

Part I

PRESIDENTIAL ADDRESS

I have the honor to address you on this important day as the President of the American Psychological Association. It is a privilege to stand before you and to share with you the challenges and opportunities that lie ahead. The American Psychological Association is a proud and distinguished organization, and it is my honor to lead you in the future. We will continue to work together to advance the science and practice of psychology, and to improve the lives of all people.

As President, I will continue to work with you to advance the science and practice of psychology. I will continue to work with you to improve the lives of all people. I will continue to work with you to advance the science and practice of psychology, and to improve the lives of all people. I will continue to work with you to advance the science and practice of psychology, and to improve the lives of all people.

Thank you for your support and leadership. I am proud to be your President, and I look forward to working with you to advance the science and practice of psychology, and to improve the lives of all people. I will continue to work with you to advance the science and practice of psychology, and to improve the lives of all people.

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Introduction

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I stand before you because a not-quite-continuous PSA tradition specifies that the past President of the Association introduces his or her successor on the occasion of the Presidential Address. When Kathleen Okruhlik asked whether I would honor that tradition and introduce Bas van Fraassen, I accepted with a pleasure I retain but also with deep trepidation, currently more-or-less in abeyance. Bas is a philosopher whom I greatly admire for his penetrating intelligence, for his breadth of interest, for his clarity of expression, and for the generally light touch with which he pursues a serious and consistent purpose. I have learned much both from him and from his students. No more is required to account for my pleasure in my present role.

My trepidation had a different source. Bas and I appear to belong to very different schools and to have very different concerns. It is notable, for example, that though we are successive Presidents of the PSA, we almost never refer to each other in our writing. My Presidential address two years ago is the only place I remember having dropped his name. The image of two ships passing in the night appears to fit our relationship well. Under such circumstances, I wondered with dismay what on earth I could find to say about him?

Thinking about the problem, I've realized, however, that that way of understanding Bas' and my relationship is fundamentally mistaken. There is an obscure but central and deeply formative respect in which our views are extremely close. Recognizing that shared structural element, furthermore, permits precise localization also of our central difference, of the point from which our views diverge. In introducing Bas, I want to say a few very brief words both about what we share and about where we differ.

What ties Bas and me together is a deep commitment to the model-theoretic or semantic view of theories. Most of you will, I know, think that an extraordinarily odd thing for me to say. Bas, after all, has done a great deal to develop the semantic view. In the English speaking world, at least, his work has done more than any one else's to make it central to the practice of the profession. I, on the other hand, with one minor exception, have never opened my mouth on the subject.¹ With respect to the semantic view of theories, my position resembles that of M. Jourdain, Molière's *bourgeois gentilhomme*, who discovered in middle years that he'd been speaking prose all his life.

What forced that discovery upon me was the original German version of Wolfgang Stegmüller's *Structure and Dynamics of Theories*, received from its author late in 1973 together with an inscription that persuaded me I had to read it. Since I knew neither set theory, nor model theory, nor the German vocabulary in which they were put to use, it took the better part of two years before I finished. But I persevered enthusiastically because, from early in the book, I developed a steadily increasing conviction that, unlike Hilbertian axiomatization, the model-theoretic way of formalizing scientific theories produced something recognizably like what scientists learn and use as the basis for their practice. It really did, I thought, provide a basis for rational reconstructions.

That conviction reached a resounding climax when I encountered Stegmüller's discussion of paradigms, a notion he properly found obscure, but which he argued persuasively should probably be construed as models for a set-theoretic structure. I thought his discussion caught what I had in mind better than any other I'd seen. It gave me from the start an important new way of looking at my own work, and it has continued since to influence the way I formulate it. And it's given me, as well, a deep appreciation of the way Bas reconstructs theories and of the way he discusses the results of his reconstructions.

Seeing that shared model-theoretic element in our work permits recognition of some other shared elements as well. In accounting for the authority of scientific knowledge, both Bas and I give a central role to observation and experiment. Both of us also take a relatively pragmatic view of what is involved in theory choice. And neither of us thinks that the truth of theories can be their correspondence, through and through, with the real world. There are other parallels as well, but I shall not pursue them further here. Instead, I shall speak, even more briefly, of what has made such parallels so difficult to see. I want now, that is, to point towards the precisely localizable point where Bas' views and mine come apart.

If you are still finding what I've said deeply implausible, an important reason is within easy reach. Bas distances himself from the traditional forms of positivism by emphasizing that models are not statements or other linguistic forms but rather objects in the world. I, on the other hand, am the person who insists on talking about what I've variously called language change, or lexical change, or something else of the sort. Though I've recently been increasingly replacing that talk of language change with talk of conceptual change, the central linguistic element in my work is not going to go away. How can I take models seriously, agree that they are non-linguistic entities, and nevertheless refuse to suppress the linguistic element in my own account of science?

Bas himself repeatedly points to the answer. The definition of a class of models must, he concedes, be communicated in a language. But one can, he continues, use any language at all since the required definition is fully translatable from one to another, at least for languages of adequate richness which he takes to be many. That, of course, is the point in his philosophy with which I had begun to disagree even before Bas himself first formulated it. The notion that anything (or at least anything sufficiently elementary) can be said in any language is, I think, a residue of the seventeenth century, of the belief in a Universal Character, for example, or in Lockean simple ideas. No translator believes in any such universal translatability, but it continues to provide the underpinning for much of analytic philosophy.

That underpinning provides, of course, a very natural and comfortable basis for empiricism. But it is realism, rather than empiricism, to which universal translatability is essential. If you are willing, like Bas and me, to dispense with at least the standard forms of realism, limits on translatability are compatible with a very vigorous

empiricism. My central pursuit has for some years been to show how such an empiricism works.

Bas and I should not, therefore, be seen as two ships passing in the night. In philosophy of science, we disagree, I think, about only one thing: the universal translatability of whatever may pass for observation statements. That single disagreement leaves us with almost totally different problematics. But it has not and does not prevent my admiring and learning from him. As I now surrender the podium to him, I anticipate eagerly what he will lay before us.

Notes

¹The exception is my (1976), "Theory-Change as StructureChange: Comments on the Sneed Formalism," *Erkenntnis* 10:179-99. Note that the aspect of Sneed's approach from which I there dissent is the one I here specify as separating my views from Bas'.