

## Book Review

Colin Talbot.

*Theories of Performance: Organizational and Service Improvement in the Public Domain*.  
Oxford, UK: Oxford University Press, 2010.

Reviewed by Howard A. Doughty

Colin Talbot is to be commended merely for attempting this project. He is doubly to be commended for succeeding. He has taken on a formidable task, and he has convinced at least one sceptic that his approach and the application of his ideas to real-world circumstances are fundamentally valid. I am sure that I am not alone.

The first thing that Talbot does is to speak directly and sensibly about theory and then to link this discussion to both methodological problems concerning research and to practical issues of governmental processes—all within the context of a single overarching theme, namely how to improve the ways that government does its job.

The first problem is that theorists may not be terribly interested in the specific applications of theory to concrete situations. The second problem is that practitioners are probably even less interested in hearing about theoretical discussions—especially when they are tied to the internal squabbles among academics within and across social science disciplines.

It is, I regret to say, a common ailment of our culture that “theory” is imagined, even by some of the certifiably educated, to be “part of a hierarchy of confidence running downhill from fact to theory to hypothesis to guess... [but] facts and theories are different things, not rungs on a hierarchy of increasing certainty. Facts are the world’s data. “Theories are structures of ideas that explain and interpret facts” (Gould, 1983: 254). We feel comfortable with “facts,” whereas theories make us nervous; still, in the absence of theories, we cannot make sense of facts. Like it or not, we all put facts together with theory in order to make practical action possible. Those who seek to think seriously about situations will need to get beyond these cultural blockages. Talbot solves the problems in three related ways.

First, he lays out a plan or description, discussion and argument and sticks to it. The result is that a potential mash-up of confused and confusing steps are laid out and meticulously followed. Readers know at all times where they have been, where they are and where they are going to go. It is a plan, moreover, that makes sense and that is explained clearly early on. There are no surprises and no detours. As a result, seemingly disparate approaches and issues are set out in a way that maps some difficult terrain and makes what, in less skillful hands, would be a disjointed expedition into a seamless and quite enjoyable journey.

Second, he speaks persuasively. His efforts become meaningful because he so plainly and sincerely believes that this work is important. Linking theory and practice is a hard job at the best of times, but Talbot’s audience is apt to include people with considerable expertise in some parts

of the argument and little or none in others. Finding the best way to explain the elements of portions of his narrative to newcomers without boring the already well-schooled is an art in itself, and Talbot is an accomplished artist.

Third, his book has a common and consistent thread running through it. It is *about* something. That thread has a conceptual name; it is consilience; it also has a flesh-and-blood form; his name is E. O. Wilson. Talbot discusses the famous Harvard sociobiologist (and coiner of the term) in his introduction and mentions him occasionally in the text; but, his presence is never far away. He is unobtrusive, but ever watchful. He is also the source of my aforementioned scepticism.

It would be foolish to underestimate the man and his influence. For over forty years he has been a dominant figure and an inspiration to a variety of branches of the Darwinian tree, not least the now popular field of “evolutionary psychology” and even the slightly more remote domain of “behavioral economics.” Post-retirement, he has remained active and produced a number of important books, not least *Consilience* (1998). As his admirer Charles Gillispie (1998) put it: “The goal of consilience is to achieve progressive unification of all strands of knowledge in service to the indefinite betterment of the human condition.” Who could deny that?

Wilson, of course, has also been the focus of impassioned debates that are tangential to the book here under review. It is enough to say that, on the main debates in which he partook, I found myself ethically and scientifically on the other side.<sup>1</sup> It is therefore to his credit that Talbot kept my interest and at least some of my sympathy throughout.

By adopting the word “consilience,” Talbot is making the case that the many divisions within the social sciences, the natural sciences, the humanities and other intellectual approaches to understanding ourselves and our world have avoided mutual contact and necessary cross-fertilization of ideas. He steps lightly through a brief register of social sciences, commenting judiciously on what each has contributed and may potentially be able to add to a common store of knowledge if only the walls could be brought down.

In Talbot’s view, the main general elements of the problem of mutual isolation are the preferred assumptions of the two main social scientific tendencies. One is currently to be found in economics. It is the belief that the key to understanding humanity is to analyze our behavior as “rational utility maximization.” It seems that self-described “behavioral economists” imagine that they have come up with something new and exciting when all they are really doing is giving a more modern sound to ideas that have a clear pedigree going back to Hobbes. The other is to imagine each individual to be a classic *tabula rasa* or blank slate, upon which environmental factors (nurture) draw personalities, beliefs and behaviours. This is the view that Talbot claims is to be found throughout the other soft sciences, especially anthropology and sociology. To this dyad, Talbot says, we must add a balancing third view, derived from Wilson’s earlier work and given application and friendly amendments by Pinker and others. This is the evolutionary perspective that is the current field being tilled by behavioral geneticists, gene-culture co-evolutionists, ethologists (who are involved in the study of animal behaviour, especially in natural environments) and others who want to obtain intellectual nutrients from the hard or at least the harder sciences of biology.

It doesn't matter much that Talbot greatly oversimplifies and hitches himself to a dubious Darwinian horse (as I think he does). What I find gratifying is the fact that he has the temerity to raise the question and to confront his colleagues in the managerial sciences with some pretty hard assessments.

Talbot points out that many scholars in schools of business, management, administration and the like have shown themselves to be suckers (my word, not his) for an incessant stream of old wine packaged up in bright new bottles. "Management research," he says, "is beset by 'the tyranny of the new' ... cursed by self-promoting 'gurus' ... prone to fashions and fads, and to succumbing to 'halo' effects of 'successful' organizations and individuals ..." He calls this Consilience Deficit Disorder; I have called it Administration Attention Deficit Disorder. It just depends on which side of the fence you find yourself. What I do know is that wave upon wave of innovation has swept over institutional gunnels sometimes almost sinking the ship. Every year or two, the managerial class finds itself besotted with a new vision-cum-hallucination, a new style, a new technology or a new approach always dressed up in some pseudo-technical language that is never the same from ebb to flow.

Confronted with enthusiastic incoherence, Talbot begs us all to go back to basics—not elementary grammar and arithmetic, though I sometimes think it wouldn't hurt—but to foundational intellectual questions of ontology, epistemology and general issues in the philosophy of science. Only by returning to (or perhaps by visiting for the first time) these basic matters can we even begin to speak sensibly to ourselves and reach out to others in what we may hope is a common project. And, unless we do, we will simply be exchanging slogans and self-inflating over competing paradigms (seldom knowing what the word paradigm even means, whether in the manner made popular by Thomas S. Kuhn or by what Kuhn actually meant.<sup>2</sup>

What follows is an eighty-page, comprehensive, highly accessible but not at all patronizing exploration of the obstacles to good science and therefore practical proposals for the main purpose of the book—improved public sector performance based on sound theory and practice.

It has been a longstanding frustration for many of us to read breathless reports of innovations which are strong on the *how* but weak on the *why* of change. In some cases, innovation is almost seen as an end in itself, while in most others, the purpose seems to be quantitative (more, faster, bigger) with little meaningful assessment of quality (better) and less of whose interests the changes are designed to serve. Efficiency and productivity, after all, are not improvements if the program has adverse normative consequences. As an obvious example, the capacity of totalitarian governments to use sophisticated data processing methods to monitor and apprehend dissenters or selected minorities for incarceration and worse cannot reasonably be called improvements (Black, 2001).

In the second half of *Theories of Performance*, Talbot builds on his theoretical foundation and applies visibly, coherently and consistently the principles he has established. The initial step is to outline precisely what is meant by "performance regimes," his key term to describe the combination of institutional context including the identification of those persons or groups with the authority to influence public organizations and the nature of what he calls "performance interventions" to sustain, alter or improve the organizations. By analyzing and assessing specific

components of both internal and external sources of pressure and influence, Talbot establishes a schema to detect and to encourage those measures that might modify organizational behaviour. His mapping of influence includes everything from top-down managerial power to external competition, leaving plenty of opportunities for internal reform and restructuring as well.

Once the framework has been made clear, Talbot goes on to examine not only how changes for the better can be promoted, but also how and why ill-considered and theoretically flawed attempts at reform fail. A crucial part of this inquiry concerns questions of normative judgement or, put otherwise, what is the role of “values” in organizational analysis and improvement. As with some other parts of the book, Talbot’s achievement is less the satisfactory supply of answers than the thoughtful presentation of questions. I find it disappointing, for example, that structural inequities—class, race and gender, for a start—do not find a secure place in his approach, nor do we find more than a passing reference to what is commonly called “conflict theory.” Ultimately the most penetrating political values are wholly containable within representative, liberal or pluralist theories of democracy—all of which were part and parcel of the movement toward revisionist political science that got its big break with Joseph Schumpeter (1943), had its triumphal moment about 1960 (Dahl, 1956; Lipset, 1960; Milbrath, 1965), was intellectually and empirically discredited (Kariel, 1966; Pateman, 1970), but has been institutionally maintained as the official backstory for neoliberalism for the past thirty to forty years.

A truly groundbreaking work, as *Theories of Performance* advertises itself to be, would surely have addressed the nature and quality of democracy more explicitly. Again, however, Talbot’s achievement has mainly been to open management theory up to this sort of critique *within* its own doors, not necessarily to open them to the barbarians amidst the radical academic fringe on the outside.

However much Talbot finds himself succumbing to variations on systems analysis and psychological theories of political participation, he goes some way toward acknowledging that different value systems (formerly philosophies) do exist and can sometimes redefine the notion that government actions and public services can be assessed on something less one-dimensional than a customer satisfaction evaluation. His methods, derived from a multidisciplinary gallery including Bozeman (2007), Fiske (1991), Handy (1985), Cameron and Quinn (2006), Thompson (2008) are certainly abstract, a historical [ahistorical??] and in a serious sense apolitical; still, by allowing the possibility that some measure of political *thought* and not simply desiccated systemic “outputs” are in play is a step forward.

In the final phase, Talbot attempts a synthesis of the component parts of his approach: the performance regimes and organizations, the models of endogenous and exogenous pressures and interventions, and public values. Talbot rehearses his complaints about preceding research and analysis focusing, for instance, on only one or two variables (most obviously “leadership” in the absence of considering the led). And, to be honest, Talbot is not exempt from criticism himself. For instance, he writes at length about governments, agencies and policy deliberations, but rarely mentions the fact that these entities are composed of people who are subjected to innately adversarial labour-management relations, a distinctive labour process and enduring internal tensions that cannot be understood simply as one more set of endogenous tensions.

What this means, of course, is nothing more than the possibility that Talbot is on the right track, but that he has not yet travelled far enough down the road. None of this, of course, is lost on Talbot. He is disarmingly (because he seems to be sincerely) modest in his introduction: “what I have managed in this book,” he says, “is merely to outline what the question(s) are we ought to be asking and sketched some ideas about what sort of answers we should be looking for” (Talbot, 2010: 19).

The simple fact that he can provide a partial map of the territory to be explored and insist all the way to the end that we keep clearly in mind words such as “ontology” and “epistemology” as crucial to our ongoing discussion is a meritworthy in itself. The added fact that readers will be inclined to fill in some of the gaps, think critically about some of his graphics, mull over the definitions of his concepts and have several “aha!” moments as they make connections of their own is testimony enough to this book’s worth.

It is an essay into territory we only thought we had explored before, often led by tour guides over terrain with which they were neither familiar nor in which they were wholly interested. Talbot shows that the exploration he has in mind will be, of necessity, a group project and I think he will find recruits among attentive, curious and open-minded readers. That is its own reward.

## Notes

1. It may be that Wilson’s critics have overstepped by accusing him of racism, misogyny and eugenics” (Douglas, 2001); on the other hand, I find it unacceptable that Wilson has accused one of his most laudable critics, Stephen Jay Gould, of being a “charlatan” and Steven Pinker, another “rock star” Harvard psychologist in the Wilsonian tradition, has a disconcerting habit of calling critics like Gould “Marxists,” as though that somehow ended all argument.
2. Thomas S. Kuhn (1962) popularized the word “paradigm” as a term mainly intended to describe “particular coherent traditions of scientific research,” the basis for “normal science” over long periods of time. Far from identifying such a dominant and fundamental scientific approach, the use of the word and its application to the phrase “paradigm shift” has degenerated to indicate any minor shift in belief or behaviour. For a good early summary of the debate over Kuhn’s work, see Imre Lakatos and Alan Musgrave, eds. (1970).

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