fants reported to CRIMS from 2012 to 2015 suggests a positive effect of the new policy of immediate ART initiation.

This study was limited to the information available in CRIMS, and there was potential for bias related to infant death and loss to follow-up prior to entry into CRIMS as well as unequal observed time for participants. However, we provide evidence of a long delay in CRIMS reporting was associated with profoundly reduced effect on survival. Policymakers should consider integrating follow-up, EID, and ART initiation into prevention of mother-to-child transmission programs to address programming gaps and serious flaws in HIV service delivery.

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COMMENT & RESPONSE

Errors in Data Input in Meta-analysis on Association Between Initial Use of e-Cigarettes and Subsequent Cigarette Smoking Among Adolescents and Young Adults

To the Editor I write on behalf of my coauthors to report errors in our article, "Association Between Initial Use of e-Cigarettes and Subsequent Cigarette Smoking Among Adolescents and Young Adults: A Systematic Review and Meta-analysis," that was published online on June 26, 2017, and in the August issue of *JAMA Pediatrics*.[1]

First, we had inadvertently input incorrect transition probabilities of cigarette smoking initiation by e-cigarette use status and, as a result, the wrong unadjusted odds ratio of this initiation from the study by Leventhal et al.[2] After using the correct data, the correct pooled transition probabilities for cigarette smoking initiation equaled 23.2% for ever e-cigarette users and 7.2% for never e-cigarette users across all studies (not the originally reported 30.4% for ever e-cigarette users and 7.9% for never e-cigarette users). In addition, the correct pooled unadjusted odds ratio of cigarette smoking initiation by ever e-cigarette use equaled 3.83 (95% CI, 3.74-3.91) across all studies (not the originally reported 5.12 [95% CI, 4.41-5.95]).

Second, we discovered an error in the statistical code to input the adjusted odds ratio for the study by Primack et al.[3] After using the correct data, the correct pooled adjusted odds ratio of cigarette smoking initiation from the meta-analysis equaled 3.50 (95% CI, 2.38-5.16) across all studies (not the originally reported 3.62 [95% CI, 2.42-5.41]).

However, the conclusions and interpretations of the article were not affected by these errors or the corrections. As we concluded, "e-cigarette use was associated with greater risk for subsequent cigarette smoking initiation and past 30-day cigarette smoking."[1(p788)]

We have included a new eTable in the Supplement (eTable 10) that identifies the source of specific input data (eg, adjusted odds ratio) for each study. We confirm that there are no other errors in the originally published article. We have requested that the article be corrected.[4] We apologize to *JAMA Pediatrics* and its readers for any inconvenience or confusion our errors may have caused.

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4. Errors in data input in results, figure, and table. *JAMA Pediatr*. doi:10.1001/jamapediatrics.2017.4322

Hypertonic Saline and Acute Bronchiolitis: The Debate Is Still On

To the Editor We read with interest the study "Effect of Nebulized Hypertonic Saline Treatment in Emergency Departments on the Hospitalization Rate for Acute Bronchiolitis: A Randomized Clinical Trial" by Angoulvant et al and the Efficacy of 3% Hypertonic Saline in Acute Viral Bronchiolitis (GUERANDE) Study Group.[1] We thank the authors for this well-designed and clinically important randomized clinical trial. The authors concluded that hypertonic saline administration for bronchiolitis does not reduce hospital admission.

However, we would like to highlight a few important issues. First, the study was powered to detect a 10% difference in admission. However, given the prevalence of the disease, would a smaller reduction in admission rate also be clinically significant?[2] This of course would require a more resource-intense study.

We noted with interest that the Respiratory Distress Assessment Instrument score improved in the group receiving hypertonic saline. This may indicate a clinical improvement in symptoms that may not be reflected in the primary outcome of hospital admission. Given that admission criteria for bronchiolitis may differ between centers (or even between physicians), would reporting a more consistent variable, such as total hospital length of stay, be more pertinent to this clinical scenario? At this time, it is not clear which is the most appropriate clinical outcome to report for studies of acute treatment of bronchiolitis.

In addition, we also noted that 12.4% of patients (hypertonic saline group) and 9.9% of patients (normal saline group) tested negative for respiratory syncytial virus. It is possible that some of these patients have an alternative diagnosis, reducing the difference in true clinical effect seen from treatment with hypertonic saline for bronchiolitis. A sensitivity analysis, including only respiratory syncytial virus–positive cases, could help to determine whether this is a clinically important issue.

Again, we appreciate the difficulty and effort required to conduct such a large-scale randomized trial. We would look forward to any additional analysis or comments the authors could provide regarding the issues mentioned here.

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1. Angoulvant F, Bellêttre X, Milcent K, et al; Efficacy of 3% Hypertonic Saline in Acute Viral Bronchiolitis (GUERANDE) Study Group. Effect of nebulized hypertonic saline treatment in emergency departments on the hospitalization rate for acute bronchiolitis: a randomized clinical trial. *JAMA Pediatr*. 2017;171(8):e171333.

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In Reply We thank Tanguay-Rioux et al for their careful reading and response, which allows us to clarify some points of our study. First, given the burden of acute bronchiolitis in infants, a treatment reducing hospital admission even by few percentage points should not be neglected. However, as stated by Ralston,[1] demonstrating a slight efficacy in the experimental conditions of a randomized clinical trial does not imply a clinical pertinence in daily practice. For example, in a randomized clinical trial, patients are enrolled based on rigorous criteria, while a much larger phenotype of patients will be met in daily practice. The same applies to where and how the treatment is delivered. This point associated with adverse effects, and the cost of hypertonic saline (HS) nebulizations makes the clinical utility of such treatment very unlikely.

Second, we agree that both total hospital length and hospital admission are appropriate clinical outcomes to report in studies of acute treatment bronchiolitis. Indeed, these 2 criteria do not concern the same patients because total hospital length is only pertinent with the most severe patients, those requiring hospitalization. Thus, it is possible that a treatment could have a measurable effect in only 1 of these criteria, making their study complementary and not opposed. Concerning HS nebulization, it failed to prove efficacy in both hospital admission rate[2] and total hospital length.[3]

Third, as noticed by Tanguay-Rioux et al, respiratory syncytial virus (RSV)–positive cases were associated with an increasing risk of hospitalization, with an odds ratio of 2.17 (95% CI, 1.25%-3.78%; $P = .005$) according to our mixed-effects regression model. As suggested, we performed sensitivity analysis including only RSV-positive cases. By 24 hours, 165 of 327 infants (50.5%) in the HS group were admitted compared with 191 of 344 infants (55.5%) in the normal saline group. The difference in hospitalization rates between the HS and normal saline groups among RSV-positive cases was not significant according to our mixed-effects regression model using the center as the random effect (risk difference, -5.5%; 95% CI, -11.9% to 0.9%; $P = .09$). These results indicate that a selection bias linked to patients with an alternative diagnosis was unlikely. Furthermore, our results concerning the percentage of RSV-positive cases (86.4%), total hospital length (mean [SD], 3.7 [2.7] days), and other characteristics data were in line with previous randomized clinical trials concerning acute bronchiolitis strengthening the external validity of our study.[3,4]

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