Useful to Whom? Why We Need a Link Between Social Science Research and Public Management Research

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Big Questions

The theme of this workshop is new methodologies in public management, and I will start by saying that I intend to interpret the word "methodologies" broadly. The question I want to focus on is, "What should students of public management do?"

It seems to me that for PM research, this is a more pressing issue than any question of methodology in the narrow sense. Indeed, Behn (1995), who has argued that what the field needs is a clearer direction, argues that methodology in the narrow sense has outpaced researchers' ideas about how to use it : "Too often, the result is methodologically sophisticated research that addresses small, trivial issues." While this strikes me as far too generous an assessment, it is hard to disagree with Behn's argument that locating the research frontier is a more pressing issue in PM research than, say, a better understanding of multicollinearity. It may be true, as the Gore (1993) report says, that "if the car won't run, it hardly matters where we point it"—but the converse is true as well. If we don't know where to point it, it hardly matters how well it runs.

Behn's suggestion is that PM research—like, for example, physics—should focus on what he calls "the big questions." He proposes three such questions for public management, which he calls "Micromanagement: *How* can managers break the micromanagement cycle...," "Motivation: *How* can public managers motivate people....," and "Measurement: *How* can public managers measure the achievements of their agencies..."[1]

These questions themselves raise an obvious question: How do we know that they are big? Behn does not give a clear answer to this, and my own suggestion will have to wait until the end of this paper. For now, let us just assume that there is a consensus among researchers that these are indeed important questions.

But once we have some big questions, what next? Here is my claim: The big questions in PM research will not be answered without a firm connection to social science. In saying this, of course, I am implying that most public management research is not science. This may sound like criticism, but it is not. Science is only one of many useful and important things that people can do, and I don't accord it any more virtue than the others. Specifically, I agree that public management research is what Lindblom and Cohen (1979) call "professional social inquiry." What then makes it different from science?

Behn claims that PM research is not scientific *because* PM researchers do not know what the big questions are—that having consensus on some big questions will make public management research more scientific. I do not agree with this claim, or at least, do not understand it. His basis for this assertion seems to be that physicists and the like all know what the big questions are. I doubt that causality runs in this direction—that physicists are

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[1] Emphasis in original.

scientists because they know what the big questions are, rather than the reverse.

I would say, rather, that one thing distinguishing science from most public management research is that it is positive—it makes statements that may be either true or false, and tries to show that they are true. In contrast, PM research is very often normative—it tells managers what they should do.

Since there is some confusion about the relation between the two (Terry, 1999), it is worth expanding on this a bit. *All* normative statements have as their basis positive statements.[2] If I tell someone to wear a coat, that is based partly on the statement, which may be true or false, that it is cold outside. If I tell someone to lock the door, that is based partly on the statement that some people are inclined to steal things. If our positive statements are not true, then we will give bad advice. Moreover, social scientists give advice all the time. The reason people listen, if they do, is that they think social scientists may know some true things that they don't know.

Of course, there has been quite a lot of good positive work in public management, but most of it is basically descriptive in nature. This can be very useful. But another characteristic of scientific work in mature sciences is that it is "nomothetic"—it is aimed at the discovery of general laws or principles. This is what makes forward motion in science possible, and also what gives science much of its utility. PM research has not been very good at making nomothetic statements.

The usefulness critique

If one is drowning, of course, it is not very encouraging to be told that great progress has been made in discovering principles of better life-vest design. Positive, nomothetic PM research is thus open to the criticism that it is "not very useful" (Thompson, 1997, 486), especially to managers. Personally, I take this criticism to heart. I want my research to be socially useful, and so, I think, do most social scientists. Nonetheless, someone needs to worry about the long term. For the sake of future managers, if no one else, our research cannot be dictated by the frustrations of current managers.

But even if managers should not set our research agenda, they may still be helpful in keeping our minds focused on the question of social usefulness. In other words, public managers are probably a good source of Big Questions for public management research. Social scientists can direct their research toward questions interesting to managers today, even knowing that today's public managers may not see a lot of the benefits.

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[2] Behn (1995, 315) notes that "these three big questions are consciously prescriptive," i.e. normative. But actually they are positive questions with a very close link to normative statements.

Indeed, if we want to be useful to public managers, or at least useful for public management, a good place to begin might be to find out what managers themselves think are the Big Questions. One can imagine applying standard social-science techniques to this question. We might, for instance, survey managers about what they think their biggest problems are. To my knowledge, this has been relatively little done, except in specific contexts.[3] My guess, however, is that if we did this, some form of the three big questions proposed by Behn would be near the top of the list. Let us accept them as bona fide Big Questions for our purposes here.

The point to notice about these questions is that they cannot be answered without considerable social science research. Take in particular the two questions of Behn's that are most closely associated with New Public Management, the micromanagement question and the measurement question. It seems clear that to make normative statements in these areas will require positive statements from political science.[4] The problem of micromanagement, or of trust between elected officials and managers, cannot be understood without reference to the motivations and incentives of elected officials. It is primarily, I believe, a political rather than a technical problem. The measurement problem has substantial technical dimensions to it. But I believe that it, too, quickly bangs up against political issues: for example, when do elected officials *want* good measurements of accomplishments (Rubin, 1992)? The same can be said of many key NPM principles. Is it clear that elected officials will always want increased transparency, or a customer orientation on the part of managers?

The normative questions of Behn thus spawn counterpart positive, nomothetic questions. Instead of beginning "How can..." these questions begin "Why is.." or "How does..." Why is there a micromanagement cycle? Why don't politicians trust managers? How do politicians feel about having good measurement of achievements? Why do they feel that way? These questions are what we might call Medium-Big Questions.[5]

I think that it is very unlikely that we will make much progress on answering the Big Questions without first answering the Medium-Big Questions. Unless we are extraordinarily lucky, the positive foundations must be there to be able to answer normative questions. And the more we want to make statements that are useful across geographic and temporal boundaries, the less we can rely on luck. But the Medium-Big Questions approach is clearly different from much PM-related social science, which lets research directions be determined by interests of the larger social-science field. This is an approach that is

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[3] See, for instance, Elling (1986).

[4] In contrast, the motivation question would appear (although this could be wrong) to require positive statements primarily from psychology, and much less from political science.

[5] This doesn't strike me as a very good name; I welcome suggestions.

focused on finding solutions to important public management problems. If it answers big questions in other areas, so much the better.

Returning to New Public Management, it seems to me that there is an obvious candidate for a "why" question about NPM: Why do some countries have so much of it and others so little? That this is the case is a widely discussed fact: that it started in the Anglophone world (UK, Canada, New Zealand and Australia, but much less and later in the US), and has gone much further there than elsewhere. Do we have good theory of why this is?

An example

The question was brought home to me by consideration of the situation in the country where I now live, Israel. Any statements I make about it are tentative, because I do not yet know enough. But it is not a country where NPM could be said to be strong. There has been a certain amount of bottom-up reform along NPM lines, but no determined effort from the top.

This is not because the concepts are unfamiliar. Indeed, as long ago as 1989, a national unity government accepted unanimously the recommendations of the Kubersky Committee Report, which called for sweeping administrative changes (Galnoor et al., 1998). Many of these recommendations were along clear NPM lines: Spin off non-core governmental functions; decentralize personnel and budget authority; monitor results rather than procedures; set performance targets and link them to budgets. But little of the report has been implemented. The predictable internal opposition of some parts of the bureaucracy (the central control agencies) was not countered by appreciable support from the top. NPM is simply not on the political agenda in Israel to any significant extent.

Perhaps administrative reform is, in economic terms, a normal good, something that is of more interest as incomes rise, and will catch on in Israel when it equals the current economic level of the UK or Canada. Yet Israel is economically far ahead of other countries that have embraced NPM on the political level, such as Mongolia or Ghana.

Then why has acceptance been so low in Israel? There are a number of possible explanations (Schwartz, 1999). One may, for example, blame Middle Eastern culture for being hostile to administrative reform. And it is true that Israel is "Middle Eastern" in the sense that it has traditionally been renowned for its stultifying bureaucracy, and that some features of its legal system still retain Ottoman features. But in general neither Israel's politics nor its economy much resembles that of other countries of the Middle East.

Another explanation relates to the traditional focus of Israeli politics on "existential" issues, foremost among them national security. The agenda is simply overloaded (Dror, 1988). In this environment, one might wonder, who pays attention to administrative reform?

Yet this explanation seems to contradict a common explanation given for the rise of NPM in other countries: that changes were forced on countries by economic crisis. If this is true, would not a constant state of security crisis concentrate the mind in the same way? Does not management efficiency take on greater importance when the stakes are life and death?

An important reason for the lack of political interest in NPM, it seems clear, is Israel's unusual political structure. During the period of the rise of NPM around the world, Israeli government was characterized by coalitions in which smaller parties were decisive and hence extremely powerful.

The result has been that particular ministries are controlled by particular parties. For example, for almost all of the last fifteen years, the Interior Ministry has been under the control of a single religiously oriented party (known by its acronym as Shas), and for a number of years before that, under the control of a different religiously oriented party.

The religious parties had, in part, policy goals for wanting to control the Interior Ministry. The Interior Ministry has considerable control (at least of the residual, discretionary kind) over who becomes a citizen, an issue which in Israel has a religious component. For that reason, control of this specific ministry became an issue in the recent election campaign, with an immigrant-based party campaigning—under the slogan "Our control, not Shas control"—on the demand that it be given the Interior portfolio.

But Shas also had electoral reasons for wanting the Interior Ministry. It was able to use its control of this ministry to help it build up an extremely successful political machine, in the American sense of the term. Control of the ministry gave it access to a large number of political appointments, which it was able to use for patronage. The routine operations of the ministry also brought it into contact with large number of voters to whom it was able to provide services. Since the ministry was so strongly identified with the party, voters receiving these services were apt to feel that they had received them from the party as much as the government.

To this extent, Shas had an interest in effective (if not necessarily efficient) service delivery. The conflict between patronage and effectiveness is a familiar one in political machines (see Menes, 1998, for some results on the US in the early 20th century). Certainly improved service delivery would be helpful to Shas.

But in this environment, there is little political constituency for a government-wide administrative reform. Shas, to begin with, has no interest in improved management at, say, the Agriculture Ministry, nor does the party controlling the Agriculture Ministry have an interest in the Interior Ministry. Moreover, improved transparency is probably not high on the list for either party. Even if they are able to manage their own ministries effectively, they do not need to demonstrate this to the country as a whole. They only need to impress their own constituencies.

How did this peculiar state of affairs arise? Again, we may run through historical, cultural and circumstantial explanations. But in large part it is a logical, one might almost say a mathematical, consequence of Israel's voting system. Israel has one of the strongest proportional representation systems in the world: nationwide party-list, with a very low threshold (1.5%) for representation.

As a result, small parties flourish. Yishai (1994) gives Israel's strong PR system as one reason why it has been particularly hospitable to what she calls "interest parties." An interest party "assumes the name and activities of a party yet remains focused on the quest for private benefits and/or on a single issue." Such parties exist even in Westminster countries with single-member district voting, but one does not find them holding important cabinet portfolios there.

The situation was recently worsened by a well-intentioned but perverse reform in 1992, providing for direct election of the prime minister, which gave voters an unfortunate incentive to vote sincerely rather than strategically in choosing a party. The latest election had 31 parties competing, of which 15 won seats in parliament. But numerically, the situation before the direct-election law was similar—the very first Israeli parliament had 12 parties in it, and this has been the average since then—although the large parties have become much smaller since the law took effect.

A conjecture

What are the implications of all this for public management research? My assertion is that changing the Israeli electoral system would change the structure of government. Changing the structure of government would change the incentives of elected officials. Changing the incentives of elected officials would change the likelihood of management reform.

The generalization to other countries is not clear at this point, but of course it is noteworthy that the countries where NPM was strongest are, or were, Westminster countries where coalitions are not the norm. In these countries, that is, a single party may hold all the ministries, and the incentives for reform may be quite different. (The exception that proves the rule is the United States, which is Anglophone without being Westminster, and where NPM at the federal level has lagged behind other English-speaking countries.[6])

My conjecture, then, is that to answer the quintessential Medium-Big Question about NPM—why there is a lot in some places and only a little in others—we need to consider, among other things, voting systems. This idea may not be welcomed by PM researchers. When there are so many management issues that are poorly understood, why should we be looking at voting systems? There is a reasonably large group of political scientists who are doing this already.

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[6] Italy and New Zealand, both of which have recently changed voting systems, may be interesting case studies.

The problem is that it is hard to get political scientists to pay attention to public management. Students of voting systems are not blind to the larger implications of their work. For example, Myerson (1993) makes a theoretical argument that when voting systems favor minority representation (as do proportional representation systems) there is an incentive for candidates "to create special interest groups and minority conflict even when it would not otherwise exist." This sounds as though it could well be an important insight about Israel. But the next step of going from political conditions to management conditions has gotten very little attention. Moe and Caldwell (1994) argue that parliamentary systems will in general have more effective bureaucracy than separation-of-powers systems, but they appear to have in mind mainly Britain and the US, and I am not aware of any systematic follow-up of this assertion. I nominate this as another Medium-Big Question for public management researchers.

The good news is that public management researchers may not need to become experts on voting systems. If there is a clear link from voting systems to political systems, and from political systems to management, then in principle political scientists could start at one end and PM researchers at the other, and meet in the middle, the way the transcontinental railroad was built in the US.

Faulty ties

Unfortunately, as things are going now, the two tracks will end up missing each other by hundreds of kilometers. They are not aiming at the same destination. The public management literature has taken the problem of the link between political structure and management as a normative, not a positive, problem.

Some of this literature has been quite thoughtful and intelligent, especially given that its authors were handicapped by the use of such blunt-edged and floppy conceptual tools as "accountability." The controversies over NPM are a good place to see the range of attitudes in the literature. Some have sharply criticized NPM for undermining traditional modes of bureaucratic accountability. Thus Moe (1994, 118) asserts, "The net result of the Gore Report... will be a government much less accountable to the citizens for its performance."

Others have argued that accountability needs to be understood more broadly than in the traditional bureaucratic model. Barberis (1998) argues that New Public Management requires a "new accountability." Stone (1995) distinguishes among five forms of accountability, all of which, he argues, are relevant to Westminster-type governments today. DeLeon (1998) similarly attempts to develop a four-way typology of accountability, with contingent prescriptions: "...different accountability mechanisms are appropriate in different circumstances, depending on an organization's structure, which is in turn dependent (at least in part) on the type of problems it is designed to handle."

Public management theorists, then, have given thought to the articulation between political structures and administration. They do not necessarily confine themselves to traditional control mechanisms; for example, Stone (1995) discusses the market as an accountability mechanism.

But what seems to me to be lacking here is consideration of the positive dimensions of political control.[7] Oddly, it is public administration theorists, and not, say, rational-choice theorists in political science, who have implicitly adopted the "economic" assumption that the most efficient institutional form will be chosen.

Political scientists do not assume this. They see politicians as often having reelection interests that conflict with what informed voters would want, paying more attention to interest groups than to voters in general, and so on. Fiorina (1985), for example, argues that reelection-minded politicians will tend to dislike policies with hidden benefits and visible costs to constituents. He goes on to claim that if it is necessary to impose higher costs in such a situation, politicians will prefer to delegate. In such cases, will politicians want tight lines of accountability? Might they not prefer to be bypassed, and sacrifice accountability for deniability?

Standpoint epistemology

Contact between the two literatures, then, has been impeded by the fact that one is predominately positive and the other predominately normative. But the lack of interaction between the two is not just attributable to this. Another problem is what we might call standpoint. Public management researchers tend to look at matters from the standpoint of a public managers. This is quite natural; most of us have either worked in the public sector or have at least talked to a lot of managers, so we tend to look at problems from that point of view.

Political scientists, however, seem to have a natural tendency to look at problems from the standpoint of politicians. From this standpoint there is one dominant issue: getting reelected. This was a controversial idea in political science a few decades ago (Mayhew, 1974) but it is now the standard assumption in formal models. (I personally believe it to be one of the most realistic assumptions in the social sciences.) In making this assumption, of course, we need not believe that politicans are particularly venal. Rather, politicians are concerned about reelection because the system is designed that way; those who have different motivations quickly disappear from the sample. This is in many ways a good thing, of course. But the social consequences may be unfortunate when there are severe asymmetries of information, i.e. when the public is unaware of the consequences of many decisions made by politicians.

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[7] Rosenbloom (1993) urges consideration of such aspects. But it seems to me that in his final paragraphs, he shies away from the normative implications of his argument.

The missing standpoint, of course, is the public's. It is informative to look at things more from their standpoint, both positively and normatively. Positively, it is clear that we of the public have many demands on our time—if not writing conference papers, then working, taking the kids to the dentist, and so on. It is therefore unlikely that we will able to devote a lot of time to monitoring what the government does.

Normatively, taking the standpoint of the public would help clear up a lot of confusion around ideas like "control."   From a managerial standpoint, the problem of control reduces to a problem of accountability, an upward-looking concept. Or should it be outward-looking, or perhaps sideways-looking (Stone, 1995; DeLeon, 1998)?

For political scientists, who tend to use the standpoint of politicians, the problem of control looks different. The concept of control in the political science literature is downward-looking: how do politicians prevent bureaucrats from drifting away from what politicians want them to do?

Neither of these views captures the whole normative problem as seen from the citizen standpoint. From there, the problem is, how can citizens, who have the limitations we have just seen, get what they want out of government? With this perspective, we can start to think systematically about when accountability should go in which direction, understanding that citizen monitoring is the problem we are concerned about. Do we then want better control by politicians? I think (Frant, 1996) that the answer to that is clear and unequivocal: It depends. Weak citizen monitoring and strong political control are a bad combination.

Elsewhere I have put forward the conjecture (Frant, 1997, 80) that political reform may be "a precondition for, and means to, bureaucratic reform."   The conjecture above about voting systems is a part of that conjecture, but not the only part. And neither one is what I would call a big question. From a citizen standpoint, I think the real big question, of which these conjectures are part, is the question I have just raised, "How do citizens, who have limitations of time and attention, get what they want out of government?" This, of course, is not just a question about managers. It probably did not occur to Gore (1993) at the time, though it may have since, that the phrase "good people trapped in bad systems" might also be applied to him.

But is it useful?

As I have argued above, though, big questions are not in themselves enough. They need to be related to medium-big questions, and then broken down further into hypotheses like the conjectures I have put forward here. These conjectures seem to raise the possibility that answers to the big questions won't be helpful to anyone. Suppose a given manager learns he is in an environment where reform will be very difficult. Doesn't knowing this simply breed despair?

First, as I have suggested earlier (Frant 1997) political reform does happen. Governments that have trouble delivering the goods eventually face pressure for reform, with the Soviet Union being only the most extreme example. If one accepts Hamilton's dictum in Federalist 70 that "a government ill executed, whatever it may be in theory, must be, in practice, a bad government" then it is important to understand why some governments are better-executed than others.

Second, we may find that there are exceptions to the rule, so that there is more room for maneuver than we realized. But of course, we need to know the rule before we can recognize the exceptions. Then we may be able to do a kind of political "best practice research" to try to identify the factors that made a difference. This kind of information might be "useful to managers" even in the short term. Managers as a group are not inclined to despair—they just want to be given a problem to solve. They need a full understanding of what political factors are relevant to getting change implemented. The best public managers have a sophistication about this that very few academics can equal. But most managers may benefit from the ability to systematically analyze who the winners and losers are, who has power, and so on. How do we predict what position a politician will take on a particular issue? Knowing that, what levers can be operated?

I am reluctant even to use words like "levers" lest I be accused of being an anti-democratic running dog of managerialism. Who are managers, that they should be manipulating political levers? The fact is, though, that political sophistication exists. The democratic approach would be to make it more in the public domain, and less the exclusive preserve of lobbyists and interest groups.

Conclusion

The program I am sketching here is in some ways a daunting one, but I think it holds hope of real progress in dealing with our biggest questions in public management. How do Behn's physicists actually know that they are making progress? How do they know which questions are big? The major mileposts in physics have been *unifications*. The first big breakthrough in physics was Newton's theory of gravitation, a unification of the theory of falling bodies (Galileo) and the theory of planetary motion (Kepler). That was followed by the unification of electricity and magnetism, of the electromagnetic force with the weak and strong forces, of quantum dynamics with relativity, and so on. The questions that turn out to be big are those that advance the program of unification. I think the same will be true of public management. A good goal for public management over the next few decades is a unified theory of politics and administration.

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