Tis Not, Tis Not – Tis So, Tis So: Rebuttal of Rebuttal by Iacobucci, Posavac, Kardes, Schneider, and Popovich (2015) on the Appropriateness of Median Splits

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ABSTRACT

Iacobucci, Posovac, Kardes, Schneider, and Popavich (2015a) published a defense of median splits. We (McClelland, Lynch, Irwin, Spiller, and Fitzsimons 2015) and Rucker, McShane, and Preacher (2015) published criticism of each of the key conclusions from Iacobucci et al. Iacobucci et al. (2015b) then prepared a rebuttal. Iacobucci et al. believe that we have misunderstood their points, and we believe that they have misunderstood ours. Iacobucci et al. were given the last word in the pages of *Journal of Consumer Psychology*. We will use this SSRN outlet to say why the claims in their rejoinder are either incorrect or miss the point of our criticisms. First when only significant results are published, the fraction of published findings that are type 1 errors are a direct function of power divided by type 1 error rate. Second, they stress that their simulations were intended to apply to the case of zero multicollinearity; but in the real world with random assignment of subjects to conditions, the correlation between the median split variable and the treatment dummy can be substantial, particularly with small sample sizes. Third, they are incorrect to assert that in a moderated regression model with predictors X, Z, and X*Z it is not possible to get an effect of X or X*Z that is significant using median splits but not significant using continuous X. Finally, they ignore our point that the median split will bias the parameter estimate of the interaction in cases where it is in fact non-zero. We stand by every point in our original critique.

Iacobucci, Posovac, Kardes, Schneider, and Popavich (2015a) published a defense of median splits. We (McClelland, Lynch, Irwin, Spiller, and Fitzsimons 2015) and Rucker, McShane, and Preacher (2015) published criticism of each of the key conclusions from Iacobucci et al. Iacobucci et al. (2015b) then prepared a rebuttal. We were not allowed to see that rebuttal before finalizing our commentary, for reasons explained by Pham (2015). Iacobucci et al. believe that we have misunderstood their points, and we believe that they have misunderstood ours. As Iacobucci et al. were given the last word in the pages of *Journal of Consumer Psychology*, we will use this SSRN outlet to say why the claims in their rejoinder are either incorrect or irrelevant to our criticisms. We do not wish to leave readers confused about what we and said in our commentary.

The main theme of their rejoinder was that their original paper was intended to defend median splits as no effects on type 1 errors when predictors were uncorrelated. But they missed our most crucial points.

First, Iacobucci et al. (2015b) repeatedly state that reduced power from median splits is unrelated to their focus on type 1 errors. They miss the point of our Bayesian analysis that lowering power directly reduces normatively correct belief shifts from a study finding (Brinberg, Lynch, & Sawyer, 1992). They ignore our citation of Ionnides' (2005) Bayesian analysis – arguably one of the most influential papers in social science statistics published over the last decade. If (as is generally agreed) there is a publication bias for statistically significant results, the ratio of power to type 1 errors directly effects the percent of the statistically significant findings in the published literature that are type 1 errors. There is no getting around this. Iacobucci et al.'s simulations do not consider how the procedures they advocate affect the aggregate ratio of correct results to type 1 errors in a journal that publishes only (or primarily) significant findings. Pham (2015) has captured our point well: "MLISF … also explain that at a field-wide level—as opposed to the individual-paper level—conditions that lower statistical power on average increase the likelihood that a published body of knowledge contains false positive results."

Second, Iacobucci et al. (2015b) repeatedly return to claims about the case of zero multicollinearity. But the point of our Figure 3 is that any such statements are not relevant to the real world research designs where people are now using median splits in lieu of a continuous X. If one has some manipulated Z, randomly assigned, and a continuous X, the correlation between Z and X is zero in expectation – but likely not in the sample used for the study! With real data and random assignment of subjects to conditions, it is easy to get fairly substantial correlations among X and Z in models with and without X*Z. It is the magnitude of the correlation and not its statistical significance that matters, and so these problems are especially pernicious in experiments with small sample sizes. We reproduce Figure 3 from McClelland et al. (2013) below so that the point we are making is clear to the reader of this rebuttal.

Figure 3 from McClelland et al. (2015, p. 688).

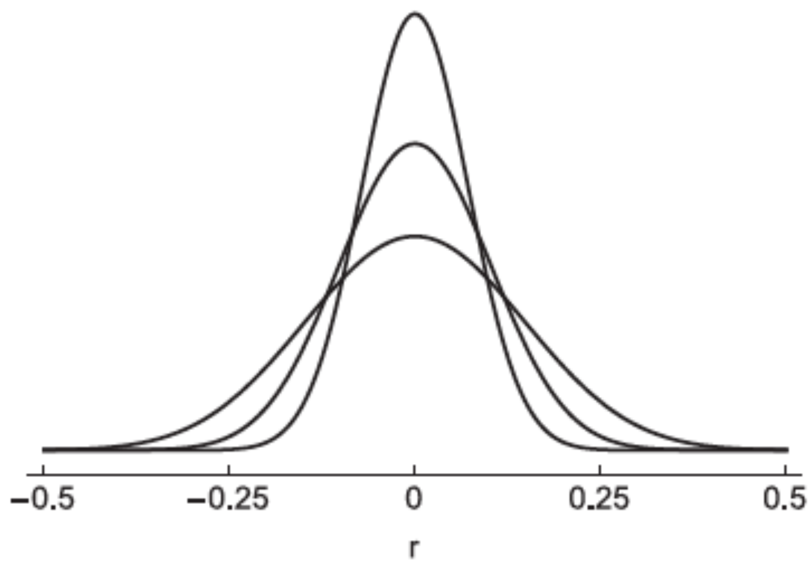

Fig. 3. Sampling distributions for correlation coefficient *r* for sample sizes of 50 (shallowest), 100, and 200 (steepest).

Third, based on their simulations, Iacobucci et al. (2015b) falsely argue that it is not possible to get an effect of X or X*Z that is significant using median splits but not significant using continuous X. They missed that this was the point of our Mani, Mullainathan, Shafir, and Zhao (2013a,b) example. Their study is precisely of the form in the paragraph above – random assignment to conditions with an interaction of that manipulation with a measured X. They obtained findings that were significant by median split but not by a regression analysis in 3 of 3 small-sample experiments. Iacobucci et al. point to influential papers by Kruglanski that used median splits. Mani et al. (2013b) defended themselves against the criticisms of Wicherts and Scholten (2013) using the same arguments proffered by Iacobucci et al. (2015b) in their rebuttal -- that other respected scientists had used median splits.

Iacobucci et al. (2015b) further missed the point of our small simulation showing that when the true correlation between X and Y is 0 in the population, when one rejects the null if either the correlation between continuous X and Y is significant or if the correlation between median split X' and Y is significant, one will reject the null 8% of the time. The implication in that simulation is that when the null hypothesis is true, in 3% of cases, the correlation between X' and Y will be significant when the correlation between X and Y is not. In other words, it is very easy to have a situation like that in Mani et al. if one relies on median splits. Iacobucci et al. say that they would never endorse picking and choosing which result to report based on its significance. We believe that their paper will be used as cover / authority by some scholars who will report only median splits after picking and choosing.

Iacobucci et al. (2015b) go on to say that linear regression can be distorting if the functional form is not linear. We covered exactly that point in our paper under "Nonlinear transformation of X implies a step-function form of the X-Y relation." In fact, we cited our own work making that point (Brauer & McClelland, 2005). Iacobucci et al. further criticize our point that median splits imply a sample-dependent threshold of the relationship between latent X and Y. They rejoin that all statistical tests are sample dependent. That's obviously true. We reiterate: even if one was serious in believing that some latent construct was "categorical" and that there was a step function relationship between latent X and Y, nobody would seriously maintain that the threshold of that step function should change from sample to sample to depend on the median X of the particular sample being tested.

Iacobucci et al. (2015b) say that sometimes people assume a linear model without testing it or looking at scatter plots or residuals. That's true. We teach our students to do otherwise. But Iacobucci et al. ignore the point that the median split implies assuming a threshold without rigorous test for its existence.

Finally, Iacobucci et al. (2015b) rebut our criticism of their simulations, where we noted that they did not in fact test the effect of a median split on X_1 on the test of its interaction with X_2. Their argument is that they are focused narrowly on the issue of whether a median split on an X_1 increases type 1 errors on an uncorrelated X_1*X_2 interaction term. Because a type 1 error by definition requires a null effect in the population, they argue that it does not matter that their "interaction" term was simply another random additive predictor not formed by the product of X_1*X_2. But the definition of an interaction *means* that the simple slope of X_1 varies as a function of X_2 (and vice-versa). This relationship cannot be obtained unless X_3 is defined as the product of X_1 and X_2 in the data-generation process. By the logic of Iacobucci et al. (2015b), they might as well say that their unrelated additive third variable X3 allows them to make claims about the effect of median splits on X1 on a quadratic model with three predictors, X_1, X_2, and $X_1^{2.}$.

Iacobucci et al. (20015b) say, "The reviewers, and the Area Editor and Editor of the original submission desired to see the effect on an interaction, and we were happy to oblige—great idea." It would not be a particularly great idea to request seeing the effect of median splits on a third additive (null) effect given we already know what happens in a two-variable additive multiple regression with one null effect. We suspect that the review team had in mind that, in general, most of the uses of continuous X_1 variables in our literature occur when researchers are interested in a nonzero interaction of X_1 with some manipulated X_2. Because Iacobucci et al. are so focused on null hypothesis testing rather than parameter estimation, they are ignoring our point that the median split will bias the parameter estimate of the interaction in cases where it is in fact non-zero.

We stand by every point in our original critique.

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