Volume X, Issue 1 Summer 2013

THE POLITICAL ECONOMIST

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

Co-Editors:
Scott Gehlbach & Lisa L. Martin, University of Wisconsin, Madison

What's Inside This Issue

From the Editors

The last two years have seen the passing of four giants of political economy: James Buchanan, Albert Hirschman, William Niskanen, and Elinor Ostrom (who served as Section Chair from 2004 to 2006). We celebrate their life and work with four essays. John Nye investigates the contributions of James Buchanan to both positive and normative theory. Daniel Drezner pays tribute to Albert Hirschman, the "purest political economist of them all." David Weimer mixes personal reflection and professional appraisal in his essay on William Niskanen. And Paul Dragos Aligica points to the continuing challenges posed by the work of Lin Ostrom and her husband and collaborator Vincent.

Also in this issue is a letter from John Huber, Section Chair, who reminds us that theory, formal and otherwise, is an important part of political economy. As John notes, theory sometimes takes a back seat as modern methods of causal inference change the way that we do empirical work. Yet the "identification revolution" has arguably made theory even more important, for reasons that John discusses. It is a useful reminder, especially as we reflect on the intellectual legacies of four "capital-T" theorists.

This is our final issue as editors of *Political Economist*. It has been a thrill over the past three years to work with so many wonderful contributors; with Mark Copelovitch, the newsletter's book review editor; and with Amanda Harris, newsletter assistant. The editorship passes with the next issue into the capable hands of Bill Clark at Michigan. We look forward to learning more about our fascinating field in future issues of the *PE*.

Scott Gehlbach
gehlbach@polisci.wisc.edu

Lisa L. Martin
llmartin3@wisc.edu

From the Chair

Is theory getting lost in the "identification revolution"?

There is a powerful movement in "political economy," as there is in political science more generally, emphasizing the importance of causal identification, of *making valid causal inferences in empirical research*. The argument behind this "identification revolution" is well-rehearsed: standard analyses of observational data, such as traditional multivariate regression with covariate adjustment, do not reveal the causal impact of variables because it is typically impossible with such approaches to understand the direction of causation, or to know if "effects" we attribute to some variable of interest are in fact due to some other unobserved variable that we have not measured. We must therefore employ other approaches that allow random assignment of the causal variables of interest (such as field, laboratory or survey experiments), or at least that employ approaches to observational data that make causal inference possible (such as regression discontinuity models, instrumental variables, difference-in-difference models, or natural experiments).

This emphasis on causal identification should be celebrated. Figuring out "what causes what" in politics, economics, and their intersection is a huge part of what we need to be doing, and though many contemporary strategies to this end are in their relative infancy, the rapid progress political scientists are making to get better at causal identification is in some respects breathtaking. At the same

continued on page 2

From the Chair...continued from page 1

time, a laser focus on causal identification can create biases in the way we think about what constitutes a good question, in the claims we make about our work, and ultimately in how deeply we really understand political phenomena. It's therefore useful to reflect a bit on the nature of these biases, and on how they might be shaping the way we go about our research.

One bias created by the "identification revolution" may concern the menu of questions we study. Some "identificationists" take the strong position that social science research that cannot solve the identification problem is not worth doing, or at least is not worth publishing in leading journals. If we move towards this position, we excessively narrow the range of questions we ask, and thus unnecessarily limit our understanding of the social processes we study. One problem is that many things we care about – democracy, growth, institutions, diversity, inequality, wealth, violence, stability, rights, participation – cannot realistically be randomly assigned, and the extent to which the natural world presents us with causal identification opportunities can be quite limited. Another problem is that many of these substantively important variables are embedded in dynamics of reciprocal causation with each other that will often frustrate the ambitions of even the most determined and talented "identificationists." Thus, good causal identification is not always possible on questions of central importance.

Does this mean we should not study such questions? Sometimes research agendas reach a point where we won't make much more useful progress until someone solves the identification problem. The theories are well-developed, there exist no data limitations on how we describe empirical associations, and the traditional empirical methods have pushed observational data to their limits. In these situations, further studies that leave unaddressed questions of causality seem a waste of time. But the number of questions on which we've reached this point might be smaller than many imagine, and there is often much to be gained from working on questions for which we cannot see clear solutions to the identification problem. Indeed, for many important questions, there is little clear theory, and providing one will be helpful in orienting empirical research. Similarly, demonstrating the presence of previously unknown empirical associations can dramatically shape how we think about social phenomena, even if we can't nail down causation. It's pretty impressive, for example, how often simple bivariate scatter plots make a lasting impact on how we think about the world around us. Add the two together – theory and empirical association – and something very useful results, including making it possible to offer much better advice about what specific type of "identification study" is likely to yield the most useful insights.

But it's hardly the case that theory is particularly important on questions where we haven't figured out how to solve identification problems. On the contrary, I think that one of the most important biases in how the identification revolution is unfolding concerns its distant relationship with theory. It seems that developing clear theories, once a central pre-occupation in political economy, is now taking a back seat to developing convincing identification strategies. Perhaps there's a sense that we have plenty of theories and that the main challenge we face is to figure out which variables actually have a causal effect. But this is wrong-headed - the very nature of research on causal identification requires a heightened rather diminished role for careful theorizing.

Many of the best strategies in causal identification essentially amount to case studies - a field experiment will be undertaken in a neighborhood during an election or a narrow set of villages; a natural experiment can be exploited as an opportunity for causal identification; a regression discontinuity strategy

continued on page 3

From the Chair...continued from page 2

will be possible in a particular context. It is the focus on the specific case that typically allows for convincing causal identification, and when done well, we can know with near certainty whether in the specific case, variable x has had a causal effect on variable y. The problem, however, lies in situating the case, which is crucial if we are to draw useful inferences about causation.

Consider the example of how district magnitude – the number of representatives elected from a district - affects the number of political parties. Suppose we want to understand whether a higher district magnitude leads to a larger number of parties, and we discover that something happened in Sweden that resulted in the random assignment of district magnitude for local elections. We are able to exploit this to do a gold-plated causal identification study, and it reveals no effect of district magnitude on the party system. What can we learn from this? That district magnitude has no effect on party systems? The answer depends on how we situate the case of Sweden, which depends on theory. In this example, leading theories argue that district magnitude should not affect party systems in especially homogenous societies (because the demand for parties in such societies is low), which helps us to situate the case (Sweden is very homogenous) and to think about how to interpret the null result. And if we had thought about the theory carefully *ex ante*, we might have looked elsewhere for a case – if we had raised enough money to randomly assign electoral laws in the subnational governments of one country, for example, theory probably wouldn't have directed us to do so in Sweden. Of course, had the study occurred in a heterogeneous country and we'd found a positive effect, we'd still need a theory to guide interpretation – the positive effect may not imply, for example, that district magnitude generally leads to more parties.

Traditional regression-type methods with observational data play a crucial role in helping us to understand how to situate cases, and to learn inductively about how to build appropriate theories. There exist famous papers of party systems that use such methods, for example, to make clear the presence of an interaction between district magnitude and ethnic heterogeneity. Without such papers, we might be at sea when it comes to building theories that can explain the null results of the Swedish study. In fact, though this is a matter of taste, I think we learn more about party systems in this example from one gold-plated traditional regression-type paper than we do from one gold-plated study in Sweden, even if the traditional approach leaves open the crucial question of causality.

Fortunately, the choice is not between the two empirical approaches. Traditional empirical approaches may play a useful role in helping us think about what a good theory might look like, but it is theory – not inductive empirical research – that enables us to convincingly situate and thus to understand the results of a gold-plated "causal identification" study, such as the hypothetical one in Sweden. And I think we should worry that the importance of theory is getting lost in the "identification revolution." Too seldom do we see serious effort to use theory to motivate and situate cases. And too often do we see conclusions from "causal identification case studies" with sweeping generalizations that overclaim – or even misjudge – what we can learn from the case.

I think it is inevitable that we will move toward a better dialogue between causal identification research and theory. Although useful theorizing can take many different forms, my own bias is that formal theories will be especially important in this regard because they not only describe explicitly a logic explaining the circumstances under which some variable x should have a causal effect on y, but also because they can do so in a way that can generate multiple observable implications from a unified framework. A formal theory, for example, can yield equilibrium predictions not only about the relationship between district magnitude and the number of parties, but also about how district magnitude affects the ideological locations of parties, or about how it affects the propensity for strategic voting. This enables us not only to situate and interpret cases, but also to think about what types of causal identification research we should do in the first place.

If it's true that the dialogue between formal theoretical research and causal identification research is not what it should be, the blame hardly lies solely in the laps of those doing the cutting-edge empirical research. Too often, formal theorizing develops with indifference to (or in ignorance of) empirical research. Theorists need to engage more directly the identification revolution, developing models explicitly directed at the topics studied in this research. In so doing, theory must do more than provide plausible interpretations of the findings from specific cases – it must also help situate the cases, thereby providing arguments for the most fruitful avenues for subsequent empirical research. What we need, then, are better synergies between theory building and causal identification studies. The utility of each without the other is quite limited compared with what they can achieve together.

John Huber
jdh39@columbia.edu

THE POLITICAL ECONOMIST 13

Feature Essay

James Buchanan: Worldly Philosopher & Constitutional Idealist

John V.C. Nye, George Mason University and National Research University - Higher School of Economics

By the time James Buchanan passed away on January 9, 2013, the economic analysis of politics had attained a prominent, even central place, in both economics and political science. That these approaches have gone from being mostly neglected half a century ago, to becoming a major preoccupation of leading scholars throughout the world today, was in no small part due to the pioneering work of Buchanan himself, both alone and in joint work with various collaborators, most notably Gordon Tullock.

Buchanan, who received the 1986 Nobel Memorial Prize in Economic Sciences, spent most of his career at three institutions: first at the University of Virginia, where he founded the Thomas Jefferson Center for Studies in Political Economy; then at Virginia Polytechnic Institute, where he founded the Center for Study of Public Choice; and finally at George Mason University, to which he moved the Center in 1983.

Buchanan made fundamental contributions to both positive and normative political economy.

His most influential work came from attacking the view of the state as a benevolent despot and substituting instead the principle that political actors should be modeled as favoring their private interests in much the same way that we model private economic agents. This idea was most influential in the research now known as public choice, but also in his work on the theory of clubs, and in his challenges to the neoclassical/Pigouvian approach to externalities.

The pioneering work in this area was of course, *The Calculus of Consent* (1962), his magnum opus jointly written with Gordon Tullock. Arguing first for the use of methodological individualism in the analysis of political actors, the authors then contrasted the costs and benefits of various majority voting systems vs. a unanimity rule. The unanimity rule best preserved the public interest but came at the expense of extremely high decision-making costs. In contrast, all other (majority voting) systems suffered from a mix of decision-making costs and various inconsistencies that led to externalities or deviations from the public interest. It was also a pioneering work in the use of game theoretic concepts and in its blend of economic reasoning with politics. Though TCOC has sometimes been challenged as caricaturing human motivation, most modern formal analysis of political behavior essentially starts from similar premises. The book is one of the most widely cited works in all of the social sciences and is notable for its almost symmetric and equally important influence in both economics and political science.

This eventually led to the creation of a major journal, *Public Choice*, and the founding of the Public Choice Society, both of which continue in good health to this day.

Buchanan's work on the theory of clubs (1965) made the case for treating many jointly used and produced goods and services as being intermediate cases between the purely private and purely public goods that had been the focus of analysis since the early work of Samuelson. It was fairly unique in its attention to the problems of both inclusion and exclusion when considering how to efficiently produce certain goods. Its ideas have still not been fully extended to the state itself, especially when states' provision of costly but non-rivalrous goods become unsustainable in a world of mobile capital and labor.

He also did important work challenging the Pigouvian approach to externalities. Especially noteworthy was his observation that in the presence of monopoly power, taxes designed to correct externalities such as pollution may in fact reduce overall welfare (1969). This is because monopolies already set price above marginal cost and reduce output below the competitive level, thus requiring the ideal Pigouvian planner to know the full extent of market distortions before setting an appropriate corrective tax. It is striking how this idea and the other impediments to efficient Pigouvian taxation he mentions are so rarely discussed in the broader public policy debate on global emissions.

In addition to his pioneering work in public choice, Buchanan also promoted a more normative theory of political economy that is just as distinctive although less well known - what he termed constitutional political economy. This area was focused on the problem of structuring the meta rules that allow both political and economic exchange to function. In this sense he saw how politics and markets alike were about actions and exchanges within a given rules structure and he sought to argue for a normative theory of how the Rule of Rules should be designed. His work was also relevant for providing a pre-Rawlsian use of the veil of ignorance in a parallel construction in his political economy.

In his work on the *Reason of Rules* (joint with G. Brennan, 1985), Buchanan laid out most clearly his perspective on normative principles for creating a framework of rules from which "normal" political and economic competition could proceed. Buchanan was convinced that the problems of self-interested actors trying to arrive at neutral rules could be circumvented by disinterested contemplation from behind a veil of ignorance. This idea, which had been used in his previous work on public choice, was now given more emphasis in helping to create conditions under which virtual unanimity could be obtained for rational rules under which subsequent political and economic exchange could occur. In his view, separating people from their direct interests by focusing on distant and abstract rules structures could promote

continued on page 5

Nye Feature Essay...continued from page 4

the reasoned rationality needed to allow for effective functioning in a world of self-interested actors in the later stages of the game.

Where Buchanan may have failed is in considering the problem of the evolution of constitutional rules themselves. Whereas his work on pubic choice demystified politics by moving political analysis away from a view of the government as benevolent despot towards one in which government agents were as strategic and competitive as private actors, Buchanan seemed reluctant to think of the evolutionary dimension behind the creation of constitutional rules in the same way. Though he was deeply aware of the historical roots of modern political structures, he wanted there to be good normative reasons for the creation of constitutions.

In private discussions about the relationship between public choice, constitutional political economy, and the new institutional economics, he conceded that there was an overlap between his work and those who sought to work more explicitly in the Coasian tradition, including North and Williamson. But he seemed uncomfortable with the notion of Hobbesian competition generating preferences for constitutional rules not grounded in reason or rights. He was reluctant to accept a view of violent conflict leading to constrained systems of rules not fully consistent with ordered reason and established principles of legitimacy. Hence his more normative, abstract view of constitutional political economy can be contrasted with more recent work by North, Wallis, and Weingast (2009) or Acemoglu and Robinson (2012) that treat the rise of modern states as compromises that evolved to permit exchange while containing violence between competing factions in society.

Buchanan thought of himself very strongly as a classical liberal and saw a normative theory of liberal constitutions as playing an important role in promoting the common good. For Buchanan, it was clear that the rules derived legitimacy from their having been chosen by the members of society acting collectively. He was always concerned for thinking of good rules as deriving from unanimity even if subsequent political negotiation under those rules was very far from a vision of cooperative exchange. It is clear that even though public choice is often portrayed as a cynical view of political man, Buchanan's view was of conflict as mediated exchange holding back more violent and coercive interactions, not as Hobbesian struggle. This perhaps explains his gentlemanly and almost utopian view of what idealized constitutionalism might accomplish in contrast to more recent theories of the rise of the state. Thus, he shied away from fully applying his individual-interest approach to politics to the constitutional question itself.

Buchanan was also notable for urging economists to focus less on equations that maximized some putative utility function and more on the overall problems of coordination and exchange. Like many of his fellow giants, including Ronald Coase, Douglass North, and Armen Alchian, Buchanan decried the descent into routine formalism that has characterized mainstream economics since the 1960s. And like Hayek he worried a great deal about the empty worship of the natural sciences that seemed to mark modern theory. Yet Buchanan was successful at publishing at the highest levels of the profession and did a good deal to inspire modern work on political economics even if most practitioners continued to ignore his concerns about empty theory and excessive modeling. It is significant that most writing in formal politics today looks more like extensions of Buchanan and the Public Choice School than the older Samuelsonian tradition of the benevolent state and omnipotent regulator.

Ultimately, as Alex Tabarrok has suggested, the great tension in Buchanan's work was that his normative theories gave us a glimpse of the ideal while his positive work in public choice suggested that such an ideal is never attainable.

References

Acemoglu, Daron and James A. Robinson, 2012. *Why Nations Fail: The Origins of Power, Prosperity, and Progress*. Crown Business.

Buchanan, James M. 1965. An Economic Theory of Clubs, *Economica*, new series, 32, no. 125, (Feb), 1-14.

Buchanan, James M. 1969. External Diseconomies, Corrective Taxes, and Market Structure, *American Economic Review*, 59, no. 1, 174-177.

Buchanan, James M. and Geoffrey Brennan, 1985. *The Reason of Rules*. Cambridge University Press.

Buchanan, James M. and Gordon Tullock, 1962. *The Calculus of Consent*. University of Michigan Press.

North, Douglass C., John Joseph Wallis, and Barry R. Weingast, 2009. *Violence and Social Orders: A Conceptual Framework for Interpreting Recorded History*. Cambridge University Press.

Feature Essay

The Purest Political Economist of Them All: Albert Hirschman's Legacy

Daniel W. Drezner, Tufts University

When Albert Hirschman passed away late last year there was an outpouring of testimonials from economists and political scientists (*Economist* 2012; Farrell 2012; Fukuyama 2012; Sethi 2012; Tabarrok 2012). At first glance, such a reaction might seem out of proportion to Hirschman's record. There is no "Hirschman" school of thought in political economy. Indeed, he was not a paradigm-builder in any sense of the word, being somewhat dubious of paradigmatic approaches altogether (Hirschman 1970a). Hirschman did not turn out many Ph.D. disciples; according to first-hand accounts he was an abysmal teacher (Adelman 2013, p. 419). By the standards of economics, Hirschman was not particularly sophisticated in his methods or modelling. Krugman (1994) characterizes Hirschman's *Strategy of Economic Development* (1958) as an "understandable but wrong" response to the failures of the economic development literature precisely because of the "discursive, non-mathematical style" of Hirschman's effort.

Nevertheless, Hirschman's intellectual legacy is quite secure. The Social Science Research Council's highest award is the Albert O. Hirschman Prize - which recognizes "academic excellence in international, interdisciplinary social science research, theory, and public communication." A cursory glance at Hirschman's citation count suggests that his influence on the social sciences is both wide and deep. He is already the subject of at least three book-length treatments of his intellectual legacy (Rodwin and Schön 1994; Meldolesi 1995; Adelmen 2013).

How did Hirschman have such wide-ranging impact with such an idiosyncratic approach? Precisely because he was so idiosyncratic. Hirschman was unconcerned with paradigmatic or disciplinary boundaries, which enabled him to develop some of the key building blocks of how to think about political economy. Anyone working on issues of economic power, economic development or economic ideas cannot do so without either building on or tangling with Hirschman's legacy. An exhaustive survey of his contributions would go far beyond these pages, but even a brief glance at Hirschman's principal ideas advanced in his major works in political economy reveal the extent of his influence.

Substance

Hirschman's first book, *National Power and the Structure of Foreign Trade* (1945), is emblematic of Hirschman's scholarly prescience. The experience of the 1930s piqued Hirschman's interest in how states could use foreign economic policy as an instrument of statecraft. His exploration of the "influence effect" of trade stressed the conscious creation of "conditions which make the interruption of trade of much graver concern to its trading partners than itself (Hirschman 1945, p. 16)." In thinking about how great powers could create such conditions, *National Power* turned traditional beliefs about how to amass economic power on their head. Mercantilists had posited that economic power was gained in world politics by exporting more than importing, thereby building up reserves of wealth. Hirschman observed that foreign trade becomes an instrument of national power only if trading partners benefitted more from the bilateral relationship than they would from substitute partners. Inculcating incentive-compatible trade relations among others – to the point of running trade deficits – is one way that states augment their ability to use the influence effect. Hirschman upended traditional *realpolitik* takes on global political economy in making this point (Drezner 2010).

In thinking through the precise methods through which states could enhance their national power through foreign trade, Hirschman (1945, p. 26) developed building blocks to think about asymmetric dependence (Wagner 1988), market power (Krasner 1976), economic statecraft (Baldwin 1985), and more for later generations of international political economy scholarship. Consider that in the span of ten pages, Hirschman observed that the pursuit of national power did not necessarily sacrifice the pursuit of national plenty – a theme Jacob Viner (1948) would elaborate on soon in *World Politics*. He further speculated (1945, p. 29) that expanded trade would lead to the creation of "commercial fifth columns" by creating interest groups within the targeted countries that would have a vested interest in not alienating the trade partner. This presaged the "second image reversed" literature that Peter Gourevitch (1978) developed thirty years afterwards. Hirschman (1945, p. 31) discussed the concept of "exclusive complementarity" that could be developed between two economies through an open trading relationship. Three decades later, Klein, Crawford and Alchian (1978) labeled this idea "asset specificity," a concept that became a conceptual cornerstone of literatures on industrial organization and political bargaining. As an aside, Hirschman also developed what is now the standard metric used to measure market concentration, the Herfindahl - Hirschman Index (Adelman, p. 217). *National Power* plays the role in IPE akin to Schelling's (1960) *Strategy of Conflict* in security studies – an *ür*-text that contained fragments of ideas to be developed by later generations of political economists.

Hirschman's most famous work, *Exit, Voice, and Loyalty* (1970b), also had theoretical and empirical ramifications far beyond the animating question of how consumers react to a deterioration in the quality of a supplier's good or

continued on page 7

Drezner Feature Essay...continued from page 6

service. Hirschman noted that the standard economic response to deteriorating quality was to exit and find a suitable substitute in the marketplace. Another option, however, was to exercise voice as a way of making the supplier respond to negative feedback. While exit and voice could be used in combination, it was likely that the threat of exit would cause the skills needed to use voice to atrophy – and vice versa. Loyalty – or, as some political scientists would say now, identity – acted as a brake on exit and an enabler of voice.

In marrying exit and voice together, Hirschman generated a number of counterintuitive hypotheses that continue to resonate today. Perhaps the most obvious was the notion that both economic and political actors possessing some degree of monopoly power preferred to see customers exit rather than use voice. Authoritarian governments, for example, might prefer to see political dissidents exit the country as asylum-seekers rather than stay and foment unrest. Indeed, Hirschman noted that Latin American governments had used this strategy in the past. At the same time, in follow-on work Hirschman (1978) acknowledged that exit – or voting with one's feet – could also spur states into more positive reform efforts. This ambivalence about the relative effects of exit versus voice is one reason why *Exit, Voice, and Loyalty* is easily cited, but harder to convert into a more workable theory of political economy (Gehlbach 2006; Drezner 2007).

Style

There are three stylistic tropes that run through Hirschman's *oeuvre*. First, he was singularly dedicated to the idea that political scientists and economists could profit from a mutual exchange of ideas – as opposed to economists dictating terms to political scientists. This was a central element of *Exit, Voice, and Loyalty* (1970b, p. 19) in particular: "Exit and voice, that is, market and non-market forces, that is, economic and political mechanisms, have been introduced as two principal actors of strictly equal rank and importance. In developing my play on that basis, I hope to demonstrate to political scientists the usefulness of economic concepts, *and to economists the usefulness of political concepts* (emphasis in original)."

Second, Hirschman's ideas are difficult to separate from his prose. It is striking that both *Exit, Voice, and Loyalty* and *The Passions and The Interests* (1977) are such slender books. This is not because they are thin on content. Rather, it is because Hirschman wrote so clearly, enabling him to express his ideas with remarkable economy. In doing so he might have unintentionally handicapped those of us fumbling to regurgitate his ideas. As Adelman (2013, p. 10) notes, in trying to describe Hirschman's books and essays, "their lucidity often left me paraphrasing what was rendered much better in the original." Adelman is not alone in this quandary – in a letter to Hirschman, Quentin Skinner conceded that, in trying explaining one of Hirschman's arguments to his students, "the points I try to make to them about it are in fact in the essay itself (Adelman 2013, p. 10)." Time-consuming investments are necessary to become adept in the ways of formal modelling, sophisticated econometrics or interpretive methods. Hirschman's work suggests that the same can be said of elegant prose.

Finally, as a gifted writer himself, over time Hirschman came to care more about how ideas and rhetoric were deployed in political discourse. *The Passions and The Interests* (1977) was a challenge to Max Weber's well-known theory that capitalism emerged out of a Calvinistic search for salvation. Instead, Hirschman argued that key thinkers and elites saw capitalism as a means through which violent tendencies in man and society could be tamed through the expansion of commercial activity. The fact that the claims of Montesquieu or James Steuart didn't quite develop in their intended manner doesn't vitiate the importance of their arguments or rhetoric. Indeed, Hirschman's point was a harbinger of later debates in economic history about the role of ideas in triggering the capitalist revolution (Mokyr 2009; McCloskey 2010). One of Hirschman's follow-up projects, *The Rhetoric of Reaction* (1991), dissected the arguments traditionally used by conservatives to oppose progressive policy reforms.

Albert Hirschman's pre-academic life was a worthy prequel to *Casablanca*. After fleeing Nazi Germany and fascist Italy to Vichy France, Hirschman helped run an escape network to secret some of Europe's greatest artists and thinkers – including Hannah Arendt, Marc Chagall, and Marcel Duchamp – out of continental Europe (Hirschman 1995, pp. 120-126; Adelman 2013, chapter five). He eventually crossed the Pyrenees himself on foot, carrying another refugee for miles. That is quite a first act, one that few in our field today could top. It is to Hirschman's credit that his intellectual legacy will far outshine his rather impressive biography. In many ways, his work is the purest example of political economy since the days of Adam Smith.

References

Adelman, Jeremy. 2013. *Worldly Philosopher: The Odyssey of Albert O. Hirschman*. Princeton: Princeton University Press.

Baldwin, David A. 1985. *Economic Statecraft*. Princeton: Princeton University Press.

Drezner, Daniel W. 2007. *All Politics Is Global*. Princeton: Princeton University Press.

Drezner, Daniel W. 2010. Mercantilist and Realist Perspectives on the Global Political Economy. In *The International Studies Encyclopedia*, Robert A. Denemark, ed. New York: Blackwell.

Economist. 2012. Exit Albert Hirschman. December 22. http://www.economist.com/news/business/21568708-great-lateral-thinker-died-december-10th-exit-albert-hirschman.

Farrell, Henry. 2012. Albert Hirschman Has Died. *The Monkey Cage*, December 11. http://themonkeycage.org/2012/12/11/albert-hirschman-has-died/.

continued on page 8

Feature Essay

The Synergy of Practice and Theory: Niskanen's Contribution to the Study of Bureaucracy

David L. Weimer, University of Wisconsin, Madison and LaFollette School of Public Affairs

My first encounter with William A. Niskanen, Jr. was in 1973 at my graduate school orientation at the University of California, Berkeley. Although I cannot remember a word spoken that day by any of the other faculty members, I remember what Niskanen said verbatim: "My name is William Niskanen. I hold a B.A. from Harvard College and a Ph.D. in economics from the University of Chicago. Chicago won." Indeed it did.

Niskanen's doctoral study in economics was followed by work as a policy analyst at the RAND Corporation, the Department of Defense, the Institute for Defense Analyses, and the Office of Management and Budget. He joined the faculty of the Graduate School of Public Policy at the University of California in 1972 and became the Chief Economist for Ford Motor Company in 1975 and briefly joined the UCLA faculty in 1980. He served on President Reagan's Council of Economic Advisors until leaving to become chairman of the board of the Cato Institute.

His training in economics and exposure to the deficiencies of bureaucracy during his early career as a policy analyst prepared and prompted Niskanen to write one of the classics of public choice, *Bureaucracy and Representative Government*. He first set out his thoughts on the topic in "The Peculiar Economics of Bureaucracy," which was published in the *American Economic Review* in 1968, and later modified them in important ways in "Bureaucrats and Politicians," which appeared in the *Journal of Law and Economics* in 1975.

The standard approach of welfare economics at the time was to assume that government and its officers selflessly intervened to correct market failures. In *Bureaucracy and Representative Government,* Niskanen joined the emerging public choice movement in assuming that public officials acted upon self-interest. Throughout the book, the head bureaucrat seeks to maximize the budget allocated to the bureau by the "collective organization," or budgetary sponsor, as a lump-sum payment for the output it produces. He began by assuming passive budgetary sponsors, but later allowed "officers" of the collective organization to act according to self-interest. Although the assumption of the budget-maximizing bureaucrat is today its primary intellectual trace in

continued on page 9

Drezner Feature Essay...continued from page 7

Fukuyama, Francis. 2012. Albert O. Hirschman, 1915-2012. *The American Interest*, December 11. http://blogs.the-american-interest.com/fukuyama/2013/01/06/albert-o-hirschman-1915-2012/.

Gehlbach, Scott. 2006. A Formal Model of Exit and Voice. *Rationality and Society* 18, 4: 395-418.

Gourevitch, Peter. 1978. The Second Image Reversed. *International Organization* 32, 4: 881-912.

Hirschman, Albert O. 1945. *National Power and the Structure of Foreign Trade*. Berkeley: University of California Press.

Hirschman, Albert O. 1958. *The Strategy of Economic Development*. New Haven: Yale University Press.

Hirschman, Albert O. 1970a. The Search for Paradigms as a Hindrance to Understanding. *World Politics* 22, 3: 329-343.

Hirschman, Albert O. 1970b. *Exit, Voice and Loyalty*. Cambridge: Harvard University Press.

Hirschman, Albert O. 1977. *The Passions and the Interests*. Princeton: Princeton University Press.

Hirschman, Albert O. 1978. Exit, Voice and the State. *World Politics* 31, 1: 90-107.

Hirschman, Albert O. 1991. *The Rhetoric of Reaction*. Cambridge: Belknap.

Hirschman, Albert O. 1995. *A Propensity for Self-Subversion*. Cambridge: Harvard University Press.

Klein, Benjamin, Robert Crawford and Armen Alchian, 1978. Vertical Integration, Appropriable Rents, and the Competitive Contracting Process. *Journal of Law and Economics* 21, 2: 297-326.

Krasner, Stephen D. 1976. State Power and the Structure of Foreign Trade. *World Politics* 28, 3: 317-347.

Krugman, Paul. 1994. The Fall and Rise of Development Economics. In *Rethinking the Development Experience,* Lloyd Rodwin and Donald Schön, eds. Washington: Brookings Institution Press.

McCloskey, Dierdre. 2010. *Bourgeois Dignity*. Chicago: University of Chicago Press.

Meldolesi, Luca. 1995. *Discovering the Possible*. London: University of Notre Dame Press.

Mokyr, Joel. 2009. *The Enlightened Economy*. New Haven: Yale University Press.

Rodwin, Lloyd, and Donald Schön, eds. 1994. *Rethinking the Development Experience*. Washington: Brookings Institution Press.

Schelling, Thomas. 1960. *The Strategy of Conflict*. Cambridge: Harvard University Press.

Sethi, Rajiv. 2012. Remembering Albert Hirschman. *Rajiv Sethi*, December 11. http://rajivsethi.blogspot.com/2012/12/remembering-albert-hirschman.html.

Tabarrok, Alex. 2012. Albert O. Hirschman: Life and Work. *Marginal Revolution*, December 11. http://marginalrevolution.com/marginalrevolution/2012/12/albert-o-hirschman.html.

Viner, Jacob. 1948. Power Versus Plenty as Objectives of Foreign Policy in the Seventeenth and Eighteenth Centuries. *World Politics* 1, 1: 1-29.

Wagner, R. Harrison. 1988. Economic Interdependence, Bargaining Power, and Political Influence. *International Organization* 42, 3: 461-483.

Weimer Feature Essay...continued from page 8

the political economy literature, the book is much richer in its treatment of bureau behavior, factor suppliers, and oversight by the budgetary sponsor. Analytically, it employs comparative statics to assess the relative efficiency of bureaus, competitive supply, monopoly (discriminating and non-discriminating), and nonprofits (discriminating and non-discriminating). It thus provides a systematic comparison of institutional arrangements, an approach that is now one of the strengths of modern political economy.

The initial and central result of the book concerned allocative inefficiency when a budget-maximizing bureaucrat with a monopoly position in supply of a good chooses output levels in response to the valuation function of a passive budgetary sponsor such that the allocated budget is at least as large as the cost of producing the valued output. Rather than limiting supply (and the corresponding budget) to the point where marginal cost equals price (as would a competitive supplier) or marginal revenue (as would a monopolist selling in a market), the bureaucrat increases the budget by pushing supply either to the point where the budget allocation just equals the total cost of supply or, in the absence of a budget constraint, to the point where the marginal valuation by the budgetary sponsor falls to zero. In the former case, all resources are used to produce output, so there is no waste in the vernacular sense, but there is allocative inefficiency because output is too high from the social perspective. In the latter case, the budget may exceed the total cost of producing the output so that there is both allocative inefficiency and what Niskanen calls "fat."

After deciding to leave academia to become the chief economist for Ford Motor Company, Niskanen agreed to lead a weekly evening seminar for his public choice students in return for dinners. It became clear in these seminars that he was troubled because, in the most common case of a binding budget constraint, his model led only to allocative inefficiency—these bureaus produced too much but did so with technical efficiency. Drawing on a critique by Jean-Luc Migué and Gérard Bélanger (1974), Niskanen (1975) reformulated the objective function of the bureaucrat to depend on the present value of income and non-monetary perquisites. Each of these, in turn, depends on the level of output of the bureau and the difference between the budget and the minimum cost of supplying the output, or what he called the "discretionary budget" and others have called "organizational slack." Although this objective function is general and would apply to any executive, the key insight is that whereas a private manager typically gets a share of the discretionary budget, which in this case is just profit, civil service rules do not allow the bureaucrat to claim any of the discretionary budget as income. Therefore, the bureaucrat takes it as perquisites, including easier management through not putting full effort into making sure that labor and other resources are fully employed. Indeed, the bureaucrat has an incentive to hide the discretionary budget from the budgetary sponsor so as not to reveal information about the true costs of supplying output that might undercut the funding request of the bureau in the next round of budgeting.

Niskanen recognized that the full assessment of bureaucratic inefficiency required attention to the interests of the budgetary sponsor. When the sponsor could be viewed as a unitary actor, say holding the preferences of the median member, the outcome depended on bargaining within a bilateral monopoly. He was also sympathetic to more nuanced treatments of the interests of the budgetary sponsor and the nature of oversight, such as that provided by Gary Miller and Terry Moe (1983).

The empirical record of the usefulness of Niskanen's model of bureaucratic behavior is mixed - see André Blais and Stéphane Dion (1991) for summaries of the first two decades of empirical tests. Clearly, the empirical assessment of the implications of the budget-maximizing bureaucrat is complicated by the need to make assumptions about the nature of oversight by the budgetary sponsor. The assessment becomes even more difficult when one follows Niskanen (1975) in allowing bureaucratic preferences to be over income and perquisites rather than just the budget. Indeed, Niskanen (1991) recognized that some bureaucrats may be zealots for the output of the bureau, and described himself as a zealot for efficiency when he worked in the bureaucracy.

Bureaucracy and Representative Government was an amazing contribution in light of the tools available at the time. Niskanen relied almost exclusively on neoclassical mircoeconomic analysis of a complicated relationship that he nonetheless saw as both strategic and inherently involving imperfect information, features that later were more naturally addressed with game theoretic tools by Jeffrey Banks (1989) and Banks and Barry Weingast (1992). As a student and practitioner of operations research, he was familiar with the original forms of game theory but once confided to me that he thought it had become a "dry hole" in terms of social science innovation. (Interestingly for someone so influential in the public choice movement, he also expressed misgivings to me about social choice theory, which he thought was "Byzantine.") Nonetheless, he went a long way toward anticipating the key elements of agency theory, which has now become a common framing for bureaucracy.

I take the liberty of offering two additional personal observations about Niskanen. First, the sometimes harsh and cold logic of his public persona belied the warmth, gentility, and generousness I observed in his interaction with students. He was always open to discussing issues with students, even when they held very different views from his.

Second, he followed his beliefs even when they were costly to him personally. In the course he taught us on policy analysis, students asked him about how

continued on page 10

Feature Essay

Elinor Ostrom: The Legacy and The Challenge

Paul Dragos Aligica, George Mason University

Elinor Ostrom, co-founder with Vincent Ostrom of the Bloomington School of Political Economy, has left behind a fascinating intellectual legacy, currently a source of inspiration in fields as diverse as political philosophy and the environmental sciences. Yet, the core of her work has always been in the field of political economy. The Ostroms' distinctive approach was considered from the very beginning an evolving part of the "Public Choice Revolution" that exploded in the 1960s. As William C. Mitchell put it in his 1988 *Public Choice* article, "Virginia, Rochester, and Bloomington: Twenty-Five Years of Public Choice and Political Science," three distinct schools of thought, each associated with particular scholars, have shaped the basic public choice assumptions. In each case "one or two dominant figures led … the effort to construct theories of collective choice: Riker at Rochester, Buchanan and Tullock at various Virginia universities, and the Ostroms at Indiana."

The Ostroms began in the 1960s with a theory of collective action based on a theory of goods, a theory that was emerging at that time from the mantle of neoclassical economics as a major building block of the new, modern political economy. In time, their work on governance created one of the main channels of the transition from public choice to the new institutionalism. The fact that Elinor Ostrom was a recipient of the 2009 Nobel Prize in Economics was a telling recognition of Bloomington School's important contributions. Yet, in the celebratory and retrospective mood created by such honors and public recognition, it is important to note that the Bloomington agenda is far from making its closing arguments. In fact, Elinor Ostrom's work remains an enterprise of unassuming radicalism that persistently invites us to reconsider the very foundations and significance of our scientific efforts. Following the logic of institutional diversity, social heterogeneity, and value pluralism to their epistemic and normative implications, Ostrom's work both closes a cycle of research on collective action, institutions, and governance and frames the next stage or the next cycle of research.

The depth and boldness of the Ostrom project are revealed when we single out the specific assumptions and perspectives it challenged. We should also take full measure of the way in which those challenges constitute a radical departure from powerful ideas that tacitly framed a vast part of modern political economy. The list of these tacit assumptions is long, but a cluster of related candidates rises to the top in any account: agent and institutional homogenization as a theoretical and methodological default position, centralization and monocentrism as key principles of governance, and "seeing like a state" as an acquired *forma mentis* in thinking about political and economic affairs. Let's take these core assumptions that the Ostrom project challenged one by one.

The typical strategy of dealing with the challenge of heterogeneity is easily one of the main assumptions that the Ostrom project challenged. The homogenization by assumption of social agents, the rhetorical trick by which homogeneity is nominally recognized as a fact and a problem but then, in the next move, reduced to a modal profile, a homogenous "representative agent," with minimalist formal features, is both popular and influential. Versions of this strategy, operating at different levels and on different aspects of heterogeneity, are prevalent, from economics and public choice to political science and social philosophy. The logic of Ostrom's perspective challenges that approach. Furthermore, it explicitly links the problem of heterogeneity to that of institutional diversity: Because

continued on page 11

Weimer Feature Essay...continued from page 9

best to avoid ethical conflicts between doing good analysis and accommodating the preferences of clients. His response was, "Keep your bags packed." A few years later, we saw him live by these words when he left his position as chief economist at Ford rather than prepare analyses to support import quotas on automobiles, which he viewed as bad public policy.

References

Banks, Jeffrey S. (1989) Agency Budgets, Cost Information, and Auditing. *American Journal of Political Science* 33(3), 670-699.

Banks, Jeffrey S. and Barry R. Weingast (1992) The Political Control of Bureaucracies under Asymmetric Information. *American Journal of Political Science* 36(2), 509-524.

Blais André and Stéphane Dion, eds. (1991) *The Budget Maximizing Bureaucrat: Appraisals and Evidence*. (Pittsburgh: University of Pittsburgh Press).

Migué, Jean-Luc and Gérard Bélanger (1974) Toward a General Theory of Managerial Discretion. *Public Choice* 17(Spring), 27-51.

Miller, Gary J. and Terry M. Moe (1983) Bureaucrats, Legislators, and the Size of Government. *American Political Science Review* 77(2), 297-322.

Niskanen, William A., Jr. (1991) A Reflection on *Bureaucracy and Representative Government*. In Blais André Blaise and Stéphane Dion, eds., *The Budget Maximizing Bureaucrat: Appraisals and Evidence*. (Pittsburgh: University of Pittsburgh Press), 13-31.

__(1975) Bureaucrats and Politicians. *Journal of Law and Economics* 18(3), 617-643.

__(1971) *Bureaucracy and Representative Government* (Chicago: Aldine Atherton).

__(1968) The Peculiar Economics of Bureaucracy. *American Economic Review* 58(2), 293-305.

Aligica Feature Essay...continued from page 10

institutional arrangements in any society emerge largely as a response to heterogeneity, and in their turn are conditions of heterogeneity, institutional diversity should be a central (if not the central) theme of institutional theory. Yet, that doesn't seem to be the case in much of the literature. Models of "markets and hierarchies" remain pivotal, although the theoretical lenses of the theory of the market or the theory of the state are obviously incapable of capturing and illuminating the wide diversity of existing and possible institutional arrangements. A replacement of the classical dichotomous typology (markets and states) with a new trinity (markets, states, and networks) is not an adequate solution. By refusing to accept such solutions, the Ostromian approach looks commonsensical. Yet, when compared to the prevalent views, it is radical. Bloomington institutionalism is ready to take institutional diversity seriously - "beyond the Market and the State," "beyond Hobbes and Smith" - and to follow to the end its analytical and normative logic.

Needless to say, this approach, making a key point out of the reality of heterogeneity, is more relevant today than ever. Diverse values, identities, principles and cultures clash in the global arena. Emigration, increasingly diverse populations within the boundaries of nation-states, demography and culture, increasingly technology-driven social segmentation and cultural heterogeneity - all challenge governance systems not only at the global and national levels but also, ever more so, at the local level. All these features revive the theme of pluralism, diversity and collective action with unprecedented intensity. The increasing preoccupation with elements of heterogeneity in current political and economic theory is unavoidable, once heterogeneity is recognized as a key feature of social reality and as a genuine political and economic challenge. In what measure is it possible to have an institutional order defined by freedom, justice, prosperity, and peace in an increasingly interdependent world of diverse and conflicting views, beliefs, preferences, values, and objectives? This is a discussion about the fundamental nature of governance (both domestic and international) in the new era. With it, we are at the core of the major political and economic challenges of our age. And at the same time, we are at the cutting edge of contemporary social science and political philosophy. The empirically grounded, applied institutional analysis of the possibility of social order, governance, and economic performance in extreme conditions, lacking consensus or convergence of beliefs, preferences and values, seems to be indeed the new frontier.

The theme of institutional diversity leads to the next contender on the list of challenged assumptions: the bias toward centralization and against polycentric, decentralized arrangements. When it comes to governance in complex social systems, the ideal of monocentrism (usually thought of as an idealized model or image of the nation state) seems to have become the default mode in thinking about governance. Ostrom and her associates challenge that. They instead advance the notion of polycentrism. Even before becoming a normative challenge, polycentricity is a challenge on descriptive, positive grounds. The elemental starting point is the basic condition of complex human societies. In real life there are multiple, competing, and overlapping action arenas, institutional levels and sources of decision-making and authority: from families, communities, and religious organizations to nations and states. None of them has absolute power and authority in the life of an individual in all aspects, at all times, and in any circumstance. This dispersion of action arenas and authority is a social fact, a feature that defines complex human societies. Thinking about society as being something created, granted, sanctioned by (and depending on) a centralized, ultimate and unique all-monitoring source of authority and power violates principles of positive political economy. It also violates normative principles. The image that emerges from the Ostrom project is one of a conglomerate of diverse institutions, materializing various degrees and forms of decentralization and centralization through functional differentiation and historical accidents, in an ongoing process of change. Inside this process, the typical institutional forms of the national state, market, or democracy take shape, or decline and fade away, form and reform, as time goes by. A governance system, polycentric governance, may emerge out of these processes, under certain conditions. If so, it will have properties favorable to institutional performance and human flourishing.

The polycentrism vision advanced by the Ostroms thus suggests that we should be cautious when advancing claims of preeminence for various institutions, be they the democratic state or the free market. Again, that caution may sound commonsensical, but when compared with once-prevalent views, it is rather radical. We should avoid looking at the world through one and only one pair of conceptual lenses. And we should avoid thinking about governance assuming one and only one institutional arrangement. The retreat from pluralism is dangerous, both on analytical and on normative grounds. With this remark, we have reached the third member of the challenged-assumptions list: "seeing like a state."

Reading through the academic literature dedicated to governance, one cannot avoid the feeling that a large part of it is written as if the public-choice revolution had never taken place, as if no new insights regarding the potential and limits of centralized, state-centered, technocratic, and bureaucratic government had been brought to the fore in the last several decades. A state-centric perspective brushes aside a large part of available empirical and theoretical knowledge on the nature, functioning, and performance of the particular institutional arrangement called the modern state. On the other

continued on page 12

Aligica Feature Essay...continued from page 11

side, the Ostroms introduced a public-choice-inspired, but post-public-choice attitude: an attempt to rethink the theory and practice of governance, as if public choice made a difference. Their logic incorporates first- and second-generation public choice insights and lessons, and reconsiders institutional theory and the problems of governance in that light. This approach simply follows the implications of public-choice lessons and attempts to substitute the state-centered view with a pluralist one: a reasonable idea of "institutional portfolio diversification," in a context in which the vulnerabilities and dangers of relying on only one institutional arrangement have become so obvious. Yet, again, this is an idea that may sound commonsensical, but when compared with then-prevalent views, is rather radical.

How powerful, subtle, and profound is the intellectual current against which the Ostrom approach ran is best illustrated by the reception of the Bloomington scholars' work itself. If anything, their work demonstrated that "seeing like a state" is not the unavoidable way to think about collective-action problems and their solutions. Collective-action solutions do not necessarily involve seeing like a state, acting like a state, or mobilizing the state. Yet, surprisingly often, despite Ostrom's explicit and persistent efforts to disentangle the idea of collective action from that of "The State," and to demonstrate that successful collective action doesn't need a Leviathan, one finds her work invoked to support state-sponsored arrangements, wrapped in vague notions of "democracy" and "participation," all having nothing to do with the spirit or the letter of that work. All done, by all accounts, in good faith. The power of the "seeing like a state" *forma mentis* among an important part of the relevant epistemic community is profound. In brief, the Ostroms' attempt to follow up on the public-choice revolution's insights, and to try to move studies of governance away from a state-centered view to a pluralist and polycentric one, remains a work in progress, one that sounds as radical today as it did thirty years ago.

Looking back, by their own account, Elinor and Vincent Ostrom saw their lifetime endeavors as having been part of a long and illustrious intellectual tradition contributing to the "science and art of the association," that indispensable constituent of a self-governing society of free individuals. Considered in this light, their work has been a continuous effort to articulate an alternative way of looking at governance and institutional order: probably "seeing like a citizen" is one good contender for a name for it; "seeing like a self-governing human being" is another. Their endeavors were driven by two convictions: First, one cannot build a self-governing society of free individuals when the prevalent, elite mode of thinking about institutional order and governance thinks like a state and strives for monocentrism. Second, the ideal of a society of self-governing, fallible, but capable human beings who are able to master the "art and science of the association" is worth pursuing because in it lies a powerful self-fulfilling prophecy. In the end, it is all about preserving, developing, and disseminating as much as possible the knowledge of this unique and fragile but vital "art and science."

Article based on excerpts from *Institutional Diversity and Political Economy. The Ostroms and Beyond*, Oxford University Press (forthcoming, December, 2013).