Self-reported Race/Ethnicity and Occult Hypoxemia: Comment

To the Editor:

I am writing in regard to the recently published article by Burnett *et al.*, "Self-reported Race/Ethnicity and Intraoperative Occult Hypoxemia: A Retrospective Cohort Study."[1] In this retrospective cohort study, the authors investigate the association between self-reported race/ethnicity and occult hypoxemia and hypothesize a greater prevalence of occult hypoxemia in non-White cohorts. Analyzing 151,070 oxygen saturation measured by pulse oximetry–arterial oxygen saturation readings in 46,253 patients, Burnett *et al.* find that occult hypoxemia was higher in Black and Hispanic cohorts than in White ones, in both bivariate (odds ratio, 1.44; 95% CI, 1.11 to 1.87) and multivariable analysis (odds ratio, 1.31; 95% CI, 1.03 to 1.68).[1]

In the introduction, the authors state that their exposure "self-reported race/ethnicity" serves "as a surrogate for skin pigmentation."[1] Previous research has addressed the nonequivalence of socially constructed racial and ethnic categories and skin pigmentation.[2,3] The use of this proxy can thus lead to incorrect interpretations or conclusions. Interpretation of this study's data should focus upon race/ethnicity rather than skin pigmentation. Limitations of using race/ethnicity as a surrogate for skin pigmentation were acknowledged in the discussion of the article but were not sufficient given the scientific and social issues raised by the surrogacy.

With the available data set, the article could have investigated race/ethnicity as the primary exposure without it serving as a surrogate for skin pigmentation. Explanations for findings could then have explored (but not been limited to) skin pigmentation, varied placement of pulse oximeters by providers, or different use of certain types of nail treatments among patients (or a combination thereof). That is, could there be nonbiologic reasons that Black and Hispanic cohorts are more likely to experience occult hypoxemia than White patients?[4] This question is important given that socially constructed categories do not have meaningful biologic distinction.[5] If future investigators are interested in studying the association of skin pigmentation and occult hypoxemia in the vein of previous research,[6] they might consider performing a prospective study and use measurement tools such as the Fitzpatrick skin phototype classification.[7]

Research on race and ethnicity in our field is important and much needed. I commend the authors for taking on this subject matter.

Competing Interests

The author declares no competing interests.

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Self-reported Race/Ethnicity and Occult Hypoxemia: Reply

In Reply:

We thank Dr. Rosenbloom for her comment on our work on self-reported race/ethnicity and intraoperative occult hypoxemia.[1,2] While we recognize race and ethnicity as social constructs, due to the nature of this large retrospective study, it was the most appropriate means to investigate this important topic. In any retrospective study, methodologic trade-offs are made, and our study is no different. We note the two references Dr. Rosenbloom provided demonstrating the nonequivalence of race/ethnicity and skin pigmentation[3,4] but also would like to reference a study demonstrating that although there is variation in skin pigmentation within racial and ethnic groups, there is a significant correlation between ancestry/ethnicity and skin pigmentation.[5] Furthermore, self-reported race has been shown to correlate with the Fitzpatrick skin phototype classification system.[6] Because of the discrepancies mentioned above and the nature of our retrospective study, we listed this as a limitation of to our study, but we believe this approach was reasonable and likely valid.

Similarly, the potential for varied pulse oximeter location and the use of nail polish/treatments were unknown variables within our data set. We therefore also highlighted these as limitations of our study. Additionally, we do not have data to support the concept that location of pulse oximeters or the use of nail polish/treatment would be different for any one group within our study. Therefore, we believe that a large data set such as ours allows us to capture the most likely and common practices and patient presentations, and it is likely valid to assume this means placement of pulse oximetry on a patient's finger and the presence or absence of interfering nail polish colors would not differ between groups. Indeed, there is no real reason to argue otherwise.

In regard to the use of the Fitzpatrick skin phototype classification, we agree that an objective measurement of skin pigmentation would be beneficial in a prospective study. This classification system was originally created for differentiating skin pigmentation in White patients[7] and was later amended to include more darkly pigmented skin,[8] but concerns regarding the functionality in patients of color exist even in this more objective approach.[9] Alternatives to the Fitzpatrick skin phototype classification have been posed, including a simple color bar tool[10] or more advanced techniques using spectrophotometry or colorimetry.[11] Either way, a prospective study of this topic would be best, although it must be noted based on the incidences measured in our study that the sample size needed is likely prohibitively large, making this a difficult study to conduct.

We hope our article sheds light on this important topic and results in additional research to improve the care of Black and Hispanic patients in the future, and we thank Dr. Rosenbloom for the measured criticism.

Competing Interests

The authors declare no competing interests.

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Point-of-Care Ultrasound Frailty Assessments: Comment

To the Editor:

We read with great interest the article by Canales *et al.*,[1] who benchmarked the predictive value of four surrogates of preoperative sarcopenia and the Fried phenotype frailty tool for predefined adverse postoperative outcomes. We salute the authors on this pioneering and important investigation and would like to highlight several points.

First, the authors used three novel, bedside, sonographic measurements of the quadriceps muscle, as well as a tomographic assessment of the psoas muscle. For the assessment of generalized sarcopenia, the abdominal muscles, including the psoas, are preferable to appendicular muscles due to the former relative independence of activity level.[2] Moreover, dual-energy x-ray has been the standard quantitative measure for total appendicular muscle mass and appears in nearly 500 publications.[3] The advantage of bedside appendicular muscle sonography should be further explored.

Second, females comprised 71 and 61% of the "Not Frail" and "Frail" study groups. The authors adjusted psoas muscle area values according to body surface area and body mass index, but not by sex. The prevailing consensus, however, is to use sex-specific cutoffs for low values due to significant sex variation of human spinal and paraspinal muscles at all body habitus.[4] This fact independently underscores the importance of equal representation of both sexes in the cohort.

Third, for the sample power analysis, the authors provide the desired discriminative magnitude and CI; the expected frequency of frailty in the study population and expected sensitivity and specificity of sonography, however, are not given.[5] Additionally, the areas under the receiver operating curves (AUCs) for each frailty surrogate and outcome measurement are presented (table 1 of Canales *et al.*[1]). The false discovery rate for these 40 statistical tests was not reported, and the authors did not analyze whether differences in AUC between the frailty surrogates were statistically significant.[6]

Last, for comparison purposes, the study provided sonographic muscle mass measurements of healthy controls. However, the race/ethnicity makeup of the cohort and controls were very different (*e.g.*, 79% Caucasians *vs.* 35%, respectively), even though skeletal muscle mass differs significantly between ethnicities at all adult ages.[7] As expected, all controls were "Not Frail" per the Friend phenotype frailty assessment (table 2 of Canales *et al.*[1]), and therefore, the diagnostic quality of a frailty test cannot be assessed in this group. Simply put, a frailty test when performed on nonfrail controls would yield specificity and negative predictive values of 1, but the test's sensitivity, positive predictive value, and AUC cannot be determined. However, sensitivity, specificity, positive and negative predictive values, AUC, and cutoff point are provided for each sonographic frailty surrogate in the controls (table 3 of Canales *et al.*[1]). Likewise, computed tomography was not performed in the controls (see, "Methods" and fig. 3A of Canales *et al.*[1]), yet the aforementioned statistical parameters are provided for psoas muscle area in controls. Perhaps table 3 should read "study cohort" instead of "heathy controls."

The authors did not comment on the surprisingly poor ability of the well established Fried phenotype frailty assessment to predict unplanned intensive care unit admission, prolonged intensive care unit and hospital length of stay, and rehospitalization (AUCs of 0.61, 0.54, 0.65, and 0.52, respectively). The sonographic and the tomographic sarcopenia assessments also seem to lack adequate discriminatory value for a prolonged hospital length of stay and rehospitalization. Unexpectedly prolonged postoperative hospitalization frequently precedes and portends unplanned nursing facility admission. The unexpected dichotomous discriminatory ability of these frailty assessments to predict the latter but not the former warrants further exploration. While we appreciate the authors' objective to determine whether appendicular skeletal sonography could be used reliably in a preoperative setting to identify frailty, addressing these points would further strengthen their findings.

Competing Interests

The authors declare no competing interests.

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