What Makes Public Administration a Science? Or, Are Its "Big Questions" Really Big?

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What are the appropriate basic research questions public administration must address if it is to aspire to the status of a science? It is the philosophical nature of the true sciences that those basic questions concern the essential character and origins of their core subject matters. Neumann proposes that the appropriate research questions for public administration, those at the level which defines the discipline, must concern the structure and dynamics of the public organization. Within the physical sciences, a new paradigm is emerging that views natural systems as nonlinear, complex, and "chaotic." This new view of open systems now obliges public administrators to readdress the dynamics of their own artificial systems—the public organizations. Thus, such basic questions as those which relate to organizational theory, public management, and the relationship of the public organization to its environment now need to be revisited under the concepts of complexity and chaos.

In his recent essay in the *Public Administration Review*, "The Big Questions of Public Management," Robert Behn (1995) has asked us to consider which "big questions" are to be of central importance for public management. His nomination of three suitable big questions involves more than the simple academic exercise of setting a future research agenda. Far more important is what the nature of those questions means both to public management and to the larger discipline of public administration.

Professor Behn is absolutely correct when he states that "any field of science is defined by the big questions it asks" (p. 314). By way of example, he cites certain specialized areas within the broad field of physics. The big questions in cosmology, for example, concern the nature of the Big Bang origins of the universe, while in theoretical (or particle) physics, the big questions concern the basic composition of matter and energy. Equally important, "the big questions about physics are what make it a science" (p. 314).

It is immediately apparent that Behn's big questions in physics concern either the basic nature of things or their origins. Surely, such questions are on their very face important and worthy of investigation, but what is it about them that makes them big questions? Why should the scientific character of an entire field of study or discipline be defined by such questions? Giuliano Toraldo di Francia (1981) has perhaps given the answers in his analysis of the place that the physical sciences occupy within the modern culture.

He suggests that the reason that mankind must attempt to answer big questions is philosophical, in that they point right to the core of what it means to be a thinking human being.

> The ascertainment of the factual structure of the environment in which we live constitutes *cosmology* in a broad sense. *Cosmology* has always been associated with [physics], especially today. Etymologically it refers to the *birth* of the universe. This supreme problem is certainly always present in the minds of scientists....
>
> But today very great interest is also focused on the birth of a number of much more particular systems or structures. The examples are many and varied: galaxies, stars, the solar system, mountains, animal species; hence by analogy, such cultural facts as writing, language, and so on.
>
> Why do we pose these kinds of problems? Why do we presume that such questions can have sensible answers? In my opinion, this is not merely an unjustified psychological attitude or an infantile curiosity. As we shall see in many cases, the facts themselves present connotations which lead us out of necessity to formulate those questions (p. 347).

Toraldo di Francia's facts take the form of "footprints in the sand." That is, when we observe the existence of some physical system—a universe, a galaxy, a solar system, a nation, an economy—we are compelled to ask such questions as "Why?" and "Whence?" We are moved to search for predecessor systems or initiating interactions. Such a searching represents "mature philosophical reflection" (p. 348), and it is such that gives the discipline in question the character of a science.

It may be asked whether such a search for origins or primal concepts can take place within the social as well as the physical sciences. Indeed, does the existence of "mature philosophical reflection" allow the social sciences to aspire to a place among the "real" sciences? Arguments against such an inclusion have a venerable history and perhaps have been best stated by positivists, who would define a science according to its empirical methods of observation, analysis, and proof.

The eminent physicist, Sir James Jeans (1981), has suggested that the positivist definitions for science may not even be wholly satisfactory for the traditional natural sciences. Besides being able to observe, measure, and predict, scientists wish to understand the processes of nature. Yet, however sophisticated be the methods,

> ...an understanding of the ultimate processes of nature is for ever beyond our reach; we shall never be able—even in imagination—to open the case of our watch and see how the wheels go round. The true object of scientific study can never be the realities of nature, but only our observations on nature (pp. 175-176).

Thus, the study of the human endeavor itself would constitute science, whatever be the characteristics of that endeavor.

However these larger arguments may ultimately be resolved, we must be content here with Behn's assumption that the analysis of big questions concerning origins and primal natures is as appropriate and meaningful for the social sciences as it is for the natural sciences.

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The True Nature of Our Own Big Questions

Given this auspicious introduction, we may ask then what are the big questions that Behn proposes for public management? In abbreviated form, they may be given as

- How can public managers break the micromanagement cycle?
- How can public managers motivate people?
- How can public managers measure achievement?

Within the context that Behn proposes for the nature of big questions in science, these three questions cannot really be placed in the same category.

It is not that Behn's questions are incorrect or irrelevant to the field. They are certainly questions worth pursuing actively. Rather, I am suggesting that Behn has entirely missed the nature of what constitutes big questions for the field—by at least two levels of importance.

His three questions are questions of application, not probes into the origins or basic nature of a discipline. Returning to his chosen example of the sciences, the big questions, those on the highest tier, reflect the human philosophical need to understand the nature and origins of the universe in which we live. At that level, broad theories are formulated and basic research, much of it qualitative and open-ended, takes place.

At the next lower level, basic data are gathered and specific hypotheses are formulated, tested, and either accepted or discarded; that is, the basic theories are investigated against an observed universe. The questions asked at this second tier concern the implications of the observations for the current theories, and the reverse. At a still lower level, basic research transitions to applied research. Here applications of the accepted and proven theories are made to concrete problems.

Indeed, we may carry the example of physics a bit further in this discussion. At the top tier of questions to be addressed by physicists, the theories concerning the basic nature of the universe's origins, the so-called Big Bang, are formulated. At the second tier, the remnants of that original event of creation are sought through the observation of the existing universe, and those observations are then folded back into the theory. Finally, in the third tier, the applications questions, for example those of the relative merits of the various observing platforms themselves—Hubble telescope or particle accelerator—may be asked and debated.

It is at this lowest level where Behn has strangely placed his biggest questions for public management.

Cardinal Sins

If indeed the greatest questions that can be found within the field of public management (and here we should begin to consider the broader discipline of public administration as well) concern the efficiency and effectiveness of the delivery of services, what does that say about the character of the field?

First, it may simply mean that there are no big questions in public management or public administration. Unfortunately, if that is the case, the philosophical *raison d'etre* for the discipline does not exist; that is, there are no questions relating to basic natures or origins. It would necessarily follow then that the field does not constitute a science and probably should be viewed as no more than a self-conscious adjunct to the political sciences, from which it separated earlier in this century.

A second, and for the discipline far more significant, assumption would be that there really *are* big questions, but for some reason, they are not being addressed. The cause of such neglect within any discipline may be the result of certain professional (or organizational or institutional) cardinal sins. I suggest that there are perhaps three such sins that would prevent an academic field from addressing its big questions: ignorance, fear, and hubris (or pride). These can be addressed, citing examples from other academic or professional areas.

Consider first hubris, which is really an exaggerated sense of pride or self-confidence. Influenced by such pride, the members of a discipline or a profession might believe that at one time there were big questions but that they have been satisfactorily answered and no longer need to be formally addressed.

Carl Builder (1994) has written an excellent and provocative study of the role of air power theory in the origins, evolution, and future of the U.S. Air Force. Builder shows that the big questions for the Air Force involve the nature of air power and the relationship of air power to the conduct of warfare. The immediate goal that was sought by professional airmen after Word War I, and toward which the analysis of those big questions was purposefully driven, was the creation of an independent service.

The answers that were uncovered indeed provided the necessary justification for independence; that is, it was satisfactorily "proven" that air power exercised independently of ground or sea power could be a sufficient means in itself for winning a major war. The expression of those answers, in the form of actual instruments of air power, was the creation of a force of long-range strategic bombers. A proud and elite segment of the aviation profession[1] within the Air Force by the time of American entry into World War II and, until very recently, was thus occupied by the bomber pilot.

Since the time of the creation of the separate Air Force in 1947, however, the geopolitical world has changed from an era of big power confrontation and potential global warfare, through the demise of the Soviet system, and now to an era of extreme international complexity. Motivated by pride in its separate status and in its high technology, the Air Force now refuses to reevaluate its big questions in terms of the new circumstances. The service has chosen instead to address only those questions of application that can be found at the lowest tier. Thus, there exists within the profession not a renewed debate about the basic nature of air power in a more complex world but rather a debate about the efficiency of applications, that is, about the relative merits of the various products of advanced technology—the specific weapons delivery systems. Its failure to revisit its big questions in a time of increasing joint operations with its sister services now threatens the Air Force's status as an independent military arm.

Consider next the sin of fear. Influenced by fear, the members of a discipline or a profession might realize that certain big questions remain to be addressed but refrain from doing so because they are, or believe themselves to be, under attack by outside elements. Here we may consider the field of professional meteorology.

Meteorologists, both in research and in practice, generally understand that the big questions in the atmospheric sciences concern the basic nature of the climate of this planet. In its investigations and observations of that climate, however, the profession may have created something of a dilemma for itself.

It so happens that the earth's climate is not static, nor may it be as resilient to outside insult as was once believed. In determining that the climate is not constant and can even change because of mankind's inadvertent intervention, research meteorologists have uncovered such phenomena as global warming, ozone depletion, and acid rain. Once the discussion of these potential threats to society reached the public and then the political forums, strong opposition to their further study developed. The very nature of the potential problems and their human causes suggested that their solutions might eventually require some forms of social or economic constraints. Thus the opposition that developed has not been professional but rather political.

There are now strong political pressures being exerted to reduce the basic climatological research that is being conducted at government facilities. There is also congressional insistence to reorganize the National Oceanic and Atmospheric Administration (NOAA) away from basic research in general and toward more applied research. In the name of economy, the agency may be compelled to address its third tier questions, which relate more to the efficient delivery of forecasting services, at the expense of its basic research into the nature of climate.[2]

Ignorance—The Case of Public Administration

Consider finally the sin of ignorance. Here we may explore the example of public management and public administration and finally address what might be the real big questions for the discipline.

If Behn correctly speaks for the field in ascribing big-question status to his questions of application, then I believe that the field is ignorant of what its big questions truly are (although there are probably elements of fear, and perhaps even hubris, at work as well). First, of what is the discipline ignorant?

If not Behn's questions, what might be the real big questions for public management and public administration? They must be questions at the most basic level—they must address the essential nature and dynamics of an element of the discipline that exists at

its very core. Specifically, I propose that the big questions are really:

- What is the nature of an organization? Of a "public" organization?
- How is the public organization related to its environment?
- What does it mean to manage or to administer the public organization?

These questions appear to address basic organizational theory. Must we go so deeply? Has the discipline not answered these questions to most everyone's satisfaction years ago?

Indeed, the study and analysis of the organization have been major areas of emphasis within public administration, and many academicians and practitioners appear to believe that the major questions surrounding the organization have been solved. It would seem, for example, that the workings of Weber's bureaucracy are for the most part understood. The subsidiary questions concerning the human relationships within the framework of the organization have been studied at great length by behavioralists. The field has also struck off into such directions as: leadership analysis, comparative management principles, organizational cultures, and social interactions within the organization. Organizations have been studied as machines, as systems, and as institutions.[3] All that seems remaining to be nailed down are the applications of various known principles toward the promotion of greater organizational efficiency and effectiveness.

Potential alternatives to the vertically oriented organizational structure, such as flat networks and temporary "ad hocracies," have even been explored. These and other such alternative designs were initially put forward as antidotes for the inadequacies of the traditional organizational form and for a time appeared to be riding on a perceived tide of change that would usher in some "third wave" or "post-industrial" society. In general, many of the new organizational forms have been found wanting by the public administration mainstream, and much of the luster has rubbed off the earlier promise for change and improvement.

What then is left to be studied about public organizations? Essentially, nearly everything—all over again.

New Paradigms Bring "New" Questions

Behn is, of course, correct when he asserts that the true sciences are known by the big questions that they address, but he has also left out something very significant. Equally important, and usually obscure to the layman, is the fact that among the scientific disciplines the big questions are never really completely answered. The big questions, by their very nature, are multifaceted and extend into dimensions of which we are never fully cognizant at any one time. Thus, while for a time it may seem that the major questions have been put to rest, in truth we have only produced the answers that our existing vision has enabled us to find. Eventually, in some way, the big problems transcend our current vision so that inevitably we become aware of some new realm where the old solutions are either inadequate or do not work at all.

Again, using the example of physics, scientists at one time appeared to have solved all the problems that could be perceived within the linear Newtonian universe. Yet, when the relativistic Einsteinian universe became apparent, physicists realized that the big problems had really not been resolved after all. In fact, in the early part of this century a "paradigm shift" had taken place within the science, which allowed new dimensions of the old problem set to become visible. Within those new dimensions, the old answers were not so much wrong as inadequate.

Now, in the latter part of the century, another new world view of nature appears to have great implications not only for the physical world but also for the social world as well, and organizational theory and the old rules of organizational dynamics may be far from settled after all.[4] The new paradigm is that of the nonlinear system. Under the guises of nonlinear systems theory, "complexity," or "chaos" theory, it is providing researchers with a new view of both physical and social systems.[5]

Many characteristics of the newer nonlinear systems distinguish them from those of systems within the older paradigm. Several might be considered to be important for application to the social sciences, and particularly for public administration and public management. Some of those characteristics are given in the listing which follows (while certainly not exhaustive, it does point toward reasons why the older understanding of organizations may now be inadequate):

- Complex problems require complex mechanisms of solution.
- Attention to the parts of the problem may not solve the whole of the problem.
- Nonlinear systems do not necessarily tend toward equilibrium.
- Mechanisms of positive feedback are widespread and may cause unforeseen deviation amplification.
- Complexity may develop spontaneously in a system.
- Natural complex systems contain a balance of both random and deterministic elements.
- Accurate forecasts for future states of the nonlinear system may not be attainable.

Because we should be concerned with the origins and basic nature of the new paradigm, it will be useful to briefly enlarge upon these characteristics through reference to a few of the seminal ideas that eventually resulted in the paradigm. Thus, we might begin to see how these new concepts developed, how they differ from the characteristics of the traditional linear system, and why they are important in the study of organizations at the most basic level.

Earlier, under the tenets of general systems theory, Ludwig von Bertalanffy[6] and others had attempted to replace the reductionist Cartesian view of the world with a more holistic Aristotelian view. Derived from the study of biological organisms, general systems theory emphasized the open system which is able to exchange both energy and entropy with its surroundings. Once given such an interaction with its external environment, a system would then be subject to "feedbacks" from that environment. In the traditional analysis of cybernetics, system feedbacks were seen to be predominantly negative (that is, amplification-reducing or deviation-counteracting). Healthy systems were thus seen to be homeostatic; that is, they tended toward equilibrium.

Maruyama (1963) has asserted, however, the ubiquitousness in the natural world of positive feedback (or deviation-amplifying) processes, which he considered to be examples of a "second cybernetics." Here Maruyama laid the basis for the study of nonlinear systems. The linear, or Newtonian, system exhibits perfect cause

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and effect. Inputs must equal outputs. The linear inputs must also be additive, so that their separate effects can be disaggregated at the output side.[7] Nonlinear systems are not additive, however, nor can the separate effects be readily disaggregated and ascribed to specific inputs. Effect can feed back into cause and result in amplification. Nonlinear systems are highly complex and do not yield to reductionist analyses.

A bit earlier, Ashby (1956; 207) had declared in his "Law of Requisite Variety" that "only variety can destroy variety" and later, with Conant (1970; 89), that "every good regulator of a system must be a model of that system." That is, to cope with complex environments, regulating organizations would have to contain similar complexity themselves. In matching the variety of the regulator with that of the environment, either the external variety may be suppressed or the internal variety may be enlarged. In the first instance can be found the methods of the Weberian bureaucracy, whereby the external environment is parcelled and contained and, in reduced form, attended to by specialized functions within the organization. Weick (1979) and others would rather first increase the variety within the organization and then attempt to deal with whole environments.

At about the time that Maruyama was uncovering the properties of his second cybernetics, the meteorologist Lorenz[8] was discovering the properties of "chaotic" systems. Making successive forecast runs with his basic atmospheric equations on the latest mainframe computers of the day, he found that with even minor variations of the input parameters the output forecasts were entirely different. He traced the problem to the nonlinearity of the atmospheric equations of motion, and it was his brilliant insight that natural open systems, such as the atmosphere, are highly dependent on their initial conditions. That is, the initial state of such a system must be known precisely and completely if reliable forecasts are to be made of any future state.

Within a different scientific discipline, the biologist Prigogine (1985) uncovered the properties of chaotic systems in his analysis of nonlinear thermodynamic systems that exist in states far from equilibrium. Under such conditions, the Second Law of Thermodynamics does not hold, at least not locally. The system consumes energy but does not run down. Entropy may decrease as the system's complexity increases. Thus, spontaneous order may emerge from apparent random systemic fluctuations.

In subsequent years, natural systems as dissimilar as the orbital motions of the solar system, the dynamics of the earth's weather, the electrical activities of the human brain and heart, and the ecological relationships between predator and prey species have been found to behave "chaotically." The similarity of the complex behaviors of different systems in entirely different contexts has been found to be not merely superficial at the descriptive level but rather concerns the experimental and theoretical details of their actual operation.[9]

Nonlinear Systems Theory and Public Administration

Systems theorists such as Jantsch (1980) have strongly suggested that the characteristics of chaotic and complex natural systems can be successfully extended to created organizations and social systems. Ashby (1956) had already suggested that the mechanism for such connection was by way of the principles of communications theory; that is, the act of designing or making a machine (and what is an organization, if not a "machine" created to perform social functions?) is essentially an act of communication from a "Maker" (who represents the natural world) to a "Made" (which represents a created world).[10]

During recent years, authors have proposed, even within the pages of this journal, that the tenets of chaos theory can be married with the more traditional perspectives of political science and public administration.[11] For example, Kiel (1993) has graphically analyzed the outputs of a state agency in Oklahoma and found that an underlying pattern in the time series of otherwise irregular outputs can be described in strictly chaotic terms. And why not? Why should anyone be surprised that the human organization would exhibit the same characteristics that may otherwise be found in natural biological systems? Kiel has begun a research process that has scarcely scratched the surface of the organizational dynamics that can be viewed from within the new paradigm.

What does all this mean for public administration and public management? It means that under the new paradigm the organization is again *terra incognita.* The implications for the discipline are truly profound. All the closed doors and all the apparently settled questions must now be reopened. The big questions have not been satisfactorily answered after all. At the most basic level, the implications of nonlinear and chaotic systems theories to the core definitions for the organization, its internal dynamics, and its relationships with its environment appear to demand a new exploratory analysis.

If, for example, organizations operate in much the same manner as do complex natural systems, then perhaps organizations may be shown to respond best to problem environments when they strive to purposefully mimic the dynamics of the natural system. Here we may find that new definitions for efficiency and effectiveness are in order. We may also find that new organizational designs may be devised, not under positivistic principles but rather under those of natural ecosystems. It might even be uncovered that it would be more productive not to attempt to manipulate an organization's bureaucratic structure. Rather, it might be useful to consider the hierarchy as representing the deterministic element of a chaotic system and instead attempt to balance it with an organizational function that replicates the random or stochastic elements of such a system.

In another potentially important area, it can be shown that natural nonlinear systems are linked with their environments in extremely complex ways. Analysis of these natural dynamics may reveal how organizations can be more successfully linked with their external publics. The end result might be more effective public

inputs into agency decision making. As a final example, relating specifically to organizational management, the analysis of the internal linkages of complex systems may show how organizational variety and complexity may be most efficiently enhanced, where internal barriers to communication can be found, and how they might be breached most effectively.

All That We Have to Fear Is Fear Itself

The list of potentially open questions about the very basic nature of organizations, their internal dynamics, and their operation is only limited by the ingenuity of the researcher. The first question that must be asked before such research can begin, however, is whether public administration has the intellectual pluck to address them. Here, for a bit, we should probably address the disciplinary sin of fear.

It is possible that public administrators, both practitioners and academics alike, have an inferiority complex about their chosen field of study and practice, and that they overly crave the intellectual acceptance of their work by their peers in the other professions. There also may exist the mistaken belief that, at the intellectual heart of any profession, the big questions should already have been solved and all that remains are the important and necessary, but derivative, tasks of tuning and applying those answers to practical matters. Thus, there may exist a fear that, should our colleagues in other professions find out that the big questions of our discipline have not been finally answered after all, public administration would be judged to be inferior and to lack the "scientific" status of those other fields.

This fear of discovery can only be enhanced by the gnawing realization that perhaps Dwight Waldo (1985) was right after all. Perhaps all that public administration can aspire to is the status of an "enterprise." As Toraldo di Francia would say, we could never be "creators" but only "builders"; technicians in our field but never true scientists. Indeed, does it not seem that the profession lacks a unified central academic element? Is it not possible for anyone, whatever his or her previous academic background, to attain the MPA, or even the DPA?[12]

The response to those strong, but otherwise unexpressed, fears must be that those characteristics of our discipline which we fear the most are actually its greatest strengths under the new paradigm. It is ironic that the vitality of public administration, as a natural system, is to be found in its great complexity, in its diversity, and in the heterogeneity of its membership. If public administration is to meaningfully readdress the organizational questions in the light of the nonlinear and chaotic dynamics, it will have to borrow generously from among the other sciences, both the hard and the soft, the physical and the social.

Individuals with such a variety of background and training already exist within the breadth of the profession. I can foresee, for example, how the collaboration of a wildlife biologist (who happens to be a public administrator within a state natural resources agency), a mathematician (from perhaps a department of management and budget), and a sociologist (from a department of social services) might eventually unlock the secrets of the organizational dynamics in this complex nonlinear world.

Rather than fearing the reopening of these basic questions, we should welcome it and embrace it. The solutions to the problems of more effective and eventually more efficient public service in the coming years will come from a reinvigorated basic research and the answers that it will produce.

Now, as Behn has invited, "let the debate begin." But let it take place at the proper level.

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Notes

1. For discussion of the relationships of the elites and subprofessions within a larger, or more inclusive, profession, see Mosher (1982; 120-133).
2. The conduct of the current debate concerning the restructuring of NOAA and its parent, the Department of Commerce, and indeed that of government-funded basic research in general, particularly in the politically sensitive areas of climate, the environment, and natural resources, can be found throughout the pages of recent issues of American Meteorological Society *Newsletter.*
3. The author does not intend here to provide a definitive summary of all the possible derivative study areas, along with citations, but rather to offer a shopping list that suggests the breadth of study that has developed in part from the current understanding of the structure and dynamics of the organization.
4. In view of the discussion which follows, it may be worthwhile for the reader to revisit Thomas Kuhn's (1970) seminal work on paradigm shifts. Although criticized in the intervening years, Kuhn's analyses appear to be increasingly relevant in the light of the operation of nonlinear systems.
5. Defining what is meant by "chaos" is no simple matter. Indeed,
   > ...there is still no universally accepted definition of the word *chaos.* Usually chaos (deterministic chaos) refers to irregular, unpredictable behavior in deterministic, dissipative, and nonlinear dynamical systems. It should be emphasized that chaos cannot be equated simply with disorder, and it is more appropriate to consider chaos as a kind of order without periodicity (Zeng, Piehlke, and Eykholt, 1993; 631) Gleick (1988; 4) more simply refers to chaotic systems as those which exhibit a form of irregularity that includes a hidden order.
6. For a good synopsis of Bertalnffy's ideas, see Taschdjian (1975).
7. It is precisely the additive property of linear systems that allows for the existence of most traditional scientific research. Without the capability to isolate certain variables, while holding others constant, cause and effect falls out of the statistical analyses.
8. For an excellent discussion of Lorenz's experiments at MIT during the early 1960s, see Gleick (1988; 11-31).
9. When mathematically analyzing or graphically depicting the time-dependent patterns of these systems' dynamics, researchers have noted that in all cases the patterns contain elements of both randomness and determinism. It has been suggested that the deterministic (or Cartesian) elements are related to system robustness, while the random (or stochastic) elements provide system flexibility. The balance of the two categories of systemic components can be termed healthy, while the preponderance of either may be pathological. Thus, systems which exhibit primarily random characteristics tend toward global chaos and collapse, while those wherein the deterministic elements prevail tend toward stasis and non-adaptability to change.
10. Interestingly, Ashby specifically cited the information theories of Shannon and Weaver (1963), who had earlier shown that meaning within the communications stream was a function of the blend of the random and deterministic elements within the stream.
11. See, for example, Daneke (1990) and Kiel (1989, 1993), also Chettle

(1995), diZirega (1989), Dobuzinskis (1987), Grossman and Mayer-Kress (1989), Kiel (1994), and Sestanovich (1993).

12. As an example, I offer myself. Educated as a classical scientist in physics and trained as a practical Air Force meteorologist, I had no undergraduate education in political science, administration, or even sociology. My subsequent masters degrees were in meteorology and environmental quality science. My DPA was completed at George Mason University during the summer of 1995.

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