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More Citation Counting: Reply to Charles Cnudde

The note by Charles Cnudde (*PS*, Fall 1986, pp. 850-852) concerning multiple authorship is easily demonstrated to be false. Since he argues that Klingemann's methodology is questionable, it is probably worthwhile to point out that Cnudde's own methodology is flawed.

Cnudde argues that "We have no reason to believe that persons with names beginning with letters in the bottom half of the alphabet are less likely to join the discipline. . . than those whose names that begin with letters in the first half of the alphabet. He then proceeds to make a count of the 100 most-cited scholars listed on pp. 657-658, and finds that far fewer than half of them have names starting with letters in the second half of the alphabet. From this he concludes that the basic methodology is grossly inaccurate, and the results should be ". . . used with care, if at all."

Unfortunately, Cnudde himself has made a false assumption that invalidates his test. As was acknowledged in the original article, multiple authorship does pose a problem—but Cnudde's method of estimating how great the problem is, greatly exaggerates its size. It does so because his method is based on an assumption that is, quite simply, wrong: there is reason to believe that fewer than half of all political scientists have names starting with letters in the bottom half of the alphabet—in fact we know this to be the case.

As the article makes clear, the sample is based on all persons teaching in graduate departments of political science, as listed in the *APSA 1984 Guide to Graduate Study*. As anyone who consults this document can verify, this listing does not

show a 50:50 split between those with names beginning with letters A-M, and those with names that start with letters N-Z. Quite the contrary, 61% of those listed have names that start with letters A through M. Consequently, on a random basis we would expect to find roughly 61 of the 100 most cited scholars to fall into the top half of the alphabet (and not only 50, as Cnudde assumes). In fact, 70 persons do. Far from showing a bias that is significant at the .001 level, as Cnudde asserts is the case, this is roughly in the right ballpark: with a sample of only 100 persons, a deviation of this size is not significant at even the .1 level; given the small size of the subsample Cnudde has focused on, the deviation from random distribution could be entirely due to normal sampling error.

Using a much larger and more reliable sample, as was done in the original article—that is, using the entire universe of persons listed in the *APSA Guide to Graduate Study*, 1984—we find that those in the first half of the empirical distribution (which has a mid-point of LEH—and not between M and N, as Cnudde assumes) get approximately 52% of the total citations. Table 1 gives the detailed picture. As the article concluded, "There is some advantage, it seems, in having a name that appears early in the alphabet—but it's a marginal one" (Klingemann, 1986: 655).

But even this marginal advantage would not necessarily have any impact on the departmental rankings: in order to do so, it would have to be systematically biased against certain departments and in favor of others. It is not.

To get an optimal measure of the impact of given individuals, one might want to credit half the citations to each author if that were true. This would not always

increase the accuracy of the measure: quite often, the first author is listed first because he or she did most of the work and deserves most of the credit. But let us concede Cnudde the strongest possible case and assume that being listed first author always reflected alphabetical order, rather than senior authorship. This phenomenon would still not bias the rankings. Cnudde argues that some departments engage in multiple authorship more than other departments, and that this produces a systematic bias against this type of department. It does not: in order for this to be the case, certain departments must not only have disproportionate amounts of multiple

authorship, but their membership must also be disproportionately skewed toward the latter part of the alphabet. Otherwise, alphabetic listings of multiple-authors would have no effect: a given department would sometimes lose credit for a colleague who did half the work but had a name late in the alphabet. What they lose on Professor Zyxrqp they would gain on Professor Abcdef. If all the members of a department had names in the last half of the alphabet (and never engaged in co-authorship with each other) would the full 2% bias associated with multiple authorship work against the department? We know of no department that gets this pattern: in most cases the

TABLE 1
The Empirical Distribution of Alphabetically Ordered Last Names
and the Proportion of Lines of Citation, 1981-85

Last Names Starting with Letter:	Proportion of Last Names, Starting with Letter:	Proportion of Lines of Citation
A	3.01	4.05
B	7.97	8.48
C	6.88	6.01
D	4.53	5.15
E	2.05	2.90
F	4.03	4.10
G	4.69	4.42
H	6.88	7.31
I	0.36	0.50
J	2.41	2.56
K	5.36	4.66
LEH	1.83	2.27
	50.00	52.41
LEI	3.10	5.68
M	8.33	7.17
N	2.38	2.63
O	1.95	1.99
P	4.50	4.44
Q	0.01	0.08
R	6.45	5.52
S	10.85	9.18
T	3.27	2.33
U	0.33	0.42
V	0.99	1.69
W	6.15	5.18
X	—	—
Y	0.56	0.10
Z	0.89	1.05
Total N	3,022	21,175

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estimated bias would be well under 2%. In fact, Northern Arizona University, Lehigh University and the New School for Social Research would have had the most reason to complain.

If one holds our method of departmental rankings up against a standard of absolute perfection, there is no question that it falls short. There is no question that it is subject to error in measurement. And not only that type of error. By accident, the total for SUNY Stony Brook was omitted from Table 2 of the original article. We have to apologize for that mistake. According to their own calculation she should have been rated 50th, just ahead of SUNY-Buffalo. But perfection is an unrealistic standard. Every year, hundreds of students and faculty make choices about where they will study or where they will teach, on the basis of un-systematic, out of date and incomplete information. We do not claim that the approach used in our study is perfect. But we do believe that it comes much closer to reality than most alternatives.

In two other recent studies cited in the article, Harvard was ranked close as 13th or 14th place. We are confident that no conceivable correction for co-authorship would move Harvard down to 13th or 14th place; we would give 50% odds that it would not even move from first to second place. There are, however, specific cases of departments that have citation counts so closely clustered that virtually any change at all would change their ranking—but published figures make it clear that this is the case, and indicate just how much (or how little) change would be needed to bring about a shift in the rankings. We do not claim to have produced a perfect measure of scholarly impact. We do think we have come up with one that's more accurate, and more objective, than the alternatives we have seen to date.

Hans-Dieter Klingemann
Free University of Berlin

A Reply to Klingemann

There are two points of disagreement between Klingemann and me. On the

most important point we are in agreement.

The first point, that any alphabetical bias in citation counts will discriminate against departments whose faculty engage in collaborative research, Klingemann discounts. He says this discrimination won't happen unless surnames of faculty of such departments disproportionately are skewed toward the latter part of the alphabet. In making this claim he assumes implicitly that scholars never collaborate with others in the same department. It is easy to imagine that collaboration within a department will reduce a department's citation count compared to a department in which no such collaboration occurs.

We expect this outcome when a citation index counts only first authors even when the two departments produce the same number of articles. Only in the case where collaboration is exclusively outside a department will it be true that, as Klingemann argues, "what they lose on Professor Zyxrrq they would gain on Professor Abcdef." The point is that much of our collaborative research takes place among colleagues in the same department. Furthermore institutions often want to encourage this kind of collaboration. Administrative decisions using first-author citations would have the effect of discouraging collaboration of this sort, perhaps unintentionally, as long as there is any alphabetical bias in the citations.

This conclusion brings us to Klingemann's most intriguing observation. He argues that there is no such bias because the discipline is skewed toward the upper half of the alphabet. Similar skewness in the citation count exhibits representation rather than bias, he says. If we believed the discipline were skewed in this way we would have an interesting regularity to explain. Unfortunately, Klingemann does not test his count of the discipline against the null model to see if his observed skewness is statistically significant. A quick test of his data shows that they do not meet standard levels. Consequently it is probable that the skewness would disappear with the selection of other lists of the population in question; better lists for comparison with publica-

tion performance or even other editions of the same list.

Klingemann is correct in noting that we do not have perfect data. The point is that we need to be aware of possible bias and what the implications of bias can be. If the population is not significantly skewed and the citation count is, evaluators of citation counts need to take that information into account. The implications of bias are of particular importance when imperfect data serve as the basis of administrative evaluations of performance.

Charles F. Cnudde
University of Texas at Austin

Ranking Departments: Reply to Frank Way

In the Winter 1987 issue of *PS* (p. 7), Frank Way argued for a "stronger index" of the relative professional standings of different political science departments in the United States. Way proposed measuring departmental productivity on a per faculty member basis (total number of journal citations divided by total number of faculty members). Way then presented a table showing the relative standings of 72 different political science departments in the United States.

I agree with Way's opinion that if the professional accomplishments of departments are to be compared on the basis of total number of Social Science Citation Index listings, it is only fair that some attention be given to the size of each department. I am troubled, however, by Way's method of counting the number of permanent members in a given department. For example, according to Way, the University of Connecticut has 44 faculty members. But, in fact, the main

campus of the University of Connecticut has only 28 faculty members (31, if one includes three faculty members assigned to the University's regional campuses). Depending on the number of permanent faculty members one uses (44, 31, or 28), the national ranking of the University of Connecticut can vary from 66 to 37. The size of this variance has given me reasons to be wary about the overall reliability of Professor Way's table.

The public ranking of departments is not a minor concern for most departments. Departmental chairs utilize available rankings to help them to persuade University administrators of the recent accomplishments of their departments and to attract badly needed resources (financial aid for graduate students, secretarial staff, teaching positions) to the field of political science. Because of these practical consequences, perhaps personal attempts at national rankings of departments should not be published in the future without a full and considered discussion of possible methodological pitfalls.

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