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Rules Are Not the Way to Do Interdisciplinarity: A Response to Szostak

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Abstract: An alternative view is offered to Szostak's (2000) multi-step guide to doing interdisciplinary research. Interdisciplinarity is presented as an intuitive process instead of Szostak's step- or rule-based process. To support the view that interdisciplinarity is an intuitive process, the actual process used in a published interdisciplinary article is compared to Szostak's steps. Only some of Szostak's steps are found to apply, and most of these seem applicable in a post-hoc fashion rather than as guides during the process. It is argued that the choice between a step- or rule-based process and an intuitive process is more than personal preference or style. Post-positivist views of science are reviewed that show that science is largely an intuitive process. It is further proposed that if this is true for science—one of the most rational and logical discipline clusters—then it should be equally true for interdisciplinarity. Even though interdisciplinary work is intuitive, steps or rules may have some use, although a list of them would be unlimited.

I WAS PLEASED TO SEE Rick Szostak's article "How To Do Interdisciplinarity." In my article "Another Approach to Interdisciplinary Studies" (Mackey, 2001), part of a collection of responses to William Newell's "A Theory of Interdisciplinary Studies" (2001), I urged that interdisciplinary efforts should be more iterative. This means that interdisciplinary work should build on previous interdisciplinary work. Szostak continues the iteration and embraces the view that "our goal must be an ongoing conversation among scholars in which we build on each other's work" (2002, p. 105). I commend him for continuing the conversation.

Szostak's effort at continuing the conversation and "integrating the debate" (p. 103) results in a multi-step approach to performing interdisciplinary work. I believe his multi-steps can be useful to those working on interdisciplinary scholarship. However, I see its usefulness as *one* model of many possible approaches and a useful post-hoc check rather than as a prescribed

set of steps or rules to be followed in interdisciplinary work. I do not think that it adequately characterizes the nature of the interdisciplinary process.

Let me set out the reasons why I see Szostak's multiple-step or rule-based vision of interdisciplinary work as inadequate. My first reason is based on the fact that when I compare my own process to Szostak's steps or rules, only some of his steps seem to apply and the multi-step process seems inadequate for my interdisciplinary efforts. I take as a specific example the process involved in my article "Fractals or Fish" published in *Issues in Integrative Studies* (1995). I will try to reconstruct the process by which that work came into being.

In my reading, which is very unsystematic, I came across an article by Stanley Fish titled "Being Interdisciplinary Is So Very Hard to Do" (1991). I considered myself to be an interdisciplinarian and held a position in an interdisciplinary program, so I experienced Fish's attack on the possibility of interdisciplinary scholarship as disturbing and a threat. I had to admit that Fish presented a strong case in his well-argued article. I was challenged to see if I could come up with a rebuttal to the arguments in Fish's article. This was the origin for what eventually became "Fractals or Fish." Szostak's step "*1. Start with an interdisciplinary question*" (p. 105), would apply here. My question was, can I come up with a rebuttal to Fish's case against interdisciplinarity? The next steps in my process seemed quite different from Szostak's. I was involved with the then new science of chaos (Gleick 1987) and fractals (Mandelbrot 1977). As I remember it, the phrase "fractal or Fish" popped into my mind. I liked the alliterative sound of it; it echoed in my mind and popped into my consciousness at unexpected times. Dare I suggest that a catchy title can become the basis for interdisciplinary work? I could turn this intuitive insight into a rule like: *find a catchy phrase that sums up your intuitive feeling about the issue or question you are considering*, but that would be a post-hoc reflective act. This suggests that Szostak's rules could be added to, infinitely I suspect, and even that, while it might be helpful, would not adequately capture the process of interdisciplinary work.

I did not utilize Szostak's steps "*2. Identify the key phenomena*" (p. 106) or "*3. Ascertain what methods are particularly relevant to the question at hand*" (p. 106). I should note that Szostak indicates that an individual interdisciplinary scholar might not use all his rules. Because chaos theory and fractals were what I was thinking about, the catchy and persistent "fractals or Fish" kept goading me, but I confess I did not perform Szostak's step "*4. Perform a detailed literature survey"* (p. 107). I performed a cursory literature survey at best. I went to the library and checked out Fish's latest work

Doing What Comes Naturally and read it (1989). That was the extent of my literature search. Admittedly, I might have benefited from a richer search, but the article didn't require it.

Szostak's step six says, *"If some phenomena (or links among these), theories, or methods identified in (2) and (3) have received little or no attention in the literature, the researcher should try to perform or encourage the performance of such research"* (p. 111). Perhaps one might construe what I did in my article in this way since I did apply chaos theory and fractals to interdisciplinarity, and I was unaware of anyone else having done this. However, I didn't do this because I identified theories and methods that had received little or no attention in the literature. Chaos theory and fractals just happened to be where I was at the time. The reoccurring mantra of "fractals or Fish" goaded me on rather than some awareness that applying chaos theory and fractals to interdisciplinarity had received little or no attention.

As for Szostak's step "*8. Evaluate the results of previous research"* (p. 111), I confess that I was not widely read in theories of interdisciplinarity or of Fish's published work, so that admonition did not apply. I also have doubts about the extent that Szostak's step "*9. Develop a more comprehensive/integrative analysis*" (p.114), applies to my process or just how useful it is. Step nine seems to me to be like putting the uninitiated on a bicycle and saying, "Ride." To be fair to Szostak, he does provide some eight subheadings in which he attempts to elucidate and amplify this step. Szostak's sub heading g.) of step nine argues that "the interdisciplinary research must attempt to understand how multiple causation and feedback loops interact" (p. 116). My response is yes, precisely! Both Newell (2001) and I (2001) spent much effort explicating this issue and just how *emergence* develops. We propose different mechanisms, but we both believe this is the key to understanding interdisciplinary integration. I do not believe that Szostak can claim to be integrating Newell's and the ensuing articles until he addresses emergence itself rather than simply offering the prescription, however accurate, that we must "understand how multiple causation and feedback loops interact" (p. 116).

The term emergence is appearing more and more in the discourse dealing with the nature of interdisciplinarity. I predict that it may even displace integration in this discourse and in theories of interdisciplinarity. Interdisciplinarians are not alone in turning their attention to emergence. Duncan J. Watts (2003), in a recent article in *The Chronicle of Higher Education*, highlights the gathering interest and effort to understand emergence in the social sciences. I believe that any discussion of the nature of

interdisciplinarity must address emergence. Emergence, of course, implies the appearance of something new, something unexpected. Thus, it would be hard to capture the process or the product by a step or rule.

I find Szostak's step nine subheadings b) and e) applicable in a post-hoc way to my article. They state: "*b) if more than one theory is involved, the range of applicability of each should be specified*" (p. 115), and "*e) theories as applied within disciplinary research may need to be adjusted in many ways to fit within a broader analysis"* (p. 116). Fish's article, I would argue, was itself transdisciplinary not disciplinary, yet I ended up concluding something close to 8 b) and e). I came to my conclusions, though, not by consciously applying something like these rules, but on the basis of a hunch and the mantra "fractals or Fish" pushing me forward. As I was reading Fish's critique of legal theorist Roberto Unger in *Doing What Comes Naturally* (1989), I became aware that his discourse was employing terms implying Euclidean geometry. Unger, whom Fish was criticizing, was using terms that were implicitly fractal. Fish seemed to rule out Unger's fractal possibilities because they were impossible within his own implicit Euclidean discourse. That was the crux of my criticism of Fish's interdisciplinarity critique. It was ensconced in an implicit Euclidian view, whereas fractal geometry provides a discourse that allows for interdisciplinarity.

Szostak's steps ten, eleven, and twelve seem like useful, common sense suggestions.

The central issue I have with Szostak's approach comes down to whether interdisciplinarity is primarily a rule-based process or an intuition-based one. Perhaps this could be passed off as a matter of personal preference or style between us. I will argue, however, that something more important and substantial is at stake.

Until the middle of the twentieth century, science was viewed as a step- or rule-based activity. Scientific research was assumed to follow the scientific method, a set of steps or rules like: 1. State the problem; 2. Formulate a hypothesis; 3. Design and carry out an experiment; 4. Make observations; 5. Record data from the experiment; 6. Confirm the hypothesis; 7. Form conclusions (Trowbridge & Bybee 1986, p. 44). The dominant philosophy of science was logical positivism which accepted the step- or rule-based approach and added some additional steps like: 8. Define all concepts operationally, and 9. Check the logical structure of the conclusions (p. 43).

Beginning in mid-twentieth century, post-positivist views of science emerged. Important post-positivist philosophers of science moved away from a step- or rule-based view of scientific research toward one that incor-

porated an intuitive view. Michael Polanyi (1958) argued that creative imagination should be accepted as an important element in science. He emphasized the importance of a tacit dimension in science. This tacit dimension is internal and preconscious. How it worked could not be captured by steps or rules or even described or written down. Karl Popper (1968) proposed that scientific research followed a hypothetico-deductive method where hypotheses could emerge from hunches, intuition, and even dreams. P.B. Medawar (1964), an advocate of the hypothetico-deductive method in science, says that forming a hypothesis is "an imaginative or inspirational act" (p. 43). Thomas Kuhn (1962), in his seminal work *The Structure of Scientific Revolutions*, substituted paradigms for the steps of the scientific method to explain how scientific research was done during periods of normal science. A paradigm is an explicit piece of research including law, theory, application, and instrumentation that serves as a model for other research. However, in periods of revolutionary science, where a new paradigm is in conflict with an old one, the new paradigm is incommensurable with the old one. This means that no steps or rules can be given for moving from the old paradigm to the new. Kuhn is adamant that neither steps nor rules guide scientific research.

> One is at liberty to suppose that somewhere along the way the scientist has intuitively abstracted rules of the game for himself, *but there is little reason to believe it*. Though many scientists talk easily and well about the particular individual hypotheses that underlie a concrete piece of current research, they are little better than laymen at characterizing the established bases of their field, its legitimate problems and methods. *If they have learned such abstractions at all, they show it mainly through their ability to do successful research. That ability can, however, be understood without recourse to hypothetical rules of the game*. (p. 47, my emphases)

Learning theory and cognitive science increasingly support Kuhn's ideas (Nickles 2003).

Post-positivist views of science thus indicate it is largely an intuitive process, not a process that can be captured by steps or rules. If this is true of science, presumed to be one of the most rational and logical clusters of disciplines, it must certainly be true for interdisciplinary work, which in Kuhn's terms is in a pre-paradigm stage and is likely to be even more intuitive and less capable of reduction to steps or rules. So I do not think that the difference between Szostak's step or rule process of interdisciplinarity and my

advocacy of an intuitive process is simply a matter of style or preference.

Let me stress again that I welcome Szostak's effort at continuing the conversation on the nature of interdisciplinarity. I think his steps can be helpful. I have indicated that some of them apply and clarify my own largely intuitive interdisciplinary process, particularly as a post-hoc review. I just don't believe they adequately capture the interdisciplinary process because any step- or rule-based approach must be inadequate for interdisciplinarity in the same way it has been shown to be inadequate for science. But given that interdisciplinarity is largely an intuitive process, does this mean it will remain highly individualized, to some degree not specifiable, and institutionally anarchical? My answer is that this is likely the case, but I do have constructive suggestions to offer. Perhaps it would be useful for interdisciplinarians to reflect on the actual process resulting in their published work and share this publicly. I am not advocating that interdisciplinary work be published as an account of the actual process used. Scholarship needs to be conveyed clearly and in concise form. This is why scientific research is presented as if the steps of the scientific method had been followed, even though the actual process was quite different (Medawar 1964). Perhaps reflections of the actual process of exemplary interdisciplinary scholarship might be appended to articles. Another suggestion would be to collect accounts of the actual processes of interdisciplinary scholarship in a monograph. It might be possible to abstract useful generalizations that could be added to Szostak's steps. I believe, though, that the list of steps is unlimited. At least an array of models of the process of interdisciplinary work might prove useful to others. It would also remind us that interdisciplinary work is intuitive and never fully explicable by steps or rules.

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