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Perspectives on Implementation Research: Responses to Lester and Goggin's "Back to the Future"

Editor's Note: In the last issue of Policy Currents, James Lester and Malcolm Goggin provided a critique of the current status of implementation research, and provided their road map for revitalizing the subfield. Among the interesting arguments made was that implementation scholars can be divided into "reformers," "skeptics," "testers" and "terminators" depending on whether they believe implementation research should be continued and, if so, whether significant modifications are needed in extant implementation theories and approaches (see Figure 1).

Figure 1: A Typology of Implementation Scholars

		Continuation of Implementation Research	
		Positive	Negative
Modifications Needed	Yes	"Reformers"	"Skeptics"
	No	"Testers"	"Terminators"

Source: Lester and Goggin, 1998, *Policy Currents* 8(3):2.

As is true of nearly all valuable research contributions, the Lester and Goggin article has stimulated comments and rejoinders, some of them quite spirited. Using their typology of implementation scholars, four thoughtful replies will be published in this and the subsequent issue of Policy Currents. In this issue, Søren Winter (who is declared a "reformer" by Lester and Goggin) and Peter deLeon (a "skeptic") each provide their reactions. The next issue will contain responses by Ann Schneider and Helen Ingram (both "terminators") and Kenneth Meier (a "tester").

New Directions for Implementation Research

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In their article in the last issue of *Policy Currents*, "Back to the Future: The Rediscovery of Implementation Studies," James Lester and Malcolm Goggin present a stimulating analysis of how scholars have responded to the theoretical challenges of implementation research. They also present suggestions for the future development of this field of research. They classify implementation researchers along two dimensions, (1) whether or not they are positive about the continuation of implementation studies, and (2) whether or not they advocate modifying the conceptual or methodological approaches to implementation studies. Based on these two dimensions they define four types of implementation researchers: "Reformers," "Testers," "Skeptics" and "Terminators".

Their typology of implementation researchers makes sense. It reflects several key facts. After a wave of extreme popularity in the early and mid-1980s implementation research has been met with more skepticism. Some prominent implementation scholars have left this research area. And the annual number of publications including the term "implementation" in their title has decreased.

Fads in political science

Whether we like it or not, fads in political science research attract researchers when the field is popular and scare them away when the interest has moved on to newer frontiers. Even if a given study substantially is about implementation, scholars might now chose to call it something else in order to be considered as belonging to a new frontier of research. This is not a new phenomenon as implementation itself was a new term which arguably addresses old topics. Most researchers consider the birth of implementation research the year of 1973 when Pressman and Wildavsky published their book, *Implementation*. But several books and articles focused on the substance of implementation earlier using different terminology for which Kaufman's *The Forest Ranger* (1960) serves as a good example. Contemporary research uses other labels for aspects of implementation, such as regulatory enforcement, intergovernmental relations, and new institutionalist public policy research. Some scholars have even been doing implementation research for most of their academic career without using that label, as illustrated by regulatory enforcement research by Kagan (Bardach and Kagan 1982; Kagan 1994) and Scholz (Scholz and Wei 1986). Implementation research should be defined in terms of the type of problems addressed by the research, rather than by the labels chosen by the scholars themselves. Implementation research should not be like belonging to a kind of lodge with its own creed in terms of proper concepts, theory and methodology. If defined in terms of the problems analyzed, the "field" of implementation research would be considered much more robust than simply by counting research under the label of implementation.

The research that began under the implementation rubric in the 1970s has substantially increased our understanding of the complexity of putting policies into place and of the barriers to goal-achievement. Nonetheless, research has not lived up to the high expectations for the field when it first emerged under that label in creating a theoretical breakthrough and in providing an accumulation of theoretical insights along a common analytical framework. Given this, I agree with Lester and Goggin's suggestion that we should think of fruitful ways to rekindle implementation research that seeks to obtain these goals.

Encouraging theoretical diversity

Yet, I disagree with Lester and Goggin's quest for *the* comprehensive theory or meta-theory on implementation. They think that such a theory can be developed "by incorporating the insights of communications theory, regime theory, rational choice theory (especially game theory) and contingency theories" (Lester and Goggin 1998b:6; see also Lester and Goggin 1998a:22-24). Other scholars could add additional theoretical perspectives to this list, leading to a theoretical mish-mash plagued by inconsistent theoretical assumptions and the "too many variables problem" to which Goggin (1986) paid our attention.

Rather than looking for *the* implementation theory we should welcome diversity in both the theoretical perspectives and methodologies applied. Such diversity will give us new insights. Some of these can then be integrated into broader analytical frameworks or models (Mazmanian and Sabatier 1981; Winter 1990; Goggin, Bowman, Lester and O'Toole Jr. 1990). It strikes me as unrealistic to think that many scholars can agree on applying one common theoretical framework.

Moving away from goal-achievement

When considering what future research directions to take, we should appreciate theoretical and methodological diversity while also learning from the limitations of the past implementation research. I can offer three suggestions for future implementation research: two conceptual ones and one methodological. Conceptually, I suggest first that we redefine the dependent variable of implementation research from goal-achievement to implementation behavior, and second that we pay much more attention to explaining variation in policy outcome. Methodologically, I suggest that we rely more on statistical and comparative research designs than the traditional case-study approach to implementation research. The purpose of these suggestions is to stimulate theory development and testing.

There has been some disagreement in the literature about the term implementation and of what is the important dependent variable in implementation research. One problem is that the concept "implementation" is often used to characterize both the implementation process and the output (and sometimes also the outcome) of the implementation process. Lester and Goggin (1998b) view implementation as a "**process**, a series of subnational decisions and actions directed toward putting a prior authoritative federal decision into effect." Thereby, they reject focusing on the output of the implementation process as "a dichotomous conceptualization of implementation as simply success or failure."

While I agree that the success/failure dichotomy is problematic, I suggest that the most important focus of implementation research is not the implementation process but the output of that process in terms of implementation behavior. This is much more in line with the classic focus of public policy research being the content of policy, causes, and consequences of policy (Dye 1976). Implementation output is policy content at a much more operational level than a law. It is policy as it is being delivered to the citizens. However, we should conceptualize the output in other ways than the common success/failure dichotomy or interval.

The most common dependent variable in implementation research so far has been the degree of goal-achievement whether defined in terms of output or outcome. The first problem, however, is that goal-achievement is a fraction where output in terms of performance of the implementers or outcome in terms of effects on target population is the numerator and the policy goal is the denominator. Using a fraction as the dependent variable makes theory building problematic when different factors explain variation in the numerator and the denominator. While the policy formation process is likely to account for variation in goals, the implementation process is likely to account for variation in performance. Therefore, a theory explaining variation in goal-achievement requires a combination of three theories: a theory of goal-setting, a theory on performance, and a theory on the relation between goal-setting and performance. Even if some implementation researchers have taken steps in that direction by incorporating the character of the policy adoption process in explaining variation in implementation (Winter 1986, 1990), such a combination of three different theoretical perspectives renders the construction and accumulation of implementation theory very complex.

Pushing it to extremes, the problem is that any attempt to make generalizations about goal-achievement based on analysis of the behavior or performance of implementers is dependent on the goal variable having a certain value. The generalization may become invalid if the goal changes. Therefore, generalizations about implementation output are extremely relativistic since statements are conditioned by the goals that are formulated. It is problematic to make generalizations about implementation performance dependent on the goals formulated when policy makers are more interested in making decisions on means or instruments than goals, goals are often invented after decisions on the means have been made in order to legitimize the means adopted, and goals are not always intended to be achieved.

The second problem of using goal achievement as the dependent variable of implementation research is that such goals can be difficult to operationalize. Much has already been written in the implementation and evaluation literature about the vagueness and inconsistencies of policy goals and the difference between official and latent goals. In addition, many policies announce only goals about the effect of the policy on the target population, but they fail to specify goals or standards for the behavior of the implementers.

This is often the case in regulatory policies. For example, the Danish agro-environmental regulation has both a general objective of reducing the nitrate pollution of the aquatic environment to a certain level and has a large number of very specific rules for the behavior of farmers. But the only objective or requirement for the municipalities that are in charge of enforcing the regulation is that they inspect farms for compliance with the rules. It is hard to gauge implementation success unless we use the goals about the effect on the farmers' behavior or the changes in the physical environment as the standard. However, from the evaluation and implementation literature we also know that other factors than the implementation output may contribute to or reduce a policy effect (Rossi and Freeman 1989).

Because of the problems of using goal-achievement as a dependent variable, I suggest that instead we look for behavioral variables to characterize the performance of implementers when delivering services or transfer payments to the citizens or enforcing regulations. The aim of implementation research should be to explain variation in such performance. This requires a lot of effort in conceptualizing and categorizing the performance of implementers at the levels of both agency and individual street-level bureaucrat.

One very intriguing question is whether we can find behavioral dimensions and classifications that are universally applicable in all policy areas, or if we should generate concepts and classifications that are different from one policy area to another. Lipsky's (1980) street-level bureaucracy theory represents an ambitious attempt to offer a universally applicable set of concepts for describing the coping behavior of street-level bureaucrats in all policy areas. However, several of these coping mechanisms apply better to implementation of social than regulatory policies (Winter 1997a), and a universally applicable classification scheme may suffer from a lack of the precision which a more policy specific set of concepts could offer. On the other hand, generalizations based on very policy specific concepts and studies would have a rather narrow sphere of application.

A middle ground is to use sets of concepts that apply to very broad classes of policies. For example, I think that it is possible to develop concepts that are appropriate to classify the behavior of implementers in almost any kind of regulatory policy. May and Winter (1998, 1999) have recently developed concepts for regulatory enforcement at both agency and individual street-level bureaucrat levels. Agency enforcement choices are conceptualized as (1) tools (use of different enforcement measures: sanctions, information and assistance, and incentives), (2) priorities (whom to target and what to inspect for), and (3) effort (use and leveraging of enforcement resources). The enforcement style of individual inspectors is defined as the character of the day to day interactions of inspectors with the target group. May and Winter conceptualize and verify in a study of agro-environmental regulation in Denmark that enforcement style has two dimensions comprised as the degree of formality of interactions and the use of threats and other forms of coercion. They also identify distinct types of enforcement styles along these two dimensions (May and Winter 1999; see also Kagan 1994 and May and Burby 1998).

One advantage of creating such conceptualization and classification of the behavior of implementers is that it is well suited for testing hypotheses for explaining variation in implementation behavior across time and space. Variables from implementation theory characterizing aspects of the implementation process would be an important basis for the creation and test of such hypotheses. Another advantage of redefining the dependent variable of implementation research is that we can integrate the study of implementation much more with bureaucratic and organizational theory and thereby obtain inspiration from these research fields that have a long tradition of studying the behavior of agencies and bureaucrats (Winter 1997b). These subdisciplines can also benefit from this type of implementation research as the latter uses dependent variables that are much more policy relevant than those applied in most studies of bureaucracy or of organization theory.

Considering methodological improvements

When we come to the issue of proper methodologies for studying implementation, I agree with Goggin's (1986) suggestion of substituting statistical and comparative designs for the single case study approach that has dominated implementation research. These are ways to avoid the classical "too many variables, too few cases problem" that has plagued implementation research. Explanations are over-determined where there are long lists of variables and only one or a few cases, making it impossible to control for third variables. The desired control is, however, possible if using comparative (May et al. 1996) and statistical designs (May and Burby 1998, Gormley 1998). If we redefine the dependent variable of implementation research from goal-achievement to a behavioral variable like the policy-delivery performance of implementers, using statistical designs with a large N or comparative designs are obviously useful for explaining variation in performance.

Need for outcome studies

The final point that I want to make is that implementation scholars as well as other political scientists have paid too little attention to explaining policy outcome and to examining the relation between implementation output and outcome. As mentioned above, few implementation scholars include outcome in their implementation models or framework (Hull and Hjern 1987, Elmore 1982, Winter 1990), while most exclude it. Although it is open to discussion whether implementation research should include outcomes or not, the preceding discussion suggests that a cleaner and less ambiguous conceptualization of implementation research can be obtained if the dependent variable is defined as implementation output behavior rather than outcome. However, this does not reduce the need for studying and explaining variation in policy outcome.

We do not have a complete understanding of the policy process unless we know how target groups respond to public policies. Despite the fact that "the authoritative allocation of values for a society" (Easton 1953) and "who gets what when and how" (Lasswell 1936) are part of the most famous definitions of politics, there are very few political science studies of how citizens respond to policy. Some would say that this is the province of evaluation research. However, this field is characterized by a focus on methods while there is very little theory development, especially extremely little political science theory. Some law and society scholars have attempted to explain variation in compliance among citizens and to a lesser degree firms. To date, very few political scientists and public policy researchers have tried to theorize and test hypotheses about variation in outcome and how implementation behavior affects outcomes. The contrast between the many studies of citizens' attitudes and behavior at the input side of politics and the very few outcome studies is striking. The study of outcomes is as much, if not more, about policy than are most public opinion studies that relate to the input side of policy.

My suggested redefinition of the dependent variable of implementation research from goal-achievement to a behavioral performance variable has not only the advantage of making it easier to explain and generalize about variation in implementation, but it is also likely to make it much easier to study the relation between implementation performance and outcome (May and Winter 1998). In such studies implementation performance at the delivery level shifts from being a dependent variable in implementation studies to being an independent variable in outcome or impact studies.

When implementation research was first identified, it was called the *missing link* in public policy research (Hargrove 1975). Later on, the study of policy design and policy instruments was identified as a missing link between policy formation and implementation (Elmore 1987, Linder and Peters 1989). It is now appropriate to turn to the study of policy outcomes as a remaining missing link in understanding the policy process. If we get back to the classical questions of public policy research (Dye 1976), the delivery level behavior of implementers is policy at its most operational level, policy design and instruments as well as the implementation process are important causes of such policies, and outcomes are the consequences of policy which we should not ignore.

In conclusion, the development of implementation theory can be stimulated by redefining the dependent variable from goal-achievement to a behavioral variable measuring the performance of implementers, allowing diversity in the theoretical perspectives applied in implementation research, relying more on statistical and comparative research designs, and finally, implementation researchers and other political scientists should pay more attention to explaining outcomes.

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Cold Comfort Indeed:
A Rejoinder To Lester And Goggin

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In a recent issue of *Policy Currents*, James Lester and Malcolm Goggin suggested that, like many social scientists, policy implementation scholars were divided, matrix-wise, into four camps — the reformers, the skeptics, the testers, and the terminators (Lester and Goggin, 1998). They categorized me as a skeptic, whom they characterize as "quite negative about [the] continuation of the study of implementation as it is currently conducted" (Lester and Goggin, 1998, p. 2). I feel somewhat concerned, maybe even responsible for this rating (at least partially) because my implementation paper presented to the 1997 meeting of the American Political Science Association was said by author Lester to have motivated their "Back to the Future" paper.[1] The *Policy Current*'s editor has invited me, as a representative of the skeptic clan, to respond to Drs. Lester and Goggin's comments.

In searching for the appropriate metaphor for riposte, I turned to Ms. Flora Poste, the post-Victorian and dauntless heroine of *Cold Comfort Farm*,[2] for a number of reasons, the least of which is that Ms. Poste describes herself as a "skeptic."[3] The young Ms. Poste, upon being orphaned, goes to live with distant (and admittedly peculiar) relatives in Sussex, in Cold Comfort Farm to be specific. She explains her mission there to her best friend as being one to bring order to the farm, for she, forever neat, is surely determined to end the higgledy-piggledy of bucolic life; in her words, "unless everything is tidy and pleasant and comfortable all about one, people cannot even begin to enjoy life. I cannot *endure messes*" (Gibbons, p. 20; emphases in original). Once there, she is introduced to the family (as it were) and the animals, including the four milk cows — Graceless, Pointless, Aimless, and Feckless. A recurring theme in the book is voiced by the aged Aunt Ada Doom, who keeps repeating the reason for her self-seclusion is that "I saw something 'narsty' in the woodshed." With Ms. Poste's activities as a guiding lambert (for, like her, academics typically abhor messes and are inevitably in other authors' barnyard refuges[4]), I would now like to offer a commentary on Lester and Goggin's essay.

To recount quickly authors Lester and Goggin's principal points, they argue that "policy implementation...is both theoretically beneficial and it has practical utility for those who must implement federal (or subnational) policies" (Lester and Goggin, p. 1). They observe the unfortunate trend in policy research to belittle, ignore, or even dismiss implementation research, claiming, to quote Sabatier and Jenkins-Smith, that it has "outlived its usefulness as a guide to policy research and teaching" (Sabatier and Jenkins-Smith, 1993, p. 1). Others, note Lester and Goggin, have substituted something called "policy design" for implementation studies, citing Schneider and Ingram (1997), again treating policy implementation as a tertiary concern. Finally, others have labelled implementation research as reflecting an "intellectual dead-end" or as "lacking in any consensual theory" (alas, *c'est moi*, 1999). Authors Lester and Goggin conclude,

> Yet, in spite of these excessive dismissals, public policy implementation continues to hold much practical interest for policymakers because it is a major stumbling block in the policy process.... We will argue...that the study of public policy implementation is very important at this particular time, that it has great practical utility, and that it is clearly not at an "intellectual dead-end" (pp. 1,3).

Lastly, they suggest, in a rather grandiose manner, that a confluence or melding of the various implementation approaches might result in a "'meta-theory' [that] may perhaps be developed" (p. 6).

Well, yes, I would not disagree in the general thrust of their apparently hopeful argument. In my article, which seemingly initiated Lester and Goggin's pique, I made it clear that I was anything *but* a skeptic of implementation as a viable part of the policy process;[5] I certainly and positively pointed out that implementation studies were too focused on policy failures and had patently overlooked the fact that every day on every level of government, policies were working, perhaps not famously in the eyes of political scientists, but, nevertheless, working, i.e., being successfully implemented. Where I am an admitted skeptic, however, is not that implementation per se is valuable but that its study in academe has largely been demonstrated to be **Pointless** and, yes, lacking in any consensual theory (read: **Aimless**). And lastly, like Ms. Poste, I view the contemporary policy implementation literature and confess, "I cannot endure messes."

Perhaps I should elaborate. I am in agreement with Steven Kelman, who after having spent a great deal of time viewing government bureaucracies from within, argues that few practitioners have gained much in the way of *systematic* policy insight from recent policy implementation literature.[6] Homilies abound, to be sure, but coherent, inclusive, and validated theories are completely lacking. Whenever *individual* case studies are used to demonstrate the contrary, they are basically isolated case studies with little if any generalizability. Alexander George (1980) has observed that President Lyndon Johnson's greatest mistake in implementing his early Vietnam strategies was trying to duplicate the successful tactics utilized by

President John Kennedy in the 1962 Cuban Missile Crisis. To suggest that a singular case study defines a theory of implementation is not only **Pointless**, it is dangerous. Whatever else policy scientists do wrong, they do — well, should — not theorize when N = 1. If I am to be drawn and quartered by authors Lester and Goggin for treating policy implementation theory as problematic (i.e., neglecting coherence and logic), I will admit to being especially skeptic as to their twin assertions of proven practical utility and theoretic rectitude.[7] Thus, I am, concurrently, right and guilty.

As to **Aimless**, one need not go much beyond their *Policy Current*s article to see the vast and amoebic array of policy implementation essays and books that, to most observers, would comprise a largely aimless wandering in search of some sort of consensus (what Lester and Goggin hold out as a "meta-theory"). What, the skeptic asks, is the consensus or common ground among "regime theory," "contingency theories of implementation" and "communications theories"? While one might prescribe the planting and blooming of a thousand flowers (on the assumption that one of the thousand might indeed be the genuine article), the helter-skelter of recent policy implementation reflects little in the way of moving toward an integrated, deductive theory for subsequent testing, to say nothing of application.

Finally, when pressed, I have put forth my own claim on **Graceless**. My APSA paper (and the subsequent article) argues for a clear-cut "bottom-up" approach,[8] with special attention being paid to the voices of the affected individuals, or what Schneider and Ingram (1997) call the "target population." No doubt that in operation this is a decidedly ungainly, possibly **Graceless** position, but it does have the rather keen advantage of possibly offering important insights as opposed to a questionable theoretic "elegance."[9] So, while maybe ungainly, it is not without the possibility of redemption. Charter schools, mothers against drunk driving, and advocacy urban planning all attest to the messy but ultimately adopted implementation stratagems.

Let me presume two assumptions: successful implementation is difficult under most circumstances; as such, there are inevitably "costs" to be incurred until one "gets it right." The question then is whether one wants to face these difficulties "up front" in a participatory mode (recognizing of course that *these* costs and disputes may also be serious and discouraging), or "after the fact," that is, when some additional damage is already done as a function of an ill-conceived implementation ploy, and perhaps the "costs" are multiplied beyond what might have been incurred by a pre-implementation accounting. That is a difficult call to make on a theoretic basis but, I contend, it lies at the root of the dispute.

In closing, we need to return to *Cold Comfort Farm*, and Aunt Ada Doom, who keeps repeating about "something 'narsty' in the woodshed." When a honest broker (some might call him a skeptic, but he could easily be a "tester" or even a "terminator") looks into the woodshed of implementation utility, there is little that strikes her as "narsty," in fact, because there is little there.[10] The constructive critic (i.e., the skeptic) tries hard to put something in the woodshed so that others may build upon it, while the traditionalist keeps insisting that there *really* is something there, if only we could see it (or if we worked a little harder or quantified more creatively).

In short, I readily admit to being the skeptic depicted by Lester and Goggin because, in fact, I want something to be in the woodshed; to take its present contents at face value — to accept **Pointless, Aimless**, and even **Graceless** as the intellectual coin of the realm — would be cold comfort indeed.

Endnotes

1. My APSA paper has been revised and is to be published as Peter deLeon, "The Missing Link Revisited: Contemporary Implementation Research" (1999-forthcoming).

2. See Gibbons (1932) for details.

3. How can one not be drawn -- indeed, hooked -- to a book that includes the following: "Dawn crept over the Downs like a sinister white animal, followed by the snarling cries of a wind eating its way between the black boughs of thorns" (Gibbons, p. 32).

4. Even Aimless, Pointless, and Graceless have their roles in this essay; Feckless is being reserved for the next book review of anything by Joe Stewart, Ken Meier, or Paul Sabatier. The sole bull, Big Business, is set aside for reasons best left to the reader's imagination.

5. Indeed, if one goes back to the early 1980s, Garry Brewer and I might be viewed as early pioneers of implementation studies in *The Foundations of Policy Analysis* (1983).

6. Helen Ingram, although portrayed by Lester and Goggin as a "terminator," would probably agree if we look at her 1990 essay on contemporary implementation research.

7. However, I should point out that skepticism is characteristic of the academy and the general history of scientific thought (see Kuhn, 1970); better the skeptic than the acolyte.

8. For examples of bottom-up implementation, see the work of Hjern (1982), Hjern and Hill (1983) and Lipsky (1980).

9. I am quick to point out that in this case, I am *not* an economist.

10. I recall that it was Gertrude Stein who once described her hometown of Oakland by saying, "there is no there there."

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Tom Anton, Brown University
John Schwarz, University of Arizona

3) Ad Hoc Reform Committee
(to consider (a) whether to separate the section presidency from the program chair, and (b) whether to offer a best paper award and/or other awards, and if so, whether to give a monetary prize)

Mary Anne Steger, Chair, University of Northern Arizona
Kenneth Wong, University of Chicago
Lenneal Henderson, University of Baltimore

President's Report:

I've spent the last five weeks working without much interruption (save for five days of theater and restaurants in New York during the holidays) constructing the panels for the APSA meetings in Atlanta next September. This is a good time to report on the outcome of those efforts and to offer a few reflections on the process.

The Public Policy section was allotted 17 panels for 1999, a drop from the 1998 lineup of 19 panels. Although we are the second or third largest organized section in terms of membership, there were at least 10 other sections with more panels. But the panel allocation is formula-driven, and our relatively smaller number reflects our fairly light average attendance at panels, lodged in the bottom quartile somewhere between the section on Undergraduate Education and Political Communication. If more people came to our panels, we would get more panels the following year.

But even if the allocation were doubled, it could hardly accommodate the demand for panel slots. I received 194 paper proposals and 19 full-blown panel proposals, the latter complete with title, chair, discussants, and, most important, coherence. The first impression, after recovering from the shock at the size of the stack of proposals that turned up in the mail during finals week, was of the vibrancy and diversity of this field. Lots and lots of people are doing interesting work. Thus began the task of making decisions. The job is alternately exhilarating (I had a lot of wonderful proposals to work with) and depressing (I had too many wonderful proposals to work with). In the end, I think we have a great array of panels, ranging from a roundtable on affirmative action to a panel on post-positivist policy analysis to a panel on policy learning in Britain. But many excellent papers and many fine scholars went into the reject pile. Here are the numbers: I accepted 5 of the 19 panel proposals, an acceptance rate of 27%, and 37 of the 194 paper proposals (19%). I was able to offer a few paper proposers poster slots, but these are clearly regarded by most people in the profession, rightly or wrongly, as second or perhaps fourth prize. The paper acceptance rate, incidentally, is lower than last year's 24%.

I have a few observations on the process. First, it is difficult to find a panel slot for the lone paper on a topic no one else is working on. Sometimes, these papers look like true pioneering forays that deserve a hearing, but finding other papers that "fit" in terms of subject matter, theory, or methodology is difficult. Sometimes I (and other section heads too) ignore this problem and construct panels that have little pretense about coherence. One thing that would help: in the future, we should require paper proposers to supply a short list of keywords on their proposal, highlighting not only the subject matter but the theoretical contribution and perhaps the methodological issues they think might be of interest to the section. That would help to bring papers together that on the face of it may not appear to have much in common.

A second observation is that APSA appears to be a younger scholar's game (I say this on the basis of the paper proposals for one year to one section). Perhaps the bulk of the profession is in fact young, or if not young in age at least of relatively recent Ph.D. vintage, but it surprised me that among paper proposers who supplied the date of their Ph.D. (a requirement on the form for individual paper proposals), only 13% had received the Ph.D. prior to 1980. In contrast, about three-quarters had received the Ph.D. after 1990 or were still in graduate school. One question I would raise is whether senior scholars are beginning to opt out of the APSA competition, preferring instead to present their work at smaller meetings. It is true that senior scholars appear to be better represented on the panel proposals, but only a few of these can fairly be accepted. Will established scholars submit to this competition in the future?

This raises a third and related observation. Over a third of the paper proposers to this section are graduate students. I accepted at least 7 of these, and many more that I could not accommodate are doing creative, interesting work that will no doubt eventually appear in the journals. Graduate students, no less than the rest of us, deserve a forum, and we ought not begrudge them whatever leg up an appearance on an APSA panel gives them in the competitive job market. But if graduate student submissions represent an increasing proportion of paper proposals, which I believe is the case, is the APSA meeting going to become predominantly a job audition for Ph.D. students? To what extent can we maintain a balance between the needs of these new members of the profession and those of more established people? I have no ready solutions to this problem, but I hope that the association, perhaps beginning with our section, can begin to think about the issue. I propose to raise the question at the business meeting of the section in Atlanta. I hope to see you all there and to hear your views.

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President
Public Policy Section

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Skewered

(A Column Dedicated to The Preservation Of Rotisserie Political Science)[1]
by Joseph Stewart, Jr.
(This issue's guest author: Carol L. Silva)

-100 points for failure to submit a skewered column

[1]See Meier, Kenneth J. and Joseph Stewart, Jr. 1992. "Rotisserie Political Science." *PS* 25(3).

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