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RESPONSE

I–O Psychology and Intelligence: A Starting Point Established

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Abstract

The goal of our focal article was to initiate a conversation on how I–O psychology can reengage in research on intelligence. We are encouraged by the ways in which the commentators have furthered this conversation. We discuss 4 overarching themes in the set of commentaries: the extent that I–O psychology is contributing to the intelligence field, outsiders' impressions of I–O psychology's contributions to intelligence research, ways I–O psychology can contribute to intelligence research, and ideas about what prevents us from doing this research. We hope this collection of articles serves to truly ignite research by our field on this critical construct.

There were two primary objectives of our focal article. The first goal was to spark a discussion among I–O psychologists regarding the need to reestablish a comprehensive program of research on intelligence. The second goal was to begin to create a research agenda for studying this construct in our field. After reading the various commentaries, we believe that these objectives have been achieved. With perhaps one exception (i.e., Cucina, Gast, & Su, 2012), the commentaries appear to agree that intelligence deserves to be a ''hotter'' and more prominent topic of inquiry within our field than it is currently. Even if the groups differ a bit about exactly how dire the situation is, most of the commentaries tend to agree that more research should be carried out on this important construct (e.g., Postlethwaite, Giluk, & Schmidt, 2012). In fact, many of the commentaries cite new and exciting theoretical and empirical research being conducted on intelligence that currently has not been focused on in our field but could arguably serve as viable starting points for studying intelligence in applied settings across the subfields of I–O psychology.

For example, Lang and Bliese (2012) discuss the utility of the nested-factor conceptualization of intelligence to account for the oft-cited positive manifold among ability measures. This new conceptualization of intelligence is an area that should be examined by our field, given that it leads to different predictions than the classic Spearman (1927) model currently embraced by our field—in that the relative importance of *g* would be reduced, compared with specific cognitive abilities, when it comes to predicting job performance. Huffcutt, Goebl, and Culbertson (2012) suggest that executive functioning, a construct discussed and empirically studied in the scientific literature since the 1930s,

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should be explored in the applied literature. Weinhardt and Vancouver (2012) suggest that we should stop interpreting research demonstrating a heredity–intelligence relationship as if it shows that the environment has no effect on intelligence. These authors propose a series of quasi-experimental studies to examine the efficacy of various environmental interventions. Both Ackerman and Beier (2012) and Brouwers and Van de Vijver (2012) stress that I–O psychologists need to play to their key strengths and develop more contextualized perspectives of intelligence. Ackerman and Beier espouse the utility of ''Brunswik symmetry,'' a concept similar to ''psychological fidelity,'' for enhancing predictive validity. Finally, Helms (2012) called upon our field to draw upon the climate and culture literatures to develop a conceptually rich understanding of environmental differences to help interpret the dynamics of race differences on intelligence in work settings. Clearly, as we had hoped, these commentaries provide support that there are more questions that need answering regarding the construct of intelligence than one would have deduced by looking at our literature.

Before discussing the four overarching themes that we identified among the commentaries, we need to clarify a couple of issues that apparently caused confusion in our focal article. In particular, Lievens and Reeve (2012) indicated that we equated *g* with intelligence. We apologize if any reader interpreted our focal article in this manner. As pointed out by Ackerman and Beier (2012), this misinterpretation was probably caused by the title of the focal article and the first line of the abstract. Our actual intent was to encourage our field to go beyond its current conceptualization of *g* and intelligence as equivalent. We hoped to begin pushing our field to leverage the current scientific literature on intelligence to generate creative, applied, and basic research to be published in I–O psychology research outlets. Moreover, in our encouragement of the field to go beyond *g*, we were not focusing on more controversial notions of this such as Gardner's (1983) theory of multiple intelligences or new constructs such as Mayer and Salovey's (1997) emotional intelligence. We were instead focused on the notion of intelligence that centers on processes such as reasoning, drawing inferences, and information processing. Although these new conceptualizations of intelligence (e.g., multiple intelligence theory) may show promise, our article was focused on asking our field to go beyond *g* to more fully capture the domain of intelligence.

Lievens and Reeve (2012) also indicated that we wanted to limit future research to traditional measures and theories of intelligence. We were surprised by this assertion and that was certainly not our intent. A central premise of our article is that new theories of intelligence should be considered, and new measures of intelligence should be developed and tested in our field just as other areas of psychology have been doing for some time (e.g., education and clinical psychology). Thus, when Lievens and Reeve state that new alternative measures of intelligence have emerged and should be empirically tested, we couldn't agree more.

To be clear, we endorse the multimethod, multi-trait approach to construct validity, and we recognize that there are many different ways to measure a single construct. Our point was that measures of intelligence need to be based on sound theory and that a wider array of theories is needed (more than just *g*). For instance, Carroll's (1993) theory of intelligence could be used to create measures as could the nested-theory approach discussed by Lang and Bliese (2012). What we find to be unfortunate and unacceptable is that the tests commonly used in our field do not reflect a sound theoretical foundation and are often narrow, deficient, and limited in format.

We are calling for our field to break this status quo and to change its current practice. In her commentary, Helms (2012) also discussed I–O psychology's role in maintaining this status quo. Our focal article expresses the need to move forward as

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a field through modern and up-to-date research on intelligence so that we progress toward a new paradigm regarding this important construct. However, we think it is important to note that we are not calling for the wholesale abandonment of 100 years of research and statistical methods as implied in a few of the commentaries. Rather, we believe that it is necessary to build on what is good and empirically validated and to move beyond what is not.

To What Extent Is I–O Psychology Contributing to the Intelligence Literature?

One disagreement between the focal article and several of the commentaries was the extent to which I–O psychology is contributing to the intelligence research literature. In our article, we stated that I–O psychology, as a field, had abandoned the study of intelligence. Specifically, after a cursory review, we estimated that only 2–3% of articles published in the *Journal of Applied Psychology* (*JAP*) and *Personnel Psychology* (*PP*) were related to intelligence. Postlethwaite et al. (2012) disagreed with this estimate, and they estimated that 4.6% of the *JAP* articles and 8% of the *PP* articles focused on intelligence. Even if these larger estimates are used, we believe that our initial argument is still valid. Moreover, Cascio and Aguinis (2008) found that from 1963 to 2007 only 12.57% of the articles in *JAP* and only 19.57% in *PP* focus on predictors of performance, which include many heavily researched constructs other than intelligence, such as personality. Taken together, there really isn't evidence in the I–O journals of a vibrant intelligence research agenda. Thus, although we might quibble over how dire the situation is, there seems to be agreement that relatively little research is being published in our journals regarding this key construct.

Postlethwaite et al. (2012) make an excellent point that I–O psychologists might be publishing their research on intelligence in non-I–O journals. Although this is promising and suggests that I–O psychologists might not have abandoned this area of inquiry, it leads to a second and perhaps more important question that we should ask as an entire field: Why are these researchers publishing these articles outside of I–O? Are articles being submitted to I–O journals only to be rejected and, thus, eventually being published outside of the mainstream of our field? And if so, why is this the case? We strongly doubt that a lack of high-quality research being submitted to these journals is the reason. Both we and the majority of the commentaries clearly agree that intelligence should be a hotter topic than it currently is in the I–O journals. What does the limited publication of this type of research say about our field and its contribution toward understanding one of our most important constructs?

What Are Outsiders' Impressions of I–O Psychology's Contributions to Intelligence Research?

If the construct of intelligence is a hot topic in other areas of psychology, then what is the consequence of not having I–O psychology recognized as contributing to this literature? What is the consequence of I–O psychologists just publishing in non-I–O journals? We believe that one consequence is that the I–O literature becomes less relevant to the vast majority of the field of psychology. We believe that our field ultimately risks becoming viewed as a second-rate science whose only purpose is to take well-established findings and test their generalizability to new populations. We are concerned that the field is already well on its way to being viewed in this way. In recent and highly visible reviews of the intelligence field, the contributions of I–O psychologists to intelligence research are either not mentioned (e.g., Deary, 2012) or only raised in the context of validity generalization and score differences between groups (e.g., Nisbett et al., 2012). Even in reviews of personnel selection, there is no mention of I–O psychologists making contributions to

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understanding the construct of intelligence (e.g., Sackett & Lievens, 2008). As is clear from these reviews and several of the commentaries, I–O psychology as a field is not continuing to make substantial and sustained contributions to the construct of intelligence.

We agree with Oswald and Hough (2012) and Postlethwaite et al. (2012) that there are times when conducting translational science is positive and appropriate. For example, we agree that I–O psychologists should not use half-baked ideas in high-stakes testing situations. For instance, Oswald and Hough recommended that our field use theories that have withstood research tests. They note as stated in their title that we should be ''staying (just) behind the cutting edge of intelligence theories'' (i.e., intelligence ''1.5''). Although we don't completely agree, we can see some merit in their argument. However, the bigger problem is that reality does not at all reflect our field as being just behind the cutting edge. Our field is more like intelligence ''0.50'' than intelligence ''1.5.'' If we were staying just behind the cutting edge of intelligence theories, we would be thrilled. However, we currently are nowhere close to that edge and don't see the field reaching it any time soon. The commentaries from intelligence researchers, in particular, make it clear how far behind we are as a field. As noted by Huffcutt et al. (2012), executive functioning has been discussed and measured since the 1930s. It seems time to consider such a construct as something more than ''half-baked,'' take it out of the oven, and at least consider it for use within our field.

This should not be taken to imply that there are no I–O psychologists doing cutting-edge intelligence research. Some I–O psychologists, including a number of the commentators, are making contributions. However, as pointed out by Postlethwaite et al. (2012), these I–O psychologists are publishing most of this work in non-I–O journals. We are focused on our field, as a whole, and our practice, as well as the impression that our practice creates for the rest of the psychological community about the contribution of I–O psychology to intelligence research. From the commentaries, we believe that there are a couple of problematic impressions that currently exist about the state of intelligence research in I–O psychology. We are viewed as the protector of the status-quo and not open to change (Helms, 2012); not connected to context of work, which we seek to predict (Brouwers and Van de Vijver, 2012); and outdated (Ackerman & Beier, 2012; Lang & Bliese, 2012; Lievens & Reeve, 2012). To the degree that these impressions are true (and we believe they are), it is a problem for the field and should serve as a call to action to reinvigorate research on intelligence in I–O psychology.

Furthermore, we think the extent that I–O psychology is behind and outdated affects applied practice in the field when it comes to using intelligence tests in the workplace. The original intelligence theorists (e.g., Spearman, 1927) made a strong case that a wide range of different tests and formats is needed if one is to assess intelligence. A number of intelligence researchers (Alfonso, Flanagan, & Radwan, 2005; Chen & Gardner, 2005) have argued that many conventional tests only capture narrow aspects of the construct (e.g., linguistic and quantitative) using limited formats (e.g., written form and multiple choice). If one were to examine the typical intelligence tests used in our field, it would lead to a serious reconsideration of whether we have a sound understanding of this construct and how it is measured. Many of these tests consist of no more than a few verbal and quantitative scales. Thus, what passes for an intelligence test in the practice of I–O psychology may not meet the basic theoretical requirements for measuring intelligence. One cannot infer intelligence from a test consisting of a few verbal and quantitative scales. The fact that some in our field do not see the problem in making these inferential leaps further underscores how far behind we have fallen when it comes to truly understanding this complex and important construct.

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How Can I–O Psychology Contribute to Intelligence Research?

We believe that I–O psychology can take many different directions to contribute. In our focal article, we laid out some directions and our commentators have pinpointed quite a few more. It is our position that there is not one particular direction that should be taken and that all those offered in this exchange have value. It is not helpful for the science of I–O psychology to dismiss any topic at this point, including measurement and new models. As Ackerman and Beier (2012) suggest, some research will succeed and some will fail, but that is what is required for the field to move forward, and we should not shy away from the risks of failure. Although our commentators mention many different directions for research, we noticed two overarching possibilities that emerged in the commentaries: (a) contextualized intelligence research and (b) exploring the role of specific cognitive factors.

Contextualized Intelligence Research

As noted by Brouwers and Van de Vijver (2012) and Helms (2012), research on intelligence should adopt a contextualized view (as opposed to the decontextualized view that currently exists). That is, research should focus on how intelligence is manifested in the workplace, how the context influences the manifestation, and how the context is embedded in the measurement of intelligence. As both Brouwers and Van de Vijver (2012) and Helms (2012) argue, this approach would require a careful analysis of work environments and job requirements. These are tasks that I–O psychologists are well prepared to execute given our strength in conducting job analysis and understanding work climates and organizational cultures. Lievens and Reeve (2012) point to a number of specific directions that this type of research could take especially from a measurement point of view, including using simulations to capture cognitive abilities. As Weinhardt and Vancouver (2012) argue, a focus on the context creates the possibility of designing interventions that allow for a greater manifestation of intelligence at work.

Specific Cognitive Factors

Similar to what we argued in the focal article, most of the commentators discuss the need for I–O psychology to return to a focus on more specific cognitive factors (Ackerman & Beier, 2012; Lang & Bliese, 2012; Lievens & Reeve, 2012; Oswald & Hough, 2012). A number of the commentators argue that there is a role for both broad and specific factors and that research should focus on how the use of specific factors can improve our understanding of intelligent behavior as well as our prediction of it. Oswald and Hough suggest the specific factors of crystallized and fluid intelligence as independent cognitive factors that could be useful for understanding intelligent behavior at work. Huffcutt et al. (2012) suggest that research on executive functioning is very promising and provides a path that other areas of psychology have traveled with much success. Ackerman and Beier suggest the concept of Brunswik symmetry as a principle that can guide efforts focused on how more specific cognitive factors can enhance prediction beyond the general factor. Lang and Bliese offer the nested-factor theory as an alternative theoretical framework for understanding the role of more specific cognitive factors. We believe that all these recommendations represent great starting points for reviving research on intelligence in I–O psychology.

What Is Keeping Us From Getting There?

From the focal article and the commentaries, it is clear that research on intelligence is both needed and possible. The question is what is preventing us from doing it? Although we identified a number of barriers in the focal article, the commentaries highlight the two greatest obstacles: (a) the ''mission accomplished'' mentality

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and (b) an emphasis on prediction and application at the expense of basic research.

As we noted in the focal article, the field seems to have embraced a ''mission accomplished'' mentality. We found the differences in beliefs about the state of research between the I–O psychologists and the intelligence researchers striking. For example, Cucina et al. (2012) argue that support for the existing theories is vast and attempts to create new models of intelligence will not be fruitful. This contrasts to the intelligence researchers' commentaries (e.g., Ackerman & Beier, 2012; Brouwers & Van de Vijer, 2012), as well as the I–O psychologists who are focused on intelligence research (e.g., Lang & Bliese, 2012; Lievens & Reeve, 2012), who take the position that new approaches and measurement will be fruitful and should be pursued. Moreover, the recent reviews of the intelligence field make it clear that there are still debates about the support of the existing theories and need for measurement-focused research (e.g., Deary, 2012; Hunt, 2011; Nisbett et al., 2012). What motivated the focal article is our belief (apparently shared by many of the commentators) that the position of Cucina et al.—that research should not be directed at new approaches and methods—is representative of a large part of the field. It is our hope that the focal article and commentaries help counteract this mentality that is out of sync with the current state of intelligence research and push the field to once again make contributions toward understanding one of our most important constructs.

We agree with many of the commentators that I–O psychologists need to become more current on intelligence research and theory and that we need to more critically review and evaluate the current state of the intelligence field. We believe that this would lead to a less certain mentality about our knowledge and one that is more encouraging of basic and applied research. To be clear, we do not believe that the field should abandon what has been done in the past. There is still a lot to learn from past work and much that has been overlooked due to the excessive focus on psychometric *g* (Ackerman & Beier, 2012). We believe that it is time to build on the past and explore new areas. Although many other fields of psychology including those with the same concerns about application (e.g., education and clinical), have taken up this challenge, the field of I–O has not. There is no question that some in our field are doing this work, but it is not being published in the journals that academic and practicing I–O psychologists tend to read. It is time that the field as a whole refocuses on expanding our knowledge and exploring new ideas when it comes to intelligence. It is clearly needed and possible to achieve, as demonstrated by our commentators.

In addition to a ''mission accomplished'' mentality, there is an overemphasis on application and prediction. Intelligence, even in I–O, is not just about prediction and applied research. Although prediction is important for an applied field, it must be balanced by research focused on description and explanation of the underlying construct. We believe that both basic and applied research is needed. They are not mutually exclusive, and conducting basic research does not come at the expense of applied research. However, the reverse is not true. The emphasis on applied and translational research has come at the expense of basic research in I–O psychology. We cannot see how the field could build on existing research and theory or pursue new research areas without a more solid foundation of research that explores the nature of the intelligence construct and develops theory-based measures of it. Intelligence is too important to our field for us to simply wait for others to do research that we can translate into practice and apply in the workplace.

Concluding Thoughts on Intelligence 2.0

The goal of our focal article was to initiate a conversation on how I–O psychology can reengage in research on intelligence. We

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believe that the commentators have helped further this conversation with thoughtful directions for future research and insights on the state of intelligence research in I–O psychology. We agree with the majority of the commentators that I–O psychology needs to engage in more and different research on the intelligence construct and that such research is both possible and likely to be fruitful. It is time for us to stop being victims of our own success (Murphy, 1996) and step out of our comfort zone (Ackerman & Beier, 2012; Brouwers & Van de Vijer, 2012). We cannot move the field forward by discouraging research or limiting what can or should be done. It is time to try new research directions and encourage these efforts. We hope this collection of articles serves to truly ignite research by our field on this critical construct.

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