

access to a power that it is reluctant to relinquish. Thus it is reluctant to follow Smedslund's recommendations, not because he is logically wrong—if psychology is to model itself on the natural sciences, then it ought to follow his proposals—but because, as his own work inadvertently reveals, psychology is at war with itself over its own scientific pretensions.

### Note

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## On Pseudoempiricism, Pseudoductionism, and Common Sense

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There are three reasons why I relish this occasion to observe a brilliant and unflinching rationalist feast on the empirical agenda of general psychology and report that it is indigestible. The first is personal. In 1985–1986, it was my good fortune to dine with Jan Smedslund and David Rosenhan once a week at the Stanford Faculty Club to discuss psychology, anthropology, religion, and life; to observe psychologic (PL) in the making; and to have my own brand of anthropological constructivism chewed over by this master of rationalism and critic of pseudoempirical universals. In my view, psychology, like every other ethnoscience, is an interpretive enterprise (see Shweder & LeVine, 1984), which, being an interpretive enterprise, cannot be reduced to either empiricism (inductive logic) or, as Smedslund would have it, necessary truths (deductive logic). It is with great pleasure that I am reminded of the intellectual excitement of old cherished encounters.

I also relish the occasion for another reason—I am in deep sympathy with Smedslund on a number of points (see Shweder, 1991; Shweder & Fiske, 1986). I think he is right that the metatheoretical concepts of psychology (feeling,

belief, want, emotion, ability, etc.) and the structure of those concepts should be made more explicit, that the conceptual framework of ordinary language (feeling, belief, want, emotion, ability, etc.) ought to be the source of the metatheoretical concepts of psychology, and that various PLs should be theoretically constructed as hypothetical models of possible psychological worlds.

He is right that an empirical psychology based (as it ought to be) on the psychological concepts of ordinary language is not likely to discover many (or any) pure or fundamental or contingently universal laws of psychology and is more likely to make its mark by documenting carefully the historically derived, and ethnographically local psychophysical (task dependent), psychocultural (schema or meaning dependent), and psychobiological (prior time dependent) facts of human functioning.

He is right that many of the proposed universal laws of general psychology (e.g., the law of effect) are not empirical generalizations, that social scientists have been insufficiently attentive to the ontological and epistemological status of their propositions, and that we should always be asking of

our propositions questions such as the following: Are those propositions about language or about the world or about both or neither? Are they contingent or necessary? Necessary in what sense? Can they be coherently denied? How is their validity or usefulness to be judged? Can such propositions ever be disproved? If yes, what would count as totally convincing disproof? If no, under what circumstances should they be abandoned anyway? With what alternative axioms or assumptions might they be replaced?

But this is where my deep sympathies end. At least this is where I think they end. Which brings me to the third reason I relish this occasion.

The issues Smedslund raises are aimed at the very heart of general psychology, and they provide every reader with an opportunity to confront his or her own views about the nature of knowledge of human mental states and the contents of consciousness. Of course that is no easy task. For those happy few who welcome such opportunities, there is the added difficulty that one confronts in Smedslund's essay a veritable thicket of profundities. I suspect that some of my own lingering doubts about PL are related to some of my surviving confusions about what precisely it is that is being argued. So this is the occasion I have been waiting for since Palo Alto to pin things down in my own mind.

In making my way through the shock of PL, I have tried to not get bogged down in quibbles over details. Thus I am not going to quibble over Smedslund's particular examples of pseudoempiricism, even though there is plenty to quibble about there.

For example, here is one of several quibbles I plan to avoid. Concerning sequences in cognitive development, Smedslund states that "if the tasks are related by implication, then one knows a priori that one task must be acquired before the other."

But, one might nuzzle in reply, if that were so, then every taxonomy would have to be learned from the topmost level down. Because "elm" implies "tree," which implies "plant," which implies "living thing," which (ultimately) implies "being," which (ultimately) implies "nothingness," the child would have to start life as a speculative philosopher or theologian. That is certainly not the order—topmost level down—in which ordinary language taxonomies get lexicalized (D'Andrade, 1990), and it is unlikely that is the way they get learned. Typically we learn such orderings by being brought in somewhere in the middle of the taxonomic hierarchy (often because someone is pointing at something and bathing it in descriptions) and then we move up and down piecemeal.

Moreover, temporal sequencing is not really a logical requirement of a relationship of logical implication. Although it is true that we can hold no coherent notion of uncolored green that does not mean that the idea of "color" must be acquired before the idea of "green." The two notions could have been acquired together simultaneously or even in bits and pieces by dialectical iteration.

But, remember, the preceding is just a niggling example of quibbles to be avoided for the sake of coming to terms with the central and potentially profound challenge of Smedslund's piece.

I have also resisted the inclination to see if I could come up with worthy counterinstances of genuine empirical universals in psychology. After reflection, I realized that such an exercise would serve no purpose other than to dare

Smedslund to reinterpret each instance as pseudoempirical, or show that it is reducible to nonpsychological processes, or argue that it is not genuinely universal, which is a rather easy argument to make, given that very few generalizations in psychology have never been tested in more than a few cultures. Moreover, such a dare would not really touch what I take to be the central issue of the essay, PL—what it is about and whether it is the way to go.

Is it the way to go? Well, that depends on what it is about. What does Smedslund really mean by PL? As I read it, his argument is built around a somewhat slippery and moving intellectual object. I think I detect strong, moderate, and weak versions of PL in his article. I like the weak version a lot.

### A Strong Version of Psychologic

The strong version of PL is very Kantian. As every school-child knows, Kant, in response to the Humean empiricism of his day, tried to prove that the very having of an "experience" (whether a sense impression or a somatic sensation) and the very making of an "empirical observation" presupposed knowledge of certain a priori or noncontingent concepts (time, space, similarity, etc.) and propositions (e.g., for every event there is a cause).

Before Kant, many British empiricists had argued that all knowledge is and must be based on experience. Not so, Kant reasoned: There are logically prior concepts and propositions that make experience itself possible, and those concepts and propositions have the force of necessary truths. They cannot be coherently denied. Their validity is presupposed by any attempt to deny them.

On this strong reading of PL, one concludes that Smedslund has in his heart of hearts set himself a task analogous to Kant's—that is, the deduction of the a priori concepts and propositions presupposed or entailed by any "common sense" belief and desire psychology. Although he stops short in this essay of claiming that he has actually come up with the synthetic a priori foundations of psychology, it is fairly clear (from his allusions to "'natural' axioms" and "'correct' definitions" and from his avowal that he is inclined to believe "that there is only one basic PL embedded in all human languages and cultures") that Smedslund is tenacious in his pursuit of PL not simply because he wants us to be more precise and explicit about our meanings, because he has a low tolerance for ambiguity, or because he thinks theoretical modeling has been short-changed in psychology, but rather because he is on a search for the absolute, eternal foundations of psychological knowledge.

It is in the context of a strong Kantian reading of the essay that I hazard an inference about Smedslund's view of science. Notice that despite Smedslund's rhetoric about pseudoempirical research, it is not his point to argue that no knowledge is empirical. The problem, as he sees it, is not that psychology cannot be an empirical undertaking but that as an empirical undertaking it can only discover local culture- or procedure-specific regularities or symptoms and not immutable universal truths. Although Smedslund does not actually come out and say it, I suspect he takes it as axiomatic that the most dignified or genuine end of science is the formulation of eternal truths. And I suspect it is because Smedslund wants psychology to be a genuine science, and supposes that a genuine science must reveal eternal truths,

that he is so unenthusiastic about the empirical agenda in psychology, which he is willing to hand over to the historian. To the extent that a strong reading of PL is a valid model of Smedslund's state of mind, he has turned to PL for scientific salvation.

I do not much like the strong version of PL. It makes me nervous (although I can provide you with no formal or strict definition of *my* state of mind!). I share none of its purported nostalgia for eternal truths and "natural" axioms, and I do not accept the premise that a genuine science must pursue them. Moreover, I am dubious of the fateful humanities-versus-science choice that is the by-product of a strong version of PL—the choice between extreme historical particularism (which Smedslund views as coherent and legitimate, but which he does not prefer, presumably because it is history, not science) and PL (the deduction of logically necessary truths as the route to scientific respectability and eternal truths). Those are not, in my view, the only alternatives to the stalled quest for a general psychology based on empirically induced uniform psychological laws (for discussions of the limitations of the general psychology principle of psychic unity and the prospect for a more pluralistic cultural psychology, see Shweder 1990, 1991; Shweder & Sullivan, 1990). Deep down inside I fear the radical rationalism in the strong version of PL. I worry that on a "strong" diet of PL we might starve to death on a thin gruel of truisms and tautologies.

More crucially, even if we assume, for the sake of argument, that there are "natural" axioms and "correct" definitions, it seems to me that the "pseudoempirical" has its place in the sun. I am happy to invoke the old Kantian dictum that, although percepts without concepts are blind, concepts without pseudoempirical percepts are vacuous or empty. The dictum suggests that it is only through their "pseudoempirical" application that "natural" axioms and "correct" definitions come to life, full-blooded. Here is an example that illustrates the point.

If there is any axiom that qualifies as a "natural" axiom certainly it must be the logical axiom that "nothing can be both A and not A." Indeed, as if to confirm Smedslund's worst fears, several generations of anthropologists, starting with Levy-Bruhl (1910), thought they could empirically determine whether that logical axiom was universal in the thinking of the peoples of the world. They were a bit confused. They failed to recognize that if they were able to understand the thoughts of others, then the axiom, whose presence or absence they were absurdly trying to empirically document, was confirmed in the very act of translation that produced such spurious empirical disproofs of the axiom as a Bororo Indian asserting in native dialect "We are red parrots" or "Sorcerers are bush cats." If there are natural axioms, then it is wrong to think we can disprove them or establish their universality by empirical means. It is wrong to think that their validity is an issue for inductive research.

Nevertheless, even when it comes to "natural axioms" there is an important empirical side to our involvement with them. Notice the way anthropologists today deal with supposed violations of the axiom "nothing can be both A and not A." They give the axiom life by putting it to use in constructing an interpretation of experience, perhaps by treating "sorcerers are bush cats" as a metaphor, as an example of parallel descriptive systems of the "table salt is sodium chloride" variety, as a deliberate or dramatic parody of axiomatic demands for consistency, or even as a bad translation.

Churchman (1961) made the point nicely that there is a constructive dialectical interaction between the axiom and experience. True, the axiom is presupposed and undeniable, and experience does get structured because we apply the axiom. Yet the axiom also gets reinterpreted as we order our experiences. We constantly reinterpret what "A" and "not A" and "can be" mean.

To paraphrase Churchman, the table before me is both green (on top) and black (underneath). I guess nothing can be both A and not A in the same place and at the same time. The top is green to me but gray to you (who are color-blind). I guess nothing can be both A and not A in the same place and at the same time and to the same person. It is green to my eye but hard to my hand. I guess it can not be both A and not A in the same place and at the same time and to the same person and in the same channel for experiencing.

We could, of course, go on, making interpretive use of the axiom to subdivide the "same person" into noncontradictory subsystems (multiple selves), but the point is made. How we concretely and precisely end up ordering our reality is underdetermined by the axiom alone. The axiom gains its reality as we decide which things we will treat as the relevant set of things that "A" cannot be. In reality, the axiom and the experience, the a priori concept and the a posteriori percept, give each other life.

#### A Moderate Version of Psychologic

A second type of reading of the essay has Smedslund proposing a moderate version of PL, in which the axioms and definitions are "plausible" for some interpretive community, but are not incorrigible, undeniable, or "natural." In this moderate version of PL, knowledge is a priori and noncontingent but only relative to axioms that are themselves not a priori. In other words, what ends up as a priori and noncontingent is internal to an axiomatic system, which is itself contingent. The axioms are deniable, although certain deductions are necessary, given the axioms.

There is a problem with this moderate version of PL. Unless Smedslund can establish that there is an empirical basis to the plausibility of his axioms (which he is barred from doing by the very nature of his critique of empirical universals) or that there is a sound deductive basis to the plausibility of his assumptions (which would require a strong version of PL), it is hard for me to understand why anyone should really worry about being convicted of "pseudoempiricism."

Notice that Smedslund does not really establish that the negation of the propositions he examines (e.g., the negation of the law of effect) is strictly senseless or absurd. Had he done that, he would have achieved a strong Kantian version of PL. Instead he appeals to a "nonstrict" procedure, which is really all he can hope for, given his moderate version of PL. That procedure is to start with a proposition that psychologists endorse as true and then to come up with plausible axioms and definitions from which it follows logically, of necessity. I think I can guess why he dubs this procedure "nonstrict."

One reason is that there is a distinction between the "validity" of a deduction and its "soundness." As Posner (1990) noted:

The importance of the distinction between validity and soundness is shown by the fact that a syllogism

can have a true conclusion even though both its premises are false: "All Spartans are wise; Socrates is a Spartan; therefore Socrates is wise." This syllogism—at once valid and unsound—illustrates the limitations of syllogisms [and I would add, of all deductions] as devices for reasoning to the truth. (p. 43)

Here we have an example in which, if those were our axioms, it would have been "pseudoempirical" for us to go out and investigate whether or not Socrates is in fact wise. The lesson I draw from the example is that one should not stop investigating the world empirically just because someone can come up with some "plausible" axioms or definitions from which the proposition follows "of necessity," unless one can demonstrate (inductively or deductively) that the axioms are not false, which is precisely what one cannot do given the moderate version of PL. A nonstrict conviction of "pseudoempiricism" may not be worth worrying too much about.

It seems to me that one does not need to worry much about a "relativized" conviction of "pseudoempiricism" either. As Smedslund moves from a strong to a moderate version of PL, he himself diffuses the force of his own critique, for he acknowledges, quite frankly and properly, that one person's "pseudoempiricism" could be another person's mandated empirical investigation.

According to the moderate version of PL, "necessity" is internal to a conceptual scheme, and there is no "natural" set of axioms to which one must owe allegiance. If that is so, then one is free to switch one's axioms, and if one is free to switch axioms I can see little reason to a priori rule out empirical research traditions of any kind, not even some of those exotic research traditions studied by anthropologists.

Indeed one might argue that the "pseudoempirical" research of others ought to be encouraged. For one thing, even if the reasoning of some other research tradition is "unsound" (derived from wild or false premises) it may lead researchers in that tradition to take an interest in aspects of the empirical world that others might ignore; they might even accumulate some descriptive knowledge about some aspect of reality. Second, one might even be startled by the "pseudoempirical" research findings of some other tradition into a reconsideration of one's own axioms and definitions; one might even be led by those findings to reinterpret reality guided by some other conceptual scheme.

Concerning the exotic research traditions studied by anthropologists, I am reminded of a story told to me by a friend, an Africanist, who had the remarkable experience of being asked to serve on a traditional three-person inquisitorial panel to determine whether witchcraft was the cause of the death of a native lion hunter. In my friend's opinion of the case, the lion hunter had died because he had been mauled by a lion; his "luck" had run out. It made no difference to my friend that the deceased happened to be an expert lion hunter. It made no difference that several eye witnesses testified that the hunter had shot his rifle repeatedly from point-blank range at the lion and that the bullets had no effect on the beast. It made no difference that the lion hunter's wife had been having an affair with another man. It made no difference that the lover had fled.

The Africans on the panel took a different view. They decided to seek the opinion of an expert diagnostician of

witchcraft, who was a member of another tribe. After an autopsy and careful examination of the intestines of the deceased man, the expert testified that the lion was a witch. Bullets could do it no harm.

Given the axioms and definitions of my Western friend, it was an a priori and noncontingent truth that the lover was innocent, that witchcraft had nothing to do with the death of the lion hunter, and that it was a waste of everyone's time and resources to inspect and taxonomize the intestines of the deceased.

Given the axioms and definitions of the other members of the panel, it was utterly unreasonable for my friend to dismiss all the "facts" of the case as pseudoempirical. (Shall we call them "factoids"? One person's facts are another person's factoids?) The panel voted to convict the lover of witchcraft. Somewhat exasperated by the skepticism of my friend, the chief of the tribe assured him that if my friend's wife were to have an affair and my friend were to suddenly die in an uncanny and inexplicable way, he, the chief, would not dismiss it as "bad luck."

Now whatever one thinks of the axiom of witchcraft or of the conviction, at the very least it seems clear that in the service of their axioms and definitions, the members of that tribe have come to know quite a lot about human anatomy. As wild as this case may seem to some readers, I'll bet there are many analogous examples in the history of science and of psychology, examples of factual knowledge gathered under the influence of strange assumptions. It would be a shame to stop inspecting the world just because from the point of view of some optional axiomatic system (some moderate version of PL) the investigation seems "pseudoempirical." Closet Lamarckians will know what I mean (see Steele, 1979).

But perhaps the claim I find most worrisome in the moderate version of PL is the idea that human mental states are conceptual entities, by which Smedslund means that they can be given a formal or strict definition in terms of individually necessary and jointly sufficient criteria. He offers such a definition for the mental state of "sadness." "Sadness," according to Smedslund, is "how we feel when something we want comes to be seen as unattainable or irrevocably lost"; that is, "P in C at t believes that something he or she wants to attain has become, and will remain, unattainable."

Now, I have no difficulty with the practice of constructing hypothetical models in which one stipulates a semantic association between some criteria (concept) and some word, where by "semantic" I mean that the definition is strict—in other words, the criteria are jointly sufficient (e.g., there are no marriageable unmarried males who are not bachelors) and are individually necessary (e.g., there are no marriageable married males who are bachelors). Some nouns in ordinary language (e.g., *bachelor*) may have their meaning by virtue of strict definitions, by virtue of a stipulated semantic association between the word and some concepts or criteria.

The main problem, however, as I see it, is that many, perhaps most, ordinary language terms for human mental states are meaningful all right, but not because they have semantic meanings of that type. And there is a limit to what one can learn by analyzing them as though they were labels for individually necessary and jointly sufficient criteria. A semantic analysis does not typically yield very many logically entailed truths.

Thus, for example, "sadness" is not, of necessity, as Smedslund argues, however one feels when one comes to

believe that the things one wants have become permanently unattainable. If something one wanted came to be seen as unattainable or irrevocably lost, it would be quite wrong to say "I feel sad" if the way one felt was nauseous or startled or relaxed or full of pleasurable sensations.

Moreover, it is perfectly coherent (indeed it may be quite revealing of someone's character) to report in English, truthfully and accurately, "I feel sad every time I attain what I want." When someone uses "sad" that way, we know what they mean. They are telling us that their bodies are touched by success in a rather special way, for example, with feelings such as listlessness or an unpleasant tightness in the chest, and that they feel moved to manage their success in a rather special way, for example, with indifference and by withdrawing from things.

Although I have been suggesting that the particular way one feels, the particular way one is touched somatically (as distinct from the particular construal one places on one's circumstances), is not irrelevant to the meaning of "sadness," I am not arguing for an alternative logical criterion. My examples should not be taken to suggest that it is "feelings" of a particular kind, rather than a situational construal of a particular kind, that is logically implied by the ordinary language term "sad."

Quite the contrary. My point is that very little, perhaps nothing, is logically implied by the term *sadness* or by many other ordinary language words for mental states. Thus, "feelings" do not provide a sufficient account of the emotions. One can experience the tightness of a heart muscle or feel listless without being "sad." Indeed "feelings" do not provide a necessary criterion for the emotions either. One can say "I am sad about that" without feeling anything special at all, but simply as a way to convey sympathy or regret about someone else's loss or to reiterate the nature of one's interpersonal relationship to that person.

The implication, as I see it, of this very sketchy analysis of the meaning of the English ordinary language term *sad* is that meaning is not reducible to semantic meaning or deductive reasoning (which means it is not reducible to PL, in either its strong or its moderate version). Very little about "sadness" is logically implied by an analysis of the various meanings associated with the ordinary language label.

If I am right, then there is a problem with PL in its moderate and strong versions, for it is carried on under the pretense that ordinary language terms for mental states are strictly or semantically definable. Because that pretext is false, PL runs the risk of condemning itself by coming up with arbitrary definitions or, more likely, by representing relevant but logically unnecessary facts (e.g., conditions of perceived irrevocable loss) under the guise of what might be facetiously dubbed pseudoductive truth.

In my view, if one cares to dig deep in the world of psychological facts, it is best to concede straight away to Smedslund his basic and important critical point. When it comes to the current state of our knowledge of persons and their states of mind, it is a matter of "symptoms" and their imaginative interpretation all the way down. That bothers Smedslund, so he turns to deductionism as an alternative to empiricism. It does not bother me. If there are logical essences to states of mind, they are either vacuous or very hard to find, which leaves plenty of room for an interpretive or constructivist psychology of mind.

## A Weak Version of Psychologic

A third type of reading of the essay has Smedslund proposing a weak version of PL, which urges us to worry about the modal and epistemic status of our propositions, state our axioms, clarify our definitions, and construct a set of PLs as hypothetical models of logically possible psychological worlds. This seems to me a very worthy enterprise, and I think Smedslund is right that it is advisable to start the enterprise with the analysis of ordinary language terms for human mental states.

Ordinary language terms for psychological states such as *sad* may not be semantically definable, but that does not mean that the investigation of their use and meaning can be set aside. They are the source of the concepts that a (weak version) psychologist will need to construct hypothetical models of logically possible psychological worlds.

For example, I would argue that ordinary English language emotion terms (e.g., *sadness*, *guilt*, *anger*, *shame*, *envy*) are names for a certain type of interpretive scheme. It is the type of interpretive scheme that is concerned with the symbolic or intentional aspect of "self"-involving somatic experiences. Emotion schemes are concerned with the symbolic or intentional aspects of somatic experience in that they look through somatic experience to what it is about. They represent somatic experience not simply as a feeling (e.g., as tiredness or a heartache) but as a perception and a plan. Indeed, in representing somatic experience (e.g., tiredness and a heartache) as a perception and a plan (e.g., as "sadness"), our interpretive schemes for the emotions constitute the emotions by transforming our feelings into symbolic events.

As an interpretive scheme, "sadness" offers an account of what would make this or that somatic experience (e.g., a tightness in the chest) an accurate or valid or justified perception (e.g., the validity condition of "irrevocable loss," as described by Smedslund). It offers us predictions about how one might be inclined to behave (e.g., the inclination to withdraw from interpersonal contacts) and about what thoughts and images are likely to come to mind (e.g., death, decay, and decline). It offers guidance about how one ought to manage those experiences, inclinations, and thoughts.

"Sadness," for speakers of our language and members of our culture, is the entire interpretive scheme. No piece of it is necessary or sufficient to define "sadness" in the abstract. Any piece of it might be the meaning of *sadness* as the term gets used in this or that context. Most important for a weak version of PL, the parameters of the scheme—varieties of somatic experiences, varieties of situational construals, varieties of behavioral inclinations, varieties of management strategies, and so forth—can be ordered in a logical grid of possible psychological worlds, which can then be empirically evaluated on a global scale.

For an anthropologist and cultural psychologist like me, PL in its weak version is indeed one of the ways to go. I do not have to go to India to discover whether *bachelors* are "marriageable unmarried males"; the association of the word and the criteria is a semantic or logical fact about the language I use. And I do not have to go anywhere if "sadness" is, by stipulated definition, whatever people feel when the things they want seem unattainable.

Fortunately, because the semantic or logical component of

*sadness* (and of other terms for mental states) is at a minimum, there is an important sense in which it is conceivable for “sadness” to be different in India. At the very least we might discover that in one culture people experience “fatigue and heartaches,” but not when valued things become unattainable, whereas in another culture people withdraw from the world when valued objects are irrevocably lost but this withdrawing has been coupled or harnessed in ontogenetic development to a very different somatic experience. If all our universals are known, by strict definition, in advance, thinking will be the only way to travel, and, alas, the armchair will be the only route to salvation. I think a weak version of PL is more fun, although not quite as much fun as dining with Jan Smedslund.

### Notes

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## The Pseudofundamental in Psychology: Psychologic and Psychologism

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In most cultures with which I am familiar, one of the marks of the educated classes has been an appreciation of the “finer things”—fine art, fine cuisine, and great literature. In this sense, Jan Smedslund’s article, “The Pseudoempirical in Psychology and the Case for Psychologic,” provided this author a truly cultural experience. I must confess I do enjoy a good criticism of traditional empirical psychology. There has, in recent years, been no shortage of such criticism on both methodological and philosophical grounds (e.g., Faulconer & Williams, 1985, 1990b; Gergen, 1985; Polkinghorne, 1983; Rychlak, 1988). Smedslund’s arguments against pseudoempirical research and, more important, his indictment of the discipline for its failure to recognize the pseudoempirical are on target and compelling.

Any psychologist interested in the scientific status of the discipline, its epistemological integrity, or its philosophical foundations should endorse Smedslund’s arguments against false claims made for the power and role of traditionally empirical investigation. He makes a reasoned and important call for greater conceptual clarity and more careful thought on the part of psychological scholars, and for a better understanding of the possibilities and limits of empirical research.

Smedslund argues that the proper role of empirical research is not, strictly speaking, epistemological, at least not as commonly practiced, in the interest of “finding empirical laws.” Rather, empirical investigation might be very useful

in establishing local orders—that is, “mapping” or “determining what is locally the case.” This seems to be a reasonable goal for science, consistent with much of the spirit of postmodern thought and with contemporary views on the philosophy of science (e.g., Polkinghorne, 1983, 1990; Taylor, 1987).

However, Smedslund seems to be unsatisfied with this role for science, and seeks to go beyond in order to rescue a more traditional and fundamental project for psychology as a science. He seeks, in the new discipline of psychologic (PL), to establish a conceptually necessary system of definitions, axioms, and postulates that will serve as a scientific language and provide a species of clarity and certainty and, thus, necessity in psychological understanding. Such certainty and necessity have been the distinguishing goals of modern thought (see Faulconer & Williams, 1990a), and, in this sense Smedslund’s project is not sufficiently radical to serve as an important corrective for the misguided, overly empirical discipline he criticizes.

I argue in the remainder of this commentary that his project fails because it is a psychologism. This is, it accepts the notions on which the traditional view, which is the object of its criticism, is founded and falls prey to the same problems. Psychologism is content to accept “states” as explanations of behaviors and embraces the “metaphysics of things” (Williams, 1990) as the necessary grounding of a science of

