

**Article: “Top Twenty Commentaries”**  
**Author: Lee Sigelman, etal.**

**Issue: November 2006**  
**Journal: *American Political Science Review***



***This journal is published by the American Political Science Association. All rights reserved.***

---

APSA is posting this article for public view on its website. APSA journals are fully accessible to APSA members and institutional subscribers. To view the table of contents or abstracts from this or any of APSA's journals, please go to the website of our publisher Cambridge University Press (<http://journals.cambridge.org>).

This article may only be used for your personal, non-commercial use. For permissions for all other uses of this article should be directed to Cambridge University Press at [permissions@cup.org](mailto:permissions@cup.org).

## TOP TWENTY COMMENTARIES

## The American Political Science Review Citation Classics

—Lee Sigelman, *The George Washington University*

In any scholarly field, some works are widely acknowledged as classics, whereas the rest—indeed, the great majority—are little noted nor long remembered. Indeed, according to the “Iron Law of Important Articles,” the number of significant articles increases only to the extent of the square root of the number of published articles (Holub, Tappeiner, and Eberharter 1991). It follows that as a research literature grows, important articles constitute an ever-decreasing proportion of the total output.

Which of the thousands of articles that have appeared in the *Review* over the years stand out as the most important? The answer obviously depends on how “importance” is defined. One standard approach is to gauge an article’s importance in terms of the attention it has received within the scholarly community, as indicated by the number of references to it in the research literature. Although this approach is open to criticism on many grounds,<sup>1</sup> it is, for want of a better alternative, the approach pursued here.

Most of the articles that have appeared in the *Review* since its inception in 1906 have rarely if ever been cited. At the opposite extreme, some have had a substantial impact by political science standards, though not necessarily by the standards of at least one of our neighboring disciplines. Overall, by the end of 2005, some 155 *Review* articles had been cited 100 or more times, and one (Peter Bachrach and Morton S. Baratz’s “Two Faces of Power”) had been cited more than 500 times. By comparison, more than twice as many articles in the *American Sociological Review*, the “flagship” journal in sociology, had topped 100 citations, and a score had surpassed the 500 mark (Jacobs 2005, 1).<sup>2</sup> This interdisciplinary difference, which is especially striking because political science is a much larger discipline than sociology, appears for the most part to be a manifestation of the attention that sociological research receives from outside of sociology—a phenomenon less apparent in political

Lee Sigelman is the editor of the *Review*. Lee C. Michael provided invaluable research assistance, and Robert Axelrod offered insightful comments. The inspirations for this project were Jerry Jacobs’s analysis of citations to articles published in the *American Sociological Review*, 1936–2004 (Jacobs 2005), and Fred Shapiro’s solicitation of authors’ retrospectives on articles published in the *Yale Law Journal* (Shapiro 1991). The citation counts given here were obtained between October 26 and November 9, 2005, from the online *ISI Social Sciences Citation Index* (Thompson Scientific 2005).

<sup>1</sup> For a recent overview of criticisms of using citation frequencies as a measure of scholarly impact, see Monastersky 2005.

<sup>2</sup> Although the citation frequencies of the leading *Review* articles pale by comparison to those of the leading *ASR* articles, by other standards they are relatively high. For example, only one article published in top accounting journals between 1963 and 1982 was cited as many as 40 times (Brown and Gardner 1985, 86–88).

**TABLE 1. High-Impact (100+ Citations) Review Articles, by Decade**

Decade	Articles with 100+ Citations	Percentage of Total
1906–1915	0	0%
1916–1925	0	0%
1926–1935	0	0%
1936–1945	0	0%
1946–1955	1	1%
1956–1965	14	9%
1966–1975	49	32%
1976–1985	37	24%
1986–1995	51	33%
1996–2005	3	2%
Total	155	101%

science.<sup>3</sup> It is the very rare *Review* article that has become a standard point of reference or an object of encompassing interest for those in other disciplines, consistent with the image of political science as a “borrower” discipline theoretically and methodologically.

Table 1 shows when each member of the *Review*’s “100+ Club” appeared. The earliest (L. S. Shapley and Martin Shubik’s “A Method for Evaluating the Distribution of Power in a Committee System”) was published in 1954, and only two others (Seymour Martin Lipset’s “Some Social Requisites of Democracy” and Robert A. Dahl’s “A Critique of the Ruling Elite Model”) hail from the 1950s.<sup>4</sup> The numerous articles that even the most illustrious political scientists of an earlier era (e.g., Carl Friedrich, V. O. Key, Harold Lasswell, Charles Merriam, and Hans Morgenthau, to name just a few) contributed to the *Review* are now largely forgotten, bespeaking political scientists’ inattentiveness to their disciplinary forebears.<sup>5</sup>

At the other end of the time scale, within just a few years of their publication in 2000, two *Review* articles (Paul Pierson’s “Increasing Returns, Path Dependence, and the Study of Politics” and Gary King et al.’s “Analyzing Incomplete Political Science Data: An Alternative Algorithm for Multiple Imputation”) had already joined the 100+ Club. A more typical citation path for frequently cited articles, though, is the one followed by “Two Faces of Power.”<sup>6</sup> As can be seen in Figure 1, the *Review*’s all-time citation leader was by no means an instant classic. It was not until several years after it appeared that citations to it reached double digits per year; but since rising to that level it has displayed impressive staying power, belying the notion that scholarly articles “have only a brief moment in the sun when they are likely to be cited frequently”

<sup>3</sup> An obvious exception is the Bachrach–Baratz article, which has been very widely cited outside of political science. On the citation of *ASR* articles outside of sociology, see Clemens, Powell, McIlwaine, and Okamoto 1995, 460.

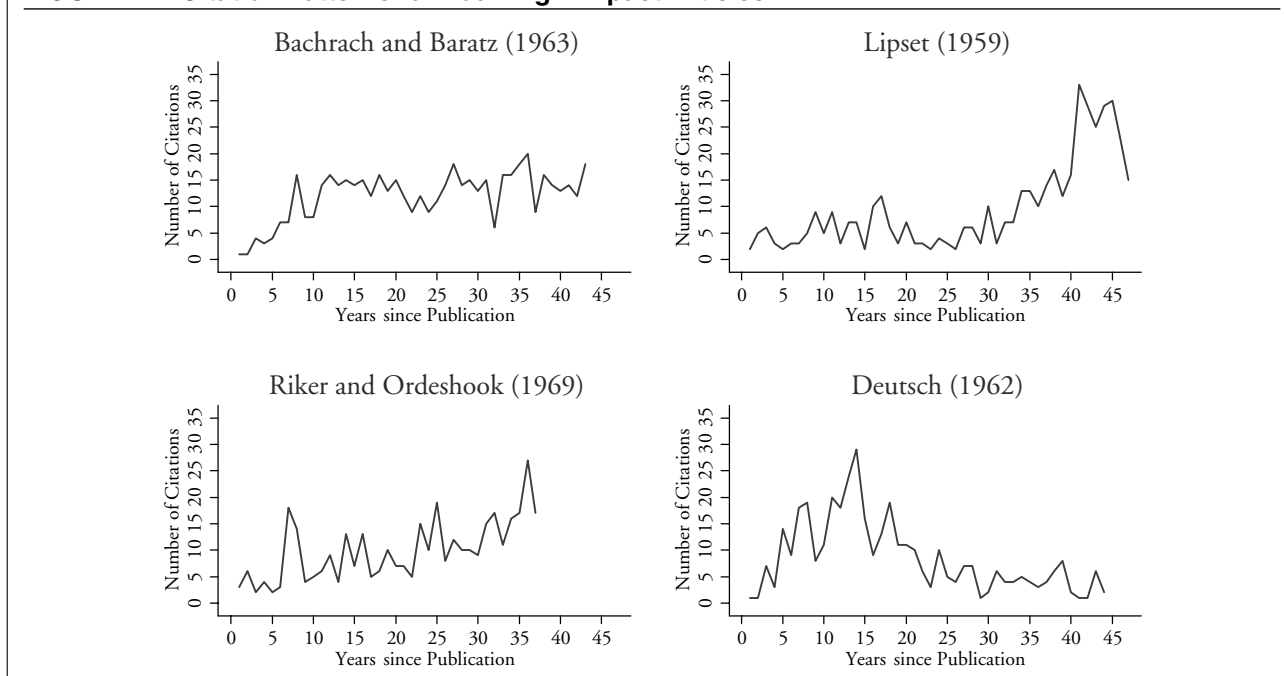
<sup>4</sup> By contrast, 16% of the *ASR* articles in the 100+ category were published prior to 1960 (Jacobs 2005, 1).

<sup>5</sup> Of course, many of these authors’ main contributions were published as articles in other journals or as books. Even so, I think that the general point holds.

<sup>6</sup> For a more general consideration of growth and decay in citations, see, MacRae 1969.

**TABLE 2. The 20 Most Frequently Cited *Review* Articles, 1945–2005**

Rank	Number of Citations	Author(s)	Year Published	Title of Article	Volume, Date, Pages
1	543	Peter Bachrach and Morton S. Baratz	1962	Two Faces of Power	56 (December): 947–52
2	492	Douglas A. Hibbs, Jr.	1977	Political Parties and Macroeconomic Policy	71 (December): 1467–87
3	482	Jack L. Walker	1969	The Diffusion of Innovation among American States	63 (September): 880–99
4	467	Gerald H. Kramer	1971	Short Term Fluctuations in U.S. Voting Behavior, 1896–1964	65 (March): 131–43
5	457	Warren E. Miller and Donald E. Stokes	1963	Constituency Influence in Congress	57 (March): 45–56
6	456	James G. March and Johan P. Olsen	1984	The New Institutionalism: Organizational Factors in Political Life	78 (September): 734–49
7	455	Seymour Martin Lipset	1959	Some Social Requisites of Democracy: Economic Development and Political Legitimacy	53 (March): 69–105
8	398	Nathaniel Beck and Jonathan N. Katz	1995	What to Do (and Not to Do) with Time-Series Cross-Section Data	89 (September): 634–47
9	387	David R. Cameron	1978	The Expansion of the Public Economy: A Comparative Analysis	72 (December): 1243–61
10	380	Karl W. Deutsch	1961	Social Mobilization and Political Development	55 (September): 493–514
11	377	William H. Riker and Peter C. Ordeshook	1968	A Theory of the Calculus of Voting	62 (March): 25–42
12	360	L. S. Shapley and Martin Shubik	1954	A Method for Evaluating the Distribution of Power in a Committee System	48 (September): 787–92
13	345	Herbert McClosky	1964	Consensus and Ideology in American Politics	58 (June): 361–82
14	340	Arthur H. Miller	1974	Political Issues and Trust in Government: 1964–1970	68 (September): 951–72
15	317	Robert Axelrod	1986	An Evolutionary Approach to Norms	80 (December): 1095–1111
16	311	Michael W. Doyle	1986	Liberalism and World Politics	80 (December): 1151–69
17	287	Nelson W. Polsby	1968	The Institutionalization of the U.S. House of Representatives	62 (March): 144–68
18	282	Ronald Inglehart	1971	The Silent Revolution in Europe: Intergenerational Change in Post-Industrial Societies	65 (December): 991–1017
19	278	Zeev Maoz and Bruce Russett	1993	Normative and Structural Causes of Democratic Peace, 1946–1986	87 (September): 624–38
20	259	Edward R. Tufte	1975	Determinants of the Outcome of Midterm Congressional Elections	69 (September): 812–26

**FIGURE 1. Citation Patterns for Four High-Impact Articles**

(Stewart 1983, 171). Most of the other articles on the *Review*'s "Top 20" list have followed more or less the same route, but some notable exceptions are also shown in Figure 1: the late blooming of Seymour Martin Lipset's "Some Social Requisites of Democracy," the quasi-linear growth of references to William H. Riker and Peter C. Ordeshook's "A Theory of the Calculus of Voting," and the surge and decline in the impact of Karl W. Deutsch's "Social Mobilization and Political Development."

Space considerations preclude listing all 155 articles with 100 or more citations.<sup>7</sup> The top 20 are identified in Table 2. Why have these articles in particular stood out from all the rest? Citation frequency data alone can only identify the standouts and cannot even begin to account for their prominence.<sup>8</sup> Thus, for pertinent insights let us turn to those who are best qualified to comment: the distinguished scholars who either produced these articles or who are standing in for those who did.<sup>9</sup>

<sup>7</sup> A full listing is available on request.

<sup>8</sup> As Garfield (1979, 246) puts it, citation counts "say nothing about the nature of the work, nothing about the reason for its utility or impact." For an exploration of the determinants of the citation frequencies of articles in one field, see Ayres and Vars 2000.

<sup>9</sup> From the authors of the twenty most frequently cited *Review* articles or appropriate commentators on those articles whose authors are deceased or otherwise unavailable, I requested brief observations about why an article had such an impact, whether their views have changed since it was published, and how, in retrospect, they might write it differently.

## REFERENCES

- Ayres, Ian, and Frederick E. Vars. 2000. "Determinants of Citations to Articles in Elite Law Reviews." *Journal of Legal Studies* 29 (January): 427–50.
- Brown, Lawrence D., and John C. Gardner. 1985. "Using Citation Analysis to Assess the Impact of Journals and Articles on Contemporary Accounting Research." *Journal of Accounting Research* 23 (Spring): 84–109.
- Clemens, Elisabeth S., Walter W. Powell, Kris McIlwaine, and Dian Okamoto. 1995. "Careers in Print: Books, Journals, and Scholarly Reputations." *American Journal of Sociology* 101 (September): 433–94.
- Garfield, Eugene. 1979. *Citation Indexing: Its Theory and Application in Science, Technology, and Humanities*. New York: Wiley.
- Holub, Hans Werner, Gottfried Tappeiner, and Veronika Eberharter. 1991. "The Iron Law of Important Articles." *Southern Economic Journal* 58 (October): 317–28.
- Jacobs, Jerry A. 2005. "ASR's Greatest Hits." *American Sociological Review* 70 (February): 1–3.
- MacRae, Duncan, Jr. 1969. "Growth and Decay Curves in Scientific Citations." *American Sociological Review* 34 (October): 631–35.
- Monastersky, Richard. 2005. "The Number That's Devouring Science." *Chronicle of Higher Education* 52 (14 October): A12.
- Shapiro, Fred R. 1991. "The Most-cited Articles from *The Yale Law Journal*." *Yale Law Journal* 100 (March): 1449–1514.
- Stewart, John A. 1983. "Achievement and Ascriptive Processes in the Recognition of Scientific Articles." *Social Forces* 62 (September): 166–89.
- Thompson Scientific. 2005. *ISI Web of Knowledge. Web of Science. Social Sciences Citation Index*. Philadelphia: Thompson Scientific Corporation. <http://isiknowledge.com/> (accessed October 26–November 9, 2005).

**1. Peter Bachrach and Morton S. Baratz. 1962. "Two Faces of Power." *American Political Science Review* 56 (December): 947–52.** Cited 543 times.

—Clarence Stone, *University of Maryland*

The year after the 1961 publication of Robert Dahl's *Who Governs?*, "Two Faces of Power" criticized the treatment of power in this about-to-become classic work. Offering a third way neither elitist nor pluralist, Bachrach and Baratz made no defense of Floyd Hunter's reputational approach and they largely by-passed the literature on social stratification. Instead of revisiting past controversy, they sought to identify a fundamental feature of the political process, offering the second face of power as a wide view of how the game of politics is played.

Consider an analogy. Economists study markets in which businesses compete, subject to the axial constraint of supply and demand. From Adam Smith on, they have supposed a largely benign process. Antitrust advocates see a more complex game in which players seek to rig the rules and squeeze out competition. *Who Governs?* treats electoral competition as the political counterpart to the free market of classical economics. The process is open and above board, with power observable in the give-and-take of initiatives and vetoes. In "Two Faces of Power," Bachrach and Baratz assume an antitrust perspective and ask what might not be revealed in immediately observable transactions. Rather than follow the action on contested issues, they urge scholars to step back and assess the flow of the decision process in order to fathom the constraints within which decisions are made. How did these constraints take shape? What political edge do they provide, for whom, and how are they maintained? Actors not only expend energy promoting and opposing discrete policy decisions (the first face of power) but also invest resources in arrangements through which strategic advantages are secured (the second face of power).

Bachrach and Baratz reject the notion that conflict is a neutral arena in which power is exercised. Instead, standing on the shoulders of Schattschneider, they argue that the contours of conflict are themselves a manifestation of power—at least partially the results of efforts to protect privileges and head off unwanted challenges. "Two Faces of Power" forms an important link between the notion of the mobilization of bias and current interest in the new institutionalism. Bachrach and Baratz treat politics as a drama in which no foregone conclusion is played out and a rich mixture of elements is manipulated by the players. Issue framing, investment of effort to create or protect procedures for governing, and strategic alliances provide building blocks from which durable political arrangements are constructed. Yet, although seeing politics as tending toward

---

Peter Bachrach is Emeritus Professor of Political Science at Temple University. He joined the Temple faculty after leaving Bryn Mawr College, where he first collaborated with Morton Baratz.

Morton Baratz was a long-time faculty member at Bryn Mawr College, where he served as Chair of the Department of Economics. He subsequently joined the faculty at Boston University where he was director of the Urban Institute, and later served as Vice Chancellor of the University of Maryland, Baltimore County.

Clarence Stone is Research Professor of Political Science and Public Policy at The George Washington University and Professor Emeritus of Political Science at the University of Maryland. He has been honored with the APSA's Ralph J. Bunche Award and its Career Achievement Award.

bias, Bachrach and Baratz stay clear of determinism in its various forms. In their treatment, political systems are not easily penetrable by the disaffected, but political fixes can be overturned or at least modified with the right set of countermoves.

Writing when the behavioral "revolution" had newly gained ascendancy and universal laws of causality were the pot of gold at the end of the research rainbow, Bachrach and Baratz neither embraced the new science of politics nor sought to dismiss it (even though their article was included in the 1967 antibehavioral collection edited by McCoy and Playford [1967]). "Two Faces of Power" and its follow-up study of antipoverly politics in Baltimore took seriously the need for empirical findings, but also acknowledged a guiding interest in social justice. By treating inequality as a central problem, Bachrach and Baratz asked not only "who gets what and how" but also "who gets left out and how" (1980, p. 105). Thus they resisted a narrow treatment of politics and instead suggested scenarios showing why a broad conception is important.

Although bowing to the precept of observability, Bachrach and Baratz expanded the ground rules for the study of political power so as to allow freely for qualitative assessments, informed by normative concerns. They were less concerned with measuring power precisely than with understanding how strategic disadvantage is perpetuated, but might nevertheless be overcome. Were they to enter the discourse today, they would face the continuing challenge of how to differentiate the creative part that political agency can play in a context heavily structured with uneven advantages. The genius of Bachrach and Baratz was to open a line of inquiry so vital to the study of politics that the quest remains, even as partial and important insights accumulate.

## REFERENCES

- Bachrach, Peter, and Morton Baratz. 1980. *Power and Poverty*. New York: Oxford University Press  
 Dahl, Robert A. 1961. *Who Governs?* New Haven: Yale University Press.  
 McCoy, Charles, and John Playford, eds. 1967. *Apolitical Politics*. New York: Thomas Y. Crowell.

**2. Douglas A. Hibbs, Jr. 1977. "Political Parties and Macroeconomic Policy." *American Political Science Review* 71 (December): 1467–1487.** Cited 492 times.

—Douglas A. Hibbs, Jr., *Göteborg University*

I began working on "Political Parties and Macroeconomic Policy" in 1974, delivered the paper at the August 1975 meetings of the World Econometric Society in Toronto and the APSA in San Francisco, and shortly thereafter submitted it to the *Review*, where it languished for more than a year before it was accepted (just barely, I sensed) after a change of editor. The article's title is somewhat misleading because the evidence pertained to the influence of government partisanship on macroeconomic outcomes.

---

After holding faculty positions at MIT and Harvard, as well as teaching across Europe and the United States, Douglas Hibbs became a Professor of Economics at Göteborg University, from which he retired in 2005. He remains a senior fellow at the Center for Public Sector Research at Göteborg University.

Analysis of how partisan forces affect intervening policies came afterward in a series of my own works (Hibbs 1986, 1987, 1994; Hibbs and Dennis 1988) and in important research by many others. The closing sentences of the 1977 article sum up what motivated my research: “Macroeconomic outcomes . . . are influenced to a significant extent by long- and short-term political choices. The real winners of elections are perhaps best determined by examining the policy consequences of partisan change rather than by simply tallying the votes.”

My orientation departed from those prevailing at the time. In the 1960s and early 1970s, economists viewed policy analysis mainly in terms of benign government planners aiming to maximize “social welfare” functions subject to constraints. During the same period, political scientists regarded voting and elections as largely *sui generis*—as autonomous end points of research. The agenda-setting framework for the political economy of electoral democracies was Downs’s monumental 1957 treatise *An Economic Theory of Democracy*, which assumed purely office-motivated policy behavior. I felt that those orientations mistakenly neglected conflicts of economic interest among voters and associated cleavages in economic objectives among parties that are fundamental forces shaping macroeconomic policies and outcomes.

During the course of my research I learned of an unpublished 1974 paper by Tufte on electoral cycles in economic policy (which formed the core of his famous 1978 book *Political Control of the Economy*) and of the prepublication version of Nordhaus’s famous 1975 article “The Political Business Cycle”—both based on the Downsian assumption of election-driven policy. The Tufte and Nordhaus papers prompted me to test for election period signals in U.S. unemployment as a side hypothesis to my primary focus on partisan influences, and in a footnote I reported being unable to detect significant effects related to the U.S. presidential election calendar. The evidence accumulated over the subsequent 30 years in hundreds of studies of dozens of democracies by and large squares with the inferences that I drew from sparse data in my 1977 article: partisan effects on macroeconomic variables are commonplace, whereas election calendar effects are sporadic, although, as Drazen (2001) pointed out in a review article, there are signs of regular preelection movements in fiscal variables of the sort featured in Tufte’s work.

The political–distributional foundations that I laid out for the partisan model—that the interests and preferences of core party constituencies predispose Left governments to pursue relatively expansive policies intended to raise growth and lower unemployment and Right governments to pursue relatively restrictive policies designed to contain inflation—did not arouse much controversy. The main object of contention was the macroeconomic foundation: an exploitable Phillips curve hinging on “nonrational” adaptive price expectations. That foundation was dealt a crippling blow in academic circles by the rational expectations revolution in macro theory that was sweeping the hearts and minds of up-and-coming economists just as my work on partisan cycles (and Nordhaus’ work on electoral cycles) appeared.

About a decade later, partisan models incorporating rational expectations and derived from nominal “surprise” theories of business cycles then in vogue were proposed by Chappell and Keech (1986) and Alesina (1987). These so-called “rational partisan theory” setups rested on the hypothesis that monetary (inflation) surprises created by unanticipated election victories by parties with divergent macroeconomic objectives were the source of partisan effects

on output and unemployment observed in data. However, despite the best efforts of Alesina and collaborators to make the case for partisan policy surprise models in a series of empirical studies, surprise theories of business cycles, whether or not rooted in unanticipated changes of governing parties, have been abandoned owing to the lack of supporting evidence.

The facts are that unanticipated changes in nominal aggregates (the money supply, the price level) have nothing much to do with expansions and contractions of output and employment, while Phillips curves founded on sluggish price adjustment yield remarkably good fits to the data. For those reasons traditional Phillips curves—which never died among politicians and central bankers, most of whom always perceived a dynamic policy tradeoff between inflation and unemployment—have since made a comeback among mainstream, empirically grounded macroeconomists. A good example is Mankiw’s 2001 article “The Inexorable and Mysterious Tradeoff Between Inflation and Unemployment.”

My own program for advancing partisan theory was spelled out in my 1994 article. The central idea is that partisan policymakers are uncertain about the structure of the macroeconomy and hence about the impact of policies on stimulation of extra output and employment as opposed to needless extra inflation. Partisan objectives are consequently adjusted dynamically in the light of experience. Franzese and Jusko (2006) review a large body of research on partisan cycles conditioned on a much broader range of structural, institutional, and strategic contexts. This rich literature takes us in the right direction and likely will progress steadily and productively.

## REFERENCES

- Alesina, Alberto. 1987. “Macroeconomic Policy in a Two-party System as a Repeated Game.” *Quarterly Journal of Economics* 102 (August): 651–78.
- Chappell, Henry W., Jr., and William R. Keech. 1986. “Party Differences in Macroeconomic Policies and Outcomes.” *American Economic Review* 76 (May): 71–74.
- Downs, Anthony. 1957. *An Economic Theory of Democracy*. New York: Harper.
- Drazen, Allan. 2001. “The Political Business Cycle After 25 Years.” In *NBER Macroeconomics Annual 2000*, ed. Ben S. Bernanke and Kenneth Rogoff. Cambridge, MA: MIT Press, 75–117.
- Franzese, Robert J., Jr., and Karen Long Jusko. 2006. “Political-Economic Cycles.” In *Oxford Handbook of Political Economy*, ed. Donald Wittman and Barry Weingast. Oxford, UK: Oxford University Press, 545–64.
- Hibbs, Douglas. 1986. “Political Parties and Macroeconomic Policies and Outcomes in the United States.” *American Economic Review* 76 (May): 66–70.
- Hibbs, Douglas. 1987. *The American Political Economy: Macroeconomics and Electoral Politics*. Cambridge: Harvard University Press.
- Hibbs, Douglas. 1994. “The Partisan Theory of Macroeconomic Cycles: More Theory and Evidence.” *Economics and Politics* 6: 1–23.
- Hibbs, Douglas, and Christopher Dennis. 1988. “Income Distribution in the United States.” *American Political Science Review* 82 (June): 467–90.
- Mankiw, N. Gregory. 2001. “The Inexorable and Mysterious Tradeoff Between Inflation and Unemployment.” *Economic Journal* 111 (May): C45–C61.
- Tufte, Edward. 1978. *Political Control of the Economy*. Princeton: Princeton University Press.
- Nordhaus, William D. 1975. “The Political Business Cycle.” *Review of Economic Studies* 42 (April): 169–90.

**3. Jack L. Walker Jr. 1969. "The Diffusion of Innovations Among the American States." *American Political Science Review* 63 (September): 880–99. Cited 482 times.**

—Frank R. Baumgartner, *The Pennsylvania State University*

Jack Walker was the author of three of the 100 most cited articles in the history of the *Review*.<sup>1</sup> Nowhere is his creativity and imagination more on display than in his "Diffusion" article. Comparing the three articles allows some conclusions about why "Diffusion" had such a great impact and illustrates the thinking of a great scholar, mentor, and colleague.

Jack was a contributor to many fields. After I invited Jack to give a lecture some years ago, a colleague stopped me to say how glad he was that a theorist was visiting. Another thanked me for inviting a state politics expert; a third was thrilled to have an interest-group scholar; and a fourth, an agenda-setting pioneer. Jack's first big splash in the profession came when he was just two years into the tenure track, when he took on some of the biggest establishment figures in the discipline with his provocative "Critique of the Elitist Theory." It was combative, addressed major issues of power, and was perfectly timed to coincide with the rise of a new, more critical form of pluralist analysis. (Graduate students take note: it was also a revision of an essay he had drafted for his qualifying exams.) His second major contribution was "Diffusion," also written as an assistant professor. His third major article, "Origins and Maintenance," took aim at a major theme in the literature on group mobilization, suggesting important ways that elite-level actors, including the state itself, affect social mobilization. The continuing upward slope in citations to "Diffusion" and "Origins" make clear that these articles still attract significant attention even decades after their original publication.

As influential as "Critique" and "Origins" have been, "Diffusion" is clearly in a different class. What makes this so? Unlike the other two articles, "Diffusion" is neither critical nor combative. Rather, it launched an entirely new field of research. Like both "Critique" and "Origins," but to an even greater extent, "Diffusion" has attracted considerable attention outside of political science. Indeed, more than four of every ten citations of it have been from other disciplines, including sociology, economics, health, law, education, business, geography, and several others—20 in all, by my count.

Overseeing an internship program in Lansing when he was an assistant professor, Jack noted that lawmakers often asked agency officials whether other states provided precedents for programs under review (Interview with Jack Walker). He was struck by the rise of national professional communities, experts who had intense communications within networks of expertise rather than within the local political environment. This concern with "knowledge communities" informed his later work on lobbying as well. It typifies his approach to political science, for he took an idea that had barely been noticed by others but was there for all to see. It had wide-ranging applicability and implications, as the subsequent literature demonstrated. The work has been cited in 20 different disciplines, and it is almost as widely used outside of political science as within our discipline. Within political science, the

---

Jack Walker was a member of the Political Science faculty at the University of Michigan before his untimely death.

Frank R. Baumgartner is Distinguished Professor of Political Science at the Pennsylvania State University. Jack Walker was his senior honors thesis advisor, dissertation committee member, co-author, and sometimes tennis partner.

<sup>1</sup> In addition to "Diffusion," his "Critique of the Elitist Theory of Democracy" and "The Origins and Maintenance of Interest Groups" were 59th and 95th, respectively.

article led to some controversy and methodological critique (something Jack never minded!), and generated an entire new field of research that remains vibrant today (in fact, three other articles on diffusion, by Mohr, Gray, and Berry and Berry, are on the *Review's* 100+ list). Not everyone who has cited "Diffusion" has read it carefully; as the first cite in its field, it appears often to be used as a simple reference to justify an assumption that diffusions do, in fact, spread rapidly. In fact, Jack was just as interested in those innovations that did not diffuse and in those states that were proud to be laggards.

One sign of Jack's creativity comes from remembering that all this stemmed from a department head's assignment to supervise an internship program in the state capital. With nothing else to do between meetings, he developed an entirely new research paradigm. The list of scholars having published three articles in the list of top *Review* citation-getters includes such luminaries as Warren Miller, Donald Stokes, Ronald Inglehart, Phillip Converse, and others. No one appears on this list four times. If Jack had not been killed in a car accident sixteen years ago, in the prime of his career, he may well have broken that tie.

## REFERENCES

- Berry, Frances Stokes, and William D. Berry. 1990. "State Lottery Adoptions as Policy Innovations: An Event History Analysis." *American Political Science Review* 84 (June): 395–415.
- Gray, Virginia. 1973. "Innovation in the States: A Diffusion Study." *American Political Science Review* 67 (December): 1174–85.
- "Interview with Jack Walker." *Current Contents* February 11, 1985.
- Mohr, Lawrence B. 1969. "Determinants of Innovation in Organizations." *American Political Science Review* 63 (March): 111–26.
- Walker, Jack L., Jr. 1966. "A Critique of the Elitist Theory of Democracy." *American Political Science Review* 60 (June): 285–95.
- Walker, Jack L., Jr. 1983. "The Origins and Maintenance of Interest Groups." *American Political Science Review* 77 (June): 390–406.

**4. Gerald H. Kramer. 1971. "Short-Term Fluctuations in U.S. Voting Behavior, 1896–1964." *American Political Science Review* 71 (March): 131–43. Cited 467 times.**

—Howard Rosenthal, *New York University and Russell Sage Foundation*

Gerald Kramer, Steven Brams, Peter Ordeshook, Samuel Popkin, and I were MIT undergraduates interested in political science in the late 1950s. Kramer, after taking a job at Rochester under the new chairman, William Riker, spent 1966–1967 at the Cowles Foundation at Yale University studying econometrics under Marc Nerlove. He began investigating the question of whether macroeconomic fluctuations had any effect on voting behavior. After discovering, somewhat to his (and Nerlove's) surprise, that there did seem to be some effect, he began exploring the question in depth,

---

Gerald Kramer completed his doctoral work at MIT in 1965 and joined the Department of Political Science at the University of Rochester in 1966. He subsequently served as a faculty member at Yale and then at the California Institute of Technology, from which he retired as Professor of Political Science in 1985.

Howard Rosenthal is a Professor of Politics at New York University and a visiting scholar at the Russell Sage Foundation. He is a Fellow of the American Academy of Arts and Sciences and at the John Simon Guggenheim Memorial Foundation.

He is indebted to Gerald Kramer for extensive comments and revisions to this piece.

developing a more formal theoretical framework and also using several different econometric approaches, which jointly led to this article.

The question had been studied by several earlier researchers, but in rather primitive fashion and with inconclusive results. Kramer wrote an extensive, thoughtful literature review. His analysis represented a giant step in methodology with respect to almost everything published previously. One innovation was that he did not just write down a regression equation with an *ad hoc* collection of plausible-sounding variables, but derived the equation from a formal model, based on “bounded rationality” assumptions (rationalized more fully in Kramer and Lepper 1972). The model started by recognizing that voting on the basis of the incumbent’s past performance is a shortcut for voters who cannot possibly be well informed. The dependent variable was the aggregate vote share for the Republicans. The vote was represented as a sum of three parts: the party’s “normal” vote, incumbency, and the error term. The incumbency part combined the effect of raw incumbency advantage and the effect of past economic performance.

A central modeling issue was how to measure performance, which Kramer saw as the difference between an observed short-term growth rate and an expected rate. He pointed out that the expected rate was unobservable; thus, the estimated effects of incumbency and performance will tend to be underestimated, the underestimate being an increasing function of expectations. He chose to focus on the House vote in the aggregate as the best measure of the overall popularity of the incumbent’s party “team,” seeing it as less subject to individual candidate effects than the presidential vote.

Another important methodological innovation concerned coattails effects. Despite his focus on House elections, Kramer added an equation for presidential elections, and specified a variance-components structure for the error terms (variance-components models were an active interest of Nerlove’s at the time), with an additional parameter that captured a “coattails” effect. The two-equation model contained two very strong assumptions—that the effects of the independent variables would be the same in presidential and congressional elections, and that the noncoattails part of the error term was the same for presidential elections, congressional on-year elections, and congressional midterm elections—which would be relaxed by later researchers, but which did permit maximum likelihood estimation of the coattail parameter. Kramer’s basic finding was that what really mattered, accounting for about half the variance in the vote itself, was the change in real personal income during the election year. Inflation, unemployment, and coattails did not matter. The addition of a trend term for the normal vote led the model to account for 62% of the variance in the congressional vote. There was no net raw incumbency advantage.

An important implicit finding, not explicitly discussed in the article, was that only economic fluctuations over the year preceding the election had any effect; voters paid no attention to earlier years. Kramer had experimented extensively with different lags—both longer and shorter—and lag structures, using both annual and quarterly data, in an unpublished 1967 manuscript, but because none of these specifications yielded better results than the annual data with a 1-year lag, he simply reported those results, intending to publish the lag results in a separate article. But he was obliged to abandon the project after discovering that his erstwhile Cowles colleague Ray Fair (who, according to Kramer, had been shown the unpublished 1967 manuscript), had essentially replicated his findings in a 1975 paper, based on a slightly different sample period and data set, which had been accepted for publication in an economics journal (Fair 1978).

As it turned out, a data error in the annual series used in the *Review* article was discovered by George Stigler (1973). On learning of the error, Kramer reran his models on the corrected data, and was relieved to find that most of his original findings held up, except that inflation now had a modest independent effect. That bobble aside, and aside from its substantive explanatory findings, the model also seemed to have some modest forecasting ability, according to Atesoglu and Congelton (1982), who used it to forecast postsample congressional elections from 1966 to 1980, and found that its predictive power “compare[d] favorably with the post-sample forecasts of various [macro]economic models,” when judged by the usual criteria. The use of such models for forecasting elections, especially presidential elections, has since become something of a mini-industry, though in recent elections the various macro forecasting models, particularly the ones that amount to single-equation variants of Kramer’s, seem to have lost their predictive ability (Bartels and Zaller 2001).

Kramer’s interest was primarily theoretical, in trying to model the dynamics of interactions between the economy and the party system, and he mentioned the possibility of forecasting only as an aside. But even for explanatory modeling, the decision to focus on the congressional vote as the best measure of the overall popularity of the parties, although defensible for the sample period he used, is no longer tenable, as individual congressional races have become increasingly decoupled from national and presidential trends because of incumbent perquisites such as the franking privilege and earmarks, independent and differential incumbent-challenger access to campaign finance, gerrymandering to protect incumbents, and the like. One consequence of this decoupling is that divided government has become much more common. Kramer measured incumbency by the presidency, but recognized that divided government posed a problem. His sample period covered the 68 years ending in 1964, during which only 8 of the 35 elections occurred under divided government; the next 42 years would produce 22 years of divided government. Another possible implication of this decoupling is that there may be no “normal vote”; the lagged House vote turns out to be significant (Alesina, Londregan, and Rosenthal 1995).

Kramer believes that other important structural changes since 1964 as well, in both the U.S. economy and the American political process, invalidate his model. For most of the twentieth century, the pains and gains of economic fluctuations were widely distributed: “a rising tide floats all boats,” and conversely. But this has ceased to hold in recent years, as the incomes of lower class and middle-class earners/voters have remained virtually stagnant, even during periods of respectable aggregate economic growth. Equally important is the recent emergence of “base politics” as the norm, in which the parties contest elections not by courting moderates and independents, or the representative “average” (or “median”) voter, but by concentrating on mobilizing their respective electoral bases. He thinks that a more realistic approach to modeling the dynamics of electoral-economic interactions in the present era would probably require a more disaggregated approach, perhaps as sketched out in section 3 of Kramer and Lepper 1972.

Kramer’s results were questioned by survey researchers who had been unable to replicate their findings with cross-sectional survey data. Kramer (1983) addressed the question of why this was so, and whether it was possible to reconcile the aggregate- and individual-level findings. He made the important point that the cross-sectional variation that the survey researchers were working with was related to the underlying structural parameters of interest in a quite different and more tenuous way than that of the time-series variations that the macro modelers were using; and that under plausible

data and parameter assumptions, the cross-sectional survey estimates would be downwardly biased, and could even have the wrong sign. Kramer (1971, 1983) thus made two seminal contributions to why we say, "It's the economy, stupid."

## REFERENCES

- Alesina, Alberto, John B. Londregan, and Howard Rosenthal. 1993. "A Model of the Political Economy of the United States." *American Political Science Review*, 87 (March): 12–33.
- Atesoglu, H. Sonmetz, and Roger Congelton. 1982. "Economic Conditions and National Elections, Post-Sample Forecasts of the Kramer Equations." *American Political Science Review* 76 (December): 873–74.
- Bartels, Larry M., and John Zaller. 2001. "Presidential Vote Models: A Recount" *PS: Political Science and Politics* 34 (March): 8–20.
- Fair, Ray C. 1978. "The Effects of Economic Events on Vote for President." *Review of Economics and Statistics* 60 (April): 159–73.
- Kramer, Gerald H. 1983. "The Ecological Fallacy Revisited: Aggregate- versus Individual-level Findings on Economics and Elections, and Sociotropic Voting." *American Political Science Review* 77 (March): 92–111.
- Kramer, Gerald H., and Susan J. Lepper. 1972. "Congressional Elections." In *The Dimensions of Quantitative Research in History*, ed. William O. Aydelotte, Allan G. Bogue, and Robert William Fogel. Princeton: Princeton University Press, 256–84.
- Stigler, George G. 1973. "General Economic Conditions and National Elections." *American Economic Review* 63 (May): 160–67.

**5. Warren E. Miller and Donald E. Stokes. 1963. "Constituency Influence in Congress." *American Political Science Review* 57 (March): 45–56. Cited 457 times.**

—Robert S. Erikson, *Columbia University*

In just 13 pages, Warren Miller and Donald Stokes's "Constituency Influence in Congress" established the agenda for the next half-century of research on congressional representation. Along with its sister paper, "Party Government and the Salience of Congress" (Stokes and Miller 1962), "Constituency Influence" famously documents the general impoverishment of voters' knowledge of Congress and its members. Of at least equal importance is its analysis of congressional behavior as a response to oft-ambiguous constituency views, incorporating rich interview data of the sort not seen since. It is no wonder that Miller and Stokes' paper is among the most heavily cited—and influential—of all papers that have graced the pages of the *Review*.

At the heart of "Constituency Influence" is the well-known "diamond model" of political representation. Consider a

diamond with constituency opinion at the left node and roll-call voting at the right node. The path from constituency to roll call is moderated by the representative's personal position (top node) and perception of constituency opinion (bottom node). One pathway from constituency opinion to roll call voting is via electoral selection: constituency opinion → representative's opinion → roll-call behavior. The second pathway is via the representative's anticipation of electoral sanctions: constituency opinion → representative's perception → roll-call behavior. Miller and Stokes added the nuance of reciprocal causation, whereby representatives' personal views color their perceptions of constituency opinion while the perceptions of constituency opinion modify their personal views.

For over 100 U.S. House districts across three issue domains, Miller and Stokes estimated constituency opinion via district-level aggregates from the 1958 Survey Research Center (later NES) postelection surveys and applied standard techniques to measure the representatives' roll-call positions. The truly innovative aspect was the measurement of the personal opinions of the representatives themselves as well as their perceptions of constituency opinion via the Congress members' responses in personal interviews. No study before or since has been so bold as to connect members' personal views to those of their constituents and their roll-call voting in this way.

"Constituency Influence" must be judged in the context of its time. Like virtually everything set to print by the four authors of *The American Voter* (Campbell et al. 1960), "Constituency Influence" displays a gracefulness to its prose that appears remarkable when read today. It introduced the methodological repertoire of "causal modeling" and "path analysis" to the pages of the *Review*, albeit with much of the detail embedded in the footnotes. Like any ambitious paper, "Constituency Influence in Congress" has some limitations. Typical for empirical papers of its day, the statistical presentation is sketchy, preventing easy replication. The various measures are on different metrics, so that one cannot say, for instance, whether representatives are to the left or right of their districts or whether their public postures are to the left or right of their private beliefs. (This, of course, is a limitation that frustrates contemporary work on representation as well.) The major limitation certainly was the estimation of constituency opinion with sample sizes that current standards hold to be too small for reliable analysis.

"Constituency Influence" concluded with a seemingly out-of-equilibrium result: "Congressmen feel that their individual legislative actions may have considerable impact on the electorate, yet some simple facts about the Representative's salience to his constituents imply that this could hardly be true" (p. 54). While noting the obvious possibility that members of Congress have an inflated sense of their visibility to constituents and perhaps could get away with more than they think, Miller and Stokes to their credit also saw the possibility that members of Congress know what they are doing. Miller and Stokes set the questions that remain central today regarding how much and by what mechanisms a not very political public achieves representation by its elected leaders.

## REFERENCES

- Campbell, Angus, Philip E. Converse, Warren E. Miller, Donald E. Stokes. 1960. *The American Voter*. New York: Wiley.
- Donald E. Stokes, and Warren E. Miller. 1962. "Party Government and the Salience of Congress." *Public Opinion Quarterly* 36 (Winter): 531–46.

---

Warren Miller joined the faculty of the University of Michigan in 1956 and served there as Professor of Political Science and director of Center for Political Studies from 1956 to 1981, until joining the faculty of Arizona State University in 1982. He created and directed the Inter-university Consortium for Political and Social Research, helped found and became the first president of the Social Science History Association, and served as President of the American Political Science Association in 1979–1980.

As Professor of Political Science at the University of Michigan from 1958 to 1974, Donald Stokes was program director for the Institute for Social Research and chairman of the Department of Political Science until he became dean of the graduate school from 1971 to 1974. Stokes spent the rest of his career at the Woodrow Wilson School for Public and International Affairs at Princeton University until his death in 1997.

Robert S. Erikson is Professor of Political Science at Columbia University. A former editor of the *American Journal of Political Science*, his major interests include electoral politics, public opinion, and policy representation in the United States.

**6. James G. March and Johan P. Olsen. 1984. "The New Institutionalism: Organizational Factors in Political Life." *American Political Science Review* 78 (September): 734–49. Cited 456 times.**

—James G. March, *Stanford University*

—Johan P. Olsen, *University of Oslo*

The request to interpret the meaning of the historical record of citations for our article on the "New Institutionalism" is in the tradition of asking successful entrepreneurs to explain their successes, and is probably equally misguided. We interpret the charge as an invitation not to describe the real mysteries of fashion in citations but rather to explore why this article might have been cited if citation counts reflected valid judgments of scholarly significance.

The context for the article is a long history of intellectual struggle between partisans of two logics for taking, describing, or assessing human action. The first logic is one of consequences. It is a logic that sees human behavior as driven by anticipation of its consequences and evaluation of those consequences by some kind of utility function that reflects the desires of the actor. In discussions of political systems, scholars attracted to such a logic talk of the pursuit of interests and rational choice. They emphasize incentives and the complications of understanding rationality in an ecology of rational actors.

The second logic is one of appropriateness. It is a logic that sees human behavior as driven by a commitment to an identity and its rules. In discussions of political systems, scholars attracted to such a logic talk of institutions, routines, and identities. They emphasize rules as the residues of historical experience and the complications of rule-following in a world in which obligations are subject to interpretation and in which individuals function within numerous simultaneous identities.

The struggle between these two visions is complicated by their generality and their inclinations to mutual subsumption. It is not hard to imagine either that rational calculation is simply a special case of appropriateness or that rule-following is simply a special case of rational calculation. Nevertheless, there is little doubt that ideas of consequentiality have achieved considerable contemporary standing, not only in economics but also in the other social and behavioral sciences, including political science.

In this article, we noted that the preeminence of logics of consequences was perhaps transitory, that there were signs in all the social sciences of interest in logics of appropriateness and institutions built around identities and rules. The interest both recalled an earlier set of traditions and presaged a future in which routines and rules might become more central to political understanding. The citation record of the article can be treated as possible confirmation of that interest and some indication that in the swings of fashion within political science, the garbs of rules and institutional thinking are appearing more on the streets of political science.

Swings of fashion in ideas are, however, substantially less interesting than their scholarly elaboration, an elaboration

that often anticipates, rather than follows, their popularity (see March and Olsen 2006a, 2006b). Both in North America and in the rest of the world, serious efforts have been made to strengthen institutional analysis in two major ways. The first is a series of careful ethnographic and historical studies of political institutions and the ways in which they develop, interpret, and execute rules and identities. The second is the exploration of a set of theoretical ideas that picture rules as the carriers of experience and that understand changes in rules as (1) incremental changes in existing rules on the basis of learning from experience; (2) the diffusion of rules among institutions; and (3) the endogenous generation of new, distinctively novel rules.

The primary traditional and current strengths of political science are probably more consistent with the first set of developments than the second. Political scientists know much about institutions and the complex ecologies of rules that infuse political processes. They are distinguished by their ability to penetrate institutions in order to study them and by their feeling for the nuances of political life. They know somewhat less about the models of other fields of scholarship, such as evolutionary biology, epidemiology, linguistics, and chemistry, which inform an understanding of the creation, reproduction, and modification of rules.

The result is a reasonable division of labor; but like all systems involving a division of labor, it requires some links among the parts. Their mixed experience with economic models notwithstanding, students of the institutions of political life need to tie their elegant interpretations of institutional reality to the crude approximations available in theoretical ideas in order to further a joint product that contributes to political knowledge. That romantic idea stimulated the paper we wrote; it still stimulates us.

## REFERENCES

- March, James G., and Johan P. Olsen. 2006a. "Elaborating the 'New Institutionalism.'" In *The Oxford Handbook of Political Institutions*, ed. R.A.W. Rhodes, Sarah A. Binder, and Bert A. Rockman. Oxford: Oxford University Press. Forthcoming.
- March, James G., and Johan P. Olsen. 2006b. "The Logic of Appropriateness." In *The Oxford Handbook of Public Policy*, ed. Martin Rein, Michael Moran, and Robert E. Goodin. Oxford: Oxford University Press. Forthcoming.

**7. Seymour Martin Lipset. 1959. "Some Social Requisites of Democracy: Economic Development and Political Legitimacy." *American Political Science Review* 53 (March): 69–105. Cited 455 times.**

—Larry Diamond, *Hoover Institution, Stanford University*

"Some Social Requisites of Democracy" is, by official count, the seventh most cited article the *Review* has published. If we include citations to it as it was reproduced in *Political Man* (published the following year), we would surely find it to be one of the most influential political science essays of the past half-century. I suggest five reasons why.

James G. March is the Jack Steele Parker Professor of International Management (Emeritus) and Professor of Education, Political Science, and Sociology (Emeritus) at Stanford University.

Johan P. Olsen has been the Research Director of ARENA (Center for European Studies, University of Oslo) since 1994. He received the APSA's John Gaus award in 2003 and its Aaron Wildavsky award in 2004.

The authors are grateful for the support of the Sloan Foundation, the Spencer Foundation, ARENA- Center for European Studies, University of Oslo, and the Norwegian Research Council.

Seymour Martin Lipset was the Caroline S.G. Munro Professor of Political Science and Sociology at Stanford University and the George D. Markham Professor of Government and Sociology at Harvard University before becoming the Hazel Professor of Public Policy at George Mason University.

Larry Diamond is a senior fellow at the Hoover Institution, Stanford University, and founding coeditor of the *Journal of Democracy*. He is also codirector of the International Forum for Democratic Studies of the National Endowment for Democracy.

First, Lipset's effort to understand the social bases of stable democracy encompassed one of the most powerful and enduring themes in comparative politics, and built on the work of many of the great political and social theorists, from Aristotle to Tocqueville and Weber. Writing in 1958, with democracy still largely a "Western" phenomenon and with most of Africa still under European colonial rule, Lipset did not anticipate that well over half of the independent states of the world would become democracies. Yet clearly, the expansive scope of global democratic change and aspirations has stimulated scholarly interest in its facilitating conditions. Today, much of comparative politics revolves around this issue.

Second, although his key thesis was remarkably simple and concise—"the more well-to-do a nation, the greater the chances it will sustain democracy" (p. 75)—his essay was theoretically rich in identifying a nexus of causal factors leading from level of economic development to prospects for stable democracy. Only two of his modernization variables (national income and education) have stood well the tests of time and more complex methods as drivers of democratization. But the key intervening variables that he adduced—changes in political culture, class structure, civil society, and state-society relations—have endured exceptionally well as explanations, spawning in themselves vast literatures. To this day, these remain the key social determinants of the democratic prospect (Diamond 1992; Lipset 1994).

Third, Lipset's thesis was embedded in the larger body of modernization theory, which would mobilize considerable evidence demonstrating that rising levels of income and education have diffuse impacts on attitudes and values, and through them on political systems. The effects are far from neatly linear, but higher levels of economic development do tend to generate the trust, tolerance, autonomous participation, and valuing of freedom that facilitate democracy (Inglehart and Welzel 2005; Inkeles and Smith 1974).

Fourth, Lipset's hypothesis about economic development and democracy—advanced at the time with crude and merely suggestive statistical measures—has since been supported by a vast array of statistical studies. Although there is debate about whether economic development actually causes democracy, clearly economic development *sustains* democracy (Przeworski et al. 2000), and virtually every multivariate analysis of the determinants of democracy identifies economic development as a powerful factor. Moreover, considerable case study evidence supports Lipset's (1994) (frequently overlooked) assertion that "a 'premature' democracy that survives will do so by (among other things) facilitating the growth" (p. 72) of the critical intervening variables, such as broader literacy, a vigorous civil society, limited inequality, and a democratic culture.

Finally, Lipset was also right in asserting that the stability of democracy (uniquely among political systems) depends on legitimacy, and that this belief heavily depends on effective performance, especially early in a regime. Here again Lipset gave us a rich set of propositions that have shaped political science theorizing about the need for democracies to moderate conflict, the value in doing so of cross-cutting cleavages, and the importance for new democracies to avoid threatening "the status of major conservative groups and symbols" (p. 87), a theme that resonates powerfully in the transitions literature (e.g., O'Donnell and Schmitter 1986).

Lipset was not the first to make these various arguments, but he was the first to state them clearly and systematically to a new generation of empirical social scientists, at a time when dozens of new nations were gaining independence and when transitions to and from democracy would become one

of the dominant aspects of national development demanding explanation.

At the high end of the spectrum of development, Lipset's theory has held up remarkably: all but one (Singapore) of the 25 most developed states are democracies, and democracy has never broken down once established in a relatively rich country (Przeworski et al. 2000). However, contrary to Lipset's expectation, about two in every five poor countries (with low "human development" on the UNDP scale) are democracies today. It remains to be seen whether these countries can consolidate democracy, but to the extent they do so it will be by accumulating legitimacy through effective performance and by building up the supporting social and cultural requisites that Lipset identified in his seminal 1959 essay.

## REFERENCES

- Diamond, Larry. 1992. "Economic Development and Democracy Reconsidered." In *Reexamining Democracy: Essays in Honor of Seymour Martin Lipset*, ed. Gary Marks and Larry Diamond. Newbury, Park, CA: Sage, 93–139.
- Inglehart, Ronald, and Christian Welzel. 2005. *Modernization, Cultural Change, and Democracy: The Human Development Sequence*. Cambridge: Cambridge University Press.
- Inkeles, Alex, and David H. Smith. 1974. *Becoming Modern: Individual Change in Six Developing Countries*. Cambridge: Harvard University Press.
- Lipset, Seymour Martin. 1994. "The Social Requisites of Democracy Revisited" (1993 Presidential Address, American Sociological Association). *American Sociological Review* 59 (February):1–22.
- O'Donnell, Guillermo, and Philippe Schmitter. 1986. *Transitions from Authoritarian Rule: Tentative Conclusions about Uncertain Democracies*. Baltimore: Johns Hopkins University Press.
- Przeworski, Adam, Michael E. Alvarez, José Antonio Cheibub, and Fernando Limongi. 2000. *Democracy and Development: Political Institutions and Well-Being in the World, 1950–1990*. Cambridge: Cambridge University Press.

### 8. Nathaniel Beck and Jonathan N. Katz. 1995. "What to Do (and Not to Do) with Time-Series Cross-Section Data." *American Political Science Review* 89 (September): 634–47. Cited 398 times.

—Nathaniel Beck, *New York University*

—Jonathan N. Katz, *California Institute of Technology*

Much as we would like to believe that the high citation count for this article is due to the brilliance and clarity of our argument, it is more likely that the count is due to our being in the right place (that is, the right part of the discipline) at the right time. In the 1960s and 1970s, serious quantitative analysis was used primarily in the study of American politics. But since the 1980s it has spread to the study of both comparative politics and international relations. In comparative politics we see in the 20 most cited *Review* articles Hibbs's (1977) and Cameron's (1978) quantitative analyses of the political economy of advanced industrial societies; in international relations we see Maoz and Russett's (1993) analysis of the democratic peace; and these studies have been followed by myriad others. Our article contributed to the methodology for analyzing what has become the principal type of data

Nathaniel Beck is Professor of Political Science at New York University. He previously served on the faculty of the University of California, San Diego, and as editor of *Political Analysis*.

Jonathan N. Katz is Professor of Political Science at the California Institute of Technology. His research interests focus on American politics, political methodology, and formal political theory.

used in the study of comparative politics; a related article (Beck, Katz, and Tucker 1998), which has also had a good citation history, dealt with analyzing this type of data with a binary dependent variable, data heavily used in conflict studies similar to that of Maoz and Russett's. Thus the citations to our methodological discussions reflect the huge amount of work now being done in the quantitative analysis of both comparative politics and international relations.

Our methodology deals with the analysis of time-series–cross-section (TSCS) data. Quantitative comparative politics and international relations have leaned heavily on this type of data since the early 1990s. The early quantitative research in comparative politics, such as Cameron's (1978), used simple cross-sectional regression on typically fewer than 20 countries. With so few observations, it was impossible to tease out complicated relationships. The late 1980s saw arguments over whether results were stable if a single country was excluded. The alternative was to use more standard time-series analysis, but to examine each nation's time-series separately, as was done by Hibbs (1977). TSCS analysis pools these time series, allowing for rigorous quantitative analysis of both the temporal and the spatial dimensions of the data. TSCS data are complicated, and users of such data have been very interested in the appropriate methods for analyzing these complicated data.

Our article is also heavily cited because it deals with important issues relevant to political science research. Twenty years ago the small coterie of political methodologists taught out of standard econometrics texts. But the issues relevant to political scientists are not necessarily those relevant to economists. In our case, econometricians have developed sophisticated methods for studying panel data, which appear to look like TSCS data. Given econometricians' interests, they focused on data sets in which the cross-sectional units consisted of a huge number of survey respondents observed over a very small number of survey waves. Methods suitable for the analysis of panel data are not appropriate for TSCS data, and vice-versa. One reason our work is cited by political scientists is that as political scientists we worked on methodological issues relevant to the study of political questions.

Our article also reflects a growing interest in the finite sample properties of the estimators we all use. Econometricians tend to live and die by asymptotic properties; but we all have finite samples and sometimes what is good asymptotically is not good for finite samples. We started with a commonly used estimator that was fine asymptotically but displayed rather poor finite sample performance (at least for typical TSCS data). We showed that in typical data sets least squares, combined with a correction for standard errors, could be a huge improvement over the "asymptotically superior" estimator.

In thinking about our article, we cannot help but remember that we submitted it to the *Review* after it was rejected by the only other political science journal that at the time published serious methodological articles. It is good to remember that a rejection letter is not the same thing as a death sentence.

What do we wish we had done differently? The study of TSCS data has become vibrant with many ongoing debates. One point that almost everyone understands, or should, is that there is no magical method (or statistical package command) for analyzing TSCS data. The most critical issues are about specification, not about the choice (and surely not the mechanical choice) of an estimation method. Because of the context in which we were writing, we focused on estimation methods, though we always tried to move researchers away from mechanistic thinking about statistical analysis.

The ongoing debates have focused our current work on more critical issues of specification. But because we now live in a world where methodological debates are commonplace, we do not worry that we missed many important issues in our article.

## REFERENCES

- Beck, Nathaniel, Jonathan N. Katz, and Richard Tucker. 1998. "Taking Time Seriously: Time-Series–Cross-Section Analysis with a Binary Dependent Variable." *American Journal of Political Science* 42 (October): 1260–88.
- Cameron, David. 1978. "The Expansion of the Public Economy: A Comparative Analysis." *American Political Science Review* 72 (December): 1243–61.
- Hibbs, Douglas. 1977. "Political Parties and Macroeconomic Policy." *American Political Science Review* 71 (December): 1467–87.
- Maoz, Zeev, and Bruce M. Russett. 1993. "Normative and Structural Causes of Democratic Peace, 1946–1986." *American Political Science Review* 87 (September): 639–56.

**9. David R. Cameron. 1978. "The Expansion of the Public Economy: A Comparative Analysis." *American Political Science Review* 72 (December): 1243–61. Cited 387 times.**

—David R. Cameron, *Yale University*

The article started as a long paper on distributional inequality in the advanced capitalist societies that I presented at the 1976 annual meeting of the APSA. Using OECD data on the post-tax distribution of income in 12 countries, I found that the extent of inequality varied inversely with the extent to which Social Democratic and other leftist parties had governed in recent years. Control of government by left-of-center parties was closely associated with the extent of and growth in the extractive capacity of the state, defined in terms of the ratio of all public revenues to GDP, and the extent of and growth in extractive capacity were closely associated with the degree of distributional equality. But there were perplexing anomalies. For example, the Netherlands, where the Left had seldom governed, experienced the largest increase in the ratio of revenues to GDP since 1960 and was the most egalitarian in terms of the post-tax distribution of income. Belgium and Ireland also experienced unusually large increases in the ratio of revenues to GDP despite infrequent control of government by left-of-center parties.

Mindful of those countries' high dependence on trade and drawing upon Robert Dahl and Edward Tufte's (1973) discussion of the relationship between small size and trade dependence, the work of Odd Aukrust (1977) and Assar Lindbeck (1975) on "open" economies, that of Gerhard Lehbruch (1977) on incomes policies, and that of Robert Gilpin (1975) on neomercantilism, I incorporated a measure of openness—the ratio of exports and imports to GDP—in

---

David Cameron is a Professor of Political Science at Yale University and Director of the Yale Program on European Union Studies. His current research involves the impact of enlargement on the EU and its new member states, the effort to draft a constitution and reform the institutions of the EU, and the expansion of policymaking authority in the EU.

He wishes to express his gratitude to Charles O. Jones, the managing editor of the *APSR* at the time, for accepting the article for publication, to the anonymous reviewers and those he thanked in the article for their helpful comments and suggestions, and to all those who found it sufficiently illuminating or provocative to cite it in their own work.

the analysis. The measure was very closely related to the extent of the increase between 1960 and 1975 in 18 countries in the ratios of public revenues to GDP and social spending to GDP and, indeed, trumped all other explanatory variables, including the partisanship of government.

It is no doubt the article's conclusion that trade openness was the most important source of the expansion of the public economy between 1960 and 1975 that has caused it to appear on this list. That conclusion suggested that any account of change over time in taxes or spending would have to take the effects of openness into consideration.

In recent decades the strong positive cross-national correlation that I reported between openness and the increase in the relative size of the public economy has become slightly negative. The largest increases in the relative size of the public economy after the mid-1970s did not occur in the countries that were most dependent on trade but, rather, in countries that were, if anything, relatively closed. In some of the countries that were most dependent on trade—most notably, Belgium, the Netherlands, and Ireland, the earlier anomalies—the relative size of the public economy increased only slightly or decreased (see Cameron and Kim 2006).

Notwithstanding the change from a strong positive to a moderately negative cross-national relationship, the article's suggestion that a high degree of openness may, depending upon the partisanship of the government and the structure of labor market institutions, cause governments to offer compensation through social policy in exchange for wage moderation remains, I think, an intriguing hypothesis in an era marked by increasing global interdependence and economic "openness."

A large literature has accumulated that uses pooled cross-sectional time-series analysis to determine, among other things, whether openness affects the magnitude and direction of annual changes in social spending. The results vary but generally suggest that neither the extent of openness nor the magnitude of increase in openness is associated with increases in social spending. However, using that mode of analysis, we have found that, although increases in the extent of openness had a contractionary effect on total revenues and expenditures in the period after 1970, large trade deficits and deteriorations in the balance of trade had, as the compensation hypothesis would suggest, strong expansionary effects on expenditures and budget deficits (see Cameron and Kim 2006).

## REFERENCES

- Aukrust, Odd. 1977. "Inflation in the Open Economy." In *Worldwide Inflation: Theory and Recent Experience*, ed. Lawrence B. Krause and Walter S. Salant. Washington: Brookings.
- Cameron, David R., and Soo Yeon Kim. 2006. "Trade, Political Institutions and the Size of Government." In *Globalization and Self-Determination: Is the Nation-State under Siege?*, ed. David R. Cameron, Gustav Ranis, and Annalisa Zinn. London: Routledge.
- Dahl, Robert A., and Edward R. Tufte. 1973. *Size and Democracy*. Stanford: Stanford University Press.
- Gilpin, Robert. 1975. "Three Models of the Future." In *World Politics and International Finance*, ed. C. Fred Bergsten and Lawrence B. Krause. Washington: Brookings.
- Lehmbruch, Gerhard. 1977. "Liberal Corporatism and Party Government." *Comparative Political Studies* 10 (April): 91–126.
- Lindbeck, Assar. 1975. "Business Cycles, Politics and International Economic Dependence." *Skandinaviska Enskilda Bank Quarterly Review* 2: 53–68.

**10. Karl W. Deutsch. 1961. "Social Mobilization and Political Development." *American Political Science Review* 55 (September): 634–47. Cited 380 times.**

—Bruce Russett, *Yale University*

I believe that the high rank of this article both indicates its importance for a transnational generation of scholars of comparative politics and international relations and reflects the enormous influence of Karl Deutsch's other writings from that time.

Most of the themes of Deutsch's career can be found either here or in his other contemporary article in the *Review* (Deutsch 1960). The core of that career was his personal experience of destructive nationalism in Central Europe, from which he fled with his family to the United States in 1939. His theory of nationalism (Deutsch 1953) focused on the social mobilization of previously repressed ethnic and social groups and the need for their assimilation into the national culture. This in turn implied government capabilities, institutions able to respond to the demands of the newly mobilized. For Deutsch, a social democrat, big government could be oppressive, but competent and responsive government had the capacity to temper political conflict by satisfying people's needs broadly, across society. It was in maintaining a favorable ratio of capability to demand that peace and representative government might be maintained. This perspective carried over into his work in international relations, on the sources of peaceful amalgamation and integration among countries.<sup>1</sup>

Deutsch grew up with the humanistic education of a European intellectual. He wedded it with a scientific capacity derived from his study of mathematics and optics, and then with his determination to measure as many social and political variables as possible. His 1960 article was largely a conceptual inventory and shopping list; this one from 1961 delivered real goods, and it is not surprising that it garnered nearly eight times as many citations. In it he proposed a string of indices of particular aspects of social mobilization, illustrated them with what at the time was a very impressive array of data, and posited an underlying conceptual dimension with which they would be closely correlated. He thrived on data to stimulate, probe, and test his theories. Data were meant "as aids to political judgment, not as substitutes for it" (Deutsch 1960, 40). These two articles gave him the leverage to acquire one of the very first National Science Foundation grants to an international relations scholar, establishing the Yale Political Data Program to gather, publish, and energetically analyze it. In doing so he provoked a generation of scholars to evaluate, use, and vastly supplement anything he alone could do.<sup>2</sup> He wanted his example of publishing all the

---

Karl Deutsch was Stanfield Professor of International Peace at Harvard University. He joined the Harvard faculty after stints at MIT and Yale. He served as President of the American Political Science Association, of the International Political Science Association, and of the Society for General Systems Research.

Bruce Russett is Dean Acheson Professor of International Relations and Director of United Nations Studies at Yale, and Editor of the *Journal of Conflict Resolution*. He has been President of the International Studies Association and the Peace Science Society (International). With Deutsch and others, he compiled the *World Handbook of Political and Social Indicators*.

<sup>1</sup> Deutsch (1960) expresses his interest in the balance of domestic to international transactions, following Deutsch et al. 1957.

<sup>2</sup> The quintessential Deutschian product was Russett, Deutsch, Lasswell, and Alker (1964), but the project allowed us all to go in very different directions in our work then and after. The *Handbook* went through two more editions and finally was replaced by Internet sources.

data for anyone, anywhere, to use, to be emulated, and in time it was. (He would surely revel in what the internet can do.)

Unlike some of the top 20 articles from his era, the citation count for this article fell off sharply after about 15 years, and then after another five years to an average of only about five citations per year. Because it was fundamentally about a specific kind of data (usually aggregate) and their manipulation, it is not surprising that most people stopped citing it. New and better data became available and widely used. Subsequent statistical techniques developed much more systematic procedures for multivariate analysis and for identifying the kind of underlying dimensions he hoped to find. This article talked about “significant” and “critical” thresholds of development; subsequently the profession learned to think rigorously and routinely about nonlinear relationships. Much of the theory was stated loosely, but key elements of it have been formalized by others. Our contemporary studies of civil conflict measure greed and grievance, deprivation and state capacity, with no apparent need to cite Deutsch—but the concepts and some of the indicators nevertheless emerge from his work.

Just because Deutsch’s work made such a deep early impact, its very success contributed to its later obsolescence. This now somewhat quaint article serves still as a prominent artifact of our intellectual history.

## REFERENCES

- Deutsch, Karl W. 1953. *Nationalism and Social Communication*. Cambridge: M.I.T. Press and Wiley.
- Deutsch, Karl W., Sydney Burrell, Robert Kann, Maurice Lee, Martin Lichterman, Raymond Lindgren, Francis Loewenheim, and Richard Van Wagenen. 1957. *Political Community and the North Atlantic Area*. Princeton: Princeton University Press.
- Deutsch, Karl W. 1960. “Toward an Inventory of Basic Trends and Patterns in Comparative and International Politics,” *American Political Science Review* 56 (March): 34–57.
- Russett, Bruce, Karl W. Deutsch, Harold Lasswell, and Hayward Alker. 1964. *World Handbook of Political and Social Indicators*. New Haven: Yale University Press.

### 11. William H. Riker and Peter C. Ordeshook. 1968. “A Theory of the Calculus of Voting.” *American Political Science Review* 62 (March): 25–42. Cited 377 times.

—Peter C. Ordeshook, *California Institute of Technology*

The initial motive for writing “A Theory of the Calculus of Voting” was not the argument that subsequently became the focus of academic discussion, the necessity for positing a D term (“citizen duty”) to render the act of voting rational. Instead, our purpose was to show that people reacted “rationally” (i.e., logically) to an election’s competitiveness. Admittedly the data we brought to bear here were weak, but with the addition of D the question arose as to whether

---

William Riker served as chair of the Department of Political Science at the University of Rochester from 1962 to 1977. President of the American Political Science Association in 1983, he remained at the University of Rochester as Professor Emeritus of Political Science until his death in 1993.

Peter Ordeshook is Professor of Political Science at the California Institute of Technology. His research interests include election processes, international relations, and applications of game theory to politics.

we made voting rational only by rendering the concept of rationality a tautology: rational people voted because, with D, we assumed they were rational. However, one response with which I am sure Riker would agree is to note an asymmetry in our thinking about social processes. Few people claim to eat for a reason other than satisfying their own (private) needs as opposed facilitating the (public) objective of diminishing world hunger, and positing a growling tummy as a reason for entering a restaurant is hardly taken as an instance of rationality turned tautology. Yet the argument that the benefits of voting are dominated by a private D term as opposed to some public PB calculation occasions precisely that accusation.

One difficulty with this accusation is that we still do not know what rationality means as a basic concept. The core formal structure of “rational choice theory” is game theory, which is itself an attempt to define “rational.” The notion of a Nash equilibrium in noncooperative games, for example, and the hypothesis that people choose corresponding strategies is an idea about what rationality means in specific interactive situations. In turn, refinements of Nash equilibria that seek to dispense with their common overabundance in models are attempts to refine our understanding of “rational” and “rationality.” In part, then, the survival of this article in the literature and the attention paid to the weight that ought to be given to a PB versus a D term can be attributed to the fact that the search for the meaning of rational—both theoretical and substantive—is a core element of our research agenda.

But that is only part of the explanation. Few if any economists are concerned with why some people prefer carrots to potatoes. Political scientists, though, must focus on the substantive content of choice. Dissatisfaction with the mere addition of D derives, then, from the legitimate view that its determinants require specification before it becomes a meaningful construct. The nature of our discipline dictates that we address in detail the parameters of choice, such as the impact of competitiveness on turnout (P); the implications of negative campaigning on individual beliefs about candidates (B); the incentive to vote strategically as a function of election rules (PB); the impact of ballot forms (C); why turnout varies between presidential and local elections; and why voting and nonvoting are largely habitual acts (D). Thus, the signature expression of “A Theory of the Calculus of Voting,”  $R = PB - C + D$ , frames much of our research into elections and voting and casts that research as refinements of the application of the notions of rationality and rational choice.

Of course, one might argue that an essay’s decades-long survival marks the absence of theoretical advancement. That may or may not be true, but it speaks to another matter. Of the many formal essays attributed to the Rochester (read: Riker) school, it is perhaps surprising that this article has the longest legs. It suggests that a discipline’s seminal contributions are not those replete with notation, lemmas, and theorems; rather they are ones that address basic matters in simple (nonmathematical) ways. The primary challenger to this supposition is McKelvey’s (1979) proof of global intransitivities in multidimensional issue spaces, because the “mathematical sophistication” of  $R = PB - C + D$  pales in comparison to that proof’s content. But it is the general implications of McKelvey’s result that capture our interest and not its mathematical elegance. Indeed, it was Riker’s (1982) subsequent volume *Liberalism Against Populism* that revealed the result’s full substantive meaning. Perhaps there is a lesson here, then, that goes beyond the subject with which this article deals and extends to how one goes about doing research of lasting value—to activities that do more than add a

few lines to a vitae or offer a model of a nonexistent universe. Political science is (or ought to be) an applied discipline, and generally the critical contributions are those that encourage us to fit otherwise disparate empirical patterns into a coherent paradigm. That, I suspect, is the explanation for the citations that this article continues to accumulate.

## REFERENCES

- McKelvey, Richard D. 1979. "General Conditions for Global Intransitivities in Formal Voting Models," *Econometrica* 47 (September): 1085–1112.
- Riker, William H. 1982. *Liberalism Against Populism: A Confrontation Between the Theory of Democracy and the Theory of Social Choice*. San Francisco: Freeman.

**12. L. S. Shapley and Martin Shubik. "A Method for Evaluating the Distribution of Power in a Committee System,"** *American Political Science Review* 48 (1954): 787–92. Cited 360 times.

—Kenneth A. Shepsle, *Harvard University*

Next to having one's book prominently displayed on the racks of every airport bookstore in the country, receiving recognition for an article written more than half a century ago is a close second. Indeed, for some of us it is the very highest form of intellectual acknowledgment. The Shapley–Shubik power index, invented more than 50 years ago, is a familiar concept in the analytical lexicon of political science. In brief, it is a measure of the *ex ante* likelihood that an individual will be *pivotal* in transforming a nonwinning coalition into a winning one. This depends on the voting or decision rule, on the one hand, and on the number of votes or decision weight possessed by an individual, on the other. The "power" of an individual, so determined, may be aggregated to measure the power of a faction, party, proto-coalition, or voting chamber (the latter in a multicameral setting).

Power is in quotation marks earlier because, as a discipline, political science has never completely resolved exactly what is meant by the concept. Indeed, Riker (1964), in an article that should be more widely acknowledged, concludes that the concept is so riddled with ambiguities that we should ban it from our vocabulary. (That advice has stuck with me all the years since I first encountered it as a graduate student, so that I still have trouble writing out the word.) Nevertheless, Shapley and Shubik's short *Review* article stimulated a light industry of efforts to refine an index of power. Early efforts by Dahl (1957, 1961) and March (1957), stimulated in part by Shapley and Shubik's essay and culminating in the oft-cited piece by Bachrach and Baratz (1962), represented an early trickle that became a steady flow for much of the rest of the century, with a recent surge as Euro-

peanists became absorbed with reforms in European Union institutions.

Political scientists, enamored as they are of the study of power, have long sought a simple, compact measure. Shapley and Shubik, employing the logic of simple games in von Neumann and Morgenstern's cooperative game theory, provided just such a measure. What it provides is a *mechanical* characterization, stripped bare of "any of the sociological or political superstructure that almost invariably exists in a legislature or policy board" (Shapley and Shubik 1954, 791). That is, it takes seriously only the mechanism—voting rule, decision procedure, game form—by which official resources are transformed into outcomes. Thus, their measure of power provides a *causal* account of political results. This link between power and cause is, I believe, one of the very important reasons this paper and many like it have had such impact on the study of politics.

Reading it (again) a half-century after it appeared, I am struck by several things. First, there is humility. Shapley and Shubik believed, and many formal theorists still believe, that the early steps in attacking a problem should consist of simple, stripped down, almost toylike models of a process with the aim of pinning down the *operating characteristics* of a decision-making mechanism in its purest form. From this base one can add complications ("sociological or political superstructure") or, in an equilibrium framework, do comparative statics analysis from which empirical expectations may be derived.

Second, there are nonobvious conclusions, one of the hallmarks of their formal approach. Consider three.

- Power as they measure it is not proportional to votes (or other political resources), for changes in the probability of being pivotal are not proportional to them.
- In their simple tricameral characterization of the American system of legislation, with the ratio of "votes" being (in 1954) 1:96:435 for the president, Senate, and House, respectively (ignoring overrides of presidential vetoes), and where the agreement of each body is required to pass legislation, the ratio of "power" nevertheless amounts approximately to 2:1:1—with the president as powerful as both legislative chambers combined.
- It is obvious that someone controlling a decisive proportion of votes (say, the majority of shares in a publicly traded corporation) possesses *all* the power. What is less obvious is that someone controlling some votes may have *no* power, even in less extreme circumstances. Such an agent is labeled a *dummy* player in the game. He or she is never in a position to transform a nonwinning coalition into a winning one. A four-player majority-rule game with votes for each player given by {3, 3, 2, 1} is such a case. The fourth player is neither sufficient nor necessary to the "winning" status of any coalition of which he or she is a part. (Riker [1964] points out that a weighted voting scheme among towns on Long Island in the 1960s actually created one town as a dummy in this sense.)

Finally, this article constitutes an effort to bring rigor to an ambiguous concept, not by replacing more conventional modes but instead by supplementing them with mathematical and other forms of deductive reasoning. By focusing analytical methods on a phenomenon arising in well-described empirical settings, Shapley and Shubik's article constitutes one of the earliest exemplars of formal political theory that has come to be a prominent feature of the modern scholarly landscape.

---

Lloyd Stowell Shapley is Professor Emeritus of Mathematics at the University of California, Los Angeles. His many honors include membership in the American Academy of Arts and Sciences and the National Academy of Sciences, along with the receipt of the John von Neumann Theory Prize.

Martin Shubik is the Seymour H. Knox Professor of Mathematical Institutional Economics at Yale University. Specializing primarily in strategic analysis, he is the author of approximately twenty books and over 200 articles.

Kenneth Shepsle is the George D. Markham Professor of Government at Harvard. He is a member of the National Academy of Sciences and the American Academy of Arts and Sciences.

## REFERENCES

- Bachrach, Peter, and Morton S. Baratz. 1962. "Two Faces of Power." *American Political Science Review* 56 (December): 947–52.
- Dahl, Robert A. 1957. "The Concept of Power." *Behavioral Science* 2 (July): 201–15.
- Dahl, Robert A. 1961. *Who Governs?* New Haven: Yale University Press.
- March, James G. 1957. "Measurement Concepts in the Theory of Influence." *Journal of Politics* 19 (May): 202–26.
- Riker, William H. 1964. "Some Ambiguities in the Notion of Power." *American Political Science Review* 58 (June): 341–49.

**13. Herbert McClosky. 1964. "Consensus and Ideology in American Politics." *American Political Science Review* 58 (June): 361–82. Cited 345 times.**

—Jack Citrin, *University of California, Berkeley*

—Nelson W. Polsby, *University of California, Berkeley*

"Consensus and Ideology in American Politics" is one of a trio of *Review* articles in which Herbert McClosky (1958; McClosky, Hoffman, and O'Hara 1960) reported on the early results of his empirical inquiries into the ideological foundations of American mass and elite political attitudes and beliefs. This extremely ambitious research program engaged McClosky (who died March 13, 2006) for the next half-century, producing, among other outcomes, two major books (McClosky and Brill 1983; McClosky and Zaller 1984), numerous doctoral dissertations by his students at Berkeley, and a rich course of study in political psychology that constituted a substantial and lively subfield in his department that attracted a great many of its most promising graduate students.

McClosky began to build this extraordinary intellectual edifice at the University of Minnesota, where he completed his doctoral work as a political philosopher. His always eclectic interests led him to early publications on theories of sovereignty, majority rule, and the systematic, ideology-led subversion of democracy by the Soviet Union (McClosky 1949a, 1949b; McClosky, Hoffman, and O'Hara 1960; McClosky and Turner 1960).

The Minnesota intellectual climate proved to be bracing, and as he began his work there as a teacher of theory and comparative government and in the humanities program, McClosky was increasingly drawn to the work of his stellar contemporaries in social psychology, who were, among other activities, creating and elaborating that famous survey instrument, the Minnesota Multiphasic Personality Inventory (MMPI). These psychologists, notably the highly original Paul Meehl, themselves possessed the flexibility and range to appreciate the added value that McClosky, with his deep knowledge of philosophical foundations of human life, could bring to their immensely successful collective enterprise.

---

After two decades at the University of Minnesota, Herbert McClosky moved to Berkeley, where he established his influential program of teaching and research in behavioral political science. He was elected to the American Academy of Arts and Sciences, served as Vice President of the American Political Science Association, and worked with or led numerous research foundations and councils.

Jack Citrin is Professor of Political Science and Associate Director, Institute of Governmental Studies at the University of California, Berkeley. His research interests include political sociology and nationalism.

Nelson W. Polsby is Heller Professor of Political Science at the University of California, Berkeley, where he has taught American politics and government since 1967. He is a Fellow of the American Academy of Arts and Sciences, the American Association for the Advancement of Science, and the National Academy of Public Administration, and is a former Editor of the *Review*.

McClosky undertook a rigorous and thorough formal program of re-tooling with the Minnesota psychologists on a multiyear Social Science Research Council grant that extended over half a decade, at the end of which lay a pot of gold in the form of a major grant that enabled him to conduct a broad survey of American citizens in comparison with 3,020 delegates to the 1956 Democratic and Republican national party conventions. These respondents completed elaborate questionnaires that contained a large array of scales on the style of the MMPI but in addition were framed by the writings of both liberal and conservative classical philosophers.

"Consensus and Ideology" and McClosky's other works from this period thus reflect a marriage between two traditions of scholarship rarely at the command of a single investigator, and this, we believe, is the source of the lasting resonance that this article has had with new generations of political scientists over the last half-century. One mark of his achievement is the large number of issues he raised more than 40 years ago that remain at the center of new methodological and empirical work. Among these one must include the learning of democratic norms, the gap between attitudes and behavior, the degree of coherence among dimensions of liberalism and conservatism, and partisan polarization.

## REFERENCES

- McClosky, Herbert. 1949a. "State Sovereignty: Alexander H. Stephens' Defense of Particularist Federalism." *Review of Politics* 11 (April): 170–83.
- McClosky, Herbert. 1949b. "The Fallacy of Absolute Majority Rule." *Journal of Politics* 11 (November): 637–54.
- McClosky, Herbert. 1958. "Conservatism and Personality." *American Political Science Review* 52 (March): 27–45.
- McClosky, Herbert, and Alida Brill. 1983. *Dimensions of Tolerance: What Americans Believe About Civil Liberties*. New York: Russell Sage Foundation.
- McClosky, Herbert, Paul J. Hoffman, and Rosemary O'Hara. 1960. "Issue Conflict and Consensus Among Party Leaders and Followers." *American Political Science Review* 54 (June): 406–27.
- McClosky, Herbert, and John E. Turner. 1960. *The Soviet Dictatorship*. New York: McGraw-Hill.
- McClosky, Herbert, and John Zaller. 1984. *The American Ethos: Public Attitudes Toward Capitalism and Democracy*. Cambridge: Harvard University Press.

**14. Arthur H. Miller. 1974. "Political Issues and Trust in Government: 1964–1970." *American Political Science Review* 68 (September): 951–72. Cited 340 times.**

—Arthur H. Miller, *University of Iowa*

The importance of citizen trust in government derives from its role in democratic theory. Presumably, the decisions of trusted authorities and institutions are more likely to be accepted as legitimate and worthy of support than are those of distrusted leaders and institutions.

Substantial reaction and interest were provoked by the initial presentation of "Political Issues and Trust in Government" at the 1972 APSA Annual Meeting. This paper presented the first longitudinal analysis of trust. The major finding that trust in government had dramatically declined in the mid-1960s was reported by major newspapers and radio outlets across the country. The TV networks reported the findings on the news as well as *Good Morning America* and *Today*. This response confirmed that "Trust in Government" had tapped a subject that not only was relevant to political

---

Arthur Miller is Professor of Political Science at the University of Iowa. He has written extensively on issues related to political attitudes and behavior.

theories, but spoke to the political concerns of the broader public as well.

The academic response to the published version of "Trust in Government" started with Jack Citrin's (1974) insightful "Commentary." Citrin acknowledged that a major contribution of "Trust in Government" was that it shifted explanations of political trust from the earlier focus on social characteristics and personality (McClosky and Schaar 1965) to evaluations of government performance. Whereas earlier theories had suggested that trust would remain stable over long periods of time, "Trust in Government" argued that if discontent accumulated across a set of authorities, distrust of the government institutions and regime would increase. This generalization hypothesis continued to be the focus of much subsequent work inspired by "Trust in Government" (e.g., Lipset and Schneider 1983).

Another set of studies inspired by "Trust in Government" raised questions about whether the focus of the National Election Studies (NES) items measuring trust reflected assessments of current incumbents as opposed to judgments about the system or regime (e.g., Citrin and Muste 1999; Muller and Jukam 1977). My initial interest in trust arose partially out of such measurement concerns.

While a graduate student at the University of Michigan, I served on the 1970 NES survey as a research assistant to Angus Campbell. At that time I already had a strong interest in political alienation, diffuse support, responsiveness, and trust. These interests had developed from my sense of the turbulence of the 1960s, from taking seminars with Gamson and Walker at Michigan, and from the literature on alienation (e.g., Aberbach and Walker 1970; Easton 1965; Gamson 1968). As a graduate student I constructed a series of survey questions that I felt would make valid measures of diffuse support. Although the 1970 study did not have enough space to accommodate the new items, I lobbied successfully to have all the trust items included in the study (the 1966 study had only two items) and subsequently used them for "Trust in Government." Although measurement questions continue, time and many other studies (including my own reports in Miller 1974 and Miller, Goldenberg, and Erbring 1979) have demonstrated the importance of the trust items as leading indicators of deeper discontent and support.

As levels of trust continued to decline in the United States, an explosion of studies addressed questions raised in "Trust in Government," such as the implications for compliance and for voting or participation. Other studies raised additional explanations for declining trust, such as the rise of television and its cynical messages about politicians. Similarly, the decline in trust in the United States stimulated comparisons with other countries (e.g., Miller and Listhaug 1990). More recently, the durability of distrust prompted tests of the hypothesis that distrust influences policy preferences.

"Trust in Government" made a contribution to political science because it added to conceptual, empirical, and normative theory building regarding the relationship between citizens and government. It presented the first evidence of what was to become a long-term trend of growing political distrust among U.S. citizens. It shifted the focus of scholarly discourse on explanations of political distrust from slowly evolving social and personality factors to political considerations of government performance. It raised a series of hypotheses relevant to investigation by a wide array of related substantive disciplines including comparative studies, sociology, mass communications, psychology, and law. Many of the conclusions reached by "Trust in Government" were controversial, thereby provoking subsequent scholars to examine these issues from different perspectives, but this is exactly how the science of politics is advanced.

## REFERENCES

- Aberbach, Joel D., and Jack L. Walker. 1970. "Political Trust and Racial Ideology." *American Political Science Review* 64 (December): 1199–1219.
- Citrin, Jack. 1974. "Comment: The Political Relevance of Trust in Government." *American Political Science Review* 68 (September): 973–88.
- Citrin, Jack, and Christopher Muste. 1999. "Trust in Government." In *Measures of Political Attitudes*, ed. John P. Robinson, Phillip R. Shaver, and Lawrence S. Wrightsman. New York: Academic Press, 465–532.
- Easton, David. 1965. *A Systems Analysis of Political Life*. New York: Wiley.
- Gamson, William A. 1968. *Power and Discontent*. Homewood, IL: Dorsey.
- Lipset, Seymour Martin, and William Schneider. 1983. *The Confidence Gap: Business, Labor, and Government in the Public Mind*. New York: Free Press.
- McClosky, Herbert, and John H. Schaar. 1965. "Psychological Dimensions of Anomy." *American Sociological Review* 30 (February): 14–40.
- Miller, Arthur H. 1974. "Rejoinder to 'Comment' by Jack Citrin: Political Discontent or Ritualism?" *American Political Science Review* 68 (September): 989–1001.
- Miller, Arthur H., Edie N. Goldenberg, and Lutz Erbring. 1979. "Type-set Politics: Impact of Newspapers on Public Confidence." *American Political Science Review* 73 (March): 67–84.
- Miller, Arthur H., and Ola Listhaug. 1990. "Political Parties and Confidence in Government: A Comparison of Norway, Sweden and the United States." *British Journal of Political Science* 20 (July): 357–386.
- Muller, Edward N., and Thomas O. Jukam. 1977. "On the Meaning of Political Support." *American Political Science Review* 71 (December): 1561–95.

**15. Robert Axelrod. 1986. "An Evolutionary Approach to Norms." *American Political Science Review* 80 (December): 1095–1111. Cited 317 times.**

—Robert Axelrod, *University of Michigan*

I have long been interested in the question of how cooperation can emerge in a world of egoists without central authority. Over a period of five years, culminating in my book on the *Evolution of Cooperation* (1984), I published a series of studies that explored the emergence and maintenance of cooperation in the context of the two-person iterated Prisoner's Dilemma (PD). I was well aware that two-person interactions can tell only part of the story of cooperation in societies, so I thought about various ways of building and sustaining cooperation when one person's actions can affect many others. Unfortunately, the most straightforward way to extend the two-person PD game would not sustain cooperation unless something else was added to solve the collective action problem by preventing free riders.

In looking for mechanisms that actually work in societies, I was impressed with the power of social norms to maintain specific types of cooperation. For example, the norm against cheating can be sustained by individuals who punish cheaters whenever they are detected. The problem is that punishment is typically costly to the punisher even if

---

Robert Axelrod is the Arthur W. Bromage Distinguished University Professor of Political Science and Public Policy at the University of Michigan. In addition to such awards as the Newcomb Cleveland Prize of the American Association for the Advancement of Sciences for an outstanding contribution to science and the National Academy of Sciences Award for Behavioral Research Relevant to the Prevention of Nuclear War, Professor Axelrod is the President of the APSA.

beneficial to the community. In the paradigm of egoists without central authority, no one would want to pay the private costs of punishing a violator, and soon the norm itself would collapse. My article proposed that one way to build and sustain cooperation in such a setting would be for individuals not only to punish the norm violator but also to take vengeance on anyone who did not punish the norm violator. I explored the dynamics of this situation with an agent-based model in which relatively effective strategies would be used more than less effective strategies in the future.

The principal result of this evolutionary approach was that if the level of vengeance was initially high enough, selfish acts (such as cheating) would be suppressed. To be successful you had to punish defectors lest you be punished for *not* doing so. The simulations of the agent-based model demonstrated how and under what conditions cooperation would emerge in a setting where large numbers of people are affected by each selfish act.

The article also informally discussed social norms in relation to other mechanisms for sustaining cooperation in groups, such as law, power differentials, internalization, social proof, and voluntary membership. For most readers, this informal discussion of how various mechanisms complement each other was probably far more useful than the formal model. For example, the article discussed how a law is likely to be effective when supported by an existing social norm, whereas the existing norm, in turn, is reinforced by the law.

This article proved far more compelling than I expected it to be. For example, it was mentioned (along with my two-person work) in the citations for two awards I won shortly thereafter, a Five-Year McArthur Prize Fellowship and the National Academy of Sciences Prize for Behavioral Research Relevant to the Prevention of Nuclear War.

It is hard to evaluate the influence of this work. The formal modeling of social norms did pick up markedly after this article was published in 1986, but several earlier works also promoted this trend. What is clear is that my terminology of “metanorms” did *not* catch on, and the concept had to be “reinvented” under other labels such as one of the (many) meanings of “indirect reciprocity.” The influence of the model is perhaps more clear in the growing literature on agent-based models and evolutionary approaches to game theory. I attribute much of whatever influence it did have to my decision to treat norms in behavioral terms, rather than rely on the standard definition of social norms in terms of beliefs and values.

Another aspect of the article’s influence was its relevance to a wide range of problems in political science and beyond. Within two years of publication, it was cited not only in political science journals, but also in journals of economics, sociology, law, philosophy, anthropology, and animal behavior.

The article has apparently stood the test of time. Unlike the typical scholarly article, which receives fewer and fewer citations after the first four or five years, the annual citation rate for this article shows no signs of declining after 20 years. The citations in the second decade after publication actually exceeded the citations in the first decade by 30%.

I attribute much of the attention this article has received to its role as an extension of my earlier work on pair-wise cooperation. More generally, both projects benefited from the widespread desire to provide a “hardheaded” rationale for cooperation.

## REFERENCE

Axelrod, Robert. 1984. *The Evolution of Cooperation*. New York: Basic Books.

16. Michael Doyle. 1986. “Liberalism and World Politics.” *American Political Science Review* 80 (December): 1151–69. Cited 311 times.

—Michael Doyle, *Columbia University*

“Liberalism and World Politics” had an impact because it is short and (by my standards) well written. I am sure its brevity has attracted anthologists. The more substantial reasons for its impact seem to me to be the following five:

First, the article addressed an important anomaly. Traditional theories of democratic foreign policy said that democracies would inherently be peaceful. The mainstream of the literature on war and peace refuted that claim. A group of carefully done empirical studies suggested that democracies were peaceful, but only with one another. This article resolved the anomaly by outlining the logic of how states that were both liberal and representative would maintain peace with one another, but not reliably with nonliberal, nonrepresentative states. It argued that although states throughout history have gone to war with one another to advance both their rational national and nonnational governmental interests, liberal democratic peoples have, by and large, respected one another’s sovereignty and security.

This argument was inspired by Kant’s “Perpetual Peace,” (Kant 1970) which held that liberal representative republics do not wage war against one another (though they sometimes do go to war against nonrepublican peoples). The reasoning was that liberal republics are responsible to the majority of electors, who, unlike monarchs and dictators, cannot regularly displace the costs of going to war on others. If, moreover, those electors respect liberal principles, then they will respect the rights of peoples similarly free to express their rights, and will negotiate rather than fight over differences of interest. Respect for rights of property and the benefits of commercial exchange will reinforce these moral commitments.

But the same respect and self-interest that generates a self-enforcing peace among liberal republics produces distrust of states that oppress their subjects and compound conflicts of interest between liberal republics and nonliberal autocracies that neglect property rights and restrict trade and other contacts. This is what makes for a peace among liberals, but not between liberal and nonliberal states.

Second, it was interdisciplinary. It combined the international relations of war and peace with political and social philosophy. It drew on Machiavelli, Schumpeter, and Kant to distinguish imperial, pacific, and internationalist variants of popular republicanism.<sup>1</sup>

Third, it bridged methodological divides. It reflected my long fascination with Carl Hempel’s (1942) “The Function of General Laws in History” and yet it gave a central place to interpreted, inter-subjective ideas and norms as causal factors. It lent itself to both quantitative and qualitative assessment. It argued that significant insights could be produced by the combination of institutions (representative republics), ideas (human rights), and interests (commerce, investment) that did not produce significant outcomes in isolation.

---

Michael Doyle is the Harold Brown Professor of International Affairs, Law and Political Science, Columbia University. He spent two years on leave to work as assistant secretary general and special adviser to UN Secretary-General Kofi Annan. He currently serves as a board member of UNDEF (the recently established UN Democracy Fund).

<sup>1</sup> I later changed my mind about how to interpret Machiavelli’s views (see Doyle, 1997, Chapter 2).

<sup>2</sup> I sent the latter a paper on how to preserve, protect and expand the community of liberal democratic states, published by Doyle in 1992.

Fourth, it provided a framework for thinking about politics that linked to real world political discourse ranging from Madison to Gladstone to Wilson and Reagan (whom it criticized for being half-right). I sent memoranda—with no appreciable effect—to the Dukakis and Clinton campaigns.<sup>2</sup> Nonetheless, what came to be called “democratic peace” served as a framework for thinking about democratic foreign policy after the Cold War.

Fifth, and importantly, it has attracted a large legion of many of the most talented scholars in international relations. Some were convinced, and still are, that it was wrong; others, that it was right. They turned their energies and insights into demonstrating their convictions, which generated many citations. (I acknowledge that if more had been convinced that it was right—or wrong—it would have had less “impact,” as measured by the citations that have been tallied). Scholars contributed new insights on the relative weights of institutional, normative, and material influences, and the relative significance of disputes and armed conflicts, perceptions and realities, and transitional versus established democracies. It would exhaust my quota of words to begin a bibliography of the excellent literature. Rather than doing so, I will simply refer to the bibliography in a recent study by David Rousseau that both surveys and contributes to the debate that followed (2005, 351–73).

## REFERENCES

- Doyle, Michael. 1992. “An International Liberal Community.” In *Rethinking America's Security*, ed. Graham Allison and Gregory Trevorton. American Assembly and Council on Foreign Relations. New York: Norton, 307–36.
- Doyle, Michael. 1997. *Ways of War and Peace*. New York: Norton.
- Hempel, Carl. 1942. “The Function of General Laws in History.” *Journal of Philosophy* 39 (January): 35–48.
- Kant, Immanuel. 1970. *Kant's Political Writings*, ed. by Hans Reiss and trans. by H. B. Nisbet. Cambridge: Cambridge University Press.
- Rousseau, David. 2005. *Democracy and War*. Stanford: Stanford University Press.

**17. Nelson W. Polsby. 1968. “The Institutionalization of the U.S. House of Representatives.” *American Political Science Review* 62 (March): 144–68. Cited 287 times.**

—Nelson W. Polsby, *University of California, Berkeley*

I frequently tell my graduate students that a good indicator that a piece of work has attained the status of a classic is the extent to which it is cited irrelevantly. Although I have been vaguely conscious in the years since this article was published that it has turned up in a lot of footnotes, I have not followed its career in the literature closely enough to be able to tell whether by this austere criterion it qualifies.

The idea for the article occurred to me more or less fully blown one sunny California afternoon in 1965 or 1966 at the Center for Advanced Study in the Behavioral Sciences. That evening I described to my wife why I thought such an article would fill a significant intellectual gap, bringing together

---

Nelson W. Polsby is Heller Professor of Political Science at the University of California, Berkeley. He is a Fellow of the American Academy of Arts and Sciences, the American Association for the Advancement of Science, and the National Academy of Public Administration, and is a former Editor of the *Review*. He thanks the late Ralph Tyler and the late Preston Cutler for running a superb think tank whose supportive and flexible ethos persists to this day. CASBS staff member Miriam Gallaher and then-Stanford graduate student Barry Spencer Rundquist helped assemble the time series, an enormous job well done.

large and disparate sets of time series data, organized in a theoretically informed way. I thought this would give a coherent picture of a big piece of the historical development of the U.S. House of Representatives, which I was studying at the time.

The Center provided resources and help in putting the time series together, and, luckily, the argument worked pretty well once they were all assembled. The theoretical framework was adapted mostly from Max Weber and Emile Durkheim. The historical focus was greatly informed by ideas percolating among a stimulating group of students of the House with whom I was then in close communication, notably H. Douglas Price, who in turn had been influenced by Samuel Huntington's imaginative work on political development.<sup>1</sup>

That, to the best of my recollection, is where this article and its much less well-known companion article on the seniority system, published in the *Review* in 1969, came from (Polsby Gallaher and Rundquist 1969). I still think they are good articles, and I am glad others have found them useful. They are best understood, I think, as a historical description of a particular institution rather than as themselves a “theory” specifying causes and effects with broad automatic application to other legislatures or institutions. Many other legislatures have, to be sure, institutionalized, but not necessarily in the historically contingent way the House did. I have written about this with reference to the British House of Commons, which as I see it institutionalized in accordance with its own constitutional logic sometime in the seventeenth century (see Polsby, especially p. 207 footnote 26).

The main theoretical claim that this article makes is that various elements of institutional practice—growing adherence to boundaries, increased internal complexity, and increasing reliance on universalistic criteria for internal decision making—hung together and cohered to produce the organizational structure of the contemporary House of Representatives. The relevant middle-range theoretical ideas come, I suppose, from organization theory, and I would hope that somewhere down the line this article might prove useful in understanding organizations not configured as hierarchies, as so many organizations that actually get studied are. I would like especially to acknowledge the influence of my teacher Charles E. Lindblom in pushing my ideas in this direction (see especially Lindblom 1959).

In the 38 years since “Institutionalization” was published, there has been much good work by many scholars on the history of the House and more generally on Congress. Just a short while ago in *How Congress Evolves*, I tried to account for more recent trends in congressional development, focusing on somewhat different variables (parties, elections, demographic and technological changes) and a different time period (Polsby 2004). Despite the differences, the mode of argument pursued in “Institutionalization” helped me write my new book, as I hope it may have helped others.

## REFERENCES

- Fenno, Richard. 1998. “Introduction.” *Explorations in the Evolution of Congress*, by H. Douglas Price. Berkeley: Institute of Governmental Studies Press.
- Lindblom, Charles. 1959. “The Science of Muddling Through.” *Public Administration Review* 19 (Spring): 79–88.

---

<sup>1</sup> The influence of invisible colleagues enabling individual achievement cannot be overestimated in my opinion. See Richard Fenno's discussion of the House group from which I drew sustenance (Fenno 1998).

Polsby, Nelson W. 2004. *How Congress Evolves: Social Bases of Institutional Change*. New York: Oxford University Press.

Polsby, Nelson W. 1975. "Legislatures." In *Handbook of Political Science*, ed. Fred I. Greenstein and Nelson W. Polsby. Reading, MA.: Addison-Wesley, 257–319.

Polsby, Nelson W., Miriam Gallaher, and Barry Spencer Rundquist. 1969. "The Growth of the Seniority System in the U.S. House of Representatives." *American Political Science Review* 63 (September): 787–807.

**18. Ronald Inglehart. 1971. "The Silent Revolution in Europe: Intergenerational Change in Post-industrial Societies." *American Political Science Review* 65 (December): 991–1017. Cited 282 times.**

—Ronald Inglehart, *University of Michigan*

"The Silent Revolution in Europe" was the first presentation of the intergenerational value change hypothesis. It argued that the publics of postindustrial societies are shifting from giving top priority to "Materialist" goals such as economic and physical security to placing increasingly strong emphasis on self-expression and the quality of life. Throughout history, survival has been insecure for most people, forcing them to give top priority to survival needs. But in advanced industrial societies, the economic miracles of the postwar era and the emergence of the welfare state gave rise to conditions under which much of the postwar generation grew up taking survival for granted; they give increasingly high priority to "Postmaterialist" values such as belonging, self-expression, and free choice. Because people's basic values do not change overnight, those who experienced the insecurity of World War I, the Great Depression, and World War II would continue to emphasize Materialist values, but we would expect to find an intergenerational shift toward Postmaterialist values.

If this is true, the implications are far-reaching. Quality of life issues should gradually displace class-based issues such as income redistribution and state ownership of industry as the main axis of political conflict. New types of political movements and parties should emerge to champion the new issues. Social class voting should erode, as support for change shifts from its traditional working-class Materialist base to Postmaterialists from relatively secure middle-class backgrounds.

This hypothesis grew out of analysis of the May 1968 protest movement in France, which I originally approached from the perspective of Marxist class conflict; that approach was widespread at the time and dominated the rhetoric used by the participants themselves. But survey data blatantly did not fit the implications of a Marxist model: in the aftermath of the May events, workers who had previously voted for the Left shifted to support the Gaullist regime, whereas young, predominantly middle-class students constituted the main base of support for radical social change. A large share of the public had taken part in various protest activities, and their responses to the question, "What did you hope to attain?" revealed large differences between the motivations of older and younger generations, and between economic strata, that led me to develop the intergenerational value change hypothesis.

This article tested that hypothesis against 1970 survey data from six Western countries. I found the predicted intergenerational differences: among those age 65 and older, Materialists were 12 times as numerous as Postmaterialists across the six countries; but among the postwar generation,

Postmaterialists were slightly more numerous than Materialists. With data from a single time point, it was impossible to determine whether these age differences reflected intergenerational value change (which implies gradual but pervasive value change) or life-cycle differences (which implies that the young would eventually become just as Materialist as their elders, so that no overall value change would occur).

Initially, critics of the intergenerational value change thesis argued that the observed age differences merely reflected life-cycle effects. But today, cohort analysis of data covering three decades demonstrates that the respective birth cohorts did *not* become more Materialist as they aged: they remained fully as Postmaterialist as they had been in 1970. Moreover as younger, relatively Postmaterialist cohorts replaced older, relatively Materialist ones in the adult population, the predicted intergenerational value shift occurred: in 1970, Materialists were four times as numerous as Postmaterialists across the six nations, but by 2000, Postmaterialists outnumbered Materialists in all six countries (Inglehart and Welzel 2005). The central issues of political conflict have shifted, with the rise of environmentalist movements, the women's movement, gay liberation, and other lifestyle movements. Moreover, social class voting has declined in most advanced industrial societies; in the last two U.S. presidential elections, for example, the vote polarized much more strongly on lifestyle issues, such as abortion and same-sex marriage, than on social class.

When the postwar generation first became politically relevant around 1970, it manifested itself as student protest. Today, generational cleavages are less striking. The 1970s adage, "Don't trust anyone over 30" would translate into "Don't trust anyone over 65." But value-based polarization continues, and recent research demonstrates that Materialist/Postmaterialist values are just one indicator of a much broader cultural shift from Survival values to Self-expression values. This shift is bringing changing values concerning gender roles, sexual orientation, work, religion, and child-rearing—and it is conducive to the spread and flourishing of democratic institutions (Inglehart and Welzel 2005).

## REFERENCE

Inglehart, Ronald, and Christian Welzel. 2005. *Modernization, Cultural Change and Democracy*. New York: Cambridge University Press.

**19. Zeev Maoz and Bruce Russett. 1993. "Normative and Structural Causes of Democratic Peace, 1946–1986." *American Political Science Review* 87 (September): 624–38. Cited 278 times.**

—Zeev Maoz, *University of California at Davis*

—Bruce Russett, *Yale University*

This article—one of only two on the "top twenty" list published later than 1986—undertook two tasks. The first was to examine whether the "democratic peace" was a spurious relationship that could be disconfirmed by several possible alternative explanations. The second was to provide a stepping

Zeev Maoz is Professor of Political Science at the University of California, Davis. His research interests include strategy and international security affairs, international relations theory, decision making and negotiations, and political methodology, with a special interest in Middle East politics and Israeli security policy.

Bruce Russett is Dean Acheson Professor of International Relations and Director of United Nations Studies at Yale, and Editor of the *Journal of Conflict Resolution*. He has been President of the International Studies Association and the Peace Science Society (International).

Ronald Inglehart is Research Professor in the Center for Political Studies at the University of Michigan. To explore cultural change and its consequences, he coordinates a world-wide survey of mass values and attitudes, the World Values Survey.

stone—to be revised and improved by others—for converting the democratic peace from a theoretically loose, substantially intuition-driven project into a research program in which theory and empirics reinforced each other.

Our collaboration stemmed from submission of a manuscript by Maoz and Abdolali (1989) to the journal edited by Russett. That was then the most systematic analysis of the democratic peace, using Polity regime data, scales for violent disputes (not just war), and a long time period. It found evidence that democracies rarely fight one another, but it did not support the idea that democracies are more peaceful in general. Russett, considering the accumulating evidence for a democratic peace but troubled that all the analyses had been solely or nearly bivariate, knew that few scholars would take it seriously until it had passed various multivariate tests that controlled for influences like wealth, relative power, alliances, and contiguity. He was far from confident that it would pass, but felt that this required a more thorough set of empirical tests. When he was a Fulbright scholar in Israel during spring 1989, he met Maoz in Tel Aviv and asked him to collaborate. The first outline of their collaborative research was written on a napkin in a restaurant. The collaboration was done mostly by e-mail after Russett's return home.

At this point Maoz made the important theoretical innovation of insisting that what were becoming known as the normative and institutional explanations should be treated as alternative hypotheses for their relative contributions, and he articulated the theoretical propositions more carefully. We established a claim to priority with a simple version using contingency tables (Maoz and Russett 1992), and then published the more sophisticated analysis in the *Review* article and in parts of Russett (1993).

We confronted several fundamental problems in working on this study. One was to outline in a more coherent manner the underlying assumptions of both explanations in a manner that would allow deducing seemingly inconsistent behavior by democracies (that they are pacific with respect to each other, but sometimes belligerent toward nondemocratic states). Another problem entailed differentiating the normative explanation from the structural one on both a theoretical and an empirical level. We also struggled a great deal in attempting to find empirical measures of democratic norms. Finally, we spent much effort in trying to develop critical tests for these explanations.

Subsequently this work became an expanding democratic peace research program to include the effects of trade (e.g., Oneal, Oneal, Maoz, and Russett 1996) and international organizations (e.g., Russett and Oneal 2001; Pevehouse and Russett 2006) over a much longer time period (1885–2001) into what we now call the Kantian Peace project. Maoz also wrote some of the most detailed and scientifically compelling defenses of the democratic peace (e.g., Maoz 1997, 1998). Since 1993 the project has continued with at least 18 co-authors and many hundreds of other scholars who built on, complemented, or criticized our work to make the research program central to understanding the effects of internal and transnational influences on national foreign policy decisions. It has also been influential in policy circles, although in some cases the findings of this program have been misused and misinterpreted by politicians.

## REFERENCES

Maoz, Zeev. 1998. "Realist and Cultural Critiques of the Democratic Peace: A Theoretical and Empirical Reassessment." *International Interactions* 24 (January): 1–89.

Maoz, Zeev. 1997. "The Controversy over the Democratic Peace: Rearguard Action or Cracks in the Wall?" *International Security* 23 (Summer): 162–98.

Maoz, Zeev, and Nasrin Abdolali. 1989. "Regime Types and International Conflict, 1816–1976." *Journal of Conflict Resolution* 33 (March): 3–35.

Maoz, Zeev, and Bruce Russett. 1992. "Alliances, Wealth, Contiguity, and Political Stability: Is the Lack of Conflict between Democracies a Statistical Artifact?" *International Interactions* 17 (July): 245–67.

Oneal, John, Frances Oneal, Zeev Maoz, and Bruce Russett. 1996. "The Liberal Peace: Interdependence, Democracy, and International Conflict, 1950–85." *Journal of Peace Research* 33 (February): 11–38.

Pevehouse, Jon, and Bruce Russett. 2006. "Democratic IGOs Promote Peace" *International Organization* 60 (Fall): 969–1000.

Russett, Bruce. 1993. *Grasping the Democratic Peace: Principles for a Post-Cold War World*. Princeton: Princeton University Press.

Russett, Bruce, and John R. Oneal. 2001. *Triangulating Peace: Democracy, Interdependence, and International Organizations*. New York: Norton.

**20. Edward R. Tufte. 1975. "Determinants of the Outcomes of Midterm Congressional Elections." *American Political Science Review* 69 (September): 812–826. Cited 259 times.**

—Edward R. Tufte, *Yale University*

Bernard Berelson wrote a memoir about *Human Behavior: An Inventory of Scientific Findings*, a book that brought together 1,003 findings about human behavior with some reasonable supporting evidence. Berelson offered a threefold grand summary of all social science research: (1) Some do, some don't. (2) The differences aren't very great. (3) It's more complicated than that.

A type 3 study, my 1975 *Review* article suggested that U.S. congressional midterm elections were more complicated than the then-prevailing view: a pendulum swing against the incumbent president party's caused by interacting turnout and preference effects unique to midterm elections. My idea was that the magnitude of the swing in the national congressional vote against the White House party could be explained historically—and also predicted—by national macroeconomic conditions prior to the election and by the incumbent president's approval rating. The model fit well enough that it was possible to back-calculate, say, William Howard Taft's presidential approval rating, absent the existence of Gallup Polls then, for October 1910 on the basis of the 1910 midterm election vote and 1910 economic data. I decided not to mention such antic possibilities in the *Review*.

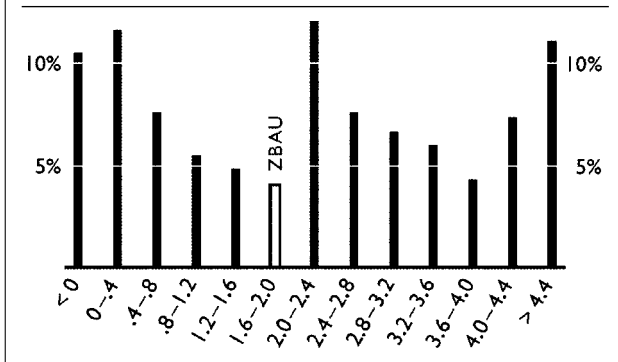
My work politely fit into referendum models of elections and in particular Gerald Kramer's fine study of economic conditions and presidential elections. The midterms research grew out of my earlier 1973 *Review* article on seats and votes, which described how nationally aggregated preferences of voters translated into congressional seats. That translation had decayed so that by the 1970s, changes in the national party vote for congressional candidates produced small and biased changes in the party composition of the House of Representatives. This grotesquely undemocratic arrangement has worsened since, delinking changes in voter preferences from changes in party composition of the House for most congressional elections.

The midterm model was extended in my 1978 book *Political Control of the Economy* to on-year congressional and

---

Edward Tufte was Professor of Politics and Public Affairs at Princeton University, and then at Yale University, Professor of Political Science, Statistics, and Computer Science, and a Senior Critic in the School of Art.

**FIGURE 1. Values of Published t-Statistics for 248 Regression Coefficients in All 17 Then-Published Studies of Economic Conditions and Election Outcomes**



presidential elections. That book dealt largely with how politics affected macroeconomic conditions via the electoral-economic cycle and partisan preferences in economic policies. Put together, the two *Review* articles, *Political Control of the Economy*, and some models in my *Data Analysis for Politics and Policy* (1974) measured how economic and political factors translated into economic policies, the votes of citizens, and who got elected.

In addition to providing a quantitative description of some causal factors in elections, this work encouraged validation by honest prediction (now a thriving industry) rather than by after-the-fact statistical tests.

As the literature on political economy grew in the late 1970s, I developed doubts about multiple regressions on historical data. For all 17 studies of elections and economic conditions published by 1978, I constructed distributions of published statistics (Figure 1), such as published values of  $R^2$  and t-statistics (as opposed to hypothetical distributions under ridiculous null hypotheses). Because competing researchers on a topic seek to publish decisive results, the meta-analytic result is a peculiar distribution of published test statistics in the literature on a particular topic: heapings that strongly favor particular points of view and few results in the dreaded Zone of Boredom, Ambiguity, and Unpublishability (ZBAU). For t-tests on regression coefficients, for

example, ZBAU values fall between 1.6 and 2.0. The 17 then-published multiple regression studies of economic conditions and elections contested whether the national economy affected election outcomes (readers of a certain age will recall the divergent decisiveness of Kramer versus Stigler). Sure enough, the distribution of published t-values reveals a thinness in the ZBAU region, a result replicated for studies of economic conditions and presidential approval ratings, and for the distribution of published values of  $R^2$ .

As it turned out, at age 35, I had completed my work on political economy. I had expected to study the microeconomic side (tentative book title: *Economic Control of Politics*). Instead, returning to my quantitative roots and seeking to produce forever knowledge, I came to write four books on visual and statistical evidence: *The Visual Display of Quantitative Information* (1983, 2001), *Envisioning Information* (1990), *Visual Explanations* (1997), and *Beautiful Evidence* (2006).

As a Stanford sophomore in 1962, under the wonderful influence of Kenneth Prewitt and Raymond Wolfinger, I first subscribed to the *Review*. Perhaps even then I expected to publish here. But I never imagined that I would write something for the *Review* in the nostalgic first-person singular. I am grateful for the opportunity.

## REFERENCES

- Berelson, Bernard. 1987. "Citation Classic: Bernard Berelson and Gary Steiner, *Human Behavior: An Inventory of Scientific Findings*. (New York, 1964)." In *Contemporary Citation Classics in the Social and Behavioral Sciences*, ed. Neil J. Smelser. Philadelphia: ISI Press.
- Tufte, Edward R. 1973. "The Relationship Between Seats and Votes in Two-Party Systems." *American Political Science Review* 67 (June): 540-54.
- Tufte, Edward R. 1974. *Data Analysis for Politics and Policy*. Englewood Cliffs, NJ: Prentice-Hall.
- Tufte, Edward R. 1978. *Political Control of the Economy*. Princeton, NJ: Princeton University Press.
- Tufte, Edward R. 1983. *The Visual Display of Quantitative Information*. Cheshire, CT: Graphics Press.
- Tufte, Edward R. 1990. *Envisioning Information*. Cheshire, CT: Graphics Press.
- Tufte, Edward R. 1997. *Visual Explanations: Images and Quantities, Evidence and Narrative*. Cheshire, CT: Graphics Press.
- Tufte, Edward R. 2006. *Beautiful Evidence*. Cheshire, CT: Graphics Press.