

# If You Want to Be a Good Fill-in-the-Blank Manager, Be a Good Plain-Vanilla Manager

Steven Kelman, *Harvard University*

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I do a fair amount of public speaking, and I usually start with a joke, an anecdote or—preferably—a self-deprecating comment. I'm not going to do that today, because I feel so touched and honored by receiving this award from my scholarly peers that I don't even slightly want to make this a laughing matter.

Partly this is an occasion for me to think back about the mixture of family background, conscious choices, and chance that led me into the field of teaching and research in the area of public administration and public management. An interest in politics came from my family. My dad, who died just about two years ago at the age of 89—having continued to work a few days each week until a week before he died—came to the United States at the age of 19 as a Jewish refugee from Austria. Arriving without speaking a word of English, he got a job only three years later as a reporter for the *Washington Post*. My mom, who will turn 90 in eight days and whose first job after graduating from college was with the federal government, as a War Department classification analyst, was—like a certain kind of woman of that generation—endlessly active in community life, as a precinct organizer and PTA president. Some of my early memories are being dressed up as a walking political advertisement for Halloween during the 1956 presidential election and of spending entire election days at the polling station playing with the lever-style voting machines. An interest in teaching came initially from liking school, and, I think, by the time I entered college, I had decided I wanted to be a college teacher.

The move into public administration and public management was somewhat more of a coincidence. As a first-year graduate student at Harvard, I took a course with James Q. Wilson called “Bureaucracy,” and I really liked it. When I was on the job market, Richard Zeckhauser, a Kennedy School economist and later to become a good friend and next-door office neighbor, had heard

me give a presentation at a Harvard-wide seminar on regulation and decided he wanted to attract me to the Kennedy School. When I accepted the job offer, I had no idea what I was getting into. I didn't realize that the school was dominated by economists and had few political scientists; I also didn't realize that, as a professional school, its approach was far more prescriptive than anything I had been used to from grad school.

Thinking back on my years of training as a political scientist, as a grad student, and then the 30-plus years I have spent at the Kennedy School, makes me very much appreciate two things about this award.

The first is that it is so nice to see this award inside APSA because the empirical study of government organizations has come close to disappearing from mainstream political science. My teacher, Jim Wilson, did not really regard himself as a “public administration” scholar, but he did of course do empirical work on government organizations. But who are the James Q. Wilsons of the next generation? Probably the closest we come is scholars such as Dan Carpenter, John Brehm, or Robert Waterman. Most of the mainstream political scientists—in a tradition often dubbed “McNollgast”—who look at government organizations are really interested more in Congress than in agencies, and in particular in demonstrating that agencies are relatively helpless tools of congressional domination. And the normative orientation of these scholars expresses in my view an extremely simplistic view of the proper relationship between agencies and Congress, which—following Herman Finer's view (1941, 336) in the famous Friedrich-Finer debate that “the servants of the public are not to decide their own course; . . . the elected representatives of the public . . . are to determine the course of action of the public servants to the most minute degree that is technically feasible”—suggests that any independent activity by agency officials is antidemocratic. (These scholars even use the term “shirking” taken from principal-agent theory, somewhat bizarre in this context, which appears to try to resolve a serious debate by using value-laden language.) Given the near-disappearance of empirical work or theorizing on government organizations themselves in contemporary political science, this award is one way to keep this old tradition alive

The second is that in honoring the tradition of public administration, we honor something I especially value about the best in the tradition. This is the mixture of empiricism and prescription—of holding on to a perhaps old-fashioned idea that research, at least in its choice of topics and research questions, might seek to improve the performance of government in undertaking its important

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*Steven Kelman is the Weatherhead Professor of Public Management at Harvard University's John F. Kennedy School of Government. He is the author of many books and articles on the policymaking process and improving the management of government organizations. From 1993 through 1997, Dr. Kelman served as Administrator of the Office of Federal Procurement Policy in the Office of Management and Budget. He led Administration efforts in support of the Federal Acquisition Streamlining Act of 1994 and the Federal Acquisition Reform Act of 1995. He can be reached at [steve\\_kelman@harvard.edu](mailto:steve_kelman@harvard.edu).*

missions. This was very different from my training in political science, where we were taught to keep our distance from what we were studying—indeed, I remember being amazed to learn there were political science professors who weren't interested in government and some who didn't even read a newspaper. Even Jim Wilson insisted, too much in my view, that he was trying to avoid prescription and that his findings pointed mostly to features of public management that were impossible to alter. Coming to the Kennedy School, I quickly abandoned the attitude instilled in me in graduate school and adopted a professional school's greater enthusiasm for prescription, taking as my ideal a quote—which I think many of the great scholars of public administration would endorse—from Albert Hirschman's essay "Morality and the Social Sciences" (1981) that I cited at the beginning of my book *Making Public Policy*:

It is ... possible to visualize a kind of social science that would be very different from the one most of us have been practicing: a moral-social science where moral considerations are not repressed or kept apart, but are systematically commingled with analytic argument, without guilt feelings over any lack of integration; where the transition from preaching to proving and back again is performed frequently and with ease; and where moral considerations need no longer be smuggled in surreptitiously, nor expressed unconsciously, but are displayed openly and disarmingly. Such would be, in part, my dream for a "social science for our grandchildren." (Kelman 1987)

In my lecture today, I would like to report some results from two new papers of mine, one that will be appearing next year (Kelman and Myers forthcoming) and the other just completed (Kelman, Hong, and Turbitt 2011), and to reflect on some larger points that come out of those results. The research is in different areas of public management, though both involve choices by managers about what management and leadership techniques to use in running an organization.

In one paper, my coauthor and I examine top agency executives during the Clinton and second Bush administrations nominated by outside experts for having attempted to execute an ambitious set of goals involving significant change in the organizations they led. The goal was to see if there were differences in management techniques used by successes and by a control group consisting of those who tried ambitious change but failed (along—because of a dearth of nominations in the tried but failed category—with counterparts to the successful leaders; i.e., those in the same positions at the same agency as a successful leader, except during a different presidential administration). We interviewed these executives with an open-ended question of how they sought to execute their strategy and coded spontaneous mention of different management techniques.

In the second paper (part of a larger project), my coauthors and I examine whether there are management practices associated with the effectiveness of an interagency collaboration in England and Wales, called Crime and Disorder Reduction Partnerships (CDRP's), in reducing crime. CDRP's provide a rare opportunity for empirical research on whether there are ways collaborations are managed that are associated with improved social outcomes. First, by statute, they exist in every local authority in England and Wales, so there are enough of them to permit quantitative analysis. Second, their aim is crime reduction, and crime data are available. We surveyed leaders of these collaborations and asked them to choose which of a number of management techniques were higher or lower priorities for them.

In the research on top agency executives, we found that a number of management and leadership techniques within an executive's control differentiated successful leaders from controls. The largest differences were in the use of strategic planning, performance measurement, reorganization, proactively working with interest groups, and use of slogans. Our second finding was that the small group of unsuccessful leaders used techniques specifically for managing change as frequently as successful leaders did, so use of such techniques did not differentiate successes from failures. Instead, what differentiates the groups is that failures were less likely to use techniques associated with general good management. Our conclusion, involving considerations generally ignored in the literature on change management, was that *if somebody wants to be a good change manager, they should seek to be a good manager in general*.

In the research on the interagency anticrime collaboration, we find there are indeed many management practices associated with greater success at reducing crime, mostly exhibited through interaction effects such that the practice in question is effective in some circumstances but not others.

The most dramatic finding is that the practices most consistently relating to improved performance are ones associated with what we call a "light" version of traditional hierarchical management.<sup>1</sup> So, for example, working to get partners to deliver on commitments, performance measurement, and removing "blockages" inhibiting the partnership's operational work are associated with crime reduction. By contrast, techniques proposed for managing collaborations specifically have more mixed effects. While five of the six "hierarchy light" practices tested improved crime performance in many circumstances, only two of the five collaboration-specific techniques (trust-building and developing tailored local initiatives) did so. (For example, encouraging information-sharing or equal power among partner organizations seldom helped.) Again, *to be a good collaboration manager, you should be a good manager in general*. Furthermore, three of the management techniques interacted with the local government's comprehensive performance assessment score, such that techniques for managing a collaboration worked better in an environment where single hierarchical organizations worked better. This suggests that if you want collaborations *among organizations* to be successful, you need to worry about *the health of individual component organizations* as well.

So both of these two studies, in quite different contexts, provide evidence for the view that using general good management techniques is tied to success in different specific management contexts more than is use of techniques unique to those contexts. I think these are interesting findings. I also confess that when beginning the data analysis for both papers, I wasn't expecting that these, for both papers, would be our most striking results. So when I saw the results, for example, on the work on managing interagency collaborations, I realized I needed the paper to engage the debate in the existing literature on managing such collaborations between the majority strand, which sees the management of collaborations as something new and unique, and a minority strand—illustrated by papers by David Thatcher (2004), Michael McGuire (2003), and Stephen Page (2003)—that sees successful collaborations as nascent or proto hierarchies.

I should note that by saying that skills at management in general turn out to be associated with good performance in these two different contexts, I am not saying that skills at management in

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general equals POSDCORB or a traditional, rule-bounded hierarchical bureaucracy. Successful executives in the first study were more likely to use a strategic planning process to establish goals, more likely to use performance measures in managing their organizations, and more likely to use a participative management style, than others—none of which are management behaviors particularly connected with a POSDCORB tradition.

These studies may be seen in the context of research on the topic of “management matters” undertaken by some previous winners of the Gaus Award, such as Larry Lynn, Ken Meier, and Larry O’Toole (Lynn, Heinrich, and Hill 2000; Meier and O’Toole 2002; Moynihan and Pandey 2005; Ingraham, Joyce, and Donahue 2003). This is particularly relevant in the context of the cross-agency collaboration study, since that literature has hitherto involved single organizations only.

However, as I thought about these results, it started to strike me they perhaps also have implications for a question scholars and practitioners of public administration have perennially debated: namely, to what extent public-sector management is “the same” or “different” than private-sector management. If being a good manager period is the best way to be a good change manager or collaboration manager, then perhaps being a good manager period is also the best way to be a good public manager or private manager.

Debates about this issue have a storied history going back to the beginning of the field of public administration.<sup>2</sup> Today, many associate advocacy of so-called “generic” management—a phrase used negatively by public administration scholars such as Gary Wamsley (1990), Chris Pollitt (1990), and Guy Peters and Jon Pierre (2003), though I’m not sure who invented it—with the New Public Management, and of “public administration is different” with the “traditional” values of the field. However, of course, public administration at its origins in the first part of the twentieth century had much in common with the study of business management, and what are now seen as the “traditional” values of the field represented a break, starting in many ways with Dwight Waldo, with that earlier tradition.

The founders of public administration in the first decades of the twentieth century saw the field as closely tied to the general study of management. Woodrow Wilson wrote in “The Study of Administration” (1887) that “the field of administration is a field of business.” The field’s first textbook, White’s *Introduction to the Study of Public Administration* (1926), saw “conduct of government business” as similar to “conduct of the affairs of any other social organization, commercial, philanthropic, religious, or educational, in all of which good management is recognized as an element essential to success.” Gulick, the most influential public administration scholar of the era, wrote in his essay “Science, Values and Public Administration” (1937) that “there are principles . . . which should govern arrangements for human association of any kind . . . irrespective of the purpose of the enterprise . . . or any constitutional, political or social theory underlying its creation.” Simon—yes, that is Herbert Simon—Smithburg, and Thompson’s textbook *Public Administration* (1950) stated that “large-scale public and private organizations have many more similarities than they have differences.”

The founders of the field emphasized performance as a topic of study—or, to use the idiom of that era, promotion of “economy and efficiency.” White’s text stated that “the objective of public administration is the most efficient utilization of the resources at

the disposal of officials and employees.” Gulick wrote that “in the science of administration, whether public or private, the basic ‘good’ is efficiency.” The founders of public administration were reformers, promoting good management as a means to improve government performance. White argued that growth of the state’s role in society had increased interest in “the business side of government.” He continued, “More and more clearly it is being understood that the promise of American life will never be realized until American administration has been lifted out of the ruts in which it has been left by a century of neglect.”

When Dwight Waldo published *The Administrative State* (1948), he established a counter-current in public administration that then, for decades, became the main current. He denounced the preoccupation of the founders of public administration with efficiency. And he rejected their aspirations to science, not (as with Simon’s *Administrative Behavior*, appearing around the same time in 1948) for poor execution, but rather for ignoring values, particularly the importance of democracy. He argued the founders sought expert administration, questionable from a democratic perspective, and centralized hierarchy, violating democracy at work. Waldo believed the field needed to redirect attention toward creation of “democratic administration”—greater popular participation in setting direction for agencies and greater employee participation inside them. This emphasis, of course, highlighted ways public organizations were different from private ones.

For decades, Waldo’s approach came to dominate public administration scholarship. One may speculate that it was attractive for a field traditionally close to political science but now disdained by that discipline for preoccupation with “manhole covers”: by turning to political philosophy, public administration might regain its esteem. An analysis of public administration theory (Denhardt 1990) concluded that the main change between the 1950s and the 1980s was a shift from “positivist” research on organizations to “subjective” discussions of the relationship between administration and politics. In a document growing out of the Blackburg Manifesto of the 1980s, Wamsley (1990, 24) referred to “debilitatingly irrelevant intellectual baggage” inherited from the field’s founders—“(borrowing) heavily from private-sector management techniques” rather than developing “its own theories, concepts, norms, or techniques.” It was not until the emergence of the so-called New Public Management in the 1990’s that a trend reemerged in public administration that emphasized the possible applicability of general management ideas to public management, and it is fair to say, I think, that much of this trend was driven by practitioners more than scholars (particularly in the U.S.), and that it was in general not welcomed by public administration scholars.

How should we evaluate this debate?

Clearly, public management is unlike private management in some ways. A senior executive’s interactions with Congress are qualitatively different from anything most senior private executives experience. Or, to take an operational example, rules for hiring staff in the public sector, and (perhaps except in the most-extreme unionized environment in the private sector) rules for rewards and punishment, are similarly different between public and private management. At the same time, clearly, public management is *like* private management in some ways. In both sectors, clear goals motivate employees, and functional stovepipes inside an organization make communication and cooperation harder. Furthermore, government organizations aren’t all alike

(Perry and Rainey 1988)—some are closer to private ones (Social Security Administration), others further away (Department of State).

There is a famous quip by Wallace Sayre (quoted in Allison 1983, 453) about this question to the effect that “public and private management are fundamentally alike in all unimportant respects.” I like this quip partly because it is clever—always a virtue in the academic culture. But I also like it because the word “unimportant” highlights that judgments about this question unavoidably have a significant evaluative dimension—what is *important*, and what is *unimportant*. Whether the claim about the importance of differences between public and private management is true or is not, in my view, a metaphysical or ontological question about the nature of reality, but rather a question of pragmatism in the sense advocated by William James (1978, first published in 1907) as trying “to interpret each notion by tracing its respective practical consequences,” to ask “What difference would it practically make to anyone if this notion rather than that notion were true?”

may consider being treated like the customer of a private concern to be a threat.” Some, such as Radin, are also skeptical of the “performance movement” in public management (Radin 2006) for fear that process values will be swept aside.

For others, the evaluation has to do with one’s views of the use of quantitative methods and “generic” social science in public administration research. At least since the duel between Herbert Simon and Dwight Waldo many decades ago, those emphasizing similarities between public administration and management in general have often sought to be part of a wider social science community and to use methods common among social scientists, while those emphasizing differences have tended to stay ensconced in the world of public administration and to favor a discursive, essayistic scholarly style.

I count myself, on pragmatic and not ontological grounds, as a supporter of the viewpoint emphasizing similarities between public and private management—just like the similarities between change management, or collaborative management, and management in general. And if critics of this point of view have attached

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For some, I believe, the evaluation of whether public management is fundamentally like or unlike private management seems ideological: those friendly to business often emphasize similarities, while those who are hostile tend to emphasize differences. (It should, however, be noted that some on the far right, particularly economists and “rational choice” political scientists, emphasize differences between government and business management as a way to argue that government can never, ever be expected to work properly, and see public management’s uniqueness as an argument for moving as much as possible out of government and into the private sector.)

For many public administration scholars, to argue for the view that public and private management have many similarities seems to denigrate special, unique, and treasured features of government, such as “public service motivation” among government employees (Perry and Wise 1990) or attractive ideas of democracy, fairness, and due process. Pollitt complains (1990, 7) about new public management’s “favourable analysis of the achievements of the corporate sector during the last half century.” Savoie (1994, 146) objects to “enthusiasm . . . for the merits of private enterprise.” The antibusiness tone is illustrated by a catty aside of Peters (2001, 45, 206), who appends in a footnote to his comment that supporters of use of the “customer” metaphor in public management see it as trying to provide “the same expectations of quality that they have when dealing with a private-sector firm”: “Those of us who deal regularly with airlines and Blue Cross–Blue Shield

to it the vaguely derogatory term “generic management”—which somehow suggests lower quality—I will attach the vaguely derogatory term “uniqueism”—which somehow suggests an ideological cult, like Trotskyism or Scientology or something—to the opposite view.

From a pragmatic perspective, one reason to favor the suggestion that public and general management are more similar than different is that, as an empirical matter, it is more likely that senior agency leaders will ignore the generic features of management unless reminded of their importance than that they will ignore the special features. Thus, for example, attention to one’s political environment and the organization’s operating capacity are both important. But I believe that few senior executives in government are likely to forget about the importance of their political environment, while many do forget about the importance of execution and performance. My own recent research on senior federal executives showed that almost all spent more time on managing their external environment than their internal organizations, a result corresponding with findings in other research on senior government executives (e.g., Kaufman 1981).

From a pragmatic perspective, my second—and most important—worry about uniqueism is that it erects a tariff wall shielding public sector organizations from other organizations, particularly firms, which creates problems both for scholarship and for practice. They are the same problems tariff walls generally create—behind the wall, people get lazy, because of a sort of

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monopoly protection of their domain from outside competition or comparison, and thus they don't perform as well.

This has been a problem for public administration scholarship. Separation of public administration from mainstream organization studies has resulted in the creation of a modestly sized separate public-sector organization research community behind the wall. In 2006, the Academy of Management had almost 17,000 members (Academy of Management 2006). By contrast, in 2005, the section on public administration research of the American Society for Public Administration had 355 (American Society for Public Administration 2006), and the public administration section of the APSA numbered 515 (American Political Science Association 2006).

But, behind this tariff wall, scholarly performance has suffered. None of the political-philosophy style essays by public administration scholars from the heyday of the post-Waldo era is known among actual political philosophers. And, in terms of social science-style research, public administration is only now beginning to undertake a transition to modern methods that became common elsewhere decades ago. An examination of dissertations for 1981 concluded only 42% "tested a theory or a causal statement" and 21% had research designs even "potentially valid" (McCurdy and Cleary 1984). Fewer than a fourth of articles in *PAR* between 1975 and 1984 discussed relationships among variables (Perry and Kraemer 1986). Larry Lynn (1996, 7) wrote 15 years ago that public administration had failed to "develop habits of reasoning, intellectual exchange, and criticism appropriate to a scholarly field." There has been real progress in recent years—due mainly to the work of younger scholars such as Tony Bertelli, Carolyn Heinrich, Dave Lewis, Matt Potoski, and Andy Whitford—but progress has been a long time coming, and it is notable that these young scholars rejuvenating the field generally have close ties with the outside social science world—Heinrich with economics, the others with political science.

The tariff wall also creates problems for practice—for public-sector performance. It does so in two ways.

First, uniqueism makes it more difficult to learn from the good performance of others, since the assumption is that their practices have no relevance inside one's own organization. So, say uniqueists, government has little to learn from private-sector management practices.

Second, uniqueism shields an organization from criticism for poor performance through comparison with the performance of others. So, say uniqueists, it is inappropriate to use "private sector" standards to criticize the performance of government organizations. Thus, uniqueism becomes a version of relativism, according to which one may not criticize government organizations using standards taken from the performance of organizations outside government, just as relativists argue one may not criticize the moral practices of one culture against the standards of other cultures.

If every unit is unique, none has anything to learn from others, nor can performance be unfavorably contrasted with others. One's own organization is insulated by the tariff wall from learning or from critical comparisons. For both reasons, performance will suffer.

This problem often exists even inside government, where learning from good practices in other agencies or jurisdictions—including from international experience—or even by other units within one's own organization, is hurt by the frequent claim inside

government units that "we are unique" or "if you've seen one x, you've seen one x."

Finally, behind the tariff wall are nurtured important currents in public administration doctrine that even denigrate the importance for *government* organizations of the primary standard by which organizations *in general* are judged—the standard of performance. This is particularly important because clearly there are indeed features of a government management environment that make it harder to execute and deliver results in the public sector—shorter time horizons of senior agency leadership, civil service rule constraints, a larger number and greater ambiguity (often) of goals.<sup>3</sup> There are also, of course, process values such as democracy and fairness that may compete with performance as standards by which to judge government. All of these are reasons to keep the spotlight on performance—because it is easy to forget its central feature as a standard against which to judge organizations. These are *not* reasons to underplay its importance.

However, public administration scholars frequently quote the dissent of Justice Brandeis in *Myers v. United States* (1926): "The doctrine of the separation of powers was adopted by the Convention of 1787, not to promote efficiency but to preclude the exercise of arbitrary power." Although one of course cannot draw a clear line between "politics" and "administration," it is fair to suggest that the constitutional design that sought to make quick action more difficult—thus hurting "efficiency"—was intended in the first instance for adoption of policy (in the legislature and executive branch), not for managing organizations in executing policy. It is interesting to note that the case from which Brandeis' words are taken involved a law Congress had passed requiring that Senate-confirmed postmasters could not be removed from office by the president without the approval of Congress. I can think of few today who would regard such a requirement for congressional approval before a presidential appointee could be removed to be a wise policy. Indeed, Hamilton's argument in *Federalist* No. 70 for "energy in the executive" (Madison, Hamilton, and Jay 1987, first published in 1788) endorses this distinction: "A feeble executive implies a feeble execution of the government. A feeble execution is but another phrase for a bad execution." It is absurd to suggest that our constitutional design requires that DMV employees be surly or that government information technology projects routinely come in over budget and schedule. The line of argument suggesting that "of course government is inefficient, it is supposed to be inefficient" becomes, I fear, yet another excuse, masking as an ideology, nurtured behind the tariff wall.

Let me say one more thing before I finish. Nothing that I am arguing today is meant even slightly to denigrate the special values or roles of government. As I hope my opening words made clear, these are as important to me as to most others in the public administration tradition. The journal I edit, the *International Public Management Journal*, has featured two symposia in the past two years on public service motivation, and I like to see this journal as a premier location for publishing scholarship on this topic. My view is closer to the opposite. Government's activities are worthy enough so it is of great moment that they be performed well, and we must not ignore lessons from any organization, or shield ourselves from comparison, in ways that detract from the obligation to improve government performance. Improving government performance—and good public administration scholarship in the service of improved performance—are too important to permit to languish behind tariff walls. ■

NOTES

1. "Light" in the sense that these collaborations do not have available the upfront hierarchical management techniques associated with the employment relationship of hiring staff and directing their daily performance, but only some "back-end" techniques of *ex post* follow-up.
2. This section is based on Kelman (2007).
3. I should in passing note that sometimes whatever empirical evidence exists on the extent to which these differences actually exist does not support the common assumptions (Rainey and Bozeman 2000).

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