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Letters

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 Zyxraq 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

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.

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