Central to the discussion launched by Latham’s article is the question of the primacy or, one could say, the balance between two aspects of psychology: its contribution to the scientific body of knowledge and its assistance in solving practical problems. Many debates and a long list of publications have been devoted to the relevance of scientific and applied psychology. The editorship of *Applied Psychology* has chosen an interesting variation of this question, namely the relative attractiveness and desirability (for their journal) of more theoretical and academic articles on the one hand, or of more practical papers that have immediate practical implications on the other.

Latham attempts to answer this question by relying primarily on his own research and experience to support his preference, an approach that is both unusual and intriguing. We as commentators were asked to do the same. In the next few pages I will, therefore, draw upon my own work experience for an elucidation of my personal choice in this field of tension between theoretical interest and practical relevance. Since it is the science of behaviour, psychology will always make reference to practical implications. As in everyday practice, the psychologist wants to know why people behave as they do, and which internal or external conditions determine or stimulate this behaviour. The only criteria that distinguish psychology from gossip, journalism, and novelistic writing are rigorous methodological discipline and empirical testing, in addition to, of course, the search for generalisable laws and relationships as opposed to an interest in particular cases or individuals. Consequently, in a world full of erroneous generalisations and simplistic stereotypes, the scientific psychologist plays a critical, negating and often disenchanted role.

I have always welcomed this stringent role of science. Particularly when dealing with people, their destinies, and their major decisions in life (education, work, partners, health), professional psychological support should
be based on empirically based predictions and justified conclusions. Non-scientific casuistry and intuitive evidence are as unacceptable as pseudo-scientific approaches, examples of which abound: astrology, graphology, neuro-linguistic programming, reincarnation, psychokinesis, and many others.

In my work and my research the stimulus to start a project was almost always being struck or confronted with a relevant accident, phenomenon, or problem. But when attempting to solve the problem I was generally not satisfied with a technological answer or with the solution of a single case. I have always tried to look for further-reaching and generalisable solutions, and I realised that this often required a deepening of both the theoretical framework and the analysis. So, although having affinity for applied research (“the origin of this type of research is a question, problem or difficulty in the functioning of individuals in a societal or organizational context” (Drenth, 1998, p. 299)), in my actual scientific work I inclined towards pure research (“this type of research is carried out for the sake of acquiring insights and generating generalizable laws or relationships, not to find out whether these have any practical usefulness or whether they yield applicable results” (Drenth, 1998, p. 297)). Also I have always been convinced that pure research, although not aiming at applicable results, can (and in fact does) have social or organisational relevance. Pure research is often needed to further our insights, to expand the frontiers of our knowledge and to find new technological and instrumental applications. When the European Commission advocates for the necessity of creating new knowledge industries in Europe (EC, 1995), they talk about the need for new knowledge and not merely the application of existing knowledge.

As to the original question posed by the editors of Applied Psychology, my preference is for articles which cover relevant issues and questions, but which analyse these from an empirical-theoretical and generalisable perspective. Such publications may and, as stated above, often do have direct or indirect implications for professional practice. However, it is primarily the opportunity to make a contribution to the scientific knowledge of (relevant) human behaviour which has interested me and has motivated my work. Let me illustrate this with a number of examples from my work experience and my research career.

One of the first major decisions in my life was the choice of an academic discipline. After long periods of incertitude and wavering between medicine and psychology, I chose the latter because of its (supposed) scientific and research orientation. Medicine seemed chiefly to prepare its students for a practical profession. Both perceptions were wrong. Medicine at that time (1952) was already a solid scientific discipline in which research was highly valued and a research career in this field was quite possible. On the other hand, psychology in the Netherlands in the first half of the 1950s still had a

strong affinity to the humanities, and was rooted in philosophical schools of thought, such as existentialism and phenomenology. Its practical starting point was a mixture of psychoanalysis and “verstehende” psychology. It adhered to the paradigm put forward by “Gestalt Psychologie” and personalism, in which much more was expected by the often intuitively working psychologist than the mechanical drawing of conclusions from experimental and objective measurements. Observation, interview, the qualitative analysis of test performance, as well as projective tests and graphology, it was argued, offered much richer insights than did quantitative and experimental methods. Like a salmon swimming upstream, I developed a strong distaste for the popular subjective approach, and a strong affinity for the “statistical” method. I took “useless” courses in mathematics, statistics, biology, and physiology. The writings of Thorndike (1949), Cronbach (1949), Anastasi (1954), Guilford (1954, 1956) and Gulliksen (1950) served to strengthen my resolve. Meehl’s *Clinical versus statistical prediction* (1954) was a revelation. The discoveries were exciting and new statistical methods intriguing. Of course, little computer support was available at the time and I had to carry out my first Wherry Doolittle regression analyses (Garret, 1947, Ch. 14) and factor analyses (Fruchter, 1954, Ch. 5) by hand and table calculator.

My first employer after graduation was the Royal Dutch Navy. I served two years as personnel officer (military duties), and was responsible for the selection and appraisal of navy personnel (both enlisted and professional). Although the “clinical practice” in selection was still common at that time, there were already small pockets of more “positivistic” and quantitatively oriented selection psychologists. The naval selection bureau, led by my colleagues Van der Giessen and de Wolff, was one of these. This work experience further stimulated my interest in researching and critically testing common and generally accepted practices. We discovered that objective tests predicted training achievements better than did the popular observation and situational tests (Van der Giessen, 1957). We found that independent observers in assessment-centre-type situations showed very low inter-rater reliabilities. We ascertained that Rorschach indicators had no relationship with subsequent (disciplinary) behaviour of navy personnel. We learned that officers were not able to distinguish more than two or three factors in their performance appraisals (De Wolff, 1963). We found out that objective tests predicted training achievements better than an overall judgement based on an extensive interview by an experienced psychologist, even if the latter had knowledge of the test scores and their regression weights. In fact the overall evaluation of the psychologist had a lower validity than the combined and weighted test scores. With great interest we took cognizance of the relevant (mostly American) literature. The *Journal of Applied Psychology, Personnel Psychology, Psychological Bulletin* as well as the reports of the Office of Naval Research, and the US Air Force and Army Personnel Offices became compulsory reading.
I used part of my time in the navy to write my PhD thesis. My Masters thesis had focused on the measurement of dissatisfaction of workers at one of the Philips companies, and their motives for leaving the company. In a series of post-exit interviews I was struck with the discrepancies between the expectations of most workers and reality. This had prompted my interest in the question of the quality of available or gathered information in the choice of a job or, more generally, a vocation. The writings of Super (1957) and Cattell (1957) and Atkinson (1958) made it clear that motives, interests, and personality factors could be measured objectively, and that such an investigation into motives for vocational choice could deepen our insights into the important choice processes of individuals. The entry of two groups of some 20 newly enlisted recruits each week offered an excellent opportunity to collect the necessary data for such a study. The study itself (Drenth, 1960) was an attempt to find an answer to the question as to what are the underlying motives on which a choice of a career in the Royal Dutch Navy is based, and as to whether a prediction of subsequent behaviour and achievements can be made on the basis of the structure of motives, expectations, and attitudes at induction. Clearly, the first inducement for the study was a practical question: why do people choose a career in the navy, and can we predict future behaviour from the nature of their motives and the intensity of the motivation? But the aim was to show that objective measures of personality, interests, and motivation could provide a better insight into the determinants of a vocational choice.

At the time, in the Netherlands as in the rest of Europe, no one was yet using computers in psychology for large-scale data analyses. A Fulbright scholarship provided a welcome opportunity to get acquainted with these new developments. This combined study at New York University (Katzell, Weitz, Barret) and work at the Social Science Research Division of Standard Oil Company New Jersey, in New York. I spent most of the 1960–61 period with this research group at SO, chaired by (former NYU professor) Ed Henry and Harry Laurent. I enjoyed the occasional consulting visits of Douglas McGregor, and the T-group training sessions given by Blake and Mouton. I also had my first experience of international contacts and conferences: APA convention, Eastern PA meeting, New York State PA meeting, visits to Stanford University (Bavelas, Harrel), University of Tennesee (Cureton), University of Michigan (Likert, Seashore, Tannenbaum, Katz, Coombs), University of Iowa (Lindquist), New York State University (Hollander), HUMRRO, Washington (Crawford), ONR, Washington (Trumbull, Glenn Bryan, Petrullo) and defence research groups in Pensacola, Lackland, San Diego and Colorado Springs (all by order of the Royal Dutch Navy). In that year I learned the importance of international contacts both for one’s personal growth and in order to avoid an excessively provincial viewpoint in science and research. Since then I have
always valued internationalisation and encouraged this in my staff. Next to reading international journals it is the best way of keeping up to date! This was also one of the major motives (many years later) for launching the bi-annual series of European Congresses of Psychology by organising the first congress in Amsterdam (in 1989). The aim was to give European psychologists a better chance of meeting other scientific and professional psychologists, without having to travel all over the world, as is often required in participating in world congresses (IAAP, IUPsyS). It really pleases me that this initiative has developed into a successful tradition (Amsterdam, 1989, Budapest, 1991, Tampere, 1993, Athens, 1995, Dublin, 1997, Rome, 1999, London, 2001).

At the end of the 1950s and the start of the 1960s the Standard Oil Division itself was involved in a very interesting research project, the Early Identification of Management Potential (EIMP, Laurent, 1962). The study was designed to help answer two operational problems of concern to the organisation: how does one determine success in management, and how does one identify employees who have the potential to be successful in management positions? Large samples and data sets were used and a variety of potential predictors were tried out. With great enthusiasm I joined the group, working on both intelligence and motivational measures. Again, the project was prompted by an urgent practical question. But I was particularly fascinated by two fundamental aspects of this project. One was the novel experience of working with a (at that time) fairly large computer. The other aspect was the wider impact of the more basic issues of the study: the definition and development of criteria, the validation and cross-validation of capacity tests, biodata, and personality measures, the restrictions of concurrent validation studies for longitudinal use, etc.

After my period in the United States I became lecturer at my Alma Mater, the Vrije Universiteit in Amsterdam in 1962 and full professor in 1967. I taught tests and measurements, research methods, cross-cultural psychology, and an array of courses in the field of work and organisational psychology. At the beginning of the 1960s Dutch psychology underwent a rather sudden and significant change from a humanitarian, intuitive psychology to a quantitative, experimental, empirical psychology. A book whose influence cannot be overemphasised in this respect is A.D. de Groot’s Methodology (1961). So I returned to a much more favourable climate (as far as my own scientific orientation was concerned) than when I was a student and just starting my work as a psychologist. Still in the practice of psychology in the Netherlands much of the old tradition prevailed. At the time I made myself rather unpopular within certain circles by criticising (sometimes in the more popular media) pseudo-scientific practices such as graphology, Rorschach interpretation, and the use of outdated tests such as the Pfister test, the Baumtest, the Szonditest, and other relics from...
the psychological museum. I also denounced the more regular projective techniques (Drenth, 1965) and the interview (Drenth, 1988).

I was asked to become the Secretary, and later the Chairman of the rather influential ‘Test Research Committee’ (TRC) of the Dutch Psychological Association (NIP). This committee evaluated and rated the various tests that were available on the Dutch market. Many low ratings were awarded, due to lack of norms, lack of data on reliability and/or validity, or because the test authors simply borrowed American, English, or German data to support their quality claims. The TRC considered translating and adapting the American ‘Technical Recommendations for Psychological Tests and Diagnostic Techniques’ (APA, 1954, 1966) for Dutch use. It was my conviction that the Dutch psychology community and psychology students needed an explicated introduction into the scientific principles of test theory and test interpretation rather than a series of technical recommendations. No comprehensive Dutch textbook of test theory existed at that time. Accordingly, I wrote such an introductory textbook with the title *The Psychological Test* (Drenth, 1966), which soon became required reading at most Dutch universities. It was revised in 1975 and again (with Sijtsma) in 1990. The book is still used by students and professionals today.

Again, the principles prevailed over the techniques, the general over the specific, the theory over the applications. A solid theoretical foundation, laid by careful teaching on the basis of a substantial handbook, and continuously updated by reading articles and listening to scientific presentations is, in my view, the best condition for a professional contribution to society. The professional psychologist, of course, should be trained to make the connection between the generic principles and the more specific applications.

Later, when participating in major international comparative research projects (Industrial Democracy in Europe, Meaning of Working, Decision-making in organisations) I always happened (was forced?) to play such a critical, methodological role. When it came to publishing the results of these studies (IDE, 1982; MOW, 1987; Heller, Drenth, Koopman, & Rus, 1988; IDE, 1993) I was always asked to write the chapter on the methodology of the study or on the methods of comparison.

In the Handbook of Work and Organizational Psychology (published together with my colleagues Thierry, Willems, & de Wolff; English edition, 1984; the second, revised edition with Thierry & de Wolff, English edition, 1998), in my chapter on “research in work and organizational psychology principles and methods” I posed the following question: “Why devote an extensive chapter in a handbook on an applied field such as Work and Organizational Psychology to scientific research and its methods? Has not enough research been done and enough understanding gained to shift the emphasis toward the application of this understanding instead of doing even
more research? And when it comes to tackling problems in practice, isn’t the focus on a certain approach, the instilling of certain attitudes and stimulating problem-solving processes rather than on generating or using scientific knowledge?” My answer to this question was that scientific research will remain essential for the development of future understanding; many questions have not fully been answered (as yet) and many others have not been studied at all. Moreover, new problems which need to be studied arise almost daily in the modern world of work and organisations with its fast developing technologies. Then, even practitioners who are not actively involved in research will have to rely upon past research or on the assistance of external research agencies. Knowledge of research principles is indispensable in order to be able to critically evaluate ideas and conclusions, weigh proposals, and to derive proper implementations (Drenth, Thierry, & de Wolff, 1998, p. 12).

Of course, it was not all sweetness and light. This scientific attitude brought me in conflict with the critical social movement that took shape among students and junior intellectuals in the 1970s. I was accused of maintaining the social status quo by my supposedly neutral, objective stance, and by my refusal to take (of course the proper) a priori side in the Marxist class struggle. Our publication on the functioning and efficacy of the work council in Dutch industrial companies (Drenth & Van der Pijl, 1966) was criticised for not being “engaged” enough. In the north of the country a large-scale national survey and experimental study on the effects of shift work in the Netherlands (Hoolwerf, Thierry, & Drenth, 1974) was obstructed because our results did not allow us to conclude that “shift work was unacceptable for all categories of workers and under all circumstances”. It was not always easy to defend a strict scientific approach against these revolutionary audiences. One positive spin-off of this movement was a critical reflection on the role of psychologists and the choices they have to make in the possible conflict of interests between individual and system (society, institute, organisation). We have not exactly kept aloof from this debate (Drenth, 1967, 1970, 1972, 1973, 1994).

This is not to say that I have not been involved in “technological” research, such as the development of tests, scales, and inventories. I have published a number of such tests and personality inventories for the Dutch market, but again, the real challenge for me was the development of principles of application and the creation of a general (decision) model for the use of tests, rather than the operational test-construction itself. Such a challenge was offered by the opportunity to get involved in test development in developing countries (Drenth, 1975). Several such projects were initiated and carried out in the 1970s and the 1980s (Surinam, Indonesia, Tanzania, Kenya and Southern Africa (Botswana, Lesotho, Swaziland)). General principles that had to be addressed in this type of research included the

relationship between school grades and aptitude tests (Drenth, 1977), cultural differences and comparability of test scores (Drenth & Van der Flier, 1976), fair selection and discrimination (Van der Flier & Drenth, 1980; Drenth, 1989), predictive versus concurrent validity in developing countries (Bali, Drenth, Van der Flier, & Young, 1984; Omari, Drenth, & Van der Flier, 1983), and the optimal balance of achievement and intelligence measures in the prediction of school performance (Drenth, Van der Flier, & Omari, 1983).

In the course of the 1980s I was invited to take on further administrative and management responsibilities including: member of the Board of ZWO (Dutch National Science Foundation, 1983–86), Rector Magnificus (Vice Chancellor) of the Vrije Universiteit (1983–87), President of the Royal Netherlands Academy of Arts and Sciences (1990–96) and Chairman of the Social Science Research Council (1999–present). My methodological orientation and (selective) reading of more generic and theoretical literature, continuous PhD supervision, and some modest teaching obligations allowed me to keep myself reasonably abreast of developments and productive. But when working in the science management, even dealing with ethical and normative aspects (Drenth, Fenstad & Schiereck, 1999), the quality of the scientific and educational endeavour (Drenth, 1994, 1995) and the primary importance of pure and fundamental research (Drenth, 1996) have remained nearest to my heart.

The title of this article suggested a contradistinction between truth and utility. In the course of this expose it has become clear that in most cases no such opposition exists, and that the two elements often coincide. At the same time I tried to exhibit my predilection for ‘truth’ in those cases where there is a tension between the two poles, and where choices have to be made. I have also tried to demonstrate that this preference has been congruous with the career choices I have made throughout my working life. Which of the two (preference structure and career path) is the chicken and which is the egg is, of course, difficult to tell.

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In preparing these brief comments about Gary’s portrayal of his own career and the many contributions he has made to both theory and applications in applied psychology, I was struck by a parallel between his and my careers as scientist-practitioners. I completed a PhD at Minnesota in 1954 with an emphasis on the psychology of individual differences and an overlay of industrial psychology. I then spent two years as a research associate with Minnesota’s Industrial Relations Center before joining 3M Co. in 1956 as Manager of Employee Relations Research.

While at 3M, I had a wealth of opportunities to carry out applied research studies to develop new selection systems for salespersons, engineers, research and development personnel, and entry level managers. I also helped establish systematic assessment methods to aid in identifying managerial talent. Perhaps the most wonderful feature of the six years I spent at 3M Co. was the freedom to publish everything I did—first in the form of internal technical reports which were distributed to key officials within the company, and second in the form of articles in applied psychology outlets. During those years at 3M, coworkers and I published articles based on applications we had carried out at 3M. Between 1956 and 1963, we published articles in both trade outlets and scientific journals. Over that span, portrayals of what we were doing appeared in Personnel Psychology, Journal of Applied Psychology, American Psychologist, Educational and Psychological Measurement, Personnel, Contemporary Psychology Personnel Administrator, and in 1962 a special review chapter titled ‘Personnel Management’ in volume 13 of the Annual Review of Psychology.

Because of this outburst of publishing during my time at 3M Co., I had the extreme good fortune to be able to return to the University of Minnesota as an Associate Professor of Psychology with tenure to fill the spot vacated by D.G. Paterson upon his retirement.
Overlapping the above activities in time was the founding, in 1967, of Personnel Decisions, International (PDI) by Wayne Kirchner, Lowell Hellervik, and me. Over the last three decades, under the able leadership of Hellervik, PDI has grown into a worldwide organisation with 17 offices in the USA and seven offices in countries located in Europe, Asia, and Australia. In 1975, my colleagues Walter Borman, Leaetta Hough, and I founded an applied research organisation—Personnel Decisions Research Institutes—which currently has offices in Minneapolis, Washington, DC, and Tampa, Florida.

It seems to me that Latham’s approach as both scientist and practitioner represents a model that is quite similar to what has been so satisfying to me. We both have had special opportunities to make contributions to both our science and our practice. I believe we both have had magnificent opportunities to work with and be stimulated by intelligent colleagues and students. We both have had an abundance of opportunities to encounter exciting intellectual challenges in both our practice and our science.

Experiencing the sense of this symbiosis between our science and our practice has been astoundingly rewarding to me and I am certain that Gary has experienced rewards and joys in the same way. His portrayal in the foregoing article most certainly confirms that reality.

Latham’s short article contains an abundance of wisdom about how science is nurtured by practice and how practice advances on the basis of good science. By describing his own experiences as a practitioner and scientist, he provides a vivid portrayal of the symbiotic relationship between science and practice. Careful observation in practice leads to meaningful questions which may be addressed systematically by carefully designed experimental studies. In turn, results from such studies lead to significant improvements in practice. As Latham states in the foregoing article: science drives practice that drives science; knowledge from research is seamlessly transferred to practice, the results of which form the basis for subsequent research.

As Latham points out, an important breakthrough in learning how to enhance productivity in work settings came as a result of an important publication edited by E.A. Locke in 1968 (Locke, 1968).

Subsequently, as described by Latham, 25 years of programmatic research by many investigators has yielded a number of important generalisations about the results of goal setting. For example:

- Participation yields higher goals and greater success in achieving goals than goals based simply on external assignments alone.
- The higher the goal, the greater is the productivity.
- Rewards related to money and praise are effective and about the same.
- Assigned goals and goals developed as a result of participation yield the same results when goal difficulties are equivalent.
• Providing a rationale for goals is important as a basis for goal accomplishment.
• When goals require cognitive abilities as opposed to strictly motivational components, participation in goal setting is especially important.
• Perhaps most interesting has been Latham’s and others’ demonstrations that variable ratio reinforcement schedules can be applied effectively to increase productivity in experimental and in work settings over levels of productivity shown by continuous reinforcement schedules (Yukl, Wexley, & Seymore, 1972; Latham & Dossett, 1978).

The foregoing summary of Latham’s brief portrayal of significant findings over the last 30 years, dating from Locke’s seminal article in 1968 (Locke, 1968), is welcome because it portrays so effectively how additive research can be conducted to enlighten what we know about fundamental questions concerning individual and group productivity in work settings. His discussion of this research is especially noteworthy because he has presented a rich mixture of research and results completed in both laboratory and field settings. The result of these avenues of research has been especially notable in that they have been additive and they have been parametric.

Finally, Latham has offered a number of worthy comments about the current state of industrial and organisational psychology as it is characterised in its dual role as both science and practice. His summarising comments are perhaps the most important portion of the paper in that he strongly emphasises the critical role served by the duality implied by the science-practitioner model in our activities as industrial and organisational psychologists. Latham’s own career has included activities involving different sites and many roles ranging between academic, industrial, and consulting settings and functions. Accordingly, his own career has provided a rich blend of teaching and conducting both basic and applied research activities, of mentoring students and peers in both work and academic settings, and of informing others in all these settings about useful advances and practices that may enhance and improve circumstances in workplace settings. Accordingly, he has in his own career demonstrated with clarity and precision the very important points he has made in his discussion section of the foregoing paper.

Latham’s parting comments are of central importance in assuring future advances in both our science and our practice. It is true that practitioners and academics have equal roles in contributing to the knowledge and benefits derived from deductive research based on inferences derived from hunch-like theory. In effect, scientists and practitioners will, by working in unison (or by adopting the scientist-practitioner model as individuals),

generate new knowledge and transfer knowledge from our journals to build a better world in the years immediately ahead. I endorse wholeheartedly the wisdom of adopting these important values as we pursue the practice and science of our field in the years ahead.

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**Applied Psychology and Science: Differences Between Research and Practice**

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Latham’s discussion of reciprocal transfer of learning between journals and practice has a unique personal perspective provided by his interpretation of his empirical efforts and the published studies that were antecedent to his research. He cites personal values and philosophies as the source of his orientation toward applications of research findings. His personal values and his knowledge of antecedent research literature are important in the direction and success of his research. Applied psychologists are what they are for many reasons. Individual differences among us are no less prevalent than in the populations of workers we study. My after-the-fact sense making of why I became an I/O psychologist is hardly unique but it is different from

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I would like to thank Patrick Laughlin for reading and commenting on an earlier draft of this manuscript and for his enlightening discussions of the issues considered in this article. Archie Green’s seminal works on laborlore contributed far more than the few references to his research would indicate.

Latham’s. I want to study important fundamental qualities of individuals’ lives. More than any other field of behavioural science, I/O psychology is concerned with one of the few essential elements of the lives of individuals. In the United States, and other nations in the industrialised world, we are defined privately and socially by what we do. You are what you do. Doing nothing negates our humanity; to do nothing is to be nothing;

Work, whether pleasant or painful, helps define individual identity. Strangers ask, “What do you do?” We reply to casual or ideological queries by naming skills or places of employment. We relate occupation to race, ethnicity, gender, region, and religion in struggling to comprehend the essential reality of self or community. Our daily tasks give lives coherence; by contrast, the lack of work denies our basic humanity. Workers uncomfortable with abstract discourse assert, “I am a workaholic” or “Hard work’s my middle name”. Philosophers may translate such vernacular lines into “I work, therefore I am” (Green, 1993. p. 13).

Few societies or cultures today have true “coming of age” ceremonies. Acquiring full-time work may be the only obvious event marking a transition from childhood to adulthood, from dependence to independence. Job loss, through layoff, firing, or retirement is a major life-event; it often marks a return to dependency on some entity or person. Dependency in an individualist culture such as the USA (Hofstede, 1980; Triandis, 1989, 1990) is psychologically equivalent to being shunned in a collectivist culture; one’s identity is threatened. The increasing importance of jobs and work in the lives of women and the concomitant reduction of the priority of marriage and families reinforce the centrality of work in all our lives.

The self is no longer subordinate to king and church. Man has replaced God and king at the centre of our mundane lives; the self, our essential persona, is defined more by work than by any other element of our lives with God and king banished from the central role. Others can study peripheral aspects of individuals’ lives such as learning, cognition, and perception. I am interested in the central core of our identities.

There are many points in Latham’s article on which we agree. The most important point of agreement is his main point: practitioners, no less than academics, should read the scientific journals in applied psychology and related fields. As an academic, I could hardly disagree. Journals are the repository of the research we do; if they are not read, our research has no utility. If they are read by other academics, but not by practitioners, the research has no utility for the practitioners. It may have less utility as research. Or it may not. And this is the crux of my disagreement with Latham. I would argue that as research, its utility is not diminished by being unread by practitioners.

There are tensions between researchers and practitioners in applied psychology. On this we also agree. The reasons offered by practitioners for
not reading journals may be reflections of such tensions. We may agree on the source of the tensions; research done by scientists often does not address immediate problems of organisational managers. We diverge following these points of agreement.

For Latham, applicability in organisations, not replicability, is the sine qua non of research and publication. He states that “If the journal article does not transfer, if it does not inform the practice of psychology, it was arguably not worth publishing.” (Emphasis added.) Research is supposed to contribute to knowledge. A judgement that a research study should not be published because it does not inform practice, if followed by rejection from the editor, means the research does not fulfil its goals of contributing to knowledge solely because it could not be applied. It does not become knowledge. This application-as-the-touchstone-of-research orientation appears to derive from the scientist-practitioner model of applied psychological research and training. This perspective, in its strongest form, argues we should be scientists and practitioners; our research should be applicable and we should be the ones applying it. In a form that seems to be endorsed by Latham the scientist-practitioner model is an ideal for which we should strive but researchers and practitioners can exist in I/O psychology as long as the research has applicability in the here and now, in the immediate, foreseeable, defined future. (This short span of time is required unless we assume prescience in organisational managers.) This perspective on science and practice does justice to neither.

Consider some immediately important manifestations of the importance of work. A fired worker in Miami returned to his former place of work after 14 months and killed five former coworkers, wounded one, and killed himself. His suicide note read, “The economic lynching without regard or recourse was something very evil. Since I couldn’t support my family, life became nothing. I also want to punish some . . . that helped bring this about.” Another disgruntled worker, thinking perhaps he was about to be fired, went to his place of work in Honolulu and killed seven of his coworkers.

“Going postal” is in the vernacular in the United States. It usually refers to somebody killing coworkers or supervisors because of the removal of a job from one’s life or because of real or imagined problems at work. These violent acts testify to the importance of jobs held and jobs lost. Extreme anger and interpersonal aggression on the job cause real and immediate dangers. The consequences are serious. Solutions are needed. As applied psychologists, we might conclude that these extreme manifestations of anger and aggression in organisations should be studied; our research should be oriented toward solutions of the phenomena.

A different perspective would argue that researchers should focus on understanding general constructs underlying overt interpersonal aggression,
not just on a few extreme overt acts. Such a focus may have a 20-year payoff in terms of applicability ... or it may never be applied in work organisations. Just as suicide is the culmination in a long series of self-destructive behaviours (Farberow, 1980), “going postal” is but one, isolated, manifestation of interpersonal aggressiveness and abuse individuals dispense and experience in organisations (Glomb, 1998; under review). A focus on the extremes of a dimension loses sight of the importance of understanding the process that may—or may not—culminate in all forms of aggression. It is possible that our studies on aggressiveness and anger in organisations may not lead directly to solutions to violence and killing in the workplace. Researchers are members of organisations and part of humankind. It is hardly irrelevant that we have offered no solutions to problems of workplace violence, but solutions to practical problems are not requirements of research. Social psychology went through a similar “crisis” about 25 years ago. There was much concern about the myriad studies on attitudes and behaviours but no solutions to the problems of racial prejudice and racial violence. Gergen’s article (1973) prompted a discussion of the issues (e.g. Schlenker, 1974). After the opposing views were aired, social psychologists generally stopped telling each other what to do and went back to doing scientific research on general problems of social behaviour.

The goals of science and practice are different. Practitioners want to solve this problem in this organisation at this time. These are not mean goals. Issues of generality are not important because (a) this problem may never come up again, (b) it may come up again but not in my expected (short) tenure in this organisation, and (c) if it does come up again, we can try the same intervention that solved it this time. These are understandable reasons for avoiding issues of generality. The immediate problems are solved while avoiding investments in long-term research that may or not generate a payoff.

Science is concerned with how human and organisational processes generate cognitions, behaviours, attitudes, emotions and other manifestations of fundamental constructs. The goals of scientific research are to establish general relationships, differences in variance, among constructs across populations, settings (including cultures and organisations), time, and instruments. The emphasis in research is on constructs. Scales, variables, or individual behaviours are important as indicators of constructs. (They are also highly fallible indicators if studied one at a time.) Research implies a different time span than most practitioners use. Good research often may take 20 or 50 or 100 years before its applicability—if any—is known. Practitioners have an understandably shorter time span.

Researchers’ and practitioners’ questions come from different sources. Researchers’ questions (should) come from theory and conceptual models of fundamental constructs. These might include relations between attitudes
and affect—considered as different reactions to work situations—and work vs job withdrawal—considered as two dimensions of behavioural reactions to work situations (Weiss & Cropanzano, 1996; Hanisch & Hulin, 1990, 1991; Hulin, 1992; Hanisch, Hulin, & Roznowski, 1998). This is a focus on work and job withdrawal rather than turnover or tardiness. If work and job withdrawal, rather than tardiness and turnover, are recognised as important to practitioners, we can collaborate to our mutual advantage.

That researchers have devoted so much effort to studying practitioners’ questions, e.g. turnover as an isolated manifestation of job withdrawal, is a consequence of the conception of good science offered by Latham but shared by many. It reflects the influence of the scientist/practitioner model on applied psychology. This may be one reason for the lack of general theories in applied psychology and organisational behaviour. Practice oriented research together with a lack of integration of individual findings is characteristic of our field of study today.

The myth that science progresses as we accumulate individual studies on isolated behaviours rests on the comforting delusion that science is advanced by breaking problems into tractable components—individual, isolated, behaviours. Reintegrating these myriad components into a picture of an underlying whole is dismissed as non-problematical. Towers of knowledge will be built when individual researchers toiling in the brickyard have created enough bricks of knowledge. Reintegrating the bits of local knowledge about isolated behaviours into a coherent statement of behaviours in organisations is left to unspecified others.

The Devil, not God, is in the details of individual studies of isolated, bottom-line relevant, behaviours; reassembled shards of egg shell seldom conjure up the image of poor Humpty-Dumpty. So we wait for that audacious genius to one day succeed where the king’s men—and women—failed: a general theory of organisational behaviour will somehow emerge from the welter of studies of isolated costly behaviours and will integrate findings on the many manifestations of important constructs in organisational sciences. While awaiting the arrival of the audacious genius, whose timeliness resembles that of Godot, we can do our parts by studying general constructs regardless of any immediate applicability.

Practitioners’ questions come from specific problems related to the social systems of organisations. Today these problems often reflect recruiting and retaining employees, integrating HR systems of two organisations that have merged, integrating temporary workers into crew-system operations, or the effects of asynchronous communications on performance of geographically or temporally distributed work groups. Yesterday different problems were attracting attention; tomorrow there will be still others. These problems may all be manifestations of a few underlying fundamental concerns but the efforts of practitioners are understandably focused on
solving their particular problem, not addressing fundamental questions about underlying constructs and theoretical models. Practitioners’ evaluations and willingness to read research studies will likely vary as a function of the degree to which the research satisfies their needs. But we will never learn about the few underlying general constructs that account for many manifest behaviours and attitudes if we study problems and behaviours one at a time. There may truly be nothing quite so useful for a practitioner as a good theory but a theory without supporting data will not be used.

The goals of science and the goals of practice are different. Attempts to blend them into one model ill-serves the needs of practitioners or researchers. Contributions of basic research to practice are not necessary for the research to be good science. Contributions of practice to science are not required for the practice to be sound.

If Father Gregor Mendel (1822–84) had been forced to work under Latham’s guidelines, he would have been told to work on something that informed practice at the monastery rather than flower colour and position, plant height, pod shape and colour, seed shape and colour, and flower position of sweet peas—perhaps solve a practical problem by determining whether horse or sheep manure produced better crops. His research would have been rejected by adherents of Latham’s model; it did not inform practice, should not be published, and would not be read. By 1900 his work, published in 1866, became the basis of modern genetics.

George Boole (1815–64), the inventor of mathematical logic, was doing basic research on human cognition. His work, *An Investigation of the Laws of Thought*, and his work on the mathematics of logic, is the basis of all computer programming. To have required Professor Boole to do immediately useful research that addressed practitioners’ problems, would have done extraordinary damage to modern computer science. Every internet search engine is based on instantiations of Boolean operators; practitioners who search the internet, while perhaps blind to the basis of the engines, benefit daily from Boole’s research done over 150 years ago.

To require research to address problems of organisational practitioners or risk rejection assumes prescience among practitioners. How can anybody—practitioner or researcher—know what findings produced by what studies are—or will be—useful to organisational managers? A belief by practitioner reviewers of articles submitted to journals in applied psychology that research must inform practice is misguided at best. It assumes practitioners know what problems will need to be solved tomorrow. Otherwise, the research will be oriented toward solving yesterday’s and today’s problems. The belief imposes a set of goals, irrelevant to science, on the scientific enterprise, an enterprise that is important in its own right.

Doing sound research with internal and external validity is difficult. Requiring researchers to do research that has the desiderata of good science

plus applicability to current organisational concerns can only make good research more difficult and very likely have less generalisability. Requiring researchers to pursue both sets of goals is no more useful than requiring practitioners to do basic research. Practitioners must translate research findings into programmes and interventions that can solve today’s problems in organisations and prevent some of tomorrow’s potential problems. As Latham notes, this is difficult and often clumsy. It is, however, the practitioner’s job, not the researcher’s.

The history of research on recruiting and retention in organisations can inform us about the effects of practitioners’ goals on empirical research. Unemployment in the United States in 1982 and 1983 was hovering near 10 per cent; in Canada it was between 11 per cent and 12 per cent. Research on turnover, job withdrawal, attitudinal and affective antecedents of job withdrawal was difficult to do in work organisations or the US Military. Turnover and retention did not appear to be immediate problems; neither did enlistments. I argued then (Hulin, 1984) that dissatisfied individuals who want to leave an organisation will withdraw one way or another even if they cannot leave their present job because of the lack of alternative jobs. In spite of appearances, organisational withdrawal was a serious problem that was masked by a suppression of the turnover rate by unemployment. Nevertheless, few organisations wanted to support or conduct basic research addressing an issue that was not a problem for them at that time. Many of the studies I did on turnover and job withdrawal were done in organisations that for one reason or another had high levels of turnover (Hulin, 1966, 1968); such organisations may not have been representative of the population of organisations. Some studies were bootlegged by including them as parts of other research that addressed immediate problems in an organisation (Harrison & Hulin, 1989; Hom, Katerberg, & Hulin, 1979; Miller, Katerberg, & Hulin, 1979). Although the research met many of the desiderata of scientific studies, each study could have been improved by a change of focus from one withdrawal behaviour to a behavioural construct as we did later (Hanisch & Hulin, 1990, 1991). The argument that dissatisfied employees will do something—withdraw from their work even if they cannot withdraw from their job because of local economic conditions—was not convincing; we lacked empirical data to support the argument. Singing a chorus of “Fifty Ways to Leave Your Lover” also fell flat.

Unemployment in the United States in November 1999 is 4.1 per cent. Recruiting and retention in work organisations are two sides of one of the most serious problems human resource managers have today. Basic research on turnover is now easier to do; organisational managers can see immediate needs for it. But it is no more important to applied psychology or organisational science than it was 20 years ago; basic research done then on work...
and job withdrawal would be paying substantial dividends now. Organisations might be spared current recruiting wars to replace departing members. Twenty years later we have done some basic research and have empirical support for the theoretical arguments advanced earlier. The research that is used today would have been unread or, worse, rejected 20 years ago because it did not inform practice.

Research on micro issues in the temporary work industry 20 or 25 years ago was also scarce and difficult to do. Temporary workers were a minor concern to business organisations. My research on temporary workers appeared as chapters in books published in Europe (Joray & Hulin, 1974, 1978) and was probably unread by practitioners in the USA. Today contingent workers are no longer a minor issue in work organisations (Hulin & Glomb, 1999). Unfortunately, we know little about the motivations of temporary workers, how to integrate them into crews that operate complex technical systems, or how to supervise and motivate them when many contingent rewards used in work organisations are not available to them. Basic research done 20 years ago would repay investments organisational managers may have made in it although those benefiting from the research may not be those who invested in it. Long-term investments of this kind require a belief in the utility of a greater common good that will flow from basic research. Practitioners are unwilling to invest in research programmes that have a 20-year, or even a five-year, payoff period. They are unlikely to be in their current position in 20 years to benefit from their investments; even a five-year payoff may be beyond their expected tenure. They may also reject research with a 20-year time perspective. This latter is likely according to Latham and seems to portend serious problems for applied psychology in general. The problems will grow more serious as practitioners with this perspective are included on journal review boards.

Differences between scientists and practitioners are inevitable. Tensions may follow from these differences. If, however, a study done on human behaviours in organisations is not read, or is negatively reviewed and consequently does not appear in a journal where it can be read, solely because it does not “inform the practice of psychology” and therefore should not be published, we fetter scientific researchers with the goals of practitioners, goals irrelevant to research. The solution to the tensions is not to require scientists to adopt the goals of practitioners. It is for us to recognise the differences and exploit them . . . and refrain from telling each other what to do.

Research and publications with the potential to contribute to an understanding of so fundamental an aspect of our lives as work should be unencumbered by a requirement that they inform practice. An understanding of individuals and work is essential. Such knowledge is also important for a larger arena than applied psychology. The importance of

work and a job in defining individuals goes beyond the boundaries of organisations or applied psychology. It informs all behavioural science about a fundamental element of individuals’ lives. Our understanding may be followed by applications to, say, aggression and anger in the workplace or interventions in a community following a plant closing. Applications will surely be difficult, perhaps even clumsy, as Latham notes. But difficulty and clumsiness are because the human problems being studied are difficult and complex, perhaps more complex than the subject of any other scientific undertaking. We often attempt to apply research findings that are too narrow because we have done research on the symptoms of underlying general problems rather than the problems themselves. The findings may be broad and theoretical and be equally difficult to apply. However, general solutions come from unfettered basic research.

To pursue the goals of research without adding to the difficulty of this enterprise by encumbering researchers with the goals of practitioners does not imply a rejection of practitioners or their goals. It acknowledges a different set of goals; a different orientation toward the nexus of research and practice related to things organisational; a different reward structure for researchers and practitioners.

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I/O Psychology: Working at the Basic–Applied Psychology Interface

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INTRODUCTION

Taking a position on whether I/O journals should contain more theoretical or practically-oriented publications is like asking a person to choose between their senses, such as sight and sound. It is true you can function without one of these senses, but few would seriously argue that people function better with one sense than with both, or that having more of one sense and less of another would be optimal for all conditions. So it is with I/O psychology—a discipline which works at the interface of basic and applied psychology and which depends critically on input from both theory and practice. In my view, one simply cannot work at the interface of basic and applied psychology, be it in educational, clinical, or industrial/organisational psychology, without an appreciation that both scientific and practically oriented research publications contribute to the advancement of the discipline.

In this short paper, I address issues related to the theory–practice balance in journal publications from three perspectives. First, I consider the question of balance in terms of evaluating a single study, and discuss an alternative framework for making a decision about how to evaluate the potential “utility” of a study to the field. Second, I address the broader question of balance in a journal’s content. I suggest that concerns about balance at this level, such as whether a journal is too “theory-heavy” are less determined by the journal than by the state of the field. Finally, I discuss balance in terms of the personal and situational characteristics that appear to foster valued work at the basic–applied interface.

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BALANCE AS A JUDGEMENT CALL ABOUT A STUDY/PAPER

Consistent with convention in most psychology sub-disciplines, research suitable for publication in I/O psychology journals should provide a clear overview of the research objectives, employ a sound research design, appropriate methods, appropriate analysis, and clear interpretation of the data. These features represent necessary but not sufficient conditions for publication. The more difficult question posed in this special issue pertains to forecasting whether tests of a particular theory, a set of research findings, or write-up of a unique research opportunity may result in a meaningful contribution to the discipline. Obviously, no one knows for certain what the impact of a study will be until long after the publication decision needs to be made. But, as Latham (this issue) points out, the theory versus practice distinction provides little discriminating information for making such a decision because influential publications may be theory-based or practice-based, and may take place in either the laboratory or the field. Indeed, as Latham’s description of his research career clearly indicates, contributions to the field often involve a continuous interplay between basic and applied paradigms.

Nonetheless, decisions about the importance and “utility” of research submissions must be made in an uncertain environment in which the “correctness” of the decision is unlikely to be determined for years, if at all. Rather than focusing on the theory versus practice orientation for making such decisions, I suggest a different perspective that emphasises the breadth of a study’s implications and applications.

The theory–practice debate implies that primacy is accorded to the origins of a study, rather than to its findings. Theoretically oriented studies, for example, are typically considered to be those that derive directly from a consideration of theory, though the study may be carried out in a lab or field setting. In contrast, practically oriented studies are typically considered to be those that derive directly from an applied organisational/societal problem (though the study may be carried out in a lab or field setting). Though applied journals typically encourage both kinds of studies, journal formats often encourage researchers to frame study questions theoretically. As such, this format may give the edge to studies that derive directly from theory, and so be interpreted as discouraging the publication of practice or action-oriented research studies.

In I/O psychology, however, most researchers would agree that the importance of an article lies not so much with the origins of the study, but rather with its findings, and in particular the implications/applications of findings for adding to the knowledge base and affecting future research. The implications of a study’s findings may be theoretical, such as in articles that provide disconfirming evidence for a theory, or may be practice oriented,

such as in articles that provide evidence on the validity of a particular method, test, or managerial procedure. Implications may further be narrow or broad, as when research findings have importance for a particular element of a theory or procedure, or challenge a theoretical perspective or established managerial practice. Research that has broad implications/applications for theory or practice is proposed to hold potentially higher value to readers than research with narrow implications in either domain.

A major advantage of the implications/applications perspective over the theory–practice distinction lies in the de-emphasis of the research setting or conceptual origins of the work in favour of a greater emphasis on the potential importance of findings. This is particularly important given the increasing use of new technologies and high-fidelity simulations that enhance realism in the lab. Coupled with the growing use of sampling methods and statistical procedures to reduce confounds in field research, old distinctions between theory-based and practice-based research have become increasingly blurred. That is, research of all types seeks to maximise realism and minimise confounds. The focus on findings places lab and research studies on a more equal footing and provides a common metric by which to evaluate the “utility” of seemingly disparate research endeavors.

JOURNAL CONTENT BALANCE: MANAGEMENT OF A NATURAL PROCESS

The adoption of an implications/applications perspective to single research submissions does not, however, address the broader issue of journal balance over time. To a large extent, I propose that balance at this level depends critically on the state of the field and the particular topic under consideration. The “context of discovery” and “context of justification” provide a useful perspective from which to consider the basic vs applied balance issue in journal content. In the philosophical tradition of Riehenbach (1938), the context of discovery is concerned with how scientists come up with ideas. The received view of the philosophy of science is that inspiration can come from many sources, including deduction from extant theory to induction from real-world phenomena. In this sense, articles that describe research focused on discovery (i.e. an I/O phenomenon in search of an explanation) very often provide critical grist for the science. Such articles may be keenly inspirational to the science, such as the findings regarding the pattern of aircraft accidents due to “pilot error” (Fitts, 1947), the influence of goals on task performance (Locke, 1966), and the influence of the group member opinions on individual decision making (Sherif, Harvey, White, Hood, & Sherif, 1961).

Discovery of interesting phenomena can be exciting, but such work only goes so far, and a journal devoted only to documenting such things would...
be incomplete, because the reader would never know how or whether to attempt to use such findings in different field settings. As a result, during the early stages of research into a phenomenon or theory development, emphasis is placed on empirical evidence that demonstrates the phenomena or provides compelling support for the basic proposed mechanisms and processes under investigation. In this context of justification, studies that narrow in on mechanisms and processes through control and removal of potential confounds are essential. Such studies provide support for a theoretical explanation of the phenomena, even though such research may show little overt relationship to the complex real-world events that stimulated the theory. Accumulation of findings in this approach provides support for basic tenets of the theory and illuminates eventual implications for practice. Arguably, few of these publications are likely to have immediate practice relevance in that operations and findings are often not directly transferable to applied settings. Nonetheless, such publications have long-term relevance for applied psychologists who seek to develop new practices based on sound research foundations. Such publications also alert practice-oriented researchers to new perspectives, provide a forum for evaluating potential points of contact between science and practice, and facilitate alternative perspective-taking on related practice issues.

Of course, a journal only devoted to the context of justification would be incomplete and perhaps ultimately sterile. As theories mature and explanations for new phenomena accumulate, progress shifts away from the context of justification. At this point, research grounded in realistic settings may again be more useful. Unlike research aimed at “demonstrating” new phenomena or principles, research at this stage, conducted in complex, noisy realistic settings, provides the potential for identifying limiting conditions of existing theories and for stimulating the development of new, alternative practices. Practice-based research at this stage has the further advantage of refining the methods and tools by which theoretical processes are assessed and evaluated, and is more likely to be regarded as having immediate and long-term relevance for both theory and practice. Conversely, in this later stage of theory development, journal articles that focus on further justification, that is, on refining smaller points in a theory or minor disputes within a theoretical perspective are usually difficult to follow for all but the most knowledgeable researchers in the area. Such studies are often less useful for advancing the discipline as a whole than studies that promote technology transfer or extend the theoretical perspective in innovative ways.

As the context of discovery/justification perspective suggests, theory is not the only source of progress at the interface of basic and applied I/O psychology. New technologies, political events, and changes in the size and composition of the workforce may alter human resource management
practices and workplace behaviours in ways that reveal new phenomena worthy of study. In the USA, for example, the implementation of new technologies that permit off-site work has raised a host of questions about the role of individual differences in self-management and managerial practices that facilitate high performance in these environments. In Europe, the break-up of the Soviet Union and the introduction of competitive management practices have led to new theory and research on individual differences in personal initiative and their influence on organisational outcomes. In the USA, increasing diversity in the workforce and low unemployment rates have fostered increased research interest in recruitment practices and their effects on job choice and employee turnover. As such, practically oriented research that documents these changes and their effects on workplace behaviour and organisationally relevant criteria plays a potentially powerful role in stimulating new theory and research.

**BALANCE IN THE CONDUCT OF RESEARCH: PERSON AND SITUATION CONSIDERATIONS**

As Latham’s (this issue) research history suggests, the process of conducting research that has both theory and practice implications is to some extent idiosyncratic, and involves a complex interplay between researcher characteristics and situational opportunities. In an attempt to identify the determinants of valued research projects, Campbell, Daft, and Hulin (1982) identified regularities in the person and situation characteristics that accompany such research. Consistent with Latham (this issue), Campbell et al. (1982) suggested that researcher factors such as competency-based curiosity, creativity, and persistence, and responsiveness to new research opportunities play a far more important role in the production of valued I/O research than does the theory versus practice-oriented distinction. The Hawthorne studies provide a case in point. These field studies, undertaken originally as a consulting project, yielded data inconsistent with theories of that era, yet provided the early empirical foundations for research in work attitudes and group processes that continues today. The lasting importance of these studies cannot be ascribed to the theoretical or practical project aims alone. Rather, the value of the work derives from the unique way in which theoretically grounded research, conducted by curious and knowledgeable researchers in real-world settings, led to new ways of thinking about basic organisational problems and conducting field research.

An alternative perspective to the question of what balance in the conduct of research may require can be derived from Boehm’s (1980) distinction between the traditional scientific model and organisationally driven research. As Boehm (1980) noted two decades ago, academic research tends to follow the traditional scientific model, whereas organisationally driven field studies
often follow an alternative research model. Although Boehm (1980) argues that the two models can be mutually supportive in advancing the field, she also notes several important differences in the way that research in each model proceeds. In contrast to the traditional model, for example, research in the alternative model starts with a pressing organisational problem. Researchers must connect the problem and proposed solutions to relevant theory and “sell” the research project to the organisation. As such, researchers working at the interface of basic and applied I/O psychology must be facile with both models, and must be alert to opportunities that will permit investigation of basic research questions in the context of solving organisational problems.

Working at the basic–applied interface in I/O psychology is a daunting task for most researchers. I/O scientist-practitioner training programmes seek to facilitate the development of knowledge, skills, and expertise requisite for operating effectively in both the basic and applied research environment. Although some individuals proceed to develop highly successful careers on the basic–applied interface, for many persons successful work at the interface occurs as a result of collaboration with researchers who possess complementary expertise. Collaborative arrangements in which individuals have disparate talents and resources, but share mutual research interests, provide one viable method for working at the interface, and have been increasing in frequency during the past decade. However, collaborative arrangements require that interested parties share a common understanding of the research and organisational problem. Journal articles provide a critical means for fostering greater awareness of potential interface research opportunities and for forming collaborations.

In summary, the question of journal balance may be considered from three related perspectives: individual studies, journal content, and researcher competencies. In each of these perspectives, the distinction between theory- and practice-oriented research is less important than questions related to the potential implications of a research submission, the state of knowledge and practice in different topic areas, and the cumulative expertise and resourcefulness of I/O researchers working alone or together at the basic–applied psychology interface. Real progress at the interface requires input from both scientists and practitioners; failure to interest and involve both groups in the research enterprise bodes poorly for the field as a whole.

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The lead article by Gary Latham is on the topic of practically oriented versus theoretically oriented researchers and their preferences when publishing and using published research. There is ongoing debate on this topic within the field of applied psychology. Some describe the debate as follows: practically oriented researchers, typically in company or consulting settings, say there is less and less value in publishing and the published research. Theoretically oriented researchers, typically in academic settings, contend that basic research should be preferred and research on practical problems will not advance the science of applied psychology.

In his article, Gary Latham describes how he finds value in the published literature and uses it in his practical research, translating it to the practical problem to be addressed. Then, in a reciprocal manner, he shares what he learns from his practical research by publishing the results in academic journals. Gary is a fine example of this and argues for this model of reciprocal transfer between science and practice.

I agree with Gary on many of his points and find that his reciprocal model does exist, but in various forms. I have worked in what can be described as applied or practical settings for my entire career as an industrial/organisational (I/O) psychologist.

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I find in my own work and that of other I/O psychologists working in corporations, we do in fact use the published literature when addressing real-world problems and practical research issues. For example, in my work and that of the I/O psychologists on my team, there are numerous examples of finding great value in the published literature for practical research issues. These include research on job satisfaction, employee attitudes/business outcomes, transfer of training, motivation, goal setting, and self-efficacy studies, to name a few. As a recent example, for model building on employee motivation and other variables, we reviewed published articles on motivation in the major applied psychology journals.

The age of technology allows us to search on-line quickly for relevant published literature for whatever practical research issue we need to understand and address. We can search vast numbers of published articles on topics, looking across numerous journals rather than just the ones we subscribe to. Methods to search for relevant publications on-line will continue to improve in the future and become quicker and more accurate—and easily allow the ability to find any relevant published research for the problem at hand, including published research beyond the specific journals of our field.

This leads to the point that many applied psychologists in the field of I/O psychology are not just relying on their own field of published literature when faced with practical research issues. Other journals, publications, and information sources may prove equally or more useful, and on-line systems allow for easy access to such publications. For example, we looked at the published literature on demographic studies when trying to better understand and predict global trends in employee issues. Another example is using publications by sociologists on cross-cultural topics; these have helped us as we research job satisfaction across more than 75 countries.

When published literature within our own field yields little relevant research for a practical problem, we still rely on the theoretical research we learned in our education. For example, we recently searched for published literature within the I/O psychology journals on cross-cultural issues in performance appraisal, and found little. However, our “theoretical grounding” in performance appraisal, motivation, and measurement from our respective I/O psychology educations was helpful in the design, item construction, scaling, and how to assess the effectiveness of the new performance appraisal approach.

Having a theoretical grounding—a deep understanding of the various theories of our field achieved through education—is at times more valuable than whether a published study on a particular topic has been read. We may not transfer a published journal article into practice every day, but understanding the theoretical research of our field allows us to transfer theory into practice on a daily basis. Companies find this of great value.

In his lead article, Gary also mentions the value of small professional groups (e.g. Summit Group) to share research and to facilitate reciprocal transfer between science and practice. I agree with him that groups such as these are of immense value, perhaps well beyond reading journals. Groups such as the Mayflower Group (global companies who benchmark survey data and share research), the Dearborn Group (I/O psychologists in applied settings), and the Information Technology Survey Group (high technology companies from the USA and Europe engaged in surveys) are of great value in sharing research and findings. Because these groups are made up of researchers in applied settings, one could argue that they do not allow for full reciprocal transfer between science and practice. However, some of these groups bring in leading academic researchers to speak on relevant topics or to collaborate on research, which encourages this transfer.

Excellent research has been shared in these groups on timely, important issues affecting people’s work lives. An important value of these groups is that they provide methods to transfer and share practical research with others in applied settings, who in turn transfer it into their own work. Members also share their research at larger professional meetings, such as the Society for I/O Psychologists, so sharing with the broader profession also occurs in many instances. However, many of these studies do not get published; they may have “field” flaws that would not meet the rigors of the journals in our field and/or the topic may be considered too sensitive (e.g. employee attitudes) to have the specific results published.

Some new groups are forming whereby applied researchers in a number of companies sponsor relevant up-to-the-minute research needs that are then conducted by well-regarded academic researchers (Global Research Consortium is an example of one of these groups). Such groups may be forming to fill the void of published research that does not consistently transfer to issues facing companies today. The company I work for belongs to one of these groups. Recent research topics include: multi-cultural joint-venture leadership teams, virtual teams, human resources practices that are effective across cultures and those that are not, and distant leadership. As a check, I reviewed all of the articles published in 1999 by the *Journal of Applied Psychology* to see if any of these topics, or related topics, appeared. None did. This was for only one journal, but it does highlight that journals in our field may not have published articles that transfer well to workplace issues in the changing organisations of today. As an example of lack of relevance to emerging topics, a study published in this journal in 1999 on “the effects of stand-up and sit-down meeting formats on meeting outcome” would have been more relevant if it had been on “the effects of face-to-face and teleconference meeting formats . . .”, which is an increasingly common meeting format in companies.

This leads to two related points. First, not all theoretically based publications build well on theory or practice. I could provide examples, but
I am certain the reader has read articles that fit this description. Hypotheses for this usually have to do with the need for academicians to publish (or perish), resulting in some publications of low value from a theoretical or practical perspective, or both.

Second, the published literature that is practically based will not necessarily be of value to all applied researchers in all organisations. I point out above that a study of stand-up and sit-down meetings is not relevant to practical issues in my (high-technology, global) organisation, yet the study may still be relevant to a researcher in a manufacturing setting seeking ways to improve the workplace, including meetings.

Greater collaboration between academicians and practitioners would increase reciprocal transfer and, I believe, ultimately advance both science and practice. A recent article by Rynes, McNatt, and Bretz (1999) provides greater understanding of the outcomes from academically based research within organisations. Their findings provide preliminary evidence that increased interactions between researchers and practitioners provide value to both parties, although they also found that most top-tier organisational research projects by academicians do not result in immediate application in organisations. Rynes et al. argue for academic–practitioner collaboration as the main hope for continued viability of the scientist-practitioner model.

Perhaps other approaches for collaboration should be explored, such as those described earlier whereby a group of companies sponsors research that is relevant to current issues and can also be designed to advance science. There also may be approaches for collaboration that can be transferred from other fields of science with strong practitioner-science orientations. For example, in genetics research, also a relatively new field, there is collaboration between applied researchers (employed by pharmaceutical companies) and those working in academic settings. An example of this collaboration is a very significant research endeavour to “break the genetic code”, called the human genome project. This research involves applied and academic researchers globally.

To end this commentary on the positive note I believe this topic has, I offer the following comments. The theoretical basis of our education is extremely valuable for those of us in applied settings. It is my experience that researchers in applied settings use their education and grounding in the theoretical literature on a daily basis—in how we approach problems, look at issues, and recommend approaches.

Those of us in applied settings also search for and use published research and information that helps us do our work, much of which comes from publications in our field, but may also come from a broader range of published studies in other fields. We seek new ways, including collaboration on sponsored research, to have relevant applied research findings so we can
better understand and address important human issues in companies today and into the future.

The purpose of a lead article in this journal is to present controversial issues that stimulate debate and discussion. Debates that look at “practitioner versus academician” issues can lead to divisiveness. They also can lead to dialogue and greater collaboration. I hope the latter is the outcome of this discussion, and I appreciate the opportunity to share my views.

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