
COMMENTARY

Too Many References, Just Cut a Few and It Will Be Perfect: APA vs. Chicago

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Emperor Joseph II: An excellent effort. You have shown us something . . . quite new tonight. It's just that, occasionally it seems to have . . . How would you say, director?
Court music director: Too many notes, your Majesty.
Emperor Joseph II: Exactly! Very well put! Too many notes!
Mozart: I don't understand. There are just as many notes, Majesty, as I require, neither more nor less.
. . .
Emperor Joseph II: Your work is ingenious. It's quality work. And there are simply too many notes, that's all. Just cut a few and it will be perfect.
Mozart: Which few did you have in mind, Majesty?

— *Amadeus* (1984)

The purpose of my rebuttal is to provide a sociocultural critique of Wolff-Michael Roth and Michael Cole's arguments and of the current *Mind, Culture, and Activity* (*MCA*) policy regarding referencing. In sum, Roth and Cole write that some unnamed *MCA* authors (and some scholars in general) abuse the reference practice by name-dropping and political networking, which reflects "a growing tendency to excessive and inappropriate citation" (p. 93). The current new *MCA* editorial referencing policy, which emerged out of these alleged practices, can be characterized as a procedure of minimizing references. My main rebuttal is that the *MCA* editors' reasoning and policy about referencing is based on a positivistic paradigm.

Let me start with a personal anecdote. When I was a postdoc, a colleague of mine with a strong information-processing background in psychology looked over a draft of my paper and made the following comment that was very surprising to me at that moment. My colleague

criticized me for citing research older than three years. He argued that science has a cumulative nature and, thus, we scholars should focus only on the latest research. According to him, tracing the history of ideas is a matter for the history of science but not for science itself. He acknowledged that there can be some legitimate exceptions to his three-year rule—the rule he learned from his advisor in a graduate school—but they would be rare exceptions that have to be explicitly justified by the researcher. Versed well in contextualist literature on science practice, I judged my colleague’s approach to referencing as positivistic, rooted in several erroneous assumptions:

1. The cumulative nature of the science practice;
2. That scientific practice is the pure development of ideas (any contamination by social relations of power is bad and has to be cleaned out);
3. Facts preexist research out there and have to be discovered by scientists (no social construction—truths and ideas are impersonal, eternal, and nonauthorial);
4. Proceduralism: that social life can be rule-governed.

Given his background in an information-processing psychology paradigm, my colleague did not have trouble accepting all these assumptions and my charge that he was a positivist.

I doubt that the *MCA* editors would so easily accept that they are positivists or that their reasoning and policy regarding reference practice is positivist, but this is exactly what I see in their article and *MCA* editorial policy. I do not assume that Wolff-Michael Roth and Michael Cole are positivists like my colleague. But I argue that positivism “acquired them” as they dealt with the issue of potentially parasitic referencing practice and in their adoption of an editorial policy aiming at combating this parasitic practice.

DECONSTRUCTION OF THE *MCA* EDITORS’ POSITIVISM: HISTORY AND SOCIOLOGY OF REFERENCING

In their article, the *MCA* editors provide three observed purposes of references and citations in scholarly work in social sciences—two of them are legitimate (a and b, see below) and one is illegitimate (c), from the *MCA* editors’ point of view:

Our observations have led us to question whether citations are about (a) citing (and referencing) the authors of ideas to be discussed and developed to give credit where credit is due and to provide roadmaps to the relevant literature for those concerned, (b) about having one’s own ideas cited (and referenced) for the same purposes, or (c) sighting and being sighted within what come to be established as communities and schools for purposes that are extraneous to the process of knowledge building. As editors of *Mind, Culture, and Activity*, our goal is to support the development of (new) ideas/theories. Although we are interested in the development of community, we are not interested in supporting referencing practices that in some cases appear to amount to no more than name-dropping and articulating acquaintance with other scholars in a manner that fails to develop the ideas being discussed.¹

¹Wolff-Michael Roth and Michael Cole, “The Referencing Practices of *Mind, Culture, and Activity*: On Citing (Sighting?) and Being Cited (Sighted?),” *Mind, Culture and Activity* 17, no. 2 (2010): 93. It is interesting that the *MCA* editors acknowledged the communal and institutional purposes of referencing as a legitimate part of the science practice but they did not discuss in their editorial the appropriate use of referencing for this purpose and its boundaries.

Despite the *MCA* editors' claims to provide "referencing in a cultural-historical context," in my judgment, they did not do that but instead provided rather keen sociological observations that reference practice can contribute to scholars' professional survival and institutional (and financial) promotions that at times can interfere with primary academic goals of the science practice such as searching for truth and providing intellectual investigations. I do not want to claim that the history of referencing practice in science is my field or that I am very knowledgeable in that, but from what I know the modern referencing in social sciences has emerged in the fields of what can be called philology and history (and before that in theological commentaries on sacred texts²) and then it served the role of modern hypertext in a form of footnoting.³ In his historical analysis of footnote practices,⁴ Grafton argues that rapidly emerging footnote practices served diverse and complex purposes and functions for the scholars and scientific communities such as for establishing validity of the claims, providing a methodology for working with data, providing sources and ways of working with them, building credibility and reputation for the author, engaging in debates, providing support and rebuttal, establishing proprietary claims on ideas and discoveries, building coalitions and oppositions among colleagues (what can be called also paradigms), securing institutional and political support for themselves and their coalitions, attacking institutional and undermining political support of their opponents, making personal attacks, validations of allies, and self-elevation and self-promotion (if not self-glorification), humility, civility, respect, and polity, academic ambition, mission, and vanity, and so on. As Grafton noticed, by the end of eighteenth to the beginning of nineteenth centuries, the space of footnotes became comparable if, at times, not larger than the main text of the scholarship, and its significance grew tremendously, often overshadowing the main text. Using a Bakhtinian framework,⁵ it is possible to characterize the main text of this philology-history scholarship as a finalized summary of findings while seeing footnoting as dialogic drama of ideas, paradigms, academic camps, and ambitions.

I hypothesize that the natural and social sciences have differently appropriated the historical practice of footnoting both as an ideology (an espoused theory) and as practice-in-action. I speculate that ideologically, positivistically oriented natural sciences have two contradictory tendencies of (1) eliminating any authorship of ideas—the truth does not have authors; and (2) claiming proprietary ownership for their ideas in a form of patents, worship, copyright, reputation, awards, grants, and immortality (i.e., a permanent place in the history). The practice-in-action of natural sciences has been keenly described and analyzed by Latour⁶ arguing that referencing serves to change modalities of the scientists' statements by elevating some into facts and diminishing

²For example, consider rabbis' interpretative marginal notes on the Talmud and the Torah.

³Anthony Grafton, *The Footnote: A Curious History* (Cambridge, MA: Harvard University Press, 1997). This function of footnotes as hypertext is acknowledged in the Chicago style manual, University of Chicago Press, "The Chicago Manual of Style Online" (Chicago: University of Chicago Press, 2006), <http://www.chicagomanualofstyle.org/home.html>.

⁴And I would also call it "genealogical analysis" after Foucault.

⁵Mikhail M. Bakhtin, *Problems of Dostoevsky's Poetics*, vol. 8, *Theory and History of Literature* (Minneapolis: University of Minnesota Press, 1999); Eugene Matusov, *Journey into Dialogic Pedagogy* (Hauppauge, NY: Nova Science Publishers, 2009). Although, the *MCA* editors employed Bakhtin's notion of dialogism to describe the science practice in their article, in my view, they missed an important point that the scholars' ideas have authorial and personal nature rather than just contribute in an impersonal process of accumulation of facts.

⁶Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Cambridge, MA: Harvard University Press, 1987).

others into illusory artifacts (i.e., fallacies). It is interesting and revealing that the *MCA* editors also applied Latour's description of natural sciences⁷ without even questioning appropriateness of this analysis for social sciences⁸ (or at least for some nonpositivistic fields and paradigms): "In the social studies of sciences, the attribution of ideas has been shown to be an important aspect of the 'hardness' of a fact."⁹ The main functions of referencing in positivistically oriented natural sciences is to make an abbreviation of previous research, crediting contributions, disagreeing, and aligning troops for paradigmatic wars, so to speak. Established facts, unanimously recognized by the relevant scientific community, become de-authored and any reference is dropped. The *MCA* editors worry that some scholars resist this positivistic trend and still refer and cite the original scholars: "In many instances, authors cite one or, worse, more sources even when the statement in which the citation occurs has become common knowledge and constitutes a truism."¹⁰ In my view, "truism" is cleansing statements out of their authorship, responsibility, agency, will, political management of relations, emotions, and personal evaluative intonation—it is an idea that truth can exist without people.

In the social sciences, the situation seems to be both similar to and different from that which obtains in the positivistically oriented natural sciences and paradigms. Some areas and mainstream paradigms within them try to follow the image of natural "hard" sciences. Mainstream psychology is one of them. It has designed the famous APA style of referencing to promote positivistic goals of referencing, so well articulated by the *MCA* editors.¹¹ The APA style tries to minimize footnotes and sees references as a necessary evil (almost as waste of paper) that has to be controlled and disciplined, cleaning it from any impurities distracting from establishment of the truth.¹² Discipline requires procedures. For example, in a few hours (sic!) after the

⁷Latour and Woolgar studied a biology lab in California, Bruno Latour and Steve Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (Beverly Hills, CA: Sage Publications, 1979).

⁸It is also possible to raise this issue for natural science, but this is another issue.

⁹Roth and Cole, "The Referencing Practices of Mind, Culture, and Activity: On Citing (Sighting?) and Being Cited (Sighted?)," 95.

¹⁰*Ibid.*: 97.

¹¹Points a and b, *Ibid.*: 92., but also arguably some others not mentioned by the *MCA* editors. I wonder how *MCA* came to a decision of using the APA style for their journal—how much they considered negative consequences of this decision in time when the journal was started. In the history of immediate practice Chicago was associated with history and APA with, of course, psychology and "science." Hence, the decision of most of the discipline of education to move to APA was seen as a move to ally itself with "real" science (St. Julien, personal communication, May 2010).

¹²"Because they can be distracting to readers, such footnotes should be included only if they strengthen the discussion. A content footnote should convey just one idea; if you find yourself creating paragraphs or displaying equations as you are writing a footnote, then the main text or an appendix probably would be a more suitable place to present your information. Another alternative is to indicate in a short footnote that the material is available online as supplemental material. In most cases, an author integrates an article best by presenting important information in the text, not in a footnote," American Psychological Association., *Publication Manual of the American Psychological Association*, 6th ed. (Washington, DC: American Psychological Association, 2010), 37–38. Please compare this statement with "footnote virtue" as articulated by the Chicago style manual: "Readers of scholarly works usually prefer footnotes for ease of reference. Where the notes are closely integrated into the text and make interesting reading, they belong at the foot of the page. They also belong there if immediate knowledge of the sources is essential to readers," University of Chicago Press, "The Chicago Manual of Style Online." To be fair the Chicago style manual also warns about excessive use of footnotes, but it is much more appreciative of them. Also, it is interesting and revealing that the APA style manual is intellectual commodity for sale (see Amazon.com) while the Chicago style manual is available for a free 30-day trial on their website, <http://www.chicagomanualofstyle.org>.

submission of my manuscript to *MCA* I got the following e-mail from the editor: “As you prepare your manuscript, please attend to the editorial policy concerning referencing, as per editorial that Mike Cole and I published in the second issue of this year. This means that your reference list will likely come down to one-half if not one-third of its current length . . . but it may not. In any event, as it currently is, the paper employs referencing practices that are inconsistent with the editorial policy, and I would like you to address those, too.”¹³ Although the editor provides a loophole for himself by the use of his wording “likely” and ambiguous “. . . but it may not”; his application of the “editorial policy” is apparently mechanical, probably based on the ratio of the pages of the main text and the pages with the references.

In contrast, anthropology, history, and sociology have developed the Chicago style¹⁴ of referencing closely approximating footnoting practice in philology and history. Often, the footnote includes not just references about where to find the source but also the author’s evaluation and explication of the connection of this source. I speculatively argue that in these fields, like in philology, the science practice is seen as essentially authorial and hermeneutic—truths have authorships, histories, localities, cultures, personalities, responsibilities, and names. Despite all progress in science, references to Plato, Aristotle, Marx, Freud, and so on, will remain not only because they are very important contributors but because ideas are authorial.¹⁵ Yes, footnotes take space and paper, but in my view they are worth it.¹⁶

DOES NAME-DROPPING REALLY EXIST? IF SO, WHY? IS IT A PROBLEM?

The *MCA* editors claim that there is an increasing tendency of name-dropping and abuse of the referencing practice among *MCA* (potential?) authors and elsewhere in social sciences. I am both (1) not sure that it is true and (2) not surprised, if it is true. The *MCA* authors provide structural evidence that this phenomenon of name-dropping exists. They pointed to the length of the reference lists (as in the case of the manuscript that I submitted to *MCA*), and in their editorial, they provided seven examples of “bad practice” referencing. But this rather interesting and thought-provoking analysis presented by the *MCA* editors is not convincing for me. Based on the Bakhtinian framework, to which I subscribe, I argue that observations of other people’s behaviors can generate plausible hypotheses requiring further testing but are never definitive evidence about the people’s subjectivities and meaning of their actions.¹⁷

¹³*MCA* editor, personal e-mail communication, May 8, 2010.

¹⁴I asked the *MCA* editors to allow me using the Chicago style in this article to demonstrate for the readers what it is about and its strengths. Readers can contrast and compare the Chicago and APA styles.

¹⁵For more discussion of this claim, see Bakhtin, *Problems of Dostoevsky’s Poetics*.

¹⁶The material, financial, and environmental costs of footnoting and hypertext may become moot in the future. I expect that with the development of paperless digital publications and improved e-reader devices there might be further experimentation with nonlinear hypertext formatting.

¹⁷See my empirical work about preservice and inservice teachers where my colleague and I investigated the danger of such excessive finalizing and objectivizing that teachers do about their students and the *MCA* editors did about authors, Eugene Matusov and Mark P. Smith, “Teaching Imaginary Children: University Students’ Narratives About Their Latino Practicum Children,” *Teaching and Teacher Education* 23, no. 5 (2007).

Let me consider and closely interrogate just one of their examples (I tried to select the most “obviously bad” one!):

Another unfortunate citation practice is to provide long lists of references where it is not quite clear what precisely is being referenced and in which ways the various authors or works are similar and in which ways they might differ. Take the examples provided in Examples 2 to 4.

EXAMPLE 2

In her publications (1989, 1992, 1995, 1996, 1996b, 2000, 2001, 2001b, 2002), the author provides rich and concrete examples of the projects under discussion. The projects (which could last for up to a year) are always based on a theme important in children’s lives.

...

A reader might ask, what is the function of the referencing in these examples? Example 2 would quite as well function without any, or at most one or two of the references. In terms of “hardness of fact,” there is no need or interest to increase the number of references to the same author. This is so in particular because neither the statement nor the text that follows addresses any one of the publications cited. The statement is broad and the specific nature of the “concrete examples” is neither presented nor discussed. One may ask in particular how different publications from the same year are; did the author so rapidly change his or her approach to the processes at work in the project? The reader may be interested in knowing what precisely the differences are that warrant different citations. Or the reader may simply take the list as a statement about the prolific nature of this author work on the project in question. In all events, the purposes of such extensive citation are left unclear.¹⁸

The *MCA* editors’ reasoning sounds compelling except it can be very wrong. What if the author considers the longitudinal research trajectory of the female scholar and that showing such provides basis of the author’s analysis?! If this is the case, the extensive referencing is legitimate. There are also other possibilities for legitimate extensive referencing like, for example, research referencing less known scholars, whose work is gaining new significance after it might have been forgotten, or not even known. Based on the positivist assumptions about the structure and functions of referencing, the *MCA* editors jump to their conclusion that this practice is bad. However, from the fact that “in all events, the purposes of such extensive citation are left unclear” does not necessary mean that the reference practice is bad or inappropriate or abusive. In my view, instead of the *MCA* editors automatically asking the author to cut references, as is now the policy, why don’t the *MCA* editors ask the author (or a reviewer) to justify the particular referencing as a part of the review process? If the author can successfully justify it, than the practice is scholarly legitimate, DESPITE whatever structure and genre it is using.

Let me reiterate my point. I do not challenge the *MCA* editors’ gut feeling that there might be some change in ways how *MCA* potential authors use references. I do not challenge that the *MCA* editors have legitimate and plausible suspicion about a possibility for a parasitic reference practice. They do. However, I argue that by observing a particular structure of referencing it is impossible to prove that it is a case of parasitic referencing. The issue is not that the *MCA* editors used imaginary examples and not real (I agree with the authors that it would have been

¹⁸Roth and Cole, “The Referencing Practices of Mind, Culture, and Activity: On Citing (Sighting?) and Being Cited (Sighted?),” 97-98.

unethical to use real examples). The issue for me (and Bakhtin¹⁹) is that observations about other people's behavior (e.g., text) alone, without addressing the actor's subjectivity, can never be sufficient to draw conclusive judgment about the meaning of that behavior. Observations on people's behavior generate plausible hypotheses and suspicions, which can help to initiate dialogue. If changes in the structure of referencing practice really exist as the *MCA* editors claim (systematic research is called for), it may be driven by many other possible reasons, including gradual shifting paradigms in psychology and other social sciences to become less positivistic and a growing tension with the APA style of referencing, in addition or in spite of the editors' explanations.

On the other hand, the parasitic referencing aiming primarily at masking the author's lack of grounded work and/or at excessive networking exclusively to raise citation indices can take place despite the fact that the *MCA* editors did not provide convincing evidence of it. There have been changes in academic institutions relying more on mechanical citation indices than on judgments of peer experts (i.e., external and internal reviewers) in promotion and hiring decision making.²⁰ These new oppressive (in my view) practices put pressure on junior and even seasoned scholars to publish in higher Impact Factor journals and to increase mutual referencing to promote themselves and their colleagues. In their own turn, academic journals try to brag about their high or increasing Impact Factors and "tough" editorial policies to attract more authors to submit their papers to the journal.²¹ For example, in February 2010, I got an e-mail from the Elsevier publishing house informing about the Impact Factors for their academic journals in the field of education and proudly announcing that *Economics of Education Review* has increased its Impact Factor from 0.557 in 2007 to 0.804 in 2008—mazal tov! There should be a systematic research program to investigate how widespread these oppressive policies of using journals' Impact Factor for hiring and promotion in the U.S. academia are (as far as I know they have a huge widespread in Asia and the European Union) and if they indeed promote parasitic referencing practices among scholars.

On the other hand, social networking is not necessarily bad when it is done moderately. Science practice is never only about ascetic development of ideas. Furthermore, purification of science can be very harmful and oppressive. Professional and institutional survival, management of author's reputation and credibility, attacking opponents, strengthening allies, even academic vanity are important and legitimate concerns in developing the academic genre.

Also important for emerging scholars and scholars who came from other fields and want to develop a new expertise, being apprenticed into the scholarly community in the particular field, dialogic referencing provides guidance for their path in many important ways: It (1) can provide a set of names for whom a certain field of interest is important, (2) provides information on who

¹⁹Bakhtin insisted that in contrast to "the exact sciences," social sciences and humanities deal with consciousness that has to be addressed to access its subjectivity. "The exact sciences constitute a monologic form of knowledge: the intellect contemplates a thing and expounds upon it. There is only one subject here—cognizing (contemplating) and speaking (expounding). In opposition to the subject there is only a voiceless thing. Any object of knowledge (including man) can be perceived and cognized as a thing. But a subject as such cannot be perceived and studied as a thing, for as a subject it cannot, while remaining a subject, become voiceless, and, consequently, cognition of it can only be dialogic," Mikhail M. Bakhtin, *Speech Genres and Other Late Essays* (Austin: University of Texas Press, 1986), 161.

²⁰See XMCA discussion of the Impact Factor and its negative influence on academic journals in July 2008.

²¹This sadly affects even *MCA*, see the editor's XMCA email posting on July 4, 2008, http://lchc.ucsd.edu/MCA/Mail/xmcamail.2008_07.dir/0027.html.

stands out in the field, (3) lets them know who attends to whom, (4) shows what fields and or persons are not often included, (5) lets them know who can be their narrow audience of support, and (6) defines their paradigmatic and dialogic opposition within and outside of the field.

CODA: NEGATIVE CONSEQUENCES OF THE MCA CURRENT PROCEDURALISM

Finally, I want to discuss negative consequences of the *MCA* current procedural editorial policy to limit the number of references in papers submitted to *MCA*. I see at least two problems with the *MCA* procedural editorial policy limiting the number of references. First, it handicaps certain nonpositivistic paradigms and scholarships promoting historicism and dialogic investigations. In these paradigms, truths and facts are authorial and historical. Common sense and nature do not erase voices and responsibilities for statements. Second, it distorts voices and histories and washes out important nuances and tensions. For example, there is a growing tendency I noticed in our “the *MCA* field” that everything that Vygotsky mentioned gradually becomes attributed to Vygotsky—it does not matter that it might be contributions made by, for example, German Gestalt psychologists like Kohler or Lewin or by Hegel or by Marx. Similarly any achievement in the Soviet Activity Theory becomes automatically attributed to Leont’ev. It leads to social construction of “historical geniuses,” “classics,” “Mozarts in psychology.” Secondary, tertiary, quaternary more current sources replace the primary sources in zeal to reduce the number of references.

MY RECOMMENDATIONS

I think that although the *MCA* editors do not provide convincing evidence of parasitic referencing practices, their worries rooted in the changes of the institutional academic practices of hiring and promotion—where peer judgments of the quality of a candidate’s scholarship are replaced or weakened by the citation indices²²—can be real and valid. A systematic study of this possible phenomenon is needed.

Meanwhile I suggest that the *MCA* editors:

1. move away from their positivistic assumptions about science and the *MCA* academic community—that the legitimate reasons for citation are only in development of (impersonal) ideas and (making) reflecting consensus in the relevant scientific community;
2. stop the current editorial practice of proceduralism—the mechanical limitation of the number of references in submitted papers;
3. switch from the APA style of referencing to the Chicago style of footnoting. The Chicago style is more inclusive and allows scholarship with diverse paradigms not to compromise with their methodologies—it allows to have near APA style if the author wants it;
4. the *MCA* editor should presume the innocence of the author and ask the author to explain and justify his or her practice of what appears to be parasitic referencing on case-to-case

²²And/or when “legitimate” academic journals that are counted for promotion are those with the high Impact Factor.

basis. The Chicago style helps to clarify diverse legitimate referencing practices by the authors.

The goal of my rebuttal is a friendly call to the *MCA* editors to reconsider their approach to the possible problem of parasitic referencing practice and their *MCA* policy aiming at addressing this problem. I argue here that their approach and policy has been unfortunately hijacked by positivism. Instead, I suggest that their approach and journal policy should be informed by contextual judgment, dialogism, and sociocultural historicism that I describe here.

What do you think?

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