The Neglect of the Political: An Alternative Evidence-Based Practice for I–O Psychology

DEAN BARTLETT
London Metropolitan University

This commentary argues that the treatment of evidence-based practice offered in the focal article by Briner and Rousseau (2011) hampers the project of developing an evidence-based industrial–organizational (I–O) psychology by neglecting political aspects of organizational life. I aim to identify and challenge a bias against the political that permeates many of the arguments and analyses put forward in the article in ways that are not entirely obvious but are detrimental to its stated central objective “to help move I–O psychology in an evidence-based direction.” Finally, I propose an alternative version of evidence-based practice for I–O psychology, one that is able to accommodate the phenomenon of organizational politics and that, in doing so, has the potential to enhance practice while remaining true to evidence-based principles.

A Neglect of the Political

Given that the focal article acknowledges that “power and politics are fundamental to decision making and also surround the identification and use of evidence in organizations,” it is surprising that it devotes only a single paragraph to the topic. Furthermore, the sparse treatment of power and politics that it does offer is inadequate in several respects. First, it is difficult to identify a clear line of argument about what the article does say regarding the role of power and politics. It suggests that senior leaders may feel that they should use their own subjective expertise (as opposed to evidence) in making decisions and that such individuals (who, along with other organizational actors, inevitably hold vested interests) will find it difficult to hide such interests under an evidence-based regime. Therefore, presumably, they will resist any move toward evidence-based practice, and the focal article does indeed acknowledge that an evidence-based approach will prove challenging (and, one could therefore surmise, be difficult to implement) in organizations with highly political cultures (for which I would read organizations in general as I have yet to come across an organization that is not highly political). What is puzzling is that, given the centrality and strength of the influence attributed to power and politics in the evidence-based project, the article then appears to go on to suggest, albeit implicitly, that they may be best ignored or at least deemphasized.

The article states that “although it is impossible to remove politics from evidence and decision making” and then goes on to argue that by making decision makers more accountable, such agents of organizational power are likely to welcome the distinction...
between research evidence and politics. A more plausible counter argument could be that the often “inconvenient truths” that evidence-based practice can bring into the spotlight are unlikely to be welcomed by the powerful elite within an organization. The scant regard given to politics and the language used are both suggestive of the idea that if it were possible to remove politics from evidence and decision making then it would be desirable to do so. Not only does this run contrary to the notion of evidence-based practice as a pragmatic science (i.e., as I–O psychologists, we have no choice but to pragmatically accept the existence of such politics), but the desirability of neglecting the role of organizational politics is also questionable. If they really are important influences, then we should be incorporating them into our analyses. Indeed, if they do exert as powerful an influence as the focal article suggests, then we should perhaps go even further and afford them a special significance in our analytical rubric. This would seem a desirable avenue to explore not only in developing a comprehensive evidence-based practice for I–O psychologists to use in their work but also in terms of the successful implementation of the evidence-based project as a whole—a distinction that I found somewhat blurred in the focal article itself. It is only by embracing organizational power and politics and using them as a key analytical dimension in our research and practice that the stated aim of the article can be achieved.

The relative neglect of politics in the particular formulation of evidence-based practice put forward in the article can be related to previously published critiques of the dominance of positivism within I–O psychology more generally and the attendant calls for a more critical approach, particularly from within the British and European traditions. Such accounts portray the “scientific evidence” to which the article refers as a “totalising meta-narrative which erases plurality through discursive closure” (Johnson & Cassell, 2001, p. 137). The focal article states that evidence-based practice involves different types of evidence from various sources; however, it is also clear that the emphasis is very much biased toward scientific evidence, not least by the explicit admission that the article concentrates on only one of the four sources of information it identifies as essential to evidence-based practice: critically evaluated research evidence.

Given the inherently social and deeply political nature of organizations, it could be argued that the wrong meta-narrative (i.e., a “scientific” one) is already being privileged, particularly from the academic side of the scientist–practitioner divide. A call for an immediate injection of the version of evidence-based practice proposed would, therefore, only exacerbate the problem even further. As the discipline of psychology increasingly values the insights that alternative interpretive paradigms can offer, it could perhaps be a backward step to adopt a model of evidence-based practice that privileges scientific discourse over and above other types of evidence (in particular, such evidence that relates to the pervasive influence of power and politics in organizations). This is particularly so given that evidence-based practice does, at least in principle, have the scope to incorporate such critical and alternative forms of evidence, although as I will argue below, the precise instantiation of evidence-based practice put forward in the focal article may perhaps not be best suited to the task.

**Alternative Models of Evidence-Based Practice**

Although drawing on previously published literature that bemoans the science–practice gap and that characterizes academic research as increasingly “pedantic” and practice as increasingly “popularist,” the focal article fails to acknowledge that some of the authors of that literature envisage a rather different modus operandi and corresponding professional identity for the I–O psychologist than that proposed. Hodgkinson (2006), for example, suggests...
that our unique selling point lies in one particular embodiment of evidence-based practice (the scientist–practitioner model). This is different from the quadripartite formulation offered in the focal article as it encapsulates a broader conceptualization of the underlying scientific basis of practice. This reading of practice emphasizes the ability of practitioners “to critically evaluate the impact of their interventions and adjust their actions accordingly” (Hodgkinson, 2006, p. 174), rather than emphasizing the use of scientific findings to inform professional practice.

Aside from the varieties of evidence-based practice already mentioned, there are yet further alternative versions that we could pursue. For example, we might consider privileging the political over and above the scientific, although not going so far as to exclude the scientific. Such an exercise could suggest a somewhat different operationalization of evidence-based practice than that offered in the focal article. Instead of dividing the sources of relevant information up into four quadrants as suggested, this could imply an alternative tripartite model that combines two of the quadrants (“evidence from the local context” and the “perspectives of those who may be affected by intervention decisions”) into a single dimension that might be labelled “proximal information.” We could propose that this dimension would then include both hard and soft contextual data but would extend its range of convenience by adding a more politically sensitive set of questions to those suggested in Table 1 of the focal article. Such questions are myriad but could include, for example: Whose interests am I serving in this situation? Whose voice is being silenced? What assumptions underlie the rationale for the intervention and can they be challenged? What categories are being used to describe and analyse the situation and are they conceptually sound? It is true that some of these questions could be incorporated into the “perspectives” quadrant of the framework offered. However, I would suggest that the critical and political flavor of these additional questions offers analytical insight and explanatory power over and above the questions posed in Table 1 of the focal article. Take, for example, the binary category of manager/employee that can be found there—and which breaks down when we consider that managers are also employees. This observation opens up a whole new line of enquiry surrounding the psychology of these organizational actors—a line of questioning that is otherwise closed down but that is, nonetheless, the very essence of what I–O psychology is (or should be) about.

The availability of different varieties of evidence-based practice and the difference in usage and understanding across and even within professions raises doubts about the idea of a universal model on which the logic of the focal article appears to depend. If each community of practice requires its own particular version of evidence-based practice, then the development of a generic framework that is then compared to I–O psychology makes little sense. Following on from this, that I–O psychology is found wanting when such a comparison is made could also be challenged. In the acknowledged absence of empirical data about what practitioners actually do, it could be argued that I–O psychology is already firmly evidence based (in its own particular way), and the suggestion made in the article that evidence-based practice is not well known or understood within the field of I–O psychology would therefore be misplaced. If this is the case, then to attempt to move toward some sort of idealized generic version of the approach that has no empirical basis or that is not enacted by practitioners would be akin to fixing something that we do not yet know is broken and, even if it were, for which we do not have an appropriate spare part to fix rather we must rely on borrowing one from outside our discipline.

The focal article acknowledges that I–O psychologists are often not the decision makers but are instead drafted in as technical experts to implement decisions arrived at by managers in a nonevidence-based
way, based on short-term goals and vested interests. It suggests that “perhaps the only way to overcome” the problem of a lack of interest in evidence-based practice is, essentially, by appeal to reason through trying to persuade managers of the benefits of the approach. A more political analysis might suggest that managers already know about the benefits of the approach and so don’t need persuading. Rather than showing a lack of interest, managers may choose to actively resist an evidence-based approach for the very reasons outlined in the focal article (i.e., it would reveal their politicking, which would be unacceptable for them and to other stakeholders whose interests would be different). It is only by appeal to the political that we are able to identify, understand, and overcome this resistance. Consequently, the move toward an evidence-based approach will not be helped by adopting a reading of that approach that privileges a rational-scientific discourse to the neglect of the political—we need both.

Problems With the Proposed Remedies

Finally, I would like to turn to the suggested remedies in the focal article, which are thrown into question by the critique offered here. With respect to an increased use of systematic review, it makes little sense to invest considerable resources in reviewing evidence that is generally regarded as irrelevant to practice and that, even if it were not, is likely to be ignored or rejected by those who wield power within organizations due to their desire to avoid the legitimacy of their power being challenged. In addition, it is also worth noting here the research evidence concerning the tendency of individuals to ignore and misinterpret scientific information that is inconsistent with their own point of view (e.g., Munro, 2010).

In relation to the call for more practice-oriented research, the problem is that, through their neglect of the political, academics have not equipped either themselves or practitioners with a suitable set of constructs to enable them to analyze the political influences at work in any given situation. They are therefore unable to operationalize such influences in ways that would be conducive to making them open to research through, for example, the formulation of appropriate (practice-oriented) research questions or data collection instruments. Instead, although practitioners are acutely aware of the importance of organizational politics, they are forced to rely on their own experience and subjective readings of situations in dealing with them because their training leaves them ill-equipped to carry out a more sophisticated, objective, and evidence-based analysis.

It is this final issue that lies at the heart of the matter. The problem has been cast in terms of a knowledge-production problem or a knowledge-transfer problem. The focal article suggests potential remedies from each perspective—academics should do more practice-oriented research and practitioners should “do” more systematic reviews (the ambiguity over exactly what this means is intentional because I was a little unclear as to whether the suggestion was that practitioners are viewed as end users of systematic reviews or as producers or coproducers of such reviews). Other writers have offered similar solutions, even though they have chosen to label them differently (e.g., “engaged scholarship” or “bridging scholarship”); however, the critique presented here suggests an alternative role for the I–O psychology scholar in bridging the science–practice gap: constructing a framework for evidence-based practice that accounts more systematically for the important influence of power and politics in organizational life rather than relegating them to the personal judgement, hunch, or impression of the practitioner.

A Cautionary Note

I conclude my commentary with a cautionary note. In the interests of stimulating
The neglect of the political

debate, I have purposely aimed to challenge, to be controversial, and even to have a little fun along the way. However, we must also be careful not to throw out the baby with the bathwater. The critique that I offer is not a fatal one for the evidence-based project. It is perfectly feasible that due consideration of the missing political dimension within an evidence-based framework could overcome the problems highlighted. I have suggested an alternative operationalization of evidence-based practice, but in doing so, I am not suggesting that such critical and political analyses are necessarily better or more useful in and of themselves. Rather, I would argue that a model of evidence-based practice where alternative and critical voices are heard and where the growing body of rigorously scientific evidence about how power influences behavior (e.g., Lammers, Stapel, & Galijnsky, 2010) is incorporated will only serve to strengthen our evidence-based analyses and thereby our practice. Indeed, to not incorporate such evidence runs counter to the very principle of an evidence-based approach even on its own terms.

References


