outcome between the epochs in the 2 groups of hospitals. This raises the possibility of a statistical calculation error because the authors do not provide an explanation for this degree of deviation of aOR from the raw unadjusted OR. If the aOR for this measure is indeed insignificant, then the author's conclusion that SpO_2 policy changes had no impact on any ROP incidence needs to be revised.

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https://doi.org/10.1016/j.jpeds.2020.11.005

Reference

1. Foglia EE, Carper B, Gantz M, DeMauro SB, Lakshminrusimha S, Walsh M, et al. for the *Eunice Kennedy Shriver* National Institute of Child Health and Human Development Neonatal Research Network. Association between policy changes for oxygen saturation alarm settings and neonatal morbidity and mortality in infants born very preterm. J Pediatr 2019;209:17-22.e2.

Reply

To the Editor:

Dr Srivatsa voiced concerns about our report investigating the impact of changing oxygen saturation alarm limit policies on neonatal outcomes among extremely preterm infants. Dr Srivatsa correctly notes that the magnitude of difference between the unadjusted ORs and aORs for the outcome of "any retinopathy of prematurity (ROP)" was approximately 40% for infants in hospitals without a policy change. This magnitude of difference was not seen for other outcomes.

In fact, the aORs are less than the unadjusted ORs across epochs for most outcomes assessed in both hospital groups. We could speculate about exactly why the degree of difference is higher for the outcome of "any ROP," but we know it is due to adjustment for 1 or more of the important baseline covariates included in the model. Many of these covariates varied significantly between epochs. We confirm that the observed difference was not due to a calculation or reporting error.

Further, we dispute the notion that our conclusion need be revised. The strength of our study design is that we included hospitals without a policy change as a comparison group. This allowed us to isolate the impact of the policy change itself from secular trends in practice and outcomes that would be observed in a traditional before/after study following a policy change. Although the aOR for "any ROP" suggested improved outcomes in epoch 2 for both groups, the interaction between hospital group and epoch was not significant. This supports our conclusion that a policy change was not associated with meaningful improvements in the outcome of any ROP.

In conclusion, the difference between the unadjusted ORs and aORs is due to the adjustment for covariates that may have differentially impacted the outcomes, leading to different degrees of difference between the unadjusted ORs and aORs across outcomes.

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Do B-type natriuretic peptide levels accurately predict outcome in infants with congenital diaphragmatic hernia?

To the Editor:

We have read with interest the study by Guslits et al that investigated the prognostic effect of B-type natriuretic peptide (BNP) in predicting the outcomes of infants with congenital diaphragmatic hernia (CDH).[1] Infants with atrial septal defect, ventricular septal defect, or patent ductus arteriosus were included. However, it may be important to exclude subjects with any other disease that influences ventricular volume expansion and pressure overload, because BNP is a cardiac neurohormone secreted by the ventricles in response to volume expansion and pressure overload.[2] We are very interested in the echocardiographic parameters of those infants, to understand if these heart diseases could have an impact on their right volume and pressure. Alternatively, an additional control group with similar heart diseases but without CDH could be included.

In the present study, the authors sought additional biomarkers that could longitudinally assess illness severity due to pulmonary vascular disease and right ventricle