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Enhancing the Social Impact of Research in Work and Organizational Psychology - Beyond Academia

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Abstract

The purpose of this special issue of *European Journal of Work and Organizational Psychology* is to give much-needed airtime to how, when and why research in work and organizational psychology does and does not make a difference in the "real world." We review existing coverage of this topic, in the contexts of long-expressed concerns about how our discipline does not make enough difference, the governance of academic institutions and the Covid pandemic. We then present the ten papers selected for this special issue, summarizing some of their main arguments. Collectively, the papers offer conceptual analyses of impact, case examples of impact in the form of a single piece of research or a long programme of it, and insights derived from critical management and feminist literature. We offer six general observations from the papers, discuss some points not covered in them, and suggest how work and organizational psychology can, and perhaps must, be conducted differently if it is to survive as a viable discipline and profession.

The impact of scientific research - defined as "an effect on, change, or benefit to the economy, society, culture, public policy or services, health, the environment, or quality of life, *beyond academia*" (HEFCE, 2014, p. 6; italics ours) - has long been a topic of interest to policy makers, funders, and citizens, as well as to researchers themselves. The belief that scientific knowledge is a force for the general good has been a trademark of Enlightenment thinking and a guiding principle for the development of European universities and research centres (Outram, 2019).

What (and Who) is Research ‘For’?

For many years, concerns have been raised about the disconnect between published research and the interests of the wider public (Lindblom & Cohen, 1979). These appear to have intensified during the 21st century. In work and organizational psychology and related areas recent editorials have condemned “the triumph of nonsense” (Tourish, 2020) and “the crisis of relevance” (Birkinshaw, Lecuona, & Barwise, 2016; Hoffman, 2016), stating that “industrial-organizational psychology has lost its way” (Ones, Kaiser, Chamorro-Premuzic, & Svensson, 2017). The underlying observation is that, although the number of published articles is ever-increasing due to publication pressures worldwide, these efforts do not seem to be matched by an increased ability to address real-life employee and organizational issues (Anderson, Herriot & Hodgkinson, 2001). If anything, some of the most prestigious and widely cited articles in our field seem to be a world apart from the concerns of practitioners or citizens, and virtually indecipherable by anyone other than the members of a given academic micro-tribe (Tourish, 2020). Hambrick (2007), in his semi-comical takedown of the management field’s obsession with making a theoretical contribution above all else, wrote:

> “After years of comparing notes with colleagues about the rejection letters we have received, it seems the most annoying passage—which I am sure editors have preprogrammed for handy one-click insertion—is this one: The

> reviewers all agree that your paper addresses an important topic and is well argued; moreover, they find your empirical results convincing and interesting. At the same time, however, the reviewers believe the paper falls short in making a theoretical contribution. Therefore, I'm sorry… etc., etc., etc." (p. 1346)

To top it off, even if practitioners *would* be interested in our research and *would* be able to understand our use of terminology, theory, and methodology, the issue of academic publisher paywalls poses yet another barrier to dissemination beyond academe. This has led several funding agencies, such as the European Research Council, to make open-access publishing by grant holders mandatory (and budgeted for).

The crisis of relevance is typically attributed to perverse incentives and hypercompetition in academia (Edwards & Roy, 2017). This is particularly palpable when interacting with junior scholars at conferences and workshops, especially those on tenure tracks, who state they "must publish in AMJ" (i.e., Academy of Management Journal). They frantically ask for advice about "publication strategies", or announce that they plan to start doing research on the topics that truly interest them "after they are safe" (i.e., get tenure). Anecdotally, some young scholars seem to have been socialized into thinking that the goal of doing research (and/or getting a PhD) is to publish *in and of itself*—as an end, rather than a means, of doing research that matters. This creates a culture of 'the tail wagging the dog', where researchers are riddled with anxiety due to pressures to publish in high status journals, where high status is defined by position (or sometimes simply presence) in the journal ranking list favoured by their employer or peer group. Whether the journal of choice is one that potential users of the research would read is rarely a significant consideration. Researchers, especially perhaps junior ones, often struggle to come up with 'good' research ideas and get access to data considered appropriate by such journals. Meanwhile, so many societal problems (and datasets) remain unstudied and unsolved.

It is disheartening to see junior scholars leave academia because they no longer see what (and who) research is actually ‘for’. Many imagine the academy to provide a space where deep and independent thinking is expected and encouraged, but find it to be more like a ‘publication factory’, in which teaching and service is piled onto those not strategic enough to ‘game the metrics’ against which they are evaluated. (On the other hand, of course, are the tens of thousands of junior scholars who want nothing more than to keep doing research but cannot secure a non-precarious position). There trends are, at the very least, striking for a field that has had its origins in practice, driven by real-world problems, from its very conception (for a historical review see Koppes, 2014).

So, although a lot of published research without a doubt has had an ‘impact’ on institutional rankings and individual scholars’ careers, as well as on the impact factors of journals, little of it appears to have had any impact on ‘the real world’—and in many cases has lacked even the slightest intention of doing so. Bartunek and Rynes (2010), for instance, set out to examine the ‘implications for practice’ sections of all articles published in the top five management journals between 1990 and 2010, and found that only 51% of them even *had* an identifiable implications section to begin with. Reflecting on the typical writing style of these sections, the authors wrote:

> “Although most implications for practice sections also make prescriptions, they offer those suggestions tentatively, using language such as *may* or *possibly* 74% of the time. This writing style, which is consistent with academics’ reluctance to make claims that go beyond their immediate data, probably discourages practitioners from imagining ways in which academic findings may be applied to a variety of situations.” (p. 108)

Concerns about the apparent lack of impact of social research have filtered through into the mindsets of politicians and policy makers, as well as university administrators, and even journal editors and publishers. The result has been the emergence of the so-called ‘impact agenda’—a call for academics and their institutions to demonstrate the socioeconomic benefits resulting from their research, especially where it is publicly funded

(Penfield, Baker, Scoble & Wykes, 2017). This has opened up a wide range of questions which this special issue seeks to examine and answer. These include the definition and measurement of impact, how research projects can be designed and implemented to maximize the chances of impact, the institutional arrangements for monitoring and evaluating it, and the negative sides of impact. There is also the question of how impact relates to other criteria by which academic research is evaluated, such as rigour, depth, originality, and generativity (i.e. impact on other academic work, but not necessarily on social institutions or everyday practice) (Belcher, Rasmussen, Kemshaw & Zornes, 2016).

The Rise of the Impact Agenda

The rise of the impact agenda in the social sciences has been ubiquitous and, at least in the UK, dramatic. Stung by criticisms that earlier assessments of research quality in the UK neglected impact, in 2014 the assessment of research conducted every six or seven years across the UK university sector, known as the Research Excellence Framework (REF), explicitly set out to evaluate the impact of academic research on wider society. Performance on REF metrics has major consequences for university funding and prestige. Academic institutions, in addition to producing research outputs (mainly publications), and showing they were a supportive and well-managed environment for research, were required to submit a number of case studies demonstrating the impact of their research outside academia. These were later placed on open access platforms, allowing anyone to judge for themselves the extent and nature of impact (https://impact.ref.ac.uk/casestudies/). Example cases are: Changing approaches to the production of cars (University of Bath); Identifying and eliminating bottlenecks to entrepreneurship and development (Imperial College London); and Defining the duty to promote equality in UK equality and discrimination law (University of Oxford).

Thus the UK appears, not for the first time, to have the dubious honour of being at the forefront of institutional trends to regulate, monitor and evaluate the work of academics, even as it unleashes the forces of the free market and competition on its universities. The introduction of impact in 2014 was deemed a success (inevitably) and it features again in the 2021 REF, this time with a slightly higher weighting in the overall assessment than in 2014. The UK is not alone, however. Its penchant for government-initiated research evaluation has spread to many other countries over the last two decades, and there is every reason to think that the addition of impact will also be a trend-setter. Hong Kong has adopted it similarly to the UK, whilst Australia has taken a somewhat different approach in some respects (Williams & Grant, 2018).

As with the evaluation of research quality as a whole, the evaluation of impact is part of a broader and understandable move to enhance the accountability of scientific institutions, especially when they are in receipt of substantial sums of public funds. The evaluation of impact is aimed, in particular, to correct the tendency in the social sciences for rigour and relevance to move in opposite directions. As numerous commentators have pointed out (Alvesson, Gabriel, & Paulsen, 2017; Dodge, Ospina, & Foldy, 2005; Fincham & Clark, 2009; Hodgkinson & Rousseau, 2009; Kieser & Leiner, 2009) articles published in 'star' journals have become more rigorous and obscure and of limited or no value to 'practitioners', such as policy makers, managers, administrators, trade unionists, clinicians, entrepreneurs or consultants.

Few researchers would reject attempts at demonstrating and evaluating the impact of academic research. However, the rise of the impact agenda has resulted in the growth of a bureaucratic machinery, set of metrics, and discourse around impact which has generally not been welcomed by researchers themselves (Chubb & Watermeyer, 2017). In a short period of time, many universities in the UK and increasingly elsewhere have set up impact offices, issued impact directives, employed impact consultants and journalists, and elicited every kind of evidence—plausible or spurious—to demonstrate the impact of their work. A new ‘impact professional’ makes his or her presence felt in research committee meetings, and the word ‘impact’ is now a buzzword in university public relations as well as in their directives and policies. In some instances, majestic claims of impact are based on a few testimonials from practitioners, company directors, or public administrators. In reality, the link from a specific ‘improvement’ to a particular research discovery and associated publication is often approximate at best.

Dictionary definitions of impact as “the action of one object coming forcibly into contact with another” are a useful reminder that it involves two separate objects and that their interaction and the outcomes of this interaction involve force. This mechanistic conception of impact, after the model of colliding billiard balls, shapes the conceptualization of how science ostensibly interacts with society. Akin to a hammer hitting a nail or a meteorite hitting a planet, knowledge is seen as having a sudden, forcible and measurable effect on society and its institutions; or conversely, as failing to have such an effect. Application of such a concept to how academic research affects or should affect the many social institutions and ecologies it inhabits is obviously flawed, or, worse, a naked exercise in power seeking to submit academic research to the interests of the powerful.

Another issue is that an overly mechanistic understanding of impact stifles the chance discoveries and serendipity that fuel some of the most exciting research findings (de Rond & Morley, 2009; Gabriel, 2013; Merton & Barber, 2004; Roberts, 1989). It would also potentially subordinate the generation of knowledge to the interests of the rich and powerful, and virtually put an end to academic freedom of inquiry—turning researchers into consultants at best, and paid functionaries at worst. (We have on occasion been told by corporate practitioners, for instance, that we should do research into the topics that interest them at their simple request - for free - since they “already pay for our work in taxes”.) A mechanistic approach also fails to reflect how knowledge, at least in the social sciences, is mediated, combined, refined, supplemented, modified and redefined in its applications. Practitioners, whether policy makers, civil servants, managers, professionals and so forth, do not typically read a piece of academic research and then decide to implement it. A piece or pieces of academic research may be brought to their attention by a professional publication, discussion or forum; they may become aware that some of their partners, associates, comparators or competitors have introduced an innovation based on a piece of academic research. They may then investigate the nature of the innovation, judge different ways of implementing it, combine it with other innovations, before introducing it to their practice. In all this, practitioners may function more like cooks, trying different recipes and adapting them to their needs, rather than as billiard players striking a ball in way as to strike another ball, sinking it in a pocket (Gabriel, 2002).

Many other routes to impact can be identified (Reed, 2018). First of all, policy makers often invite a famous academic to help them because he or she is famous and has general expertise in an area. Then for the sake of demonstrating research impact, that academic’s research is somewhat retro-fitted to the impact. The second, related, route, is that an academic is commissioned to work on an issue of practical relevance to policy makers or managers, who use what was found because they asked for it (and likely paid for it) in the first place. Again, this makes use of the academic’s general expertise in a field, and/or the academic manages to craft a research publication out of it. This is an art in itself, and is often done by adding a few bits to the project which address an issue deemed academically significant, without necessarily telling the project sponsor. However, this route is often tough because researchers tend to find it hard to meet the standards of academic rigour and practical relevance at the same time. This demonstrates how easily impact becomes a 'game' where the focus is on how to make impact *look* good, rather than to *have* good impact.

What Might 'Impact' Look Like in Work and Organizational Psychology?

There is substantial discussion of the factors that inhibit the 'real world' impact of research in work and organizational psychology and related areas (e.g., Anderson, Herriot & Hodgkinson, 2001; Bartlett & Francis-Smythe, 2016; Bartunek & Rynes, 2014; Briner & Rousseau, 2011; Gelade, 2006). Notable in these discussions is the assumption, usually implicit, that impact or relevance of research to practice is 'good', and to be striven for. There is also a general acceptance (again, often implicit) that there is a gap between what researchers provide and what practitioners need, and that it should fall on researchers to make their research relevant to practice, rather than having practitioners work harder to uncover the relevance of academic research. However, the treatment of academics and practitioners as entirely separate groups has also been challenged (Symon, 2006; Hodgkinson, 2006).

This concern about a lack of impact may be exacerbated by the prominence of the 'scientist-practitioner' model in work and organizational psychology. The scientist-practitioner model assumes that professionals in our field should be proficient in conducting both research and practice (even if their job description at any given time does not require both), and being able to link the two. At the very least, they should be able to evaluate the utility of research for practice and practice for research, and respect the value of both types of activities. Whether it reflects reality or an aspirational goal (Rupp & Beal, 2007), the scientist-practitioner model implies that a divide between research and practice is a challenge to the effectiveness of the work and organizational psychology profession and the identity of its members (Bartlett & Francis-Smythe, 2016).

A dominant way of thinking about the gap between research and practice was introduced by Anderson et al. (2001), who suggested a simple two by two matrix with methodological rigour (and/or theoretical sophistication) on one axis and practical relevance on the other. The ideal is considered to be ‘pragmatic’ science which is high on both, but it is argued that the academic system tends to lean towards ‘pedantic’ science (high and low), whilst the perceived needs of organizations tend to militate towards ‘popularist’ (low and high) or ‘puerile’ science (low and low). Hunt (2018) framed it a little differently, suggesting that research published in academic journals tends to be *explanatory* and *methodological*, whereas practitioners want research that enables them to help people and organizations understand and change themselves. This, Hunt argues, requires *descriptive* and *application* research—i.e. work that defines the current situation and then uses theories and concepts to identify how to change, not just recommend that change should be made. The scarcity of the latter types of research was analyzed by Briner and Rousseau (2011), who found that often there are far fewer studies directly relevant to a particular problem than might be expected.

Interestingly, closely allied fields such as human factors and ergonomics do not seem to suffer from the divergence of research and practice (Chung & Williamson, 2018). Anecdotally, many REF impact case studies are based on Operational Research, a sub-field of management where academic journals publish articles that apply or extend existing concepts or algorithms to solve an organizational problem. In contrast, articles in work and organizational psychology rarely start with an organizational problem to be solved. Even when they do, theory has to be tested, probably with some novel element, and not simply applied. Furthermore, the organizational problem fades into the background rather than being the motivating force of the paper (Hambrick, 2007).

Some solutions to the scientist-practitioner divide have been proposed, ranging in scope from the ‘macro’ such as evidence-based management (e.g. Rynes & Bartunek, 2017), to the ‘meso’ such as integration of consultancy and scientist-practitioner models (Bartlett & Francis-Smythe, 2016), and the ‘micro’, including specific proposals for better processes for collaboration between researchers and practitioners (e.g. Anderson, 2007; Gelade, 2006; Hodgkinson, 2011; Walker, 2008). Gelade (2006) suggests that practitioners value academic articles where there is a series of short responses from other authors because this helps to unearth the practical possibilities and limitations of the focal paper. He also argues that work and organizational psychology academics should be bolder in making practical recommendations from their research. Hodgkinson (2006), however, cautions against this, because the limitations of any given study put a limit on what can legitimately be inferred from it. In their history of the UK's National Institute of Industrial Psychology (NIIP) Kwiatkowski, Duncan & Shimmin, (2006) point out that psychologists are trained to be sceptical and therefore hesitant about application, such that Gelade's suggestion would require a change of mindset. Symon (2006) advocates having more practitioner reviewers of academic papers.

Hodgkinson, Herriot and Anderson (2001) suggest that researchers could be trained more in socio-political as well as methodological skills (see Jalil, Zhu & Alonso, 2020 for what these skills might be). These authors also state that methodological rigour could be redefined somewhat to reflect the needs of user communities. This creates an interesting contrast to the heated replicability, statistics, and preregistration debates being held in social psychology right now (e.g., Koole & Lakens; 2012). Hodgkinson et al.'s (2001) suggestions would perhaps promote the descriptive and application research that Hunt (2018) advocates. Walker (2008) and others have suggested that using journalists to write accessible summaries of research can help, and Anderson et al. (2001) urge governments to introduce research funding evaluation personnel and processes that favour pragmatic science.

There is also some acceptance that there is necessarily a tension and some divergence between researchers and practitioners (Anderson, 2007; Bartunek & Rynes, 2014). Further, not all the evidence indicates that the divide is proving fatal for impact. Bartunek and Rynes

(2014) cite Baldridge, Floyd & Markoczy (2004) who found a small positive correlation between academic citations and practitioner ratings of article relevance to practice. Gelade (2006) in an informal consultation with practitioners he knew, found higher somewhat higher perceived relevance of academic articles than he expected. Rynes, McNatt & Bretz (1999) found that when researchers and organizations engage deeply with each other and commit resources to a research project, the likelihood of implementation of research findings increases, and so does the number of academic citations of published research arising from the project. Bartlett and Francis-Smythe (2016) surveyed UK work and organizational psychologists and found that they expressed considerable interest in and use of scientific research despite some clients not being interested in the evidence underpinning interventions. Notably, their assimilation of research was not necessarily direct from research articles. Books, professional networks, and articles that they considered ‘scientific’ but were not published in academic journals all featured quite strongly.

Importantly, there is also the recent initiative headed by Gudela Grote and Jose Cortina under the auspices of the Alliance for Organizational Psychology, which has produced a manifesto for better research in work and organizational psychology. Much of this concerns the impact and integrity of what we do (Grote, 2017). Arguably, work and organizational psychologists have so far largely side-stepped the ethical and conceptual debates around impact, some of which we have discussed earlier in this editorial. It is, indeed, relatively rare for published research in work and organizational psychology to explicitly consider who the research will benefit, and whether this might come at the cost of other stakeholders (Lefkowitz, 2008). Bal and Doci (2018) have made a case that when psychologists *do* seek to have impact, it often serves capitalist and neoliberal agendas - and therefore the rich and powerful in society - in a rather uncritical way (e.g., increasing employee productivity to improve the ‘bottom line’ of corporations). However, their arguments have been strongly criticized by some (e.g. Guest & Grote, 2018) as over-simplistic and under-estimating of the ethical and critical faculties of members of our profession, as well as who benefits from this type of research, and in what ways.

How Does the COVID-19 Pandemic Change Things?

As we were finalizing the articles for the publication of this special issue, the spread of the COVID-19 virus was disrupting virtually every social practice and institution on the planet. The effects of the virus are likely to be long-term and diverse. There is ample evidence already that academic research, and the institutions that host and publicize it, are likely to be profoundly affected. A substantial amount of research funding has already shifted to disciplines and research programmes that can directly contribute to the management and defeat of the virus. Equally, however, the onslaught of the virus has substantially re-oriented the priorities of many universities away from research and more in the direction of teaching, student support, and cost efficiency. Particular focus is the delivery of relatively safe yet affordable teaching to students, reducing their exposure and that of faculty to infection.

The majority of academics consequently spent the summer of 2020 (in the Northern hemisphere) reconfiguring their courses for online or mixed mode delivery. The usual set of academic conferences and meetings was substantially reduced and conducted virtually. It would be fair to say that the anxieties unleashed by the virus on many fronts in Higher Education appeared to trump the anxieties over research evaluations and the exigencies of exercises like the REF in the UK. Indeed, in light of COVID, even the REF director himself was forced to concede that:

> "We were seeking to balance a number of things in developing revisions to the exercise. In particular, ensuring a fair approach, that could make some allowance for the effects of COVID-19 on submissions, while minimising additional burden on the submission process." (Hackett, 2021)

The COVID crisis is exacerbating the stresses and strains that have afflicted higher education institutions for some time, particularly the way research in the social sciences is conducted and funded. These have been noted and debated for some time and, in the view of some make the current system unsustainable. A recent think piece on doing meaningful research asked:

> “How long will governments be prepared to fund work that has little social value in the light of ever more pressing claims on their budgets? How long will the tolerance of the voters and the public last for what increasingly appears as tribes of detached professionals fiddling while Rome is burning? How long will students and their parents endure intolerable levels of indebtedness and foreclosure, financing increasingly for non-academics irrelevant research and poor education leading to non-existent jobs? How long will the majority of academics themselves put up with the joyless task of breathlessly chasing the next article hit while the pressures on them keep piling up? How long will they continue to accept inequalities that see some star academics earning many times the salaries of those who carry the burdens of teaching?” (Alvesson et al., 2017, p.141)

It seems to us that the current pandemic is likely to make the old system of organizing and funding social research in disciplines like work and organizational psychology even less sustainable than it has been in the past. Impact and relevance will, if anything, assume even higher priority in determining how to divide the diminishing pots of research funds among institutions and individuals, making the discussions promoted by this special issue even more topical and critical.

This Special Issue

Our stated aim in this special issue was to give researchers and practitioners in work and organizational psychology new and significant insights, ideas and tools that will enable research in our field to make a greater positive impact than it has so far in the world outside academic research. Generally speaking, we received four types of articles: conceptual papers, empirical papers spanning a single study or research project, empirical papers spanning a research program that extended over the course of several years or even decades, and papers taking a critical approach. This variety is an immediate help towards achieving the aim because it offers a range of insights, ideas and tools. Nevertheless, in the first instance, we received fewer papers than we had hoped for, itself an indication that discussing impact in relation to their own work is not a favourite pursuit among the majority of work and organizational psychologists. Moreover, several of the papers we received did not sufficiently fit the remit of this special issue. However, amongst the submissions were a number which gave us cause for optimism, and even inspiration. We therefore believe that this has been a valuable venture and one from which other scholars in the field can derive many useful lessons. Some of the ultimately accepted articles are directly of the 'how to enhance the relevance of your work and reach a wider audience' variety. Others concern the more subtle ways in which impact may occur, in some cases against the political forces opposing it, in other cases triggering unexpected chain reactions with beneficial results. Yet others address the more fundamental issues of the meaning and value of impact, and its relative importance in our work as academic researchers. Taken together, across the ten articles in this special issue we hope to have avoided some of the inanities of the impact agenda, while recognizing that academic researchers have a collective mandate to demonstrate that their work has a positive effect on the societies and institutions they are part of.

Conceptual Papers

In the first article in this special issue, Ulrica von Thiele Schwarz, Karina Nielsen, Kasper Edwards, Henna Hasson , Christine Ipsen, Carl Savage, Johan Simonsen Abildgaard, Anne Richter, Caroline Lornudd , Pamela Mazzocato, and Julie Reed (2021; this issue) propose ten principles for impactful research, which they dub the 'Sigtuna Principles' after the Swedish town in which many of their deliberations occurred. Specifically focussing on organizational interventions, the eleven authors from various academic fields conducted a lengthy and detailed collaboration to synthesize and distil their knowledge and experience about making interventions successful. They argue that scientific rigour and practical impact are separable but potentially compatible concepts, especially if the linear assumption of science leading to impact is abandoned in favour of knowledge being co-produced and utilised. The principles are 1) Ensure active engagement and participation among key stakeholders; 2) Understand the situation (starting points and objectives); 3) Align the intervention with existing organizational objectives; 4) Explicate the program logic; 5) Prioritize intervention activities based on effort-gain balance; 6) Work with existing practices, processes, and mindsets; 7) Iteratively observe, reflect, and adapt; 8) Develop organizational learning capabilities; 9) Evaluate the interaction between intervention, process, and context; and 10) Transfer knowledge beyond the specific organization. The authors elaborate helpfully and fully on each of these. They point out that most of them are applicable at multiple stages of organizational interventions, that there is no particular temporal or other consideration governing the order in which the principles are presented, and that whilst some may be familiar to work and organizational psychologists, others may not be. Whilst the principles have been developed in the context of intervention research, it might be argued that their applicability is much wider, and that work and organizational psychology would benefit from considering their applicability to every research project they undertake.

In the second article in this special issue, Helen Hughes, Matthew Davis, Mark Robinson and Alison McKay (2021) identify six assumptions underlying the notion of impact: it focuses on change, it implies that impact occurs *after* a research project, that impact should be attributable to the research, that it can be objectively measured, that it occurs in partnership with practice, and that it does not address adverse impact. The authors then problematize these assumptions in their discussion section, adopting a socio-technical systems framework (Figure 1). The paper starts with an analysis of the competing pressures faced by academics. The authors state that our field is an applied one at heart, and that it is nothing short of obvious that our research should address real-world problems. Nonetheless, they argue, specific pressures from higher education institutions (cf. “publish or perish”) create a context where real-world impact and academic merit are sometimes at odds with each other, isolating the academic community from the rest of society, and creating "rigorous but trivial (pedantic) science" (p. xx). In their discussion, they propose that academic departments should hire for complementary skills, perhaps even creating industry-facing research positions (and hiring people who can act as liaisons or brokers) in addition to 'strictly academic' profiles. This paper outlines four alternative approaches to impact-oriented academic research, organized along two dimensions (Figure 2): problem-driven vs. theory-driven researcher focus, and short-term vs. long-term impact timescale. It discusses the trade-offs, advantages, disadvantages, and risks of each approach (Table 1), and lists specific recommendations for how to do impactful academic research prior to, during, and after research projects in collaboration with practice (Table 2). We believe this paper can be of use to individual researchers, departments, higher education policy makers, and non-academic practitioners, in helping imagine and chart the different ways in which impact can be achieved, while still safeguarding the academic value of the research done. At the very least, the four approaches described offer a framework for acknowledging and appreciating all the different ways in which the work of academics can add value to society. The paper acknowledges that reliable and valid research designs and data are needed to avoid adverse impact—for instance, when research that lacks rigor creates an impact on practice, based on biased or methodologically flawed conclusions.

Empirical Papers: Single-Standing Studies and Research Projects

In the third article of this special issue, Kamal Birdi (2021) presents a thorough and thoughtful evaluation of a training intervention called CLEAR IDEAS which he designed to increase innovation in organizations. CLEAR IDEAS is rooted in organizational research on creativity and innovation, including much of Birdi's own at the University of Sheffield, UK. Birdi uses Kirkpatrick's four-level system for training evaluation to gauge the impact of CLEAR IDEAS via its use with public sector managers as part of a broader managerial qualification, though the model has been used in many other contexts too. He demonstrates that not only was the content and delivery of the CLEAR IDEAS model very positively received, it also in many cases led to change in behaviour in the trainee's workplace, and in some of those cases, to very notable organizational benefits. Importantly, Birdi identifies factors which militate for and against the successful implementation of learning from the training (Table 4). These provide a useful checklist for researcher who wish their research to be communicated and implemented successfully. However, some factors affecting the extent of use and benefit from the intervention are well outside the researcher's control, such as unanticipated changes in organizational priorities, which must surely be especially widespread during the Covid pandemic. Many work and organizational psychology researchers have had the experience of organizational priorities changing during or shortly after their work, so in evaluating the impact of research we should be mindful of the *potential* for impact as well as impact achieved. Birdi's work is also a reminder that for many academics, teaching is indeed a pathway to impact, especially if the attempted practical application of what has been taught is built into the assessment of learning. As long as that assessment majors on analysis and reflection rather than uncritical acceptance of the course material and a claim (real or otherwise) that it "worked", this is both an effective and an ethical route to increasing the probability of impact.

In the fourth article in this special issue, Tony Huzzard (2021) positions real-world impact as an objective that can be defined and planned for from the very beginning of a research project - in this case, a Horizon 2020 grant application which contained a work package dedicated to stakeholder engagement. The paper then describes the experiences and challenges faced in the implementation of this work package, concluding that ad-hoc stakeholder engagement is probably more feasible and realistic for academic researchers than systematic, continuous engagement. In contrast to typically lofty claims about collaborative research in prescriptive conceptual pieces, not only does this author speak from experience, but he also challenges the typical assumption that research dissemination is something to start thinking about after a project is completed. Although Huzzard believes that impact is a rather ambiguous concept, he settles on “the ambition that scientific endeavour concerns rather more than knowledge production for its own sake” (p. xx). The central concepts of the paper are stakeholder engagement and collaborative scholarship, which are positioned as necessary conditions for achieving impact. The paper describes many contextual elements that challenge stakeholder engagement in long-term research projects, one example being that in the German branch of the project they had to go through the unions first to reach the employees. We believe that this piece about the QuInnE project - which studied how job quality and innovation affect each other, and the effects of their interaction on job creation and quality - will help readers design better research projects and identify potential pitfalls and contingency strategies *before* they occur.

In the fifth article in this special issue, Rosalind Searle and Charis Rice (2021) focus on three activities that connect academic research to practice: dialogue, knowledge generation, and dissemination (Figure 1). They identify *trust* as the key variable that will influence the extent to which research will be taken up by practitioners. The authors draw from a study commissioned by the Professional Standards Authority (PSA) in the UK, in which they developed a taxonomy of misconduct among healthcare workers. Based on a literature review and a large dataset containing real-life incidents logged by the PSA, Searle and Rice identify different antecedents and processes that can help practitioners detect and prevent professional misconduct, which they organize under three headers: ‘bad apples’ (i.e., individual factors), ‘corrupted barrels’ (i.e., social learning), and ‘poor cellars’ (i.e., situated depletion). As in Huzzard’s paper (this issue), these authors make the point that creating an impact on practice requires creating predictive rather than retroactive insight. That is, rather than reflecting on and analyzing problems after they occur (and then ‘theorizing back’ to possible causes), impactful research helps practitioners *prevent* problems before they occur. This is important especially for high-stakes problems (like climate change or Covid-19)—and, in the case of this paper, the issue of misconduct in healthcare, a sector that deals with particularly vulnerable service users.

In the sixth article in this special issue, Drew Mallory (2021) offers a detailed and compelling account of an appreciative inquiry project aimed at enhancing diversity and inclusion in a large US research university, aimed at making the voices of its marginalized members heard and listened to. Appreciative inquiry has, for several decades, been a tradition seeking to bring together research and organizational transformation as part of a series of interventions, prompted not by a diagnosis of organizational problems but by a collaborative attempt to generate positive imagery. Growing from a social constructionist perspective that approaches organizations as products of human interactions, appreciative inquiry tries to engage different organizational actors in redefining organizational realities and bringing about positive change. Instead of focusing on dysfunctions, appreciative inquiry has sought to amplify an organization’s positive qualities and propensities for growth and development. The project described in the article started promisingly but came to an abrupt end, following the election of Donald J. Trump as US President. This political event supplanted the social agenda of social inclusion and diversity by a resurgent supremacist discourse across America’s institutions which seemed to undermine the project’s legitimacy and intimidated many of its participants. Mallory concludes that researchers and practitioners intent on delivering some impact are often “blinded by the optimism of inquiry” (p. xx) and can be liable to be “overwhelmed by super-organizational forces” (p. xx) which may contribute to the failure of interventions. His article and recommendations act as important reminders that the efficacy of organizational interventions is affected by wider social phenomena which 'impact' on the activities of researchers.

Empirical Papers: Cumulative Bodies of Work and Long-Standing Research Programs

In the seventh article in this special issue, Filip Lievens, Paul Sackett, and Charlene Zhang (2021) look back at a ten-year research program they ran about the use of situational judgement tests (SJTs) in high-stakes selection contexts. From their decades of combined experience and expertise, they picked the specific case of a medical school admission process for which they were asked to design a number of tests. The authors describe the initial design of the admission test, and how and why the SJT changed over the years as more data came in, but also as the priorities of administration evolved and changed (cf. validity improvement, cost reduction, candidate experience, diversity). This article is a good example of researchers clearly valuing (and excelling at) both academic rigor and practical relevance, as is clear from the style and approach of the article itself, as well. The authors seamlessly mix theoretical concepts with practical application, even offering a brief historical review of selection and assessment as a longstanding field of research. Another interesting aspect of this article is that it offers a highly quantifiable case of research impact, as the field of selection and assessment has a very strong psychometric focus (which is not necessarily true of other topic areas within work and organizational psychology). For instance, the authors were able to follow up the admitted cohort of medical students longitudinally, and could demonstrate that the SJT designed by them was able to predict physicians' job performance nine years later. The legitimacy of the authors as renowned researchers, and of the theory and methods within the field of selection and assessment (that has a 100-year old history) enabled them to treat the admissions program as a test-site for running field experiments and controlled variation, while simultaneously improving the real-life admissions process, thus creating measurable impact beyond academia.

In the eighth article in this special issue, Sharon Parker and Karina Jorritsma (2021) discuss the large body of work generated by the Centre for Transformative Work Design located in Australia. Figure 1 offers a comprehensive overview of antecedents of work design at the level of employee, leader, sociodemographic, and broader contextual influences—coupled to possible interventions at the individual, organizational, industry, and national level (Table 1). The authors then go on to describe how they have attempted (and often succeeded) at creating real-life impact at each of these different levels. Their article is probably the 'grandest in scope' of the special issue, and also aims to be actionable to its readers—linking to various training and workshop materials they have used in the past at the different 'levels' of impact. One might also say that the narrative of the article is aspirational. Sharon Parker's journey from PhD student to PI (principal investigator) of an ARC (Australian Research Council)-funded lab filled with talented PhD students and postdocs demonstrates that, indeed, a single research team can in fact make a difference for the field. Reflecting back to our earlier point about junior researchers losing sight of what research is 'for', one could argue that role modelling impact is at least as important as turning it into a new metric to strive for.

Critical papers

In the ninth article in this special issue, Marianna Fotaki (2021) explores the impact of academic research on mitigating work inequalities and, in particular, gender inequalities. Specifically, she assesses the impact of cumulative feminist scholarship over many decades, including some of its most abstract representatives. Instead of focusing on specific organizational interventions, Fotaki broadens considerably the scope and meaning of 'impact' by detailing how critical feminist scholarship seeps through innumerable social discourses and agendas. In this regard, Fotaki problematizes both 'knowledge' and 'impact', by arguing powerfully that, at least in management scholarship, both knowledge and impact are gendered concepts, i.e. they embody veiled assumptions that sustain and perpetuate different political structures. Unveiling these assumptions is already an indication of impact. Starting with the Economic and Social Research Council's (ESRC, 2019) distinction between instrumental ("influencing the development of policy, practice or service provision, shaping legislation, altering behaviour"), conceptual ("contributing to the understanding of policy issues, reframing debates") and capacity building ("through technical and personal skill development") aspects of impact, she demonstrates how feminist scholarship, even when not explicitly aimed at any of these aspects, has sparked a thorough reconceptualization of the meaning and effects of gender in organizations and society. While these have not always translated into policies, they have widely redefined political agendas. Of special interest is her argument that managerial and organizational conceptions of impact frequently inhibit real political impact by redefining social problems as problems of administration, an argument that goes back to the founders of critical theory – the emasculation of politics through its transformation into administration. In concluding she makes a strong plea for the coupling of theory with activism in producing tangible social impact for justice and equality.

In the tenth and final article in this special issue, Mats Alvesson (2021) - coming from the same critical theory tradition as Fotaki, and one of the founding figures of Critical Management Studies (CMS) - addresses a commonly raised criticism of this important theoretical current. The criticism is that, like its Frankfurt School predecessor, it is all talk and no action, or more damningly that it mistakes talk for action. Recognizing the risk of becoming a minor subtheme in academia and failing to reach wider audiences, Alvesson and his colleagues have been developing the concept of critical performativity as a means of emancipating performativity from narrow managerial interests. Critical performativity aims to deploy management theory and in particular CMS in pursuit of progressive agendas. Critical interventions in organizations are caring, pragmatic, engaged and normative and aim at alleviating some of the surplus suffering visited by today's organizations on their members, their customers and the wider public. Alvesson offers a telling example of an intervention which he made, initially through a series of lectures and subsequently through a publication in a mass circulation Swedish newspaper, in which he argued for a less routine- and rule-based approach in public administration. Along established critiques of bureaucracy, his intervention was meant to highlight that purely bureaucratic responses to organizational problems deepen the problems which they ostensibly seek to alleviate. The piling up of more rules and more routines is intended to defend against various anxieties of getting things wrong. However, ultimately the rules and routines exacerbate anxieties, both those concerning the problems that remain unresolved, and those prompted by the rules themselves. Alvesson's intervention was met with an unexpectedly strong and warm response by senior officials in public administration and triggered a series of measures aimed at addressing the dysfunctions that he had signalled. Like Fotaki (this issue), Alvesson's article showcases a type of academic activism, albeit not of an overtly political type. Instead, he advocates the development of effective communication techniques through which critical theorizing can reach wider audiences and filter through into policy and practice.

Reflections and Conclusions

Taken together, we believe that these ten papers offer many useful, and actionable, insights into the "real world" impact of research in work and organizational psychology. These include:

1. The specification of principles and guiding questions for researchers to address if they wish their research to make a difference, and the identification of what the implementation of those principles might look like during the research process.
2. The repeated observation that impact is not linear, where "pure" science is conducted and then implemented after its completion. Usable knowledge is co-produced by researcher and research users, and indeed those roles may on occasion blur into each other. This is

not a new insight in, for example, management research, but perhaps work and organizational psychology is yet to do much with it. Alternatively, after its heavily practical origins (Kwiatkowski et al., 2006), work and organizational psychology may have largely forgotten how to do it, despite some notable exceptions in this special issue. Indeed, it may still be happening now, but not getting published in academic journals.

3. Arising from the previous two points, the imperative for researchers to become, and remain, engaged with the contexts they are working in. This may well require a considerable commitment from the researcher, to engage at inconvenient or unpredictable times, and perhaps for more time than the researcher’s job description assumes.
4. The contribution of approaches that might be seen as “radical” or “unscientific” by many work and organizational psychologists is considerable. By examining and exposing power relationships and discourses in work and society, feminist and critical management approaches can bring much insight into what is going on and to whose benefit. Arguably, and perhaps paradoxically, they may resonate with lived experience much more than the psychology most readers of this journal are accustomed to.
5. The power of organizational and societal factors in assisting or inhibiting the impact of research is clear. Whilst work and organizational psychology researchers can build in ways of guarding against this (e.g. working within existing organizational processes and culture as much as practically and ethically possible), often it is outside the researcher’s control.
6. The importance of the initial credibility of the researcher to the potential research users, and the subsequent building and maintaining of trust, is clear. Research users turn to eminent and well-known researchers for advice. Eminent and well-known can refer to (i) a local reputation (where local can mean local to a geographical area, organization or

industry), (ii) reputation within the academic discipline/profession, (iii) reputation in a specific network of research users or (iv) prominence in media, including social media.

We believe the insights offered by this special issue will be valuable in enabling researchers who care about impact to achieve it more, and researchers who have hitherto not been particularly interested in impact to become more so. Nevertheless, some important issues have not featured heavily. One is the potential for negative impact. Reporting of impact is normally framed positively (e.g. saving a million Euros) when sometimes it could equally have been framed negatively (e.g. reducing the number of jobs available in the organization by 30). Several of our ten papers consider ethical issues in impact, but often there may be no right answer as to whether an impact, or lack of it, is a good or a bad thing. It may depend on the value frame one adopts. Despite this, there may be near consensus amongst work and organizational psychologists about the desirability of some things, such as diversity and inclusion. After all, “adverse impact” has been a preoccupation of selection research for a very long time.

Second, our papers do not say much about the academic systems and reward structures which arguably work against the achievement of impact. At the start of their journey, many doctoral researchers avow a primary aim of changing something for the better through their work. Two and a half years later, their main concern is achieving publications in “good” journals and getting an academic job – if, that is, they still want one. As far as we can see, academic promotions for staff with research in their employment contract still depend primarily on publications in leading journals, with obtaining funding somewhat behind, and teaching and research impact even further behind. This is of course a generalization, but one we feel confident in making. So research impact is more for the good of the university than the academic. As such, it becomes an extra task, to be done in the "spare" time that rarely materialises, and/or by people who are no longer seeking promotion or tenure, or indeed to retain a high reputation amongst their academic peers. It is clear from the papers in this special issue that achieving impact usually takes time and trouble, so the opportunity cost is high. Some academics with the right personal circumstances and personal goals may be content to be impact specialists, and perhaps universities will tolerate or even quietly support a limited number of such people. But this does not prevent impact being a career cul de sac.

An obvious way forward is for employers of academics to reward research impact more fully via promotion and tenure criteria. However, this would likely require a collective will – universities may feel it is too risky to be a first mover in that respect. An alternative model would be for leading journals in and around work and organizational psychology to encourage and accept different kinds of submission, most notably ones which report on attempts to apply existing or develop new theory in the context of trying to make a difference in the real world. This would, we hope, put an end to the obligatory and normally speculative and weak sub-section near the end of most papers headed "practical implications". Instead, the whole paper would be framed around practical implications and the effects (or lack of them) of the research. As a counterbalance, it must be acknowledged that this special issue includes several examples of academics who, via research by them and/or their teams, have managed to combine publications in prestigious journals with making a difference. We know of others too, but we think these people and the circumstances that assist them are relatively rare. Nevertheless, they exist, and learning from them could be a good first step.

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