

# Letters

## Rainbow Coalition in Philadelphia

I enjoyed Carlos Muñoz and Charles Henry's "Rainbow Coalitions in Four Big Cities: San Antonio, Denver, Chicago and Philadelphia" (*PS*, Summer 1986, pp. 598-609). But as one who has studied and been active in Philadelphia politics, please allow me to qualify their observations on the City of Brotherly Love.

First, the authors paint Mayors Joseph S. Clark and Richardson Dilworth as liberal reformers succeeded by two ethnic, machine-style politicians who ended their reforms, the Irishman James (H. J.) Tate and the Italian Frank Rizzo. They then identify Mayor W. Wilson Goode's predecessor, William Green, Jr., as one "whose politics were closer to those of Clark and Dilworth than Rizzo" (p. 601). But appearances can be deceiving. In fact, Clark and Dilworth were early products of the same machine that gave Philadelphia Tate and Rizzo. Though more liberal than Rizzo—and that's not hard—Billy Green, Jr. was embraced and advanced by the machine largely because his father (deceased when Jr. ran) was one of the machine's earliest and strongest kingpins. Moreover, the Republican machine that ran Philadelphia for decades until 1951 survived in large measure as many of its workers became Democratic "turncoats," keeping their jobs in City Hall and in the precincts.

Secondly, and more important in light of the article's focus on "rainbow coalitions," the authors note that in 1975 a black, Charles Bowser, ran an unsuccessful independent candidacy for mayor. "His efforts," they write, "resulted in only a 4% increase in black registration (to 29% total)" (p. 601). They then credit black city councilman Lucien Blackwell's 1979 independent candidacy

with raising total black registration by eight percentage points. For the most part, that's true. But they miss a vital key to their story by failing to note that it was Bowser who, in opposing Green for the Democratic nomination in 1979, built the city's first real "rainbow coalition." Bowser, of course, lost, but gave Green (and the machine) a run for its money—and its workers. More to the point, among other non-minorities on his ticket, Bowser featured a white for City Controller and an Italian who had worked for the machine (my father, incidentally) for Sheriff. Working-class whites did not support the ticket, but this primary fight helped set the stage for Goode by mobilizing black political organizations, further enervating an already sick machine, and raising the possibility, as yet unrealized, of the sort of coalition with which the authors are concerned.

John J. Dilulio, Jr.  
Princeton University

## Perils in Citation-Counting

"Ranking the Graduate Departments in the 1980s" by Hans-Dieter Klingemann in your Summer 1986 issue represents a useful addition to the growing literature on the assessment of the relative standings of graduate departments of political science. In conjunction with the standard reputational and publication type assessments, Dieter's careful measurement of professional standing based on frequency of citations gives us useful tools for tracking the quality of our programs and for judging how far each of our units has come or has yet to go. Having said this, I would add that the citation tool can still be greatly improved. Let me suggest a number of possible revisions in the Klingemann methodology.

1. It is important that the total citations for a department be divided by the number of rostered faculty so that we end up with a per capita measure. Without doing so, there is the danger that a high quality and well regarded department with few members will be lost in the tidal wave of sheer numbers of large departments, regardless of their quality.
2. A measure of this sort will inevitably receive a great deal of attention from political scientists, potential graduate students, and university administrators. It is important, therefore, that this type of measure be periodically recalculated and published so that rankings do not get locked and immobilized in our disciplinary memory. Outside of the top ten or so departments, standings can change quite dramatically over a short period of time given the movement of key scholars, and our assessment measure should be sensitive to these changes. My own department, to take a case in point, was not ranked in the top seventy in the Klingemann article, yet using our 1986-87 AY roster, we now rank approximately 25th because of two important hirings in the past year and a half.
3. The use of the Social Science Citation Index greatly under-credits the contributions of some scholars who are the co-authors of articles but not first in alphabetical order. Klingemann is sensitive to this problem and after looking at the issue of co-authorship, claims that the distortion in the data is insignificant. It is my belief, based on the recalculation of the index citations for my own department, that while the distortion is not great in general and in the long run, it can seriously distort the contributions of junior scholars who have not yet had the opportunity to take the lead in a collaborative effort. Those research departments with a heavy representation at the junior end of the scale, therefore, may be given significantly less credit than they deserve. I'm not certain how to rectify this problem, given the way citations are credited in the Index, but we should at least be sensitive to it.

4. Any general use of the Social Science Citation Index must, finally, factor out the considerable number of self-citations. The point is so obvious that I need not belabor it.

I hope these comments are helpful and add to the discussion of how we might best assess departmental quality.

Edward S. Greenberg  
University of Colorado, Boulder

## Deadwood

Although Hans-Dieter Klingemann's rankings of departments on the basis of *Social Science Citation Index* listings provides a stronger basis for judgment than reputational analysis or numbers of publications, as he effectively argues (*PS*, Summer 1986), it has one rather obvious shortcoming of its own: it does not adjust for size of department. Surely this biases the results in favor of large units, even if the "deadwood" proportion may be high.

This bias can easily be eliminated and a stronger index created by making an adjustment for size, and thus creating a *per faculty member* index.

Using that approach, and the same source for number of faculty as Klingemann had used for his list of persons, differences in rankings do occur, some of which are striking. For example, Cal Tech goes from 46 to 5 in the ranking, and UC San Diego changes from 17 to 11. UC Riverside, not on the Klingemann list (apparently because an earlier name of one faculty member was not figured in) becomes number 24 on a per capita basis.

Since faculty members who are not research-productive would contribute relatively little to a department's professional standing (no matter how valuable their teaching and service contributions may be to their departments), a ranking that is size-adjusted may come closer to what we intuitively think counts most than one that is not so adjusted.

As you can see by the attached table, we do agree on one thing, Harvard still ranks first.

Frank Way  
University of California, Riverside

**Letters**

	<b>University</b>	<b>No. of Lines</b>	<b>No. of Faculty Members</b>	<b>= LLPM</b>	<b>Adjusted Rank</b>	<b>Klingemann Rank</b>
≥ 100	Harvard	9362	37	253	1	1
	Stanford	6352	27	235	2	4
	Yale	7219	38	190	3	2
	Johns Hopkins	2573	16	161	4	12
	Cal Tech	1149	8	144	5	46
	UC Irvine	2150	15	143	6	15
	Chicago	3645	27	135	7	6
	Rochester	2136	16	134	8	16
	Michigan	6444	49	132	9	3
	UC Berkeley	5203	45	116	10	5
	UC San Diego	2052	20	103	11	17
Columbia	3553	35	102	12	7	
≥ 60	MIT	3250	33	98	13	8
	Brandeis	1713	18	95	14	26
	Wisconsin/Madison	3243	35	93	15	9
	Duke	1932	21	92	16	21
	Princeton	3229	36	90	17	10
	Cornell	2235	27	83	18	14
	Washington/St. Louis	1495	21	71	19	29
	Iowa	1389	20	69	20	35
	Northwestern	1889	28	67	21	22
	Illinois	1854	30	62	22	23
≥ 50	Hawaii	1840	31	59	23	24
	UC Riverside	701	12	58	24	(68)
	Houston	1403	25	56	25	31
	Michigan State	1347	24	56	25	37
	Ohio State	1727	31	56	25	25
	UCLA	2594	47	55	28	11
	Indiana	1967	36	55	28	18
	NYU	1248	23	54	30	42
	SUNY Buffalo	1015	19	53	31	51
	UC Santa Barbara	1227	24	51	32	43
	Denver	924	18	51	32	54
	Arizona	1304	26	50	34	40
	Florida State	1347	27	50	34	38
	Kentucky	1191	24	50	34	44
Southern California	1939	19	50	34	20	
≥ 40	Minnesota	1297	27	48	38	41
	North Carolina	1403	29	48	38	32
	Arizona State	1403	30	47	40	34
	Georgia	1325	28	47	40	39
	Rutgers	2284	50	46	42	13
	Georgetown	1135	25	45	43	47
	Pennsylvania	846	19	45	43	59
	Texas	1473	33	45	43	30
	Wisconsin/Milwaukee	980	23	43	46	51
	Boston University	754	18	42	47	63
	Carnegie-Mellon	1565	37	42	47	28
	Maryland	1572	38	41	49	27
	Oregon	776	19	41	49	62
	Virginia	1424	35	41	49	31

(continued)

	University	No. of Lines	No. of Faculty Members	= LLPM	Adjusted Rank	Klingemann Rank
≥ 30	Washington, Univ. of	1100	28	39	52	49
	Pittsburgh	1058	28	38	53	50
	Temple	867	23	38	53	58
	Catholic	656	19	35	55	71
	CUNY Grad School	1967	58	34	56	18
	Claremont Grad Sch	1184	35	34	56	45
	Florida	917	27	34	56	55
	Massachusetts	980	29	34	56	34
	Nebraska	712	21	34	56	66
	SUNY Albany	705	21	34	56	67
	UC Davis	726	22	33	62	65
	Illinois/Chicago	888	27	33	62	56
	Notre Dame	846	26	33	62	61
	American	1114	35	32	65	48
	Connecticut	1368	44	31	66	36
	Syracuse	747	24	31	66	64
	Kansas	860	29	30	68	59
SUNY Binghamton	684	23	30	68	68	
< 30	Fletcher	885	36	25	70	57
	Northern Illinois	663	28	24	71	70
	South Carolina	670	49	14	72	69

## Mistaken Identity

I was, to be sure, gratified to find myself rated “#14” nationally among specialists in comparative politics, when a colleague recently called my attention to Hans-Dieter Klingemann’s article in the Summer 1986 *PS*. I must, however, acknowledge the considerable help I received in making the top 20.

That help has been provided by my friend and colleague Professor *Walker* Connor, of Trinity College, Hartford, whose own considerable string of citations in SSCI was evidently added to mine. The confusion is natural. Prof. Connor writes frequently on ethnopoliitics, and has turned his attention in recent years to ethnic politics in the USSR; my own work is in Soviet and East European affairs also. My self-esteem—and his as well, I trust—will survive the admission that each of us, roughly, accounts for about half the 338 lines with which I am credited.\* SSCI at

\*This is not to be construed as a disclaimer with respect to the Guggenheim Fellowship I am credited with receiving (p. 731 of the same issue). That, I am happy to say, is true.

times enters citations to my work under his name (“W”), to his work under mine (“WD”), to further confuse matters.

Presuming to write on our joint behalf, I must hold us innocent of active contribution to the confusion: we have never co-authored an article or book, though the prospect of thus bedeviling indexers is an interesting one. Our caution did not, however, prevent us from being booked into the same single room at a conference some years ago, nor has it, obviously, spared us other problems of merged identity and misdirected mail! “Number 21,” you just made the cut.

Walter D. Connor  
Russian Research Center,  
Harvard University  
and Boston University

## Recruiting Women

As I end my term as APSA Chair of the Committee on the Status of Women (CSW) I would like to make an observation of interest to departmental recruitment committees.

## Letters

I have received many copies of job announcements during my term. These are usually covered with a letter asking me, as chair of the CSW, to call the announcement to the attention of women. Most of these are junior positions.

This is not a useful exercise, and should not be considered part of a departmental affirmative action program. The chair of this committee has no network to reach new Ph.D.'s and other possible candidates other than the one at the disposal of any department: the APSA *Personnel Service Newsletter*. The CSW is designed to investigate general questions and develop policy recommendations and projects of relevance to the status of women in the profession.

The chair and other members of the CSW may be helpful to departmental recruitment committees through their ability to answer specific questions about the process and problems of recruiting women.

Virginia Sapiro  
University of Wisconsin-Madison

## Statistical Double-Talk?

I can usually shrug off what I call the "junk empiricism" that dominates our professional journals. But the Brams and Kilgour article, "Is Nuclear Deterrence Rational?" in your Summer 1986 issue cannot be dismissed so lightly.

It is intellectually dishonest and politically immoral. It represents a crude apologia for deterrence clothed in the garb of value-free science. No sophisticated understanding of game theory is required to recognize this.

Worst of all, it trivializes the issue of nuclear war by reducing it to the level of statistical double-talk. Do Brams and Kilgour regard themselves as the intellectual heirs of the late and unlamented Herman Kahn? If so, they deserve no serious attention from those of us who look at the future of the human race as an issue not reducible to a "game."

It is articles such as this that have rendered political science a discipline un-

worthy of respect from those who are really concerned about "politics."

Michael Engel  
Westfield State College

## Brams and Kilgour Respond

We are saddened that Professor Engel chose not to pursue any intellectual issues that our short article, "Is Nuclear Deterrence Rational?" attempted to raise. Instead, we are accused of intellectual dishonesty and political immorality; we are lumped with Herman Kahn (not cited in our article), statistical double-talk (no statistics was used), and value-free science (our purpose was to explore the rational foundations of deterrence, not offer a disquisition on its ethics, though we make a number of policy recommendations for avoiding nuclear war in several of our research papers).

Guilt by association with the alleged bogeyman of political science—"junk empiricism" is also mentioned—is meretricious caricature and not a substitute for serious analysis. We wish Professor Engel had tried some analysis himself instead of casting off game theory, a deep and profound theory of interdependent decision making on which scores of books and thousands of articles have been written, as not worthy of his attention because it has "game" in its title.

Steven J. Brams  
New York University

D. Marc Kilgour  
Wilfrid Laurier University

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I would appreciate your listing this notice in the next issue or two of *PS*. With many thanks for your help,

David Wilsford  
University of California, San Diego

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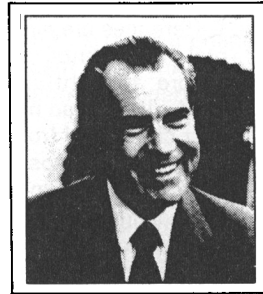
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