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FAX COVER SHEET

TRANSMITTED TO FAX NUMBER 212-652-5047Date: 7/12/99To: Eugen Tarnow

Phone number: _____

From: Ross Vasta

Office Phone Number: _____

Remarks:

Here is the editorial history
of the ms.

Good luck.

Number of sheets transmitted (including this cover sheet): 9



June 22, 1979

Charles A. Kiesler
American Psychological Association
1200 Seventeenth Street, N.W.
Washington, D.C. 20036

Dear Dr. Kiesler:

I wish to submit the enclosed manuscript entitled, "Author! Author!: The matter of publication credit", for publication in the American Psychologist.

Thank you for consideration.

Sincerely,

A handwritten signature in black ink, appearing to read "Ross Vasta".

Ross Vasta, Ph.D.
Associate Professor

American Psychologist

MARY BEYDA *Assistant to the Editor*
1200 Seventeenth Street, N.W.
Washington, D.C. 20036

10 July 1979

Dr. Ross Vasta
Department of Psychology
State University of New York
College at Brockport
Brockport, NY 14420

Dear Dr. Vasta:

Re: A-6-043-79

The manuscript that you recently submitted to the American Psychologist has been assigned to an associate editor, William Bevan, for handling. Any inquiries you have concerning your paper can be sent to him at the following address:

Dr. William Bevan
Department of Psychology
Duke University
Durham, North Carolina 27706

Please use the above reference number in any communication to the associate editor. You should be hearing from Dr. Bevan within two to three months.

Sincerely,


Mary Beyda

American Psychologist

WILLIAM BEVAN *Associate Editor*
Department of Psychology
Duke University
Durham, North Carolina 27706

July 19, 1979

Professor Ross Vasta
Department of Psychology
State University of New York
Brockport, New York 14420

Dear Professor Vasta:

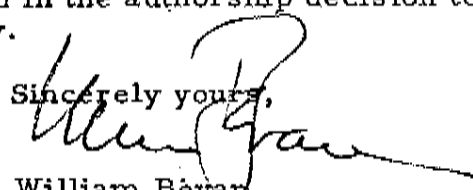
Re: A-6-043-79

I have read your manuscript, Author! Author!: The Matter of Publication Credit twice and I have reluctantly concluded that it is not suitable for publication in the American Psychologist. My reasons for this decision are several fold.

1. The American Psychologist is not a primary research journal and we are moving away from publishing the results of individual studies except in exceptional cases.
2. The manuscript is far too long and goes into too great detail to suit our format and purpose. Again, we are not a primary research journal.
3. I am skeptical that it provides information that is not already familiar to any but the newest recruit to the research ranks. Like sex before the seventies, the matter of how authorship is settled is little spoken about but widely understood in the community.
4. Joint authorship decisions like decisions about sex are more complex matters than they may appear on the surface and they do or should have a degree of intimacy about them. I am skeptical that they can be legislated or set by formula but must ultimately be negotiated by the parties involved. This is a view which you obviously share. I don't deny the existence of rape-real or psychic-and I think that the data of your item 3 is by far the most important of your paper. It warrants publicizing but I think can't be properly highlighted in your present mode of presentation.

I think the most immediately useful application of your data would be to send the results of Item 3 to the committee on Professional Ethics of the APA urging that they give more attention to a serious problem. You also might want to prepare a paper that is less prescriptive in its approach and that is more directly focused upon the behavior involved in the authorship decision to be submitted to a journal in social psychology.

Sincerely yours,


William Bevan

cc: Ms. Mary Beyda



July 24, 1979

Charles A. Kiesler
American Psychological Association
1200 Seventeenth St., N.W.
Washington, D.C. 20036

Dear Dr. Kiesler:

The enclosed article was recently rejected for publication in the American Psychologist (letter of rejection attached). With all due respect to Dr. Bevan's comments, I cannot agree that the content of the article is "... already familiar to any but the newest recruit to the research ranks." On the contrary, I feel strongly (and I hope you will agree) that this information is potentially very valuable and should be disseminated to the APA audience.

If you would be willing to reserve a final rejection decision, I would be happy to resubmit the article in revised form by (a) eliminating the prescriptive aspects of the article, and (b) reducing the overall length of the manuscript to one that you suggest.

Thank you for your additional time and consideration.

Sincerely,

Ross Vasta, Ph.D.
Associate Professor

American Psychologist

CHARLES A. KIESLER *Editor*
1200 Seventeenth Street, N.W.
Washington, D.C. 20036

28 August 1979

Dr. Ross Vasta
Department of Psychology
State University of New York
College at Brockport
Brockport, NY 14420

Dear Dr. Vasta:

Re: A-6-043-79

At your request, and with Dr. Bevan's agreement, I have sent your manuscript, "Author! Author!: The Matter of Publication Credit," out for review. I rarely take on such re-reviews of my associate editors' decisions, but since Dr. Bevan did not send your manuscript out for review, I decided to do so in this instance.

Since this is a difficult time to pin down reviewers, it may be a month or two before I have a decision for you. I will do my best to speed up the process.

Sincerely yours,


Charles A. Kiesler
APA Executive Officer

CAK:mb

American Psychologist

CHARLES A. KIESLER *Editor*
1200 Seventeenth Street, N.W.
Washington, D.C. 20036

16 October 1979

Dr. Ross Vasta
Department of Psychology
State University of New York
College at Brockport
Brockport, NY 14420

Dear Dr. Vasta:

Re: A-6-043-79

We have completed our review of your manuscript, "Author! Author! The Matter of Publication Credit," submitted to the American Psychologist. Copies of the comments of two reviewers are enclosed for your information.

As you can see, both reviewers recommend against acceptance. I agree with both reviewers and must therefore tell you that we cannot accept the paper for publication in the American Psychologist.

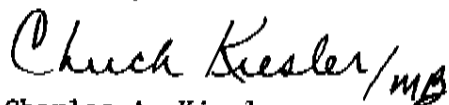
The survey is really rather preliminary in nature. The number of written-in answers illustrates that point, and their content shows how the survey instrument could have been modified to improve it.

Of course, the study is not without merit. However, the press for space in the AP is such that we are able to accommodate only about 10% of the manuscript submitted to us for review. Thus, many otherwise meritorious manuscript simply cannot be accepted.

If you decide to try to publish the paper elsewhere, the comments of the reviewers should be very helpful in the process of revision. Perhaps JSAS or Professional Psychology would be appropriate outlets.

Thank you for letting us see the manuscript.

Sincerely,

Handwritten signature of Charles A. Kiesler in cursive, with the initials 'mb' written at the end.

Charles A. Kiesler
Editor, American Psychologist

CAK:mb

Enclosures

OCT 4 1979

September 26, 1979

American Psychologist Review of manuscript (A-6-043-79)--received Sept 6

First, I believe the effort this author gave this issue should be commended. The detailed analysis of the data is particularly noteworthy. Second, I do not, however, believe this paper is worthy of AP publication as an article for the reasons that follow. It could be considered as a "comment". Third, despite the author's statement (page 1) about "the question was eventually resolved amicably," it seems to me that the design and interpretation both suggest an unresolved element which colors the report.

According to the author, the Casebook says "it is not expected that a person be considered joint author unless he had an appreciable part in...a) the design, b) data gathering, c) writing, or d) the thinking that went into e) the study, or f) the report of it." The study seems to me to as much suggest that many psychologists (as well as the author) oppose these principles and/or do not practice them -- particularly younger and less productive individuals. In a similar vein, let me comment that The American Heritage Dictionary (closest at hand) defines "substantive" as: "independent in...function, ...real, ...essential, of substantial amount..." and, as such, I have difficulty with the use of the term "nonsubstantive" on page 8, line 15; page 9, line 20, and implied in the top of page 19 -- this is particularly in relation to the role and/or importance of data collection, as the Casebook defines this as part of what is "appreciable" and the author presents that it is the second rank factor in determining ranking of authors (actually that time and responsibility are)).

More importantly, the "conclusions/guidelines" are hardly more usable than the ethics/casebook wording. Number 1 discusses "major scientific contribution" but ignores the data on "time devoted" and the Casebook standards. The second half of it is conditional and leaves the matter to judgment. Number three is based only on "average" rank and ignores the percentage data, with the sentence "other factors should play lesser roles in the decisions" reopening the non-specific. Number four is a weak conclusion (I noted a stronger "recommendation" in the margin). Number five is a non-statement. I do not think that conclusion six fits at all (it is a different type of conclusion than the first five). The paragraph after "six" seems to argue against rationality from a normative view. The next paragraph is ~~even~~ more meaningless to me. Hence, the conclusions and/or recommendations contribute little or nothing to clearing things up. (Maybe the normative practices and recommendations should be separated.)

There are several places where, I feel, the author overstates his/her conclusions of the data; several of these have been noted in the margin. The author seems to not weight/consider the spontaneously given (written in) answers enough. On structured surveys, written in answer requires thinking/concern/motivation. When there are enough to form pools it often means an important option was left out and the idea is probably unrepresented. The data on question 7 (bottom of page 10) and question 11 (bottom of page 12) are examples.

The most interesting relationship is between "giving credit" and the "nature of credit given" and its relation to age and productivity. It is not concerned in any major way (e.g. a wrapping up discussion).

A major rewrite/reorganization is needed for publication in any form. The emotional investment and language bias must be taken out. I could not support this as a major AP article; I could support a rewritten form as a "comment".

I can not recommend the publication of manuscript A-6-043-79 in the American Psychologist.

Although the title creates the impression that this will be an interesting report, I doubt that many subscribers will read their way through it. It is long and not tightly written. The report suffers from other problems as well. We are not told how names were "randomly" chosen from the directory or why the sample was stratified as it was. A more serious defect, however, is the complete lack of any attempt to tie this report to any of the literature on scientific communication (Merton, Crane, and others) despite the use of references to phenomena described in this literature, including noblesse oblige and the Matthew effect.

Finally, it is not clear how one can create guidelines out of responses to a survey such as the one reported here.

If this report is ever published, I do hope that all 303 respondents to the survey will be listed as co-authors with their names anagramatized and listed in order of distance of their residence from Peoria.