

Critiquing the Critique: A Response to William H. Meredith

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Professor Meredith and his associates have made important contributions to the study of the history of family science. Meredith's published reports, along with those by Burr and associates, provide important information in an accumulating sociology of knowledge regarding family science. In Meredith's work on the study of family science, measurement of productivity has been focused on specific journal publications. This work, by nature, is exclusionary and based on a personal preference for a given series of publication outlets. The technique recognizes publications without an estimate of impact. In Burr and associate's work, reputational data are obtained using an available, non-random sample. This technique is limited by subjective measurement and the sampling strategy employed. In contrast, the use of *Social Science Citation Index* (SSCI) provides a researcher with a much broader (but likewise not fully representative) journal source that offers the potential for examining productivity (frequency) and impact (citation history). I will not labor further on such advantages, given our methodology is detailed in both the original and revised manuscripts that are published in *Family Science Review* (FSR). However, one's choice of methodology should be based on interests, objectives, and methodological focus.

In response to Meredith, every scientist and every methodology should have its critic. Meredith has been insightful in his concerns about our methodology, data, and analysis. On several of his concerns we are in agreement. On others we disagree.

He is correct in stating that it is very difficult to determine publication credits for individuals with common names; particularly for those individuals who publish without middle names or initials. Within our stated methodology we have attempted to reduce this problem in the revision published in this issue. Therefore, I believe we have addressed this criticism in our second paper in an open and forthright manner. We agree that one should identify the location of the school (department, university, etc.) and be reasonably certain that the title fits the interest of the scholar. One would think that this would be easy criteria to fulfill. Wrong! Faculty members move around, take short-term positions, go on leave, and even occasionally take sabbaticals. They frequently credit the institution where the work was completed at the time of the study or scholarly undertaking. Some hold joint appointments within and across institutions. At times they credit one location, at other times they credit another. Likewise, considerable confusion can emerge from titles themselves. Family science faculty have very wide disciplinary training. Many have multiple interests and publish in numerous disciplines. To use myself as an example, I have publications in education, family relations, psychology, psychiatry, sociology, business, law, medicine, biology, child development, and other allied social science disciplines. While my interests are varied, I am not as unusual as one might initially think. Many faculty publish in more than one discipline, at a given time or over time. This makes it very difficult to determine

publication credits "absolutely." Therefore, the best alternative is to obtain an updated resume from individuals--if one is interested in an absolute obtrusive versus an estimated unobtrusive methodology. Our interest, however, has been in exploring an unobtrusive method for the study of eminence. Further, the use of specific journal outlets as a source base easily would miss the breadth of faculty productivity in family science. There are risks, however. One may over-credit a given faculty member's productivity as Meredith correctly has observed. The dilemma one faces then is to under credit by selecting a few journals or risk over-crediting by using a more general data source.

Let me now turn from general concerns and reflections about Meredith's critique and take specific exceptions. We clearly have recognized in our re-examination of the data from SSCI that we are constricted by certain limitations. Such considerations are detailed in the manuscript in this issue of FSR. We maintain our reanalysis is a reasonable (although not absolute) count based on the technique employed. Anyone can come along and use a different standard, criteria, or technique and challenge a data base. Individual data points, in isolation from the full 1200 faculty records, can be equally misrepresentative. While recognizing the limitation of our estimates, to challenge our data and the conclusions we draw would require one to make 1200 observations on 6 criteria (7200 individual data points).

Let me illustrate. Table 3 is based on a combined score across multiple indicators with a rank order weighting. To criticize this score would require the full 7200 data points, placed in rank order, with an application of a weighting system. One would have to be extremely insightful to challenge the results in this table without the establishment of the full data set. For example, a given faculty member can be very high on two or three criteria and low on others. If the ranking on these two or three criteria is high enough, that individual can make the list in Table 3. Indeed, many individuals on that list were extremely high on two or three criteria only, but the ranking resulted in their being recognized using the criteria we have selected.

To conclude, I am in agreement with Meredith's concerns about precision and use of this technique. Meredith's critique, our revised analyses, and this reply reflect such concern. Nonetheless, from the beginning we state our data are estimates only. (No less could be said about Meredith's estimates of productivity based on a select journal source.) Because of our concerns about possible misuse of our findings, we also used a historical time period, believing that no professional would make major substantive or programmatic decisions upon an historical data set. Perhaps we have erred by not using a more distant time period. Our interests, from the beginning, have been to construct a technique that can supplement those suggested by Meredith, Burr, and their associates. Based on criticisms by Meredith, we completed a recount that resulted in corrections in our findings (the article in this issue). We have been open to any and all concerns about this technique and have recognized them in our first and/or second publication. To continue further in this debate would be superfluous.