

The Agent–Environment Interface: Simon’s Indirect or Gibson’s Direct Coupling?

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A fundamental problem for ecological and cognitive psychology alike is to explain how agents are situated, that is, functionally coupled to their environments so as to facilitate adaptive actions. Herbert Simon (1969/1996) argued that such coupling is artifactual (rule governed), being mediated by symbol functions and necessarily involving information processing. An alternative to this computational approach is offered by James Gibson’s (1979/1986) view that the interface is natural (law governed), being a direct informational coupling rather than a symbolically mediated one. This latter view necessarily involves the agent’s awareness, whereas the former, being mechanistic, does not. I review the coupling problem from historical, logical, and semantic perspectives. I give arguments that the computational approach provides an inadequate account of situated adaptive actions and founders on the symbol grounding problem, whereas the ecological approach does a better job on both. Personal comments are interspersed throughout, providing an autobiographical perspective on issues germane to these topics.

Perhaps the composition and layout of surfaces constitute what they afford. If so, to perceive them is to perceive what they afford. This is a radical hypothesis, for it implies that the “values” and “meanings” of things in the environment can be directly perceived. Moreover, it would explain the sense in which values and meanings are external to the perceiver.

—James J. Gibson (1979/1986)

But, actually, an affordance is neither an objective property nor a subjective property; or it is both if you like. An affordance cuts across the dichotomy of subjective–objective and helps us to understand its inadequacy. It is equally a fact of the environment and a fact of behavior. It is both physical and psychical, yet neither. An affordance points both ways, to the environment and to the observer.

—James J. Gibson (1979/1986, p. 129)

An artifact can be thought of as a meeting point—an “interface” in today’s terms—between an “inner” environment, the substance and organization of the artifact itself, and an “outer” environment, the surroundings in which it operates. If the inner environment is appropriate to the outer environment, or vice versa, the artifact will serve its purpose.

—Herbert Simon (1969/1996, p. 6)

Ironically, affordances, far from removing the need for internal representations, are carefully and simply encoded internal representations of complex configurations of external objects, the encodings capturing the functional significance of the objects.

—Alonso Vera and Herbert Simon (1993, p. 41)

Contrary to Gibson’s (1977) view, the thing that corresponds to an affordance is a symbol stored in central memory.

—Alonso Vera and Herbert Simon (1993, p. 20)

Sir, I have found you an explanation, but I am not obliged to find you an understanding.

—Samuel Johnson (Boswell, 1791/1998)

In this article, I give, in part, an eyewitness account of some of the events and attitudes surrounding the original publication of Simon’s (1969/1996) influential book *The Sciences of the Artificial* and furnish an appraisal of where things now stand after the publication of the third edition of this book in 1996. The tone of many of my remarks are personal and should be recognized as such. It is hoped that an occasional first-person voice will help the reader recognize that these issues of 3½ decades ago are still unresolved and of paramount significance to our field today. The autobiographical thread, I hope, will also convey some of their historical vibrancy without being too discursive (of course being somewhat discursive is in the nature of chronicles).

The preceding epigraphs provide a convenient summary of the main issues and attitudes to be discussed. Please read them carefully, and return to them every so often as the commentary develops.

I offer a final word: One position is a foil for another if it makes the latter position seem better by comparison. In this commentary, Simon's (1969/1996) computational view is used somewhat mercilessly, I must admit, as a foil for Gibson's (1979/1986) ecological view, which he sharply attacks (Vera & Simon, 1993). A more sympathetic and, I hope, balanced account of these same issues can be found in Shaw and Shockley (2003), and a more comprehensive account of issues touched on here can be found in Shaw (2001) and Shaw and Turvey (1999).

SETTING THE STAGE

The late Herbert A. Simon, a Nobel Laureate for 1978 and one of the founders of computational psychology, was the quintessential representationalist of the extreme computationalism variety. In 1957, according to Dreyfus (1972), Simon had prophesied that within 10 years most theories in psychology would take the form of computer programs. Although much progress has been made in cognitive science with respect to computational techniques and the general use of computers in psychology has explosively expanded, nothing even close to Simon's prediction has yet materialized. For Simon was not predicting merely the widespread use of computers in psychology but that programs themselves would be devised that adequately modeled human thought, perception, and action.

Yet surprising to me was to discover that Simon was nevertheless somewhat ecological in his attitudes and sensitivities. This does not mean he was a Gibsonian or even sympathetic to the program, for he had strong misgivings and was the primary architect of an alternative approach that was quite antithetical to it. This article provides a commentary on some of the chief ideas of the third edition of Simon's (1969/1996) *The Sciences of the Artificial* published in 1996 but tailored to reveal certain fundamental similarities and contrasts with the thought of James J. Gibson (1966, 1979/1986). Hence, there is no attempt to review the book in its entirety.

Also, much of this commentary has been motivated by an important article by Vera and Simon (1993), which appeared as the target of discussion in a special issue of *Cognitive Science* dedicated to the topic of situated action. As far as I know, this article is the only place in which Simon contrasts his computational psychology with Gibson's ecological psychology, especially challenging Gibson's interpretation of "affordances" and repudiating his notion that perception is direct and unmediated. I found it more than a little curious that Simon would on one hand accept the legitimacy and usefulness of the affordance concept, and reject the main reasons Gibson gave for introducing the term on the other. What hubris allowed him to do such a thing?

Consequently, exploring Simon's anti-Gibsonian but quasi-ecological attitudes are the main topics of this article. In the process, I use the occasion to defend Gibson and attempt to clarify his views along the lines that my colleagues (Michael Turvey and William Mace) and I have been developing for about 3 decades. Whether such efforts have made us neo-Gibsonians rather than strict Gibsonians is a fair question. Of course, I believe we are interpreting Gibson rather than revising him, but this is more a question of scholarship than intent. I sometimes think that had he lived until now, Gibson himself may have become a neo-Gibsonian, even a revisionist, as he had done at least twice before. Gibson would have continued to refashion old ideas and to introduce new and exciting ones. Hence, we would be in the best of company.

An inherent limitation of the Vera and Simon (1993) criticism of Gibson's theory is that apparently their only source was an early chapter on affordances published in Shaw and Bransford (1977). I seriously doubt that this early source is an adequate substitute for having read Gibson's (1950, 1966, 1979/1986) books in their entirety—many times! This was Vera and Simon's choice, and although this narrowness in scholarship makes little logical difference to the argument they make in defense of their own position, it does make their arguments seem more ex cathedra than enlightened and their criticism of the ecological approach more cavalier than reasoned. Often one receives an indelicate whiff of *argumentum ad fiat*.

Nevertheless, we should appreciate the criticism because serious attempts to answer critics' complaints can help raise the level of mutual understanding of the participants while helping neutral parties better grasp the significance of what each position stands for. An immediate dividend gained from these two sources alone, Simon's (1969/1996) book and the Vera and Simon (1993) article, is recognition that a *prima facie* case can be made that Simon's computational psychology is both unexpectedly *ecological* and unavoidably *situated*—in the current use of the terms. Indeed, Simon was forced to make this begrudging admission to fend off critics' charges that his theory of information processing was too narrow. The generous reader might see adumbrated in Vera and Simon the harbinger of a computational ecological psychology—a thesis more deliberately proposed and adroitly defended by Wells (2002) and far better explored by Clancey (1997) and Clark (1997). Effken and Shaw (1992) also discussed the “new AI [artificial intelligence]” from an ecological psychology perspective.

In the late 1960s I was a neophyte in cognitive psychology when the movers and the shakers of the new field began contesting the most promising avenues for its future development. Reviewing these alternatives will help psychologists assess our progress as a science and perhaps suggest what still remains to be done. Considering the competing forms of computationalism will also put Simon's extreme computationalism in perspective by revealing that a less popular computational philosophy existed that was more congenial to ecological psychology than either the simulation or AI approaches. This third alternative was called the *synergy* approach (unrelated to Haken's synergetics approach). I fancy

that had it been in the driver's seat for the last 30 years rather than AI, we Gibsonians might look today on efforts to develop a computational ecological psychology with greater approbation and less pessimism.

DIVERSE COMPUTATIONAL PHILOSOPHIES

In 1968 I was invited to lecture at the California Institute of Technology (Cal Tech) on the application of abstract machine theory to cognitive psychology—my unique forte at the time. Cal Tech, Massachusetts Institute of Technology (MIT), and Carnegie Mellon University were each home to one of three fledgling computational philosophies and therefore were struggling for hegemony in the new field. My host explained to me how Cal Tech's philosophy differed from its two competitors:

Imagine you are given a contract to develop a computer application for playing chess as expertly as possible. Depending on your venue, there are three strategies you might consider: the simulation approach under development at Carnegie Mellon (by Simon and Newell), the artificial intelligence approach being promoted at MIT, or, finally, the synergistic approach favored here at Cal Tech.

He went on to explain that at Carnegie Mellon the computer model would likely take the form of a program designed to simulate the play of a Russian grandmaster on the assumption that human experts have insights and competencies that a machine did not but might be programmed to emulate. By contrast, if you were at MIT, you might instead apply AI techniques to try to build a program that played the best game of chess possible, whether or not it did so as humans do—all that matters is that it play brilliantly.

At Cal Tech the computational philosophy differed from those of the other two universities. The aim was not to simulate human experts, or even to create artificial experts, but to treat the computer as a tool designed to serve the purposes of human agents rather than to be a stand-alone device capable of autonomous goal-directed activities. Hence the program, if successful, should couple the computer with a human expert so as to create a maximally effective synergistic partnership. As a synergy, the successful human-computer system should play a better game of chess than either a human expert or computer expert alone. A successful synergistic program is designed to facilitate the interactive accommodation of the best features of both components while allowing them to compensate reciprocally for each other's intrinsic limitations.

Whereas the aim of the first two strategies is to contrive context-free computational mechanisms that can be plugged into any chosen task domain—chess playing, medical diagnostics, or whatever—the synergistic approach cannot aim for context-free mechanisms because its program cannot, in principle, stand alone. It depends on the expertise of a cooperative human agent. Essentially, it seeks to form

a human–computer partnership. Compared to simulation and AI programs, this lack of perfect autonomy may be seen as detracting from the usefulness of synergistic programs because they depend on a human interface to interact with real-world task situations.

This complaint that the synergistic models lack autonomy would be legitimate if it were avoidable and if the other two approaches were unqualified successes—an opinion that even the staunchest proponents would have to admit is arguable. Their autonomy is limited no less than the synergistic strategy by a need to be interpreted, debugged, attuned, and reattuned to real-world situations by a human agent. Hence, the demonstrable lack of autonomy of each kind of model could be taken as evidence that favors the validity of the synergy method because of the three views, only it recognizes the recalcitrant fact of failed autonomy—indeed, it is designed to exploit this fact.

On the other hand, if all the synergy style of modeling amounts to is business as usual with computers, such business as is required for all programming—non-modeling as well as modeling tasks—then it hardly counts as a distinct modeling strategy. However, one may argue that because a synergy is sought that essentially seeks to form a human–computer partnership—a kind of cooperative, “social” dyad, as it were—then this is a bit more than business as usual with computers. It requires a specific theory and clever solutions to some outstanding human–computer interaction problems—problems that today are referred to as situated action and situated cognition problems. Did the synergy approach foreshadow a computational approach to ecological psychology? At least one might argue that although their philosophies and methodologies may differ, the scientific feasibility of one is logically linked to the other because they both depend on modeling a “smart” coupling between agents and their environments.

Next, I take a look at Simon’s (1969/1996) information processing approach as represented in his book and in Vera and Simon (1993) and simultaneously, as promised, use it as a foil to help clarify Gibson’s view.

SIMON’S BOOK IN THE CONTEXT OF THE TIMES

There is much to admire about Simon’s (1969/1996) modest little book, *The Sciences of the Artificial*, originally published in 1969 and now in its third edition. This book stimulated the interest of many psychologists and social scientists in the budding field of cognitive science. The author blessed us with elegant, uncluttered prose so one might better concentrate on the substance of his hypotheses and arguments without being distracted by too much niggling detail. He spoke simply and directly with the authority of one who truly believes in the promise of a fledgling field and yet is quite aware of its difficulties and modest accomplishments. Most remarkably, he managed to communicate the book’s technical thesis without pages of dense equations, programs, or extensive flow diagrams. The language is nearly

jargon free, therefore, even today one might well recommend it to those who wish a relatively painless introduction by a major founder of the field to the origins and aspirations of the field of AI during the period of its inception.

Simon's (1969/1996) book was revised in 1981 (second edition) with the addition of an important chapter, "The Architecture of Complexity," and again in 1996 (third edition) with another new chapter, "Alternative Views of Complexity," being inserted to introduce the former chapter. This newest chapter considers concepts and tools for the analysis of complexity but in no way qualifies Simon's previous thesis or blunts the message of "The Architecture of Complexity" chapter. Simon's innovative thesis is that life forms are basically simple, inheriting their apparent complexity from their environments. Simon said: "The thesis is that certain phenomena are 'artificial' in a very specific sense: They are as they are [e.g., complex] only because of a system's being molded, by goals or purposes, to the environment in which it lives" (p. xi).

This thesis stands in refreshing contrast to the typical cognitivist thesis that the mind and brain system is inherently complex, with layers on layers of computations making it perhaps more complex than anything else in nature. For Simon, however, such complexity is more apparent than real—being true of the expressed behavior, whereas the computations that produce that behavior remain simple. To this end, Simon constructed a syllogism to support his belief in the following man-machine analogy.

First Premise

As we succeed in broadening and deepening our knowledge—theoretical and empirical—about computers, we discover that in large part their behavior is governed by simple general laws, that what appeared as complexity in the computer program was to a considerable extent complexity of the environment to which the program was seeking to adapt its behavior. (Simon, 1969/1996, p. 21)

Simon parlays this observation into what is, for him, an important methodological insight: namely, that the relation of program to environment invites us to see computers as being organized like human programmers—at least intellectually. If so, then the next major premise follows.

Second Premise

For if it is the organization of components, and not their physical properties, that largely determines behavior ... then the computer becomes an obvious device for exploring the consequences of alternative organizational assumptions for human behavior. (Simon, 1969/1996, p. 21)

Thus, psychology need not wait for solutions to problems of neurological organization but can move forward by studying how computers do what they do whenever they do anything that humans do. This brings us to the final premise.

Third Premise

But I have argued that people—or at least their intellectual component—may be relatively simple, that most of the complexity of their behavior [like that of computers] may be drawn from their environment, from their search for good designs. (Simon, 1969/1996, p. 138)

From these premises Simon asserts the following conclusion.

Conclusion

Instead of just studying experimentally and observing naturalistically how humans adapt to a complex environment, one might instead and more conveniently study computer simulations of such adaptive behaviors, with realistic expectations of discovering the design principles underwriting their organizations (Simon, 1969/1996).

I now take a closer look at some of the problems raised by this simulation thesis.

GROUNDING AND SITUATING OF SYMBOLS

A potential criticism of Simon's simulation thesis is that it fails to address two very important issues.

First, how do symbols conjure up in our understanding the objects, events, or circumstances to which they refer? If we label something, then the label is the symbol and the something is the referent that grounds the symbol's meaning in some original denotative context. Without such grounding, the symbol's meaning is left "free floating."

Second, and of equal importance as the grounding question, is to ask, "How is a symbol situated?" Situating a symbol requires establishing its conventions (rules) for use in different contexts. Whereas the grounding involves a symbol's denotative contexts, situating involves its connotative contexts. Consider an example.

If I tell a friend who is a healthy, robust athlete that he looks well, it means something quite different than my telling the same friend after a grueling battle against cancer that he looks well. In both cases, although denotatively speaking we refer to the same thing—our friend's state of health—common usage allows for connotative differences; these connotative differences are revealed by making explicit the tacitly agreed-on implicit phrase "under the prevailing circumstances and standards" (e.g., referring to *fitness*₁ standards, as applied to athletes, as opposed to *fitness*₂ standards, as applied to terminal cancer patients). If we interchange the sit-

uations and use the wrong standards—*fitness*₁ in the place of *fitness*₂ and vice versa—our statements would be inappropriate and misleading, although they continue to refer to the same grounding, namely, my friend's state of health.

Thus, there are two kinds of conventional rules involved here: Grounding requires rules of reference that specify what the symbol denotes (refers to), whereas situating requires rules of usage that specify what the symbol connotes (means in context). Usually such rules are not explicit but come as part of the presuppositional framework shared by native speakers of the same language culture.

Furthermore, as a shorthand for the previous discussion, I shall speak of such denotative and connotative conventions being “reducible” for a symbol if both its grounding referent and situating contexts can, in principle, be made explicit by perceptual means (e.g., by demonstration or example). I shall have occasion to return to the grounding and situating of symbols, in cases in which their conventions are either reducible or irreducible, as the case may be.

In anticipation, a case is made that unless direct specification (in Gibson's sense) is involved in both the grounding and situating of symbols, Simon's simulation approach is unable to guarantee that the involved conventions are reducible. Consider a case that illustrates how ungrounded symbols might nevertheless take on a life of their own when situating efforts are rather extreme. The main point of the example is to suggest a way that symbolic representations (internal models) may assume a kind of fictive or virtual reality even when their conventions are irreducible.

COST OF MAKING UNGROUNDED SYMBOLS COME ALIVE

Imagine a slightly delusional bride who, as an orphaned child with no known relatives and to avoid loneliness, invents a fictitious sister as a playmate. She convinces her new husband that although she has no sister, he must act as if she did if he wants her to be happy. Because he wishes only for her happiness, he agrees to cooperate in every way. Thus, they set out to orchestrate carefully all the various and sundry ways that others might be persuaded of her existence. They invent a place for her in their lives thereafter, even setting an extra place for her at the dinner table. They are so convincing in their scam that friends, neighbors, and the husband's relatives come to believe in the fictitious sister's existence. Over the years this ruse begins to steamroll, precipitating a full-blown clinical case of *folie à deux* (shared delusion).

For instance, they begin to place presents for the mythical sister under the Christmas tree, put her in their wills, get her a post-office box, subscriptions to magazines, memberships in social organizations, a place on donor lists of charity and political organizations, a social security number, an e-mail address, a Web page, a driver's license, declare her beneficiary to their insurance policies, co-owner of

their home, a telemarketing job that she does from her home, payment of her taxes, a disabled dependent on their income tax, collect her social security retirement checks, and finally—because all good things must come to an end—a funeral, complete with the burial of her coffin marked by an elaborate headstone, followed by the lighting of votive candles in the cathedral and annual pilgrimages to weep by the grave side on the anniversary of the sister’s “death.” In this way, we can see how symbols, although merely virtual entities, may become situated in the real world by taking up actual space in virtual environments (i.e., being listed, counted, and corresponded with), having an institutional, social, and familial identity, and using actual resources.

As pointed out, grounding and situating symbols necessarily involve reducing conventions through perceptual means. One criticism of Simon’s simulation strategy is that it does not involve such perceptual means where they are most needed. Here is one reason for this criticism.

It is often said that the proper study of mankind is man himself; Simon suggested the corollary that a proper study of mankind might also be computers that may serve as a convenient source of solutions that also apply to humans. The study of the artificial is a surrogate for life forms. This simulation strategy has been attacked for being too reductionistic in flavor (Clancey, 1997; Clark, 1997; Rosen, 1991). For if we study only the intellectual analogies between artificial and human systems, are we not ignoring the embodiment problems in which actions are guided by perception in real-world situations? Simon did not suggest that this should be our only study, but his emphasis is clearly on the side of intellectual modeling rather than situated modeling, and his technique of choice is clearly simulation. This approach favors the goals of systems that stand alone, outside of contexts, and belies any claim of strategic interest in situated cognition.

As is seen a little later, ecological psychology takes an entirely different tack, being more in line with the notion of synergy modeling with its natural concerns for how systems are situated. Under attack from theorists who thought he had shortchanged the problem of situated cognition and situated action (Clancey, 1997; Costall & Leudar, 1996; Greeno, 1989; Lave, 1988; Suchman, 1987), Simon made a belated attempt to show that his approach had always been in line with situated action and even to some extent with ecological psychology (Vera & Simon, 1993). Simon’s critics were not appeased.

Was Simon justified in his claim of being a proponent of ecological or situated theory? I consider his case.

SIMON’S BRUSH WITH ECOLOGICAL PSYCHOLOGY

Another way of framing Simon’s thesis is to say that the agent is made to look complex by using relatively simple decision making and control capabilities in response to the complicated demands and local vicissitudes of the task environment.

Viewed this way, the complexity arises in situ and de novo at each engagement of a new task but is not present in the agent per se otherwise. If so, Simon's syllogism implies that very simple rule systems might be used to simulate goal-directed behaviors even though the behaviors themselves appear inordinately complex. This however puts me in mind of something I once said.

Once as a budding young cognitive psychologist, I was challenged by a radical behaviorist of some repute to explain how the new psychology would explain the abundant evidence supporting stimulus-response association theory. My brief answer was quite irksome to him. I said, "I think association theory only applies when the 'mind' is turned off." One might likewise conjecture that, according to Simon, the principles of computational psychology appear in their most elegant and unvarnished form when the system fails to engage a situated task, that is, when the requirement for being situated is also "turned off."

If so, in Simon's view, then a theory of complexity is needed for the environment and for understanding the agent's interactions with task situations but not for understanding the agent taken in isolation. The agent, whether computer or life form, will be a simple system of rules. This is Simon's ploy for avoiding complexity that has so bedeviled simulation theory. However, do agents and their agency as psychological creatures functioning in real-world tasks have any meaning when they are de-situated and not ecologically involved? Is a simple rule system even capable of being an agent in the full psychological sense of the word? I consider the daunting requirements.

Local exigencies are the inevitable demands placed on an agent in controlling its behavior by the details of the current context that may thwart or misdirect its efforts. *Local expedients* are the tailored solutions that the agent brings to bear to nullify the local exigencies and thereby accommodate its behavior to the current task demands. *Situational dynamics* is a term I use to denote the processes by which such context conditioned variability is imposed on the agent's behavior by the perturbing details of real-world contexts (e.g., a bicycle rider zig-zagging to avoid potholes while trying to stay on course). Such perturbing details are surely typical and ubiquitous.

I can now summarize what I mean by situated action, that is, the actions of a situated agent: To be a situated agent, a system must apply local expedients that are capable of overcoming local exigencies that arise inevitably from any real-world situation. Notice that by situating agents one situates all the agent's capabilities at once; hence, situated action, situated perception, situated cognition, situated problem solving, and so forth are as unnecessary and as misleading as early attempts of psychologists to isolate mental faculties. Such attempts are a throwback to the reductionistic strategy of mechanists who believed that to divide was to conquer. It now seems that such divisions simply eliminate the truly interesting and challenging properties of complex systems. By situating the agent, it is unnecessary that each of these faculties be treated separately. Ecological psychology should have as its goal a theory of situated agency at the ecological scale, and let the rest follow as it may.

Here is the major thwart to a simulation strategy. No rule for predicting the contextual exigencies can be written; no way is known for simulating specific contexts because their denotative richness and their connotative implications are unbounded. A theory of contexts, as characterized by different kinds of challenging details, would be just as hard to come by as a description of the commonsense knowledge used in different real-world (connotative) contexts. Both are too broad and unpredictable to be simulated.

Nature, however, has solved this problem by endowing agents with perceptual means to become situationally aware of those affordances that serve the agent's interests (intentions) and sufficiently effective in their actions to realize the relevant goals afforded. Furthermore, being situationally aware is a simpler requirement for agents evolved for such duty than being computationally able to simulate such a capability. The inability to achieve the latter has nothing to do with having the ability to achieve the former.

Whereas a programmer must be able to describe all relevant situations before they can be entered into a simulation program, agents need only be aware of what the details afford. Indeed, awareness is needed to apply any rule, even a simple one. This clearly cannot be accomplished by writing a prior rule, for such leads to an infinite regress without awareness ever becoming grounded. *Hence, it is a gross and flagrant error to think that there can be a rule-based simulation of awareness when awareness is needed for the detail to which the rule must apply.* This point cannot be emphasized too strongly!

Being situationally aware of all relevant detail may be like having a look-up table with endless entries prescribing what is to be done when certain thwarting detail is encountered in every case imaginable; but how would the look-up table ever get constructed for each agent, and even if it did, how would the retrieval issues get resolved? An agent who had to consult such a table would be stultified at every move seeking the proper prescriptions for behavior. An agent who had to have such a table to act would be forever exploring every context with every possible intent in mind to see what detail might be appropriate to that given intent. There must be a better way. Consider the attempt to simulate a master chess player.

It is cheating to begin with the playing of a game, for the first requirement is for the agent to recognize in what situations it is appropriate to play the game. Just seeing a chessboard and pieces is not sufficient. Is it your board or someone else's (e.g., on the counter at a store, or one someone else is getting ready to use, or one belonging to a person who wants to play but is known to become homicidal when he loses)? How well should you play against a young child, against a person who is terminally ill that you would like to cheer up, against another master? In tournament play, which classical strategies should you choose at which point in a game against whom? A simulation must have a look-up table for all strategies, indexed against all social situations on all different occasions, played under all possible varieties of provisional conditions. Describing all the relevant detail and factors needed to situate a symbolic agent in a virtual game is only feasible because the virtual situation is virtual and not real.

In real-world situations it would be wise to include a real agent in the loop as the synergy approach admonishes so that at least one system component might contribute the requisite situational awareness—especially because there is no substitute. Computational resources might then be used to help focus the agent where needed or broaden its perspective.

In short, even assuming that rules for simulating the awareness of real-world contexts were possible, the list of ad hoc provisos that would be needed to allow a programmer to take into consideration all situations and their provisions is a demand impossible to fulfill operationally. The fallacy committed by thinking that such unrealistic demands might be satisfied is known as Tristram Shandy's paradox—the fallacy of trying to describe in words (or formulae or programs) that which is experienced in real time in real-world situations. Because any radical simulation project inescapably encounters this paradox, we would do well to examine it.

THE TRISTRAM SHANDY PARADOX

From 1760 to 1767, Laurence Sterne (1760–1767/1983) published nine volumes of a comic novel titled *The Life and Opinions of Tristram Shandy, Gentleman*, as an “autobiography” of Tristram Shandy who is committed to the impossible task of omitting nothing from his life's story. Sterne's volumes are actually a meditation on performatory storytelling putatively describing, in nonlinear episodes, Shandy's view of his own life. Sterne has Shandy write so slowly that it takes him a year to complete only 1 day in his life; given the unfinished result, this is still way too fast. Thus, the most recent event recorded is the day that occurred 1 year ago, the 2nd day 2 years ago, and so forth. Each additional day takes him another year to complete. The threads of events splay out in all directions, as one tangent after another is engaged in a futile effort to omit nothing of consequence—resulting in a hopeless jumble of discursive narratives, with cause and effect chains never quite reaching closure.

The question is why does 1 day take a year to describe—even incompletely and incomprehensibly? The answer is obvious in this one regard: Whereas narrative logic of events must be organized sequentially by the writer using explanatory “time tags,” the natural logic by which the events unfold requires no such imposed order; and so, it is with futile attempts at simulations of real-time situated events. Time tags are not required for real events that have an intrinsic time course but are indispensable for those same events when symbolic conventions are employed in their description—making them appear as endless digressions.

This is the difference between knowledge by description as opposed to knowledge by acquaintance, between third-person attempts to describe from the outside first-person events experienced from the inside. Simulations are always necessarily of this sort in their vain effort to replace direct perceptions with indirect ones involving representations.

Perhaps the closest a third-person description may approach first-person experiences would be in hypertext formulations of the narrative. However, even then real nonlinear choices that happen spontaneously in life must be organized by a judicious and laborious adherence to conventions that steer the reader down multilinear avenues and byways, often ending in cul-de-sacs or simply petering out. Such conventions must be changeable in a nearly arbitrary fashion, as later events reveal facts that require recasting the interpretation of earlier events. Hence, some rules for updating what has already been written would make continual revision unavoidable and, I fear, unspecifiable by any rule. No wonder Sterne's Shandy is a frustrated autobiographer who alternates between the comical and the pathetic.

Guthrie (n.d.) makes the so-called Tristram Shandy paradox explicit:

For a precise view of the problem, I will show the paradox numerically. The paradox posits an autobiographer who writes on every day passed. Since it takes Shandy one year (=365 days) to complete one day, then in terms of a one-to-one correspondence it would appear to be futile on a finite level:

Observed History: 1 day, 2 days, 3 days, 4 days, 5 days, ...

Recorded History: 365 days, 730 days, 1095 days, 1460 days, 1825 days, ...

It would seem mathematically impossible for Shandy to complete writing on all the days passed. Since each day yields an additional 365 days to write then it would seem that the longer Shandy wrote, the further behind he would get.

An even more intriguing insight into the Tristram Shandy paradox as a logic of digression was suggested by Parker (1997). She suggested that Sterne (1760–1767/1783) conceived of Tristram Shandy's autobiography as a way to subvert linear narratives of its time, which move predictably to a steady state in which their action ceases. Moreover, if Sterne had had access to the graphical representations of contemporary nonlinear dynamical systems theory (i.e., chaos theory), then his narrative might have been appropriately represented as a chaotic, or strange, attractor such as the butterfly attractor (see Figure 1).

Although here we see only a two-dimensional representation of this strange attractor, it actually evolves in a multidimensional state space. Its trajectories diverge at one time and almost converge at another but never actually intersect. Instead, they are attracted to unstable points that are never reached. Tristram Shandy's complex narrative with its digressive logic behaves like a chaotic dynamical system that explores a bounded arena of infinite possibility. This is worse than an NP-complete problem, although both show that linear excursions can never express the totality of nonlinear ones involved. (NP problems are those that can be solved by a nondeterministic Turing machine in polynomial time, e.g., the traveling salesman problem. Unfortunately, at this time the only algorithms we have are exponential



FIGURE 1 Butterfly attractor.

in time as a function of the problem size. In a landmark paper, Karp, 1972, showed that 21 intractable combinatorial computational problems are all NP complete.)

I think Parker (1997) hit the nail on the head, just as did von Neumann (1949/1966), who expressed pessimism at the prospect of giving explicit formulations of complex phenomena in either words or formulae. Some levels of complexity cannot ever be simulated; they must be directly experienced to be fully appreciated. The Tristram Shandy paradox is the metaphor for the theme explored in this article—the decline of mechanism.

THE GENERAL SEMANTIC FALLACY: WHERE SYMBOLS TAKE ON A LIFE OF THEIR OWN

Simon (1969/1996) explained that the kinds of symbols he had in mind are physical rather than abstract:

Symbol systems are called “physical” to remind the reader that they exist as real-world devices, fabricated of glass and metal (computers) or flesh and blood (brains). In the past we have been more accustomed to thinking of the symbol systems of mathematics and logic as abstract and disembodied, leaving out of account the paper and pencil and human minds that were required actually to bring them to life. (p. 22)

If, like Simon, one treats symbols as physical objects, then their existence and manipulation should require work be done to sustain them in their role as representations (Shaw & McIntyre, 1974). In addition to the physical role of a symbol, it has a primary semantic role as a representation. We recognize that different physical

symbols may play identical semantic roles, as in the case of written words and the words spoken by different voices or notes on a musical score and the same notes played by different instruments. However, to confuse the physical symbol with the semantic referent is to pervert the role of the symbol as a semantic function.

It is for this reason that it is wise to distinguish the *symbol vehicle* (physical object that functions as a symbol) from the *symbol function*, which may be carried out by different symbol vehicles. Not to make this distinction is to confuse the meaning of the symbol with the symbol vehicle, as when we confuse flesh and blood individuals with the social stereotypes we have set up, or react to the shouted warning “Fire!” as if there is a fire. The general semantic fallacy is the unwarranted identification of the two. This is why prejudice, being based on a fallacy, is deservedly abhorred. Of course there is no fallacy if the identification of the two is warranted; rather, the symbol function serves its legitimate purpose, for example, as in escaping from a fire because of an early warning.

I dramatize this fallacy to make clear how serious the abuse of symbol functions can be and how extreme forms of radical representationalism may create a kind of virtual but false reality by exploiting them—a pseudo-reality that simulates nothing real but is utterly fictitious and yet can incur real costs.

The situating process, such as the symbolic sister cum sister-in-law, can be insinuated as symbols of great import and significance into people’s lives to whatever extent people allow themselves to become obsessively delusional. In a nonpejorative sense, people should recognize that ideals expressed institutionally become situated into their lives and the culture in just this way—ideals ranging from legends like Robin Hood, Santa Claus, and the tooth fairy to national heroes (e.g., Tomb of the Unknown Soldier) and religious icons (e.g., recall Albert Schweitzer’s famous book *In Search of the Historical Jesus*).

I should not forget to mention scientific ideals that are later repudiated (e.g., phlogiston, the ether) or innocent people wrongly convicted by kangaroo courts on trumped up charges, the Spanish inquisition, the Salem witch trials, the infamous Nazi conspiratorial campaign against European Jews, gypsies, and political dissenters, and the unfounded propaganda, in general, that for centuries has moved nations to go to war against other nations for distorted and ill-conceived motives such as religious differences (e.g., the Crusades and other so-called holy wars). Situating ungrounded symbols is no less serious business because it is fictive.

To return to my earlier example, we saw how a social contrivance such as a mythical sister can be made to enjoy symbolic familial relationships as well as communal, social, and even legal ones. As this network of relationships becomes more extensive and its details more complete, the symbol becomes a “living” symbol with its own biographical history. The main point of the sham-sister example is to illustrate what is meant by the idea of *generative closure*, the idea that a complex of real objects can exhibit more mutual entailments than there are objects actually in the complex. The complex is a kind of functional scheme that organizes parents, siblings, sons and daughters, aunts and uncles, and brothers and sisters by shared

blood lines. However, it also can be legally extended by marriage contracts to “relatives-in-law.” Here the abstract schema for the complex (e.g., family tree) comprises all relations a wife might have and not just the ones that the wife in question does have, hence the potential for unaccounted for sisters who may be unseen or previously unknown. Circumstantial evidence, as opposed to eyewitness grounding of the mythical sister, may be sufficient as long as no recalcitrant skeptic decides to check all credentials back to their source.

More generally, having generative closure over the functional relations of a complex means that if a real object in the complex were removed, it would leave a “hole” that might be filled by a symbolic surrogate. It also means the manufacture of whatever credentials and relationships that a real object in the complex might have had can be inherited by a surrogate that replaces it. An only child still belongs to the complex with a potential for siblings. This is what “only” here connotes. By contrast to this generative *completion*, as the filling of empty places in an incomplete complex schema, one can also see generative *impletion* at work whenever the truth is embroidered in people’s attempts to pay homage to departed friends, relatives, and public figures—especially as shown in ancestor worship and the forging of heroic legendary figures from more modest factual lives.

In this way, in the case of the sham sister, one can see how even an ungrounded symbol can have a place manufactured for it by situating it in the relational complex. When formalized, this is called *possible worlds logic*, in which interpretations of objects and events are taken as pragmatically true if they remain consistent over the greatest number of interlocking scenarios (i.e., possible worlds) that attempt to incorporate the putative facts. Here history books are different from historical fiction in just this way. Also, the judgment of good science fiction as opposed to bad science fiction is based on how well the former fits with accepted scientific facts and principles and how poorly the latter does.

The fact that symbols may be situated even when they are not grounded in existence shows clearly that the situating of a symbol and its grounding are logically distinct; one may situate an ungrounded symbol (unfounded beliefs that control behavior) but one cannot ground a symbol without situating it to some extent (to see its grounding is to see its situation). Situating can be achieved through second-hand knowledge dissemination, but grounding requires firsthand experience.

These cases show that a symbol functions best when it fits most tightly into our possible world scenarios. This is the way that a symbol becomes situated. Grounding a symbol requires, in addition, that one can be made ostensively aware of the direct specification of the symbol’s referent. Without that, people have no firsthand knowledge and must rely on the situating of that knowledge secondhand in their belief systems. It is the playing off of situating as a kind of coherence theory of truth and meaning against grounding, a kind of correspondence theory, which perhaps characterizes best the ecological theory of meaning. In it there is room for both direct specification vis-à-vis grounding and indirect specification vis-à-vis situating. These are dual processes pertaining, respectively,

to increasing the scope of a symbol's insinuation into people's experiences as opposed to the precision with which it is incorporated.

If I see something bizarre that is utterly alien to me and label this experience, then that label is grounded, if not very well situated, beyond the context in which I experienced it. Conversely, if I see something so familiar that I scarcely take note of it but can readily label it if called on to do so, then that label is well situated in my experience but not very well grounded. In conclusion, one can see that a symbol serves as a virtual (fictive) substitute for real entities with real relationships to other real entities that it itself cannot fully enjoy. As a fiction, the symbol can only inherit those meanings bestowed on it by virtue of its membership in the relational complex. Take away its grounding situation in the real complex and the symbol becomes only a naked object stripped of its power to represent.

The success of actors, magicians, propagandists, advertising agencies, and con artists rests on how well they can promote the general semantic fallacy convincingly to a segment of the public for a certain length of time. Their incomes and uses of facilities and resources are real costs that may or may not serve legitimate purposes. They do serve a legitimate purpose if both of the following conditions are satisfied: first, the referent of the symbol function exists so the representations are grounded; and second, if they are properly situated in that what they afford is clearly specified.

In science, the general semantic fallacy gives rise to theories whose models may, at best, be consistent but untrue. Hence, there is no substitute for models being held to empirical accountability.

SIMON AND THE GIBSONIAN ALTERNATIVE

There is a real danger that simulation models may unwittingly promote the general semantic fallacy and waste substantial resources because they blindly pursue goals blocked by the Tristram Shandy paradox. Is it wise to shift the lion's share of the burden of explanation to simulation environments, as Simon admonished us to do, where complexities may be too easily fabricated through symbol function abuse? Is this not going to make the simulation approach to interface design theory too chancy and costly to pursue? I am not suggesting that simulation is useless—in restricted contexts it can be a useful tool (e.g., doing dry runs to train people when the actual trial runs would prove too dangerous or costly)—only that it would be too dangerous to depend on it as a sole or primary means to theory construction and evaluation.

A viable alternative is to be sought in synergy approaches that appreciate and demand embodiment, situating, and grounding. I am not alone in recommending this cautionary advice. One articulate defender of the need for embodied and embedded cognition, such as what robots might require, made this appraisal of the simulation approach:

Simulation offers at best an impoverished version of the real-world arena, and a version impoverished in some dangerous ways: ways that threaten to distort our image of the operation of agents by obscuring the contributions of environmental features and of real physical bodies. (Clark, 1997, p. 96).

The Gibsonian thesis is of course different from Simon's, which argues for a simple agent but a complex environment: Ecological psychology, being synergistic, aims to strike a balance between the two components of ecosystems so as not to obscure the contribution of either to the interaction. This strategy involves moving to higher levels of environmental description, that of objects and events and their invariant information that specify their affordance properties—those functional descriptions that allow the environment to be seen in terms of the actions and choices organisms might actually need to make. Simon made statements that seem on the surface to repudiate Clark's (1997) criticism of the simulation approach while also being consistent with the ecological thesis just offered:

This position should not be interpreted as suggesting that internal representations should be the central focus of investigation in understanding the relation between behavior and cognition. On the contrary, information processing theories fundamentally and necessarily involve the architecture's relation to the environment. The symbolic approach does not focus narrowly on what is in the head without concern for the relation between the intelligent system and its surround. ... A fundamental problem for cognitive modelers is to interleave internal and external states in order to achieve naturalistic behavior. (Vera & Simon, 1993, p. 12)

Also, just as others have attempted to align ecological psychology with situated action or cognition theory (Clancey, 1997; Costall & Leudar, 1996; Greeno, 1989; Lave, 1988; Suchman, 1987), so Simon tried to co-opt his critics by aligning his computationalism with situated action or cognition theory. For instance, in opening their article, Vera and Simon (1993) declared the following:

In this article, we wish to examine whether SA [i.e., situated action] is actually antithetical to symbolic manipulation. To anticipate our conclusions, we find that there is no such antithesis: SA systems are symbolic systems, and some past and present symbolic systems are SA systems. The symbolic systems appropriate to tasks calling for situated action do, however, have special characteristics that are interesting in their own right. (p. 8)

One must wonder what it tells us about situated action or cognition theory that attempts have been made to align it with both computational and ecological psychology? Can it really stand on such neutral ground, or can ecological and computational psychology actually be so similar in spirit? Simon thought situated action theory can be neutral but that situated action theorists do not see this because they have unfortunately and mistakenly thrown in with ecological psychology in de-

nouncing representational theory. These issues are a little slippery at first because Simon clearly implied that his view is ecological, but it soon becomes clear that it is not ecological in the Gibsonian sense. Like Gibson, Simon's theory is philosophically layered; hence, it is possible to find different degrees of agreement and disagreement at different layers.

Although an unrepentant cognitivist and unrelenting mediation theorist, Simon, like Gibson, was also a functionalist. Furthermore, he not only used the term *affordance* but gave a kind of begrudging recognition of its centrality to his cognitive computationalism. This brief isolated flirtation with ecological psychology and situated action or cognition theory seems to have been only a temporary expedient designed to confound his critics. If so, then one is not surprised that Simon (so far as I have been able to determine) never returned to these issues after the special issue of *Cognitive Science* in 1993 ("Situating Action," 1993). Neither is one terribly surprised that he used the concept of affordance so perversely, violating Gibson's original intent when he coined it, as clearly indicated by the quote in the epigraphs asserting that an "affordance is a symbol stored in central memory" (Vera & Simon, 1993, p. 79).

Despite Simon's tentative embrace of ecological psychology, it is still instructive to ask why he did so at all. Simon was too serious a scientist and scholar to merely act perversely. I suggest another reason. He saw the ecological approach as anathema to his own view, recognizing that if it stood, then his must fail. Both could not be simultaneously true. A coupling between an organism and its environment could not be accomplished by direct specification (information in Gibson's sense) but only by symbolic mediation (information in Simon's sense). His attitude toward this competitive theory was dismissive if not contemptuous. I see no evidence that he ever seriously tried to understand it. I could be wrong, but this was the impression I gained from my few personal conversations with him. His apparent disinterest in our approach might justify our disinterest in his approach, but it does not diminish the value of using him as a foil for understanding Gibson.

Another point of intersection and conflict concerns Simon's and Gibson's inconsistent hypotheses regarding how organisms as perceiving, acting, and thinking agents interface with their environments so as to perceive it, act on it, and think about it (e.g., make plans and decisions about goals). Simon and Gibson had two diametrically opposed hypotheses regarding how agents and environments interfaced: For Simon it was indirect, being mediated through symbolic representations; for Gibson it was direct, being unmediated and achieved by specification.

Perhaps, the key comparison to be made is whether the coupling envisioned is assumed to be law or rule governed. A rule, as intuitively conceived in cognitive science, is akin to the notion of a recursive rule as used in abstract machine theory, the mathematical foundation of computational science. Not always recognized, or left unemphasized if recognized, is that Gibson also used the term *rule* but in a quite different sense than recursive. Because these might be confused, I need to make their differences clear. In fact, one of the main aims of this commentary is to explore this issue. One chief difference to be discovered is that whereas Gibson's use

of rules holds because it is underwritten by law, Simon's are only underwritten by convention.

The key issue in both cases is how these two conceptions of rule relate to or rely on the deeper notion of natural laws. A full understanding of this topic suggests that I delve still deeper into Simon's (1969/1996) motivations for *The Sciences of the Artificial*. Consequently, I do so next in a way tailored to my purposes.

THE SCIENCES OF THE ARTIFICIAL

As clear as Simon's (1969/1996) book is overall, there are certain difficulties faced by any reviewer of any persuasion. One difficulty is that Simon sometimes makes equivocal pronouncements where one might have hoped for something more definite. This is not to say his views are wishy-washy, only that in his candor and conservatism Simon refused to feign a clarity that seems to him unjustified by the state of current knowledge. For example, he used such phrases as "to hedge my bets" rather than just "making a bet," or "having the air of necessity" rather than simply "being necessary."

In fairness, however, such circumlocutions do not detract from Simon's theses but actually strengthens their credibility although by no means makes them entirely convincing. Moreover, by judiciously avoiding the temptation to make overclaims, Simon's judgment in other matters seems more prudent and trustworthy. Furthermore, Simon's prose, in addition to being eloquent, has a most remarkable rational appeal, even to one whose orientation may be quite different. I often found myself liking the way Simon said something even when I disagreed with what he said.

Mostly, however, the book (Simon, 1969/1996) is for those who buy into the general computational philosophy. Whether a champion of AI or simulation, the appeal for the computationalist is that Simon's arguments project clear directions for theory and research. Alternatively, even those who are neither computer scientists nor psychologists might expect to find in this book issues of fundamental importance to their fields. I review his chief claims.

Simon (1969/1996) recognized four properties as indicative of the artificial:

1. Artificial things are synthesized by human agents—either intentionally or unintentionally.
2. Artificial things may simulate natural things in some ways but not all.
3. Artificial things can be characterized intentionally in terms of functions, goals, and adaptation.
4. Artificial things connote the imperative as well as the descriptive.

Under the broad heading of *the sciences of the artificial* and in contrast to the sciences of the natural, Simon addressed a number of issues of significance to cognitive psychology, an important one being how we should conceptualize life forms as

opposed to inanimate objects. He offered many important, difficult, and often surprising theses, and defended them most eloquently if not always cogently—a fact, however, which seems not to have lessened their general appeal among cognitivists and computationalists.

By contrast, because they focus on the natural, the theses of ecological psychology, it seems to me, arise in contradistinction to the theses Simon proposed for the artificial. Hence, careful consideration of his theses may help us understand our own a little better.

AN ECOLOGICAL EXEGESIS OF SIMON'S MAIN THESES

Simon listed the following characteristics among his most significant claims about artificial phenomena. Following each, I note the exceptions ecological psychologists might take, which in all but one case implies a counter thesis.

Psychology Deals Primarily With the Artificial Rather Than the Natural

Here, assuming I take the natural as being law governed and the artificial as rule governed, ecological and computational psychology are at loggerheads regarding whether laws or rules are the most appropriate explanatory construct. Gibson (1979/1986) himself introduced the notion of a rule for the perceptual control of action (e.g., locomotion or manipulation). Does this imply agreement with Simon's thesis? No, as Gibson's attempt to elucidate his sense of rule makes clear:

I asserted that behavior was controlled by *rules*. Surely, however, they are not rules enforced by an authority. The rules are not commands from a brain; they emerge from the animal–environment system. But the only way to describe rules is in words, and a rule expressed in words is a command. I am faced with a paradox. The rules for the control of locomotion will sound like commands, although they are not intended to. I can only suggest that the reader should interpret them as rules *not formulated in words*. (pp. 232–233)

One could just as easily substitute the term *symbol* for the term *word* without changing Gibson's intent, and thus rule out programs represented in the head as a means for reifying the imperative thrust of rules for the perceptual control of actions. This is, perhaps, Gibson's most explicit statement about the meaning of rule as he used it. It implies a clear rejection of Simon's computational thesis. Perhaps what he had in mind was more akin to the medieval logicians' notion of deontic law than to a recursive rule.

The *deontic law* is the basis for an imperative logic, one that governs the obligatory actions that must be taken because they are needed to attain some sought after

end (i.e., if z is the end sought and w must be done to achieve z , then one is obliged to do w to have z). As Simon (1969/1996) pointed out, there seems to be a need for “a distinct logic of imperatives, or a normative, deontic logic” (p. 115); therefore, it is not surprising that “there have been a number of constructions of modal logic for handling ‘should,’ ‘shalts,’ and ‘oughts’ of various kinds” (p. 115). Although Simon’s means–ends analysis seems a natural expression of a deontic law and might have made this medieval notion central to his approach, he demurred from its pursuit because in his opinion “none of these systems has been sufficiently developed or sufficiently widely applied to demonstrate that it is adequate to handle the logical requirements of the process of design” (p. 115). (Later we shall see that Gibson posed rules for action that have need of a logic of imperatives. Moreover, a law at the ecological scale is deontic in just this sense.)

Consequently, although Simon (1969/1996) was quite aware of the relevance of deontic laws to his notion of means–ends analysis, he rightly demurred from such treatment because he found the current understanding of the logic of such laws both flawed and incomplete. In spite of this, and with a renewed interpretation of deontic law, it seems to me that both Simon’s and Gibson’s rules for intentional actions may be appropriately treated as different renditions of the notion of a deontic law. This claim deserves treatment at another time. (This promissory note is offered because I have, for some years, worked toward an acceptable interpretation of the deontic law for ecological psychology.)

One should also note carefully that both ecological and artificial sciences do not differ because the former is *physical* and the latter *mental*—in keeping with the old Cartesian chestnut—but rather both sciences putatively have physical foundations. Indeed, it would quite miss the point if Simon were simply dismissed as being a mentalist. Although he did rely on symbolic representations as mediating internal states, symbols are for Simon physical objects that have psychological consequences.

Consequently, it is nearer the truth to say that Simon’s science of the artificial has more to do with Gibson’s indirect perception by means of artifacts than it does with Gibson’s direct perception that does not depend on artifacts at all. For this reason, it behooved Simon to try to relegate direct perception to being a derivative epiphenomenon of efficient computational architecture, as he indeed did.

Finally, Simon divided the world into natural and artificial phenomena, whereas Gibson, not denying the existence of artifacts, strongly disagreed that this should be a fundamental distinction:

Why has man changed the shapes and substances of his environment? To change what it affords him ... this is not a new environment—an artificial environment distinct from the natural environment—but the same old environment modified by man. It is a mistake to separate the natural from the artificial as if there were two environments, artifacts have to be manufactured from natural substances. It is also a mistake to separate the cultural environment from the natural environment, as if there

were a world of mental products distinct from the world of material products. There is only one world, however diverse, and all animals live in it, although we human animals have altered it to suit ourselves. We have done so wastefully, thoughtlessly and, if we do not mend our ways, fatally. (Gibson, 1979/1986, p. 130)

For Gibson affordances are most natural, and all things have them, artifacts not excluded; hence, artifacts are as natural in their affordances as anything else in the environment. Some things have bad affordance and some good regardless of their origin; our job is to act so as to maximize the good ones and minimize the bad ones. Thus, any design theory to be of ecological significance, must shoulder this responsibility.

The Inner Environment Is Simple, Whereas the Outer Environment Is Complex

Here Simon meant by the *outer environment* a physical environment that gives rise to functional meanings, and by *inner environment* he meant the constitution and organization of the agent biologically. How the functional description is to be handled vis-à-vis the notion of affordances is the issue and a source of disagreement. Still there is broad agreement on where a smart coupling is required. Ecological psychologists should, then, applaud Simon's choice of problems as well as his general functionalism—if not the exact form it takes.

The interface sought should relate rather than cleave the seam separating agents from their environments and, unlike Simon, I think it theoretically more useful to seek a balance in complexity across the two components—that there must be a fit between what the environment furnishes and what the agent can attain—if adaptability is to be achieved and sustained.

On the other hand, we should not agree with Simon's second claim that the organism and environment are disproportionally complex. The complexity of the environmental situation and the organism should be functionally matched at the moment the agent commits itself to an action that proves successful. Failure may result, however, from the inability of the actor to solve Bernstein's degrees of freedom problem, for example, in achieving macrocoordination over its micro-neuromuscular-skeletal variables. How and why this functional balance should be achieved has been spelled out in mathematical detail (but I fear generally ignored because it is too abstract). The case for favoring theoretical interpretations of the agent–environment coupling that keeps complexity functionally balanced across the ecological interface can be found in Shaw, Flascher, and Kadar (1995) and Shaw, Kadar, Sim, and Repperger (1992).

Finally, tacitly, if not explicitly, such a balance principle provides the theoretical and methodological foundations for what has been called *ecological interface design* (Effken, Kim, & Shaw, 1997; Vicente, 1999). Moreover, for this reason my colleagues and I (Shaw & Kinsella-Shaw, 1988; Shaw, Kugler, & Kinsella-Shaw,

1990; Shaw & Todd, 1980) were motivated to introduce the idea of intentional dynamics as a way to explain how effectivities can match affordances under conditions that allow actions to succeed in goal attainment.

Complex Systems Can Be Reduced to Simpler Form by Proper Description

On its face, this claim seems unquestionably valid but surprisingly an opposing case can be made that, for systems beyond a certain level of finite complexity, no verbal description or mathematical formulation can reduce its complexity. In fact, an even more pessimistic conjecture may be warranted. John von Neumann (1949/1966), the great Hungarian American mathematician and to many the father of the computer revolution in the United States, as Turing was in Great Britain, made a startlingly pessimistic claim about the difficulties faced by those who would design so-called pattern recognition programs:

It is not absolutely clear a priori that there is any simpler description of what constitutes a visual analogy than a description of the visual brain. ... Normally a literary description of what an automaton is supposed to do is simpler than the complete diagram of the automaton. It is not true a priori that this will also be so. There is a good deal in formal logics to indicate that the description of the functions of an automaton is simpler than the automaton itself, as long as the automaton is not very complicated, but that when you get to high complications, the actual object is simpler than the literary description. (p. 47)

A little later in this book, von Neumann (1949/1966) gave a hint as to what he might have had in mind:

It is characteristic of objects of low complexity that it is easier to talk about the object than produce it and easier to predict its properties than to build it. But in the complicated parts of formal logic it is always one order of magnitude harder to tell what an object can do than to produce the object. *The domain of validity of the question is of a higher type than the question itself* [italics added]. (p. 51)

It is interesting to note that von Neumann (1949/1966) posed this conjecture decades before the class of NP-complete problems were identified by Cook (1971) and the search for them came into vogue (Karp, 1972). This and more recent developments in the study of complex systems lends credence to von Neumann's conjecture being prescient rather than merely speculative. As von Neumann said, "there is a good deal in formal logics" (p. 47) to support this conjecture. What sort of evidence might be germane?

It is clear that in the green years of the field there was little sympathy for taking von Neumann's (1949/1966) conjecture seriously, in spite of his prominence. Cog-

nitive psychology in the 1960s was on the upswing and many of us as psychologists were holding onto the coattails of McCulloch and Pitts (1943), Craik (1943), Chomsky (1957, 1965), and Miller, Galanter, and Pribram (1960).

Simon's (1969/1996) attitude in the first edition of his book was as typical of its day as his attitude in the third edition is for today. For instance, Miller et al. (1960) voiced the sentiments of most cognitive psychologists in their classic book when they claimed the following:

It seems to the present authors, that attempts to simulate psychological processes with machines are motivated in large measure by the desire to test—or to demonstrate—the designer's understanding of the theory he espouses. History suggests that man can create almost anything he can visualize clearly. The creation of a model is proof of the clarity of the vision. If you understand how a thing works well enough to build your own, then your understanding must be nearly perfect. (p. 46)

It is not clear at all that the history to which Miller et al. (1960) referred recounts any successes with complex systems. In 1976 I gave the following response to Miller et al., which I still endorse today with even more confidence:

Moreover, contrary to what Miller et al. claim, history shows that the mere knowledge of how to construct something, whether it be fire, a table-top planetarium, a picture, a cake, or an internal combustion engine, is far from being sufficient for a scientific understanding of the object created. *To be able to do something implies only that you know how to do it, not that you understand what it is you did* [italics added]. This is analogous to the fact that the learning of a skill may be quite different from the execution of the skill or that the programming of a computer may not involve the principles necessary to understand the most significant functioning of that program. *The skill, like the program, may serve as nothing more than a link in an immensely large nomological net of facts and principles which provide the full context required to understand the real significance of either skill or program* [italics added]. (Shaw, 1976, p. 163)

As one who had struggled vainly with the problem of contexts for two decades—first as a Chomskian psycholinguist, then as a Piagetian, and finally as a Gibsonian—I fancy I had caught a glimmer of what was later to be celebrated as the problem of situated cognition and learning (Clancey, 1997). The problem proved both as recalcitrant as it was slippery.

In 1969, after some scathing reviews, I was finally able to publish an article reviewing the conjecture. The article was written while I was participating in a seminar on computational complexity taught by Juris Hartmanis at Cornell (Shaw, 1969, 1976). The article had already received high marks from Hartmanis—a pioneer and leader in the new field of computational complexity theory. At his insistence, my article only attempted to review a few fundamental theorems from formal logics and mathematics that von Neumann (1949/1966) might have used to make his case had they existed at the time. I was careful to avoid philosophical declarations but restricted myself to arguments based on the current state of the art

of complexity that seemed to give foundation to von Neumann's conjecture: such issues as "speed-up" theorems (Blum, 1967; Hartmanis & Stearns, 1965), decomposability methods (Hartmanis & Stearns, 1966), and order-type complexity indexes (Minsky & Papert, 1969).

Despite this, I was pilloried and attacked in the field for daring to suggest that there may have been a rational basis for taking the conjecture seriously—especially because Gödel had himself thought there may have been something to it. Gödel gave this account of what he thought von Neumann (1949/1966) must have meant:

I think the theorem of mine which von Neumann refers to is not that on the existence of undecidable propositions or that on the lengths of proofs but rather the fact that a complete epistemological description of a language *A* cannot be given in the same language *A*, because the concept of truth of sentences *A* cannot be defined in *A*. It is this theorem which is the true reason for the existence of undecidable propositions in the formal systems containing arithmetic. . . . Now this theorem certainly shows that the description of what a mechanism is doing in certain cases is more involved than the description of the mechanism, in the sense that it requires new and more abstract primitive terms, namely higher types. (as cited in von Neumann, 1949/1966, pp. 55–56)

Gödel went on to say that his same theorem was proved by Tarski a little earlier. With two notable logicians backing up von Neumann's conjecture, one might have thought it would have been taken more seriously than it was.

Ironically, despite the criticism I received for publicizing this conjecture, or perhaps because of it, I was still granted a lucrative and prestigious 5-year National Institutes of Health career development award to study such issues as they pertained to psychological modeling. I was surprised but felt partly vindicated. Still nothing much came of these efforts until Penrose (1989, 1994) and later Rosen (1991, 1999) revived this criticism in their arguments that living systems are complex exactly because they exhibit behaviors not algorithmically computable—a topic that has generated much controversy. The field has a habit of either ignoring what it cannot abide or trying to kill the messenger's credibility.

Many people who favored rule-governed learning and championed the budding field of computational psychology were extremely put off by my publicizing and defending this pessimistic conjecture and told me so in no uncertain terms. One of these was Simon, who told me at a later conference in Vail, Colorado (see Klahr, 1976), more or less, that I should not be surprised at being attacked because if the conjecture were true it cast aspersions on the new field of simulation at just the time support was needed if progress was to be made.

Not surprising, given the field's tendency for killing the messenger, Penrose (1989, 1994), who has severely criticized the strong computational thesis in two much publicized books, has himself in return been roundly criticized by ardent computationalists (e.g., see Penrose, 1996, in the electronic journal *Psyche*). From a history and sociology of science perspective, it is interesting to note that the temper of such debates has not changed.

Penrose (1996) expressed chagrin at the response of those true believers who take strong exception to his use of Gödel's theorem to justify rejecting the extreme computationalism thesis:

For those who are wedded to computationalism, explanations of this nature may indeed seem plausible. But why should we be wedded to computationalism? I do not know why so many people seem to be. Yet, some apparently hold to such a view with almost religious fervour. (Indeed, they may often resort to unreasonable rudeness when they feel this position to be threatened!) Perhaps, computationalism can indeed explain the facts of human mentality—but perhaps it cannot. It is a matter for dispassionate discussion, and certainly not for abuse!

Miller et al. (1960) might have applauded the reprise of their optimistic theses a quarter of a century later, as echoed by the two notable cognitive scientists, Churchland and Churchland: “Church's Thesis says that whatever is computable is Turing computable. Assuming, with some safety, that what the mind-brain does is computable, then it can in principle be simulated by a computer” (Churchland & Churchland, 1983, p. 6).

However, Churchland and Churchland misunderstood the import of the Church–Turing thesis. It does not imply results that entail “that a standard digital computer, given only the right program, a large enough memory and sufficient time, can ... display any systematic pattern of responses to the environment whatsoever” (Churchland & Churchland, 1990, p. 26). As Copeland (1997) observed:

This no doubt explains why they think they can assume “with some safety” that what the mind-brain does is computable, for on their understanding of matters this is to assume only that the mind-brain exhibits a systematic pattern of responses, or is characterized by a “rule-governed” input–output function.

Copeland (1997) was quite right to point out that the Church–Turing thesis does not entail that the brain (or the mind or consciousness) can be modeled by a Turing machine program, even when buttressed by the belief that the functional brain (or mind) is amenable to precise scientific description, or even if it exhibits invariant patterns of responses to the environment that seem to conform to certain rules. (For details, see Copeland, 1997, author of the Church–Turing Thesis entry to the online *Stanford Encyclopedia of Philosophy*.)

Miller et al.'s (1960) optimism about how simulation would solve our problems seems, in more recent times, like a child whistling in the dark, for numerous unforeseen problems and mysterious structures have been shown to emerge from increased complication (e.g., fractal sets, nonlinear dynamics, criticality; Casti, 1994). These surprises lurking in the depths of even simple dimensions of complication should make us more mathematically circumspect and scientifically

prudent, discouraging sweeping generalizations about truly complex systems. Perhaps the jury is still out on von Neumann's (1949/1966) conjecture, but I suspect its pessimism now seems more realistic than fanciful.

Life Forms Seek Satisficing Rather Than Optimal Solutions to Their Problems

Most ecological psychologists strongly agree; we recognize that although optimality may be a useful idea for analytical mathematical modeling, it is pragmatically false. Consequently, I find this last thesis the least objectionable, as long as we do not confuse a satisfactory outcome with an unlawful one. To protect the law concept, we must admit to a kind of graded control, or determinism with an admixture of nondeterminism, if one prefers.

In quantum physics there is a model for how to represent such tolerant but suboptimal solutions that are, more realistically, encountered in psychology. Hamilton's principle, by which particles somehow select the least action path, is an idea that carries over to optimal control theory—with one important difference: There the initial conditions for the control law may be modified to conform to particular constraint conditions. Felicitous modifications of initial conditions, something not possible in physics, in contrast to inert particles, allows a system with a proper control law to find an acceptable path; it can do so by minimizing a cost functional such as least time, least distance, or least fuel. Mathematically, this is accomplished by application of a technique known as the *calculus of variations*.

Unfortunately, even these control theoretic solutions are analytically too exact to mirror how real agents perform on situated tasks. Such solutions are too idealized to be appropriate in either quantum mechanics or psychology because of the limitations imposed by uncertainty in the former and indecision in the latter.

A more realistic path for a particle (or an agent) to follow is one that expresses the so-called effective action, the least action path overlaid with uncertainty. This, in effect, hides the ideal path solution inside a kind of blurred strip—yielding at best a path corridor comprising the superposition of all possible alternative path solutions satisfying the cost functional (Mensky, 1993). In situated tasks for agents rather than particles, one should expect something similar. The paths that present themselves on repeated trials will be found to meander and involve detours that avoid situational thwarts or that reflect less than perfect control. These blurry path corridors are what is meant by tolerant suboptimal solutions—solutions that depart from analytic perfection because of local exigencies but that are still satisfactory (or “satisficing,” to use Simon's apt expression).

For Gibson, a path solution is tolerant if, in spite of the small errors, it nevertheless realizes the affordance goal that the agent intends. *Intentional dynamics* is the name I have given for the collection of methods (e.g., various Monte Carlo methods) that discover the controls on initial conditions that are sufficiently adequate to get the job done intended by the agent. Unlike the initial conditions

on dynamical laws controlling particles, these initial conditions are altered to agree with the intended final condition. I say more about this later when I discuss rules for the perceptual control of actions or, as Simon (1969/1996) denoted them, *means–ends analyses*.

In what follows, I discuss how closely Simon's approach agrees with Gibson's, paying special attention to Simon's brand of ecological functionalism. If, as Gibson supposed, a goal (e.g., an affordance) is directly perceived, then how the relevant initial conditions can be tailored to achieve intended final conditions, that is, finding the proper control law, is explained. *The agent simply sees what is to be done to eradicate the differences between current information and the information specific to the goal.* As unlikely as this may sound, reasons are given later for its plausibility.

On the one hand, if, as Simon supposed, perception cannot be direct but must be mediated by symbolic representations that specify the goal, then the problem, it seems to me, is made enormously more difficult. I argue that it is not only made more difficult but may even become intractable when situated rather than idealized tasks are considered.

Clearly, the Gibsonian strategy depends on direct perception being a feasible alternative to mediating symbolic expressions. Hence, I consider this problem next.

SIMON ON THE DIRECT PERCEPTION OF AFFORDANCES

Direct perception is so central a concept for ecological psychologists that it naturally penetrates deeply into the ontological assumptions of the whole field of psychology, meaning that to understand how perception can be direct is to understand the world in quite a different way than traditional psychology, indeed, even than traditional physics and materialistic philosophy allow. The objects of direct perception, depending on whom you read, are some or all of the following: (a) things, (b) events, or (c) the affordances of things and events (see the special issue of *Ecological Psychology*, "How Are Affordances Related to Events?" 2000). Even more iconoclastic is the claim that (d) affordances per se are all that we truly perceive—an ontological claim that I have called *affordance imperialism* (Sanders, 1997; Stoffregen, 2000).

On the contrary, Gibson (1979/1986) gave a list of things that might be perceived along with their affordances:

My description of the environment ... and of the changes that can occur in it ... implies that places, attached objects, objects, and substances *are what are mainly perceived* [italics added], together with events, which are changes of these things. To see these things is to perceive what they afford. (p. 240)

Here it is clear that it is not only affordances that one perceives, although one also perceives them in the same act of perceiving these other things. What would it

mean, and how useful could it be, to perceive affordances but not to perceive those things that do the affording? For example, how plausible is it to say that one sees the pourability of water but not the water that is pourable? This has been an “in-house” argument that probably concerns few if any outsiders.

As the captions clearly show, although Simon deigned to use the term *affordance*, he and Vera interpreted it quite contrary to Gibson’s intended usage (Vera & Simon, 1993). Regardless of which of the four interpretations one endorses as defining the objects of direct perception, none are consistent with Simon’s indirect interpretation. How could Simon go so far astray from the intended use? How could he defend treating affordances in such a disparate fashion from its conventional usage?

For Simon, affordances were not empirical primitives but derived from computational primitives. To my mind (Shaw & Shockley, 2003), Simon made a grave mistake in his criticism of Gibson’s notion of *direct* when Vera and Simon (1993) wrote the following:

A functional description of the world (i.e., a description in terms of something like affordances) is one that allows simple mappings between our functional models of what is out there (e.g., road curves to the left) and our functional actions (e.g., turn to left). However, the resulting simplicity of the relation between these two functional representations does not imply that the relation is somehow “direct” or unmediated. It is, in fact, complexity of mediation (in the form of many representational layers) that affords this simplicity. Simplicity, in turn, gives the relation the phenomenological character of being direct. (p. 21)

I call this a grave mistake because it is wrong in two ways: It implies a solution is in hand for a problem that still has the status of a mystery while at the same time committing an egregious category error; it mistakes a symbol for the referent it denotes—a case of the general semantic fallacy discussed earlier. Why would Simon make such an undefended and perhaps indefensible assertion? Simon did so because he had no choice given his commitment to the precept that all psychological functions are reducible to computation, that is, symbol manipulations. Hence, Simon seeks in one grand gesture to bring Gibson’s ecological psychology under his own theory. For Simon, psychology is one of the sciences of the artificial in which rules governing the design of artifacts are paramount. Should the reader think it preposterous to attribute such extreme computational reductionism to the 1978 Nobel Laureate, consider Simon’s (1969/1996) own words:

The thesis is that certain phenomena are “artificial” in a very specific sense: They are as they are only because of a system’s being molded, by goals or purposes, to the environment in which it lives. If natural phenomena have an air of “necessity” about them in their subservience to natural law, artificial phenomena have an air of “contingency” in their malleability by the environment. (p. xi)

Hence, any system or phenomenon is deemed artificial if they have about their character the “air of ‘contingency,’” such that they might have been otherwise, and natural if they have about them the “air of ‘necessity,’” meaning presumably that they could not have been otherwise. Moreover, and most surprising, is that because living systems are adapted for purposeful behaviors, the same conditions that make a system such as a computer, a hammer, or a farm artificial apply to them as well. Simon (1969/1996) declared:

Notice that this way of viewing artifacts ... applies equally well to many things that are not man-made—to all things that can be regarded in fact as adapted to some situation; and in particular *it applies to the living systems* [italics added] that have evolved through forces of organic evolution. (p. 6)

Hence, humans and animals belong to the category of the artificial as well and, thus, like any man-made artifacts, fall under Simons’ proposed new science.

By an act of redefining category boundaries, Simon settled by fiat the age-old mind–body problem, for if one class of artificial mechanism can have “minds,” then why cannot other less wonderful mechanisms, such as robots, computers, and toasters have at least graded mental capacity? They all have symbol functions in their make up. This shows Simon’s hubris and allowed him to deny without virtue of argument the aspirations of ecological psychologists who strive to attain the status of a natural science for psychology. It also excludes biology as well from deserving such status. A successful theory of AI would, presumably, explain them all.

Surely, there is no reason to follow Simon in the a priori exclusion of psychology and biology from the natural sciences. The issue is not whether they are the results of an evolutionary process that might have been otherwise but whether there are laws that allow for graded determinism. Since the decline of the rigid notion of 19th-century mechanism with its assumption of absolute determinism, laws for nondeterministic simple phenomena have been found in quantum and statistical physics, and there is a growing scientific industry that seeks natural laws for complex phenomena (Wolfram’s, 2002, efforts notwithstanding).

Ecological psychologists have worked on both fronts to make a strong case for psychology being one of the natural sciences of complex (graded) nondeterministic phenomena. We need not let Simon’s own aspirations displace ours and reduce our science to the bedrock form of extreme computationalism that he espouses.

If we disallow his postulate that life forms are artificial because they are adapted to affordance goals requisite to their survival, then neither do we need to accept his belabored and facile characterization of affordances as symbols to be computationally manipulated as representations stored in central memory, and neither would we wish to reify information for affordances in this way.

Recall the earlier quoted passage in which Simon allowed that affordances might be used to name “a functional description of the world” that permits “simple mappings between our functional models of what is out there ... and our functional

actions” (Vera & Simon, 1993, p. 21). Simon would have us believe that directness in our awareness of them is not really primary; rather, it is no more than an epiphenomenon that somehow rides piggyback on computationally layered architectures—but this somehow is never even addressed. In fact Simon (1969/1996) himself acknowledged elsewhere that we do not yet have a clue to understanding this problem. This is recognized as Chalmer’s (1996) “hard” problem, namely, discovering how our experiences have the character they do because our neurophysiology processes function the way they do—a problem in need of philosophical clarification before sensible scientific hypotheses can be framed.

Gibson offered a solution but it would not make the mechanist happy: Direct perception follows from the ecological realist’s thesis that things appear as they do because that is the way they are, as taken in reference to the perceiver as actor at the ecological scale. However, notice this is not naïve realism as sometimes claimed. Naïve realism is absolute: Things appear exactly as they are, unconditionally.

Perhaps, then, one can concur:

So long as Simon maintains that the inner environment of the symbol functions is not the central issue, and admits that we presently know nothing about how our phenomenological experiences originate in neural functions, then his attempt to reduce direct experiences to indirect symbolic representations is little more than hand waving. (Shaw & Shockley, 2003, pp. 430–431)

Simon spoke apodictically but really had not made his case as fully as needed to be convincing.

PERORATION OF SIMILARITIES AND DIFFERENCES

Let us pause to review Gibson’s and Simon’s most important points of agreement and disagreement. First and foremost, they were both functionalists of roughly the same generation rather than behaviorists or mechanists (with Gibson, born 1904, being 12 years senior to Simon, born 1916). Simon, however, was a reductionist; Gibson was not. They sharply differed also as to the nature of the functionalism espoused. Although both exhibited ecological sensibilities, they were not ecological in exactly the same way. Likewise, they were both friends to situated analyses—with Simon leaning more toward situated cognition than situated action (see the special issue of *Cognitive Science*, “Situated Action,” 1993) and Gibson the other way around (see the special issue of *Ecological Psychology*, “Situating Action,” Costall & Leudar, 1996).

For both theorists, situated analyses remained implicit. Gibson’s whole approach emphasized the active agent behaving in its environment, doing the work of adaptive living, whereas Simon’s approach emphasized the agent’s deciding

which next step might be most adaptive and yet practical. In general, Gibson's actor navigates with intuitive awareness through a complex world by detecting invariant information for socially shared affordances according to ecological laws, that is, deontic rules for the perceptual control of goal-directed actions. By contrast, Simon's thinker must consult an internal model for each situation—a model that must be continuously updated “online”—and then apply a deontic rule (means–ends analysis) to decide at each moment what next step to take to reach its goal.

For Gibson, the intuitive agent can be any organism, from microbes to insects and from insects to fish, birds, mammals, and humans as long as they have evolved adaptive intentions and sufficient situational awareness to conform to the relevant deontic laws at the ecological scale. For Simon, the agent must also have situational awareness or its functional equivalent, awareness of the situation's internal model, and in addition, sufficient cognitive abilities to make proper deontic decisions based on means–ends analysis. Thus, Simon's approach seems more elitist than Gibson's in having restricted application to agents with highly evolved cognitive capacities.

However, of course, one of the tasks of Simon's design theory is to avoid this restriction by endowing all creatures with cleverly designed interfaces. Unfortunately, there is no hint of how nature might have evolved such designs except where the intuitions of human programmers are allowed. For by Simon's own admission, computers are designed in humans' images for human purposes. There are none designed by insects or other life forms; there are only the life forms themselves. Although the simulation strategy can still be applied, one must ask if it is appropriate.

For Gibson, the functional coupling of organisms with their environments is direct, having no need to reify an informational interface (beyond its causal support), its design arising naturally as a product of evolutionary preattunement as well as ongoing attunement through learning and the education of attention; for Simon, by contrast, a whole new science of artifacts (symbolic representations) is required to deal with the problems of how the interface was or should be designed and reified. Also, for Gibson, the design arises lawfully at the ecological scale, whereas for Simon it is contrived by rules of symbol production and manipulation that emerge somehow from some kind of computational roots in a way never entirely clear.

Simon and Gibson were both information theorists—but not of the Shannon variety. Whereas Simon believed information had to be symbolically represented and processed to be meaningful, Gibson thought information only had to be detected to yield its secrets. It is not “out there” (purely objective), as so many critics and interpreters have erroneously claimed; nor is it “in there” (purely subjective), as so many psychologists feel compelled to believe. Rather, information is relationally duplex, pointing people both toward their environment and toward themselves; it bridges between what traditionally was called the *subject* and the

object, the first-person (direct) experiences (knowledge by acquaintance) and the (indirect) experiences shared secondhand (knowledge by description)—although the latter depend on the former (Grote, 1865; James, 1911; Russell, 1912). For Simon, information comes in only one form—symbols; there is no direct specification only indirect representation.

Simon, like Gibson, recognized the coevolution of animals and their environments but saw information as inhering in the agent's changing attitudes toward the environment—attitudes that were learned. The environment is physical and law governed, being coupled to rule-governed symbols housed in the agent's memory. By processing these symbols, the agent is able to make reference to external artifacts (e.g., books, maps, movies, recipes, etc.); hence, not all information is stored in the agent but may be stored in the external environment as well—a notion with which Gibson agreed.

Gibson's view contrasts sharply with Simon's on the priority indirect perception deserves and the role secondhand information plays. For Gibson, firsthand information was not carried by representations, although representations may convey secondhand information. *Simon's notion of artificial sciences, as interpreted through his information processing theory, fits rather nicely under Gibson's notion of indirect perception.* This insight is worth strong emphasis; please take note of it. Thus, Simon's theory is extremely limited because it leaves no room for direct perception and thus leaves unexplained the act of ostensive specification by which symbols, as physical objects, come to refer beyond themselves (Shaw, 2001). In short, if direct perception is not primary, symbol functions of objects have no point of origin.

Simon believed an agent must engage in information processing before acting; Gibson did not. Rather, Gibson believed that information just needs to be picked up and used by an agent. Simon's information processing necessarily introduces mediation by symbol structure manipulation. If we are to use Simon's view as a foil for clarifying Gibson's direct perception, one needs to be clear about what Simon meant by indirectness—mediation by symbolic representations. Symbol systems manipulate symbols. Here is what Simon (1969/1996) said symbols are (for more details, see Newell & Simon, 1976):

1. Symbols are physical patterns (e.g., chalk marks, electrical impulses) that can be constituents of symbolic expressions (i.e., symbol structures).
2. A symbol's actual meaning comprises the pattern of activations between associated symbol structures that some outside stimulus induces.
3. A symbol's potential meaning comprises the entire framework of associations and connections that might be activated by imposed stimulus.
4. A symbol's linguistic, historical, and environmental context adds meaning to it.
5. Symbols may designate objects and processes that the symbol system to which it belongs can interpret and execute.

In addition, Simon (1969/1996) stated the defining properties of a physical symbol system:

6. A symbol system (e.g., a computer, a person) has processes capable of manipulating symbolic expressions—“processes that create, modify, copy, and destroy symbols” (p. 22).
7. A physical symbol system evolves over time, producing and developing a collection of symbol structures that can and often do serve as internal representations (e.g., images) of environmental properties and structures to which the symbol system seeks to adapt.
8. A symbol system has “windows on the world and hands, too” (p. 22) by which it acquires information to be encoded into internal symbols as well as produces symbols that initiate actions back onto the world.
9. All cognitive, perceptual, memorial, and reasoning processes involve symbol manipulation, as does the preparation for and initiation of actions.

There are many criticisms that may be levied against the physical symbol hypothesis as a theory of meaning, most of which have already been raised against Locke’s associative theory of meaning and its more contemporary renditions (Dreyfus, 1995; Korb, 1995). Most critical are the criticisms that it fails to explain reference, how symbols are grounded, and to delimit the expansion of irrelevant meanings built up by associations in the arbitrary situating of symbols. The idea of the generative closure of coalitions developed throughout this article is my suggestion for how an ecological theory of meaning (invariant specification of affordances) avoids both of these criticisms by offering a new look on grounding and situating.

In summary, to fully appreciate Simon, one must see that his theoretical psychology is founded on a commitment to symbols as the vehicles of thought, and thought as dominant over perception and action. To fully appreciate Gibson, one must come to see that direct perception is a conclusion rather than an assumption, that perception is the primary source of all adaptive thinking, that perception and action are equal partners, that they mutually interact and reciprocally dominate one another (i.e., a perceiving–acting cycle), and that cognitive abilities are elaborations of these more evolutionary primitive processes. Few battle lines have ever been so clearly drawn.

Earlier, I discussed how generative closure can lead to the creation of living symbols that have imputed physical existence and live as parasitic epiphenomena. The very property that allows this abuse in the symbol function also allows the generative closure needed to underwrite direct perception.

COALITIONS: A HISTORICAL NOTE

For ease of reference, I call any system with the kind of functional closure that arises from situating and grounding experiences and that conforms to the four

kinds of information control (discussed later) a *coalition*—a term I originally introduced in a book to describe how aphasic errors, although propagating insidiously, only make sense with respect to the function they have in the pragmatics of normal language use (Jenkins, Jimenez-Pabon, Shaw, & Sefer, 1975). The term was used more abstractly later to try to express the *generative closure* properties in which meanings and intentions are involved (Shaw & Turvey, 1981; Turvey, Shaw, & Mace, 1979). The complex of functionally defined relationships endows the invariant properties of agents and their environments with meaning and intentionality so that the properties make reference to other components in the complex through the generative closure property, fashioned after a similar property in mathematical group theory. Let us see how this works.

The group property of generative closure says that any member of the complex generatively specifies the total membership of the complex, that to be a member of the complex is to be directly cospecified by the other members. *Directly* means that nothing is cospecified indirectly through something else brought in from the outside, for the something else would be an interloper falling outside the closure property and might be eliminated without injury to the integrity of the complex. Say, for example, we did artificially interpolate a chain of mediators between two natural members of the complex. Finding a direct route through a complex of such indirectly connected sites amounts to finding the shortest shortcut, that is, one that puts each natural member just a step away from another natural member.

Relational complexes with this power to eliminate the interpolated artifacts are said to exhibit *commutativity*—the fundamental property that also allows natural parts to add up to structured wholes that surpass the natural parts in aggregation—an intuition the gestaltists appreciated. Relational complexes that can be reduced to one step cospecifications are said to be involutorial and may be modeled by the involutorial group. The involutorial group is usually modeled by a scheme involving four complex numbers:

$$(i^0, i^1, i^2, i^3) = i^n \text{ for } n = (1, 2, \dots, 4) \text{ and } i = (-1)^{1/2}.$$

For example, $i \times i = -1$, $i \times i \times i = -i$, and so forth. I do not go further into this here because the argument has been presented in detail elsewhere (Shaw et al., 1990). Also, the notion of generative closure as based on the (cyclic) group closure property has been experimentally illustrated by Shaw and Wilson (1976). Here is the background on this idea and Simon's and Gibson's reaction to it around the time that I had come up with it. (It was the key idea funded by my 1970–1975 career development award.)

I had the opportunity to present the argument for generative closure (an idea I generalized from Chomsky's, 1957, 1965, more restricted idea of generative grammars) at the first cognition and instruction conference held in Vail, Colorado, in June 1974. Simon, who was a discussant of the paper, made favorable comments—but I suspect this was because the elimination of extraneous artifacts idea (discussed later) was not a part of the original paper (Shaw & Wilson, 1976). This

is what the editor of the conference report had to say about this work in the preface of the published volume (Klahr, 1976):

Shaw and Wilson ... address issues of process and structure from a more abstract—almost philosophical—position, but they also provide concrete examples from Shaw's work on perception. The central issues concern the ability to understand an entire concept from experience with just a subset of its instances. Such an ability, Shaw and Wilson argue, lies at the heart of understanding invariance. (p. xi)

I think this is still true.

Ironically, the idea originated from Gibson's (1950) discussions of transformational groups and the properties they leave invariant, which he had borrowed from Ernst Cassirer's 1944 paper titled "The Concept of Group and the Theory of Perception." Around this same time, I discussed this idea of generative closure with Gibson and thanked him for putting me on to it. To my great surprise, Gibson staunchly denied ever having read Cassirer—not relinquishing his denial until I pointed out his use of Cassirer's ideas in his first book (Gibson, 1950, pp. 153, 193). Gibson laughed, shaking his head.

For the record, among other things Gibson said in 1950 was the following:

The geometry of transformations is therefore of considerable importance for vision, and it is conceivable that the clue to the whole problem of pattern-perception might be found here. ... A transformation is a regular and lawful event which leaves certain properties of the pattern invariant. (p. 153)

Gibson also said later: "If we are ever to understand exactly what yields a perception of shape we must study the dimensions of variation of various shapes" (p. 193).

For years to come, I was to parlay these insights of Cassirer, Gibson, and Chomsky into a theory of event perception (Kim, Effken, & Shaw, 1995; Shaw & Cutting, 1980; Shaw, Flascher, & Mace, 1996; Shaw, McIntyre, & Mace, 1974; Shaw & Pittenger, 1979; Warren & Shaw, 1985), which to my delight finally appeared as a topic in its own right in Gibson's (1979/1986) last book. Interest in the topic also, with help of many, led us to hold the first event perception conference at the University of Connecticut in 1981, from which the idea for an International Society of Ecological Psychology got its initial boost (Warren & Shaw, 1985).

I now take a closer look at the idea of generative closure, which in one of several incarnations has appealed to such dissimilar minds as Chomsky, Simon, and Gibson.

GENERATIVE CLOSURE: A CLOSER LOOK

The generative closure property makes integrable compositions possible whose differentiations cannot exhaust their essential properties. Closure bestows the gestalt

property on an integrable but possibly undifferentiable complex. I suspect Gibson coined the term *affordance* because he needed some way to label this fourfold emergent ecological complex—fourfold because it is simultaneously an expression of the *exterospecific*, *propriospecific*, *expropriospecific*, and *proexterospecific* forms of information. (The same terms can usefully be applied to control as well.) The closure property not only explains what makes information ecological (relationally defined between agents and their environments) but also what makes it direct. Mainly, for our purposes, it shows how directness is a conclusion not an assumption.

Figure 2 tries to make this important point clear and places the main logical difference between Simon and Gibson in sharp relief. The nodes designated E and O can be taken as representing Simon’s outer and inner environments, respectively. As Simon (1969/1996) said: “The artificial world is centered precisely on this interface between the inner and the outer environments; it is concerned with attaining goals by adapting the former to the latter” (p. 118). Because nodes E–O and O–E mediate the ordered relations between the outer environment E and the inner environment O, Simon would have one represent these indirect relationships by symbols denoting cognitive representations. Such symbols constitute what Simon (1969/1996) meant by “the artifact as interface” (p. 6).

My take on Gibson’s idea is more parsimonious; it would have one recognize that indirect (artificial) closure is a lower order description than is needed and replace it by its direct commutator description—indicated in the figure by a pair of dual diagonal arrows. (Of course Gibson would never characterize direct specification in these terms; they are mine, but I fancy he would not disapprove!) The right diagram introduces the notion of a commutative relation (or commutator). Whereas ordinary commutators select alternative paths to a given path, a direct commutator always makes an indirect path into a direct path by connecting its initial and terminal nodes while ignoring what goes on in between. Thus, the mediating chains of multiple nodes may be ignored because they play no necessary role.

Logically speaking, then, one must conclude that the diagrams for Simon and Gibson are fundamentally different, with Simon’s being contained in Gibson’s as a

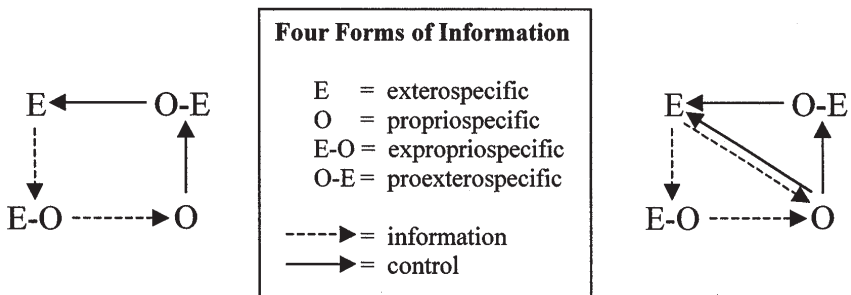


FIGURE 2 Cyclic closure (left) and direct commutators (right).

subdiagram. This allows the use of indirect means of specification whenever legitimate—that is, when secondhand information is substituted for firsthand information. Ultimately, at the level of direct perception of affordances or other environmental properties, the symbolic representations are not needed and become semantically superfluous whenever the closure property of higher order information holds. This is what I called “elimination of extraneous artifacts” earlier. Here is a little more on this idea.

REDUCIBILITY OF CONVENTIONS AND LAWS

Simon (1969/1996) recognized the essential role intention must play if symbols are to do their job:

An artifact can be thought of as a meeting point—an “interface” in today’s terms—between an “inner” environment, the substance and organization of the artifact itself, and an “outer” environment, the surroundings in which it operates. If the inner environment is appropriate to the outer environment, or vice versa, *the artifact will serve its purpose* [italics added]. (p. 6)

Again, we are told by Simon (1969/1996) that “the outer environment determines the goals of the inner environment” (p. 11). In essence, design strategies orchestrate properties of the inner environment so that they serve the goals of the outer environment. Central to the nature of artifacts “are the goals that link the inner to the outer environment” (pp. 10–11).

The coupling of the inner and outer environments can be hypothesized as indirect, as in Simon’s thesis, which invokes information processing by symbol manipulations, or direct, as in Gibson’s thesis (in my words, not Gibson’s), bringing direct commutators to the forefront as the purveyors of direct specification. This thesis was made even clearer by Gibson (1966):

We tend to think of direct stimuli from the terrestrial environment as being like words and pictures instead of realizing that words and pictures are at best man-made substitutes for these direct stimuli. Language and art yield perceptions at second-hand. This second-hand perception no doubt works backwards on direct perception [on the direct commutators], but knowledge *about* the world rests on *acquaintance* with the world, in the last analysis, and this is our first problem. (p. 28)

This is such an important point, I look at a variety of ways to illustrate it.

Gibson (1979/1986) stated: “What the philosopher called foresight is what I call the *perception of the affordance*. To see at a distance what the object affords on contact is ‘necessary for the preservation of the animal’” (p. 232). Here adaptive success depends on prospective control, which in turn depends on anticipa-

tory information. Such information may come to us firsthand by direct apprehension or secondhand by indirect means. Here are some cases that elucidate this fact in several different ways, each providing an entrée to a different set of issues and concerns.

Case 1: Firsthand Versus Secondhand Knowledge

If I ask you for a favor directly, I can have your response directly, but if I ask a mutual friend to ask you for me, then I can have your response only indirectly, requiring the mutual friend to act as a mediator. This means not only can I then not know your response until the friend responds, but if the friend is unable to respond or chooses not to, the closure is broken and the symbol function thwarted. The mediating friend performs a kind of symbol function for my request, which is roundabout and secondhand. Here it is clear that without my original request of the friend to mediate for me, the friend would serve no function and become superfluous.

Therefore, here is the main point once again. The original request carries the root intention of the social transaction; this root intention requires that a direct commutator exist in principle. To Gibson, symbolic representations are just such superfluous entities having no power to get the epistemic process going, although they may play the role of connecting meanings after the process is initiated.

Case 2: Epistemic Grounding of Symbols

Simon placed great stock in the notion that his symbols are physical rather than abstract. Simon's theory stands to Gibson's as remembering the combination code for opening a safe stands to cracking the safe by feeling the tumblers fall into place and thereby learning the combination. The sequence of left-right turns by a specific number of degrees is a physical code that defines a successful path to the goal of opening the safe for whatever reason—to alter or check on the contents of the safe.

An analysis of the relation between using a code to open the safe versus opening a safe to discover the code is instructive. It illustrates, by analogy and example, the major similarities and differences between Simon's theory of interface design and the Gibson-inspired ecological interface design (Vicente, 1999). The code is information specific to a goal—opening the safe—which is used to guide the action. One perceives to act (specifically, uses code to guide opening the safe) and conversely one acts to perceive (specifically, e.g., to check contents in the safe).

The conventions for the use of symbols must be reducible. If they are, then they are underwritten by natural law. Conventions are reducible in the current sense in those cases and only those cases in which an effective action achieves realization of an intended goal, that is, by following a rule whose convention rests on realizable affordances. In this example, the relevant convention specifies how if the dial on a safe is manipulated according to a sequence of symbols (e.g., 0, right twice stopping

at 16, left to 23, right back to 0), the action affords opening the safe. The idea of reducibility may be made clearer by considering a case in which the convention is not reducible, and in which there is no effective action—such as when the combination on a safe is changed so the previous action is now ineffectual.

However, even here a new effective action may be discovered by safe cracking techniques (say, by listening to or feeling the tumblers fall into place). Here a new code can be associated with the effective action by reading off the dial the code sequence corresponding to the points where the tumblers fall.

Notice that a symbol or symbol sequence is useless if its intention fails to reduce to an affordance–effectivity fit through an effective action. One can adjust the intention, for example, by having a pseudo-code to confound any unauthorized person trying to open the safe. A pseudo-code is a double convention code, one rule for translating the false code into the true code and the other to apply the code to guide the interfacing of actions (twiddling the knob) to the safe. Note that the rule translating one code into another is useless if it is not followed by a rule for realizing the code’s intention by effective action. When this is the case, then one can say the rule conventions satisfy the reducibility condition. This condition satisfies a symmetry condition between the agents effectivities and the affordances supporting the actions. To reiterate: This is what we mean by an affordance–effectivity fit—a shorthand for the reducibility of convention argument.

Hence, by this analysis, Simon’s view of affordances is wrong in two fundamental ways: First, an affordance cannot be a symbol, as he claimed, because as a goal it is essential to a symbol’s convention being reduced. Second, an affordance can be realized as a goal by an effective action whether or not there is a symbol at all. Finally, I strongly emphasize that *symbols are useful if and only if their conventions are reducible*. Such is the case when the symmetry condition holds between affordance and the effectivity (effective action) that realizes it.

Later, we shall see that the symmetry condition expresses how symbols depend on ecological laws for their validity. This is tantamount to saying that rules that satisfy the reducibility condition are underwritten by laws.

Case 3: Informational Transparency

Consider one of my favorite examples of direct perception. I perceive the shape of the cavity in my back molar by probing its boundaries with a three inch metal pick. Clearly I am in contact with this dental tool, which is in contact with my molar’s cavity, but I am also in contact with my tooth. How can this be? It can be because probing with a tool not only coimplicates both the probed and the probing agent but is a successful action only if the probe (tool) is informationally transparent to both the object probed and the control of the action required to guide the probe.

This transparency condition for information here in the case of tool use is a formal analogy to the reducibility condition for symbols. This is a crucial insight for beginning the approach to an ecological study of language—or better, of communi-

cation. It starts with the realization that communication through language (reducibility of conventions) and communication through tool use (transparency of media to information) have much in common. Ecological psychologists will, in my opinion, not make progress on the language problem until this analogy is explicitly understood.

To be direct is an either-or proposition in the sense of an exclusive disjunct (i.e., either-or but not both). It matters not how remote or how near the ends of the linkage are. All that matters is whether in the final analysis the linkage, in its entirety, is a medium over which the influence is transparent from one end to the other. Specification fills the gap regardless of its breadth or the complication of the medium. It could include the central nervous systems, tools, or other linkages. A ship whose signal gets weaker and weaker as it increases its distance from the shore is still in contact with its shore base until the medium of support for the signal collapses under a severely lopsided signal-to-noise ratio. This remote sensing brings me to the next case.

Case 4: Insight as Direct Apprehension

Like the ship, an actor navigating in the world may perceive the direct effects of its intended remote target over arbitrary distances—as long as the medium for the information field does not collapse, then it can maintain contact with the target. In a more abstract vein, imagine the insight that allows a creative person to see the most elegant route to the end of a task or to the solution of a problem. The gestaltists called this ability to apprehend directly the solution to a problem *insight*.

Insight that allows a mathematician to prove a theorem is no different in kind than insight that allows a person to select a navigable path to a final destination. More generally, *insight* might be defined as an intuitive act of maintaining awareness of the final consequence of a train of choices of arbitrary length. Here, as with any other medium, the train of choices must be a medium transparent to goal-specific information. Such direct apprehension of remote intended effects are as common among artists as they are among mathematicians who intuit the theorem to be proved ahead of time (how else?) or soccer players who position themselves for a header because they see the cross developing from other players.

To reiterate my main point, direct epistemic acts are no more perceptual than they are cognitive, nor more cognitive than they are action based. Gibson's magnificent insight was to see how epistemic directness could be teased apart from causal mediation if one only recognizes that the function of information is different from the physical media that support it. Although information is no less physical than other aspects of energy distributions, it acts directly, as a veritable action-at-a distance, to keep the agent in contact with affordances of consequence.

Without affordances the information would signify nothing, and without the information agents would roam aimless (if they perchance to exist). Directness means that affordances are knowable at a distance ahead of time; hence anticipa-

tion, forecasting, and planning are possible. My colleagues and I have not seriously studied the affordances of solution spaces to problem sets, but we have made a beginning (Barab, Cherkes-Julkowski, Swenson, Garrett, & Shaw, 1999; Shaw, Effken, Fagen, Garrett, & Morris, 1997).

Case 5: Invariant Optical Information

I ask that you carefully ponder this: The way an optical instrument (e.g., telescope or microscope) produces an image from a real-world scene is evidence *not that we see the world (indirectly) as images* but that *we directly detect the invariant optical information specific to the scene*—which also produces the image.

Whereas the image is appropriate to the simple, flat, homogeneous screen on which it may be focused, the invariant optical information is appropriate to the complex, multidimensional, multilayered, multifaceted resonating detector systems of the central nervous system. Whereas it takes the latter to view the former, the former cannot under any circumstances view the latter. Whereas it takes the whole organism to react meaningfully and purposively to the invariant information specific to an environmental situation (and thereby to produce properly situated actions), no computational, symbol manipulating system can do likewise; it is to meanings (affordances) and not symbols that actions must be directed if a system is to be situated.

In sum, to be *indirected* to symbols is to be misdirected from meaning unless the symbols are made transparent by direct specification of their referents regardless of the length or indirectness of symbol chains. Symbols as object have no intrinsic power to specify; as objects per se they are referentially opaque and only become transparent by virtue of such direct specification. I return to elaborate this point later. First, let us try a more cognitive example to see if the notions of *direct* and *indirect* generalize from perceptually driven to cognitively driven actions.

Case 6: Situating Rules

One may follow a recipe and produce an ethnic dish from unfamiliar ingredients by comparing their labels to the names in the recipe and apportion by cook's measures the recipe's stated quantities. Similarly, one may mechanically follow the steps in preparation and cooking as stated in the cookbook. Of course, this is not the same as a master chef who constructed the recipe as a record of his or her situated actions—defined here as taste experiments involving years of trials combining and apportioning a broad range of varied ingredients as well as successes and failures with a plethora of preparation and cooking strategies. Hence, the novice's use of the recipe is the indirect result of the master chef's attunement of the selection, preparation, and performance stages of cooking through extended direct experience.

In short, although it is true that a novice without being properly situated in his or her cooking actions can produce a crude approximation to the expert's properly

situated actions, he or she will lack facility to improvise when faced by less than felicitous means. As in Locke's nightmare (discussed later), the novice who cannot properly situate his or her actions may produce something by rote that he or she scarcely, if at all, understands. The hedge "scarcely" is needed here because in the mere attempt he or she will begin accruing direct experience that may, depending on talent, initiate the situating process; the direct perception of what the cooking situation affords may produce resonances across subsequent situations whose information is invariant with prior situations.

In this way, the novice's actions begin insinuating themselves into all situations to the degree that they share the same affordances. Recipes do not resonate, cooks do—meaning that resonance to invariant information in cases of properly situated action require that the agent be insinuated into that situation by an *adaptive resonance process*. It does not work if the agent is only in a symbolic representation of the real situation, although the agent might become adept at using the symbols (e.g., like mechanically following a recipe by rote rather than by direct participation through resonances to taste and cooking experiences).

It is just that where perception is concerned, the evidence is the experience of the functionally defined, temporally extended, ecologically scaled environment. Given how much confusion has surrounded this point, it is worth taking a moment to review what *indirect* means in hope of sharpening our appreciation of what *direct* must mean. My chief aim is to shed light on how the direct perceiving of affordances is synonymous with keeping in contact with the environment.

Indirect means of encountering the world through description and depiction cannot and should not be made fundamental. To duplicate facsimiles, shove the copies into the head, and call them the memorial basis for perception was to Gibson ludicrous and superfluous, not to mention unparsimonious, for it makes perception an internal process with no access to the world except through a dictionary look-up table that is impossible to validate independently. It also leaves as a mystery how such a look-up table could come to be in the first place if no direct perception were involved.

It ultimately confounds the semantic problem of intentionality with Locke's claustrophobic nightmare. In Locke's nightmare, each of us is trapped in his or her own mind like being in a locked trunk with no key. Perception then becomes cognition in the sense of reflection. All one can do is rummage around through one's own ideas as if they were curios whose provenance is long lost. Action fares no better because we are no longer able to act on or even toward the world but are restricted to acting on symbols and then toward their mental dictionary representations. Thus, Locke's nightmare precludes there being knowledge of the world if all knowledge is secondhand.

Earlier I considered some formal intuitions (generative specification, group closure, and direct commutators) that might suggest ways to mathematize the theory of direct specification. I say *direct specification* to cover all bases: direct perception, direct memory access, direct control of action, direct inference, direct learning, di-

rect problem solving, direct concept acquisition, and so forth. The theory of directness as the absence of mediation is not limited to perception. It is not a casual idea, sometimes holding and sometimes not holding, as some commentators on Gibson seem to think. It is a bold assault on the claim that indirectness can be a primary source of knowledge.

It is not, however, a denial of the reality of symbol functions; that is, it still allows that certain objects, when treated consistently by learned conventional rules, can promote indirect specification of the direct circumstances under which the rules were learned. Why is such an easily dispelled misunderstanding so often repeated?

When something challenges the status quo in science but is not understood, then myths grow up around the offending view aimed, either wittingly or unwittingly, at making it seem preposterous and undeserving of serious consideration. This has happened time and time again in the case of ecological psychology and especially in regard to direct perception—around which an aura of mystery has grown up. It might be wise to dispel some of these most obvious myths before proceeding.

MYTHS ABOUT DIRECT PERCEPTION

Myth 1: Ecological Psychologists Do Not Believe in Indirect Perception

This is patently false. Gibson also allowed perception to be indirect where paintings, photographs, TV, movies, virtual realities, and other facsimiles of reality are concerned, although he strongly opposed the traditional attempts to reduce all perception and cognition to being indirect, that is, mediated by memorial states or inferential processes.

Gibson was no fool; he would never deny anything so obvious and, in fact, wrote extensively on this topic (see, for instance, Part 4 of his 1979/1986 book, in which he discussed depiction and the structuring of light by artifice; see also Turvey & Shaw, 1979, on the primacy of perceiving). Such indirect processes have their place, but Gibson was quite sure it was foolish to try to replace direct processes with them.

Myth 2: Ecological Psychologists Do Not Believe in Memory

This is also false. Acceptance of direct specification does not entail the rejection of memory as persisting knowing nor memory as portable knowledge, but it does lead one to treat memory differently from orthodoxy. We reject the computer metaphor that treats memory as storage; memory is not for us a receptacle in which things are placed—whether those things be ideas, images, facts, or charges that cloak internal

states with symbolic representations (Carello, Turvey, Kugler, & Shaw, 1984; Shaw & McIntyre, 1974). I prefer to think of it as an attunement of a resonance process rather than as an “item-in-the-box” theory (Turvey & Shaw, 1979). I say more about resonance theory later.

Myth 3: Ecological Psychologists Do Not Believe in Inference

This is also quite false. We do not and should not deny a legitimate role to inference as a cognitive process; however, we prefer to construe it as a way of linking current indirect evidence, perhaps through mediating propositions, to future or past direct experiences. To evaluate the indirect evidence one must ultimately link it up to directly specified evidence. This is why we call it *direct inference*. Even some of the staunchest critics of the direct perception hypothesis have had to relinquish this point. For instance, Fodor and Pylyshyn (1981) made this candid realist’s admission:

Even theories that hold that perception of many properties is inferentially mediated must assume that the detection of *some* properties is direct (in the sense of *not* inferentially mediated). Fundamentally, this is because inferences are processes in which one belief causes another. Unless some beliefs are fixed in some way other than by inference, it is hard to see how the inferential processes could get started. Inferences need premises. (p. 155)

Also, I add, the premises need to be reducible (grounded).

I find little to disagree with here—except I would amend their statement to read “it is impossible to see how the inferential processes could get started.” As was seen earlier, Simon (1969/1996) also recognized that “symbols must have windows on the world and hands too” (p. 22). One wonders if a rapprochement is possible if other prejudices could be overcome. Clancey (1997) made a strong case that Fodor and Pylyshyn (1981) simply missed the point of Gibson’s ecological level of description. Fodor and Pylyshyn missed the point because they did not relinquish internal state descriptions in favor of relational ones.

Satisfying the inheritability of truth conditions either makes or breaks an entailment theory. Grounding a proposition so it can be evaluated in terms of its pragmatic truth value requires evaluation in a real-world context. Although quite relevant, situated logic is a technical study in its own right, and therefore I will not explore it here (but see Barwise & Perry, 1984).

Ecological psychologists also recognize that indirect perception is the meat and potatoes of being an artist, photographer, or composer, just as indirect inference is the staple technique for historians, archeologists, and sleuths, as well as being involved in forecasting by medical doctors, weathermen, economists, pollsters, and political pundits.

The depth of misunderstanding of Gibson's radical hypothesis is illustrated by a quote from a leading author (Clark, 1997) of an otherwise interesting and insightful book:

A related view of internal representation was pioneered by the psychologist James Gibson (1950, 1966, 1979/1986). This work made the mistake, however, of seeming to attack the notion of complex mediating inner states *tout court*. Despite this rhetorical slip, Gibsonian approaches are most engagingly seen only as opposing the encoding or mirroring view of internal representation. (p. 50)

Clark (1997) missed the mark in his understanding of what is theoretically at stake. Neither Gibson nor his followers reject mediation *tout court* but repudiate the mentalist use of internal states as well as the behaviorist's use of external states in trying to model what is primary in epistemic activities. A pox on both their houses! Such models never succeed, although failure does not seem to discourage a futile succession of repeated attempts. In their stead, as I have been at pains to show, we favor the use of relational (dual) states as being primary.

It seems to me, speaking abstractly, that the rock bottom level of description that best serves psychology is to be found at the ecological scale where the direct commutators live—unfortunately, the sense of this claim is that direct perception is a natural outcome of describing systems at the ecological scale. To me this is such a crucial postulate of the ecological approach that to ignore it is to misunderstand completely the strength and plausibility of the approach. If internal states are not needed, then much of the force is removed from criticisms of the direct specification thesis. Are they needed?

CAN WE IMPROVE ON INTERNAL STATE SPACE ANALYSIS?

Having converted to ecological psychology in the 1970s from being a committed computational cognitivist in the 1960s, I became increasingly suspicious of internal states—except when treated as ancillary physical or neurological constructs. Here is a quick sketch of why my attitude changed from being cognitive to ecological.

Like so many young Turks, under James Jenkins tutelage during my Minnesota postdoctoral studies (1965–1967), I had rebelled against the establishment that was thoroughly and rigidly behavioristic. The behaviorists were coming under increasingly harsh criticism by such cognitivist gurus as Noam Chomsky, James Jenkins, Jean Piaget, George Miller, Karl Pribram, and Ulric Neisser. Like so many others, I became profoundly convinced that psychology needed a nonbehavioristic alternative to explain language acquisition and cognitive devel-

opment. The answer seemed at the time to require something akin to Craik's (1943) internal model theory.

Craik (1943) proposed the hypothesis that thought models or parallels reality, that the essential problem for psychology was not the mind, self, or sense data but symbolism. Such symbolism is instantiated in mechanical devices designed to aid human thought in calculation. Furthermore, Craik argued, that thought performs a simulative function by providing a convenient small-scale model of natural processes. Simon appealed to us at the time because he seemed to have been squarely in Craik's corner. The first crack in my allegiance to internal state theory (e.g., simulation models) came when I visited Cal Tech, as reported earlier.

The synergy strategy takes issue with this simulative proposition by avoiding what we later called a FOIF—a *first-order isomorphism fallacy* (Shaw & Turvey, 1981). Simulation theories assume that features of (mental, neural) representations can be mapped one to one onto the environmental state of affairs they represent, for this is how they accrue their meaning. Gestalt theories emphatically disagree and consider this a fallacy; instead, they are concerned about how things become organized in similar ways, their organization as wholes, and thus not their individual features. Such laws of organization are considered transposable over different concrete instantiations of a pattern (e.g., circles drawn on a blackboard with chalk or made by giant stones at Stonehenge; a melody played on different instruments or hummed by different voices).

Gestaltists (e.g., Koffka, 1935) rejected the FOIF in favor of a second-order isomorphism between the laws that explain the organization of the environmental state of affairs and those that account for the organization of the (neural) representation. As a budding theoretical psychologist, this move away from the crass correspondence theory of meaning promoted by Craik (1943) and Miller et al. (1960) and endorsed by Simon (1969/1996) sounded better to me, especially when I later learned that Gibson (1979/1986) said that one of his prime motivations was to ecologize Gestalt psychology. Soon I came to believe the gestaltist had a better idea. From the gestaltists I discovered Gibson, who seemed to me to have an even better idea as an answer to the question, "Why reproduce that which can be dealt with more directly and elegantly by invariant specification?" This was, after all, in keeping with my belief in the rightness of von Neumann's (1949/1966) conjecture.

Let us look at FOIFs more closely with an eye on what they imply. Whereas feature correspondence is first-order isomorphism, correspondence of the transposable holistic properties of representations is second-order isomorphism because it is of a higher type. To try to reduce the latter to the former is to try to reduce a thing of a higher type to something of a lower type, and this buys into the fallacy of reductionism—the vain attempt to legitimize FOIFs. Von Neumann (1949/1966) suspected that there were reasons why reductionism is a dangerous intellectual game, and the logician Gödel and the formal semanticist Tarski proved it independently (see the editor's introduction to von Neumann, 1949/1966).

The gestaltists were as unhappy with Craik's (1943) type of solution as Gibson, arguing instead that a more useful strategy involves second-order isomorphisms. These models instead express invariant principles of organization that are transposable over the configurational properties of situations, thus treating particular instances as tokens of a more abstract class of situations. Because Gibson (1979/1986) had admitted that one of his goals was to ecologize Gestalt psychology, he replaced their phenomenalism and its dependency on subjective experience with the social invariants we all share, and their configurational properties with those invariants that inhere in relational properties that couple organisms to their environment. Affordances are an example of such interactional invariants. Moving from the gestaltists' phenomenalism to ecological realism was to me Gibson's better idea.

Simon, however, is not guilty of this fallacy in all aspects of his theory but seemed to argue for both forms of isomorphism—first-order for the representations achieved by simulation and second-order for the sharing of organizational principles in his search for common laws of design. However, the latter is corrupted by the former, and therefore the whole theory is tainted by his endorsement of the radical simulation hypothesis—an indefensible endorsement of the FOIF.

If Simon's laws of design were not based on this FOIF, then he would perhaps have been more open to Gibson's move to still higher order forms of isomorphism. We (Shaw & Turvey, 1981) argued, in the explanation of Gibson's ecologically scaled functionalism, that his notion of affordances as *invariants of invariants* (i.e., compound invariants) demands a still higher order isomorphism. We (Shaw & Turvey, 1981) called this *third-order isomorphism* and claimed it implicates even a fourth-order one—the latter we called *coalitional* to distinguish its concern from that of lower order gestalt holism that deals with only invariants over structures. The fourth-order isomorphism of a coalition adds the property of generative closure, as already discussed. Whereas the third-order isomorphism deals with invariants of invariants over structure (i.e., invariant properties of second-order isomorphs), coalitions express the closure of third-order isomorphism under ecological boundary conditions.

An agent's actions are guaranteed to be successful if and only if they are embedded in its ecosystem treated as a coalition (Shaw & Turvey, 1981). To be coalitional is to exhibit generative closure, as discussed earlier. Actions, no less than symbol functions, must be grounded and situated at the ecological scale. This means they must be defined over dual states of an ecosystem—states that complicate, deontically speaking, both the relevant environmental affordances and the agent's effectivities needed for realizing the action in question. The organism's effectivity for utilizing the environmental support involved in realizing an affordance goal must follow directly from a rule for the perceptual control of action, and most important, this rule must be underwritten by an ecological law.

I appreciate the difficulty of comprehending these ideas without elaborate discussions and illustrations. For this reason, the rest of the article is aimed at develop-

ing these ideas further. Key to this topic is seeing how the generative closure property of coalitions allow direct specification as the basis for actions as well as perceptions. This requires gaining an understanding of two notions: first, ecological states as dual states that complicate the agent and its environment simultaneously; and second, models of systems that manage to carry out goal-directed behaviors without entailing internal states.

In the next section I offer a first pass on these two topics.

DUAL STATE MACHINES: RELATIONAL SYSTEMS WITHOUT INTERNAL REPRESENTATIONS

In the early 1970s, after the scales fell from my eyes and I became a cognitivist reprobate, I began seeking both the best way to formally interpret ecological states and a way that internal states might be reconceptualized so as to retain their methodological usefulness, even if shorn of their putative explanatory value. The proposal for a dual-state machine that Todd and I (Shaw & Todd, 1980) offered three decades ago still seems to me to be on the right track and worthy of serious consideration by those with a computational bent but who do not uncritically assume internal state representations are necessary to psychological explanations:

In fact the ecological approach to perception ... proceeds upon the assumption that they [organism and environment] must be treated jointly and that they entail a mutually defined, integral unit of analysis whose "states" are neither internal nor external. Although it may be useful for methodological reasons to focus temporarily on a single interpretation in isolation, one cannot lose sight of their reciprocal nature without losing something essential. (Shaw & Todd, 1980, p. 400)

We go on to elaborate what general purpose internal state descriptions serve in modeling psychological systems, as seen from our ecological perspective:

The existence of so-called "internal states," $Q(t)$, is nothing more than a convenient fiction of contemporary computer science methodology, which allows the programmer, in lieu of evolution and learning opportunities, to provide machines which have no natural histories, $H(t)$, with artificial ones. ... Algorithmic models of perceptual phenomena ... may provide a useful summary of the complex histories of animal-environment transactions by which the perceptual systems under study might have become attuned. (Shaw & Todd, 1980, p. 400)

On the other hand, such theorists seeking models should not fall under the spell of mechanistic reductionism. Reductionistic idealism is just as bad as reductionistic materialism, for they both depend on being fixated at the level of the FOIF. But why reductionism at all when there is good reason to incorporate higher order isomorphisms into one's theory—either as a gestaltist (second-order) because of

transposability, or as a traditional ecological psychologist (third-order) because of affordances as invariants of invariants, or as one who adopts coalitions (fourth-order) as models of ecosystems because of the generative closure property, as I do?

In any case, theorists are admonished to be circumspect and not take the internal state description fostered by this methodological tool of programming as license to reify the ghostly states of mind. An excellent review of Shaw and Todd (1980) and perspicuous discussion of the consequences of this old way of thinking for cognitive neuroscience has been given by Clancey (1997, chap. 12), for which I am appreciative.

If one rejects Simon's physical symbol hypothesis with its reliance on simulation as being an instance of a FOIF because it requires correspondence between the world and internal states, then there are new options to consider in state space analysis. State space analysis is popular because of the history and usefulness of differential equations in physics and engineering. As pointed out, Todd and I (Shaw & Todd, 1980) introduced the rudiments for a new kind of state space analysis in 1980 in a short BBS commentary on Ullman's (1980) target article attacking Gibson's direct perception. Todd and I (Shaw & Todd, 1980) believed then, as I do now, that a state space comprising relational states might serve us better than ordinary state space with its singular states. This followed from seeing the need of states that have one foot in the environment and the other foot in the organism to model affordances.

Thus, what we had in mind might better be called *dual-state* space analysis. I have had a hand in developing this idea further with every generation of students who have worked with me since then. Only in the place of states, even dual states, we have come to appreciate the additional power and appropriateness of dual path descriptions (for an introduction into path spaces for psychology, see Kadar & Shaw, 2000; Shaw, Kadar, & Kinsella-Shaw, 1995; Shaw, Kadar, Sim, & Repperger, 1992; Shaw & Kinsella-Shaw, 1988).

The use of internal states as the primary construct in psychological modeling carries an insidious flaw, a danger of leaving perceptual knowledge unfounded as well as short-circuiting the ability for staying in contact with the world through the grounding of the actions of situated agents. Simon (1969/1996) agreed in part with this dual-state space sentiment. I find it perplexing that he could hold to internal (cognitive) state descriptions while at the same time harboring the following dual (ecological) state sentiments:

The artificial world is centered precisely on this interface between the inner and the outer environments; it is concerned with attaining goals by adapting the former to the latter. The proper study of those who are concerned with the artificial is the way in which the adaptation of means to environments is brought about—and central to that is the process of design itself. (p. 113)

Simon (1969/1996) further argued: "Symbol systems are almost the quintessential artifacts, for adaptivity to an environment is their whole *raison d'être*. They are goal-seeking, information-processing systems, usually enlisted in the service of the

larger systems in which they are incorporated” (p. 22). Yet, Simon also argued that “symbol structures can, and commonly do, serve as internal representations (e.g., ‘mental images’) of the environments to which the system is seeking to adapt” (p. 22).

Simon (1969/1996) recognized also that for a symbol system to be useful, it must have windows on the world and hands, too. It must have means for acquiring information from the external environment that can be encoded into internal symbols, as well as means for producing symbols that initiate action upon the environment. (p. 22)

How can symbols have “windows” unless we admit, as even Fodor and Pylyshyn (1981) did, that at some time or other they were either directly perceived as grounded or as being situated with other symbols that were?

From the preceding quote, one sees that Simon (1969/1996) clearly recognized that symbols must be relational in at least one sense; they are assigned two simultaneous locations (i.e., dual states). Although they reside in the agent as mental constructs, they must designate properties and events in the world. They must have, as it were, one foot in each camp. A convention must be learned to make this relationship hold. Sans convention, this is reminiscent of Gibson. For Gibson (1979/1986), this symbol function is supplanted in perception by what I earlier called *direct commutators*, the purveyors of the ecological relations born, in general, by information and more specifically by affordances:

But, actually, an affordance is neither an objective property nor a subjective property; or it is both if you like. An affordance cuts across the dichotomy of subjective-objective and helps us to understand its inadequacy. It is equally a fact of the environment and a fact of behavior. It is both physical and psychical, yet neither. An affordance points both ways, to the environment and to the observer. (p. 129)

Hence, it would seem that what separates Simon and Gibson is not what they believe must take place if adaptive knowledge is to be possible, but how it could. Little wonder that when Simon was confronted with the affordance concept, he identified it with a symbol—for he had no place else to hang its function except on an internal state. One sees this in the following quotation:

Contrary to Gibson’s (1977) view, the thing that corresponds to an affordance is a symbol stored in central memory denoting the encoding in functional terms of a complex visual display, the latter produced, in turn, by the actual physical scene that is being viewed. (Vera & Simon, 1993, p. 20)

The nub of Simon’s and Gibson’s disagreement seems to devolve on the willingness to use internal states in their explanations of how agents can know their environments. Can Gibsonians really get by with a theory of this knowing relation that avoids the FOIF implied by using internal states? In spite of arguments to the contrary (e.g., Wells, 2002), I think they can. I consider this question next.

A SCIENCE IN THE FIRST-PERSON VOICE

Although using internal states is bad, it is not so bad as reifying them. The tricky issue for most computationalists to grasp is that one does not necessarily have to assume that states of being aware of something are internal. They may instead be dual states, which is tantamount to requiring that the description of states of awareness be cast at the ecological scale. Why the temptation to treat them as internal? Here is one reason.

From a third-person perspective, for example, that of the scientist or programmer, states of being aware of something are seen as being private because they are not shared at the moment by others. However, private experiences are still observed and therefore observable. If one person or species can participate in such experiences (notice the locution here!), logic does not allow excluding the possibility that others do as well. For on what grounds could they be excluded other than philosophical prejudice? Gibson (1979/1986) like Simon had his own syllogism that underwrites his theory:

At the ecological scale, the basic premise is compounded and fourfold:

A thing means what it is. (ontological premise)

To perceive is to be aware. (epistemological premise)

To be aware is to be aware of some thing. (psychological premise)

To be aware of some thing is to know its meaning. (intentionality premise)

Therefore:

To perceive something is to know its meaning. (direct perception conclusion)

There are no terms omitted from this argument; hence, there is no room for the addition of mediating constructs or anything else to this syllogism. The conclusion follows directly from the premises. Collectively, the four premises as a compound define the relational complex that is identified as the basic ecological premise. The total meaning of perception being direct is conveyed fully by the whole argument (the compound ecological premise); also, as promised earlier, it is revealed to be not an assumption but a conclusion. Because this is so, if one buys into the compound ecological premise, one cannot avoid accepting direct perception as an unavoidable implication—indeed, as a direct inference.

Therefore, it is difficult to see Simon as an ecological psychologist despite his obvious ecological sentiments because he refuses to buy into the premises of the Gibsonian syllogism—which makes direct perception a theorem of ecological psychology.

Furthermore, this argument also implies that ecological psychology is science to be done in the first-person voice of the actor-perceiver, unlike other natural sciences

that are carried out in the third-person voice of the scientist as observer-experimenter. The former is expropriospecific and proexterospecific (ecological psychology), whereas the latter is either exterospecific (objective physics) or propriospecific (subjective psychology).

Ecological psychology puts the first-person voice into a science of agents—agents who are trying to sustain adaptive behaviors by maintaining dynamical contact, in some felicitous way, with a potentially changing reality where threats and opportunities abound. On the spot, opportunistic awareness of what is changing and what is persisting is demanded. Direct perception is evolution's answer to this demand for situational awareness as well as for the grounding of actions so they can be realistic rather than fanciful. Grounded situational awareness is the agent's direct perception of what surrounds it, what is changing, and what is emerging. Moreover, in keeping with the Tristram Shandy paradox, there is no time for the agent's cognitive machinery to grind out representations, nor any need for it.

Direct perception answers the question of how agents can stay grounded in reality rather than merely having knowledge of it in the form of true beliefs. One need not assume animals have belief systems to believe that they have realistic expectations; their having attunement to the relevant affordances is truth enough. No belief is involved. Actions that succeed need not be believed to exist to have the consequences they do. The field of cognitive psychology has been too preoccupied with illusions, false beliefs, misperceptions, and other anomalies and not sufficiently occupied with discovering the ecological laws animals follow in functioning as adaptive as they are observed to do. Seeing that this is so is one consequence of adopting the first-person voice in science.

Conversely, by treating agents as artifacts, Simon's science retains the traditional third-person voice used by mechanists. Structuralists such as Titchener tried to have a science in the first-person voice, just as behaviorism tried to have a science in the third-person voice. Both failed because they isolated their respective premises, destroying the integrity of the compound ecological premise. Both quantum physics and relativity physics have placeholders for agents who observe and hence leave open the possibility of introducing the first-person voice into science. However, quantum physics fails because of the measurement problem (collapse of the wave function) and relativity theory because of frame discrepancies (breakdown of general covariance over reference frames).

Unfortunately, in both physical cases they can only approximate the ecological stance through extralogical concessions to the imperfections in the respective theory: Quantum physics allows it only where the mathematics of the wave equation fails to explain collapse and relativity physics only where the principle of general covariance fails to hold (i.e., where no tensor invariant solutions hold over frames). What is needed is a science not based on the weaknesses of other sciences but on its own inherent strengths. Since Simon's ploy of reducing all systems that have been adapted or that are self-adapting to artifacts, they become fodder for the sciences of the artificial, and the first-person voice is lost.

Not so with Gibson. In accepting no less than the compound ecological premise, agents retain their voices and their proper share of responsibility for success or failure in adaptation. Mechanism, on the other hand, fails. Because of its inherent dependency on less than the full complement of premises, it cannot be relational; not being relational, it cannot be intentional (in the broad sense of making reference beyond itself).

Thus, mechanisms serve awareness but cannot specify its content. To truly serve well they must have malleable rather than fixed state sets. Becoming aware of *this* when before you were aware of *that* is to change from a state set that includes *this* to one that includes *that*. Systems governed by rules cannot react to spontaneity; rules are hindered by too much inertia. Rules may work for persistent phenomena, but try to imagine a rule for recognizing spontaneity in happenings.

Consequently, I have always found it more than a little ironic that the most common complaint raised against ecological psychology is that it cares too little about mechanism, as if it should. We as ecological psychologists are warned that our approach will not command attention from the scientific community until it couches its explanations in mechanistic terms—as if the mechanistic philosophy would rule forever! On the contrary, it is well recognized that the reliance on mechanism in science is in general decline (d'Abro, 1952). Here is a typical example of such shortsighted criticism:

One might claim that certain information about the world is picked up without processing intermediate types of information, but in support, one must produce the smart device itself and show the secret of its operation. ... This type of analysis is still missing from the Gibsonian approach. ... We believe that their call for a radical reconceptualization of perceptual psychology will not meet with much favorable reaction until the mechanism underlying some *perceptual* process is revealed. (Kubovy & Pomerantz, 1981, p. 450)

Of course, Kubovy and Pomerantz (1981) were quite right in their expectations of what the field would accept as an explanation but quite wrong in implying that a mechanistic explanation is all that should be scientifically acceptable. For ecological psychologists and contemporary physics, laws rather than mechanisms are the preferred basis for explanations (d'Abro, 1952; Feynman, 1967). In the long run, one might well ask: Will scientific contempt be more likely deserved by ecological psychology or mechanistic psychology? Which is more in keeping with contemporary scientific trends and which more old fashioned?

The role of awareness is of paramount importance to science in the first-person voice because it is synonymous with direct perception. Next I show why.

AWARENESS AS DIRECTNESS

I am not aware of something being directly perceived, I am only aware of the something. Put differently, directness and awareness are the same; one is not an accom-

paniment or a garnish for the other, and they are both self-presenting the way pains and colors are—being incapable of being mediated. The semantics here are the same as for noticing. I cannot notice that x is the case without being aware of x , and I cannot notice x and not do so directly. The problem that traditionalists have who accept Cartesian mind–matter dualism is that they want to place awareness on the psychical side of the “epistemic cut” and the object of awareness on the material side. Gibson would not allow this: For him my direct perception of an environmental property, object, or event is also the awareness of a mutual property of self to which that property, object, or event is referred. Under this mutuality of cospecification, the agent and environment components retain their distinction but their separateness is no longer real.

There is no object of my awareness other than the object itself taken in reference to me. No addition to me is necessary; no internal state is required, for it is the whole system that resonates, not some local part (as Grossberg, 1980, argued). There is no awareness of a second object, which acts as a symbol or representation of the first object, that makes me notice the first object, for if there were, then by such direct noticing the mediator is made superfluous. If a mediator can be made superfluous, why is it needed in the first place? How did its mediation function originate?

How could a symbol become associated with its referent object without my noticing the referent object being associated with the symbol? It would be like Alice noticing the smile of the Cheshire cat without noticing the cat, or noticing the affordance without noticing the object that exhibits the affordance, or one object as being in an adjacency relationship without noticing at the same time that a second object is as well. There can no more be just one object noticed in associations, adjacencies, symbol functions, or representations than there can be the sound of one hand clapping. Because such subtle logic rarely wins any converts to the theory of direct perception, I try a different line of attack.

This logic denies that the epistemic cut should follow the fault line of the Cartesian mind–matter dichotomy. Instead, Gibson suggested that no cut at all be placed at the organism–environment synergy. However, with respect to epistemic cut of knower versus known, where most psychologists and philosophers are happy naming the divide the subjective–objective, Gibson would rather we repair the cut entirely by a kind of relational integration, for this is the main purpose for introducing ecological scaling in the first place.

If Gibson would have us replace the word *objective* with *socially invariant*, with what should we replace the word *subjective*? In its place Gibson used the term *awareness* in a way that is not meant to connote the person being in a particular internal state, namely, a state of consciousness. About this Gibson (1979/1986) wrote the following:

Perceiving is an achievement of the individual, not an appearance in the theater of the mind. 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 an 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 which one is aware. (p. 239)

One might quibble and ask: Is not being aware synonymous to being in an internal state of awareness? For Gibson, as for James (1911), the answer has to be an emphatic “no!” For although an experience with concurrent awareness of the world and the self must surely have both physical external (environmental) support and internal neurological support—on this point Simon and Gibson agree—the ecological function and the psychological meaning of this necessary condition is first and foremost its socially invariant function. Here Simon and all other critics that I have read misunderstand Gibson’s take on the issue—with Clancey (1997) being perhaps a notable exception. I tried to make the ecological position clear in a recent *Ecological Psychology* article (Shaw, 2001), but I try again in this context.

This idea of being aware of x is akin to James’s (1911) radical empiricistic notion that we are in the experience rather than the experience being in us. Dewey (1896) also understood this and made it a cornerstone of his brand of functionalism. He claimed that we interpolate into the core meaning of a stimulus what our actions with respect to it should be, as opposed to tacking responses onto it as associated linkages. This idea is also reflected by Mace’s (1977) apt title that admonishes one to “ask not what’s inside your head, but what your head’s inside of.” Such an experience involves an awareness of the other and the self concurrently. Therefore, it is neither internal nor external, nor both, if you prefer. It exhibits simultaneously a mutuality of environmental information and a reciprocity of perspectival information. It expresses awareness of the world in a chorus of first, second, and third voices.

To refer to such experiences as being internal states is simply grossly inappropriate because they are *observables* (in my extended use of the physicist’s term); they are socially shareable rather than private. Hence, ecologically invariant experiences are really not objective but social, not external (belonging only to the environment) nor internal (belonging only to the agent).

Finally, let us return to where we came in and reconsider the case in favor of laws at the ecological scale.

RULES AND LAWS IN LIEU OF MECHANISM

Ecological rules are expressed in the imperative mood (i.e., are deontic), as Simon proposed for his rules, but apply to the agent as actor and not to the programmer or designer attempting to simulate agents (although such rules may help inform programmers of what rules need to be simulated or help interface designers set up their problems so that solutions are possible).

Experiences may be shared by others on the same or different occasions because they are not always private but common public social experiences. Humans have all shared the same types of experience such as pain, love, fear, anger, joy, and so forth, even if the token experiences were different, that is, the objects of those experiences differed. Gibson's notion of invariant environmental information allows one to go beyond the claim that we as humans can share only the same types of experiences to validate the claim that because we are aware of the same affordances, we can share the very same experiences.

If this claim seems strained or strange to you, then it is likely that you treat experiences as mental rather than as social constructs and concentrate unduly on their differences rather than their invariant core of similarities. It is not just information about the environment; it is socially invariant information about the environment. It is socially invariant because it remains invariant over different points of observations that might be occupied by different observers of similar design and attunement at different times. This is a consequence of generative closure and the most crucial feature of experiences as ecologically construed (i.e., as coalitional). To drive this point home, I typically cite the insightful title of the paper by Mace (1977) given earlier. It bears repeating as the ecological mantra:

Ask not what's inside your head, but what your head's inside of!

Mace, of course, asked that one situate the agent as a whole and not just his head.

Because all creatures, from insects to humans, share the same general environment, although different habitats (where they live in that general environment) and niches (how they live there), they will share all affordances that hold in general and some others that are invariant over their habitats and relevant to their niches. This is what Gibson meant by affordances and information being objective in some sense. Such objective (socially invariant) information is lawful to the extent that it can be counted on when acted upon.

In his 1979 book, Gibson (1979/1986) gave 10 examples of ecological laws, each of which can be used to underwrite a rule for the perceptual control of action. Such rules are deontic in the sense discussed earlier because they follow the law of obligation on which imperative logics should be based. I also mentioned earlier that I had something to say on this issue; here is a sketch of what needs to be explained more elaborately at some other time:

For each ecological law identified, it is possible to write over it a rule that uses the law so as to allow perceptual information to control an action. I ask you to note carefully the deontic structure of the rule and how it is underwritten by a law at the ecological scale while all the time preserving the first-person perspective of the agent of choice. The rule is, of course, to be followed by the agent—whether available through the agent having discovered it and then learned it or having it evolved into the agent's design. This is the essence of the ecological interface design problem.

Here are some examples of ecological laws from Gibson's (1979/1986) list that have universal application across all properly situated and attuned members of any species of agents:

Flow of the ambient array specifies locomotion, and non flow specifies stasis. (p. 227)

Here Gibson (1979/1986) meant the *optic* array and that if the flow is *global*, it specifies *self-locomotion*, whereas if *local*, then it specifies that something else moves. A rule for the perceptual control of action can easily be devised from an information law by simply finding its first-person voice, changing the mood of the statement from the indicative to the imperative, and incorporating a controlling final condition (purpose) set in the optative mood (the mood in which needs, wants, and intents are defined). To wit:

If I want to move, then I must cause the optic array to undergo a global transformation!

As a hypothetical conditional, it is not yet well posed. It needs to be unconditional, namely, it must provide a definite answer to the question (interrogative mood), Do I want to move (optative mood)? Yes? Then I must follow this rule (imperative mood) that abides by the relevant law (indicative mood). An important implication is that agents are not compelled to act by a law but must learn the rules that apply the law appropriate to their intents. Particles have no option but to move as the law dictates—once initialized for them. Agents have the option to alter their intents; however, once the intent is in place and the law adopted that can finalize that intent (help reach the goal unless thwarted by unforeseen circumstances), then the agent must abide by the law. This conveys the imperative import of ecological law and reveals clearly its deontic nature.

Hence, an ecological law applies just as inexorably for agents as dynamical laws do for particles. Whereas the dynamical law must be initialized to apply, the ecological law must be finalized. (Both must satisfy a set of boundary conditions impressed by the law being situated in a particular case.) Finalizing an ecological law may require that the agent select, from the set of rules that it knows, the proper one for satisfying the intention it holds. Elsewhere this has been called the *fundamental problem of intentional dynamics*; for Simon, it is the fundamental problem of design theory. The two approaches are very similar, both being related to but not reducible to traditional optimizing techniques and utility theory.

Here is another ecological law that is less general—applying only to certain kinds of actors but is nested under the previous law:

Flow of the textural ambient array just behind certain occluding protrusions into the field of view specifies locomotion by an animal with feet. (Gibson, 1979/1986, p. 229)

This law is specific to pedal animals and thus, unlike the first law, does not apply to all animals. It is nevertheless general to all pedally locomotive creatures and underwrites a rule for action as well:

If you want to walk, then cause the optic array to undergo a global transformation such that the leading edge of the feet delete the textual array and the trailing edge accretes it!

In this, one sees that ecological laws and the action rules they underwrite may be graded from the universal to the specific.

The key point to recognize is that these are rules that hold in the first-person and not just for one person but for all persons that fall within its range of application. To reiterate, whereas some laws are sufficiently general to be considered universal laws at the ecological scale in that they hold without exception (e.g., the first law mentioned previously), other laws may be less general (e.g., laws that underwrite rules for aquatic, arboreal, or flying creatures). Laws therefore need to be graded for the rules they enforce, from less to more general, up to their maximum degree of social invariance.

One of the virtues of being able to treat psychological phenomena as law governed is the economy it gives to explanations. If one has laws, then one does not have to worry about discovering underlying, deeply rooted mechanisms to have an explanation. Law-based explanations, unlike mechanisms, tend to be logically shallow and highly ramified. The more general the law, the more shallow and more ramified its application. Mechanisms that are “smart” in Runeson’s (1977) sense become unwieldy and fragile if allowed to grow too large. This is, I truly believe, a consequence of the truth of von Neumann’s (1949/1966) conjecture. Have you ever had to redo a program or a proof because it got so unwieldy that it was safer and less time consuming just to do it over?

REDUCING THE WONDERFUL IN NATURE TO THE COMMONPLACE IN SCIENCE

One must be careful not to romanticize nature. Hardheaded scientists who think that to be objective one must dispel wonder in favor of the familiar, even if banal, will never accept Gibson’s use of a phenomenological basis to his science (Kadar & Effken, 1994). It is recognized that awareness is not a third-person observable but a first-person experience. Observables can be formalized, experiences cannot; but they might be shared. Note that because of this sharing (social invariants), Gibson’s phenomenological basis to ecological science avoids the subjective flaw (introspective method) that precludes mentalism from making serious advances. Having awareness of *x* as a social invariant is like having cake and eating it too—one gets subjectivity and objectivity wrapped up in a single package.

Simon (1969/1996) also wanted his cake and to eat it too. Here is what he said is the goal of science: “The central task in a natural science is to make the wonderful commonplace: to show that complexity, correctly viewed, is only a mask for simplicity; to find a pattern in apparent chaos” (p. 1). In short, to make the wonderful comprehensible by decomposing it into simple parts and simple movements, the dream of every mechanist is a love affair with reductionism. Simon, though, wanted it all: wonder as comprehensible, complexity as simplicity. Forgive me, but this sounds a little like Orwellian doublespeak. Simon must have realized that he damaged his credibility by speaking too plainly of his goals; therefore, he amended his statement:

This is the task of natural science: to show that the wonderful is not incomprehensible, to show how it can be comprehended—but not to destroy wonder. For when we have explained the wonderful, unmasked the hidden pattern, *a new wonder arises at how complexity was woven out of simplicity* [italics added]. (p. 3)

I am indeed nearly seduced by the eloquence of these last lines, but it hides an insidious assumption, and one I believe, as von Neumann (1949/1966) did, likely to be quite false. The phrase “how complexity was woven out of simplicity” assumes that complexity is predicative and hence recursive—that if one only uses a divide and conquer, all will be well. Yet, what if complexity is neither predicative (e.g., decidable in the logician’s sense) nor recursive? Ironically, being recursive and predicative is what the systems theorist par excellence Rosen (1991, 1999) called the defining characteristics of simple systems. What if complexity is by nature rather than artifice (formal description) a limitless source of generatively specified impredicativities, that is, undecidable predicates, as von Neumann, Penrose, and Rosen all suspected? What then?

Complex systems that can be reduced to simple systems are not complex at all but mistakenly described. Wonders that reduce to the banal are not wonderful at all but sadly denuded spectacles. Magicians’ tricks may be so reduced, but nature is no magician to have its secrets so easily revealed. Still one knows of the wonder because one can directly perceive it; it is a self-presenting fact about the universe, not a symbol stored in central memory. Of course, this is an attitude to be shared, not an argument to be won.

A more manageable query that needs attention that grows from this aesthetic sense of wonder and our stubborn concupiscence as rationalists is this: How can some systems be so shallow in terms of logical layers and yet so ramified in their generality? Gibson (1979/1986) approached an answer to this question with his notion of invariants of invariants (what was earlier encountered as third-order isomorphism). He recognized that the difficulty psychologists typically have had in understanding his concept of invariants (second-order isomorphism) would be exacerbated in the case of affordances as higher order invariants. With respect to the typical psychologist, Gibson made this pessimistic prediction:

He may concede the invariants of structured stimulation that specify surfaces and how they are laid out and what they are made of. But he may boggle at the invariant combinations of invariants that specify the affordances of the environment for an observer. The skeptic familiar with the experimental control of stimulus variables has enough trouble understanding the invariant variables I have been proposing without being asked to accept invariants of invariants. (p. 140)

Gibson opened the door for going even further toward generality to be obtained by composing invariants of invariants to get higher and higher order affordances: “Nevertheless, a unique combination of invariants, a *compound* invariant, is just another invariant. It is a unit, and the components do not have to be combined or associated” (Gibson, 1979/1986, pp. 140–141). In this way explanations might be achieved that become more logically shallow as they become more highly ramified. Coalitions then, as fourth-order isomorphisms, should give us models that are most shallow and most highly ramified in their applications to nature.

In fairness, it should be pointed out that Simon (1969/1996) surely agreed that systems should be broader but less deep and has discussed extensively how symbol processing systems might capitalize on these desirable properties. For instance, he told the story of two watchmakers, Tempus and Hora, one who benefits from modular construction if the watch is dropped, and the other who does not. If one has hierarchic depth in a system to contend with, then modularity can help insulate against mistakes made at one level from propagating across all levels. This has been a popular context-free strategy where situating and grounding the components is ignored.

Hence, there is a clear difference between Gibson’s and Simon’s two proposals, for modularity is mechanistic and invokes layers and layers of mediation, whereas compounding invariants keeps each higher order level just as direct as the one below it and hence as situated. For Gibson, situational awareness is defined over these higher order invariants under which the contextual meanings are nested and recoverable by differentiation of the available information. For Simon, the recoverability seems both less assured and more mysterious.

Prospective control by anticipatory information is a necessary ingredient for any adequate theory of situated action. On this Simon and Gibson agreed, as is seen next.

PROSPECTIVE CONTROL

In 1709 Bishop Berkeley reminded us that the chief end of perception is to enable agents “to foresee the benefit or injury which is like to ensue upon application of their own bodies to this or that body which is at a distance.” Two hundred seventy years later, as mentioned earlier, Gibson (1979/1986) endorsed this adaptive function for vision:

What the philosopher called foresight is what I call the *perception of the affordance*. To see at a distance what the object affords on contact is “necessary for the preservation of the animal.” ... But it must be able to see affordances from afar. A rule for the visual control of locomotion might be this: so move as to obtain beneficial encounters with objects and places and to prevent injurious encounters. (p. 232)

Simon (1969/1996) provided a very similar formulation of rules for action as produced by means–end analysis:

The distinction between the world as sensed and the world as acted upon defines the basic condition for the survival of adaptive organisms. The organism must develop correlations between goals in the sensed world and actions in the world of process. When they are made conscious and verbalized, these correlations correspond to what we usually call means–ends analysis. (p. 210)

It is obvious that Gibson’s rule for the perceptual control of action, like Simon’s, must also have the agent develop such correlations between the information it intends to receive and the information that it comes to receive after proper movements. Simon (1969/1996) goes on to summarize the rule this way: “Given a desired state of affairs and an existing state of affairs, the task of an adaptive organism is to find the difference between these two states and then to find the correlating process that will erase the difference” (p. 210). If one did not know it was Simon speaking, one might have well imagined it to be Gibson.

There are two conditions that such rules for adaptive action should satisfy. First, it does not matter how one erases the difference between the information specifying a current state of affairs and information specifying the future state of intended affairs as long as it gets erased. This means that, as in quantum theory, the only measurements that need be compared are remote before and after termini; what goes on in between is only important insofar as it is the means that contribute to the ends intended.

Second, what is to be established is a correlation between goals and actions such that the latter eventually comes into phase with the former. Hence, the adaptive process is one of establishing phase correlations, not one of constructing mediating causal chains. This may sound like the same thing, but it is not. As all students are taught in a first statistics class, *correlation does not mean causation*—meaning that just because two remote events are correlated does not imply that they are causally related. Hence, explanations based on correlations are not mechanistic because they need not be mediated by causal chains.

Likewise, they need not be mediated by anything else either, not by mental representations, ethereal ether, or ghostly ectoplasm. The correlation may just as well be thought of as a direct specification that it performs lawfully as an action-at-a-distance.

CONCLUSIONS

Here I have aired my prejudices so that there can be no misunderstanding as to what I believe is at stake, namely, that some radical, ecological version of science must replace the mechanistic science most psychologists adopt uncritically. Also, the new ecological science should, to remain a science, retain the fundamental appearance of the old science in its appeal to laws of nature. Simon and others of his ilk challenge the idea of the new ecological science that puts knowing agents back into the equation. They make a rebellious countermove away from traditional law-based science—a move that makes the Lockean solipsistic nightmare unavoidable.

Recall that Locke argued that all thought is about ideas and that these ideas either come from perception or reflection—no other source. Hence, he is only recognizing this fact when he says: “Whatsoever the mind perceives in itself, or is the immediate object of perception, thought, or understanding, that I call an idea” (Locke, 1706/1974, pp. 111–112). Locke placed ideas, by definition, as the sole objects of thought, just as Simon placed symbols. Both condoned the cognitive paradox by which one can then only know the virtual objects of thought—ideas and symbols—and not their real-world referents. Locke himself recognized this paradox and merely avoided discussing it, producing instead a new theory later in his treatise (*An Essay Concerning Human Understanding*) that was inconsistent with the earlier one (Russell, 1945).

I do not see how Simon, or any other representationalist for that matter, either avoids Locke’s paradox or resolves it. For Gibson the task is different but not easier. Although Gibson took the objects of thought, like Locke, to be the objects of perception, those objects are not virtual objects (ideas or symbols) but the real objects themselves (reread captions). The difficulty with this direct realist’s view, not confronted by the indirect realist’s account, is that perception takes time; therefore, the object perceived cannot be the present object but must be a retrospective one. If retrospective, how then can it avoid being like a persisting image, or memory trace, a virtual object? In such a case, perception is indeed indirect in that it is mediated somehow by these virtual objects.

To my mind, Gibson offered a brilliant solution to this apparent direct realist’s paradox: This difficulty is overcome by getting rid of “objects” of perception treated as being at a fixed punctiform moment in time (James’s, 1911, so-called specious moment); these are replaced with events whose temporal courses dynamically express invariant properties. (This is the reason that in my work I have adopted paths rather than states, as indicated earlier.) It is these temporally sustained invariant properties of process (persistence over change) that one experiences directly—not some retrospective object of a frozen past moment. These processes (objects of perception construed as paths) resonate over time and space and remain the same objects of memory or thought at later moments if their invariants are preserved. For they are their invariants and not something else! Thus, no virtual objects are needed if one carefully reformulates what is the real object, as perceived or otherwise referred to.

A FINAL WORD

Where have we come? In opposition to Simon's clearly stated "axioms" for a design theory for how artificial systems couple to their environments, I can now state the axioms for an ecological interface design theory:

- Direct perception is a consequence of the self-referentiality of ecological law.
- Rules underwritten by such laws require no mechanism—indeed, can have none.
- Direct specification is a consequence of a system's generative closure property.

These axioms have three corollaries:

- Law-based symbol systems have reducible conventions and require no rule-based mechanism.
- Symbol systems not based on law have irreducible conventions and so require a rule-based mechanism.
- Information underwritten by ecological law is direct specification (acts at a distance); all other forms of information require a symbol manipulation mechanism.

Whereas the set of axioms and corollaries express the sense of both direct and indirect perception, or more generally specification *née* information, the last two corollaries locate the place where computational theories enter legitimately—under indirect specification. There is no room whatsoever for extreme computationalism. Recall Gibson's (1979/1986) radical thesis: "If so, to perceive them is to perceive what they afford. This is a radical hypothesis, for it implies that the 'values' and 'meanings' of things in the environment can be directly perceived" (p. 127).

If one accepts these conclusions and their implications, then it is time to make a beginning toward the new ecological science that Gibson's radical hypothesis called for. To his critics who reject direct perception out of hand, Gibson might have echoed Dr. Samuel Johnson, who is reputed to have said (by Boswell, 1791/1998) in response to a dim carping critic: "Sir, I am obliged to give you an explanation but I am not obliged to give you an understanding."

Gibson's congenial nature, however, would never have allowed him to inflict so unkind a cut. At least Gibson did succeed in giving us an opportunity for understanding a vision of psychology that is quite superior, in my opinion, to the competing view of extreme computationalism and its attendant philosophy of mechanism. Also, quite rightly, it does not rule out an important role for a computational ecological psychology—the latter will just not be center stage.

ACKNOWLEDGEMENTS

I thank Peter Kugler for helpful comments on earlier portions of this article, as well as the students in my graduate seminar: Bruno Galantucci, Steve Harrison, Mike Richardson, and Theo Rhodes. Special thanks to Bill Mace who offered much needed help on all aspects of the article. My apologies to them all for any problems that remain, for which I am of course solely responsible.

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