High-Quality Reviews:

Example #1:

Reviewer Recommendation: Reject
Manuscript Ranking: Below Top 20% (not suited for PRS)
Confidential Comments to Editor: Not suitable for publication in PRS. Might be suitable for PRS GO with revisions.
Comments to Author: This is a small retrospective study including only 43 patients who underwent reclosure of wound dehiscence. There was no control population who underwent conservative treatment of wounds to be able to directly compare the data between these two groups. The authors state that this approach may be safer and more cost-effective than healing by secondary intention, however they have no direct control group to be able to make this statement. This is a considerable flaw. The references should be changes to alphanumeric characters and numbered appropriately. It is unclear from the methods section whether or not all patients had a fascial dehiscence or just superficial skin dehiscence. The authors do not address this in the methods section. If a fascial dehiscence occurred, how was this exactly managed? If there was a mix of patients that had fascial dehiscence and some that did not, what was the differential outcomes between these two? There was also heterogeneity with respect to the type of reconstruction. Some patients underwent debridement and primary reconstruction at that time, others had delayed reconstruction after several debridements. What were the surgical indications for being able to close the patient rather than ongoing debridements? Were there any specific criteria that the authors used to be able to determine whether or not the wound was ready for closure? The authors state that the median healing time using this approach was 27 days. It is unclear what this number would be in an appropriate control group being treated with conservative therapy. It is impossible to say that this healing time is favorable without any control group. Overall this retrospective, uncontrolled, small study of wound dehiscence treated with reclosure does not add significant convincing evidence and particularly comparative to conservative treatment in wounds. There is also no algorithm proposed to make a decision whether or not a patient can undergo reclosure versus open wound therapy.

Example #2:

Reviewer Recommendation: Reject
Manuscript Ranking: Below Top 20% (not suited for PRS)
Confidential Comments to Editor: The current paper and topic has the potential to be very interesting. That said, the choices made in the data analyses make the paper hard to decipher or draw meaningful conclusions. The authors provide an interpretation that may or may not be reached independently by all readers based on the information presented. The authors also leave out or only partially explain important aspects. For example, length of stay should be included in the study. Lastly, and importantly the analysis doesn't feel like a cost savings study of ERAS. Rather it feels like a comparison of costs of a modern versus a historical cohort. Non-ICU flap monitoring would not be considered in many peoples ERAS pathways--its just current best practice. I think the authors could have crafted things differently to get to a more meaningful and interesting outcome. In its current form the paper just isn't compelling.
Comments to Author: This paper looks at costs associated with ERAS pathways. The authors report an 8% reduction in overall costs attributable to nursing care provided outside the ICU environment. As a reader, I am not sure all of the data is presented in such a way to draw the same conclusion independently without the help of interpretation by the authors in the discussion section. LOS is also not specifically addressed as a cost driver which is a somewhat curious choice by the authors. Lastly, and importantly the analysis doesn't feel like a cost savings study of ERAS. Rather it feels like a comparison of costs of a modern versus a historical cohort. Non-ICU flap monitoring would not be considered part of many peoples ERAS pathways--its just current best practice. Below are specific comments by section. TRAS groups has longer surgery, more fluids in all categories. Can authors please discuss the impact of these differnences on relevant outcomes for the paper including, cost, complication, LOS? Hospital readmissions are double the rate for ERAS. Is it possible the patients are being moved home too soon? In the first study from this cohort , readmissions were no different, but this is not the case for the current group. The authors omit this important fact from the discussion so a few comments should be added. Why is LOS not reported for the study? Its briefly mentioned to be shorter for the ERAS group in the results section. That said, it should be listed in Table 2 or elsewhere. In addition, why was it not included in the univariate and multivariate covariates which impact cost? It may be captured in other direct costs, but this should still be addressed by the authors.

Why is hospital readmission not included in the adjusted analysis Table 5? It would seem to be an important contributor to Medicare Part A and overall costs which needs adjustment. Its certainly is near significance on the univariate (Table 4) so should be included in the multivariate analysis.

So where do the cost savings come from with the ERAS (Table 6)? Do hospital costs include room and board or are they separate? Where are ICU costs captured in this table? What is included in "other"? If the conclusion is that ERAS is cheaper, then the reader needs to see the granular data.
It is also interesting that the authors have attributed ICU stay as a cost contributor to the TRAS compared to the ERAS pathway. I don't think most breast cancer free flap patients would go to an ICU any longer for monitoring even without an ERAS pathway. I think this is more related to historical patient care in centers (Haddock paper ref 22 is from 2010), than anything attributable to modern ERAS pathways. Perhaps the authors can comment on whether this was a shift in practice following ERAS institution or more simply evolution of current practice.

The authors indicated transfusion is a cost driver, but its unclear why TRAS should have a greater transfusion rate than ERAS. Again this would suggest that traditional care had a lower threshold for transfusion, than current day. I am not sure this is unique to the ERAS.

Example #3:

Reviewer Recommendation: Major Revision
Manuscript Ranking: Top 10%-20% (good; okay if materials needed for PRS)
Confidential Comments to Editor: None
Comments to Author: The purpose of this work was to assess the value of intra-operative imaging, utilizing an O-arm, for management of orbital fractures. The authors retrospectively reviewed orbital fracture repairs over an 8-year period and analyzed the effects of real-time 3-D imaging on fracture management and intra-operative decision-making. They found that, in nearly half of their cases, the additional information obtained from intra-operative imaging changed management in some fashion. The majority of changes were related to manipulation or modification of orbital plates. This is an interesting study and I commend the authors on tackling a difficulty subject and critically evaluating their own experience. I think the authors should address the following issues: 1. There is some literature on 3-D c-arm imaging for assessing intra-operative positioning of zygomatic fractures, which the authors have referenced. The authors should compare the results to their own. What are the advantages of their O-arm over a 3-D C-arm? -Wilde F, Lorenz K, Ebner AK, Krauss O, Mascha F, Schramm A. Intraoperative imaging with a 3D C-arm system after zygomatico-orbital complex fracture reduction. J Oral Maxillofac Surg. 2013 May;71(5):894-910. -Heiland M, Schulze D, Blake F, Schmelzle R. Intraoperative imaging of zygomaticomaxillary complex fractures using a 3D C-arm system. Int J Oral Maxillofac Surg. 2005 Jun;34(4):369-75. 2. What was the rationale for using the O-arm in the cases that were included? There is the potential for selection bias, as the authors report 182 cases of orbital fracture repair over the study period, but only 101 cases where the O-arm was used. They should provide a statistical comparison of those included and excluded to ensure that the samples were similar with exception of the use of the O-arm (i.e. similar demographic and clinical characteristics). 3. Can the authors comment on the rate of revision of their orbital fractures prior to the use of the O-arm? I would be very surprised if they had a 44% revision rate prior to the use of the O-arm, which begs the question as to whether the changes that were completed intra-operatively were "over-treatment" of the scan and may not have made a difference clinically. It would be informative for the authors to provide data on the 81 cases during the same period where the O-arm was not used and provide a critical comparison of the results of

those fracture repairs versus those done with the O-arm. This head-to-head comparison would allow them to make a stronger statement about the clinical value of the O-arm.

4. In the 23 cases where a post-operative CT was done without an obvious clinical indication, why was the imaging obtained?

5. Even though, to the authors' best approximation, the O-arm delivers 50% less radiation than a conventional helical maxillofacial CT, it still delivers a lot of radiation. Do the authors feel that this radiation expense is justified, particularly in young patients? Perhaps they can offer suggestions or an algorithm on when the O-arm should be considered as an adjunct.

6. Perhaps the biggest flaw in this study is that there is no objective outcome data regarding enophthlamos, loss of zygomatic projection, widening of mid facial width, or diplopia, etc. While the authors make a compelling argument about the intra-operative findings and the influence of the O-arm on intra-operative decision making, they fail to transition this argument into one about improved clinical outcomes. Apropos to point #3, how does one know that the long term results are any better or worse than without the O-arm? Again, a direct comparison of cases where the O-arm was used to those where it was not, during the same time period, would be useful. Since they have 3D imaging from the O-arm, perhaps the authors could compare, for unilateral injuries, the pre- and post-operative orbital volumes between the affected and unaffected sides for both the O-arm group and the non-O-arm group and see if the orbital volumes were actually restored more accurately with the O-arm.

7. How much does the O-arm cost? What were the average operating times for cases with and without the O-arm? In this era of cost-containment, it behooves all of us to begin to consider the costs and benefits of additional devices/tools. If there is no substantiated clinical benefit from the O-arm (which may be the case), then the device is an unnecessary expenditure. Even if there is a documented clinical benefit, the authors need to demonstrate that it is cost effective. Does using the O-arm add OR time? At a rate of approximately $50-70/minute in the OR, 10 additional minutes in the OR for the O-arm can add a significant cost to the case (in addition to the cost of the device itself).

8. I find issue with Figure 3. The (C) panels are good and demonstrate the effect of modifying the plate clearly. The (A) and (B) images are sub-optimal for a few reasons. First, 1A and 2A appear to be at the same level, whereas 3A is a little lower, as the entirety of the zygomatic arch can be seen (and does not appear to be fully reduced). The same can be said for 1B and 2B, which appear to be in a similar section, whereas 3B is a bit more anterior. It is also difficult to tell from the images chosen, but the infero-medial wall of the right orbital fracture does not appear to be adequately reconstructed even after modifying the plate (though, admittedly, this may be due to the fact that 3B is more anterior than the others and, perhaps, the reconstruction is more apparent in posterior slices).

Example #4:

Reviewer Recommendation: Minor Revision
Manuscript Ranking: Top 10% (must publish in PRS)
Confidential Comments to Editor: Nice applied clinical anatomy paper that I do think is worthy of publication after revision. I can see this being cited many times.
Comments to Author: 1) .pdf p.9 - "potentiallyinjuring" should be 2 words 2) .pdf p.9 - it is very unusual for one to perform a posterior components separation in the face of a current or prior anterior components separation. This can lead to significant weakening of the abdominal wall, irrespective of the anatomical findings of this study. 3) Some figures at end of repeated 4) Interesting study that correlates with what we see in the OR. This has been described before in the references listed by the authors pertaining to posterior components separation, and therefore in that sense is not novel or new. What is new is the specific correlation with specific aspects of the abdominal wall. As long as one follows the guidelines for technique previously described (entering the TAR plane 0.5 cm medial to the NV bundles), some of the theoretical risk the authors describe in their discussion is obviated. I, therefore, believe this caution may be overstated in this paper.