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MEASUREMENT IN PSYCHOLOGY

This book traces how such a seemingly immutable idea as measurement proved so malleable when it collided with the subject matter of psychology. It locates philosophical and social influences (such as scientism, practicalism, and Pythagoreanism) reshaping the concept and, at the core of this reshaping, identifies a fundamental problem: the issue of whether psychological attributes really are quantitative. It argues that the idea of measurement now endorsed within psychology actually subverts attempts to establish a genuinely quantitative science and it urges a new direction. It relates views on measurement by thinkers such as Hölder, Russell, Campbell and Nagel to earlier views, such as those of Euclid and Oresme. Within the history of psychology, it considers among others contributions by Fechner, Cattell, Thorndike, Stevens and Suppes. It also contains a non-technical exposition of conjoint measurement theory and recent foundational work by leading measurement theorist R. Duncan Luce.

JOEL MICHELL teaches psychometrics, measurement theory and the history and philosophy of psychology in the Department of Psychology, University of Sydney. He is the author of the textbook *An Introduction to the Logic of Psychological Measurement* (1990) and co-editor of the collection *At Once Scientific and Philosophic* (1996), and has published papers in psychological and philosophical journals.

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PUBLISHED BY THE PRESS SYNDICATE OF THE UNIVERSITY OF CAMBRIDGE
The Pitt Building, Trumpington Street, Cambridge, CB2 1RP, United Kingdom

CAMBRIDGE UNIVERSITY PRESS
The Edinburgh Building, Cambridge, CB2 2RU, United Kingdom
<http://www.cup.cam.ac.uk>
40 West 20th Street, New York, NY 10011-4211, USA <http://www.cup.org>
10 Stamford Road, Oakleigh, Melbourne 3166, Australia

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First published 1999

Printed in the United Kingdom at the University Press, Cambridge

Typeset in Baskerville 11/12.5pt [wv]

A catalogue record for this book is available from the British Library

Library of Congress cataloguing in publication data

Michell, Joel

Measurement in Psychology: critical history of methodological concept/Joel Michell.

p. cm. – (Ideas in context)

Includes bibliographical references.

ISBN 0 521 62120 8

1. Psychometrics – History. 2. Psychology – Methodology – History. I. Title. II. Series.

BF39.M545 1999

150'.28'7–dc21 98–39566 CIP

ISBN 0 521 62120 8 hardback

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To the memory of my father
JOEL BLAMEY MICHELL
1917–1997

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‘We must not ask nature to accommodate herself to what might seem to us the best disposition and order, but must adapt our intellect to what she has made, certain that such is the best and not something else.’

(Galileo to Prince Cesi)

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Preface

This is a book about an error, an error in scientific method fundamental to quantitative psychology. This error became locked into established ways of doing things in that science, that is, it became systemic. Then it was compounded by a higher order error, the effect of which was to disguise the first. Because science is a cognitive enterprise, because scientific methods are fallible methods, and because all scientists are fallible cognisers, the making of errors is par for the course in science and so any particular instance of error is usually only of passing interest. In so far as scientists invite criticism and put their ideas to the test, there is some chance that errors will eventually be corrected. On the other hand, errors that become systemic are of more than passing interest because they show that science's mechanisms for correcting error are themselves fallible and able to break down. Then it is of interest to inquire into the conditions of such errors because they may teach us something about the workings of science. This book is written as a contribution to that endeavour.

In the case studied here, the first of the two errors mentioned was of a familiar enough kind. It was the error of presuming an answer to a scientific question, rather than investigating it empirically. Quantitative psychologists presumed that the psychological attributes which they aspired to measure were quantitative. There is no question that presuming instead of testing was an error in scientific method. Quantitative attributes are attributes having a quite specific kind of structure. The issue of whether psychological attributes have that sort of structure is an empirical issue because there is no necessity that such attributes should be so constituted. Despite this, mainstream quantitative psychologists (that is, the dominant tradition of those attempting either to measure psychological attributes or to theorise about them quantitatively) not

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only neglected to investigate this issue, they presumed that psychological attributes are quantitative, as if no empirical issue were at stake. This way of doing quantitative psychology, begun by its founder, Gustav Theodor Fechner, was followed almost universally throughout the discipline and still dominates it.

The second, higher order, error involved in this case was of a kind far from familiar. It involved accepting a defective definition of a fundamental methodological concept, that of measurement. Given that measurement has been central to science since ancient times and also a more or less permanent feature of non-scientific life since then, it is surprising that quantitative psychologists were able to pull this manoeuvre off. It is even more surprising that it was done, so far as I can tell, entirely in good faith. This definition continues to dominate the discipline. Most quantitative psychologists think that measurement is simply the assignment of numerals to objects and events according to rule. This definition was proposed by the psychologist Stanley Smith Stevens in 1946. Its understanding of the concept of measurement is clearly mistaken because it ignores the fact that only quantitative attributes are measurable. Of course, that feature is no accident, as will be revealed.

Because this second error disguises the first so successfully and has persisted within psychology now for more than half a century, this tissue of errors is of special interest. Errors such as these are not likely to be accidental. They are more likely to be motivated. Locating the motivation is one of my aims. I argue that these errors are connected with ideologies underlying the development of modern psychology. These ideologies are scientism and practicalism. Scientism is a commonly invoked concept and what I mean by it is the view that methods successful in certain ‘paradigmatic’ sciences must also apply to others. The concept of practicalism is one that I have taken from the philosopher John Anderson. I use this term to refer to the view that success in science derives from the solving of ‘practical’ problems. This is to be contrasted with the classical view that success in science simply means finding out how natural systems work. Anderson thought that ‘modern science does not exemplify disinterested inquiry. Its spirit has been “practical”, it has been concerned with “getting things done” . . . not just with finding out what is the case and with the “criticism of the categories” that that involves.’ I show that modern psychol-

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ogists almost universally, from Fechner to Stevens, neglected ‘criticism of the category’ of quantity and its relation to the scientific method of measurement. Inquiry in psychology had to serve the social interests of a discipline anxious to present itself as ‘scientific’ and of a profession equally anxious to present itself as an ‘applied science’. Because these interests were pursued within a wider milieu dominated by views such as Pythagoreanism, presuming that psychological attributes are quantitative would have seemed a much smaller step than, in reality, it was.

It is a curious feature of the recent expansion of interest in ‘science studies’ that few works on the history of errors in science have so far been produced. Perhaps this is because, when it comes to errors, it seems that scientists themselves ‘are far better placed to do that critical job than historians, sociologists, or philosophers’, as Steven Shapin puts it. While scientists, like everyone, need all the help they can get in identifying errors, the history related here confirms Shapin’s observation by showing how internal criticism, when valid, can eventually lead to a revolution in ideas. The fact that, in this case, revolutionary developments have had minimal impact upon established practice, shows how much an understanding of science requires an understanding of the mechanisms of the cognitive affliction I call *systemic error*.

For most of my adult career I have been involved with attempts at psychological measurement in some form or another. I served an ‘apprenticeship’ in applied psychometrics and worked for several years as a students’ counsellor and guidance officer before completing a Ph.D. in the area of attitude measurement. Since then I have taught psychometrics and measurement theory at the University of Sydney, supervised theses on psychological measurement and published in the area. Being an insider has both advantages and disadvantages when it comes to writing history. The advantages are obvious and the disadvantages considerable. There are the problems of being too close to a subject. There is also the problem that by training the insider is usually neither an historian nor a philosopher. I make no claim to be either. Hence, in this book I lean heavily, but selectively, upon historians and philosophers of science. Selectively, because I am reluctant to conform to what I adjudge, albeit as an ‘outsider’, to be the false gods currently worshipped within those disciplines. The result may not please some. This is a risk I am prepared to take because I think

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that with a topic such as this we enter disciplinary borderlands, wherein scientists may also have their say and no one claim the last word.

There certainly is more work to be done on the questions investigated in this book. First, I have confined my attention to published material because it is at this level that the errors and their history impinged upon me. In this case, the public surface of science reveals patterns worth reporting. However, I am sure that there are deeper currents than those identified here. Second, with a couple of notable exceptions, I have confined attention to material available in English. This leaves gaps because the initial error identified occurred first in German psychology and I am sure that the study of the 'quantity objection' within the nineteenth-century German psychological literature would reveal subtleties glossed over in my work. Third, the philosophy of scientific quantification remains a patchy area of study and it still awaits more comprehensive and systematic treatment than I have been able to give it here. Finally, the errors investigated here are not free-standing errors: they form part of a ubiquitous syndrome. There is a particularly pernicious form of Pythagoreanism, according to which the ostensibly qualitative features of human life are squeezed, insensitively and without second thought, into a quantitative mould. This has happened especially in a range of areas where human performance is considered and evaluated. It deserves closer philosophical scrutiny. There are many things in human life which may not be quantitative. They are no worse for that. If nonquantitative, they can be investigated in terms of their own 'categories' and such investigation is no less scientific than measurement. Quantitative structure is but one (important) kind amongst many and it holds no franchise over scientific method in its entirety.

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Acknowledgments

A first draft of most of this book was written between January and June 1995, while I was a visiting professor at the Catholic University of Leuven, Belgium. I was accorded the privilege of using Professor Luc Delbeke's personal library of books on measurement for that period and I thank him for his generosity. Professor Paul De Boeck oversaw my visit and gave me all the support I needed for my research. I thank him heartily, as well. The University of Sydney provided the financial resources necessary for that visit through its programme of special study leave and I am indebted to it, as I am to Professors Robert Boakes and Helen Beh, who as head of the Department of Psychology and acting dean of the Faculty of Science respectively, lent their support to my application. Prior to my time in Belgium, a considerable amount of the preparatory research for this book was funded by the Australian Government via ARC Grants awarded through the University of Sydney. That support was crucial. Subsequently, the Department of Psychology at the University of Sydney supported my research through departmental research grants.

The manuscript was completely rewritten between July 1997 and February 1998, during most of which time I was a Fellow with the Research Institute of Humanities and Social Sciences of the University of Sydney. I am grateful to the Director, Professor Paul Patton, and the Selection Committee for finding merit in my application, and to Professors Stephen Touyz and Ian Curthoys, head and deputy head, respectively, of the Department of Psychology, for supporting my application and for smoothing the way for my acceptance of that honour.

During the period of my research on this project, my research assistants, Fiona Hibberd and Kate Toms, were indispensable. As well, Fiona, with characteristic dedication and enthusiasm, dis-

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charged my normal teaching duties while I enjoyed my fellowship. In recent times, despite the vicissitudes of academic life, Kate has been unstinting in her generous support and personal loyalty. I will always be grateful to them both.

Friends, students and colleagues have commented critically upon earlier drafts, to a greater or lesser extent. I will not list them all, but thank them just the same, especially David Grayson, George Oliphant and Agnes Petocz. At various times I have taken encouragement while engaged in this research from remarks by David Armstrong, John Bigelow, Duncan Luce, Louis Narens and Phil Sutcliffe. Catherine Max of Cambridge University Press has, from the start of negotiations, lent her support and provided useful guidance, especially with respect to the choice of a title. She organised reviews of my first draft and the consequent revisions significantly improved the presentation of my argument. I thank them all.

Finally, over the summer of 1997/98, I took the opportunity to complete this book. I thank Anne for her indulgence and more especially for the understanding and support she has given me throughout this entire project.

JOEL MICHELL