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Intuition and Interdisciplinarity: A Reply to Mackey

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I AM MOST GRATEFUL to J. Linn Mackey (2002) for his thoughtful commentary on my article. He is correct in arguing that intuition deserves a more prominent place in a discussion of how to do interdisciplinary research. I welcome the opportunity to correct this oversight on my part. However, Mackey draws what I believe to be a false dichotomy between "rule-driven" and "intuitive" research; I will endeavor to show that intuition has an important place within a rational process of interdisciplinary research. Note in this regard that Mackey's critique is a critique not just of the process suggested in my paper, but also of the very idea of identifying an interdisciplinary process.

My intent in outlining a multiple-step process for the performance of interdisciplinary research was, first, to define *what* this entails and, second, to define as much as possible *how* this should proceed. As in my discussion of science in general, in Szostak (2003), I struggled to be as precise as possible about *how*. Intuition has a place throughout this process, but especially in those steps for which there is an obvious disjunction between the *what* and the *how*. Mackey imagines that he has discovered a new step: "formulate a catchy title." I would see his approach rather as intuitively deriving a research question: "What is the relationship between fractals and Fish's argument?"[1] I would be the first to admit that "identifying a research question" is not an easy step, nor one that lends itself readily to the exercise of cold logic. I noted strategies that a researcher might use to help identify a research question, but am happy to agree that the best of questions often arise unbidden in our minds. Other steps where intuition may loom particularly

large include developing a new integrative framework, critiquing previous research, identifying promising lines of new research, and—especially with existing library catalogues—performing a literature review.

In Szostak (in press), I develop inductively a list of the twelve broad methods employed by scholars. One of these is intuition. I note that intuition likely plays a more important role in some sorts of scholarly investigation—say trying to understand how and why art moves us—than others. Yet I recognize, as Mackey insists, that intuition plays a role in all types of scholarly investigation.

Intuition is in one important way treated differently from the other eleven methods in Szostak (in press). While I favor the use of multiple methods generally, I argue that in the case of intuition, scholars should be particularly careful of relying upon this exclusively. The reason is simple: our intuition can at times guide scholars to important insights, but can also quite easily guide scholars to misguided insights which reflect not some external reality, but only subconscious desires or perceptions. Scholars would not want to ignore their intuition but would want to subject its insights to critical analysis. However, this critical analysis should be neither too early nor overly harsh.

Mackey is quite right in noting that historians of science increasingly appreciate the role of intuition in the process of scientific discovery. He also admits, but downplays, the simple fact that intuition is far from the whole story. Importantly, he notes that scientists rarely mention their intuition in their research reports. Why is this? The practice could be attributed to a variety of malevolent motives, and is, we shall see, not entirely desirable. To at least some extent, though, it must reflect the simple fact that saying, "Hey, I had a great idea while in the bathtub the other day" would not, and should not, persuade others. Rather, the scholar needs to show how the idea can be derived from pre-existing ideas, and how it is compatible with evidence derived from methods other than intuition. Notably, Mackey appreciates that at least some steps in the proposed process facilitate a *post hoc* evaluation of ideas developed through intuition.

Science, as Mackey appreciates, is a conversation. No matter how much its advances may depend on intuition, the conversation depends on these being described in a logical manner and subjected to analysis using other methods. This means, in practice, asking whether the results of intuition could have emerged from a logical process of scientific investigation. The scientific conversation depends, then, on some consensus on how science should be done. Interdisciplinary research is more complicated than

specialized research, and thus requires a more detailed process.

The foregoing couple of paragraphs have focused on what happens after an act of intuition. We should also think about what happens before. As historians of science appreciate, acts of insight tend to come after a lengthy application of thought to a particular problem or question or theme. As noted above, these insights might take various forms: for example, formulation of a previously unasked question, identification of a new avenue of research, or solution of a theoretical puzzle. That is, acts of insight can happen at various stages of the process. But they do not just "happen."[2] Beveridge (1957) is a bit dated, but still provides one of the best discussions of examples of scientific discovery: in each case a scientist had worried about a particular problem for a long time, read and experimented, and then put the problem aside, only to find inspiration leap unbidden into his/her mind while s/he was walking in the park. Scholars who spend their lives walking in the park will rarely if ever have great inspirations. The scholar who attempts to pursue consciously a line of inquiry, but takes the occasional break from research, may get "lucky," that is, they may put together the package of information their subconscious mind needs to work with, and recognize the value of insights that emerge. In other words, an interdisciplinary scholar pursuing a rational process of interdisciplinary investigation—looking widely but in the right places for relevant information, thinking about the relevance of different theories or methods, aware of potential biases in previous research—is much more likely to produce valuable intuitions.

Mackey may himself have been lucky. He developed a research question without, apparently, much previous worry. Yet even here Mackey could not have proceeded without having previously read a fair bit about both interdisciplinarity and fractals. He may never have imagined that these two literatures would intersect in his mind. The process I outlined could potentially guide researchers to identify distinct literatures that have not previously been combined. In doing so, it would greatly encourage intuition! Note that one advantage of the suggested process is that it strives to free the interdisciplinarian to look "outside the box" of previous research.

Individual researchers must thus pursue both process and intuition; this is even truer at the level of communities of scholars. As Mackey appreciates, my concern was primarily with how an interdisciplinary scholarly community should proceed. As I noted, individual researchers can benefit greatly from consciously attempting to perform the steps in the process, but will rarely, if ever, be able to perform them all. The scholarly community as a whole, however, should strive to do so. Mackey and I both see scholarship

as a conversation; I would submit that the interdisciplinary conversation will be more productive if we share a sense of what needs to be done.

Communities of specialized scholars engage in conversations against a backdrop of fairly precise understandings of how research should proceed. As Mackey notes, these understandings are generally unwritten and often indescribable. I have argued elsewhere (Szostak 1999, 2003) that these "rules of the game" are often suboptimal, but they serve an important purpose in allowing researchers to readily understand, evaluate, and build upon each other's work. Interdisciplinary scholars likewise need a shared sense of how research should proceed. As noted above, intuition can contribute to many steps; thus agreeing on such a set of steps encourages rather than precludes the exercise of intuition. As argued in my previous article, we should strive for a process that clarifies without proscribing any valid form of interdisciplinary research.

Since Mackey focused upon a previous paper of his, let us imagine two ways that interdisciplinary scholars might react to this previous paper. One would be to exclaim: "What a great paper! I am inspired to do something completely different." As is hopefully clear from the foregoing, this can be a valuable response, but we as a community of interdisciplinarians will be severely limited in our collective contribution if that is our only response to the contributions of others. A second type of response would be to ask how we might build upon Mackey's insights. What happens if we perform the detailed literature survey he eschewed, embrace theories and methods he ignored, formulate related but different questions, and so on? One can imagine a series of papers or books pursuing the steps that Mackey skipped. The result in the end would not just be a series of insights—though it would be that too—but a coherent understanding of, in this case, whether interdisciplinarity leads naturally to disciplinarity. Specialized researchers can generally identify some of the questions that deserve investigation in their field; the interdisciplinary process allows interdisciplinary researchers to readily do the same.

There is, notably, a debate as to whether interdisciplinary analysis should ever be performed by individual scholars. Can one scholar ever master enough of (parts of) two or more disciplines to provide more than a superficial form of integration (Wilson 1996)? Practice in this regard differs considerably by field. Interdisciplinary humanists tend to work alone, while interdisciplinary natural scientists generally work in teams, and social scientists do a bit of both. Given that I do individual interdisciplinary research, it is hardly surprising that I think that it can be valuable. But I think those of us

who do individual interdisciplinary research should be aware of the potential charge of dilettantism. It is thus particularly important that we be part of an ongoing interdisciplinary conversation that has clear mechanisms for building on the inherently limited insights of individual scholars.

Mackey predicts that the intuition-dominated interdisciplinarity that he favors will always be individualized, unspecifiable, and anarchical. I would submit that such an interdisciplinarity will always be viewed with suspicion by those in disciplines, and for good reason. Scholarly communities must exhibit some sense of cumulative development (albeit replete with setbacks and revolutions). If we cannot build upon each other's insights so that these can be combined into some coherent understanding of particular issues, then we have but a congeries, a heap of papers and books. It may be enjoyable to engage in such an interdisciplinarity, but we can hold out little hope that interdisciplinary scholarship will ever shed more than isolated pinpricks of light upon pressing matters of either scholarly or public policy concern. The proposed process leaves immense scope for the exercise of individual imagination—and thus interdisciplinary research will continue to be enjoyable, exciting, and challenging—but allows interdisciplinary research at the same time to be cumulative and coherent.

Scholarship always has advanced through a process of specialization and synthesis (Collins 1998, Dogan & Pahre 1990). Interdisciplinarity is not just a parlor game, but has an essential role to play in the scholarly enterprise. My vision of a coherent interdisciplinarity is not a dream but a necessary reality if scholarship as a whole is to productively advance our collective understanding.

Mackey quotes approvingly Kuhn's view that paradigms are incommensurable. I would side with the many philosophers and historians of science who dispute this: in practice communication across paradigms—or disciplines—is always possible, though opportunities for misunderstanding are legion (see Galison 1996). I am not sure how much of Mackey's pessimism (my word) about the possibility of a coherent, cumulative interdisciplinarity is grounded in this view of incommensurability. My view of this and many other issues in the philosophy of science is that interdisciplinary scholars should beware of potential difficulties, neither exaggerate nor dismiss these in advance, and strive to add as much to our collective understanding as possible. In the course of step 5, I digressed somewhat to discuss how interdisciplinarians could empirically investigate the coherence of disciplines, subdisciplines, and interdisciplinary fields. Likewise, the question of how well understanding can be transmitted across different communities of scholars

is best evaluated in the process of doing so.[3] As noted in step 5, the trickiest issue to get a handle on in the entire interdisciplinary process is the vexed question of disciplinary "worldview." The better interdisciplinarians are able to describe what worldview entails, the better they will be able to both communicate across communities and assess the potential limits to such communication.

In the spirit of empiricism, I would close by enthusiastically seconding one of Mackey's suggestions: that interdisciplinarians be encouraged to reflect upon and report how they undertake interdisciplinary research (perhaps in a special volume; where possible, in their research reports). This would, I think, be a useful guide to other researchers—and I think we need to appreciate that there is a large body of scholars out there tempted by interdisciplinarity but without a clear idea of how to get started. It would also allow us as a community to assess what it is that we do well, and what perhaps is missing. In particular, we would gain an important understanding of how both to nurture and build upon our intuition.

Notes

1. Mackey suggests that there are a potentially infinite number of steps. I counter that there are a finite number of steps, though perhaps a very large number of ways of performing some of these. I repeat the challenge in my original paper, and invite interdisciplinarians to suggest steps that are missing from the process I outlined.
2. In Szostak (1991), I built upon Abbot Payson Usher's four-stage model of technological innovation. This can be applied to scientific innovation as well. The inventor must first "recognize the problem (area of study)," and "set the stage" by gathering relevant information this may be followed by the "act of insight," which must in turn be followed by "critical revision." The outcome of one innovative process may provide input into others.
3. Mackey suggests that I should decide between his view of how emergent properties emerge and Newell's. I do not see how this issue can be decided theoretically. Both he and Newell recognize that this question is hotly contested within the field of complexity theory. I think we will learn more as researchers try to apply different theories. We may find that one version is better at generating empirical insight, or that different versions of complexity theory are best suited to different types of investigation, but it is an empirical question.

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