

NURSING RESEARCH REVIEW FORM

1st Review

Manuscript # 2006/065
Manuscript title: Anesthesia staffing and anesthetic complications during Cesarean delivery: A retrospective analysis
Manuscript type: Regular
Number of text pgs.: 16
Number of figures: 0
Number of tables: 4
Reviewer: Susan Fetzer
Stat reviewer assigned?: No -- do you recommend a stat reviewer?

Please evaluate the following with these choices: (1) adequate, (2) inadequate (describe in written review) or (3) not appropriate (describe in written review)

Problem statement: 2
Attention to relevant literature: 2
Theoretical framework: 2
Research design: 2
Data analysis: 1
Discussion of results: 2
Organization: 2
Writing style: 1

Please rate the following topics 1-5 (with 5 being the highest rating):

Value of topic: 4
Probable reader interest in topic: 4
Importance of present contribution to nursing: 4
Priority of topic for publication: 3
Rank this manuscript for its value: 2

Reviewer's Recommendation (please type "X" after your choice):

Accept without revisions
Accept with revisions
Maybe accept with revisions XXX
Do not accept

Comments for Editor only:

**Please provide a comprehensive and integrated review of this manuscript.
Be sure to present a balanced view of the manuscript's strengths and weaknesses.**

The outcome evaluation of advanced practice nurses, such as nurse anesthetists (CRNAs), is a timely topic. The medical community has targeted CRNA practice for medical supervision through the use of the legislative process. Therefore, research that supports independent practice with comparative outcomes is needed.

However, the problem necessitating this retrospective analysis is not clearly delineated in the introduction. It is not until P6L4-13 does the reader become introduced to the implications of the study. P4-5 seem to be focused on a conclusion as to what the “study contributes to the literature” [P4L15], but not what problem the study seeks to solve. On P5L3, the author attempts to link CRNA staffing with research by Needleman et al. that has been focused on direct care providers. This research does not attempt to evaluate CRNA ratios, so the use of the Needleman paper to support the focus of the study seems forced. P 5 L12 to L3 describes the methodology of the article and do not add to the introduction. Overall, the reader would like to see a more focused attempt at delineating the problem. A focus on cost/benefit analysis, shortage of rural anesthesia services, and need to establish CRNA outcomes in this population could be discussed in depth with literature support.

The literature cited supports the author’s statements. P4L18 states that the majority of the studies found no difference and provides 5 references. It is noted that three of these are over 25 years old and probably not appropriate as anesthesia and surgical techniques have dramatically changed. The reader would appreciate a description and analysis of the 2003 and 2004 studies in the literature review, as well as a review of the Silber, 2000, study. A more in depth review of the available recent literature would offer a framework for the study.

There is no theoretical framework provided. The author may wish to describe the rationale for CRNA practice within an historical context. This framework plus an analysis of the research literature would then provide support for the hypothesis of no difference [P6L2].

The research design appears to be a secondary analysis of data supported with an updated survey to obtain retrospective information about available anesthesia staff. In the ABSTRACT, the objective is stated as “to compare frequency” [P2L5] In the INTRODUCTION [P4L2; P5L10] the objective is stated “to examine the relationship”. However, there is a proposed hypothesis of “no difference in deaths and complications rates associated with these two types of staffing” [P6L2-3]. Thus, the reader becomes confused, wondering whether the study is looking at relationships or differences.

The focus of the study is the difference in outcomes between two groups of anesthesia providers: MD’s and CRNA’s. The description of the staffing models on P4L7-12 describes four groups. Groups 3 and 4 are actually similar staffing models, the difference being employer. This may be confusing to individuals not familiar with anesthesia practices.

The organization of the Methods section describes the dependent variable first (outcomes) and then the independent variable (staffing). It would be helpful to the reader to reverse this organization. The material under the title “Hospital Data” P7 is also confusing in terms of organization as demographic data is identified in the first sentence. The material presented on P7L15-21 is key, and would be best described first. The material presented on P7L22-P8L1-4 is

demographic, and it is not clear to the reader why this material is significant to the study. If teaching hospitals were excluded, as might be expected, then this should be clearly stated.

There is no note in the Methods section related to permission to use the database or human use approval for the staffing survey.

Again on P8 the dependent variable is presented first. It is unclear on L6-13 if the author is describing the coding in the original database, or if these ICD-9 codes were recoded for the purpose of this study. The use of the words “death rates and other obstetrical complications were measured” L11-12 is difficult to understand unless the author is discussing the original derivation of the database.

The material described under the heading “Independent Variables” do not seem to agree with the literature review or the proposed hypothesis. This reader understood the IV to be type of anesthesia staffing. The introduction of patient demographics is confusing at this point of the manuscript, as are geographic location, and teaching status. Additional explanation for including these variables is needed. Finally, the last “independent variable” presented is “discharge status (death or living). This reader interprets this as an important outcome variable that should be presented as a dependent variable.

The data analysis, using hierarchical modeling, was first introduced to the reader on P9. It would have been helpful to indicate that it was suspected that there were numerous variables that might impact the staffing outcome, and that this method was intended to remove the variability they contributed. The author should be commended for consulting with statistical experts.

The results section [P10L3-7] presents a discussion of the sample of hospitals in the first paragraph. The sample is actually the number of patients cared for by CRNAs and those cared for by MD's. Information about the patients for whom the outcome is being measured is not referenced until P11L19 refers to Table 3. The reader would appreciate this information sooner in the results section. A description of the hospitals from which the sample has been drawn can be presented in a Table.

P10L13 describes the “majority of obstetrical complications” yet no percentages or numbers are provided either in the narrative or the Tables. If the author believes that these data are important, then specific values should be included.

P11L7-17 presents data analysis that is not related to the study, was not presented in the literature review or the hypothesis. However, this is important information for the study. The reader would recommend a heading of “Additional Findings” to focus on this information. In addition, the raw numbers of these complications should be provided.

P11L21-22 The statement that death rates were extremely low and there was no significant difference needs clarification. It is very likely that the reason there is no significant difference is because the rate was low, the cell size to appropriately analyze these differences should be noted.

P12L12-16 describes CRNA-only and MD only hospitals. The reader is confused as to the description; does the author mean CRNA only C-section hospitals? P13L1-6 discusses co morbidities. It is unclear whether the author is inferring that there are sample differences between the CRNA and MD C-section patients. This difference could be a major influence on the study findings and needs further description or analysis.

The discussion section reports a finding of no difference in complication rates, an important finding. The author would appreciate framing this finding in light of the proposed hypothesis. The discussion section is presented in 5 pages of narrative text. Woven throughout the discussion are pieces of the research conclusion [P13L16-18, P15L5-12] methodological decisions [P14L8-10, P14L18-20, 22-23, P17L3-22, P18L12-16], findings [P14L12-13, P15L1-4] better relocated to their respective sections. There is little discussion of how this study compares or adds to the body of existing literature as few citations are found. The only relevant item is found in the limitations section [P18L3-4]

The limitations section is useful for the reader to better understand the methodology. Some of these limitations would have been better placed under the data sources earlier in the manuscript [P15L18-23]. The statement on P16L13-17 is confusing and seems to minimize the findings. This reader believes that the intent of the study was to compare the two providers. The economic explanation that follows [P16L17-22] is not supported and this reader feels that health care providers would not be likely to sacrifice patient outcomes for economic reimbursement.

There are four Tables which supplement the results section. Table 1 is very detailed. The category and content of the ICD codes does not add to the discussion. Perhaps a simple list of general complications with an example would be adequate, for example: pulmonary e.g. aspiration. Table 2 list the variables considered for risk and how the variables were dichotomized. The reader would suggest actual n values for each category as well as the incidence of the co morbidities. Staffing is the independent variable and does not belong in the Table 2 but could be moved to Table 3. Table 3 has several items that were not part of the original study, i.e. number of deaths for all obstetrical procedures and deaths per 100K procedures. The rationale for including this data is not clear. The information in Table 4 also is not clear as to why it was included, i.e. postpartum hemorrhage which is not an anesthetic complication.

The organization of the paper has been addressed. A clearer presentation adhering to steps of the research process would assist the reader in understanding the method and findings of this important work. The writing style is clear with few grammatical errors. The writing style of the data analysis differs from that of the remaining manuscript and could use some editing for clarity and flow.