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Response to Baruch: We Weren't Seeking Canonization, Just a Hearing

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We respond to the points raised by Baruch in his critique of our introduction. We believe the critique is helpful because it directs our attention to some important questions that need addressing when applying ideas from one branch of science to another. We argue that there is value in looking elsewhere for ideas, provided that it is done carefully and with rigour.

SCHOLARSHIP IS CONVERSATION

Huff's (1999) dictum, that scholarship is conversation, nicely frames Baruch's commentary (2002, this issue) on our introduction (Gunz, Bird, and Arthur, 2002, this issue) and this response to Baruch. When a group has been working with an idea for as long as we have on this, it's always good to have a quizzical eye cast over the project and to have to answer the question: was it worth doing? As we hint at the beginning of our introduction, this thought has never been far from us, either, and it needs answering.

The question is particularly germane to the present exercise because, not to put too fine a point on it, "new science" (we still dislike the term) has attracted more than a few cranks. Some of the ideas that have emerged from the physical sciences over the past century are so far away from everyday experience that they seem quite bizarre. These ideas include, for example, objects shrinking along one dimension and gaining mass as they go faster with respect to an observer as predicted by Einstein's Special Theory of Relativity, or electrons "tunneling" through what ought to be impervious barriers (the so-called "tunneling effect," which is now being put to use in the scanning tunneling electron microscope and certain designs of transistor).

It is little surprise, then, that mystical and bizarre connections occasionally get made and enthusiastically promulgated by people who misunderstand the theory underpinning these odd phenomena, and whose contributions puzzle and irritate people who don't. Words get

into the discourse and are used indiscriminately without any regard to their original meaning: “nonlinearity” and “entropy” come to mind as prime examples of overworked terms which have become detached in the minds of many from the concepts whence they sprang. That’s not to say that these ideas don’t have profound existential implications, of course. For example, the second law of thermodynamics, which states that the entropy of the universe tends to a maximum—i.e., that the universe will eventually run down—gets people thinking about fundamental issues as few other physical laws do. But the first trap that awaits anyone trying to make the kind of connections we are attempting here is, quite simply, have we misunderstood the ideas we are drawing on so badly that our whole argument is nonsense?

This possibility has worried us from the beginning, and although nothing has emerged so far which suggests that we have fallen into the trap it is still there. However, it is not the nub of Baruch’s argument. He has five basic points, which deserve addressing. They are: 1/ introducing ideas from non-social sciences is premature given the state of theoretical development of the careers field; 2/ the physical sciences are just too different from the social sciences to be useful; 3/ the world of careers is changing so rapidly at the moment that it is inappropriate to attempt theoretical synthesis; 4/ it would have been better to introduce the ideas into the behavioural sciences generally before descending to the careers particularly; and 5/ the project dialogue does not involve physical scientists whose intellectual property the ideas are. We address each of these criticisms in turn.

WHY BUILD AN ELABORATE, HIGH-TECHNOLOGY SUPERSTRUCTURE WHEN THE FOUNDATIONS ARE INCOMPLETE?

Baruch uses a nice metaphor to describe our venture, accusing us of trying to add a glass and titanium top to a building with inadequate foundations. He points out that we acknowledge that the careers field is still in a state of theoretical turmoil—or, more precisely, that it lacks any real integration. Careers, he argues, are so complex that nobody has come up with a unifying theory. So haven’t we got our priorities wrong? Surely we should be concentrating on making the best of what we already have before we search elsewhere for ideas? Implicitly, he is making the point that our project is a distraction from more serious scholarly activity. He gives the example of the psychological contract, a robust concept making something of a comeback recently which still has plenty of life left in it. Until we have really exploited its possibilities to the fullest, and similarly for other ideas from the social sciences (he specifically identifies socio-psychology, presumably on the basis of his assertion that «First and foremost, the career is individual “property”» [p. 16]), we should stick to our knitting.

As is evident from our Introduction, we agree that the careers field lacks integration. But we fundamentally reject Baruch’s conclusion from this: we see no reason why we should not explore other fields for

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help. To borrow his metaphor, we see our project not as building elegant superstructures on buildings with inadequate foundations, but as looking for alternative foundation-construction technologies. We see that the present foundations appear unable to cope with either the nature of the ground we are building on, or the complex architecture they are intended to support.

As we imply in our opening comments here, we do not believe that foggy thinking is acceptable in ventures such as this. There is, it seems to us, a particular obligation on anyone trying to introduce unconventional ideas to a field to make sure that they are doing so with precision and clarity; naïve but uninformed enthusiasm is both inadequate and misleading. But that is different from saying that nobody should look for unconventional ideas. We made the point in the Introduction that creativity can spring from putting together previously-unconnected ideas. To draw on an example from physical science, spectroscopy was sharply disconnected from chemistry in the nineteenth century: the former was about studying the curious spectra emitted by the elements, and the latter was about trying to understand the structure of matter and how it reacts. It was only when chemists got interested in the bizarre ideas surfacing in twentieth century quantum theory that connections started to be made and an elegant theoretical structure built: spectra and chemical behaviour were two manifestations of the electronic structure of both atoms and molecules, and spectra provided valuable chemical diagnostic information.

There is a further implication of Baruch's point. In arguing, as he does, that we haven't yet adequately explored the possibilities of social science theory—and we don't disagree with this at all—he could ask: how will we know when we have? Who is authorized to make this decision? This, of course, leads us into very difficult territory. The issue of who authorizes work as "good science" has always been a thorny one, and the peer review system is as vulnerable to it as any; the literature is replete with stories about ideas which subsequently became highly influential but which took some time to find a publisher. But even at a practical level, how do we know when nothing more will emerge from the tired group of ideas that we have limited ourselves to working with? We don't want to be thought of as overselling the potential contribution of the ideas in this collection of papers, and of course it is possible that our project ultimately will lead nowhere, but we believe that the kind of thought censorship that Baruch suggests is fundamentally antithetical to the scientific process.

There is also implicit in Baruch's position an assumption that "first movers" or "pioneers" are more legitimate than those who follow after. In contrast, we would argue that the justification of good science, any good science, is that it explains. New paradigms, new concepts and new theories supplant old ones when they provide better—clearer, simpler, more precise—explanations of the world (Van Maanen, 1995). If new science perspectives on careers can do this, what would then be the point of first "filling in the gaps" around existing perspectives?

THE PHYSICAL SCIENCES ARE JUST TOO DIFFERENT

Baruch's next point is that the social and physical sciences are qualitatively so different from each other that ideas from the latter simply can't be used in the former. He quotes Shultz's dictum that atoms and molecules don't talk back; in other words, that the social sciences differ from the physical sciences in that they are concerned with objects that reflect on their context, that can develop and transform. The physical sciences, he argues, are about falsification, while the social sciences are about association. Indeed, he says, given how long "new science" has been with us, surely the obvious inference to draw about what we are attempting is that other scholars have realized that the exercise is futile? He argues that we have partially taken this point—that the two branches of science are too different from each other for the one to help the other—when we say that for much of the time an effort such as ours has to dwell at the level of metaphor. Apart from raising a minor objection, that we point out in the Introduction that we are by no means the first social scientists to import ideas from the physical sciences, we are left wondering: so what is the precise nature of Baruch's objection? At one point he argues eloquently that metaphor «can be advantageous, in the sense that the analogy can enhance the understanding of relevant phenomena,» (p. 18) citing Pondy in support of this view. He adds that: «The new science can definitely offer new and relevant metaphors to add to our understanding of the phenomena of careers.» (p. 18) However, he then proceeds: «Will it be sufficient to develop new career theory? The answer, I argue, is negative.» (p. 18) Why? If metaphor can be helpful, why shouldn't we explore it to see what it offers? What is so limiting about careers theory that it cannot benefit?

Furthermore, not all of the papers in the collection work at only a metaphorical level. Moreover, the most common concepts to run through them—for example emergence, nonlinearity, and self-organization—spring from complexity theory, which has already been widely used in the social sciences. Whether these papers succeed in their attempt is another matter, of course; our point is simply that some of our authors have indeed tried to transcend the use of metaphor.

THE WORLD OF CAREERS IS CHANGING SO RAPIDLY THAT NOW IS NOT THE TIME TO ATTEMPT A THEORETICAL SYNTHESIS

Icons of organizational career practice have been falling so fast recently, Baruch argues, that we live in too uncertain a world for it to be wise to indulge in new theory-building. There is certainly something to be said for this view. If the empirical object of one's theory is unstable, how can a theory connect with it? We have two responses to this argument: 1/ good theory should transcend—even, explain—changes in the phenomena it addresses; and 2/ we're not

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sure that the change going on is enough to invalidate what we're trying to do.

Our first point is, perhaps, rather purist, but it needs to be made. It seems to us that any theory of careers which could not cope, for example, with explaining the relationship between different kinds of organizational form and the careers of the people who move within and between these organizational forms, is a pretty inadequate theory. It has been fashionable of late to argue that much work on work careers has assumed far too readily that careers take place within bureaucratic organizations, an assumption which is untrue for an increasing proportion of the working population. Yet this argument neglects a great deal of important work, including seminal studies by the Chicago school under the influence of Everett Hughes (1958), Gouldner's (1958) identification in of cosmopolitan as well as local careers, the recognition of occupational communities (Van Maanen and Barley, 1984), and so on. In other words, not only have careers scholars recognized for a long time that work careers come in an enormous variety of shapes and sizes, they have been identifying them, classifying them and trying to build theory around them. Many careers scholars are trying to build better theories that encompass these many shapes and sizes (see, for example, the collection of papers in Arthur and Rousseau, 1996), and we see no reason to pause in our efforts. On the contrary, we think that the challenge is to see if we can put frameworks together which help us to make sense of the changes we are seeing, and that is very much the thrust of many of the papers in this collection.

Our second point addresses an empirical issue. There is by no means unanimity in the scholarly community that the organizational career is dead and that the careers world really is in the turmoil that Baruch holds it to be. Some, for example, have argued that the "boundaryless career" has been oversold as a concept (e.g., Nicholson, 1996), while others argue that for much of the working population it is an unaffordable luxury (Perrow, 1996). So while Baruch's view is certainly fashionable, and there is plenty of evidence that the world has changed since the post-World War II boom during which much early careers research was done, we think it premature to conclude that present-day careers are so much in flux and unknowable that they cannot be the subject of theory-building.

IDEAS FROM THE NEW SCIENCES SHOULD FIRST BE APPLIED TO THE SOCIAL SCIENCES GENERALLY

Baruch's point here is that «Perhaps the way forward to benefit from the new science theories would be to see how they contribute to the behavioral sciences in general, and then to apply them to career theory rather than bring them through the back door of career theory, a field in its infancy.» (p. 20).

We disagree. We find it hard to imagine how Baruch's recommenda-

tion could be carried out, which perhaps indicates our own limitations. But more than that: Barley (1989) argues that the contribution of the Chicago school was, inter alia, to demonstrate the centrality of career to the understanding of social structures. If so, then surely careers are a wonderful starting-point to explore the potential contribution of the new sciences to social science theory in general?

NO PHYSICAL SCIENTISTS WERE INVOLVED IN THE PROJECT

Baruch is not quite correct in this assertion—the project did involve contributions from people from the physical sciences and engineering—but he is substantially correct. Early on we spent time with colleagues in the physical sciences trying to gain a better understanding of the concepts and theories that so intrigued us. They patiently pointed out errors in our understanding and suggested books and articles that would help us clarify our thinking. The reaction of these colleagues to our efforts was itself intriguing. Often they would ask how we could possibly apply such concepts to social phenomena that they viewed as infinitely more complex than the physical phenomena they chose to study? As we shared our perspective on how we saw that new science might apply to careers they usually became intrigued by the possibilities. Nevertheless, the views expressed here are ours, not theirs. We feel reasonably confident that we haven't misrepresented new science concepts, but also accept that our understanding is still imperfect. All we can say is that we tried to take counsel from the world of physical science, even though it wasn't easy to do so.

CONCLUSION

Despite our differences with Baruch's position, we are grateful that he has raised the objections he has. Conversations of this kind are vital to developing ideas and testing them properly. Some wag once noted that the art of conversation consists in keeping your mind clear while the other person is talking, so that you don't forget what you were going to say next. We wonder to what extent Baruch on the one hand, and we three on the other hand, stand as cases in point? We leave it to the reader to reach his or her own conclusions.

We close by returning to a point we introduced above. Ideas that are new to a field are often unpopular and get rejected by the prevailing orthodoxy. Sometimes they richly deserve this fate, but sometimes they can be very helpful, and every now and then they turn the field on its head. We don't mean to claim that we think we will turn the careers field on its head, but we are intrigued by the ideas the authors of these papers have been working with. How are we going to bring fresh thinking into our field if we previously dismiss any attempt to do so?

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