THE 2013 JOHN GAUS LECTURE

Taking Time Seriously: Progressivism, the Business–Social Science Nexus, and the Paradox of American Administrative Reform

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In his classic book, *Reflections on Public Administration,* John Gaus (1947) wrote about the factors that he saw interacting to either increase or reduce growth in government in the United States. "I put before you," he wrote, "a list of the factors which I have found useful as explaining the ebb and flow of the functions of government." His "ecology of government" included changes in "people, place, physical technology, social technology, wishes and ideas, catastrophe, and personality." He continued, "Such [are] the 'raw material of politics' and hence of administration [and] are in themselves the raw material of a science of administration" (9).

Wait. Did he say a "science of administration"? Some of you—maybe most—are cringing at the idea that "concepts" as "soft" as those offered by Gaus can produce theory. But let us read a bit further. Gaus finished that sentence by writing: "…the raw material of a science of administration, *of that part of science which describes and interprets why particular activities are undertaken through government and the problems of policy, organization and management generally that result from such origins*" [emphasis added] (1947, 9–10). Let me say that again: "of that part of science which describes and interprets why" things happen.

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One of my arguments in this lecture is that for much too long our field has been in the process of marginalizing that last, important component of science that Gaus stresses as it relates to long-term, secular, and reciprocal trends. To counter appropriately an imbalance toward normative and prescriptive work, our field rightly joined the behavioral revolution. Our otherwise worthwhile focus on observable behavior, however, has been on linear causation, on parsimony, and on the search for independent variables rather than on interdependent secular trends as explanatory factors. It has been largely focused on predicting cross-sectional and short-term longitudinal relationships rather than on understanding the reciprocal effects of those longer-term secular forces that incorporate history, context, and contingency as explanatory variables.

I'm also going to argue that our field has taken this path largely because our two dominant methodologies today do not—in today's vernacular—"do time very well." With apologies to Richard Fenno, I call them "contextual soaking" and "statistical poking." The former is typically comprised of descriptive case studies that are largely atheoretical. The latter *does* emphasize theory building using sophisticated quantitative techniques. Yet because of data and mathematical limitations, these are incapable of analyzing the slow, reciprocal, secular, and path-amplifying variables of history, context, and contingency.

Finally, I'm going to argue that this problem is exacerbated by what—with apologies *this time* to John Gaus—we might call today's "ecology of the *study* of public administration." I want to bring this ecology to the foreground so that we are at least conscious of what the longer-term implications are of our dominant methods, career incentives, and funding mechanisms. In my judgment, by not "taking time seriously" in the sense Gaus meant it, we are unnecessarily diminishing our research and theoretical potential as a field.

I will illustrate my points by addressing a question inspired by two recent and excellent books. One is by last year's Gaus Award winner Beryl Radin (2012), titled *Federal Management Reform in a World of Contradictions.* The other is by William West (2011), titled *Program Budgeting and the Performance Movement.* The question I derived from their work and that I pose is this: Why have American administrative reform movements consistently turned to the

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Figure 1
Robert F. Durant presents the Gaus Award and Lectureship

Photo by Mike Moffa (Color online.)

latest business reforms of their days to inform public sector reform—despite persistent disappointments, outright failures, under-performance, and scandals such as those Patricia Ingraham (2005) discussed in her 2004 Gaus lecture?

Your response might be, "Well, Bob, because that's the only option available." I am tempted to "channel" Gary Wamsley and ask, "Well . . . whose fault is that?"—but I won't! So instead, let me frame the question more as a choice to make it more historically relevant. As de Tocqueville might put it, the question becomes "Why do they opt for bureaucratic administration over democratic administration?"

Bureaucratic administration emphasizes a policy-challenged public in need of bureaucratic guidance—a one-way flow of expertise from the agency to the citizen. In contrast, democratic administration envisions an educable public capable of a meaningful role in policy deliberations (Wamsley and Wolf 1996). As Gaus awardee Lou Gawthrop (1998) put it, managers become *educators* of citizens in what might be called a tutelary democracy. "Administration," said Gaus, "is ultimately education" (1947, 123). Which raises a related question for me: Why do reformers persistently do this when, paradoxically, research exists in political science and public policy suggesting that bureaucratic administration undermines citizen support for building the very in-house capacity that early progressives and their heirs find essential to good governance?

The dominate metanarrative of our founders—and our field ever since—has been straightforward: We had to build the administrative state, enhance its capacity to act, and inform it with the "logic of business" because of changes in Gaus' "ecology of government." Bureaucratic administration would create "order" and save a messy, sometimes corrupt, and always inefficient democracy from itself. Armed with strong administrative capacity informed by the business models of the day, agency experts would pursue "social efficiency." They would make policy choices that citizens would make if only they had the time and expertise. But saying, in effect, "We had to do it," offers no room for agency on the part of actors, no politically embedded choices to be made, no political tensions within movements to resolve, and no political economy of choice over time. *Case closed.*

But is it really closed? At least three alternatives to Gaus' explanation exist in an otherwise incredibly sparse literature in our field. Two of these also deny human agency to choose bureaucratic over democratic administration. Critical theorists attribute "dark," oppressive motivations to reformers and eschew any talk of the many accomplishments of bureaucratic rationality. The administrative state, with its focus on bureaucratic administration, endures because of citizens' "false consciousness" (see Farmer 2010 for a summary of this theory). A second related but distinct narrative offered by Weber (1952) applies as well. He argued that bureaucratic rationality is so powerful in controlling us that we are destined to be "imprisoned"—as he put it—in its "iron cage . . . perhaps until the last ton of fossilized coal is burnt" (181–82).

A third narrative *does* allow human agency. But it limits it to the first three decades of the twentieth century and then largely to interpersonal battles that resulted in public administration's founders partially selling out for financial support to a moneyed class bent on preserving their status by denying average citizens access to administration (Lee 2013a; Schachter 2012). As Gaus awardee David Rosenbloom (2008) points out, the politics–administration dichotomy served as a rhetorical fig leaf for the exchange of funding support. However, this historical scholarship fails to specify theoretical foundations either leading up to or continuing after "the deal" was sealed.

Clearly, the choice between bureaucratic and democratic administration was present in the early Progressive Era—and has been ever since. So why have we not taken time to invest in research that tries to sort out these questions and competing narratives on even a portion of the scale like we have with, say, public service motivation, or public–private distinctions, or red tape versus green tape, or goal ambiguity? Why doesn't the political economy that prefigures these types of issues seem to interest us much anymore? And how puzzling it is that this neglect comes amid citizen perceptions of what Johan Olsen (2004) called in his 2003 Gaus lecture a "democratic deficit" of growing and dysfunctional proportions worldwide.

Again, my answer is that our field has largely stopped taking time seriously—much as Laurence Lynn (2008) argued in his Gaus lecture that the field had abandoned its constitutional moorings after the late 1940's assault on public administration orthodoxy. I totally agree with Kenneth Meier's (2007) point in his Gaus lecture that political science has much to learn from public administration. But I also believe *we* have something to learn from political science—at least from that part of it that is still taking time seriously. Laurence O'Toole's (2010) "nodes" metaphor from his 2009 Gaus lecture captures that relationship best, from my perspective.

CONTEXTUAL SOAKING, STATISTICAL POKING, AND THE SHADOWS IN PLATO'S CAVE

We will see who we have to learn from, how, and why a bit later when we revisit the paradox question. But first, I want to set the stage for that analysis by offering a more detailed explanation of my three arguments. I owe it to you, because on their face, they may sound outrageous—perhaps even delusional!—to many of you. So I begin with a clarification: When I say that our field has lost its sense of the importance of time and historical context, I mean we have lost it in anything other than the sense of incorporating *lagged time variables* and *interaction terms* into our quantitative models. Most of you might be thinking, "No problem, Bob, that's what historians and journalists do!" But taking time seriously in the way Gaus meant it—and that I do tonight—*need not* be history or journalism. We do not have to take history as a series of unrelated events, but rather as a series of events at one point in time that affect later events and that accumulate over time to limit choices. In other words, events interact to produce what Arthur Stinchcombe (1968) called "historical causation." By this he referred to how "dynamics triggered by an event or process at one point in time reproduce themselves over time, even in the absence of the recurrence of the original event or process" (Pierson and Skocpol 2002).

Contextual soaking *could* do it (and has on occasion). But it is limited today by perverse career incentives. Its virtue is "thick" analysis, which—done well—involves time and labor-intensive triangulation of data sources. Unlike analyzing existing data sets and writing off statistical tables in testing hypotheses with clear interpretive criteria, triangulation involves extensive and painstaking archival research where research paths are unclear, sometimes serendipitous, and often emergent and filled with dead ends. Patterns emerge, if at all, gradually, sporadically, and dependent on the interpretive skills of a broadly read researcher. All this necessarily results in contextually rich—and hence, quite lengthy—manuscripts that are tough to get into journals—other than a very few specialty ones. And these typically do not rank high on impact scores—today's holy grail, despite the gamesmanship underway by some editors.

In short, it takes a great deal of preliminary research to do time well, a definite disincentive for junior faculty to undertake as tenure clocks tick. Nor are books as valued as articles in some schools or departments, especially as they become populated with economists—as is today's trend. Meanwhile, external grant funding has taken on greater weight in the evaluation of all faculty. And the kinds of sizable and prestigious grants that university leaders value the most tend to come largely from funders more excited by so-called big data than by understanding the forces that *shaped* these data to begin with. Gary King (2014) may be correct that "big data" is lessening the time and labor intensity of building social science data sets. But as he also recognizes, considerable engineering, computational, and informatics challenges lie ahead. And these are only the technological challenges!

So why have we not taken time to invest in research that tries to sort out these questions and competing narratives on even a portion of the scale like we have with, say, public service motivation, or public–private distinctions, or red tape versus green tape, or goal ambiguity?

And yet, since the 1980s, public administration scholars have largely ignored these possibilities, at least partly so as *not* to look like history or journalism. Our quest for legitimacy has occasioned a shift to quantitatively based social science methods that is especially notable in public management research. Don't get me wrong: this shift has brought huge benefits that *must—and will*—continue. But it also restricts what we study because of methodological limitations.

In particular, and as I noted, this shift in methodology does not "do time well" in the sense of being able to analyze long-term, slow-moving, secular shifts that have reciprocal effects over time. Yet in real life, we all know that these often coalesce to either prefigure, reinforce, or change the status quo at particular points in history. Thus, although advancing our knowledge appreciably on certain topics, these conventional methodologies also constrict our research questions significantly.

As Paul Pierson (2004) documents in his book, *Politics in Time*, a search for parsimony with quantitative databases has led political science to focus overwhelmingly on what he calls "tornado events"—those with short-term causes and short-term outcomes. And so have we in public administration of late, and most especially in public management scholarship. Statistical poking does extremely well in predicting a linear, instrumentally driven, and deterministic world. It can even work sometimes in a world of complex adaptive systems. But neither atheoretical contextual soaking nor theory-driven statistical poking can give us a sense for identifying "causal mechanisms" or what Lynn calls "conditional causal theory" over the kinds of extended time periods—half-centuries and centuries—necessary to deal with those secular, slow-moving, and reciprocal trends that Gaus identifies as foundational to fully understanding administrative dynamics.

In contrast, statistical poking fits *perfectly* into today's incentive structure for junior faculty. But it can also be problematic for addressing questions such as the paradox of administrative reform I put before us earlier. Again, these require longitudinal analyses of secular trends that are likely imperceptible in cross-sectional analyses and shorter-term longitudinal ones. As you know, significant data gaps and inconsistent coding in existing databases are common—even over time spans as short as 10 to 15 years. I don't even want to think about the obstacles to building them over the time spans needed for Gausian analysis if one were using, say, event history analysis.

Without question, building original, large, longitudinal data sets related to administrative issues has been done—and done well—by several of you here in this room. They have advanced our thinking appreciably. But let us be frank and talk opportunity costs. The time and effort required to build longitudinal data sets including administrative variables alone—even in the rare instances where these data exist—far exceed that of cleaning up and/or merging existing data sets, especially for those awaiting tenure. There is a reason—and an ironical one for behavioralists—that many contemporary studies of administration are based on survey data.

Again, I want to be very clear here: studies using existing data sets also have afforded rich analytical insights. They *will* continue to do so. And we *will* continue to value them. My point instead is that existing data sets afford rather narrow and defined pathways for others to follow—and then, very few pathways at that. In doing so, they push other important questions aside, because quantitative data on the scale necessary for taking time seriously are elusive.

Some of you are also probably thinking that we do incorporate interaction terms in statistical analyses, with impressive results. I could not agree more. But mathematical, practical, and interpretive limits exist on the number of interaction terms possible in an analysis. Think about the number of interaction terms necessary to handle Gaus' six ecological factors—were the data even available—and then to do so over, say, a century in some kind of interpretable way!

One other major frailty of statistical poking is minimizing the sequencing of variables. Doing so can cause us to over- or underestimate the power of a given variable at any point in time in a cross-sectional analysis. On a microscale, for example, leadership may be insignificant in terms of measured outcomes before organizational capacity reaches a critical mass, but significant afterward. Again, Pierson (2004) puts it best: it is not only *what* a variable is, but *when* it occurs in conjunction with *what other* factors that really matters. On a macroscale such as Gaus', the same caveat applies—only this time in spades.

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The quest for parsimony in explaining change has also led us to minimize the importance of the stability of events over long periods of time. Stability depends —among other things—on what historical institutionalist (henceforth, HI) scholars call path dependency, sequencing of events, and conjunctural events. Thus, with few exceptions, we seem to be where DiMaggio and Powell (1983) were in the early 1980s before they flipped the question then dominating organization theory on its head. Instead of asking, "Why do so many *different* types of organizations exist?," they asked, "Why do so many organizations *look the same*?" They, too, complained that the primary econometric techniques of their field were geared toward "explaining variation rather than its absence" (148). But ignoring stability and focusing solely on variation—at least to me—is like trying to appreciate and understand a book by only looking at the highlighted notes one writes in the margins.

Conceptually, the factors HI theorists emphasize have what Johan Olsen in his Gaus lecture called "constitutive effects." That is, decisions and implementation structures made at T_1 determine who has access and influence at T_2, which have amplifying effects in subsequent points in time. These structures allow those actors continuing access to influence future decision making. They can deny access to others, help shape any policy changes that do emerge, or at least constrain the "degrees of freedom" available to opponents when they are in power. Think FDR on Social Security or the Bush tax cuts.

Yet the long-term secular interdependence and reciprocal nature of these factors cannot be incorporated to sort out historical causation in standard quantitative analyses. Nor do most contextual soaking efforts in public administration usually look at them in terms of building conditional causal theory. In those cases, it is like trying to understand a book *without* the notes in the margins—or even the subheadings!

And these things *really* do matter. Recently, Jacob Hacker and Pierson (2010) put these concepts to work in their *New York Times* bestseller, *Winner-Take-All Politics*. Here, and in a 2012 *Presidential Studies Quarterly* article, they show us how conservatives and probusiness elements comprised a decades-long "durable policy coalition" of actors beginning in the 1980s (2012, 103). During that time, they quietly shaped regulatory and tax policy regimes to stack the deck in their favor—and against the lower and middle classes (Mettler 2011).

Their success meant that even when their opponents won elections or even passed new legislation (think the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010, Pub. L. 111–203), they had to work within the new rules of the game that placed aggressive regulation and tax increases at a political disadvantage. Certainly, attributing administrative reform to cross-sectional factors alone tells us something important. But it does so without looking at the slow-moving, secular forces that put those variables in place to begin with—that *prefigure* them—leaving us with incomplete explanations. Also, as David John Farmer (2010) puts it, focusing on shorter time spans constricts our focus to what is, not on what might have been or yet could be. This narrows our advice to practitioners and the breadth of our theorizing.

Nor is failing to do research on theoretical questions such as these because of our dominant methodologies and incentive structures a minor loss to our field. We should give serious thought to what we are doing—and what we are *losing*—when we marginalize big-picture questions of political economy. As Ira Katznelson (2013) argues, it is just as important to know how and why you got somewhere—with what tradeoffs and compromises—as to know where you wound up. Or as James March (2011) puts it, "We cannot fully understand what we are doing *now* without understanding the path by which we got here."

But I also would argue that relying only on the interpretative methodology used by Gaus is insufficient. He and others of his generation advanced our knowledge appreciably by linking politics and administration and identifying major secular trends. But what is needed today are "neo-Gausian" approaches, if you will, that take time seriously, accommodate stability and change, incorporate ambiguity and power, and offer causal mechanisms and theories suitable for testing, refinement, and debate.

A WAY OUT OF "NO WAY"

Fortunately, some of these neo-Gausian approaches already exist in the political science literature. Here I include HI, process tracing, and (less so) grounded theory. To show how this might work—and how it might add value to our field—let us return to the paradox of American administrative reform I posed earlier and examine it through an HI lens.

This might sound odd at first—that is, using the path dependency associated with HI to talk about human agency. But this is *not* the path dependency that sees frictionless determinacy; it is not the "efficient history" of neo-institutionalist economists that leads to an optimal and inevitable solution. It is the path dependency of what March and Olsen (1985) call "inefficient history." It is the HI of scholars such as James Mahoney and Kathleen Thelan (2009). They see "structured agency" within those paths, they accommodate "friction" and strategies within and across authority structures, and they see interests and power balances remaining or shifting over time to produce stability or change. Frankly, I also want to use HI because it—and the American political development (APD) scholarship it has informed—is hardly referenced in our field today, despite the obvious common interests we share in subject matter.

But let me "call the question." Why have reformers focused on bureaucratic and not democratic administration, especially given evidence of the former's negative effect on citizen support for the very lifeblood of progressivism—that is, agency capacity building?

But let me "call the question." Why have reformers focused on bureaucratic and not democratic administration, especially given evidence of the former's negative effect on citizen support for the very lifeblood of progressivism—that is, agency capacity building? For me, four major sets of slow-moving, reciprocal, and mutually path-amplifying secular trends in APD stand out as explanatory factors. These are initial conditions, the rise of a compensatory state, an emergent business–social science nexus, and the evolution of a procedural republic.

Initial Conditions

As Steven Skowronek (1982) writes, claims are first made by administrative reformers that our institutions are not up to handling whatever challenges the nation is currently facing. But to gain traction, administrative reform movements must be framed rhetorically in terms of what Sven Steinmo (2010) calls Americans' "self-conception." As such, the sequencing pattern of path dependency of administrative reforms begins with one dominant "initial condition": American exceptionalist values.

As articulated by Seymour Martin Lipset (1996), an approach-avoidance conflict with government has been at the center of Americans' attitudes since the nation's founding. Embraced at least rhetorically are limited government, individualism, community volunteerism, and faith in markets. Consequently, the cultural "default option"—and the easier "sell" politically for reformers—is to look to the best business practices of the day and to community-based partnering solutions. This is the case whether these be the War Industries Board in World War I; the National Industrial Recovery Board in the 1930s; the Planning, Programming, and Budgeting System (PPBS) in the 1960s; management by objectives (MBO) in the 1970s; partnerships and contracting in the 1980s and 1990s; or self-regulation today.

The Compensatory State

The second major and *inter*dependent secular force one identifies, and a direct result of the recurring primacy of American exceptionalist values, is the repeated reliance on what historian Brian Balogh (2009) calls a "government out of sight." Alexander Hamilton argued that to gain the fidelity of citizens to the "new" national government, they would have to see, feel, and taste what government is giving them. This would mean the *direct* delivery of goods, services, and opportunities to citizens by the federal government. Hamilton's opponents largely won out, however. They argued that allegiance was better nurtured at the time by "hiding" the national government's presence and relying instead on state and local governments, the private sector, and civil society.

Instead of a direct tax, for example, the national government relied on import tariffs throughout the nineteenth century, making tax collectors less visible by placing them, in effect, at the nation's borders and coastlines only. Instead of the federal government directly delivering goods and services to citizens, the national government incentivized what 2008 Gaus awardee Don Kettl (1987) has variously called in the twentieth century "proxy government," "third-party government," or the "contract state." As such, hiding the visible size of the federal government was hardly a twentieth-century phenomenon, as readers might infer from some contemporary scholarship (Light 1999).

Used as policy tools were federal grants, contracts, and subsidies, plus mandates. As political scientist Kimberly Johnson (2012) notes, third parties—states and localities, contractors, and the voluntary sector—delivered the preponderance of what the federal government wanted done in the nineteenth century, providing an administrative foundation for subsequent "new federalisms" in the twentieth century. And as we shall see, they continued to do so even during the rise of the administrative state.

At the same time, rather than take a direct hand in expanding commerce from the Atlantic to the Pacific, the federal government relied on subsidies and land giveaways to industry and corporate interests. In fact, the "fiction" of public–private distinctions in this sense, or of government versus business, was clear from the nation's beginning. "Publicness" (Bozeman 1987) and "networked governance" (Agranoff and McGuire 2001) go back to the colonial era (Webb 2013) and the nation's founding (Wood 2009), not to the late twentieth century—albeit on a less complex scale and for services and not just products (Johnston and Romzek 2012).

Created were the roots of what political scientist Marc Allen Eisner (2000) has called the *compensatory state*. This is a state that compensates for American's rhetorical antistatism. It does so by relying on third-party providers of goods, services, and opportunity that other national governments usually deliver themselves. In the process, the compensatory state resolves the cognitive dissonance citizens would otherwise feel. Maintained is their "self-conception"—or *self-deception*, if you will—that they are rugged individualists whose needs are met by private energy, little platoons of volunteers, and limited government.

Granted, some efforts at in-house administrative capacity-building in federal agencies occurred between 1880 and 1920, as Skowronek (1982) has shown us. This was especially true for the military as it closed down the last of the Indian wars and fought the Spanish American War and World War I. On the domestic side of such expansion, think of the Army Corps of Engineers and Bureau of Labor Statistics (BLS). Most of the growth, however, continued "out of sight"—for example, the creation of the National Bureau of Economic Research in the 1920s to supplement work done by the BLS *rather* than expand in-house capacity.

But where this growth in administrative capacity really spiraled was in the private sector. It was prompted during the Gilded Age by the need to administer the explosion of nearly 300 corporate consolidations occurring between 1883 and 1904, with a capitalization value of nearly $7 billion (Sklar 1988). This, in turn, produced the rise of a "professional class" in industry and, subsequently, professional associations representing their interests in political circles. As historian Ellis Hawley writes, this "'organizational revolution' shift [ed] [political and administrative expertise,] power and status to … financial institutions, corporate bureaucracies, and functional or occupational organizations" (1997, 5). Thus, the interaction of American exceptionalist values and its hidden-in-plain-sight strategy meant, as Leonard White (1958) wrote, that the private sector and *not the public sector* began acquiring the administrative capacity to run that state!

Put in path-dependency terms, the nation built private sector administrative capacity first and *then* built government capacity. And even then, it came only in a halting, halfway, and patchworked way during the early Progressive Era (Skowronek 1982). This helped privilege business and its administrative ideas initially, giving it an advantage that amplified its influence in government over time, as we shall see. That influence, it turn, had the reciprocal effect of reinforcing American exceptionalist faith in markets and limited government. This was true especially when the economy boomed in late nineteenth-century America, as well as after successful mobilization in two world wars that many associated with the private sector.

But Gilded Age greed, the freezing out of small business by corporate expansion, corporate–government repression of labor, and economic devastation in 1893 soon called corporate legitimacy seriously into question. On the defensive, corporations began reconstructing their legitimacy with the public. They did this by further expanding ties with a welcoming government beset by growing social needs, but with little administrative capacity to deal with them. In this instance, government chose to turn to the scientific management "expertise" that business was developing on the shop floor of industry and to expand it to society at large.

In return, as Eisner (2000, 40) points out, an enduring foundation was laid over the twentieth century for a state–corporate exchange relationship. Private interests—and I would add the voluntary or "settlement" wing of the progressive movement—were able to extract from Washington funding for more research, restrictions on market entry, higher rates of profits, subsidies, and contracts. In exchange, the private and voluntary sectors produced the administrative capacity to hide the visible size of government to meet otherwise unmet needs.

Meanwhile, a Madisonian system purposely fragmented to reflect American exceptionalist values grew more complex and opaque to average citizens during the twentieth century. As Steinmo (2010) describes, because of the Madisonian system, the American solution to problem complexity has always been *more* administrative complexity. This, in turn, affords more particularistic—rather than universalistic—benefits and tax breaks to narrow groups in subsystems, leaving the system even more complex. Replicated repeatedly are administrative coordination problems (think the Patient Protection and Affordable Care Act of 2010, Pub. L. 111–148), as well as public backlash toward the agencies trying (or not!) to put Humpty Dumpty back together again.

Thus, the interaction of American exceptionalist values and its hidden-in-plain-sight strategy meant, as Leonard White (1958) wrote, that the private sector and not the public sector *began acquiring the administrative capacity to run that state!*

The Business–Social Science Nexus

A third set of path-dependent and reciprocal secular forces propelling these trends was the striving for *legitimacy and funding* by the emerging social science, public administration, and business disciplines in the twentieth century. The animating conjuncture of historical factors that helped them realize that legitimacy was centered on what economic historian Robert Wiebe (1967) called a "search for order" amid internal strife and international threat.

Problems were too complex for ordinary citizens to handle, a theme enunciated during this era by, among others, Woodrow Wilson (1887) in his "The Study of Administration" and Walter Lippmann (1922) in *Public Opinion*. Administrators would make democracy safe for citizens by serving as "bridge builders." Stressed in the process by mainstream progressives was scientism's promise for eliminating politics and social strife. Indeed, by the late 1930s, the President's Committee on Administrative Management expanded to the world the idea of "bridge builders" in the executive branch saving democracy from itself. If *we* didn't centralize executive power and diminish the policy-making role of the legislative branch in the United States, democracy was in danger worldwide!

The quest for "objective" solutions was understandable, of course—if naïve. Politics had grown nasty amid labor unrest, spawning cries from socialists and anarchists for a major overhaul of the American political system. The nation, progressives argued, would be rescued from the "moral anarchy" raging around it by placing human affairs where they "never yet have been placed, under the control of trained human reason" (Nelson 1982, 82). Argues Barry Karl (1983), "reform progressivism" morphed into *technological* progressivism.

However, still needed to realize the progressive dream of bureaucratic administration and rationality was empirically grounded research. Into this chasm stepped the newly established social sciences and their professional associations. This, as the allure of statistical analysis took hold in industry and among politicians as a tool for calming labor–management conflicts. The American Association of Engineers was initially the dominant professional force during the first two decades of the twentieth century. But other new social science associations arose between 1900 and 1920, and they too sought legitimacy as disciplines by forging ties to government.

The APSA, for example, promoted the virtues of social science analysis, joining older associations such as the American Statistical Association, the American Economic Association, and even the American Historical Association. To them, the poverty, disease, unemployment, and social discontent spawned by industrialization and urbanization were not inevitable. They resulted from an ill-managed but improvable corporate capitalism—if only scientism were unleashed. We would then discern an "objective reality" behind the surface "noise" and "emotion" of politics that all would agree on. An epiphany on the road to Damascus through data analysis was imminent!

Was this merely a "cover" narrative to justify parochial interests? Was it a sincere (if naïve) belief that social conflict would end when statistical analysis revealed an "objective" reality? Are *we* really much different from them in our quest today in some circles for a "design science" (Shangraw and Crow 1989) or a "logic of governance" (Lynn, Heinrich, and Hill 2000)? Are we much different when we seek "best practices" without embracing Selznick's (1957) insight that a reciprocal relationship exists between administration and agency context?

Regardless, the engineering profession started it off by claiming that the inefficiencies of competition in industry could be eliminated by applying the scientific method to their operations. Moreover, the savings gained would create a rising tide lifting the boats of labor *and* their employers, thus ending this "irrational" conflict! Think Taylorism, of course—even though you know it actually inflamed labor–management conflict! Thorsten Veblen even spoke of a "directorate of engineers" leading a "revolution" to replace an older political order that "has most significantly fallen short" (Barry 1997, 266).

But if the stock of engineering fell after the revolt against Taylorism, the legitimacy of social science methodology and its professionals soared in World War I. So too did faith in the private and voluntary sectors due to their role in war mobilization. As historian Guy Alchon advises us, social scientists working inside and outside federal agencies as part of the compensatory state established their reputation as "disinterested professionals" dedicated to a "public service ideal" (1985, 10). They also created politically supportive networks with business interests—the seeds of which might best be characterized in Piersonian terms as a de facto or emergent "durable policy coalition." Combined, these developments meant that the social sciences were now tethered to corporate interests and business-supported philanthropic foundations in arguing for "nonstatist technocratic planning" (Clements 2000, 50; also see Barber 1985). And when the 1921–1923 economic recession ended, social science and something called the "new associationalism" received the credit—deserved or not.

Interestingly, Herbert Hoover—the "forgotten progressive"—led this movement. He agreed with progressives that the federal government had a role to play in addressing social problems. But he disagreed with their focus on agency experts *making* public policy. Instead, Hoover saw federal agencies playing a data-collection, analysis, and dissemination role only. They would focus social science methodology on issues and launch the results of these studies with great fanfare in Washington conferences. In this fashion, the private sector, he argued—incorrectly as it turned out—would be "stimulat[ed] ... to organize and govern itself" in the public interest (Clements 2000, 128; Herring 1936). During his eight years as Secretary of Commerce in the 1920s, he put this philosophy into practice, convening more than a thousand such conferences. Wrought overall instead of a public interest, however, were self-regulating economic cartels motivated by self-interest and propelled more by competition than cooperation.

However, still needed to realize the progressive dream of bureaucratic administration and rationality was empirically grounded research. Into this chasm stepped the newly established social sciences and their professional associations.

Nor did associationalism go away with FDR's election. His administration adopted several of the associationalist policies of his predecessor (e.g., the National Recovery Administration, which delegated power to business and associations to regulate themselves)—as have all Roosevelt's successors—along with bureaucratic administration (Hart 1994). As DiMaggio and Powell argue, "[b]ureaucratization and other forms of [organizational] homogenization emerge[d] ... [as part of a] process ... effected largely by *the state and the professions, which have become the great rationalizers of the second half of the twentieth century*" (1983, 147, emphasis added).

Granted, a visible administrative state grew. But so too did the compensatory state—to mask its visible size. With it came the further promotion of corporate-dominated subsystems. This came partly as a result of the federal government encouraging the establishment of various business associations (e.g., the National Restaurant Association) to make it easier for public agencies to engage a spiraling number of interest groups (Phillips-Fein and Zelizer 2012).

Importantly, from the perspective of durable policy coalitions, there was a reason this bureaucratic focus endured *instead of* democratic administration through the ups-and-downs of the business cycle and scandals. Thanks to the sparse and valuable historical scholarship that we do have in our field—for example, by Mordecai Lee (2013a), Hindy Lauer Schachter (2012), Camilla Stivers (2000), Alasdair Roberts (1994), and David Rosenbloom (2008)—we know how financial support of the Rockefeller philanthropies for public administration research expanded under the political cover of the politics–administration dichotomy. And we also know, mostly from historians outside our field, how a generation of German-trained economists during the years 1870 to 1890 brought the "social idea" of a more activist and inclusive state and then found their efforts attacked in a pre-tenure era by corporate funders and board members who dominated finances at US universities and cried "socialism." Writes historian Daniel Rodgers, "[i]n a polity where so much of the infrastructure of public life (libraries, parks, church edifices, and universities) came as gifts from wealthy businessmen," no less could be expected (1998, 104).

The price paid, however, was the marginalization of democratic administration; it was too politically dicey for the nascent New York Bureau of Municipal Research to take on, and it was unacceptable to the Rockefellers. This, after Frederick Cleveland forced William Allen off the board in 1914. Allen was a vocal but abrasive advocate for "efficient citizenship," democratic administration, and an activist role for public administration in social issues (Lee 2013a). For the same reasons, between 1927 and 1936, the Rockefellers' continuing largesse and public administration's quest for legitimacy cemented the demise of democratic administration for that era (Roberts 1994).

These tectonic undercurrents next got a huge boost from World War II and the Cold War. The conjuncture of the secular trends noted already with wartime contingencies and lack of government administrative capacity spawned the greatest and longest-lasting amplification yet of business associationalism and business techniques to public problems (Sparrow 1996). Created were a host of new or reorganized government bureaucracies, most especially the Department of Defense. However, as Eisner demonstrates, out of necessity, existing weak "state capacity was expanded by appending the *[administrative] capacities* of private-sector associations on to the state" (2000, 12).

With this, of course, came concerns. President Eisenhower famously warned about a military–industrial complex based on the particularistic benefits of the subsystems created. But echoes of the politics–administration dichotomy and the progressive–business–social science "rationality project" continued in the 1960s. In speeches at Yale and a White House Conference on National Economic Issues, President Kennedy evoked the language of bureaucratic rather than democratic administration: "The basic domestic issues of our time ... relate not to basic clashes of philosophy or ideology.... The fact of the matter is that most of the problems, or at least many of them, that we now face are technical problems, are administrative problems."

Then, over ensuing decades, other elements promoted by the business–social science–progressive nexus came directly *into* government agencies. First came the massive influx of social science-driven and -informed Great Society programs in the 1960s. As noted economist and participant Henry Aaron (1978) argues, however, these again produced a neo-conservative backlash and ultimately (as in Taylor's time) a questioning of social science methods as tools for designing public policy. Kettl (2006) is correct that the shift from "hard" policy aims (e.g., building infrastructure) to "soft" social policy goals (e.g., ending poverty) where demonstrating short-term benefits was nearly impossible contributed to this backlash (also see Aaron 1978). But Aaron goes further. He writes: "Social scientists, in emulation of physical scientists and mathematicians, seek simplicity and elegance, but whether the problems of social science can be solved elegantly remains unanswered" (156). He continues: "In order to reach [these goals], problems are separated into separate components that can be managed and understood" (156). In turn, "such abstraction produces theory, apparently detached from reality, that often provokes the laymen's scorn" and undermines the legitimacy of the enterprise (156).

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Likewise, the founding of the American Society for Public Administration (ASPA) in 1939 was a direct result of leaders such as Louis Brownlow and Luther Gulick wanting to create a research organization that would be more "scientific" than the then-dominant Governmental Research Association (GRA), which was primarily comprised of bureaus of municipal research. The GRA sought Rockefeller money to pay for a secretariat and central office. Brownlow convinced the foundation to refuse. He then used Rockefeller money to undercut GRA by generating a "report" (from Joseph Harris) showing that GRA-style research was not the kind of research that the new public administration needed. GRA collapsed in 1939 when Brownlow, Mosher, and senior federal practitioners (such as Harold Smith and Donald Stone) seceded from GRA to create ASPA (Lee 2013b).

Also important in promoting bureaucratic administration during the Great Depression was the Harvard Business School (HBS) (Scott 1992). Led by Dean Wallace B. Donham—and comprised of such titans as Chester Barnard (a frequent lecturer), Elton Mayo, and Talcott Parsons—the HBS was in search of its own legitimacy. The idea of teaching management skills was, after all, still novel in the 1930s. Donham and his associates saw ties to government fostered by the social science methods pioneered at HBS as the best strategy for gaining legitimacy. Not surprisingly, however, they argued that only an "elite" network comprised of *both* business and government experts was capable of dealing with the social unrest unleashed by the Depression—and by modernity itself. Built were enduring ties for HBS with government.

Over ensuing decades, these were accompanied by the PPBS, MBO, zero-based budgeting, performance management, outcomes management, reinventing government, and the President's Management Agenda. Interestingly, West (2011) finds that heirs of this durable policy coalition are now bringing PPBS to the Department of Homeland Security, Department of Defense, and National Oceanic and Atmospheric Administration. Think any of the major IT consulting firms in Washington and their campaigns to bring "enterprise management" to government. Enterprise management means treating policy areas cutting across agencies as a unified "business enterprise" connected by IT—using, of course, the consulting services that these companies provide.

Eisenhower's warning (re)materialized in the 1980s and 1990s with the market-oriented new public management (NPM). Its proponents spoke of assessing citizen satisfaction through surveys that, in my view, reduced citizenship to consumership. The NPM also paid no attention in the United States to its role in agency deliberation. But the successful "selling" of these business-inspired administrative reforms was sprinkled liberally with the language of American exceptionalist values and the reputed virtues of the compensatory state—and its expansion.

This "framing" helped advance politically a persistent drift toward, among other things, public–private hybrid organizations. Think Fannie Mae and Freddie Mac. These were justified as depoliticizing administration—much as the early progressives justified creating independent authorities. Meanwhile, treasury departments in the United States and around the world were literally turned over to Chicago-school economists who argued—at costs for which we are still paying—that regulatory jujitsu was now possible: deregulation of financial markets would actually provide greater "self-regulation" to protect the public interest. Hoover Redux, if you will.

prowess, and political access, it tends to get a rule closer to its preferences than others do.

In turn, the procedural republic contributed negatively to citizens' sense of political efficacy, as scholars such as Helen Ingram and Steven Smith (1993), as well as Joe Soss, Jacob Hacker, and Suzanne Mettler (2010), have found. The more arcane, administratively complex, and technically and legally driven agency rulemaking becomes, the more marginalized average citizens can feel and the less sense of political self-efficacy they have. Likewise, research finds that citizens receiving services through third-party actors or tax expenditures are less likely to think government is positively affecting their lives—and they have lower trust in government.

All of which makes citizens less mobilizable to support the building of agency capacity. This is, of course, a function of the compensatory state that expanded significantly during the last four decades, with the complicity of that emergent durable policy coalition that I have called here the business–social science nexus. In turn, the existence of this state not only reflects the American exceptionalist values we started with tonight, but also allocates particularistic and hard-to-coordinate benefits and costs that alienate many citizens and diminish their sense of political efficacy.

Think any of the major IT consulting firms in Washington and their campaigns to bring "enterprise management" to government. Enterprise management means treating policy areas cutting across agencies as a unified "business enterprise" connected by IT—using, of course, the consulting services that these companies provide.

Next came the "Washington consensus" on administrative reform internationally, with its penchant for cutting back the visible size of the public sector as an austerity measure. This decontextualized and dehistoricized "logic of discipline" (Roberts 2011)—with its subsequent citizen backlash internationally—was packaged by international consulting firms as a one-size-fits-all model—reaping huge profits for themselves in return. Along with it came the infusion of the "logic of business," "professionalism," and "checkbook" philanthropy into the nonprofit sector of the compensatory state. Although mixed evidence exists, some such as Theda Skocpol (2003) see these developments producing a diminution of the mass mobilization and civic education of everyday citizens provided historically by nonprofits.

The Rise of the Procedural Republic

The final interdependent and reciprocal secular trend I want to note briefly before closing is the rise of what political theorist Michael Sandel (1984) calls the "procedural republic." Specifically, the otherwise beneficial Administrative Procedure Act and statutes that followed in the wake of the Great Society, the "new social regulation," and the "rights revolution" produced a judicialization of rulemaking. Here, a striking—yet still preliminary—pattern is emerging in work by Cornelius Kerwin, Susan Yackee, William West, Kay Schlozman, and their various colleagues (Kerwin, Furlong, and West 2012; Schlozman, Verba, and Brady 2012). As Theodore Lowi (1969) and Gaus awardee Charles Lindblom (1977) warned, when business marshals its advantage over other organized groups in legal resources, technical and scientific

TOWARD A MORE SUSTAINABLE ECOLOGY OF THE STUDY OF ADMINISTRATION?

In conclusion, what did taking time seriously tell us here that conventional soaking and poking could not? Substantively, it offered an alternative developmental narrative—dare I say, theory—of American administrative reform. Unlike the ones I mentioned earlier—Gausian, critical theory, personality-driven, and Weberian narratives—this narrative stresses "structured agency," as well as the reciprocal, path-amplifying, and conjunctural effects of the four secular trends I mentioned. Paraphrasing Gaus' words in *Reflections*, by "describ[ing] and interpret[ing] why particular activities are [in this case "not"] undertaken through government," we help explain today's "problems of policy, organization and management generally that result from such origins" (1947, 9–10).

But the real story, at least for me, is this: correctly or incorrectly interpreted by me, 90% of the evidence I marshaled in making this argument comes from research *outside* of public administration and public management. And *that* says something about where our field is today. Moreover, this is just the tip of the iceberg. A variety of other "big-picture" questions await analyses that take time seriously after decades of neglect. These might be pursued with neo-Gausian approaches alone. And ideally, they might be pursued in a comparative context across nations, states, policy areas, or time periods—as I did in this lecture.

Alternatively, these approaches might be combined with soaking or poking to inform current research questions driving our field. Why, for example, could not these techniques be used to operationalize Laurence O'Toole's call in 2009 to take networks seriously in the United States, abroad, and internationally? The evolution and political economy of networks and how they have changed or remained the same over the twentieth century in different policy arenas could afford new avenues of research for us. Or why not use them to help account for unexplained variance in regression analyses, rather than relegate that variance to an undecipherable "error term"?

Doing so, of course, means getting beyond what David Rosenbloom calls—I say "playfully," he says "sardonically"—the "Who's got the bigger rigor?" question. It also requires some critical self-reflection by us as a field. Why have we seemingly lost interest in the political economy of public administration? Is it the methodological issues or the perverse career incentives I noted earlier? Is funding driving our research questions—much like it did the focus of the early progressives in the founding era? Will these interact to produce in our research the same kind of narrow technical focus Dwight Waldo (2006) condemned in the mid-twentieth century?

Gary King (2014) may be right that technology is putting quantitative-qualitative divides behind us. I certainly hope so. But to do so, I would have to believe that technology trumps existing norms and values. If history is a guide, however, it is just as likely that the relationship will be reciprocal and existing norms and values will shape and constrain the possibilities technology affords us. All I am asking is that we at least pause and think these questions through. And when we do, we just might create academic reward structures that recognize that we are, as Norma Riccucci (2010) says, a "postnormal science." And maybe—just maybe—our legacy will be a more robust and sustainable "ecology of the *study* of public administration" than the one we inherited.

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