WRITING DECISION LETTERS

Last updated January 7, 2014; send suggestions for improvement to Steve Green at Green@AnnEmergMed.org.

This document will discuss first rejection letters, then revise and reconsider letters.

REJECTION LETTERS

It is *Annals* policy that decision editors provide a reason when we reject a manuscript, even if just a single sentence. Summarizing the basis for our decision should help authors improve their manuscript before submission elsewhere, and may also help them better design future research. Providing our editorial insight can engender goodwill and encourage authors to continue to consider *Annals* for their better work.

Almost every experienced researcher can recall one or more rejection letters they have received that did not engender goodwill, but were instead perceived as spiteful, unfair, or condescending. It is our most sincere desire that *Annals* not be the source of such letters. The ideal rejection letter should convey in concise and objective fashion the specific reason(s) for the adverse decision. It should be constructive, polite, and helpful in tone. It should show evidence that you actually read their manuscript and got the general picture of what they were trying to say or accomplish. It should show respect for the manuscript and authors, and should avoid unexplained summary opinions or value judgments. If the manuscript was sent for peer review, the reviews returned to the authors should also be edited to ensure that they are also constructive and professional.

Some rejection letter mistakes follow, using quotes taken from actual decision letters.

The data presented do not support the stated conclusion.

This should never be a reason for rejection as it is totally fixable. You can always advise the authors on how their conclusion could be revised.

You have used the incorrect statistical methods.

This should never be a reason for rejection as it is totally fixable. You can always tell the authors the preferred analytic technique.

The applicability of your research to the US environment is limited.

This alone is not a valid reason for rejection. We are an international journal and want to include all of the best advances in emergency medicine, even if not immediately applicable to US practice.

Although femoral nerve blocks are increasingly used in EDs, ultrasound localization is becoming the standard for determining needle insertion sites.

Don't assume that all emergency medicine is practiced as in academic teaching hospitals. Many newer technologies are not yet standard in community EDs and assuming otherwise will make our authors think that we don't understand the "real world."

To follow are decision letter statements that convey unexplained value judgments. They are not constructive and are likely to be perceived as condescending and rude.

This does not pass the test of clinical significance, the "so-what" test. There is no useful information that can be taken from the results. We are unable to publish this work as the plausibility of the argument is flawed. This is an example of garbage in, garbage out. This research does not add much to the science in this area. This research fails to contribute new knowledge in this arena.

If a manuscript doesn't have a compelling premise, simply explain why. If it doesn't provide new information, then cite or summarize the prior research that it duplicates.

To follow are examples of better rejection letters for various scenarios.

Not a "first" case report

Annals' policy is somewhat unique in that we only publish case reports of previously undocumented disease processes, unique unreported manifestations or treatments of known disease processes, or unique unreported complications of treatment regimens (see our Instructions for Authors for full details). Although this is an interesting report, it is not the first reported case of acute disseminated encephalomyelitis in a child. Although your case may differ in some respects from the prior reported cases, these distinctions are not sufficient to warrant a new separate publication in *Annals*. Our inability to publish this manuscript does not mean that we do not regard it as of publishable quality, but rather reflects Annals' specific policy regarding case reports.

Technically a "first" case report, but the unique aspect is of marginal interest / importance

Although this is an interesting report, vague presentations of aortic dissection are well known. The relationship between this patient's aortic dissection and his hip pain is speculative and cannot be confirmed.

Too superficial a review article

Although this is an interesting study, your attempt to comprehensively summarize the ED asthma care literature renders your discussion of each particular element too superficial to be of value to our readers. Additionally, we have concerns regarding the completeness of your literature review. For example, you only cite a single study of magnesium for asthma, when in reality there are now at least seven randomized controlled trials and two meta-analyses on this topic.

Original research that doesn't add anything really new

Although this is an interesting study on a topic in which *Annals* has great interest, your sample size is substantially smaller than the following study, which enrolled 100 children: Pomeranz, et al: Rectal methohexital sedation for computed tomography imaging of stable pediatric emergency department patients. *Pediatrics* 2000; 105:1110-1114. Large samples are necessary to determine the rate of adverse events for procedural sedation agents, and we urge you to expand this study to meet or exceed the sample size of the Pomeranz paper.

Although this is an interesting study, the association between antipsychotic drugs and thromboembolic disease has already been established (Zornberg GL, Hershel J: Antipsychotic drug use and risk of first-time idiopathic venous thromboembolism: A case control study. *Lancet* 2000; 356:1219-1223). Quantification of the independent contribution of antipsychotics to pulmonary thromboembolism in your dataset would similarly require a case control or multivariate analysis, and your sample is not large enough to support this.

Although this is an interesting study, it is similar to one that we have already published: Gulla J, Singer AJ: Use of Alternative Therapies Among Emergency Department Patients. *Ann Emerg Med* 2000;35: 226-228. Although your study does have some differences, these distinctions are not sufficient to warrant a second similar publication in *Annals*.

Unlikely to have external validity

Although this is an interesting study, the frequency of analgesic administration at your hospital is not necessarily applicable to or predictive of practices at other centers and regions. It remains unclear whether your experience is indeed representative of any particular practice setting.

Too small a study to answer the question

Although this is an interesting study, your sample size is insufficient to determine whether clinicians can reliably forego blood counts in some children with sickle cell

crises. The discriminatory power of such testing is only of clinical importance if it can lead to changes in test utilization. Even if none of your 86 children had their ED or hospital care impacted by their blood counts, the 95% confidence intervals of 0 in 86 permit up to 4% of sickle cell patients to have clinically important abnormalities. Given that aplasia and severe anemia are not unusual in this patient subset and can be life-threatening if missed, you will need a larger patient sample to satisfactorily address this issue.

Too weak a study design

Although this is an interesting study, your comparison of dopamine and non-dopamine patients is difficult to interpret given that the former patients had a greater severity of illness than the latter. To reliably assess the unique hemodynamic contribution of dopamine and its impact on mortality would require a randomized controlled trial of patients with similar severity of illness.

Potentially fixable (but probably not)

Although this is an interesting study on an important topic, your primary outcome of similar wound infection rates appears at substantial risk of Type 2 statistical error due to limited sample size. Indeed, you have not provided either a sample size calculation or confidence intervals for this outcome measure so that readers can determine whether clinically important differences could have been missed.

If you believe that you can revise or expand this study to demonstrate the power to detect clinically important differences, we invite you to resubmit this manuscript to *Annals*. Any such revision, however, would be treated as an entirely new submission and no assurances of publication can be provided.

Potentially fixable methods

Although we are interested in this topic, we note that you do not detail generally accepted methodological elements for retrospective studies. (See Gilbert: Chart reviews in emergency medicine research: Where are the methods? *Ann Emerg Med* 1996; 27:305-308.) Accordingly, we are unable to initiate the peer review process. If your chart review was performed in accordance with these principles, we invite you to revise the manuscript to clearly document these elements and resubmit. It would then undergo standard peer review.

Although this is an interesting study, it is difficult to reconcile your reassuring data with existing case reports of death and serious injury. Larger series would appear necessary to reliably define the incidence of rare but serious adverse events. The validity of your findings are particularly threatened by the fact that checklists were not consistently completed on all restrained patients, and it would seem plausible that health care workers might be less motivated to complete such paperwork if indeed complications occurred. This is a topic that *Annals* has interest in, and if you believe that you can expand and revise this paper to reliably address the issues dis-

cussed above, we invite you to resubmit this work. Any such manuscript would be treated as an entirely new submission, however.

Too messy to send for review, but may be fixable

The usual process for our journal is to send each submitted manuscript to multiple experts for peer review before making a final editorial decision. However, during our editorial review we note several unclear methodological elements and internal inconsistencies in your manuscript, and believe that in its current form our peer reviewers will have too many questions to render suitably thorough reviews.

Accordingly, we request that you revise your manuscript based upon the general comments below, and we can then determine the suitability of that revision to enter the peer review phase of our evaluation. Our request for a revision is not a guarantee that the manuscript will ultimately be accepted, and this manuscript could still be rejected either before or after the peer review phase. It would be reasonable to presume that any favorable action would entail at least one more request for manuscript revision, perhaps substantial.

REJECTING AFTER REVIEW

If after review you decide that the manuscript should be rejected, ideally communicate the main non-fixable issues for the benefit of the author. Examples:

Although this is an interesting study, the reviewers were in consensus that the rarity of hepatobiliary imaging in typical emergency medicine practice limits the interest of this study. Additionally, it seems unlikely that the groups with and without opiates are otherwise similar, and this concern is demonstrated by the differential disease prevalence shown in Table 1.

Although this is an interesting analysis, it is not clear how emergency physicians can use it to improve their practice. The message that patients of all ethnicity should receive appropriate analgesia is straightforward and well established. Additionally, we have concerns regarding the lack of blinding in your chart abstraction and in your ability to control confounding variables (see attached reviews).

Although this is an interesting randomized controlled trial, our reviewers had extensive concerns with its methodology and execution. Most importantly, it is still not clear whether you have selected the optimal dose of etomidate to compare to pentobarbital. It seems essentially certain that you could have achieved similar procedural conditions by simply pushing etomidate doses further, and the critical question not answered by your study is whether such dosing is associated with an unacceptable rate of adverse effects. Other important reviewer concerns are detailed below.

WRITING A "REVISE & RECONSIDER" LETTER

Spend LOTS of time on this, as a complete and effective revise and reconsider letter will save both you and the author grief by minimizing the number of subsequent revisions. Edit the reviews to eliminate items with which you disagree, items that are demeaning or petty, or items inconsistent with the general message you wish to send.

Add editor comments to the beginning of the letter before the reviewer comments, as this helps authors focus on the most important issues. Examples:

> Your main study finding is that celecoxib and naproxen are of similar efficacy; however in your abstract conclusion you then declare celecoxib advantageous based upon a secondary outcome 9% absolute difference in dyspepsia that many readers will consider trivial. You do not mention anywhere in the paper that celecoxib is seven times as expensive as naproxen (comparison on www.drugstore.com). Your abstract conclusion should either simply report the efficacy outcomes, or if you believe that the adverse event profile deserves mention here it should be balanced with the expense disadvantage. Please remove the "platelet-sparing" comment from your abstract conclusion, since this data set shows no advantage to the use of a platelet-sparing agent.

> This is an interesting paper on an important topic. However, the validity of your findings is threatened by two core issues. First, it is unclear how the infants receiving LP in your study differ from the entire population of febrile infants seen at your ED during the study period. Were there policies in place that determined who did and who did not undergo LP? Were they enforced? How many infants did not receive LP and how many were ultimately diagnosed with meningitis? Second, how can you verify that WBC counts were not used in selecting which infants should and should not receive LP? If WBC results contributed to how your specific sample was chosen, then your findings cannot be representative of the whole population of infants at risk for meningitis. In your revision please ensure that both of the above issues are thoroughly addressed, as the inability to suitably resolve them would greatly lessen our interest in your manuscript. Also be aware that your revision may undergo repeat peer review by the same original reviewers.

> The reviewers were unanimous in questioning whether a new review of febrile seizures is needed, given the paucity of recent additional evidence and multiple existing reviews on the topic (Examples: Offringa M, Moyer VA: Evidence based paediatrics: Evidence based management of seizures associated with fever. BMJ. 2001;323:1111-4. Common emergent pediatric neurologic problems. Reuter D: Emerg Med Clin North Am 2002; 20: 155-76.Hirtz DG: Febrile seizures. Hirtz DG: Pediatr Rev 1997; 18: 5-8). Please explain in the cover letter accompanying your revision what prompted this piece and how it is different. If, for example, you are actively disagreeing with the AAP practice parameter, then this should be made a

clearer focus of the manuscript. The inability of this paper to represent something "new" in the literature will negatively impact our interest in your manuscript.

Give guidance to the author on how to interpret the oft-conflicting reviews

This is a high-quality study on an important topic. We request a revision that addresses the following editor and reviewer comments.

Editor comments:

1. The responses of our reviewers reflect a lively diversity of opinion on what a study like this should include and on what outcome is the most important. Reviewer #1 believes that the sample should be restricted to those without severe head injuries, ie, those in whom clinicians actually need a decision rule. Reviewer #4 believes that hospitalization for 2 nights is a more plausible criterion of importance than is anticonvulsant therapy for >1 week; however this editor believes the reverse. Reviewer #3 would like to see criteria that identify those actually requiring neurosurgical procedures. It is clear that you will never be able to satisfy everyone given such variation in interpretation. Accordingly, rather than attempting to impose any one perspective upon you, Annals instead requests that you simply review these clashing suggestions to gauge how your manuscript will be received by readers. You may then consider whether it is possible to modify your manuscript so as to satisfy a broader audience. This editor believes that the most compelling of these requests is the potential prediction of those who actually required neurosurgery, although the analytic limitations of this small sample are recognized.

What if the paper is really poorly written and confusing?

Although your manuscript focuses on an important and novel topic, we found your methods section incomplete and difficult to understand. For example, we cannot even be certain whether your study is truly prospective or retrospective. We have serious questions about the presence and handling of missing data. Accordingly, it is impossible at this stage for us to reliably ascertain whether your study was performed with sufficient rigor to merit potential publication in our journal. In the remainder of this letter we outline our general questions and concerns, but be forewarned that they are simply preliminary comments. It is possible that your clarification of these issues may lead us to then reject your paper based upon the clarified methodology. Regardless, your revised manuscript would undergo repeat peer review before it could be further considered for publication. Even in the best circumstance further revision should be considered likely.

Not CONSORT compliant

You have forgotten to include the required CONSORT participant flow diagram. See the CONSORT statement for an example.

Results must be reported using effect sizes with their confidence intervals. In this case, what is the absolute difference between FEV1 measures at each time point and the CI's around these differences? This helps readers ascertain whether the effect sizes include or exclude clinically important differences.

Please describe your method of randomization in greater detail. Our expectation is that you specify the CONSORT statement requisites of reporting sequence generation, allocation concealment, and implementation. It is surprising that exactly 31 patients ended up in each group. Please explain.

HANDLING A POOR REVISION

Author ignores a critical point

Thank you for your quality revision of this manuscript. However, we continue to have important concerns regarding the format of your analysis. *Annals* is willing to consider a further revision that addresses the following items:

1. We continue to have a fundamental disagreement with your decision to include the post hoc survey year dichotomization in your primary analysis. Your a priori objective was to identify variables associated with analgesia, and as such we believe that your Table 1 regression analysis should simply report these results without the added time stratification. Survey year can certainly be included as a separate variable; however there would appear to be no justification for using it in dichotomous rather than continuous fashion, and certainly no reason to subdivide race/ethnicity based upon time period. Taking a post hoc dichotomization and plugging it back into the primary analysis will tend to potentially overstate apparent differences and risk overfitting the model.

Oops, I found some new picky items after reviewing a revision.

The following comments include further dialog upon areas already discussed, responses to new elements added to the paper, and several relatively minor new comments on areas intended to enhance readability and promote brevity. We appreciate your ongoing patience with our revision requests.

We appreciate the time and energy that you have put into revising your manuscript based upon our comments. Your study is novel and important, and *Annals* continues to have great interest in it. In our earlier letter we noted that the reviewers unanimously found the paper to be confusing and difficult to follow. Although this revision clears up many of the confusing areas, it also introduces new ones, and unfortunately the manuscript remains difficult to read and in many areas lacks a progressive flow of information. Accordingly, many of the comments in this revision letter relate more to the presentation and order of your manuscript elements rather than to specific data issues.

We apologize in that you may find portions of this letter that seem new when compared to our prior letter. These result from both the new items introduced in your revision and our attempt to move to a deeper level in guiding your manuscript revision. *Annals* places great emphasis upon clarity and readability, and be forewarned that upon submission of your next revision it is very likely that we will continue to have comments and revision requests. We ask in advance for your patience with this process, and intend it to be an open and constructive. If you have any questions about any of these requests at any time, please email me at <email address>.

The writing style is horrible and disorganized, but underlying science is worthwhile enough for you to edit the authors' manuscript directly. This is typically reserved for obviously junior or foreign authors without resources, not manuscripts with experienced co-author(s) who apparently didn't take the time to provide their full expertise.

Attached for your review are suggested edits to your manuscript shown using the Word "track changes" function.

The intent of the suggested edits is to:

1. shorten the manuscript to below the 2,000 word threshold for Brief Reports (this version is 1,959 words);
2. consolidate and simplify the methods section; and
3. remove all references to IVE efficacy (eg, *Annals* does not permit references to unpublished data).

Please consider incorporating these suggested edits into your manuscript. When you submit your revision, please include a detailed cover letter explaining any suggestions that you did not incorporate, with specific page and paragraph references. Not all suggestions mandate revision, but if you choose not to implement a suggestion, you should include a detailed reason why you think a change is inappropriate.

Another option for otherwise promising manuscripts with unacceptable writing quality (in particular those where English is not the primary language) is to refer the authors to professional medical editing service, where they can pay to get the help that they need. Three such language editing support services are International Science Editing, Edanz, and SPI.

(Your manuscript) needs major work in rendering the English language writing clear and consistently intelligible. You might consider the use of a writing service; see: http://www.elsevier.com/journal-authors/author-services