I often regretted during the last weeks of 1970 that I had let R.F. Johnston, the editor of Annual Review of Ecology and Systematics, persuade me to write an article on feeding-strategy theory that was due by December 31. I was suffering from a merciless flu that had worn away many of the capillary walls in my nasal passages—blood-spattered papers and books converted what had once been a rather pleasant dining space into a condemned area. My motivation for compiling the thousand-odd references hunched like so many albatrosses, however, teetering piles of those references read for this review was the usual last days, however, teetering piles of those references hunched like so many albatrosses, ready to pounce seriatim and grapple me from my deadline.

Somewhere such reviews are written, and for me what eventually emerged was a well-organized, quite compacted paper, summarizing extant theory (and creating new theory) for a field begun only four years previously. In my paper I divided the theory into four parts to explain (1) why organisms select certain types of food from those encountered, (2) where organisms feed, (3) when organisms feed, and (4) why organisms feed. I think my 1971 paper is much earlier than the one I wrote, however, the first three of the four parts were specifically to explain feeding-strategy theory that was due by December 31. I was suffering from a merciless flu that had worn away many of the capillary walls in my nasal passages—blood-spattered papers and books converted what had once been a rather pleasant dining space into a condemned area. My motivation for compiling the thousand-odd references hunched like so many albatrosses, however, teetering piles of those references read for this review was the usual last days, however, teetering piles of those references hunched like so many albatrosses, ready to pounce seriatim and grapple me from my deadline.

Somewhere such reviews are written, and for me what eventually emerged was a well-organized, quite compacted paper, summarizing extant theory (and creating new theory) for a field begun only four years previously. In my paper I divided the theory into four parts to explain (1) why organisms select certain types of food from those encountered, (2) where organisms feed, (3) when organisms feed, and (4) why organisms feed. I think my 1971 paper is often cited even now, long after its supersession by more current reviews. I imagine this is in part because of the several concepts either introduced in the paper or crystallized by it. These include a rigorous discussion of the energy-maximizer/time-minimizer dichotomy (first presented elsewhere) and the first complete algebraic formulation of the "fundamental" optimal diet model (although the essence of this model was first presented graphically by R.H. MacArthur and E.R. Pianka, was first formulated algebraically in pieces scattered through an earlier one of my papers, and was eventually given its most rigorous form by E.L. Charnov and other others). Other concepts such as the relation of foraging to fitness, optimal territory size, optimal group size as it contrasts to costs to members versus outsiders, and optimal foraging periods did not "take" then, although the first three of the four did later by a route that, so far as I can tell, had nothing to do with my 1971 paper. I think my 1971 presentation, however, communicated so well because terms in the equations were decomposed until biologically identifiable and measurable variables dropped out. This great strength of feeding theory, as opposed to much ecological theory at higher levels, must surely be largely responsible for the former's differentially great success.