition and psychopathology.

In her second admonition, Gibson warned that we should be cautious about mixing levels of explanation and, anticipating the rise of brain imaging, that we should not make the mistake of thinking that understanding the brain would lead to understanding the mind and behavior. We have made great strides in understanding not only brain centers but also a growing number of networks involving those centers. However, I detect, particularly among the younger researchers in this area, an absence of understanding that mind and behavior are emergent properties not to be understood by even a complete understanding of the neural bases for these phenomena. As the director of a brain-imaging center, this philosophy concerns me as it concerned Gibson. More prosaic is the issue of what we ask the brain to do during imaging.

The bottom line is that behavioral psychology has made considerable progress in understanding different aspects of cognition, such as remembering. It would be absurd to think that knowledge of brain physiology and the keys to a scanner would allow you to study “memory” without knowledge of the different tasks used to model those types of remembering. My own concern echoes Gibson’s, particularly when I see the training of young cognitive neuroscientists who learn much about the brain, but little about the field of cognitive psychology, or more fundamentally, about how to do a well-controlled experiment about the mind.

Gibson’s third concern for the future had to do with the nature/nurture dichotomy. She argued that one cannot attribute causality to one or the other alone. Neither operates in a vacuum; both are always interactive. And 20 years later, this is still an issue. Even with the mapping of the human genome and the shrinking cost of identifying an individual’s particular gene structure that seems to reflect a type of Moore’s law, there is great disagreement about the role of nature and nurture in some of psychology’s great variables, such as intelligence and personality. Behavioral genetics shows that both of these variables have considerable heritability. However, the search for particular genes will almost certainly lead to the result that multiple genes contribute to each attribute of the two variables. Further, it is becoming increasingly clear that, as Gibson admonished, there is great interaction between genes and environment.

Jackie Gibson had great concern for the 20 years that would follow her talk. I suppose my own view is a bit more optimistic for the next 20 years. This business of understanding mind and behavior is indeed a complex business, and we should not be surprised that our science reveals that complexity.