

THE POLITICAL ECONOMIST

Newsletter of the Section on Political Economy, American Political Science Association

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Feature Essays

New Directions in the Study of Institutions

John H. Aldrich, Duke University

The “new institutionalism”, particularly the rational-choice based variety, came to be one the hottest trends in our discipline over the last quarter century. It is fair to say that it has moved from hot new trend to a deeply central part of the study of politics, especially the politics in democracies (and often well beyond). In this essay I ask two questions. First, why has this approach shown such durability (and resiliency)? And, second, what are the current questions that appear most likely to shape the near-term future of the study of democratic politics using the rational-choice based new institutionalism?

Trends in political science, like those

in society at large, tend to be short lived. It is therefore remarkable that the new institutionalism has come, stayed, and appears likely to remain a major part of the rational choicer’s, and political scientist’s, repertoire. Why the durability? There are, I believe, two basic reasons.

First, the terminology might have been new, but the study of political institutions has been fundamental to the use of rational choice theory in political science from the beginning. In what I believe to be the first specifically rational choice theoretic article published in the *American Political Science Review*,

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Congressional Studies and Political Economy

Nolan McCarty, Woodrow Wilson School, Princeton University

The simple insight of political economy is that economic and political processes are highly dependent. Throughout much of the 1970s and 1980s, the scholarly literature on the U.S. Congress provided much support for this proposition. This work demonstrated how political processes and institutions affected vast areas of economic policymaking, tackling such issues as tax policy, regulation, fiscal policy, trade protection, and others. It also demonstrated the ways in which economic interests affected political preferences and representation. Unfortunately, contemporary congressional studies has taken a rather dramatic turn away from political economy for a number of theoretical, methodological, and substantive reasons.¹

The theoretical and methodological turns were largely a by-product of the subfield’s successes. As more refined theoretical models were developed, data

proliferated, and new measurement techniques emerged, congressional scholars sought to pursue more general theoretical questions about the nature of legislative institutions. Often as these generalizations were refined, the focus became more abstract and moved further from economic policy and the ways in which economic conflicts and incentives structure political preferences. For example, consider the puzzle of why agricultural interests receive such high levels of subsidy and trade protection despite the precipitous decline in agriculture’s share of the economy and population. In helping to provide at least a partial explanation, congressional scholars focused on the observation that members from agricultural districts are over-represented on agricultural committees in a way that helped form an “iron triangle” or sub-government along with agricultural interest groups and executive agencies.² As

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A Letter From the *Editors*

During the 1980s and early 1990s, Congress scholars pioneered the application of economic theories to political institutions. These studies provided fundamental insights into the organization not just of legislatures, but of institutions more generally. Once a radical approach to the study of Congress, the “new institutionalism” is now the dominant paradigm in legislative studies.

This issue of *The Political Economist* revisits the political economic study of Congress. We asked Nolan McCarty (Princeton University) and John Aldrich (Duke University) to discuss the development of the sub-field and identify the key research frontiers for the next generation of scholarship. McCarty argues that the field could benefit from more interaction with the substantive concerns of political economy. Pressing questions of economic mismanagement and inequality can provide motivation for new investigations in legislative behavior. In turn, Congressional scholars can do more to develop the substantive implications of their research for these urgent policy issues. Aldrich argues that scholars must become more sophisticated in their analysis of the dimensionality of the policy space. While the assumption of a unidimensional policy space is common in the

field, Aldrich warns against being content with this approach. Instead, he asks us to wrestle with how the dimensionality of the policy space is defined. He also challenges the field to recognize the larger policy and institutional context of the policy process.

Also in this issue, we note the change in officers of the Political Economy Section. We welcome all new officers and prize committee members – please see the announcement on page 8 – and thank all outgoing and continuing officers for their service. New section chair Jeffry Frieden (Harvard University) acknowledges the efforts of your section officers in his inaugural “Letter from the Chair” found below.

The issue also contains a call for paper and panel proposals for the APSA 2007 meeting from new program co-chairs Irfan Nooruddin (Ohio State University) and Craig Volden (Ohio State University). We encourage section members to submit innovative proposals and keep the section vibrant.

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A Letter From the *Chair*

Dear colleagues,

It is an honor and a privilege to serve as chair of the Political Economy Section. The Section’s panels at APSA give us an opportunity to present and discuss outstanding scholarship. Section awards allow us to honor those who have made particularly prominent contributions to the field. And the Section’s outstanding newsletter provides serious substantive discussions of important issues in political economy.

This note is to introduce the Section leadership’s new (and continuing) members; a full list is appended. There are three new members of the Section’s Council: Scott Gehlbach, Beatriz Magaloni, and Cathie Jo Martin. They

join the continuing members – James Fearon, Nolan McCarty, and Layna Mosley – who are in the second year of their two-year terms. I thank Scott Adler, Burt Monroe, and Beth Simmons for their service.

Rob Franzese has agreed to serve as Secretary-Treasurer for another year, which will be a great help in ensuring continuity. William Bernhard and Lawrence Broz, who have done such an outstanding job with *The Political Economist*, have agreed to edit it through the end of this calendar year.

Beginning in January 2007, the editors of *The Political Economist* will be
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Shapley and Shubik used their own power index to study variations across political institutions (1954). Buchanan won a Nobel Prize in Economics in large part for his book with Tullock, *The Calculus of Consent* (1962). That book is about as clear an example of post 1980s new institutionalism as I can imagine, even if it predated it by nearly a generation. The reason for these and many other examples is that, in fact, rational choice applied to politics has historically been the study of institutions and their effects on political outcomes, whether that is Farquaharson's (1969) use of the letters of Pliny the Younger as illustrative of this point, or something more contemporary.

This leads to the second reason for current and likely future durability. It is a working hypothesis that the reason for this sustainability is quite the same as that laid out by Shepsle in his article that launched the contemporary study (1979): Equilibrium seemed to be hard to uncover, much harder than in, say, economics. This fact suggests that it might truly be rare if not nonexistent altogether. Or, in today's alternative version yielding the same effect, there are so many, if not an infinite number of, equilibriums that the world of (nearly or actually) infinite equilibriums looks very much like that absent any equilibriums. And it is the unpredictability and instability of a world with no or with very many equilibriums that brings rules to the forefront as a necessary component for understanding political outcomes. This point can be traced to Black's inclusion of his histori-

cal investigations of cyclical majorities (1956), such that new institutionalists may conclude, much like he did, that the first important response to cycling was to see what effect different institutional configurations might have on this problem. And that problem is one core basis for new institutionalism in political science.

So then, if the quest is ancient, what are the important new problems and trends to come? A useful way to summarize my thoughts is to structure them along the lines of Diermeier and Krehbiel, in their "Institutionalism as a Methodology" article (2003). If we think of their method as a way of describing the design of much of the new institutional work of the last, say, decade, then my comments are suggestions of what will push their research methodology in new ways.

Their method takes the position that the point of institutionalism is to assess the role of institutions in affecting political outcomes, and that the appropriate way to develop a coherent account and, even more, a coherent stream of accounts, of those outcomes from an institutional perspective is to hold virtually all else in the "fundamental equation"¹ of the new institutionalism as maintained hypothesis, so that the only "moving parts" are the institutional rules under study. Further, they argue that the technical apparatus should also be held (nearly) constant, specifically by using non-cooperative game theory. Doing so satisfies the Nash criterion of saying that non-cooperation is basic and that cooperation must

be derived rather than assumed. It also means that (due to Nash's own work and extensions) the question of the existence of equilibrium is essentially off the table, and in its place is the search for the number, location, and properties of equilibriums. I will propose two problems that need to be addressed from this perspective: Dimensionality and the complex domains of institutions and their interactions.

While there are a few alternatives, by far the most common maintained assumption about public preferences is that they are defined over policy outcomes. Thus, whatever one might assume about candidates and officeholders, and that generally is the Mayhebian office-seeking/reelection or possibly the Fennovian triad of election/reelection, good public policy, and power within the chamber, candidate and office holder preferences are induced over a common policy space (1974, 1973, respectively). In U.S. congressional studies, that is almost invariably a Poole-Rosenthal or comparable continuous space estimated from some behavioral measure such as roll call votes on the floor. As a result, the empirical space is a continuous space of low numbers of dimension. It is this fact that I will use as running example about my proposed research questions; in effect asking about the origin, understanding, and use of such a space.

The first is a theoretical point. It is conventional – and indeed obvious and very useful – to develop a model first in a

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Matthew Gabel and Randall Calvert. I am confident that they will continue the tradition of a high-quality newsletter with interesting and important content.

I have also been able to call upon a large number of colleagues to staff our awards committees for the coming year. Please let the appropriate committees know about worthy candidates for these awards. I also bring your attention to our new Section award, the Michael

Wallerstein Award for the best published article in political economy. The first Michael Wallerstein Award was presented at the Section's business meeting in Philadelphia, at the recent APSA meetings.

I am especially grateful to the outgoing chair, Elinor Ostrom, for all the work she has done over the past two years. She has been an inspiration to us with her dedication and energy, not only as

chair of the Section but over her long and distinguished career.

I look forward to working with you, and the new Section officers, to maintain the quality of the Section's activities. Feel free to be in touch if you have any suggestions, questions, or comments.

Sincerely,

Jeff Frieden

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scientific explanation, however, this committee-based explanation is incomplete. How did all those farmers get on the agriculture committee? Why does a majority of the House let a minority take resources from its constituents to give to those of the minority? In part to resolve such puzzles, Shepsle, Weingast, Marshall and others developed the distributive theory of legislative organization that argues committees system are designed to allow high-demanders to self-select onto committees to obtain the benefits of that jurisdiction in order to maximize the gains from exchange across issues.³ This model provides a general theoretical framework that might explain why preference outliers were allowed to serve on a committee with special parliamentary rights in the area of agriculture. A generalized argument is a scientific advance, but it moves the level of analysis one step away from the political economy of agriculture. The domain of study becomes *all* committees. The testing of the distributive theory's hypotheses must be conducted on composition of all committees, not just Agriculture. The original factoid about the Agricultural committee becomes just one datum among many.

Once the proposed explanation becomes a special case of a general model then it becomes subject to challenge and critique from alternative general models. When Gilligan and Krehbiel's informational model of legislative organization emerged as an alternative to distributive theory, the hypothesis about the agriculture committee must confront the possibility that members from farm regions serve on agriculture because they have the expertise or the greatest incentives to develop it.⁴ But again, the arguments are tested not only in the realm of agriculture but across the universe of all committees. As the scientific investigation continues, measurement and methodology become more increasingly important. So the analysis takes just one more step away from the substantive question as scholars struggle over the correct stan-

dard errors for committee medians under the null hypothesis.⁵

This trajectory occurred in literature after literature in congressional politics. Indeed the benefit of one of the great intellectual accomplishments of legislative studies — the marriage of the spatial model with roll call analysis and scaling — allows scholars to study legislative institutions without getting too bogged down in substantive details. Models of policymaking can be constructed and tested without specifying content of the policy outputs or taking a strong stand on where the preferences of the legislators come from.⁶ The “party versus pivots” debate provides an example. Both theories are specified in terms of an abstract spatial model where preferences and partisanship are taken as exogenous. The models lead to hypotheses that are tested on generic policy outputs (usually the aggregate level of legislative output) or the structure of voting coalitions with roll calls pooled across a vast number of substantive issues. Although the debate has been robust and intellectually challenging, it has been carried out in a policy substance-free way, and the insights of either theory have rarely been applied to economic policymaking.⁷ I believe these debates have certainly advanced congressional studies (and legislative studies more broadly), but they have done little to slow the divergence of congressional studies from the field of political economy.

The substantive forces leading to the estrangement of congressional studies from political economy are what I believe to be a set of misunderstandings about the nature of the American polity and economy.⁸ The first misunderstanding is the belief is that economic incentives and conflict are an increasingly unimportant influence on politics. Most pundits and many scholars now portray American politics as a culture war between Red States and the Blue States. The issues that supposedly define this new party system are abortion, gay rights, and stem cell research. The old

New Deal issues of taxes, spending, business regulation, and unionism are no longer believed to determine American elections. A second, but related, misconception is that economic problems in the United States cease to be as important as they once were. During the 1990s, not coincidentally when most of the new generation of congressional scholars was in graduate school, a booming economy and plummeting budget deficits simply made economic policy debates seem less relevant and intellectually stimulating. Given these beliefs, it is little surprise that congressional scholars have redirected their focus away from economic policy towards other political debates.

The intellectual shift of congressional studies away from political economy is unfortunate, however, because the relevance of the relationship between economics and politics is at least as great in the contemporary United States as it was earlier in our history or is currently elsewhere. First, evidence is mounting that economic issues are increasingly salient in American politics in general and legislative politics in particular. McCarty, Poole, and Rosenthal (1997, 2006), Stonecash, Brewer, and Mariani (2002), Fiorina et al. (2005), and Bafumi, Gelman, and Shor (2006) have all found that economic status as measured by current income is a better predictor of voter partisanship now than it was thirty years ago. Bartels (2006) and Ansolobehere, Rodden, and Snyder (2006) demonstrate that the importance of voters' economic policy preferences continues to be a primary influence on their behavior. Moreover, the economic motivations of voters continue to influence congressional behavior. McCarty, Poole, and Rosenthal (1997, 2006) find that economic issues continue to define the left-right spectrum in Congress and that the correlation between a House member's DW-NOMINATE score and the median income in her district has increased. Stonecash, Brewer, and Mariani (2002) have found similar results when

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one dimensional space, before extending it to a multi-dimensional setting. This is, of course, fine. However, all too often, the modeling stops with one-dimension. Indeed, the one dimensional model is taken as seriously as a multi-dimensional model in the literature. The fact that most dimensional estimation procedures yield something close to a one-dominant-dimension result makes it easy for the theorist to rest at one dimension, because it is “close enough” to a reasonable approximation of the “true” (if estimated) world.

The problem, of course, is that there is a fundamental difference between one and more than one dimension from a theoretical perspective. Prior to the “Nash revolution” in game theory shaping the Deirmeier-Krehbiel approach in the new institutionalism, this difference generated the dominant questions in the field, and many (myself included) sometimes address questions from this, let’s call it the Arrovian, perspective and sometimes from the Nash perspective. Moreover, while Poole and Rosenthal themselves early on supported the notion that Congress was essentially one dimensional, they no longer make such a case. They (and all others) actually find that at least two dimensions are significant. And, of course, that is the result from procedures that are data reduction exercises, that is, ones designed to estimate the smallest number necessary to account for the largest common variation in the voting data adequately, rather than trying to estimate the correct number of dimensions actually employed. Finally, as I will argue shortly, a lot of congressional empirical action, even in roll call voting on the floor, is (by design) eliminated from this common space. That is why it is called a common space. What is unique to one or a few Members of Congress (such as district pork) is inherently not common to most or all MCs.

Of course, all of this empirical inference is merely in support of the theoretical decision to imagine that a model of politics resting on one dimensional

choice spaces is sufficiently developed to be worth publication in general political science or social science outlets. The actual problem is theoretical. And that problem was solved conclusively by Gerald Kramer in 1973. When I discuss this paper in class, I refer to it as the “Kramer hears a Who” paper, to make it very easy for students to remember the punch-line, for, with apologies Dr. Seuss (and to Kramer and to you readers), “a dimension is a dimension, no matter how small.” That is, he showed that the median voter result is “knife-edged,” and disappears utterly, no matter how small the deviation from its conditions. We have long assimilated the lesson that the absence of symmetry, by no matter how small an amount, utterly destroys equilibrium. (Perhaps this is because Plott (1967) was so clear in his statement of this problem.) But we have known for just as long, but failed to assimilate the lesson, that the deviation from pure unidimensionality utterly destroys equilibrium just the same (and for essentially the same reason). Theoretically, a second dimension is a dimension, no matter how small. And we know empirically that there is no congress in American history that has been unidimensional in this precise theoretical sense, or even close to it. And, theoretically, there is no “approximately true.” Infinitesimal deviations utterly destroy the existence of equilibrium. There is no generalization of Black’s result, in this sense.

It is therefore my claim and my call to the discipline to say that a model in one dimension can be publishable, if at all, only in journals specializing in the area, so that the model can be seen by those who might be able to use or extend it. Such a model should not, however, be considered ready for the general readership in the discipline or in social science generally. Only those that have been extended to more than one dimension (and two is sufficient for allaying concerns about the theoretical issue) or, even better, those that derive one dimension as appropriate are ready to share with

the discipline more broadly.

So, could one actually derive the fact of one dimension as the theoretical consequence? Probably not, but this does allow me to move toward my second consideration. As with most such data reduction exercises, Poole-Rosenthal (or any other similar procedure) recovers dimensions that reflect what is common variation (1985; 1997). It does not say why the variation is common or, that is, what the dimensions stand for, let alone “mean.” It is common to interpret the major dimension as “ideological” but that is an assignment, not a demonstration. That is, it is assumed to be a left-right dimension. Of course, there is some degree of “construct validity,” in the sense that policies that we understand to be ideological often align with that dimension. Even so, it certainly does not tell us why that dimension is ideological. For example, in the 1950s, civil rights was largely orthogonal to “liberal-conservatism,” but within a few years, pro-civil rights was firmly associated with liberalism. (Civil rights and abortion are the most famous policies once orthogonal and then aligned directly with a liberal-conservative dimension, but they are not the only ones.)

Under the standard rational choice model, preferences are taken as given. The policy space is thus assumed exogenously given in many models. Is what we take to be “liberal” and “conservative” exogenously given? It is, of course, possible that there is a true ideology that defines policy spaces. It may also be the case, however, that it is endogenous, the product of actions taken by politicians in the context of the play of the game, as it were. Most importantly from this perspective, agenda control shapes what issues come to the floor and in what form they come to the vote, determining the dimensionality of the policy space.

But that raises my second point. Much institutional work in the last few years has been about the role of political parties and legislative politics. It is ordi-

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looking at interest group ratings. Finally, Lee (2006) finds that economic issues represent an increasing proportion of the Senate's agenda. For every vote to save Terri Schiavo or ban gay marriage, there are dozens on budgets, tax cuts, bankruptcy reforms, and drug benefits.

The conclusion that economic policy is substantively unimportant and intellectually uninteresting also seems to have been premature, as is the notion that congressional scholars have little to say about it. There are a number of economic issues and problems that mesh well with the interests and analytical tools of congressional studies.

Although the American economy has preformed spectacularly over the past twenty-five years by generating steady increases in *average* incomes with little inflation and a damped business cycle, the issues of economic distribution are more salient than ever as inequality continued to rise even as the economy boomed. The rise of economic inequality is a question that has attracted much interest in economics, sociology, and other subfields of political science. But congressional studies has not contributed to this debate in any significant way. This silence is somewhat surprising given that the field is well-positioned to resolve a formidable intellectual puzzle about the policy response to increased inequality. Most political economy models such as

Meltzer and Richard (1981) are essentially institution-free and rely simply on simple majority rule for predictions about policymaking. These models generally predict that taxes and transfers increase as inequality increases. Precisely the opposite has occurred in the contemporary United States. Given the evidence that voter support for redistribution has increased along with inequality,⁹ the answer to the paradox must lie, at least in part, in the failure of representative institutions to translate voter preferences into policy. Congressional scholars should be playing a central part of this debate, rather than sitting on the sidelines.

The political economy of fiscal policy is another area ripe for reconsideration. During in the 1990s, Congress basically lived within the budgetary rules it set for itself. In an era of restraint and surpluses, studying the budget process was a sure way to put political science hiring committees to sleep. Now budget deficits of enormous proportions have returned for the foreseeable future. This is due only in part to the tax cuts of 2001 and 2003. Discretionary spending has also gone up dramatically. Yet Congress is not eager to reestablish the mechanisms that helped to control spending in the 1990s. Contemporary legislative scholars ought to have something to say about why Congress was able to reform itself in the 1980s and 1990s only to have those mechanisms

falter. Pork barrel politics is yet another area where scholarly attention has waned. Some scholarly interest has been rekindled by events such as the 2005 Transportation bill. But the efforts to understand pork barrel politics are hampered by a long scholarly hiatus. Of course, distributive spending is only a small part of the federal budget. Growth in entitlement spending presents the most formidable challenges to fiscal policy. But the politics of entitlements have not been an important part of the recent literature on Congress either. This is somewhat ironic given that earlier congressional scholars such as Mayhew (1974) and Arnold (1990) provided prescient analyses of the difficulties Congress as an institution faces in making major cost-saving reforms to programs like Social Security and Medicare. Unfortunately, there has been little follow through and most of the work on these programs is being done by students of political behavior and American political development.

To close, let me stress two things that I am *not* arguing. First, I am not arguing, as some have, that the abstract, general work on legislative institutions has been a waste of time. Although this effort has diverted attention from political-economic questions, I do not believe there is necessarily any trade-off between scientific progress and engaging sub-

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narly understood as the study of one institution, the congressional party system. Much good work has been produced examining all sides of this question of the role of political parties in legislatures. It is, I believe, now time to face a more complicated version of that question. Parties and Congress are two separate institutions. Parties are prominent features of legislatures, but they are also prominent features of campaign and elections (and more). And, legislatures have a large variety of features that have little to do with political parties. There are, that is, two separate institutions. The happen to over-

lap in one portion of their activities. To model parties as only legislative entities is to dramatically underestimate the role of political parties. Akin to underspecification, it is very likely that studying parties only in Congress greatly misestimates their effects even in that context. The same words can be said about studying only the partisan parts of legislatures.

The problem with multiple institutions is that it requires something different than a single, well-defined non-cooperative game to solve. There are a number of problems with non-cooperative

games. Backwards induction is a central part of solution concepts, but we know empirically people do not act consistently with backwards induction. Change the smallest feature of the game and solutions might be very different. But in this instance, the study of institutions interacting, we simply know too little to know how to model them. We can be fairly confident, however, that the standard of using a well defined game is unlikely to be workable.

Is it worth the effort? In addition to my running example, let me give a differ-

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stantive economic policy questions. Indeed, a fair number of theoretical advances about legislative and electoral institutions have originated with economists specifically concerned with explaining economic policy.¹⁰ But because mainstream congressional studies has not been as interested in these substantive applications, the work of these economists has been largely ignored to the detriment of both congressional scholars and political economists.

Second, I am not calling for a return of a kind of economic determinism typical of the work by economists. Just as I think it is mistaken for students of Congress to downplay and minimize the role of economic motivations of voters and legislators, it would be equally regrettable if social, moral, and cultural divisions were

ignored. But congressional studies has not done a very good job of understanding the relationship between economic and social issues. Even when we concede both are important, we often assume that the issues are orthogonal and that their effects on behavior are additive. But in reality, the relationship between economics, religion, and morality is more complex as cultural and religious beliefs affect the way voters and citizens respond to economic incentives. Indeed, I believe understanding these relationships is the central question for the next generation of political economy scholarship. Given congressional studies' unmatched data source, the complete legislative record of the U.S. Congress, the field is in a unique position to understand how values and economic interest interact and are repre-

sented in the political process. If the field can pull that off, it will once again be central to the study of political economy.

¹ I use the term congressional studies to distinguish my focus from the larger field of legislative studies which, of course, focuses on non-U.S and state legislatures as well as the U.S. Congress. I would argue that the broader field of legislative studies is not contributing much to political economy, but that is mainly a reflection of the fact that it has never been focused on the core issues of political economy.

² Of course, it cannot be a complete explanation because the bias towards agriculture is true of countries with weak legislative committee systems as well as non-democracies.

³ See Shepsle and Weingast (1987), Weingast and Marshall (1988).

⁴ See Gilligan and Krehbiel (1987) and Krehbiel (1991)

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ent (and in this case, positive) example. Gisela Sin has developed the argument that what happens in one chamber or branch of government can affect the internal design of some other branch (2006). She provides systematic evidence that changes in the balance of power in the Senate and the presidency are closely related to reforms of rules in the House. She and Arthur Lupia have developed a formal model of this effect (2005) to go with the empirical evidence. Thus, it seems both possible and likely that this complex of interactions among sets of rules, combined with preferences from a larger and more diverse variety of actors (party campaign activists and congressional leadership, for example) can provide a richer base for explanation and move us toward a more complete accounting of politics within democracies. Doing so not only raises the methodological issues – how can we conceive of a form in which to study this sort of complex situation? It also raises theoretical issues. How do we understand preferences from actors with such different forms for inducing preferences (for example, campaign activists are very different from committee chairs in this regard)?

Aren't we forced to reconsider the "Riker" or "inheritability" problem (1980) in a new and even more complex form? That is, if actors in one institution help shape rules changes in some other institutional setting, we are led to ask in new ways, where do the rules come from? But, of course, it is precisely in considering this complex of questions that we can imagine the next generation of scholarship about institutions making real advances in our understanding of democratic politics.

¹ The "fundamental equation" of the new institutionalism is that political outcomes are the result of the concatenation of actors' preferences, rules, and [perhaps] history.

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⁵ E.g. Londregan and Snyder (1994).

⁶ I plead guilty.

⁷ E.g. Krehbiel (1999) and Cox and McCubbins (2006).

⁸ Of course, these factors are not directly relevant to the comparative study of legislatures. But given that the vast majority of legislative work focuses on the U.S., it probably has an indirect effect by altering the broad agendas of the subfield.

⁹ See McCall (2003) and Newman and Jacobs (2006).

¹⁰ Original contributions as well as reviews of this literature are contained in Persson and Tabellini (2002, 2005).

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UPCOMING AWARDS ANNOUNCEMENT

APSA Political Economy Awards Committees, 2006-2007



The **William Riker Best Book Award** is for the best book in political economy published during the past year. The Riker Award Committee will be chaired by Tom Romer (romer@princeton.edu). The other members of the committee include Tom Oatley (oatley@unc.edu) and David Laitin (dlaitin@stanford.edu).

The **Mancur Olson Award** is for the best dissertation in political economy published during the past year. The Olson Award Committee will be chaired by Arthur Lupia (lupia@umich.edu). Committee members also include Miriam Golden (golden@ucla.edu) and Alberto Diaz-Cayeros (albertod@stanford.edu).

The **Michael Wallerstein Award** is for the best article in political economy published during the past year. The Wallerstein Award Committee will be chaired by James Alt (jalt@latte.harvard.edu). The committee includes James Robinson (jrobinson@gov.harvard.edu) and Roger Myerson (myerson@uchicago.edu).

The **Best Paper Award** is for the best paper in political economy given at the 2006 APSA meetings. The Best Paper Award Committee will be chaired by Strom Thacker (sthacker@bu.edu). Additional members of the committee include Jonathan Katz (jkatz@caltech.edu) and Michael Thies (thies@nicco.sscnet.ucla.edu).

THE POLITICAL ECONOMIST

CALL FOR NOMINATIONS: MANCUR OLSON AWARD

The APSA Organized Section on Political Economy will present the 2007 Mancur Olson Award to the author of the best dissertation completed and accepted in the field of Political Economy during calendar year 2005 or 2006. An award committee has been constituted and is now seeking nominations. To nominate a dissertation, it is necessary to send one copy of the dissertation to each member of the award committee along with a cover letter designating the dissertation as a nominee for the Olson award. All eligible nominations must be postmarked on or before March 1, 2007.

The committee members are:

Arthur Lupia, Chair
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If you know of a qualified dissertation, please help the discipline recognize innovative new work in the field of Political Economy by nominating it for this award.

CALL FOR PAPERS FOR THE 2007 APSA MEETINGS: POLITICAL ECONOMY SECTION

Political economy is a diverse field that includes research defined by topic — substantive studies of how politics and policy interact with market forces — and by methodology—the investigation of political questions using tools derived from economics. We invite papers from both of these branches of political economy.

Substantively, we welcome paper or panel proposals studying the causes of economic outcomes such as trade openness, capital flows, migration patterns, and economic development. Similarly, we are interested in research on the causes and effects of trade and capital flows, fiscal policy, regulation, monetary and financial policies, and international aid. These papers should be theoretically and empirically rigorous and may address domestic, comparative, or international issues. Methodologically, we seek proposals for papers or panels that test, refine, critique, or support core political economy theories, especially those related to collective action; comparative political institutions; inequality and redistribution; governance; development; government performance; and legislative and voting behavior.

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Co-Chairs, Political Economy Section of the 2007 APSA Meeting

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