PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	A method to assess obstetric outcomes using the 10-Group Classification System: a quantitative descriptive study
AUTHORS	Rossen, Janne; Lucovnik, Miha; Eggebø, Torbjørn; Tul, Natasa; Murphy, Martina; Vistad, Ingvild; Robson, Michael

VERSION 1 - REVIEW

REVIEWER	Gianpaolo Maso MD Department of Obstetrics and Gynecology, Institute for Maternal and Child Health- IRCCS Burlo Garofolo, Trieste (Italy)
REVIEW RETURNED	19-Feb-2017

GENERAL COMMENTS	This is the first study that assessed interinstitutional variation of specific outcomes according to TGCS. As described in discussion, that Authors did non consider the risk adjustment, giving the explanation that the TGCS allows a comparison of clinical value, quantifying populatation'patients and different practice patterns. Nevertheless in the limitations of the study , it should be appropriate to stress that interinstitutional comparison of outomes need to take into account the results of risk adjustment. This might be specifically useful evaluating maternal outcomes according to different sizes of specific groups (i.e group 5) and differences of maternal characteristics (i.e. BMI and maternal age). Moreover the risk adjusted analysis might clarify whether specific antenatal conditions (i.e. fetal growth restriction, preeclampsia) could explain interinstitutional differences of determined neonatal outcomes (i.e. HIE, perinatal mortality)

REVIEWER	Ana Pilar Betrán World Health Organization, Switzerland
REVIEW RETURNED	23-Feb-2017

GENERAL COMMENTS	This manuscript is very clearly and well written with the objective to present data on events and outcomes using the Robson classification in three different settings in three countries (2 facilities and one national database). One of the reasons for using the Robson classification as a standardized classification system to monitor and compare caesarean section rates is that we can achieve more meaningful and reliable comparisons than with other available classification systems (e.g. indications classification) and recommendations for action can be derived. This manuscript presents to the reader how to conduct some of these comparisons. I think this is an important manuscript that can point users of the classification to some aspects of the interpretation and navigate their way into them.

	Some comments for consideration are listed below: • Were all women delivering in these three setting able to be classified? In other words, were there no cases where the information on one of the variables to classify women according of the Robson classification were missing? This is an important quality indicator for the reliability of the interpretation of the data. If that is the case, I suggest to state/explain it because it will most likely not be the case in the case of other potential users/settings of the classification and attentions needs to be drawn to this possibility. • Page 12: I think the discussion of the ratio between Groups 1 and 2 is very relevant and will be very useful in informing practices and clinical management when using the classification. These three setting are reported to have ratios of 3.1, 1.7 and 3.3 which informs on the higher intervention rates (pre-labour CS or induction) in SUH and SLO vs. NMH. Recognizing that onset of labour is one of the variables difficult to standardize across settings, I think authors could discuss if potentially any differences between definitions in these settings could be the basis for the different ratios. Because of this challenge in terms of standardizing definitions, it has been suggested that definition of spontaneous labour/onset of labour (i.e. diagnosis of labour) is reported together with the Robson classification table. • Page 12: The discussion about lower use of oxytocin in SUH vs. NMH could be discussed more clearly. According to Table 4, the lower use of oxytocin for acceleration of labour is in Group 1 (33.2% vs. 53.2%). In Group 2a the use is almost identical and in Group 5, it is higher in SUH. It is not clear if the authors are using this data to correlate with the rate of OASIS discussed in the following sentence.

VERSION 1 – AUTHOR RESPONSE

Respond to the reviewers:

Reviewer Name
Gianpaolo Maso MD

Institution and Country
Department of Obstetrics and Gynecology, Institute for Maternal and Child Health- IRCCS Burlo Garofolo, Trieste (Italy)

This is the first study that assessed interinstitutional variation of specific outcomes according to TGCS. As described in discussion, that Authors did non consider the risk adjustment, giving the explanation that the TGCS allows a comparison of clinical value, quantifying populatation'patients and different practice patterns.
Nevertheless in the limitations of the study, it should be appropriate to stress that interinstitutional comparison of outomes need to take into account the results of risk adjustment. This might be specifically useful evaluating maternal outcomes according to different sizes of specific groups (i.e group 5) and differences of maternal characteristics (i.e. BMI and maternal age). Moreover the risk adjusted analysis might clarify whether specific antenatal conditions (i.e. fetal growth restriction, preeclampsia) could explain interinstitutional differences of determined neonatal outcomes (i.e. HIE,

perinatal mortality).

We fully agree and thank you for stressing this issue. To highlight this we have added the following:
- Included in strengths and limitations: “Comparing obstetric outcomes using the 10-Group Classification System do not adjust for risk factors”
- Included in limitations in the discussion: “This does not include risk adjustment, which limits the ability to compare absolute percentages of the outcomes observed.”

Reviewer: 2

Reviewer Name
Ana Pilar Betrán

Institution and Country
World Health Organization, Switzerland

This manuscript is very clearly and well written with the objective to present data on events and outcomes using the Robson classification in three different settings in three countries (2 facilities and one national database). One of the reasons for using the Robson classification as a standardized classification system to monitor and compare caesarean section rates is that we can achieve more meaningful and reliable comparisons than with other available classification systems (e.g. indications classification) and recommendations for action can be derived. This manuscript presents to the reader how to conduct some of these comparisons. I think this is an important manuscript that can point users of the classification to some aspects of the interpretation and navigate their way into them.

Some comments for consideration are listed below:

• Were all women delivering in these three setting able to be classified? In other words, were there no cases where the information on one of the variables to classify women according of the Robson classification were missing? This is an important quality indicator for the reliability of the interpretation of the data. If that is the case, I suggest to state/explain it because it will most likely not be the case in the case of other potential users/settings of the classification and attentions needs to be drawn to this possibility.

Thank you for this important comment. We have added the following to clarify this under methods:
“The NMH had a complete TGCS-registration initially. A complete registration was also confirmed at SUS and SLO after cross checking of data.”
And additionally in the discussion: “Stressing this issue is important as the reliability of the data interpretation may be questioned if the number of unclassified cases is significant.”

• Page 12: I think the discussion of the ratio between Groups 1 and 2 is very relevant and will be very useful in informing practices and clinical management when using the classification. These three setting are reported to have ratios of 3.1, 1.7 and 3.3 which informs on the higher intervention rates (pre-labour CS or induction) in SUH and SLO vs. NMH.
Again an issue we have discussed and we have added the following: “The definition of pre-labor cesarean will additionally define in which group the women are classified (group 1 or group 2b) with an impact of this ratio.”
Recognizing that onset of labour is one of the variables difficult to standardize across settings, I think authors could discuss if potentially any differences between definitions in these settings could be the basis for the different ratios. Because of this challenge in terms of standardizing definitions, it has been suggested that definition of spontaneous labour/onset of labour (i.e. diagnosis of labour) is

reported together with the Robson classification table.

We fully agree and have added information of the different definitions used of start of the active phase of labor between SUH, NMH and SLO in the next paragraph. This is an important issue and we hope to have clarified these differences. Units using active management of labor may look to the NMH, and units using the WHO definition of prolonged labor may look to the SUH.

• Page 12: The discussion about lower use of oxytocin in SUH vs. NMH could be discussed more clearly. According to Table 4, the lower use of oxytocin for acceleration of labour is in Group 1 (33.2% vs. 53.2%). In Group 2a the use is almost identical and in Group 5, it is higher in SUH.

To clarify that SUH has a different approach towards use of oxytocin and labor progress we have added the following: “SUH has the lowest overall cesarean section rate and the lowest overall use of oxytocin. However, stratified by the TGCS, the use of oxytocin at SUH was lowest in group 1 only. SUH practice a judicious use of oxytocin that includes a definition of the start of active labor, prolonged labor and thereby indication for oxytocin use, which differ from NMH and SLO.”
And additionally: “The different types of labor management probably explain the different rates of oxytocin augmentation and prolonged labors observed (Table 3 and 4).”

It is not clear if the authors are using this data to correlate with the rate of OASIS discussed in the following sentence.

Thank you for this comment. A new paragraph is added to clarify the message.

VERSION 2 – REVIEW

REVIEWER	MASO GIANPAOLO Department of Obstetrics and Gynecology Institute for Maternal and Child Health, IRCCS- Burlo Garofolo-Trieste-Italy
REVIEW RETURNED	24-Apr-2017

GENERAL COMMENTS	I do agree with Authors' conclusions that cesarean section rates, maternal characteristics together with labor and fetal outcomes need to be defined by using the same classification system. In this regard the TGCS might be proposed as the standardized method of assessing events/outcomes and comparing their inter-institutional rates, which may contribute to the judgment of quality of care in labor and delivery.

REVIEWER	Ana Pilar Betran World Health Organization, Geneva, Switzerland
REVIEW RETURNED	19-Apr-2017

GENERAL COMMENTS	Only one further comment. In the methods, authors state that the data are presented as descriptive statistics. Given that comparisons between hospitals and groups represents an important part of the results and discussion, I suggest adding: "And any comparisons between groups and hospitals described in the manuscript do not represent statistically significant differences" for clarity.

VERSION 2 – AUTHOR RESPONSE

Again we thank the reviewers for improving comments. The suggestions are added in the manuscript.