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Relationships between Practice and Research in Personnel Selection: Does the Left Hand Know What the Right Is Doing?

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There has been growing concern, expressed by several authors internationally in Industrial, Work, and Organizational (IWO) psychology, of an increasing divide between research and practice in personnel selection. While many would recognize that selection psychology has flourished as a research-based professional practice, there have been unambiguous signs that the practitioner and scientific wings of the discipline have been moving away from each other over the past decade or so (e.g., Anderson, Herriot, & Hodgkinson, 2001; Dunnette, 1990; Hodgkinson, Herriot, & Anderson, 2001; Sackett, 1994). Indeed, the unhealthy development of a "practitioner–researcher divide" has been climed, the effects of which are undoubtedly deleterious to the synergistic functioning of the combined profession of selection psychology within IWO psychology more generally (Anderson et al., 2001). The aims of this chapter are fourfold:

1. to establish the field of the science–practice interface in selection as a *"process domain"* topic area worthy of research in its own right;
2. to argue that the most pragmatic way forwards is where a *"natural distance"* between research and practice exists *combined with* sufficient and appropriate channels for exchange between the two;
3. to describe a typographical model of four types of research generated in selection psychology; and
4. to present four historic examples of the interface between science and practice in our field, drawing from them to illustrate possible future scenarios.

This chapter considers these vexed issues in relation to recruitment and selection psychology specifically and draws from several examples of functional and dysfunctional relationships between research and practice in personnel selection historically in order to illustrate possible scenarios of the interchange between research and practice. In so doing, this chapter aims to explore possible future relations between the researcher and practitioner wings of the discipline and thus to highlight several mechanisms through which the practice–research interface can be optimized. The argument presented assumes that robust

research should be driving professional practice in one direction, while simultaneously, changes in professional practice should be stimulating new directions for research in the other. That is, that selection psychology should benefit from a bi-directional and synergistic network of relations between research and practice, with each wing of the discipline remaining in sufficiently close contact with each other to avoid isolation and division (Levy-Leboyer, 1988; see also, Rynes, Brown, & Colbert, 2002). While such an argument might seem axiomatic, in this chapter I argue perhaps more controversially for a "natural distance" between the research and practice wings of selection psychology. I argue furthermore that such a distance is not only healthy for the state of the combined profession but is necessary given the complexities of modern-day organizational science. Finally, and in counterbalance, I assert that it is not this distance or "divide" that should concern us unduly but the mechanisms and links for transfer of knowledge in both directions between the practitioner and researcher arms of employee selection psychology. Thus, that the benefits of natural distancing are partially dependent upon compensatory mechanisms for bi-directional knowledge transfer. Let me initially lay out the case for specialization of functions as the inevitable way forward for selection psychology.

Fragmentation or Specialization: Toward a Viable Systems Model of Selection Psychology

Although selection psychology has long been held to be a prototypical example of a highly successful area in IWO psychology precisely because of its science-based practice, there have been relatively few models proposed over the years to illustrate and encapsulate these relationships (Salgado, Viswesvaran, & Ones, 2001; Viswesvaran, Sinangil, Ones, & Anderson, 2001). This has been a regrettable shortcoming in the literature from both a researcher and practitioner perspective, as important questions over links between both interest groups, mechanisms to enhance practice–research interchange, competing reward pressures on researchers versus practitioners, and the transfer of key research findings into organizational practice have remained notably under-explored in selection psychology. Over the years a rather naïve, prescriptive, but unexamined set of assumptions has built up, in essence suggesting that the closer the linkages between research and practice, the better (Anderson et al., 2001). This may be the case, but then again, it may not. Indeed, I argue here that there is a natural distance necessary between research and practice for each to flourish independently and dependently of one another, similar to research and practice in the medical sciences (Rice, 1997). Undoubtedly, specialization of labor now occurs in selection psychology, with early career entrants to our profession deciding within the first few years of their careers whether a scientific track (doctoral research, post-doctoral fellowship, faculty position) or a practitioner career track (trainee consultant, junior consultant, senior consultant) is more to their calling. To switch tracks mid-career has become increasingly problematical and as science has become more methodologically and statistically complex we have witnessed an increasing specialization among younger researchers into one or perhaps at most two sub-areas of the discipline (see also Hyatt et al., 1997).

Is this specialization such a bad thing? Several arguments can be marshaled to present a sensible case that some degree of specialization is both positive and indeed an absolute necessity as selection psychology has become more complex in its scientific designs and efforts. First, consider the parallel with the medical sciences. The expectation that a medical student could go on both to perform successfully as a general practitioner and to engage in meaningful scientific research at the same time would clearly be untenable. Indeed, specialization into either career track would be seen to be absolutely necessary given the complex nature of medical research on the one hand and the demands on practicing doctors to be able to diagnose accurately patients' ailments on the other. Why should selection psychology be any different? Indeed, the complexities of research in modern IWO psychology are such that specialization is similarly necessary, and moreover, our field has grown so rapidly that simply to keep pace with a specific research area, specialization of research topics and interests is nowadays essential (Viswesvaran et al., 2001). Second, and again taking a somewhat wider vantage point, all modern-day professions to a greater or lesser extent exhibit elements of separation between research and practice. This is true in the management sciences (Hodgkinson et al., 2001), law, the medical and health sciences, the actuarial sciences and commercial insurance, industrial economics, and most pertinently, clinical and counseling psychology (Rice, 1997), to name just some possible examples of relevant comparator professions. Again, why should selection psychology be any different? Rather, the question is one of an appropriate degree of specialization coupled with sufficient mechanisms to integrate the scientist and practitioner sub-groups in order to guard against an irreparable divide between the two. Third, it can be argued that science should retain a degree of independence from commercial interests in personnel selection and that this degree of independence is therefore entirely appropriate (e.g., Dunnette, 1990). An excessively pragmatic agenda, or one determined solely by vested commercial interests, would stultify research and would critically limit the range and type of studies being undertaken and research questions being addressed in IWO psychology. Moreover, potentially controversial topics and research questions which may challenge present-day commercial practices may be in danger of never being explored should researchers be restricted to pursuing research agendas determined solely on the grounds of current commercial interests and passing consultancy fads. Fourth, and by inference, it would likewise be unhealthy for practitioners to be in some way confined to only being able to offer consultancy services in areas where scientific research offers overwhelming validation of particular methods or approaches in selection. Indeed, this would critically limit the ability of practitioners to explore new techniques and methods, to be capable of responding to emerging market demands where validatory evidence has yet to be published, and for practitioners to embrace quickly developments in organizational practices ahead of longer-term strategic research efforts (Levy-Leboyer, 1988). So equally, there is a natural distance for practice to inhabit away from the scientific research and this situation of a natural distancing is both healthy and functional for both wings of the profession of selection psychology. Fifth, and finally, there are valuable benefits to be gained on occasions from researchers pursuing lines of enquiry, whole topic areas, and commercially sensitive research away from the day-to-day milieu of demands for immediately applicable action research findings. Early research into the structure of personality and the development of exploratory factor analytical techniques are good examples of where

early-phase, speculative research turned out (in this case decades later) to be highly valued practically and commercially. Even today a research proposal suggesting that personnel scientists wanted to undertake a multi-year project that involved culling thousands of trait descriptor words from dictionaries and other sources, then trying to cluster them using statistical methods which are highly controversial and unfinished, would be none too highly rated. In other instances a natural distance allows researchers to pursue studies that may go counter to current fads or transient commercial interests (e.g., critical research into emotional intelligence), but which may well turn out in the longer term to produce findings that overturn the *zeitgeist* (e.g., meta-analytic findings now indicate that even unstructured interviews have reasonable validity). Again, the question is crucially one of the degree of this distance and the existence of mechanisms to integrate and allow transfer of knowledge between science and practice in the longer term rather than the naïve presumption that science and practice must coexist in precisely the same professional space and be utterly mutually dependent on each other on a day-to-day basis.

Modeling Science–Practice Relations in Personnel Selection

Having argued the case for a natural disjuncture between the scientific and practice wings of selection psychology as a research-based discipline, it is apt to return to the issue of modeling relations between the two. Despite Dunnette's (1990) seminal chapter calling for greater discussion of this issue in IWO psychology, notably little attention has been given to these important relations. This has resulted in few models of these relations having been proposed in the literature let alone having been validated through empirical studies and field research. One exception to this is the model originally proposed by Anderson et al. (2001). In this model we formulated a simple 2×2 factorial along the dimensions of the *rigor* of research and its *relevance* to professional practice. This produced four "cells" of types of research – *Popularist, Pragmatic, Pedantic*, and *Puerile Science*. In an extension and application of this model to selection psychology, Anderson, Lievens, van Dam, and Ryan (in press) presented a number of examples of research occupying each cell together with cut-point indicators to suggest borderlines between high and low conditions on each of the dimensions of methodological rigor and practical relevance. Figure 1.1 illustrates this latter model.

In instances where methodological rigor is low but practical relevance is high, the model suggests that *Popularist Science* can be generated. Here, although the theme of research examined might indeed be a topical one, Popularist studies fail to examine the research question(s) with sufficient methodological rigor. Such studies might have been rushed to publication in an effort to address a "hot topic" theme within selection psychology, for example, or may have been unduly influenced by vested interests in "proving" the relevance of a present fad or favored psychometric tool. Without doubt this quadrant of research in selection psychology points up the importance of independent, expert reviews of manuscripts submitted for publication, but of course not all published sources in our field are refereed journals so Popularist findings do make it into the public domain. Poorly conceived or conducted studies falling into this category, it can be argued, represent
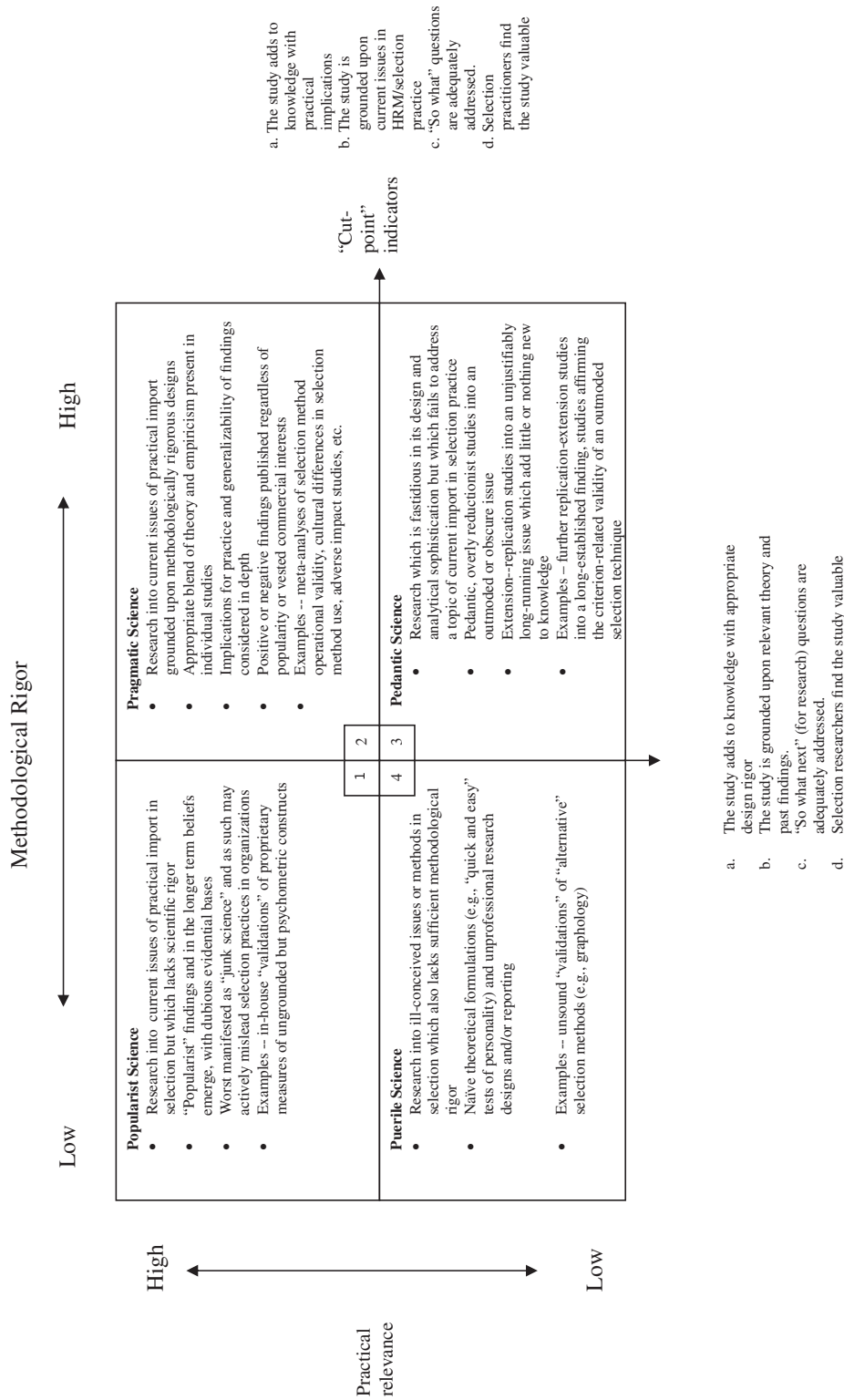
Methodological Rigor

Low — High

Practical relevance: High — Low

Popularist Science
- Research into current issues of practical import in selection but which lacks scientific rigor
- "Popularist" findings and in the longer term beliefs emerge, with dubious evidential bases
- Worst manifested as "junk science" and as such may actively mislead selection practices in organizations
- Examples -- in-house "validations" of proprietary measures of ungrounded but psychometric constructs

Pragmatic Science
- Research into current issues of practical import grounded upon methodologically rigorous designs
- Appropriate blend of theory and empiricism present in individual studies
- Implications for practice and generalizability of findings considered in depth
- Positive or negative findings published regardless of popularity or vested commercial interests
- Examples -- meta-analyses of selection method operational validity, cultural differences in selection method use, adverse impact studies, etc.

Puerile Science
- Research into ill-conceived issues or methods in selection which also lacks sufficient methodological rigor
- Naïve theoretical formulations (e.g., "quick and easy" tests of personality) and unprofessional research designs and/or reporting
- Examples -- unsound "validations" of "alternative" selection methods (e.g., graphology)

Pedantic Science
- Research which is fastidious in its design and analytical sophistication but which fails to address a topic of current import in selection practice
- Pedantic, overly reductionist studies into an outmoded or obscure issue
- Extension--replication studies into an unjustifiably long-running issue which add little or nothing new to knowledge
- Examples -- further replication-extension studies into a long-established finding, studies affirming the criterion-related validity of an outmoded selection technique

1	2
4	3

"Cut-point" indicators

a. The study adds to knowledge with practical implications
b. The study is grounded upon current issues in HRM/selection practice
c. "So what" questions are adequately addressed.
d. Selection practitioners find the study valuable

a. The study adds to knowledge with appropriate design rigor
b. The study is grounded upon relevant theory and past findings.
c. "So what next" (for research) questions are adequately addressed.
d. Selection researchers find the study valuable

FIGURE 1.1 Types of research in selection psychology
Source: Based upon Anderson, Lievens, van Dam, & Ryan (2003), developed and extended from Anderson, Herriot, & Hodgkinson (2001).

perhaps the greatest current pitfall to science-based practice in selection psychology. For organizational practices in employee selection to be based upon such unreliable findings calls into question the veracity of our claim to be a science-based professional practice. Readers of this chapter will no doubt have encountered such beliefs among personnel and line managers, often where there is an unquestioning faith in a particularly dubious method lacking proper validation via methodologically robust applied studies. Critically, therefore, it beholds our field to demand robust research to lie behind each and every applied practice in employee selection and assessment, and for faddish techniques and bandwagon methods to be subjected to scrutiny even where these methods promise lucrative consultancy fees or selection product income.

In quadrant 2, which is comprised of studies high in both methodological rigor and practical relevance, we term the type of research generated as *Pragmatic Science*. In selection situations, as in others in IWO psychology more generally, this quadrant of research should dominate our field and should form the basis of professional practice wherever practicable. Several notable examples of such Pragmatic Science are cited in Figure 1.1, but are merely illustrative of a considerably more extensive scientific basis for the field. For instance, as will be discussed later in this chapter, considerable research now supports the widespread use of tests of cognitive ability and general mental ability (GMA) in selection and this evidence generalizes across job families, organizations, and even countries (Schmidt & Hunter, 1998; Salgado & Anderson, 2002, 2003). This is just one example of where selection psychology can justifiably claim the status of a science-based practice; others will be alluded to later in this chapter. The central point of import here is that only this quadrant of the four presented in our model truly serves the long-term interests of selection psychology, for its science, its practice, and for interdependencies between the two. Following logically from this, selection psychologists need to question (a) how can we maximize this quadrant in terms of the proportion of research efforts and outputs it occupies, (b) how can we optimize links and relations between research and practice to ensure that key findings are translated into organizational practices, and (c) simultaneously, how can we ensure feedback from practice to research to ensure that researchers are pursuing themes of enquiry that are relevant, topical, and priority concerns of clients and organizations internationally?

Where methodological rigor is high but practical relevance is low, what we term *Pedantic Science* is likely to emerge. In this case studies have been robustly based upon a fastidious design, or have been analyzed with considerable attention to detail, but unfortunately fail to address an issue of topical concern for organizational selection practices. Examples of this type of research include long-running themes of enquiry into research questions of marginal or peripheral import to present-day organizational practices or where replication–extension studies fail to add anything new to well-established findings in employee selection. In effect, this is the "safe heaven" quadrant wherein a minority of researchers may be continuing to pursue pet themes of personal interest using conventional methodologies in a scientific sub-field which has become outmoded by organizational change (see also Herriot & Anderson, 1997). Here, again, the review process is critical in screening out such Pedantic Science and this is especially the case given the opportunity costs to selection psychology of such ivory tower research continuing unabated and in spite of changes in the environment of applied selection practices and emergent trends in employee selection. Over some period of time the dangers of too great a proportion of Pedantic Science

being undertaken become only too clear: science loses its relevance to professional practice and may take on a self-serving and highly dysfunctional character in increasing isolation from demands for practical relevance and as a founding basis for organizational selection practices.

Finally, quadrant 4 represents the worst-case situation of research of dubious practical relevance being undertaken using unacceptable methods and designs. This we term *Puerile Science*. Clearly, we should seek to minimize the scale and even the existence of this quadrant of research as it is of no value either scientifically or practically. Examples of this type of research do, unfortunately, exist; for instance, where basically unsound psychological assessment techniques have been "validated" through unsound methods and study designs (an invalid examination of the criterion-related validity of graphology, for example).

In our original formulation of this model, as in Figure 1.1, we present the four quadrants as being equal in size and scale. In actual fact Pragmatic Science has dominated the field of selection and assessment, perhaps with less beneficial incidents of Popularist and Pedantic Science being less evident but nonetheless present and exerting some impact upon our field. We (Anderson et al., 2001) argued on the grounds of several sources of evidence, however, that the latter two forms of science had been increasing because of dysfunctional reward pressures on researchers and practitioners in selection psychology. Such trends are certainly to be guarded against, since the former (Popularist Science) is likely to result in untheorized, unvalidated practices while the latter (Pedantic Science), as argued earlier, is liable to result in the isolation and discrediting of research from selection and assessment practices in organizations.

Science–Practice Relations: A New Area for Research in IWO Psychology

At the start of this chapter I stated that one of its aims is to establish science–practice relations as a topic for research in selection in its own right. As the situation too often stands internationally at present researchers continue to research what can best be called *"content domains"* in selection (e.g., criterion-related validity, adverse impact, applicant reactions, and so forth), whereas on the other side of the fence, practitioners continue to practice with commercially popular approaches, methods, and decision-making tools. That the findings from our research efforts may fail to influence practice should be a major concern to researchers (Dunnette, 1990; Rynes et al., 2002; Viswesvaran et al., 2001). We should perhaps approach this topic area as a challenge for micro-level organization development research – a new *"process domain"* for research studies. In other words, that the findings from studies in selection can form the basis of intervention programs in organizations, that the clients for such interventions are HRM specialists and selection practitioners in organizations who may well not be qualified psychologists, and where the overall aim of such research is to establish the efficacy of such interventions. As I argue subsequently in this chapter, a strictly scientific, rational-economic logic may not be the best approach to getting our findings translated into common practice by HR specialists. Before any of this can occur, however, researchers in selection themselves need to be persuaded that this topic area warrants being treated seriously as a scientific enterprise on its own merits. What arguments are there for this to happen?

First, applied psychologists involved in selection research would surely acknowledge that the psychological aspects of practitioners accepting, being influenced by, and acting upon our findings are relevant questions for research. Rather than the seminal scientist-practitioner model advocated so eloquently by Sackett (1994), this can perhaps be termed the "scientist-scientist model" whereby reflexivity is encouraged amongst researchers over the impact of their scientific outputs. Second, the psychological and economic costs of researchers not opening this up as a new area for research are simply too high (Rynes, Bartunek, & Daft, 2001; Rynes et al., 2002). The current situation internationally is at best that there is an imperfect synergy between research and practice in employee selection. Research fails to influence practice, practice fails to influence research, and so the cycle of isolationism is perpetuated. The crucial question is "why does robust research fail to influence practice (and, of course, vice versa)?" Of any area in selection psychology it could be argued that this is where our knowledge is most fragmented and incomplete – we know plenty about major *content domains*, far less about the *process domain* of why this knowledge fails to influence practice in organizations on occasions. True, it is not the responsibility of researchers to implement these findings in organizations on a day-to-day basis, but my argument here is that it is the responsibility of researchers to verify the impact of research findings. After all, should this not be the life-blood of organizational psychology research? Third, put bluntly, such relations are interesting in their own right. Why do organizations satisfice by not adopting the most valid and reliable predictor methods? Why are HR practitioners apparently unmoved by demonstrably huge utility gains from improving the criterion-related validity of their selection procedures? How can psychometric test development for employee selection be better grounded upon psychological theory and empirical research? Why do researchers dismiss trends in selection practices in organizations as being unscientific and therefore falling outside of the bounds for reputable enquiry? All are important questions and should stimulate further applied research and will help to build science–practice bridges by better understanding the reasons for the imperfect transfer of knowledge in our discipline. In opening up this so-called *process domain* for future research we can profitably begin by examining the lessons from the past. In the following section, I do precisely this by debating four historic scenarios of research–practice relations in selection psychology.

RESEARCH–PRACTICE SYNERGY: FOUR HISTORIC SCENARIOS IN SELECTION PSYCHOLOGY

As the title of the present chapter suggests, relations between science and practice in selection psychology are at their worst where each wing of the discipline is unaware of, or is purposely ignoring, what the other is presently working on (literally, "the left hand not knowing what the right is doing" as the popular saying goes). Note, I distinguish here between being unaware of current developments and consciously ignoring them or choosing not to take them into account, although it is difficult to attribute these two different scenarios precisely post hoc. The more problematic of the two is where either wing of our discipline is simply unaware of developments in the other. This is because such a lack of knowledge would indicate structural deficiencies in information exchange and transmis-

sion between researchers and practitioners, or vice versa, such that the one "hand" is liter-
ally unaware of what the other is doing (Dunnette, 1990; Hyatt et al., 1997). While
information transmission between the two wings of selection psychology is some way from
being perfect, it is reasonable to argue that owing to the extensive range of journals and
newsletters now in existence, ongoing contacts between practitioners and scientists, regular
conferences, and professional meetings, selection psychology benefits from well-estab-
lished contacts and channels for information exchange generally. Additionally, research
funding bodies in several countries (the USA, the UK, The Netherlands, Australia) have
increasingly emphasized practical relevance as a criterion for research program grants,
and so across selection psychology internationally the funding pressures have been toward
applied relevance over more recent years. These mechanisms and funding pressures are
likely, if anything, to have improved researcher–practitioner links at least in terms of the
transfer of knowledge and the stakeholder pressures placed upon researchers to undertake
practically relevant, applied research (i.e., Pragmatic Science).

   Overviewing the history of selection psychology, it is possible to identify four main sce-
narios of relations between science and practice, the first three being highly functional
and beneficial, the final one being counterproductive to the health of the profession.
These are:

1.   robust research informing professional practice;
2.   unreliable research failing to influence professional practice;
3.   trends in practice influencing empirical research efforts;
4.   robust research failing to influence professional practice.

This section of this chapter considers each scenario in turn and gives key examples of how
research appears historically to have influenced, or not, professional practice and vice versa
across different countries internationally (see also Highhouse, 2002 for such an historical
perspective). Unavoidably, these examples and the interpretation of whether research has
appropriately influenced practice have relied upon judgments by the present author, but
this overview is nevertheless useful and valid in illustrating the different scenarios identified
above.

Scenario 1: Robust research informing professional practice

The default position in terms of research–practice relations that all IWO psychologists
generally, and selection psychologists in particular, would like to think exists is that robust
research appropriately informs professional practice. In this scenario Pragmatic Science
routinely forms the basis for consultancy interventions, we all adhere to the strictures of
being scientist-practitioners (Sackett, 1994), and all research findings are automatically
translated into professional practice with little delay or tension between scientific and
real-world demands. Needless to say, this is an idealized scenario. More realistically, selec-
tion practices will be influenced by some of the main research findings, there will be delays
between scientific publication and their translation into professional practice, and there
will be necessary compromises between the findings from pure and applied science in our

area and the day-to-day demands of selection practitioners actually running employee selection procedures (Rynes et al., 2001, 2002).

One example of this scenario historically in selection psychology is the use of tests of cognitive ability, or general mental ability (GMA), across different countries. This example also nicely illustrates apparent tensions between scientific research methods, in this case the increasing use of meta-analysis techniques to establish generalized criterion-related validity, and the day-to-day demands of personnel practitioners to show validity in the specific situation of their particular organization and selection context. There is now an overwhelming body of evidence that tests of GMA represent the best "stand alone" predictors of job performance and training success in both the USA (e.g., Hunter & Hunter, 1984; Schmitt, Gooding, Noe, & Kirsh, 1984; Schmidt & Hunter, 1998) and Europe (e.g., Salgado & Anderson, 2002, 2003; Salgado, Anderson, Moscoso, Bertua, & De Fruyt, 2003a; Salgado et al., 2003b). Schmidt and Hunter, in their review of predictive validity studies into selection methods spanning 85 years in the USA, report average operational validities for GMA measures of .44 for predicting job performance and .58 for predicting training success. Salgado and Anderson (2002, 2003) found that in the context of European selection the magnitude of operational validities for GMA tests is somewhat higher than in the USA. Their meta-analysis across eleven countries in the European Union resulted in corrected observed validity coefficients (Rho's) of .62 for predicting job performance and .54 for predicting training success. Operational validity was not moderated by country culture, but as in the earlier US meta-analyses, was substantially moderated by job complexity, with operational validity being notably greater for high-complexity jobs. The authors also present summary evidence for the popularity of GMA tests across different countries in Europe. They report that tests of GMA are more widely used in European countries than in the USA, suggesting that this popularity in Europe had not been restricted by organizational concerns over possible employment discrimination cases being brought by applicants owing to the less stringent anti-discrimination legislation in most European countries compared with the USA. Rynes et al. (2002). in their survey of HR practitioners in the USA. found that a commonly held belief was that GMA tests were less valid predictors than the research findings indicate, leading perhaps to their lower popularity in America than in Europe. Interestingly, there is also evidence across the multiple surveys into GMA test use by European organizations that they have become more popular over time (see also Robertson & Smith, 2001). It is reasonable to argue that this increased popularity has been fundamentally influenced by the publication of such supportive and robust research findings. Of course, other factors influence the decisions of organizational recruiters over which methods to use, including cost-effectiveness, training requirements, commercial availability, adverse impact, and so forth, but it would be churlish to argue that such compelling evidence for criterion-related validity has not influenced the increasing use of cognitive ability measures in professional practice. Indeed, this example can be held up as an archetypal illustration of the benefits of pragmatic research influencing professional practice in our field internationally. There have been tensions, however, between the move by researchers toward using meta-analytical techniques to summarize criterion-related validity coefficients across multiple organizations and job types and the preferences of selection practitioners for direct, situational-specific evidence that GMA measures display validity and reliability for their particular situation (Chan, 1998; Goldstein, Zedeck, & Goldstein, 2002; Murphy, 1996). Regardless of these tensions, the increasing use of

measures of GMA by organizations internationally can be attributed at least in part to the reassurance provided by this now voluminous body of evidence supporting their use in applied selection contexts. As Salgado and Anderson (2003) conclude: "The magnitude of the operational validities found suggests that GMA measures may be the best single predictor for personnel selection for all occupations" (p. 16).

Other less positive research findings have also influenced professional practice but in this case toward the non-use of certain so-called "alternative" methods of selection. Evidence failing to support the criterion-related validity of such methods as graphology and to some extent references or testimonials has limited their use or at least the reliance placed upon these methods by practitioners (Robertson & Smith, 2001). Another example of robust research informing professional practice can be drawn from research into gender and race sub-group differences and the potential adverse impact of selection methods (Arvey, 1979a; Borman, Hansen, & Hedge, 1997). Particularly in the USA, organizations have made extensive efforts as a result of the research findings in this area to ensure non-discriminatory practices in selection, and there is evidence that organizations in Britain are following suit to be able to demonstrate the lack of adverse impact of their selection procedures (e.g., Ones & Anderson, 2002; Robertson & Smith, op. cit.). One other area that has received very recent research attention is that of applicant reactions and decision making (Ryan & Ployhart, 2000). Although it is early days, it is likely that this too will become an example of pragmatic research efforts influencing organizational practices. In summary, it can be argued across several areas of research in personnel selection that organizational practices have been fundamentally influenced by the key findings concerned. While there may be delays between publication of research findings and their translation into organizational practice, along with an imperfect alignment of the interests of scientific researchers and personnel practitioners, there is an overwhelming case that professional selection represents some of the best elements of research-based practice (Dunnette, 1990; Tenopyr, 2002).

To summarize, this first scenario represents in many ways the ideal of research–practice relations. Fortunately the field of selection psychology is replete with examples over its history of this scenario actually being the case; here I have chosen to illustrate the point with just some of the possible examples of stringent research having impacted beneficially upon professional practices in employee selection. This stated, there has been a rather naïve and unquestioned set of presumptions in our field that this scenario will automatically be the default of science–practice relations (see also Chapter 6 of Dipboye and Chapter 11 of Lievens & Thornton, this volume). However, there is no guarantee that this scenario will be the default option; quite the opposite. It behooves selection psychologists in both the research and practice wings of our discipline to ensure that a bi-directional and symbiotic relation exists between science and practice; we cannot merely take this for granted. To further highlight this point, the three other scenarios identified might just as likely typify science–practice relations in our field, as several examples throughout the history of selection psychology vividly illustrate.

Scenario 2: Unreliable research failing to influence professional practice

The second research–practice scenario identifiable is where less than reliable research findings have fortuitously failed to influence professional selection practices in organizations.

In this case, adopting an historical perspective is particularly informative (e.g., Salgado, 2001). By its very nature science develops over time, with the findings from earlier studies based upon less stringent research and analytical techniques being questioned and falsified by more recent research (Kuhn, 1970; Pfeffer, 1993). In choosing the term "unreliable" research, I should clarify that this may only turn out to be so with the benefit of hindsight and subsequent advances in research methods and analytical techniques (Ryan, personal correspondence). At the time the original studies were conducted researchers may well have used the most sophisticated approaches available to them; this is an inherent quality of advances in research across all fields (Kuhn, 1970). Indeed, falsification of prior empirical findings and proposed theoretical models is held to be a central tenet of scientific enquiry, with "normal" science in IWO psychology making progress precisely by this route (Anderson, 1998). We should not therefore be surprised if earlier findings in our field are questioned or even modified by subsequent research using more robust designs and analytical procedures. The interesting corollary to this point is that selection practices have not always historically followed research findings in our field. Of several examples perhaps the most immediately striking is the continued use of all types of interviews, even completely unstructured interviews, despite earlier narrative reviews which cast doubt incorrectly upon their likely value (chronologically: Wagner, 1949; Mayfield, 1964; Ulrich & Trumbo, 1965; Wright, 1969; Arvey, 1979b; Arvey & Campion, 1982). It was not until the mid-1980s onward that a series of published meta-analyses into interview predictive validity began to change this perception of its apparent inherent unreliability and invalidity (e.g., McDaniel, Whetzel, Schmidt, & Maurer, 1994; Huffcutt, Roth, & McDaniel, 1996; Salgado & Moscoso, 2002). In the Salgado and Moscoso (2002) meta-analysis, for instance, unstructured interviews were found to have a mean corrected operational validity of .20 whereas for highly structured interviews Rho was reported at a substantial .56 (i.e., close to the levels of operational validity reported for tests of GMA internationally, as above). McDaniel et al. (1994) reported average operational validities of .37 for all types of interviews, .44 for structured interviews, and .33 for unstructured interviews. This series of meta-analytic findings have essentially rehabilitated the credibility of interviews as an assessment device, and even in the case of unstructured interviews, some value is likely to be added to organizational selection procedures from their inclusion (e.g., Herriot, 1989). In terms of the scenario of relations between research and practice, it is abundantly clear that the popularity of interviews did not drop over the earlier period before these meta-analytic findings were known. If anything, the interview remained almost universally popular for all types of jobs regardless of the folklore knowledge among personnel practitioners that it may lack validity or reliability (Dipboye, 1997). This was a clear example of the research not influencing practice despite recruiters apparently knowing about these untowardly critical research findings. Yet, these beliefs among HR practitioners appear to persist over more recent years regardless of the publication of these important meta-analytical findings (Eder & Harris, 1999). That is, a proportion of practitioners seemingly still believe that all types of interview are inherently flawed. Here, it is likely that the more recent research findings have not been disseminated among HR practitioners perhaps as widely as they should have been in order to influence professional practice positively.

A further example of unreliable research failing to influence practice occurs in the area of personality testing, but with the twist that this research was hugely impactful in the USA

but not so in Europe. In their retrospective review of personality research and personality testing for selection over the last millennium, Barrick, Mount, and Judge (2001) hint at this differential impact. Citing Guion and Gottier's (1965) influential review, these authors concluded: "There is no generalizable evidence that personality measures can be recommended as good or practical tools for employee selection" (p. 159). This damning conclusion had a huge impact on the popularity of personality testing in the USA, and as Barrick et al. suggest, this conclusion, which has subsequently been proven to be erroneous, largely went unchallenged for a period of around 25 years. Yet, in the European Union, Guion and Gottier's dismissal failed to have anywhere near the same level of impact on the professional practice of use of personality inventories for selection purposes (Salgado & de Fruyt, Chapter 8, this volume). In point of fact, the opposite can be argued to have occurred (see also, Herriot & Anderson, 1997). In successive surveys of the popularity of personality tests over this 25-year period, there is actually evidence of a considerable *growth* in organizational use of such measures for employee selection (e.g., Bartram, Lindley, Marshall, & Foster, 1995; Hodgkinson & Payne, 1998; Robertson & Makin, 1986; Shackleton & Newell, 1994). Why this striking difference between the USA and Europe? One explanation is that the Guion and Gottier (1965) review and its conclusion simply did not reach European researchers at that time. Some 40 years ago, journals were consulted far less internationally than they are today with the advent of electronic access and much stronger links between researchers in different countries. So, one plausible explanation is quite simply that European researchers were not so aware of Guion and Gottier's conclusion, or at least not so influenced by it. Certainly academic reviews of the value of personality testing in Europe at that time were far less accepting of this apocalyptic conclusion, and their message of effectively a moratorium on the use of personality inventories for personnel selection was not even quoted in key HR texts of that period in the UK (e.g., Barber, 1973; Torrington & Chapman, 1979). Another plausible explanation is that this review slightly predated a period of considerable growth in selection consultancy firms in Europe, especially throughout the boom years of the 1980s, where many of these consultancies included personality testing as part of their product-mix and advisory services. Whatever is the explanation, the continued growth in the popularity of personality testing for employment across Europe over this period stands in stark contrast to the American experience (Barrick et al., 2001). Ironically, this might be described as a clear case of the right hand [in Europe] not knowing what the left hand [in the USA] was doing, yet this ignorance brought unforeseen benefits with hindsight. Since then, of course, the series of meta-analyses into personality inventory predictive validity have proven beyond any reasonable doubt that well-developed personality tests are robust predictors in the cultural context of both the USA (e.g., Barrick & Mount, 1991) and Europe (Salgado, 1997).

What can be concluded from these examples of this second scenario whereby unreliable research fortuitously fails to influence professional practice? The first point must surely be to acknowledge that research is not an infallible panacea upon which to base every aspect of employee selection practice in organizations. Science continues to develop, its earlier findings and conclusions sometimes (although more rarely than might be expected) overturned, and its methods and analytical conventions continue to advance. We should not forget that personnel psychology is a relatively young science, and it has been the advent and popularization of meta-analysis techniques in particular that have advanced our

understanding over the past two decades (Schmidt & Hunter, 1998). Second, we as researchers should be thankful that selection practitioners often receive our findings with a healthy degree of skepticism! By its very nature, again, research is specialized into one aspect of the wider picture, whereas the popularity of selection methods is determined by HR practitioners with imperfect knowledge of the scientific evidence and facing a plethora of different demand characteristics (Latham & Whyte, 1994). Third, this scenario provides support for my earlier assertion that there should be a natural distance between research and practice in selection psychology. It would be notably dysfunctional for every organization's selection procedure to slavishly follow the findings of every published study, or even to attempt to keep pace immediately with the sheer range of published findings in real time. Rather, a period for reflection, critical examination of the key findings, and a translation of the balance of scientific opinion into professional practice carries a much more intuitive and specious appeal. Finally, this second scenario highlights the importance of research being open to, and influenced by, developments in professional practice internationally. The question of why these findings failed to change practice is an intriguing one, and, it can be argued, one that has received too little attention by researchers in the past.

Scenario 3: Trends in practice influencing empirical research efforts

It would of course be wrong to suggest that there exists only a one-way relation between research and practice in selection. Several examples can be cited where recent trends in employee selection practices in organizations have stimulated new directions for applied research, meta-analyses, and theory building (e.g., Lievens, van Dam, & Anderson, 2002). Research into competency frameworks, multi-rater performance appraisal, emotional intelligence, computer-based testing, honesty and integrity testing, drug and alcohol testing, Internet-based recruitment, telephone-based interviews, and computer-adaptive testing are all examples of this happening. Of all of these areas, most attention recently has been directed at Internet-based recruitment and assessment. This flurry of research activity has been as a direct result of organizations moving with striking haste into web-based recruitment and selection procedures (see, for instance, Anderson, 2003, for a recent review). Especially in the USA, many large organizations have begun to rely on the Internet for both recruitment and screening purposes. Lievens and Harris (2003) quote the figure of 88 percent of Global 500 companies in the USA now using web-based recruitment procedures. This growth has been the principal factor in stimulating recent research in this area; a clear example of developments in practice influencing research efforts, it can be argued.

Unavoidably there may be a delay between changes in selection practices in organizations occurring and subsequent research being undertaken (Lievens, personal communication). Certainly in the case of Internet-based procedures this was due to the sheer speed with which organizations adopted the new technology. Should research be in such a reactive position or should we facilitate more speculative theory-building efforts and empirical studies in advance of such developments? I would argue for the latter. Especially in the case of this example it was foreseeable that some organizations would adopt web-based

solutions given their inherent advantages of cost-effectiveness, immediacy of response, convenience to applicants, and so forth. Perhaps what was not foreseeable was the scale of the adoption of this new technology. This has resulted in there being a dearth of research into the effects of web-based recruitment and assessment contrasted with huge growth in their use by practitioners in organizations (Lievens & Harris, 2003). More research is now appearing, but the wider question remains as to how we can foster more speculative studies ahead of such developments in practice in future. Perhaps selection researchers had indeed become rather too conservative in their approach and we needed more visionary thinking some years previously for the knowledge base to stay ahead of these changes in practice.

For the future it would obviously be advantageous for selection research to lead such developments in practice, that is, to take a far more proactive stance than its traditionally reactive one. A prerequisite for this to happen, once again, is for there to be sufficient feedback contacts and information channels from practice back to research such that selection scholars are continually challenged by pragmatism. Perhaps it is now timely for a top-tier journal in personnel selection to commission a special issue on the science–practice *and* practice–science interface similar to AMJ's special research forum edited by Rynes, Bartunek, and Daft in 2001? We might also facilitate a few purely speculative workshops involving both practitioners and researchers to generate likely future scenarios and trends, and to stimulate a far more visionary stance and cutting-edge thinking (and if such a suggestion sounds ludicrous, surely this is in itself indicative of our field lacking vision!). To conclude, while there are examples of trends in practice influencing research, the field could benefit substantially from better links for practice-driven research.

Scenario 4: Robust research failing to influence professional practice

The fourth and final scenario, where robust research fails for whatever reasons to influence selection practice, is potentially the most troublesome for selection psychology as a profession. In this scenario we have borne all of the costs of producing Pragmatic Science and yet have not reaped any of the benefits from its practical application. As in the case of the second scenario outlined above, this fourth scenario is less common than where robust research has influenced professional practice (Scenario 1). However, there are clearly examples of where research has apparently exerted too little impact upon selection practices in organizations (see, for instance, Rynes et al., 2001, 2002); here I focus on two in particular – the Five Factor Model (FFM) of personality and occupational personality test development, and the impact of utility analysis on selection method choice by HR practitioners.

Over the past two decades a considerable body of evidence has built up supporting the taxonomic structure of the FFM or "Big Five" as a latent model of personality and individual differences (Digman, 1990). Most impressively, this body of research indicates the applicability of the FFM across many different countries (McCrae & Costa, 1997), for a substantial range of original personality measures (Costa & McCrae, 1988; Ferguson, Payne, & Anderson, 1994), and across different languages and cultures (Yang & Bond, 1990). In relation to selection, however, the most important finding across recent research studies has been that FFM-based personality inventories display greater criterion-related

validity than personality measures based upon alternative models of personality (Salgado, 1997). This meta-analytical evidence, drawn from studies in Europe, not the USA, would lead one to expect that there should have been a quantum shift toward FFM-based personality inventories by commercial test publishers across Europe. Not only has this not been the case, but as Hough (2001) pithily observes: "I/O psychologists have been lax in attending to the taxonomic structure of their variables, perhaps due partly to excessive empiricism, and perhaps partly the result of pragmatic attention to an immediate, applied goal" (p. 21).

While this evidence for the superiority of FFM-based measures has been published relatively recently in Europe, seminal findings in the USA have been around for some years now (e.g., Barrick & Mount, 1991; Costa & McCrae, 1992). Given the now substantial body of evidence one might have expected commercially published measures of personality for occupational selection to have universally incorporated these findings into their design and underlying construct model. Yet, this has not been the case. Some proprietary tests do profess links to the FFM but relatively few measures have been developed psychometrically from their inception to be based upon this taxonomic structure (one exception is of course Costa & McCrae's, 1992, Revised NEO Personality Inventory; see also Anderson & Ones, 2003). Debate is ongoing as to whether the FFM represents the most comprehensive and parsimonious typology of normal adult personality, perhaps suggesting that commercial test publishers have merely exercised caution "while the jury is out" before becoming converts to this approach (Hough, 2001; Schmitt, personal correspondence). There is also an inevitable time-lag before research findings are disseminated into commercial practice, leaving open the possibility that commercially published personality inventories may move toward the FFM as a typological framework in years to come. For whatever reasons, the FFM, despite a considerable volume of evidence internationally having built up for its construct and criterion-related validity, does not appear to have influenced commercial test publishers as much as perhaps it should have done (Hogan & Roberts, 2001). It is true that for some proprietary personality tests second-order factors can be computed from summing raw scores on primary dimensions and that these second-order factors resemble the FFM (e.g., Hogan Personality Indicator, 16PF5, some versions of the OPQ family of measures), but for many other tests relations with the FFM are not described. This has led for calls for test publishers to fully document their model of personality underlying commercially published measures and to describe links to the FFM (Anderson & Ones, 2003). Further, HR practitioners appear to be attracted to personality measures that offer a more fine-grained level of analysis, that is, considerably more dimensions than just the five superordinate dimensions of the FFM (De Fruyt & Salgado, 2003). Regardless of these possible tensions between research and practice, or possibly even precisely because of them, it is apparent that the FFM has yet to have the impact on construct models of personality upon which proprietary tests of personality for selection are being currently based.

The second example of this final scenario, where again robust research findings do not appear to have exerted sufficient influence on professional practice, concerns the topic of utility analysis in personnel selection. In fact, this example is particularly apt as utility analysis models are founded upon the notion that HR practitioners choose between different selection methods largely on rational-economic criteria (Boudreau, Sturman, & Judge,

1997). These assumptions are a microcosm of assumptions that research generally should influence practitioner actions on the basis of objective and rational evidence in support of the added value of such interventions (Anderson et al., in press; Latham & Whyte, 1994). In the Latham and Whyte (1994) study, 143 experienced HR and line managers evaluated the persuasiveness of utility analysis outputs (dollar savings) compared with more general, narrative information on the benefits of different selection methods. Counter to some of the core assumptions of utility analysis, financially based cost-saving information influenced managers in a *negative* direction, leading the authors to conclude: "Those who rely on utility analysis and are successful in getting their recommendations accepted may be successful in spite of, rather than because of, reliance on this technique" (p. 43). Utility analysis appears to have been stultified in its usage to persuade selection practitioners as to the financial benefits of more valid predictors because of its apparent computation of huge financial savings which lack credibility among practitioners (Cascio, 1993). More problematic regarding the present chapter is whether the assumption that providing scientific evidence to practitioners is the most persuasive language in which to communicate realized benefits. Conversely, practitioners might be more swayed by the power-relations and politics of their current organization, often with HR practitioners not occupying particularly lofty positions in the organizational hierarchy. Certainly, utility analysis, as presently configured, does not compare like-for-like interventions at an organizational level of analysis, thereby perhaps leaving practitioners somewhat incredulous over the huge financial paybacks claimed. So, have our assumptions of persuasion by rational-economic evidence in selection been misplaced? And if so, could we ever expect that scientific evidence will impact upon practice as much as it should (on the grounds of rational-economic criteria) do? These are key questions and challenges that strike at the heart of research–practice relations in selection psychology; as Anderson et al. (in press) conclude:

> we have typically placed too much emphasis on selection practices as rational technical interventions and therefore often fail to have an impact in organizations . . . [rather] selection researchers should consider their interventions as organizational interventions that are subject to the same pressures as other organizational innovations. (p. 11; see also, Johns, 1993)

In conclusion, this fourth scenario presents the most troublesome and challenging scenario for selection psychologists. It undermines our self-identity as scientist-practitioners (Sackett, 1994) certainly, but, more tellingly, where substantial bodies of evidence fail to influence practice this is unambiguous evidence for a practitioner–researcher divide in our field. While many would like to think idealistically that Scenario 1 always typifies research–practice relations in IWO psychology, the examples cited above cannot be swept away in a self-delusory attempt to persuade ourselves that all is always rosy in terms of these relations. Most troublesome of all are suggestions that rational-scientific logic *itself* fails to engage practitioner interests in research findings. Rather, we may need to explore ways of communicating the key findings of selection research that take account of political realities and practitioner mindsets in organizations; for too long perhaps a minority of researchers have clung onto the mindset that practitioners should be listening to these messages regardless of their medium (Rynes et al., 2001).

Directions for Future Research in Research–Practice Relations

Having identified and described these four scenarios for research–practice relations in personnel selection internationally, the final section of this chapter goes on to consider several promising directions for future research into questions surrounding research–practice relations in IWO psychology. Of the four scenarios, the first three are clearly positive in terms of their effects and outcomes whereas the final one is negative, and potentially highly negative in the case where a substantial body of research is failing to influence selection practices in organizations. A series of important questions can be posed following from these scenarios:

1. How can we as a profession ensure that Scenarios 1 and 2 are predominant in science–practice relations, and simultaneously, that Scenario 3 is minimized or even eradicated?
2. Why has robust research failed to influence employee selection practices in some cases, whereas in others less reliable scientific evidence has been rightly overlooked by selection practitioners?
3. How can we engineer a situation where a "natural distance" exists between research and practice but also robust research findings still influence practitioner actions (i.e., Scenario 1) after being "translated" into pragmatic terminology?
4. How can we ensure the ongoing existence of strong researcher–practitioner links through structural features and channels of communication (journals, newsletters, conferences, interest groups, etc.)?
5. At an international level, how can we best ensure sharing of information and best practice experiences across national borders and cultures in personnel selection?

All five questions are central to advancements in *process domain* research into science–practice relations in selection. In the final section of this chapter I suggest two main directions for future research. Rather paradoxically, however, even a cursory review of the contributions to this important topic area in the literature reveals that research into research–practice relations is very sparse. With a few notable exceptions (e.g., Anderson et al., 2001; Dunnette, 1990; Rynes et al., 2001, 2002; Sackett, 1994), researchers have in general neglected these critically important issues, perhaps preferring instead to continue forth on their own research agendas rather than stopping to reflect upon whether their findings might be having as much impact upon professional practice as they might. This has not been a healthy situation for selection psychology specifically, or indeed for IWO psychology more widely (Viswesvaran et al., 2001). The impact of our research findings on practice is every bit as important as the contents of the findings per se, yet selection psychology has curiously and dysfunctionally neglected these issues of transfer over more recent years. Researchers may believe that their results are falling upon deaf ears, yet on the other hand, one hears embittered complaints from practitioners at conferences internationally that studies are written up in a style that engenders reactions of either non-comprehension, utter boredom, or derision of intricately designed studies that empirically confirm the blindingly obvious (i.e., Pedantic Science, see Figure 1.1). It is therefore timely to call for

greater attention to be given to research–practice relations in selection psychology, the present chapter being one attempt to highlight the importance of these issues. Two directions for future research and major lines of enquiry seem particularly valuable at the present juncture – research into practitioner beliefs and strategies of persuasion, and validation research into the effectiveness of Continuing Professional Development (CPD) training interventions in IWO psychology internationally.

In comparison with the concentration of efforts by researchers into *content domain* issues of selection (i.e., predictive validity, construct validity, reliability, adverse impact, etc.), there has been precious little research into, firstly, practitioner beliefs over key research findings, and secondly, tactics through which to change practitioner beliefs and day-to-day practices. This is a severe shortcoming in our understanding of how science might and does influence practice. Standing in contrast to the now large number of surveys into selection method use in organizations (e.g., Bartram et al., 1995; Robertson & Makin, 1986; Shackleton & Newell, 1994), for instance, our knowledge of practitioner *beliefs* about method validity, reliability, and adverse impact is at best rudimentary (see also, Robertson & Smith, 2001). In fact, the whole area of practitioner beliefs about selection methods and processes is a gargantuan one into which research has made little or no inroads (Johns, 1993; Ryan, personal correspondence). Only in relation to the impact of utility analysis has this area been breached initially, specifically concerning the persuasion of practitioners toward the use of more valid predictor methods (Boudreau et al., 1997; Latham & Whyte, 1994). Fundamental questions remain basically unaddressed, however, including which selection methods HR practitioners believe to be more valid and why, how practitioners choose between different predictors and sources of evidence on applicants in real-life selection situations, and how best we can "frame" the key research findings when presenting them to practitioners in order to persuade them toward adopting more valid, reliable, and fair predictor techniques. This paucity of research interest is also at odds with the concentration of research over several decades upon recruiter decision making *within* selection procedures at each stage in the process. Selector decision making *over* different predictor techniques, in contrast, has received almost no research attention.

Second, future research should be directed at evaluating the efficacy of different methods of practitioner training as part of professional CPD events. In several countries now (the USA, UK, Australia, for instance) compulsory CPD is an integral part of remaining licensed or chartered as laid down by the relevant national professional psychology bodies (APA, BPS, and APS). In addition, in many other countries which do not yet have compulsory CPD training, events are run to update practitioners and to educate further practicing psychologists. Given that one of our areas of specialist expertise is the evaluation of training effectiveness (e.g., Goldstein, 1997), it would be sensible to examine the efficacy and transfer of such CPD events to practice, particularly in the field of selection and assessment. CPD represents a mainstream channel through which practitioner beliefs and actions can be directly influenced and it is therefore of considerable interest to evaluate the effectiveness of alternative training interventions in this regard. Compulsory CPD is a relatively new departure in most countries; surely IWO psychologists could be much more involved in validating these interventions in future? This is especially the case for CPD training events on employee selection practices. Many such events will have a professed pragmatic element to them, usually around the theme of updating practitioners on recent developments in research and how these can be applied in practice. The transfer

of these points into subsequent practice by attending psychologists is one immediately obvious question area: Do CPD events result in genuine changes to practice, or are events merely attended and the information listened to politely by delegates in order to earn CPD credits? Clearly, such events offer a critical case site for the evaluation of research–practice relations, with the subject pool being IWO psychologists themselves. If such events are failing to persuade fellow IWO psychologists to develop and update their professional practices we should hardly be surprised if HR and line managers remain stubbornly unaffected by our collective research efforts and dissemination of key findings.

Concluding Comments

In this chapter I have argued for four main points and issues. First, that there exists a natural distance between the professional spaces occupied by the scientific and practitioner wings of selection psychology. However, as an absolutely essential corollary to this point, networking mechanisms, channels of information flow and communication, and structural means toward bi-directional influence between both wings need to be strong and healthy. Second, that there exist four types of science in selection psychology (Populist, Pragmatic, Pedantic, and Puerile Science), and that in keeping with our original formulation of this four-quadrant model, the future of both wings of our discipline can only feasibly be served by Pragmatic Science (Anderson et al., 2001). Third, that it is possible to identify four types of scenarios over the history of selection psychology of relations between research and practice: Scenario 1 where robust research appropriately informs practice, Scenario 2 where unreliable research fortunately fails to affect practice, Scenario 3 where advances in practice stimulate new directions for research, and Scenario 4 where strong research regrettably fails to change selection practices. Examples to support the existence of these three scenarios were presented, but with the caveat that, on balance, the first scenario is most often represented over time in our field. I further argued that *content domain* research should be supplemented with *process domain* studies designed to shed light upon research–practice and practice–research relations. Fourth and finally, I argue for two key directions for future research into these neglected topics in personnel psychology: practitioner beliefs and tactics to persuade practitioners on the basis of research evidence, and validation research into the current rush toward CPD events internationally.

Note

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