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COMMENTARY

Theoretical Hubris and the Willingness To Be Radical: An Open Letter to James J. Gibson

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J. J. Gibson's direct perception thesis is the cornerstone of ecological psychology. Not to understand this is not to understand ecological psychology. Beginning in the summer of 1968, when I first met Gibson, and after working with him for the next year at Cornell, I underwent a conversion crisis. I came to appreciate his thesis through a few philosophical insights that I here share with the reader through an open letter to Gibson, where I seek to illuminate the reasons for my conversion from being a Miller–Chomsky psycholinguist and a Piaget devotee to a radical Gibsonian. This conversion has influenced my work even until the present. Indeed, I am still working through its implications in all that I attempt. I share this intimate portrait of my relationship to Gibson and his profound ideas in hope that others who have struggled with his thesis might be helped along their way as I was.

Since I suspect we have all been befuddled at some time or another by the Gibsonian thesis of direct perception, let me share with you how I came to believe in it, and why I continue to struggle to make my theoretical work an elaboration of it.

Gibson (1967) was fond of quoting Kurt Lewin who said: "There is nothing so practical as a good theory" (p. 135). Psychologists have not always appreciated this fact, having become jaded by the failure of learning theory during the first half of the last century to deliver on its promise, and having witnessed the meager results

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of cognitive psychology soon afterwards, and I think, even until the present. Gibson's opinion about complacent psychologists was very critical and not always generous, saying, "On an absolute scale of intelligence, they are dullards" (p. 142). In 1967, in admonishing the field for its apparent scientific complacency, he said:

> They seem to feel, many of them, that all we need to do is consolidate our scientific gains. Their self-confidence astonishes me. For these gains seem to me puny, and scientific psychology seems to me ill-founded. At any time the whole psychological applecart might be upset. Let them beware! (p. 142)

To those who were neither complacent nor dullards, but whose caution and conservatism leads them to forsake grand theoretical schemes for more modest restricted models of "mechanisms," Gibson chided that they should not be so timid, for the goal of theory was to be "radical"—a word he liked to use for his own innovative hypotheses. In his autobiography he candidly admitted:

> As the reader may gather, I prefer radical solutions to scientific problems whenever possible. General explanations are always preferable to piece-meal explanations ("models" as they are nowadays called), and this is all that is meant by a radical theory. (Gibson, 1967, p. 134)

This is an attitude that I shared with James J. Gibson, or Jimmy, as his friends called him. And so I was never so satisfied when, after having told him about a bold new idea that would guide my subsequent work, to have him say, even if overly generous: "Bob, you're even more radical than I am. So proceed carefully with that big idea or the dullards in the field will hang you out to dry!"

I would like to tell you about my conversion to Gibson's radical idea that perception is direct, and to do this informally and somewhat intimately, so that the basic intuition that underwrites the ecological approach to psychology might not be lost among the facts and arguments that a more formal presentation would require. I use the device of an open letter to Gibson that you may read over my shoulder.

Dear Jimmy:

Recently, as I was unpacking some old boxes, some with more than a quarter of a century of familiarity, in a continuing vain effort to settle into my new home, by chance I ran across a letter I had written to you but never sent. It was written in the early fall of 1968, soon after we had first met, and expressed my admiration for your work but also confessed to being concerned about several fundamental difficulties that kept me, at that early stage of my studies of ecological psychology, from coming to an unvarnished acceptance of your notion of direct perception. In this letter, I also said I felt cursed in being bright enough to see the problem with which you were wrestling but was, apparently, too dull to comprehend the answer you had given. I needed help.

But now, after more than three decades, having come to be about the same age you were when we first met, I fancy that I have at last achieved some clarity and see that by 1979 you had provided all the answers needed to resolve my difficulties. These solutions seem clear to me now, presumably not because I have gotten brighter but, hopefully, because I have worked diligently through some of the issues that originally confused me. Consequently, in the likelihood that others may have had similar problems coming to grips with your elegant but profound thesis, I would like to chronicle my own pilgrim's progress for you and them. Perhaps it may save others precious time. Let me set the stage for my epiphany.

Jimmy, you may recall that you and Jackie [Gibson's wife, Eleanor] spent about eight weeks of the summer of 1968 teaching seminars at Minnesota where I was a virgin faculty member. Jim Jenkins, Anne and Herb Pick, and I and others at the Center for Research in Human Learning had invited the two of you, as the most impressive wife–husband team in psychology, to teach seminars at our annual summer institute. The seminar you taught was entitled "The Visual Perception of Environmental Layout." I recall this seminar vividly, for it kindled in me a fervent desire to change my course as a scientist and thinker thereafter. But I get ahead of the story.

For about two weeks in the summer seminar you had lectured us on your idea that perception was primary in that it needed no mediation by memory, expectations, inferences, or any other cognitive process. Your bald assertions created quite a hubbub among the seminar participants who were, as I recall, all cognitive psychologists by training and conviction. One claim you offered, I remember, was particularly shocking to us. You claimed that the human or animal does not have to construct an awareness of the world from meaningless energy distributions but need only detect the meaning in the invariant information conveyed by the environmental energy distributions. Even more startling to me at the time, you added, that the information did not have to be processed, it need only be detected! *It need only be detected!?* Alarm bells rang in my head for reasons I could not yet appreciate.

We were all incredulous, for what could you possibly mean by that? Didn't the information have to be processed, someone countered? Wasn't it obvious, another argued, that we constructed our cognitive maps and internal models of external reality from composing and interpreting piecemeal perceptual samples from the world's impoverished stimulation? Weren't we, as subjects, necessarily trapped in our phenomenal worlds just as the Gestaltists (e.g., Koffka, 1935), following Kant, had assumed, I argued? Surely, you could not believe that we had direct and immediate access to noumena—the unknowable, the ineffable, the reality behind phenomena that we barely glimpsed through our observations and measurements. The collective consternation among the seminar participants was palpable.

After a few disdainful remarks about Kant's dismal fallacy, you explained that you preferred not to use polar terms like *subjective–objective, noumena–phenomena,* or *internal–external* since they carried with them too much philosophical baggage. Instead *animal* and *environment,* you proposed, were better terms and quite suffi-

cient for our purposes. Most of us in the class were completely mystified about what purposes you could have in mind, so we stopped protesting and began to listen.

After attempting to explain to us the difference in defining the environment functionally at an ecological scale, as opposed to structurally at a physical scale, you went on to say that perception at the ecological scale would be direct and meaningful, while defined at a physical scale it could not avoid being indirect, requiring cognitive constructions to add the missing meaning. Furthermore, if it were true that direct perception emerged spontaneously at the ecological scale as direct and unmediated, then the problem to be understood experimentally was drastically changed. The old question of *what do agents do when they perceive?* was replaced by *how did they detect the invariants in the available information?* And, although the general answer was now obvious—namely, observers merely had to differentiate the information until it was most clear and stable in relation to the co-occurring context of action—the specific details of how they did so needed to be investigated. To achieve such clarity of perceptual purpose, learning might have to take place automatically, without demanding externally imposed reinforcement. This, you said, was in keeping with Woodworth (1947), who 20 years earlier had argued that perceiving, in the sense of detecting meaning, is its own reward.

Then you went on to state, what seemed to me, no less than a declaration of war against nearly everyone else in psychology by pointing out: If this hypothesis of direct perception were correct, then there is no need for the traditional theories of perception with their reliance on memory since all such theories will have been made quite superfluous. I seem to recall that nearly everyone's mouth was agape; I know mine was.

You further asserted that all that is needed is a good theory of how the senses considered as perceptual systems became attuned through evolution or learning, so as to resonate to the appropriate information. Such a resonant system had to be tuned to a sufficiently high fidelity to make it possible for an animal in an environmental situation, who received information specifying negative circumstances, to move to a new situation whose information specified positive ones. This "rule" for the perceptual control of action, you said, was the fundamental law of ecological psychology, and this is what neurophysiologists should search for, rather than wasting their time trying to discover the memory traces or associations responsible—since indeed there were none. As a fillip, you observed that there was the learning *of* associations rather than learning *by* associations.

This idea of resonance was discussed in the light of Karl Lashley's notion of standing wave patterns in the nervous system, and his student Donald Hebb's postulation of resonant cell assemblies and phase sequences in the brain (Hebb, 1949). In the place of Sherrington's reflex arc, echoing Dewey (1896), you insisted that the "loops" over which animals transacted business with their environments passed concurrently through the lower and higher centers of the brain, as well as through the neuromuscular system, and even more broadly through the environment–organism coupling. (Something that Neisser, 1976, later characterized as a kind of

perceiving–acting cycle or loop.) Thus in one fell swoop you united James's "stream of consciousness" and Dewey's dual "stream of behavior" into a coalesced stream of perceptual awareness that guided behavior. You had me hooked if not yet landed.

As a budding cognitive psychologist who was trained to be a psycholinguist and who truly believed at that time that "language was the best window to the mind," as a devout follower of Noam Chomsky and George Miller's formalistic style of doing generative linguistics (Chomsky & Miller, 1963; Miller & Chomsky, 1963), and as one who had just spent a year or two doing Piagetian research, your words challenged me, but I could not grasp their full implication. I felt stupid. I began to seriously doubt that language really was the best window to the mind—at least to your mind! So naturally, I argued with you, as others did who must have felt just as stupid. We complained that direct perception was simply naive realism, and as such allowed too much nonsense—dreams, illusions, hallucinations, imaginings, misperceptions—to count as real objects and that, even worse, naive realism gave no scientific account of how we came to know what was *truly* real. For that, didn't we need hypothetical constructs to run the mind's inner show?

And you, as one who always loved to argue, patiently said direct realism was the only way living systems could "keep in touch" with their everyday worlds so that adaptive acts could be successfully carried out, that vision and audition should be modeled on the so-called contact sense of touch, that the so-called proximal–distal stimulus distinction was a myth; for there is only one sensory system through which perceivers maintain contact with the ecological realities that mattered. Finally, you advised, that if we doubters paid serious attention during the course of the seminar, you would supply new reasons why such direct realism was viable as an orientation to theory in psychology. I remained skeptical, as most of the class seemed to.

Finally, after several weeks of much resistance and argument from me, by reading your books and papers, monopolizing your time for continued argument at parties, I begin to intuit something not yet clear. It was as if, by beating my head against a stubborn problem, I had finally cracked my dome to let a glimmer of light through. Consequently, at the next class meeting, I had a kind of fit of insight that I could not contain, so interrupting your lecture I fulminated: "Oh, I see, like the British philosopher, G. E. Moore's argument for the direct perception of what is good!" With a benevolent smile that I presumed you reserved for slow-witted students who finally showed signs of catching on, accompanied by an emphatic shake of your fist and a mischievous twinkle in your eye, you replied: "Exactly!" And then you asked me if I would present Moore's argument to the class at our next meeting, and explain to them the nature of my insight. I assented. Indeed, at that moment, in my delirium of delight at having scaled some conceptual Everest, I would have assented to jumping into a bonfire.

Unfortunately, over the weekend as I prepared for my talk, I grew more and more apprehensive that what I had comprehended about your thesis could not be right after all, that your idea was still some will-o'-the-wisp to be chased but

never caught, that my understanding was a mere shadow of a truth beyond my ken. But I was stuck, I had taken the bait and would have to give my talk anyway. I was troubled.

Here is the gist of that talk as I recall it.

George Edward Moore (1873–1958) was a lecturer in moral philosophy at Cambridge University early last century, a careful thinker and a much admired friend of Bertrand Russell's. In his book *Principia Ethica* (1903), Moore meant to discover, through careful philosophical analysis, the preconditions for moral conduct. To do so, he had to come to grips with the problem of whether there can be criteria for recognizing what made some acts "good" and others "bad." The main point of my talk hinged on what I took to be an analogy between Moore's program to discover the conditions under which ethical knowledge was possible and your attempt to secure the preconditions for perceptual knowledge. Most especially, I thought (a little too simply as it turned out) that you and Moore agreed that whatever is perceived is done so directly without admixture from memory, inference, or any other process. Moore's view, which was probably partly inspired by James and Peirce's pragmatism, later came to be called the New Realism, with adherents in both the United States and England (Harlow, 1931; Holt et al., 1912; Pratt, 1937).

The reasons Moore and his colleagues championed this amazing thesis was that they had a common adversary, the 17th-century British Empiricist, John Locke, whose realism was tainted by "mind–body" dualism. My dawning insight at the time was that you, like Moore, wanted a realism unbesmirched by Cartesian dualism—a view that rendered all sources of knowledge equally inaccessible because it allowed no direct epistemic contact between the knower and the known. The New Realists' analysis of mind–body dualism made it inescapably certain that no fact about the world can be known directly but can be accessed only through some mediating idea.

The basis for this pessimistic belief was encapsulated in Locke's famous dictum:

> 1. Our knowledge conversant about our ideas only. Since the mind, in all its thoughts and reasonings, hath no other immediate object but its own ideas, which it alone does or can contemplate, it is evident that our knowledge is only conversant about them. (Locke, 1690/1959, p. 167)

If Locke's egocentric predicament is allowed to stand, it says that we know our ideas only through other ideas that stand for them; this is viciously circular, for there is no independent criterion for the accuracy of what we know about things external to our own minds. This gave fodder for Bishop Berkeley's later argument in favor of subjective idealism. Apparently, Locke himself so clearly saw the danger in what he had proposed that he never returned to the issue in his later works.

Moore's tactic was to argue that Locke's concept of mind with its necessary "between-things" was wrong. Mind should be reformulated as an activity, rather than a content, where nothing is mental except acts of awareness and willing. Moreover,

under this new view, the entire content of mind is nonmental—sensa, percepts, concepts are all independent of mind, being in fact physical. Although the mind can be aware of these objects, and can contemplate them, it cannot be aware of itself since it *is* the activity of being aware (Pratt, 1937). Our awareness of such objects is immediate, and in keeping with Occam's razor, mental representations of the objects of perception are denied because they have no necessary role to play. Brentano and other act psychologists concurred with the general thrust of this argument, as you did yourself, Jimmy, when a decade after my talk you were to say:

> Perceiving is an achievement of the individual, not an appearance in the theater of his consciousness. It is a keeping-in-touch with the world, an experiencing of things rather than a having of experiences. It involves awareness-of instead of just awareness. It may be awareness of something in the environment or something in the observer or both at once, but there is no content of awareness independent of that of which one is aware. This is close to the act psychology of the nineteenth century except that perception is not a mental act. Neither is it a bodily act. Perceiving is a psychosomatic act, not of the mind or of the body but of a living observer. (Gibson, 1979/1986, pp. 239–240)

Unfortunately, I did not have these beautifully clear and concise words to guide me in planning my talk. If I had, in hindsight, I believe my uncertainty regarding your view would have been dispelled. My talk would have been a testament to my conversion. Nevertheless, I was helped somewhat by my understanding of Moore's following argument.

Moore's treatment of what can be perceived directly anticipates in a more limited fashion what you said better much later. Moore argued that we might perceive directly the meaning of situations where "good" or "bad" acts were performed because "good" was a simple, unitary "organic" predicate, without parts, analogous to, say, our experience of the color yellow, and so could not be defined in terms of anything else. (Turvey and I later were to call such "organic" predicates "complex particulars.") If so, then an experience of yellow could not be mistaken for anything else. For instance, just as the color yellow might be associated in physical space with characteristic photic vibrations, it was not the vibrations we experience but the color yellow. Yellow was simply another physical predicate like size or place. To mistake it for any of the properties with which it was associated was to commit what Moore (1903) called "the naturalistic fallacy."

On the frontispiece of his famous book, *Principia Ethica,* he cited Bishop Butler's truism as the book's caption: "Everything is what it is and not another thing." This has its counterpart in the maxim of Koffka's aphorism: "Things look as they do because they are what they are." The experience of something good was similar. For you, all experiences had this unitary character because their particulars were bound together by invariants. To claim, as the phenomenalist does, that we experience *x* by experiencing instead some *y* is to commit the naturalistic fallacy. This was

what led me to make the move from Moore's thesis to yours. I felt I had grasped at last your thought, and it thrilled me.

What theoretical hubris! Where, I wondered, did your courage come from to be so radical as to demand of yourself and others nothing less than a comprehensive theory? What made you so willing to be radical, when the whole scientific establishment and philosophical community was against such a simple if daring holistic strategy? I marveled at your intellectual courage. Three and a half decades later, I am still bowled over by it.

Only later did I discover there were flaws in Moore's argument—flaws that were inherited by the New Realist movement that grew up in this country in 1910 just after William James's death. I begin to be troubled again since I thought these flaws carried over to your ecological program as well. That summer there were other astounding revelations about the promise of ecological psychology that awakened me from my scientific and philosophical complacency—but none so deeply as this one.

If your version of the New Realist's thesis were taken to be true, then it must differ from theirs in some significant ways. After all, you did claim there were new reasons for realism. I just couldn't see what they were at the time. Let's examine the root of the problem as I saw it then.

If, as you and the New Realists argue, all we need to do is detect objects, events or other environmental situations to perceive what they mean, then each new detecting reveals a new and distinct occurrence of meaning. For to avoid the naturalistic fallacy, each object of perception must be numerically distinguished and must, therefore, be counted separately among the items of ontological furniture in the world. And this is the rub; this is the flaw in the New Realists' account of direct realism that became a sticking place for my unvarnished acceptance of your view as well. Hence my reason for the letter I began writing to you in the early fall of 1968.

My major concern at the time was that if perception were differentiation of specifics from the information resident in the ambient flux of energy, then it would exacerbate the problem by arbitrarily multiplying entities in an unbridled fashion. With each differentiation, I saw the creation of another and another and still another entity that had to be counted among the real items of experience. This made me suspect, as did your old Princeton mentor, E. B. Holt, that the New Realists had committed what Bertrand Russell had pointed out as the "Meinongian fallacy." Let me explain.

Alexius Meinong (1853–1920) was an Austrian philosopher who worked at the University of Graz. He was a pupil of Franz Brentano and is most famous for his belief in the unbridled multiplicity of so-called inexistent, or intentional, objects that nevertheless had to be counted among the furniture of the world with ordinary existent ones if perceptual experience was to be founded on a realism (Findlay, 1963). One reason that Bertrand Russell (1905) called this a fallacy was that it flies in the face of Occam's razor, which, as you know, admonishes the theorist not to multiply entities beyond necessity. This is also called the principle of parsimony, which asserts if all things are equal, then the simplest theory is the best theory. The fear that

your thesis, like Meinong's, might have crassly violated parsimony by making every experience evidence for yet another existing object, kept my allegiance to ecological psychology in abeyance for a while longer.

A word of explanation for my concern. I had read Bertrand Russell since high school, been a Russellian as both an undergraduate and graduate school philosophy major; I even named my youngest son after him. Hence I was perhaps more susceptible to his arguments than others trained only in psychology might have been. His argument that Meinong's fallacy was devastating to any theory of sensible denotation, rightly or wrongly, convinced me that the fallacy was to be avoided at all costs. The difficulties posed for psychology seemed to me dire. For if the objects of perceptual experience were arbitrary in number, with each being distinctive, then how could any agent manage to know his world? It would appear a chaotic aggregate of unrelated facts rather than a lawful system of knowledge. In short, lacking apparent structure, it would surely be an undifferentiated mess. Your claim that the mess could be cleared up by differentiating out still more specific detail promised, so it seemed, only to make matters worse.

It is easy to see with hindsight how wrong my thinking was at the time because today we know the answers … invariants, invariants, invariants! Invariants over the five kinds of sensory experiences resolve the issue of how modal particulars become relevant and make an experience organically whole; invariants over adjacently and successively ordered experiences resolve their multiplicity into coherent objects and continuous events, respectively. The socially shared meanings known as affordances, as invariants of invariants, offer all creatures with the proper scale, attunement, and motives a democracy of opportunity. Moore and the other New Realists missed the fundamental assumptions that might have saved their program. You didn't!

Now, with my Meinongian nightmare receding into the past, I marveled even more at the simplicity of the idea you had sketched, an idea so simple that if true, then no theory of processing or property binding ever again would be required to explain how we arrive at our perceptions of the meaning of objects or events in the world. We merely have to notice what they mean, an outcome achieved readily by differentiating the ambient information available surrounding every source object until we reach a rock bottom state of clarity. Perceptual systems can be likened to homing devices that hunt through the available information for useful meaning. The Swiss artist, Paul Klee (1920), one of my favorites, summed it up when he said that the purpose of art "was not to copy the visible but to make visible" (p. 56).

I have spent many sleepless nights reflecting on this simple Humean idea that perception functions to focus information until environmental facts are clear and stable, debating with myself over whether it is ridiculously simplistic or profoundly simple. I have now come to believe that it appears so simple because it is correct. But again, I get ahead of my story.

I never mailed the letter to you because you called to ask if I would like to come to Cornell as a visiting faculty member for the academic year 1969–1970. My salary

would be paid by Cornell if I would teach the undergraduate perception class and assist you in teaching your graduate seminar. David Lee, who would also be visiting Cornell for similar reasons, was to be my office mate at your and Jackie's lab located off-campus at the Tompkins County Airport. I was much surprised and flattered to be asked to come but told you that I had to decline because I had never even had a course in perception. Your reply was immediate: "Then all the more reason to teach a course in perception since that is the best way to learn." And in any case, you added to settle me down, I could use your notes. That cinched it for me. I couldn't wait to see the notes you had amassed over nearly a half century of thinking about such matters. Perhaps, there under your tutelage for a whole year, I might find a way out of my philosophical quagmire.

Consequently, after consulting with my wife, I went to my chairman and requested a leave-of-absence to go to Cornell for the next academic year, but to my dismay, he said no I could not be spared. I told my chairman that I felt I had to go anyway, and he said, "Then do not expect to return to this faculty!" I went anyway.

Once at Cornell, I became quite nervous about the perception course I was to teach because I was certain the bright undergraduates would discover me to be a fraud. I asked for the notes you had promised, and you rummaged around on your shelves, finally coming up with an extremely thin folder, having but a few pages in it. Your notes were sparse, to say the least, being merely reminders to yourself, for example, to tell the class about Helmholtz (1925), or to discuss the implications of Berkeley's new theory of vision, or Ivor Kohler's inverted prism experiments. I was aghast! Seeing my dismay and agitation, you assured me however that we could discuss in detail any such topics on perception for which I needed help. This proved a blessing in disguise, for it gave me easy access to you in spite of your busy schedule. In the end, you were right, in spite of my temerity, I did learn a lot about perception by teaching it; but I learned much more from our after-dinner discussions at your home, which always took us well past midnight and often into the early morning hours.

One of the topics on which we repeatedly burned the midnight oil was my nagging suspicion that a Meinongian fallacy may be lurking in the assumptions of your program. Resolving this fear, I pleaded with you, was to prove of critical importance in pushing me through the conceptual barriers that blocked me from full and unreserved acceptance of your direct perception thesis. To this end, we discussed William James, G. E. Moore, Bertrand Russell, and especially the American New Realists' (1910–1912) movement. We considered where they went wrong, after so promising a beginning. I do not remember us coming to agreement at that time, about how the fallacy was to be avoided, but we did agree that it should be. I gradually came to suspect, on intuition alone, that you actually did have the answers needed to do so, but we seemed unable to frame them clearly in words.

I talked to your students and some of your colleagues about this stumbling block to my belief in direct perception, but none seem to care about the problem one way or the other. Either they were true believers in spite of the apparent flaw or they did

not see that such a flaw could be logically devastating to a theory of denotative meaning. (Such attitudes can be forgiven, for technically speaking, the ramifications of the Meinongian fallacy pose such severe difficulties that some philosophers have abandoned traditional logic to avoid it and have sought refuge in the possible worlds heresy, e.g., David Lewis, 1986.) There was one exception however. Your good friend and colleague, Robbie MacLeod, the great phenomenological psychologist. He listened carefully, advising me to pick at this wound until it either became worse or it healed itself. It did heal itself because sometime during my stay at Cornell that memorable year, I eventually came to see how your unfolding ecological program might overcome this objection.

After the completion of my epiphany that began in the summer of 1968 and ended sometime in the spring of 1970, I never looked back until now. I have busied myself emulating the theoretical hubris that you taught me, to elaborate on your program in ways that I thought it needed … and damn the carping dullards!

To your notion of event invariants, I have tried to find the formal concepts in symmetry groups and transformation groups needed to defend it. I came to suggest that events involve two kinds of invariants where others saw but one—transformational invariants over successively ordered experiences and structural invariants over adjacently ordered experiences (Kim, Effken, & Shaw, 1995). I came to see that affordances as dispositions needed to be balanced by effectivities as processes that realize the affordances when they are the goal and use other affordances when they are ancillary to the goal. I came to believe that the perceiving–acting cycle was best conceived of as a path propagator in a path space–time, sculpting away from a background of noisy excitation to get at the clear and stable meaning to actions (Kadar, Shaw, & Turvey, 1997; Shaw, Kadar, & Turvey, 1997). I have come to believe that what is perceptually differentiated is a generalized action quantity that comprises both information and control coupled invariantly whenever goal-directed actions prove successful. If so, then I see the possibility for there being a conservation law in psychology that holds for agents operating in intentionally bounded workspaces.

But most of all, I have tried to deliver on that promise I made to you to go carefully with the biggest idea of all, namely, that invariant properties that are perceived directly are not equivalence classes, as nearly everyone believes, with their intrinsic transitivities. For these transitivities allow mediation to occur and thus take us back into the swamp of reactionary cognitivism with its mediational quicksand. Instead, I believe these invariants are best modeled as duality classes such that being only symmetrical and reflexive they do not allow mediation to occur.

Have I gone too slow in developing this big idea that you so generously said was more radical than any of your own? I believe now, as I did then, that this idea cannot be more radical than your own because it was your own in conception if not in words.

In closing, let me wrap this letter back around to 1970.

As it turned out, my chairman's bark was worse than his bite, for at the end of my sojourn in Ithaca, in the summer of 1970, as you know, I did return to re-

sume my assistant professorship at Minnesota. Apparently, my chairman's change of heart was helped along by the offer I had received toward the end of my stay to join the faculty at Cornell. After much soul-searching, I turned the offer down because of my commitment to finish the work that Jim Jenkins and I had underway at our Minnesota center. As you know, Jimmy, since I imagine you and Jackie had a hand in it, Cornell at one point even tried to hire Jim and me together to establish a center there, but in the end, the idea fell through. You would be pleased to know that we now do have such a center at Connecticut and it is ecological. Over the years, I have often wondered (especially when having a commemorative Manhattan cocktail) what my academic life might have been if I had worked with you at Cornell for the decade that you had left. No doubt my progress would have been better, or at least more assured. But would we, without the synergy of Bill Mace, Mike Turvey, Eleanor Gibson, Dick Neisser, Claire Michaels, Claudia Carello, Sverker Runeson, Ed Reed, Len Mark, and others, have produced a book series, an international society, a journal, annual national meetings, biannual international conferences, and centers all aimed at passing your and Jackie's heritage to younger generations? I would like to think you would have approved and have forgiven me for not accepting the most generous offer to join you and Jackie at Cornell.

Jimmy, here is the end of the story that you were not around to witness. In December 1979, while lecturing at my alma mater, Vanderbilt University, on Gibsonian theory, I received a phone call telling me that your struggle with cancer was over and that you had passed away. Although greatly saddened, I was not terribly surprised since, at Jackie's request, as you will recall, Bill Mace, Mike Turvey, and I had visited with you a few times in the months before your death. We were amazed that you would get out of bed and come to the living room to argue with us about psychology and try again to keep us on the right track. Five minutes into the discussions, we forgot your condition. Your body may have been failing you but your mind never did.

Soon after the first phone call notifying me of your passing, I received a second asking if I would join your old friend Henry Gleitman and your friend and colleague Dick Neisser in the Sage Chapel at Cornell as one of the three speakers at your memorial service to be held in a few days. I was surprised and honored to do so. I asked why me, and received the reply that you had said of your friends and students, I was the most radical. In my vanity, I took that to mean the most like you in this favored dimension. Oh, were it so!

Of the three talks at your memorial service, I was particularly struck by the eloquence and appropriateness of Dick Neisser's since he, having been one of the chief proponents of the cognitive revolution in the previous decade, was now apostate to that earlier calling. Dick told the gathering that soon after coming to Cornell in the late 1960s, he found himself terribly disturbed, "waking up in a cold theoretical sweat thinking, 'but suppose he [Gibson] is right!'" Neisser concluded: "He was right. His rightness has already made a difference to psychology, and will

soon loom larger for all of us" (Reed, 1988, p. 217). And you have continued to do so, even into this new millennium.

It is now apparent that the antidote to our "cold theoretical sweats" that you provided by example is theoretical hubris, a willingness that in spite of the carping dullards, to seek radical solutions!

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