What If We Took Researchers’ Workplace Decisions Seriously? Mundane Incentives Versus Intellectual Merit in the Selection of Research Topics

TERRY CONNOLLY
University of Arizona

For English boys growing up in the immediate aftermath of the Second World War, there was one generic fantasy hero: A junior officer (British Army, of course), blood-stained bandage around his head, waving forward a group of grim-faced troops, presumably toward a crucial enemy position. The impression was that, having bravely waved on his troops, the injured officer would be taken away to have his wounds attended to by properly attractive nurses in a safe location. The charm of this fantasy faded only as we started to realize that, in reality, we would much more probably be wavees than wavers and would be in the thick of battle long after the officer’s wounds were healing.

Dalal et al. (2010) perform a similar waving-forward function for industrial-organizational psychology and organizational behavior and the judgment and decision-making researchers, pointing us toward potentially fruitful intellectual and practical gains at the intersection of our interests. As they note, they are not the first to do so, and the track record of these exhortations as stimuli to later research is, at best, modest. The troops, it appears, are surly, if not downright mutinous. They do not dispute the potential gains of storming the targets pointed out to them, they simply choose other targets. Why?

One possibility is that Dalal et al. and the other urgers-forward take too flattering a view of what makes us tick as researchers. They implicitly see our own workplace decisions, such as choice of research topics, as motivated solely, or mainly, by the intellectual or practical gains from attaining certain research targets and in need only of guidance toward the highest yield topics. Although I would like to think that such considerations have, from time to time, been a part of my own research choices, I am humblingly aware that many more mundane factors have shaped my portfolio: available research funds, attractive colleagues, previous intellectual investments, considerations of risk, the skills I did and did not have, the ease with which missing skills might be acquired, the time required before a given study would produce results, my knowledge of some fields rather than others, probably some degree of snobbery about applications versus theory-building work, and so on, not to mention a large chunk of sheer randomness—a chance conversation on a

Correspondence concerning this article should be addressed to Terry Connolly.
E-mail: Connolly@u.arizona.edu

Address: Department of Management and Organizations, The Eller College, University of Arizona, Tucson, AZ 85721.
plane, a book picked from the remainder table, or the offer of a discarded box of jury questionnaires.

Some years ago I sketched with a colleague (Chubin & Connolly, 1982) a model of how these mundane factors might shape researchers’ choices of scientific topics on which to work. Our interest was in “research trails,” sequences of studies addressing a single topic within a scientific specialty and which, in aggregate, constitute the work being done in the specialty at any given moment. A given research trail may be associated with the work of a single individual or small team, or perhaps a few individuals or teams, but is thought of as (a) much smaller than the specialty as a whole and (b) having distinctive continuity over time. The central interest is in the connections over time within a research trail rather than the connections at a given point in time among the various trails comprising the specialty. The pursuit of such research trails seems absolutely characteristic of “normal science” in the Kuhnian sense (Kuhn, 1962/1996).

Three characteristics of research trails are relevant here. First, the researcher setting out on or continuing in a given trail can rarely predict the ultimate yield of the trail and must therefore evaluate individual studies rather myopically. The potential of the next study may be fairly clear, that of studies further ahead much less, so planning is incremental. Second, costs to the researcher (broadly conceived) tend to peak early in the trail and decline sharply thereafter, whereas marginal payoffs are roughly constant (or, at least, positive) throughout. Entry costs include reviewing the relevant literature; learning specialized techniques; acquiring suitable equipment, instruments, colleagues, and facilities; and raising research funds as an unproven outsider. Payoffs to the researcher include publications, prestige, grants, invitations to conferences, and professional visibility. These tend to accumulate later rather than earlier in established trails and retrospectively add legitimacy to the earlier work. We proposed, additionally, that most research trails do not unambiguously end in clear failure or success, but typically “peter out in studies of small impact or marginal variation” (Chubin & Connolly, 1982, p. 297). In short, the core model is one of the large initial investments followed by small marginal costs and continuing positive marginal benefits, leading to undue persistence in established trails.

To the extent these mundane costs and benefits shape the research choices of actual researchers, research trails will typically be followed longer, and with smaller yield, than would be predicted by the attention-switching model implicit in Dalal et al.’s article. At the limit, a typical research career might consist of a student making large investments in a particular trail during her PhD training and early career, initially (with luck) generating several high-impact publications in time for tenure but going on to a career-long series of related papers, reviews, book chapters, research grants, all supervised PhD theses, all adding only marginal refinements, small extensions, and variations to the original contributions. (A friend proposed a “Law of Perpetual Inquiry” based roughly on these ideas and nominated several colleagues as possible victims. “Perpetual” inquiry seems empirically implausible, but we had no difficulty identifying, at least in the work of others, research trails that had been pursued well past the point at which new studies added much value).

This is not intended to disparage all long-run, persistent development of research trails. Strong programmatic research is a hallmark of good science, and practitioners who flit from topic to topic picking up scraps of low-hanging fruit are not much admired. Nor is it simply to deplore researchers who tend to do what they know, think as they have previously thought, and find their satisfactions in filling out an important puzzle. There is much to be said in favor of a certain conservatism in science. However, the mechanics of the high entry cost-low marginal cost model offer a plausible account of why compilations of good research ideas like those in the present...
target article tend to be under exploited. The ideas may be fine, but most of us are already pretty busy and productive pushing down our current trails. Why take on a risky new investment?

How might these incentive structures be modified? What adjustments of the mundane cost–benefit equation might improve the uptake of Dalal et al.’s suggestions? Would simply alerting our students and colleagues to the perils of “perpetual inquiry” mechanisms lead them to terminate fading research trails more quickly? Is collaboration with other researchers with different intellectual investments a good approach to reducing entry costs into novel research trails? Do focused funding initiatives, perhaps around pressing practical problems, typically yield significant new research on the focal topics or simply lightly disguised retreads of the beneficiaries’ existing agendas? How about special issues of journals? Should we expect the breakthrough refocusing to come from new researchers arriving in a field or from older, better-established (and tenured!) researchers who can afford to take longer-term risks but who may be too locked into their existing trails to want to abandon them?

I will refrain from launching my own “more research is needed on these topics” battle cry, but a little more attention to our own workplace decisions as researchers may be useful. It seems clear that even thoughtful and imaginative efforts, like Dalal et al.’s, to identify neglected research topics of real intellectual and practical importance, have an uneven track record in reshaping their target disciplines. Perhaps such suggestions are ignored because researchers’ choices of research topics are importantly shaped by mundane factors such as those noted earlier as well as by consideration of the topics’ intellectual merit. Furthermore, a back-of-napkin assessment of the direction of these mundane incentive effects suggests that the balance will be toward conservative persistence within the scientific research trails on which one is already embarked, thus discouraging any rapid shift toward new topics and approaches suggested by intellectual merit alone. Perhaps a fuller discussion of the strength and management of these effects could lead to a more rapid refocusing of our collective efforts toward important topics such as those the target article identifies.

References