**Comment on “Zamariola et al., (2018), Interoceptive Accuracy Scores are Problematic:
Evidence from Simple Bivariate Correlations” -**

**The Empirical Data Base, the Conceptual Reasoning and the Analysis behind this
Statement are Misconceived and do not Support the Authors’ Conclusions**

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Abstract

A recent paper by Zamariola and colleagues is widely cited as an authority on the invalidity of the Heartbeat Counting Task as a measure of interoceptive accuracy. Given the widespread interest in this field, it is essential that papers about methods are conceptually sound.

However, only one of the authors' four criticisms appears substantiated - that people count too few heartbeats. Their arguments about "*simple bivariate correlations*" and their finding that interoceptive accuracy and heart rate correlate, depend on 'spurious correlations' arising from the overlooked point that interoceptive accuracy is a ratio. Moreover, scrutiny of the authors' data shows that their fourth criticism (that interoceptive accuracy is lower on longer trials) is confounded by differences in mean heart rate between trials. We present data from our own labs to refute it. We draw the authors' and editors' attention to these issues and trust that they will reconsider these erroneous conclusions.

1. Introduction

Interoception, and in particular awareness of interoceptive states, has been the subject of a recent exponential growth in research interest, across various sub-disciplines of psychological sciences and social-affective neuroscience (Sahib S. Khalsa & Lapidus, 2016). To date, there are two standard interoceptive accuracy tasks that are widely used for measuring Interoceptive Accuracy (IAcc) in the cardioceptive domain. In the ‘Heartbeat Discrimination Task’ (Whitehead & Drescher, 1980) individuals report the perceived synchrony of their heartbeats with a series of external stimuli (usually auditory cues). During the ‘Heartbeat Counting Task’ (Schandry, 1981) participants count their own heartbeats for several short periods of time, between the onset and offset of a signal (e.g. a tone). Interoceptive accuracy is quantified as a heartbeat perception score which relates the number of counted (i.e. subjectively reported) heartbeats to the actual (i.e. objectively recorded) heartbeats, in the various counting intervals, with higher scores indicating greater accuracy.

For n counting trials, this formula is given by:

$$IAcc = 1 - \frac{1}{n} \sum | (recordedHBs - reportedHBs) | / recordedHBs$$

(See section 7 below for more detailed comment on the Heartbeat Counting Task.)

While it has been argued that each task has advantages and disadvantages, the Heartbeat Counting Task (HCT) has come under increasing criticism (Ring & Brener, 2018). In their recent article, Zamariola, and colleagues present arguments and empirical data to support their proposal that heartbeat counting is not a valid measure of IAcc (Zamariola, Maurage, Luminet, & Corneille, 2018). They illustrate their arguments with a data set of students, $N = 572$ (386 women, mean age = 22.2, SD = 4.3, publicly available at <https://doi.org/10.5281/zenodo.1288323> that includes the actual/recorded and the reported heartbeats for each trial interval, from which interoceptive accuracy is calculated, according to the formula above. By way of contrast, we make publicly available at <https://osf.io/axv4w/> a comparable data set of $N = 394$ (303 women, ages 18-59 years, mean = 22, SD = 5.8) from our labs, with trials of the same length, in the HCT in healthy subjects. (Our sample includes fewer men and a small number of older participants than Zamariola’s sample.)

Specifically, Zamariola and colleagues put forward the following four key criticisms. Firstly, they note that IAcc, as measured by the HCT, largely depends on only one “error type”,

which is the under-reporting of heartbeats (i.e. most people count fewer than the true number of their heartbeats). Secondly, they propose that the total number of actual heartbeats and total number of reported heartbeats (summed across all the test trials) should correlate. In their data, they found only a weak overall positive correlation for the data set as taken a whole. Furthermore, they argue that the correlation between the total number of actual and the total number of reported heartbeats should increase in a linear fashion as IAcc rises, i.e. across IAcc quintile groups. They found no such rising linear trend. Thirdly, they suggest that a valid measure of IAcc should not be “*structurally tied to heart condition*” but they found that IAcc was negatively correlated with heart rate (HR). Finally, the authors report that, in their published data set, IAcc scores differed significantly across the three testing intervals of the Heartbeat Counting Task, with lower IAcc on the 45s trial than on the 25s or 35s trial. They imply that this reflects a general (but presumably under-reported) tendency to poorer performance on longer trials of the HCT. We consider these criticisms in turn and provide critical and constructive evaluation of the authors’ claims. We also address some more general issues that relate to the implementation of the HCT task, data collection and data quality.

2. Under-reporting of heartbeats

The proposal in Zamariola’s paper that heartbeat counting should result in false positives (as in ‘Heartbeat Discrimination tasks’), is not self-evident. On the contrary, it seems reasonable that when people are asked to attend to their bodies and count their heartbeats they will miss some beats, as would be the case in most perceptual signal detection tasks that have substantial levels of noise. Were they to over-count, this would imply that they were hallucinating heartbeats that were never actually experienced. The authors mention some possible reasons for this frequently observed undercounting e.g. weak sensibility in detecting heartbeats (which is specifically what the test aims to measure); participants’ beliefs about their heart rates; time-estimation confounds; or decision thresholds. They do not develop these arguments, some of which seem less plausible than others. For example, if beliefs about one’s heart rate predominantly influenced one’s heartbeat counting, this would not explain why people should believe that their heart rates are slower than they actually are (i.e. it does not explain the prevalence of under-counting). In practice, the HCT is not communicated to participants as a test of intelligence or of cognitive performance, which should minimise the incentives for them to guess, while allowing them to report what they actually sense.

3. “Simple bivariate correlations” between actual and reported heartbeats are mathematically spurious

In their second argument, the authors state: “if IAcc scores reflect people’s ability to accurately perceive their inner states, a correlation between actual and reported heartbeats should be observed and this correlation should linearly increase with higher IAcc scores. This is not the case. The correlation between actual and reported heartbeats is low overall. And, even more critically, it is higher at average than higher in IAcc levels”. (Zamariola et al., 2018, p. 13). In their data set, the overall the proportion of variance in total recorded heartbeats explained by total reported heartbeats is 2.56%.

Zamariola and colleagues divided their participants (N = 572, less 8 unidentified people who were excluded) into quintiles according to their accuracy on the heartbeat counting task (IAcc). They report that Pearson correlations between total recorded (actual) heartbeats and total reported (counted) heartbeats vary across the quintiles from: $r = .50$ for the lowest quintile; $r = .67$ for the second quintile; $r = .95$ for the middle quintile; $r = .83$ for the fourth quintile; and $r = .88$ for the highest quintile (see Figure 2A).

As in the quotation above, the authors take this “non-linear” variation in their reported values of r as a major criticism of the HCT. Considerable space in the Discussion is devoted to trying to account for this finding, for example, in terms of Simpson’s paradox. However, both the attempt to correlate total recorded heartbeats with total reported heartbeats and the decision to divide the data into quintiles by IAcc and then to correlate total reported and total recorded heartbeats, is mathematically mistaken, given the fundamental arithmetic of the formula for IAcc. As noted above, this formula is:

$$IAcc = 1 - \frac{1}{n} \sum | (recordedHBs - reportedHBs) | / recordedHBs$$

Assuming, for simplicity, there is only one trial interval ($n = 1$), the formula can be simplified to:

$$IAcc = 1 - (| (recorded HBs - reported HBs) | / recorded HBs)$$

For the large majority of people who undercount their heartbeats (as commonly reported and also found by Zamariola and colleagues) the absolute values are irrelevant, so that the formula simplifies to:

$$IAcc \cong 1 - \left[\left(\frac{\text{recorded HBs}}{\text{recorded HBs}} \right) - \left(\frac{\text{reported HBs}}{\text{recorded HBs}} \right) \right]$$

$$IAcc \cong 1 - 1 + \left(\frac{\text{reported HBs}}{\text{recorded HBs}} \right)$$

Therefore:

$$IAcc \cong \left(\frac{\text{reported HBs}}{\text{recorded HBs}} \right)$$

We can conclude that Total Recorded Heartbeats divided by Total Reported Heartbeats is a good proxy for IAcc. This is excellently well borne out by Zamariola's data (see Figure 1A) in which the two measures are very highly correlated ($r = .93, p < .001$). The outliers visible in that Figure are those participants who over-counted on one or more heartbeat counting trial or who have high variability in IAcc between trials (which may be based on faulty recording of their actual heartbeats, see comments in section 6 below on anomalies in the data).

Figure 1. Scatterplots using Zamariola *et al.*'s data set. **A:** IAcc plotted against (Total Reported HBs/Total Recorded HBs). **B:** Regression line of Total Reported vs Total Recorded HBs for IAcc between .62 and .65 showing two outliers, an otherwise very high correlation and a

slope equal to the value of IAcc. C: Total Recorded HBs plotted against Total Reported HBs for various narrow values of interoceptive accuracy: IAcc = .40 - .425 (left line); IAcc = .675 - .70 (middle line); and IAcc = .90 - .925 (right line).

Furthermore, it necessarily follows, from the ratio arithmetic, that (for that large majority of people who undercount their heartbeats), *for any particular value of IAcc*, total reported heartbeats and total recorded heartbeats will correlate *perfectly*, i.e. with $r = 1$. Moreover, *the unstandardized slope of the regression line between them will be equal to that particular value of IAcc*. In other words, if any data set is divided up by the variable X/Y and then X is correlated against Y , it necessarily follows that X and Y will be perfectly correlated and all points will lie on a straight line with slope = X/Y .

By way of illustrating this, Zamariola's data set contains 31 people with IAcc between .62 and .65 (see Figure 1B). Of the two highly visible outliers in Figure 1B, one individual (participant 221) over-counted on two of the three trials while the other (participant 540) had recorded heart rates of 50, 98 and 61 bpm for the three trials, indicating substantial measurement error. When these two individuals are excluded, the correlation between total reported and recorded heartbeats is $r = .98$. The regression line is $y = 0.13 + 1.59x$, where y is Total Recorded Heartbeats and x is Total Reported Heartbeats. Thus, the slope of the line (*unstandardized beta*) is .63, i.e. it is the given value of IAcc, which was between .62 and .65.

This mathematics is further illustrated in Figure 1C, where three distinct regression lines of excellent fit and increasing slope can be seen for three more narrow ranges of IAcc within Zamariola's data. The authors' quintile analysis is therefore equivalent to dividing up the data according to the variable (X/Y) and then drawing conclusions on the basis of correlations between X and Y . The circularity of this reasoning seems not to have been apparent.

That differences that are seen in the "*simple bivariate correlations*" of the authors' title accordingly rely only on three issues. (i) The wider the range of IAcc values included within any given quintile, the lower the correlation will be. (ii) The presence of a few people in most quintiles who over-count rather than under-count their heartbeats will result in outliers who will bias correlations. (iii) There are, in their data, other outliers who result from substantial inter-individual variation of IAcc between trials. This is indicative of errors in data recording (see Section 6 below).

The authors report of a very low overall correlation between Total Reported Heartbeats and Total Recorded Heartbeats, for their data set taken as a whole, can now be seen simply to reflect the wide range of IAcc values within that data (which is entirely to be expected in any random sample). To achieve a high correlation between these two variables one would require a data set in which all participants had very similar IAcc, as Figure 1B illustrates.

In summary, the basic premises of the authors' '*simple bivariate correlations*' rely on a fundamental arithmetic misunderstanding, leading to a misconceived analysis.

Recalculating the authors' own quintile calculation

Although their quintile analysis is not a valid argument against the HCT, for the reasons explained above, we did attempt to replicate it using the authors' published data. Oddly, our analysis found different values for r than those they publish. Using all their data, we find: for the lowest IAcc quintile $r = .51$; for the second quintile $r = .55$; for the middle quintile $r = .72$; for the fourth $r = .83$; and for the highest quintile $r = .88$. This is illustrated in Figure 2, which depicts the quintile correlation analyses. Figure 2A is the original Figure 2 from Zamariola et al. (2018), while Figure 2B is the recalculated analysis of Zamariola's data by us. Zamariola and colleagues' argument seems therefore not to be wholly borne out by their own data. However, it is obvious from Figure 2 that these correlations are highly sensitive to the influence of a few extreme outliers. As the authors do not explain the criteria by which they excluded eight of their participants as outliers, it is not possible to identify who these people were and thus to see how excluding particular individuals affects these correlations.

Curiously, our reanalysis of their quintiles does, in fact, result in a set of correlations more akin to the type of "*linear increase*" the authors were hoping to find. However, a linear pattern of increase in values of r is neither meaningful nor inevitable. Using our own data set (Figure 2C) we find: for the lowest IAcc quintile $r = .65$; for the second quintile $r = .36$ (which it can be clearly seen is strongly influenced by outliers); for the middle quintile $r = .93$; for the fourth $r = .80$; and for the highest quintile $r = .88$. In any case, these analyses miss the point. The correlation coefficient (r) gives a measure of how closely the points cluster round the line of best fit. What is much more relevant is how the slope of the regression lines (unstandardized beta) changes across the quintiles, because this slope reflects the changing values of IAcc.

A further, minor, criticism of the published paper is that in Figures 2 and 3 in Zamariola et al.'s paper, the axes seem to be labelled incorrectly. The x -axis in their Figure 2 is labelled 'Reported heartbeats' but the scale is from 0 to 120, whereas the total number of reported heartbeats in their data (summed over the three trials) ranges from 0 to 196. Similarly, their y -axis is labelled "*Actual heartbeats*" with a scale from 40 to 120, whereas the total number of actual (recorded) heartbeats in the authors' data set ranges from 87 to 212. The axes in Zamariola et al.'s Figures 2 and 3 must therefore refer to heart *rates* not to the number of *heartbeats*. However, as total heartbeats and mean heart rates are related by a constant (mean heart rates are calculated by dividing total heartbeats by 105 - which is the total trial length, 25s+35s+45s - and multiplying by 60), this slip should have no effect on the correlations.

Figure 2. A: Zamariola et al. 's (2018) original Figure 2. **B:** Our recalculation of Zamariola et al. 's Figure 2, using their published data. **C:** A quintile analysis for our own data set. (Note that our axes index heartbeats. In Zamariola et al. 's original Figure 2 the axes appear to index mean heart rates not total heartbeats.)

4. The relationship between IAcc and Heart Rate (HR)

The authors report a negative correlation between heart rate and IAcc in their data and propose that this undermines the validity of the test. Again, this relationship is a function of the ratio arithmetic that links these two variables.

We have already shown that for people who undercount heartbeats (who are in the great majority):

$$IAcc \cong \left(\frac{\text{reported HBs}}{\text{recorded HBs}} \right)$$

However:

$$\text{Heart Rate} = \left(\frac{\text{recorded HBs}}{\text{trial length}} \right) * 60$$

Rearranging this:

$$\text{recorded HBs} = \text{Heart Rate} * \left(\frac{\text{trial length}}{60} \right)$$

Therefore:

$$\text{recorded HBs} = k * \text{Heart Rate}$$

Where k is a constant = trial length/60

Therefore, substituting for recorded HBs in the formula for IAcc:

$$IAcc \cong \frac{\text{reported HBs}}{k * \text{Heart Rate}}$$

This is another example of a ratio variable of the form $A = X/kY$, where A (IAcc) and Y (HR) will tend to be negatively correlated (Herzog, Francis, & Clarke, 2019).

Rather surprisingly, Zamariola and colleagues find this relationship problematic because they propose that a valid measure of cardiac IAcc should not be related to heart condition, i.e. that “*people’s accuracy at perceiving their inner life is not structurally bound to their cardiac condition*” (Zamariola et al., 2018, p13). It is difficult to see how this can be correct and the statement implies a fundamental misunderstanding. The tasks used to assess perception of interoceptive signals in any modality will surely always depend on the specific physiology of the organ system under investigation, including its afferent and efferent physiological mechanisms and its anatomy (Craig, 2002). Thus, showing that performance on the HCT is related to the underlying physiological mechanisms of the specific modality under investigation can be construed as evidence *in favour* of the test’s construct validity, rather than against it. It indicates that what is being measured is, indeed, related to the system that is the focus of interest. The same is true for other organ-specific physiological mechanisms underlying interoceptive perceptions, such as gastric interoception (Herbert, Muth, Pollatos, & Herbert, 2012; Koch & Hong, Sung-Pyo, Xu, 2000; Whitehead & Drescher, 1980). Knowledge of the physiological *mechanisms* of interoception is, of course, fundamental for understanding that cardiac activity and perception of cardiac activity are connected and lies at the core of the nature of interoception (Craig, 2002; Fukushima, Terasawa, & Umeda, 2011; Herbert, Muth, et al., 2012; Herbert, Pollatos, Flor, Enck, & Schandry, 2010; S. Khalsa et al., 2018; Kleckner et al., 2017; Luft & Bhattacharya, 2015; O Pollatos, Kirsch, & Schandry, 2005; Olga Pollatos, Herbert, Mai, & Kammer, 2016; Schandry, Sparrer, & Weitkunat, 1986; Schandry & Weitkunat, 1990).

5. The authors report significant differences in IAcc scores across their three HCT trials

Zamariola and colleagues’ fourth criticism of the HCT follows from their finding that in their data “*IAcc scores were significantly lower for 45s interval than for 25s [$t(571)=6.61$, $p < .001$] and 35s [$t(571)=7.68$, $p < .001$].*” They comment that: “*as actual heart rate is quite constant across intervals, this variation is related to a reduction of reported bpm at the longest interval*” (Zamariola et al., 2018, p.14). They imply that their finding is an example of a general tendency to greater under-estimation of reported heartbeats in longer test trials of the HCT. However, this misses the point that, in their data, there is significant variability in the mean value of the participants’ actual heart rates across trials. In fact, contrary to their statement above, within their data, mean recorded heart rates differed significantly between all three of the trial intervals (see Table 1b).

In comparison, within our own data set, t tests show no significant differences between either IAcc or heart rate across the three test trials (see Table 1a).

Table 1a. Paired samples t tests for mean interoceptive accuracy (IAcc) and mean heart rate in bpm (HR) between trials in our own data set

Comparison pair	Mean	SEM	t	df	Sig. 2-tailed
IAcc in 25s vs 35s trials	.003	.006	.47	393	.64
IAcc in 25s vs 45s trials	.005	.006	.76	393	.45
IAcc in 35s vs 45s trials	.008	.006	1.28	393	.20
HR in 25s vs 35s trials	.205	.173	1.19	393	.24
HR in 25s vs 45s trials	.245	.182	1.34	393	.18
HR in 35s vs 45s trials	.040	.165	.24	393	.81

Table 1b. Paired samples t tests for mean interoceptive accuracy (IAcc) and mean heart rate in bpm (HR) between trials in Zamariola et al.'s data set

Comparison pair	Mean	SEM	t	df	Sig. 2-tailed
IAcc in 25s vs 35s trials	.004	.006	.70	571	.48
IAcc in 25s vs 45s trials	.041	.006	6.61	571	<.001
IAcc in 35s vs 45s trials	.045	.006	7.67	571	<.001
HR in 25s vs 35s trials	1.74	.260	6.70	571	<.001
HR in 25s vs 45s trials	.579	.264	2.19	571	.03
HR in 35s vs 45s trials	2.32	.281	8.27	571	<.001

Scrutiny of Zamariola and colleagues' data thus suggests that this finding, and therefore the authors' fourth criticism of HCT, is confounded by differences in mean heart rate across trials.

The authors collected their data using Polar Watches. It is possible to program a Polar Watch to create an 'exercise' that consists of several trials separated by short pauses (e.g. 20s). These will always be presented in the same order within the Polar Watch system. In

Zamariola's data set, recorded heart rate in the 35s trial was significantly higher than in the 25s trial, which was again significantly higher than in the 45 s trial. Such a result, in this large data set, could result from a fixed trial order of 35s, 25s and 45s, with recorded heart rate falling over the three trials. However, Zamariola et al. (2018) report that the trial intervals were randomly presented. Our inspection of the data indicates that the conclusions the authors draw about differences in IAcc between trials are confounded by the significant differences in mean recorded heart rate between trials, which must arise from some other confounding cause.

6. Further data anomalies

Zamariola and colleagues' data was collected over four different experiments, using a Polar Watch RS800CX with software Polar ProTrainer5 to extract the actual number of heartbeats for calculations of the IAcc score. The authors do not comment in their Methods about the careful methodological protocols that must be followed when using the Polar Watches for accurate heartbeat counting. The most significant issue is that with any other heartbeat recording device (for example ECG) the researcher is continuously able to check visually that the equipment is detecting the heart signal correctly. However, when using the Polar Watch RS800CX, it is not possible to identify artefacts during the recording. The only way to check is after the recording has been completed, by opening the exported data file. It is therefore essential to scrutinise these exported files for consistency of mean HRs or inter-beat intervals and to omit participants where the data is seen to be corrupt (and refine subsequent recording protocols).

The authors say that the Polar Watch RS800CX records from electrodes '*on the wrist*'. This is misleading. The electrodes are actually contained within a chest belt and the signal is transferred, in wireless mode, from the electrodes in the chest belt to the receiver in the Watch itself. If the participant is not using the belt directly round the chest (where it is dampened by perspiration during exercise), both wrists must be placed, some distance apart, directly onto the electrodes of the belt. The electrodes must be kept dampened with water, or the signal will be lost. If the participant raises a wrist at any point then the connection is broken and their data is corrupted (too few heartbeats will be recorded). Even if the participant replaces their hands immediately, it takes several seconds for the wireless connection with the Polar Watch to be resumed and some heartbeats will not be recorded. Firstly, from the Methods section of this paper, it remains unclear if the validated sensors

provided by the supplier (i.e. electrodes in the chest belt, Polar Watch RS800CX) or a customised and potentially unvalidated method for ECG assessment (e.g., cables with disposable ECG electrodes) were used. Secondly, it is obvious that if exact recording of every single heartbeat during each trial of the HCT is confounded by methodological mistakes in recording, this will lead to major errors in the IAcc score. Unless careful protocols are followed to avoid these sources of potential error, the Polar Watch (particularly without use of the chest belt) will thus not be a reliable method for recording heartbeats over the short intervals in the HCT paradigm.

A notable omission from the authors' Methods is any description of how the Watches and belts were actually used, as well as vital comments on their limitations and the inaccuracies they can introduce if they are not used according to careful devised protocols. Their data indicates that they did not adequately check for such problems.

A measure of the participant's actual (i.e. recorded) heart rate, for each trial, can be calculated from the recorded number of heartbeats within that trial. Zamariola's participants were "*healthy students*". In such a sample, in a calm condition, moderate mean heart rates can be expected (e.g. resting heart rates of 68-76 beats per minute (bpm) for people aged 20-39yrs (Hart, 2015a)). Moreover, if the HCT test protocol is accomplished correctly, one would expect only small variations in heart rate from trial to trial, within any given individual. For example, Hart (2015a) found that, over a two minute period in a resting condition, changes in mean heart rate were on average only between 1.5 bpm and 1.9 bpm in healthy students (Hart, 2015b). In a situation like heartbeat counting that requires concentration and attention during discrete trials with intermittent resting pauses, rather greater variation in heart rate can be expected (e.g. Kootz, Gold, & Cohen, 1979; Wu & Lo, 2008) but always within physiologically plausible limits.

Inspection of Zamariola's data set, however, shows many instances of large variations in recorded heart rate between trials that are physiologically implausible and therefore indicative of measurement artefacts. For example, participant 116 has a recorded heart rate of only 36 bpm on one trial but 67 bpm and 63 bpm on the other two. Participant 496 has recorded heart rate that varies from 70 bpm in one trial to 128 bpm in another; participant 81 has recorded heart rate of 101 bpm in one trial and 129 bpm in another; participant 107 has a recorded heart rate of 57 bpm in one trial and 86 bpm in another; and so on. In all, we noted

75 participants (representing 13% of their total sample) in whom the variation in heart rate between trials was in excess of 10 bpm, which is the maximum we observed within our own data (see Figure 3A).

These errors in measurement obviously affect the calculation of IAcc. For example, participant 524 has an astonishing IAcc of -.43 in the 25s trial but .96 and .97 in the other two trials, indicating that far too few ‘actual’ heartbeats were recorded in the 25s trial.

These anomalies could be due to a failure of the Polar Watch to pick up the heartbeats accurately for each counting interval, as well as noise introduced by movement or ANS changes in cardiac activity, if the test was not accomplished in the required calm setting across the test trials.

It is surprising that the authors’ major criticisms of the heartbeat counting test should be based on a data set containing many notable errors.

Our own data was recorded with ECG, using BIOPAC Systems Inc., Santa Barbara CA, MP150 version 3.7.2, or with Powerlab 8/35 box (Bio Amp 132) (<https://www.adinstruments.com>). Within our data set, trial-by-trial changes in recorded heart rate are within a range one might expect in participants who are in a physiologically calm state (Figure 3A), with maximum variation in heart rate between trials of 10 bpm. By contrast, Figure 3B with comparable statistics for Zamariola et al.’s data shows obvious errors in recorded heart rates and hence in IAcc.

Figure 3. A: Maximum differences in heart rate in bpm (HR) between any two HCT trials in our data. **B:** Maximum differences in heart rate in bpm (HR) between any two HCT trials in Zamariola et al.'s data.

7. Further Comment on the Heartbeat Counting Task

The Heartbeat Counting Task (Schandry, 1981) is very widely used to assess cardioceptive IAcc (e.g. Ainley, Maister, Brokfeld, Farmer, & Tsakiris, 2013; Garfinkel, Seth, Barrett, Suzuki, & Critchley, 2015; Herbert et al., 2012; Olga Pollatos, Kirsch, & Schandry, 2005; Schulz, Lass-Hennemann, Sütterlin, Schächinger, & Vögele, 2013). Critics, however, (e.g. Brener & Ring, 2016) suggest, for example, that participants may use knowledge of their heart rates and of elapsed time to estimate correctly. This criticism ignores that there is, a considerable and well-examined body of physiological evidence in support of the HCT's construct and criterion validity. For example, individual test performance (IAcc) is significantly related to activity of the interoceptive neural network, including the anterior insular cortex (Olga Pollatos et al., 2005; Olga Pollatos, Schandry, Auer, & Kaufmann, 2007). The heartbeat evoked potential (HEP), which indexes cortical processing of the heartbeat, is consistently correlated with cardiac IAcc (e.g. Fukushima et al., 2011; Katkin, Cestaro, & Weitkunat, 1991; Mai, Wong, Georgiou, & Pollatos, 2018; Montoya, Schandry, & Müller, 1993; Olga Pollatos et al., 2005; Olga Pollatos & Schandry, 2004; Schandry et al., 1986). Moreover, the amplitude of the HEP is positively related to: degrees of emotional arousal and related changes of autonomic nervous activity (Luft & Bhattacharya, 2015); cortisol activity (Schulz et al., 2015); and attention to one's cardiac signals (Montoya et al., 1993). These findings are consistent with results demonstrating that IAcc is similarly connected to the arousal dimension of emotional experience. For example, IAcc is robustly linked to: the intensity of emotional experience; emotional processing; and behaviour (e.g. Barrett, Quigley, Bliss-Moreau, & Aronson, 2004; Herbert, Pollatos, & Schandry, 2007; Herbert, Ulbrich, & Schandry, 2007; O Pollatos et al., 2005; Olga Pollatos et al., 2005; Wiens, Mezzacappa, & Katkin, 2000). Induction of increased sympathetic activity provokes increased cardiac IAcc (Herbert, Herbert, et al., 2012). Furthermore, activity of the right anterior insular cortex has been shown to covary with task performance in both heartbeat counting and heartbeat discrimination tasks (Critchley, Wiens, Rotshtein, Ohman, & Dolan, 2004; O Pollatos, Gramann, & Schandry, 2007) and similarly for the HEP (Mai et al., 2018; O Pollatos et al., 2005; Olga Pollatos & Schandry, 2004). Moreover, external manipulation of anterior insula activity by repetitive transcranial magnetic stimulation reduces respiratory and

cardiac IAcc as well as HEP amplitude (Olga Pollatos et al., 2016). In addition, people with higher cardiac IAcc (as measured by the HCT) show greater changes in sympathetic and parasympathetic activity during mental and emotional stress conditions but not at rest (Herbert et al., 2010). Naturally, therefore, individual differences in cardiac and cardiovascular mechanisms shape the performance in the test. Finally, person-specific and context-specific effects, as well as their interactions, account for the stability of assessments of heartbeat counting as well as heartbeat discrimination (Schulz, Lass-Hennemann, et al., 2013; Wittkamp, Bertsch, Vögele, & Schulz, 2018). These results underscore the test's validity, and they emphasise that knowledge of physiology, and of the related intervening variables, is necessary in order to develop, implement and correctly interpret interoceptive tasks.

Therefore, for reliable assessment and calculation of IAcc, participants sit in a controlled environment, in a calm physiological state. Changes in stress-related, hormonal and autonomic cardiac and cardiodynamic activity affect test performance, as well as activity of the neural interoceptive network in the brain (Durlík, Cardini, & Tsakiris, 2014; Herbert et al., 2010; Mai et al., 2018; O Pollatos, Herbert, Kaufmann, Auer, & Schandry, 2007; Schandry & Bestler, 1995; Schmitz, Blechert, Krämer, Asbrand, & Tuschen-Caffier, 2012; Schulz, Lass-Hennemann, et al., 2013). This is underscored by evidence that individual changes in cardiovascular and cardiodynamic activity (as induced, for example, by isometric exercise or mental stress) increase regional cerebral blood flow within the interoceptive neural network, including activity in anterior insula cortex (e.g. Critchley, Corfield, Chandler, Mathias, & Dolan, 2000). Underlying mechanisms that play a major role in this process are based on inotropic cardiac activity (Critchley et al., 2000; Herbert, Muth, et al., 2012; Herbert et al., 2010; Herbert, Pollatos, et al., 2007; Schandry et al., 1993). Hormonal mechanisms associated with physiological arousal, also affect activity of the interoceptive brain network (S S Khalsa, Rudrauf, Sandesara, Olshansky, & Tranel, 2009; Luft & Bhattacharya, 2015; Schulz, Lass-Hennemann, et al., 2013).

Accordingly, uncontrolled, non-standardized changes in the physiological systems that directly influence task performance (such as changes of body posture or other physical activity between testing trials, etc.) compromise the test's validity. Moreover, participants should not be allowed to manipulate their respiration during the test, which likewise influences autonomic nervous system (ANS) and cardiac activity via cardiorespiratory

coupling (e.g. Sobiech, Buchner, Krzesiński, & Gielerak, 2017). For similar reasons, somatic and psychiatric diseases and any medication impacting ANS and cardiac activity, as well as regular athletic activity that changes sympathetic-parasympathetic tone, are usually assessed within a typical HCT protocol (e.g. Georgiou et al., 2015; Herbert, Muth, et al., 2012; Herbert & Pollatos, 2014; Herbert et al., 2010; Olga Pollatos, Herbert, Mai, & Kammer, 2016). Participants are also typically asked to refrain from caffeine for several hours before testing, as this impacts ANS and cardiodynamics (Hartley, Lovallo, & Whitsett, 2004; McMullen, Whitehouse, Shine, Whitton, & Towell, 2012). This knowledge is fundamental in recognizing relevant intervening parameters and mechanisms, which are confounds of the Heartbeat Counting Task. These include: differences in BMI; regular sports activity; gender; somatic or psychiatric symptoms and disorders; and medication use. It is because such variables are related to basic physiological changes in cardiovascular mechanisms that they have been associated with heartbeat perception accuracy and must therefore be recognized and assessed when measuring IAcc (Cameron, 2001; Georgiou et al., 2015; Grabauskaitė, Baranauskas, & Griškova-Bulanova, 2017; Herbert, Pollatos, et al., 2007; Jones, 1994; O Pollatos, Herbert, et al., 2007).

Zamariola and colleagues make no comment about such intervening variables, nor about standardising settings and other necessary preconditions. This may also partly explain some of the striking anomalies in their published data.

8. Summary: Three of the authors' four criticisms of the Heartbeat Counting Task are not valid

To reprise, Zamariola and colleagues criticise heartbeat counting tasks on four grounds:

1. They note that the majority of people undercount on the HCT. This is not news. In our own data, only 3.8% over-counted. This pattern has often been reported in the past and it remains an area to be investigated (Ring et al., 2015; Schandry et al., 1993; Schandry & Specht, 1981).
2. The authors' second criticism is that they found a low overall Pearson correlation between total actual heartbeats and total recorded heartbeats. The argument here is wholly mistaken, given that IAcc is essentially equal to the *ratio* of reported heartbeats to recorded heartbeats (see Figure 1A). It is therefore mathematically incorrect to expect that recorded and reported heartbeats will correlate for the whole data set, unless everyone within that set has the same IAcc. As we have demonstrated,

for *any particular given (narrow) level* of IAcc score, this correlation will be very close to 1, illustrated in Figures 1B and 1C. What *varies* across the data set is that, as the value of IAcc rises, the *slope of the regression line* between total recorded HBs and total reported HBs will increase, and will be equal to the specific IAcc value under consideration (see Figure 1C).

The authors report that correlations between total recorded (actual) heartbeats and subjectively reported heartbeats were higher for their average IAcc quintile group than for the higher and highest IAcc quintile groups. They propose that this is indicative of a major problem of the validity of the heartbeat counting test (their second criticism of the task). Unfortunately, the quintile analysis is not only mathematically spurious but also appears to be highly sensitive to a few outliers. As we have shown, variation in correlations observed across the quintiles analysis simply reflects: the range of IAcc values within each quintile; the presence of a minority of people who counted too many heartbeats; and noise introduced by inter-individual variation in IAcc across trials (some of this potentially due to measurement error).

3. The authors' third criticism of the HCT is that IAcc is negatively correlated with heart rate. This analysis is similarly mathematically misconceived because IAcc is a ratio variable with denominator proportional to actual heart rate. Therefore, IAcc is generally negatively correlated with heart rate, analogous to the correlation between BMI and height. This is another classic example of a spurious correlation.

Moreover, that an individual's ability to perceive their cardiac activity is intrinsically dependent on specific physiological mechanisms and properties of the respective physiological interoceptive system has been shown by extensive research and is an argument in favour of the HCT, not a valid criticism of it.

4. Fourthly, the authors report that in their data they found lower IAcc in their 45s trial than in the two shorter (25s and 35s) test trials. They imply that this is a common problem but one that it is rarely reported by researchers and that this undermines the validity of heartbeat counting tests. However, contrary to their comment that heart rate was fairly constant across their three trials, recorded heart rate within their data is significantly lower in the 45s trial. In fact, all three trials have significantly different mean recorded heart rates, with mean heart rate in the 35s trial the highest and mean

heart rate is the 45s trial the lowest, which confounds their results. Comparison of Zamariola's results with the data set from our labs shows no such significant differences in either mean heart rate or mean IAcc over three similar test intervals.

Conclusion

We write to express our concern about: the soundness of the mathematical/logical arguments of the authors; significant aspects of the authors' methodology that are omitted from the paper; the quality of the data collected; and the correctness of the analyses reported and illustrated.

Given the reach of this paper and its potential impact, as well as increasing interest in interoceptive accuracy across different sub-disciplines of psychological sciences, it is important for these concerns to be discussed and any misconceptions corrected. We wholeheartedly agree that the reasons why people typically undercount when heartbeat counting remains to be explained. However, the bases of the major criticisms in this paper appear to be very unsound. As fallacious arguments have the potential to do disservice to those interested in the correct use of methods for measuring interoceptive accuracy, we look forward to the responses of the authors.

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