Transcranial Direct Current Stimulation Does Not Influence the Speed–Accuracy Tradeoff in Perceptual Decision-making: Evidence from Three Independent Studies

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Abstract

■ In perceptual decision-making tasks, people balance the speed and accuracy with which they make their decisions by modulating a response threshold. Neuroimaging studies suggest that this speed–accuracy tradeoff is implemented in a corticobasal ganglia network that includes an important contribution from the pre-SMA. To test this hypothesis, we used anodal transcranial direct current stimulation (tDCS) to modulate neural activity in pre-SMA while participants performed a simple perceptual decision-making task. Participants viewed a pattern of moving dots and judged the direction of the global motion. In separate trials, they were cued to either respond quickly or accurately. We used the diffusion decision model to estimate the response threshold parameter, comparing conditions in which participants received sham or anodal tDCS. In three independent experiments, we failed to observe an influence of tDCS on the response threshold. Additional, exploratory analyses showed no influence of tDCS on the duration of nondecision processes or on the efficiency of information processing. Taken together, these findings provide a cautionary note, either concerning the causal role of pre-SMA in decision-making or on the utility of tDCS for modifying response caution in decision-making tasks. ■

INTRODUCTION

The speed–accuracy tradeoff (SAT) refers to how people balance the speed with which they respond and the accuracy of those responses. For example, people can respond faster by reducing the amount of information that is integrated before a response is initiated, that is, lower a response threshold (Bogacz, Wagenmakers, Forstmann, & Nieuwenhuis, 2010; Ratcliff, 1978; Wickelgren, 1977). This tradeoff has been modeled with evidence accumulation models such as the diffusion decision model (DDM; Forstmann, Ratcliff, & Wagenmakers, 2015; Ratcliff & Rouder, 1998). These models are used to predict accuracy levels and RT distributions as well as quantify inter- and intraindividual variability.

The neural mechanisms underlying SAT have been explored in neuroimaging studies, identifying regions where activity is modulated as a function of variation in the response threshold. Activity in the pre-SMA, ACC, and striatum has been related to individual differences in response thresholds (Mansfield, Karayanidis, Jamadar, Heathcote, & Forstmann, 2011; Forstmann et al., 2008), as well as trial-to-trial variability in these thresholds (Boehm, van Maanen, Forstmann, & van Rijn, 2014; van Maanen et al., 2011). Activity differences in dorsolateral pFC has also been related to measures of response caution (Ivanoff, Branning, & Marois, 2008; van Veen, Krug, & Carter, 2008).

These findings are in accord with neurocomputational models of the SAT in which response thresholds are modulated by increasing or decreasing baseline activity in integrator neurons in the pre-SMA or striatum (Bogacz et al., 2010). For example, an increase in baseline firing would mean that there would have to be a relatively smaller increase from stimulus-induced activity to reach a fixed threshold. Different hypotheses have been forward on the exact role of the pre-SMA in this modulation. One hypothesis is that neural activity in the pre-SMA reflects this increased baseline integrator activity itself. An alternative, albeit, related hypothesis is that the pre-SMA modulates the baseline activity of integrator neurons that are in downstream areas such as the striatum, but pre-SMA neurons themselves do not integrate the evidence coming from perceptual areas (Gold & Shadlen, 2007).

The main goal of this study was to use transcranial direct current stimulation (tDCS; Nitsche & Paulus, 2011) to modulate activity in the pre-SMA. Prior studies have

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shown that anodal tDCS can modulate performance on tasks associated with pre-SMA such as the stop signal task. Specifically, after anodal stimulation, participants needed less time to inhibit a response, although RTs to the "go" stimuli were unchanged (Liang et al., 2014; Hayduk-Costa, Drummond, & Carlsen, 2013; Kwon & Kwon, 2013; Hsu et al., 2011).

Here we turned to a different task domain associated with pre-SMA function, asking whether tDCS targeted at this area would modulate performance on a perceptual discrimination task. In particular, we examined the impact of anodal tDCS on how people modify their behavior when instructions emphasize speed or accuracy. In deriving our tDCS predictions, we considered two mutually exclusive hypotheses concerning pre-SMA function. The first hypothesis is that pre-SMA activity contains cortical integrator neurons that represent the integrated evidence for one response or the other (Bogacz et al., 2010; Gold & Shadlen, 2007). This hypothesis would predict that tDCS should modulate the response threshold on both speed- and accuracy-stressed trials.

The second hypothesis is that pre-SMA modulates activity in downstream integrator neurons. This hypothesis is supported by evidence showing that pre-SMA activity only predicts variability in response threshold during speed-stressed trials. One could infer from this that the accuracy-stressed regime is the default setting, with pre-SMA activity relevant when speed is stressed (van Maanen, 2011). By this "downstream control" hypothesis, we would predict that only speed-stressed trials should be influenced by tDCS.

Of course, the validation of the two hypotheses has as "sine qua non" that tDCS alter the cortical excitability of pre-SMA. In the last 2 years, different review papers and recent meta-analyses have raised questions concerning the reliability of tDCS as tool to alter behavior in decision-making tasks (Horvath, Forte, & Carter, 2014, 2015; Nitsche, Bikson, & Bestmann, 2015; Strube, Bunse, Malchow, & Hasan, 2015; Kim et al., 2014; López-Alonso, Cheeran, Río-Rodríguez, & Fernández-del-Olmo, 2014; Tremblay et al., 2014; Wiethoff, Hamada, & Rothwell, 2014). Our study provides an opportunity to assess the efficacy of tDCS targeted at pre-SMA. As described below, an initial experiment failed to show an effect of pre-SMA anodal tDCS on performance in the motion detection task. We then went on to conduct two additional experiments in other labs, with the idea that this would provide an opportunity to assess the replicability of these null results.

METHODS

Participants

A total of 44 participants took part in three experiments, with the testing done at three different locations: The University of California, Berkeley (n = 15, mean age = 23.9, SD = 7.8; 8 women), the University of Amsterdam (n = 15, mean age = 25.4, SD = 6.8; 10 women), and Leiden University (n = 14, mean age = 22.2, SD = 3.0; 10 women). On the basis of a prescreening survey, none of the participants presented any contraindications for tDCS. The participants gave informed consent under protocols that were approved by the institutional review boards at the University of California, the University of Amsterdam, and Leiden University. All of the protocols were in accord with The Code of Ethics of the World Medical Association (Declaration of Helsinki).

Experimental Task

Participants performed a cued version of a motion discrimination task (Britten, Shadlen, Newsome, & Movshon, 1992). The stimuli were displayed on a computer monitor, positioned approximately 60 cm in front of the participant. Each trial started with the appearance of a fixation cross for 500 msec. This was followed by a text cue presented at the center of the display. The cue indicated that, on the forthcoming trial, the participant was to either emphasize speed ("FAST") or accuracy ("ACCURATE"). After 500 msec, the cue was replaced by a cloud of 328 white dots (3 × 3 pixels each) that moved on a black background. The cloud spanned a diameter of approximately 5°, resulting in a density of 16.7 dots/deg^2. The display refreshed at 60 Hz, and between frames each dot was displaced by one pixel to create apparent motion. A variable percentage of dots coherently moved to the left or right (see below); the remaining dots moved in a random direction. The coherence of the stimulus was defined as the probability that a dot would move in the designated (left or right) direction, for example, 50% coherence means that, during each frame, each dot has a probability of 50% to move in the main direction (Boehm et al., 2014; Mulder, Wagenmakers, Ratcliff, Boekel, & Forstmann, 2012; van Maanen et al., 2011; Ratcliff & McKoon, 2008; Palmer, Huk, & Shadlen, 2005; Gold & Shadlen, 2003; Britten et al., 1992; Newsome & Paré, 1988). The participant indicated the perceived direction of the cloud, using the left and right index fingers to press on the z- and m-keys of the computer keyboard, respectively. The dots disappeared when a response was detected or 2000 msec (no response trials). Following the response (and feedback—see below), a 1000-msec intertrial interval preceded the onset of the fixation cross for the next trial.

Experimental Design

Each participant was tested in two different sessions. In the first session, the participant first performed a practice block of 15 trials to become familiar with the task. They were then tested on a 200-trial calibration block, designed to identify the coherence level at which, with no instructions concerning speed or accuracy, the participant would be correct on 80% of the trials. This criterion was selected because it provided a target error at which

we should be able to robustly fit the DDM (Ratcliff & Rouder, 1998). In the calibration block, the level of coherence could be 0%, 20%, 40%, or 80% (40 trials/level, 20 with motion to left and 20 with motion to right). Feedback was provided after every trial in the form of a green (correct) or red (incorrect) circle that appeared in the direction of the response (Mulder et al., 2012). On the basis of the accuracy and RT data from this block, the proportional rate diffusion model (Mulder et al., 2012; Gold & Shadlen, 2007; Palmer et al., 2005) was used to estimate the motion coherence that corresponded to 80% accuracy for each participant.

This coherence level was then fixed at this value for all 600 trials of the main experimental block (Boehm et al., 2014; Mulder et al., 2012). Half of these trials were speed-stressed trials and half were accuracy-stressed trials, and within each the direction of motion was leftward on half of the trials and rightward on the other half of the trials. Note that the proportional rate diffusion model predicts a mean expected RT for a given coherence level. If the actual RT was slower than this criterion RT on speed-stressed trials, participants received a 500-msec feedback that they were "Too Slow." This procedure assured a high-speed stress on speed trials. If the participants responded within the deadline, during both the speed- and accuracy-stressed condition, they would receive, for 500 msec, visual feedback indicating if the response was correct or incorrect.

In the second session, participants only completed the 15-trial warm-up block before the experimental block; the calibration phase was not repeated and the coherence level was set to the same level as in the first session.

In each session, tDCS was applied before the experimental block. Anodal tDCS was used in one session, and sham stimulation was used in the other. The order of the sessions was counterbalanced, and the two sessions were separated by at least 24 hr. In prior studies, tDCS is sometimes applied during task performance (like Spieser, van den Wildenberg, Hasbroucq, Ridderinkhof, & Burle, 2015; Antal et al., 2004) and sometimes applied before the task begins (similar to Liang et al., 2014; Hayduk-Costa et al., 2013; Kwon & Kwon, 2013; Hsu et al., 2011). We opted to use the latter, because we did not want to add an additional experimental factor to the design (online vs. offline stimulation) that might have complicated the modeling analyses.

tDCS was applied at Berkeley using an iBox Dynatron stimulator (Dynatronics, Salt Lake City, UT). For the Amsterdam and Leiden sites, a DC Brain Stimulator Plus (NeuroConn, Ilmenau, Germany) system was used. At all three sites, two rubber electrodes (5 × 7 cm) were applied to the scalp, with the longer dimension of the electrode aligned in the lateral direction. The anodal electrode was placed over FCZ, the target location for stimulation of the pre-SMA (Spieser et al., 2015; Boehm et al., 2014; Liang et al., 2014; Hayduk-Costa et al., 2013; Kwon & Kwon, 2013; Hsu et al., 2011). The return electrode was placed over the contralateral supraorbital area (Hayduk-Costa et al., 2013; Kwon & Kwon, 2013). In the anodal session, tDCS was applied for 13 min. The amplitude strength was 1 mA, with a linear fade-in/fade-out of 15 sec. In the sham condition, the stimulator was maintained in place for 13 min, but the stimulation was ramped down after 30 sec, a duration that has been found not to induce any persistent modulation of neural function (Nitsche & Paulus, 2000).

Immediately after stimulation, the montage was removed and the participant started the experimental block. The 600 trials were completed within approximately 45 min. Importantly, the physiological effects of tDCS have been found to extend approximately 60 min following 13 min of stimulation (Nitsche & Paulus, 2001). Thus, modulation of neural activity by anodal tDCS should extend throughout the entire task.

Fitting of DDM

The full DDM (Ratcliff & Tuerlinckx, 2002) was fit using the partial differential equation method and the Kolmogorov Smirnov statistic, as implemented in the fast-dm package (Voss & Voss, 2007). This package has been found to provide robust fitting routines for the DDM (Ratcliff & Childers, 2015). Because there were 300 trials per experimental cell, there was ample data (Voss & Voss, 2007) to independently fit the data in the four experimental cells (Speed × Accuracy and Sham × Anodal). Six of the seven parameters of the full DDM were estimated separately for all four conditions (drift rate v, threshold b, nondecision time t_0, across-trial variability in drift rate sv, across-trial variability in starting point sz, as well as across-trial variability in nondecision time st_0).

The seventh parameter, the bias parameter z, was fixed at 0.5, based on the assumption that participants were not biased toward the left/right bound in the present experiment. This assumption was justified by the observation that when we grouped correct responses (left responses to left stimuli with right responses to stimuli) and error responses together, there was no left/right bias (the proportion of left responses was not significantly different from 0.5 in any of the data sets; all ps $> .10$).

The quality of the DDM model fits was visually confirmed by plotting empirical RT quantiles against predicted RT quantiles for correct and incorrect trials (see also Ratcliff, Thapar, & McKoon, 2010).

Statistical Testing

We used a series of 2 (Stimulation: tDCS vs. Sham) × 2 (Cue: Speed vs. Accuracy) repeated-measures ANOVAs, using dependent variables of mean accuracy, median RT, and response threshold (parameter b, estimated in the DDM). Accuracy scores were transformed using an arc-sine transformation to make them approximately normally distributed (Winer, Brown, & Michels, 1971).

We corrected the resulting p values for multiple comparisons, using Bonferroni correction. We multiplied the p values by the number of planned statistical tests (9) and restricted their range to (0, 1]. When the Bonferroni-corrected p value was not significant at an alpha level of .05, but the uncorrected p value was significant, we report both values, recognizing that the choice here impacts the probability of Type I and Type II errors. Although the correction is the standard in current research, we also felt it important to present both to avoid a procedure that might bias the results to support the null hypothesis.

To test the amount of evidence in favor of the null hypothesis, we also conducted the ANOVAs within a Bayesian framework, using the default Jeffreys–Zellner–Siow prior (Morey & Rouder, 2015; Rouder, Morey, Speckman, & Province, 2012). Variability across participants was modeled in the ANOVA as a random factor. The Bayes factor for the effect of an experimental factor in the model was computed as the ratio of the likelihood of the full model versus a model in which this factor was omitted. This ratio was determined using 500,000 MCMC samples. Because the motivation behind the data collection is irrelevant in Bayesian statistics (Wagenmakers, 2007), we performed the Bayesian ANOVA over all participants in all data sets at the same time. In these analyses, the location of data collection was entered into the model as a random factor.

All statistical analyses were done in an IPython Notebook (Perez & Granger, 2007) environment using an in-house Python interface for fast-dm, R (version 3.2.0; R Core Team, 2015), and the BayesFactor package (Morey & Rouder, 2015). All analysis code and original data can be found online on the first author's website.

RESULTS

RTs

Median RTs are shown in Figure 1. In all three experiments, RTs in speed-stressed trials were significantly shorter than RTs in accuracy-stressed trials. Overall, the mean RT was 486 msec in the speed trials and 619 msec in the accuracy trials (Berkeley: $F(1, 42) = 59.4, p < .001$; Amsterdam: $F(1, 42) = 64.5, p < .001$; Leiden: $F(1, 39) = 53.4, p < .001$). The Bayesian analysis also indicated that participants responded to the cues in the instructed manner, with decisive evidence in favor of a model including the SAT effect as compared with a model without this factor (Berkeley: $BF_{SAT} = 4881$; Amsterdam: $BF_{SAT} = 4518$; Leiden: $BF_{SAT} = 4718$; all data sets combined: $BF_{SAT} = 1.08e13$).

In the frequentist ANOVA, RTs were not significantly affected by anodal tDCS (Berkeley: $F(1, 42) = 1.97$, $p = 1.0$; Amsterdam: $F(1, 42) = 0.50$, $p = 1.0$; Leiden: $F(1, 39) = 0.24$, $p = 1.0$). Similarly, the Bayesian analysis showed evidence in favor of a model not including tDCS as a factor (Berkeley: $BF_{tdcs} = 0.35$; Amsterdam: $BF_{tdcs} = 0.36$; Leiden: $BF_{tdcs} = 0.39$ for all data sets combined: $BF_{tdcs} = 0.16$).

There were no interaction effects between instruction and stimulation, indicating that the RT advantage on speed trials compared with accuracy trials was similar for the anodal and sham tDCS conditions. Consistent with the results of these ANOVAs, the Bayesian analysis also favored a model that did not include an interaction term (Berkeley: $F(1, 42) = 0.44, p = 1.0$; Amsterdam: $F(1, 42) = 0.020, p = 1.0$; Leiden: $F(1, 39) = 0.032, p = 1.0$; Berkeley: $BF_{tdcs \times SAT} = 0.38$; Amsterdam: $BF_{tdcs \times SAT} = 0.36$; Leiden: $BF_{tdcs \times SAT} = 0.33$; all data sets combined: $BF_{tdcs \times SAT} = 0.22$).

Accuracy

The speed-stressed trials showed consistently lower accuracies than the accuracy-stressed trials, falling on average from 78% to 72% (see Figure 2 for individual data sets). In all data sets, this difference was significant (Berkeley: $F(1, 42) = 14.2, p = .005$; Amsterdam: $F(1, 42) = 21.4, p < .001$; Leiden: $F(1, 39) = 20.5, p < .001$), and the Bayesian analysis showed substantial evidence for a model including a main effect of instruction, an effect seen most clearly when the three data sets were combined ($BF_{SAT} = 0.92$ for the Berkeley data set; $BF_{SAT} = 1.24$ for the Amsterdam data set; $BF_{SAT} = 3.32$ for the Leiden data set; $BF_{SAT} = 57.3$ over all data sets).

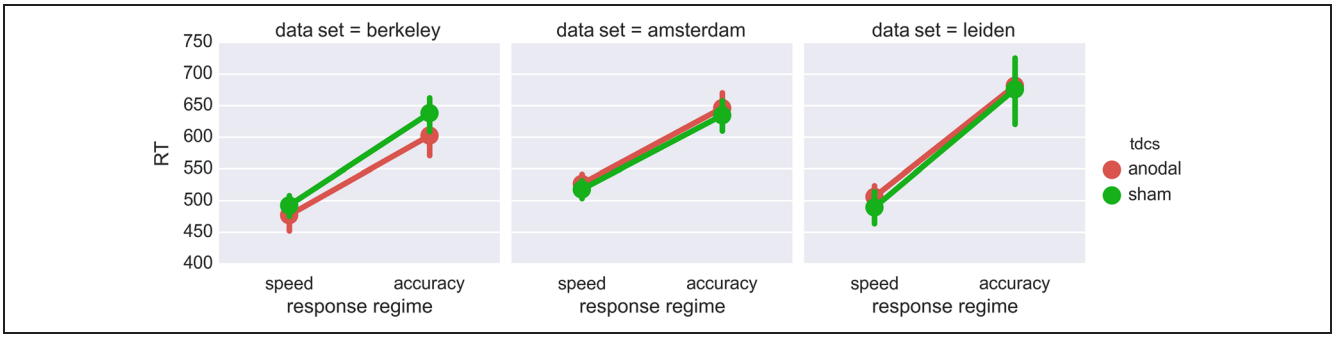


Figure 1. Median RTs. Median RTs for different data sets and experimental conditions. Error bars are bootstrapped standard errors (67% confidence interval). In all three data sets, RTs were significantly lower for the speed condition, but there was no significant effect of tDCS stimulation.

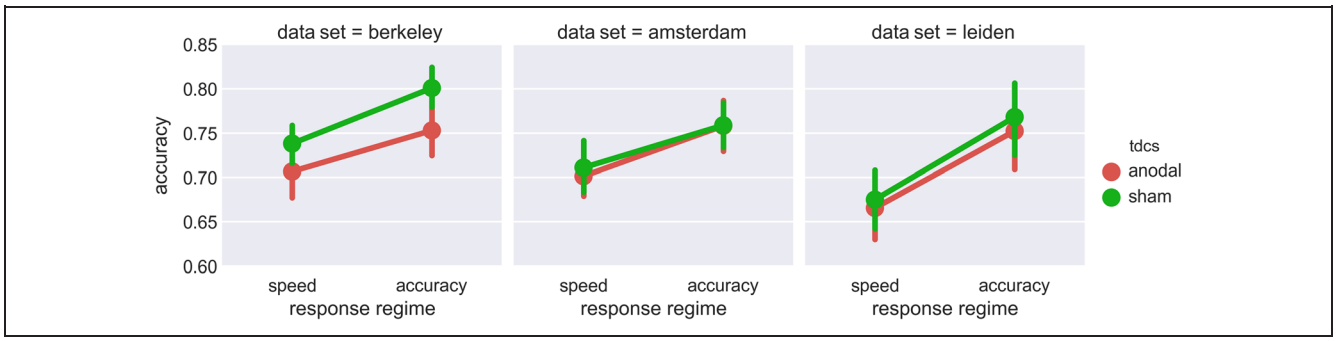


Figure 2. Mean proportion of correct responses. Mean proportion of correct responses. Error bars are bootstrapped standard errors (67% confidence interval). For all statistical analyses, these proportions were first normalized using an arc-sine transformation.

After Bonferroni correction, there was no effect of tDCS on accuracy for all three data sets (Berkeley: $F(1, 42) = 4.48$, $p = .362$; Amsterdam: $F(1, 42) = 0.12$, $p = 1.0$; Leiden: $F(1, 39) = 0.42$, $p = 1.0$). However, without the Bonferroni correction, there was a significant effect of tDCS on accuracy in the Leiden data set ($F(1, 42) = 4.48$, $p = .040$), with tDCS stimulation leading to a decrement in accuracy. The null result held for the Berkeley and Amsterdam data sets. The Bayes factor indicated evidence for the null hypothesis in all data sets (Berkeley: $BF_{tdcs} = 0.38$; Amsterdam: $BF_{tdcs} = 0.2$; Leiden: $BF_{tdcs} = 0.26$). When the data sets are combined, the results revealed substantial evidence in favor of the null hypothesis, namely, that tDCS stimulation did not influence accuracy ($BF_{tdcs} = 0.15$).

There were no significant interaction effects of instruction and tDCS stimulation in the ANOVAs of the accuracy data, and the Bayesian analysis showed little to substantial evidence for a model not including this interaction effect (Berkeley: $F(1, 42) = 0.38$, $p = 1.0$, $BF_{tdcs \times SAT} = 0.35$; Amsterdam: $F(1, 42) = 0.24$, $p = 1.0$, $BF_{tdcs \times SAT} = 0.31$; Leiden: $F(1, 39) = 0.012$, $p = 1.0$, $BF_{tdcs \times SAT} = 0.34$; over all data sets: $BF_{tdcs \times SAT} = 0.20$).

In summary, the overall pattern observed here is similar to that observed in prior studies of the speed–accuracy trade-off in the random dot motion task (Forstmann et al., 2008; Ratcliff & McKoon, 2008). We observed a very robust effect of instructions on RTs (Bayes factor of more than 10 trillion in favor of an effect), as well as a robust effect on accuracy (Bayes factor of 57 in favor of an effect). However, there was essentially no evidence that anodal tDCS induced a consistent change in performance.

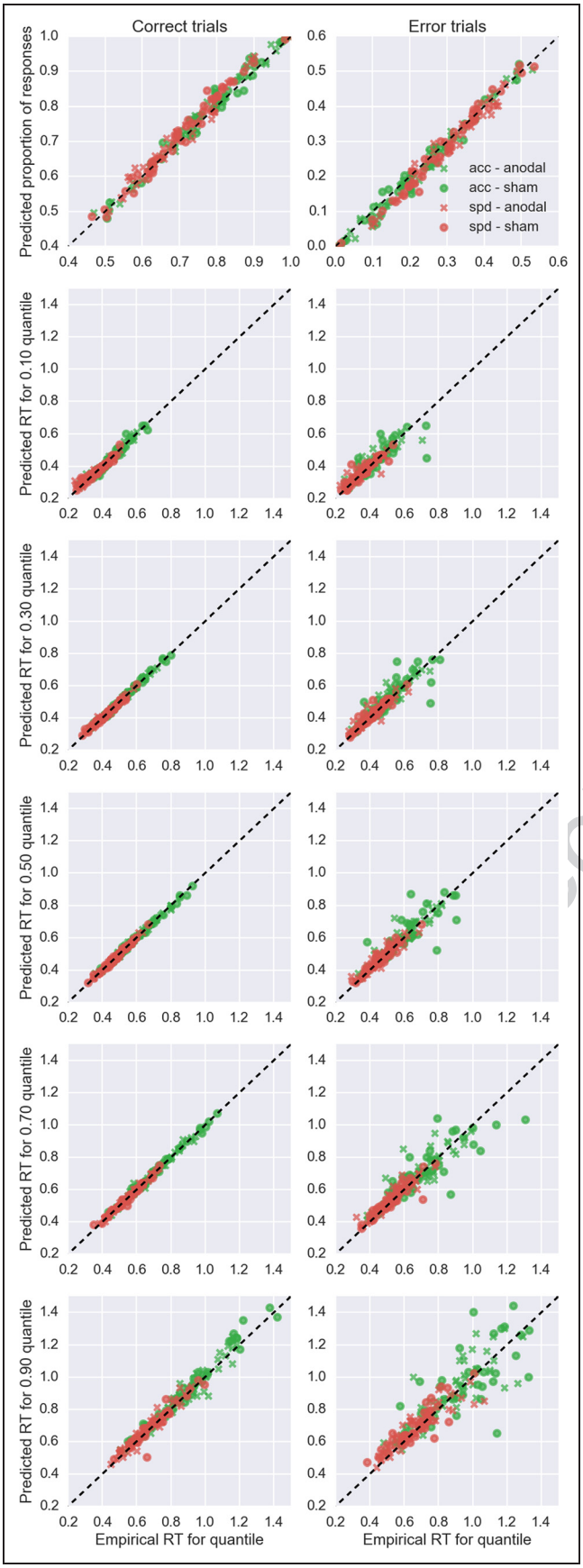
DDM Fits

The DDM was fit to all 44 participants. The mean parameter estimates and their standard deviations are presented in Table 1. Plotting the empirical RT quantiles

Table 1. Mean DDM Parameter Estimates and Standard Deviations

Data Set	*acc_spd*	*tdcs*	t_0	v	a	sv	szr	st_0
Amsterdam	acc	anodal	0.403 (0.070)	1.38 (0.84)	1.10 (0.12)	0.58 (0.19)	0.32 (0.11)	0.209 (0.071)
		sham	0.385 (0.069)	1.40 (0.67)	1.12 (0.18)	0.71 (0.12)	0.32 (0.10)	0.209 (0.082)
	spd	anodal	0.356 (0.043)	1.29 (0.86)	0.89 (0.14)	0.65 (0.20)	0.30 (0.11)	0.174 (0.079)
		sham	0.342 (0.044)	1.28 (0.80)	0.92 (0.14)	0.61 (0.25)	0.33 (0.13)	0.158 (0.075)
Berkeley	acc	anodal	0.385 (0.090)	1.53 (1.32)	1.06 (0.16)	0.62 (0.26)	0.33 (0.13)	0.209 (0.137)
		sham	0.389 (0.083)	1.66 (0.96)	1.14 (0.19)	0.68 (0.14)	0.27 (0.12)	0.203 (0.102)
	spd	anodal	0.334 (0.064)	1.52 (1.35)	0.85 (0.17)	0.69 (0.27)	0.32 (0.11)	0.138 (0.069)
		sham	0.345 (0.059)	1.63 (0.96)	0.86 (0.13)	0.67 (0.23)	0.32 (0.13)	0.147 (0.071)
Leiden	acc	anodal	0.417 (0.110)	1.35 (1.03)	1.21 (0.30)	0.71 (0.37)	0.34 (0.13)	0.244 (0.100)
		sham	0.438 (0.114)	1.44 (1.05)	1.14 (0.32)	0.61 (0.18)	0.34 (0.13)	0.234 (0.119)
	spd	anodal	0.347 (0.079)	1.14 (0.93)	0.86 (0.14)	0.72 (0.26)	0.30 (0.11)	0.137 (0.060)
		sham	0.348 (0.088)	1.18 (0.99)	0.81 (0.11)	0.74 (0.18)	0.28 (0.16)	0.128 (0.063)

Mean parameter estimates (standard deviation) for the different data sets. The bias parameter z was fixed to 0.5. t_0 = nondecision time; v = drift rate (speed of evidence accumulation); a = threshold; sv = intertrial variability in drift rate; szr = intertrial variability in starting point; st_0: intertrial variability in nondecision time.



for all conditions and the predicted quantiles for all conditions showed that the model fits were adequate, with no evident biases. The individual empirical and predicted quantiles are shown in Figure 3. Note that the predictions of the DDM model for the higher quantiles are still unbiased, but more variable than for the other quantiles. This pattern has been observed in previous studies (e.g., Ratcliff & Tuerlinckx, 2002).

DDM Threshold Parameters

Threshold (a)

The main variable of interest in this study was the DDM estimate of the threshold parameter. This parameter represents the amount of evidence accumulated before the participant makes a response. As predicted, the threshold was significantly lower in the speed-stressed condition compared with the accuracy-stressed condition (0.87 vs. 1.13; see Figure 4).This difference was observed in all three data sets (Berkeley: $F(1, 42) = 47.8, p < .001$; Amsterdam: $F(1, 42) = 43.5, p < .001$; Leiden: $F(1, 39) = 49.7, p < .001$). The Bayesian analysis showed a very strong preference for a model including a main effect of speed–accuracy condition on the threshold parameter (Berkeley: $BF_{SAT} = 68{,}060$; Amsterdam: $BF_{SAT} = 17{,}501$; Leiden: $BF_{SAT} = 13{,}276$; over all data sets: $BF_{SAT} = 2.38e{+}14$).

In terms of the core question motivating this study, there was no significant main effect of tDCS, with anodal stimulation producing no change in threshold compared with sham stimulation (Berkeley: $F(1, 42) = 1.76$, $p = 1.0$; Amsterdam: $F(1, 42) = 0.61$, $p = 1.0$; Leiden: $F(1, 39) = 1.38$, $p = 1.0$). The Bayesian analyses weakly preferred a model where the factor tDCS was omitted ($BF_{tdcs} = 0.45$ for the Berkeley data set; $BF_{tdcs} = 0.30$ for the Amsterdam data set; $BF_{tdcs} = 0.37$ for the Leiden data set). When all of the data sets are considered together, there is substantial evidence for the null hypothesis that tDCS does not affect the response threshold ($BF_{tdcs} = 0.16$).

The instruction by stimulation interaction was also not significant (Berkeley: $F(1, 42) = 1.15, p = 1.0$; Amsterdam: $F(1, 42) = 0.00, p = 1.0$; Leiden: $F(1, 39) = 0.07, p = 1.0$). Here the Bayesian analyses showed limited evidence in favor of the null hypothesis (Berkeley: $BF_{tdcs \times SAT} = 0.47$; Amsterdam: $BF_{tdcs \times SAT} = 0.34$; Leiden: $BF_{tdcs \times SAT} = 0.36$), and the combined data sets showed substantial evidence in favor of the null hypothesis, favoring a model without the interaction term ($BF_{tdcs \times SAT} = 0.23$).

Figure 3. Quantile–quantile plots of empirical versus predicted data. Empirical response proportions and RT quantiles for correct and incorrect responses, plotted against the responses proportions and RT quantiles as predicted by the DDM, given the parameters estimated by fast-dm. The plots show that the data fit well by the model, without bias. They also show that the proportion of correct trials, as well as the RT quantiles, are generally lower in the speed-stressed trials.

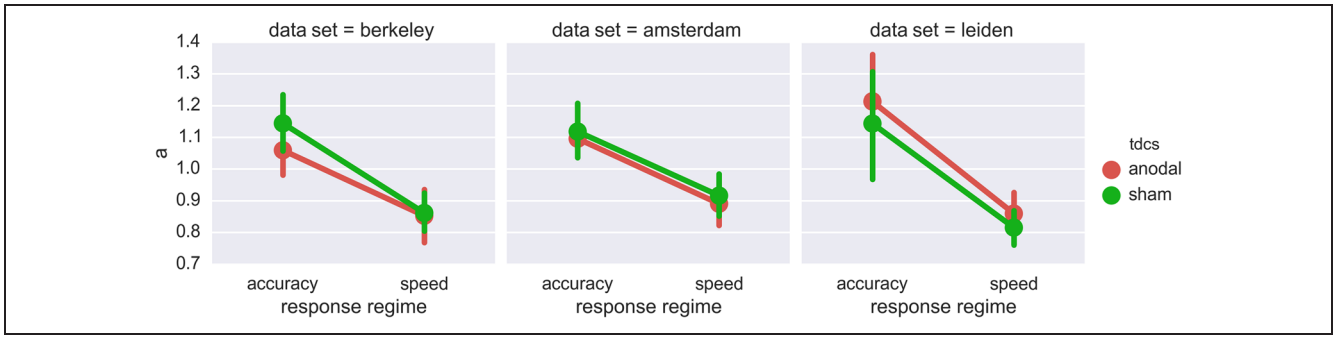


Figure 4. Mean threshold parameter *a* estimates. Error bars indicate bootstrapped standard errors (67% confidence interval).

Nondecision Time (t_0)

The participants showed consistently lower nondecision times on the speed-stressed trials in comparison with the accuracy-stressed trials (Ratcliff & McKoon, 2008; Ratcliff, 2006). The estimated values were 345 and 402 msec for the speed- and accuracy-stressed trials, respectively (Berkeley: $F(1, 42) = 18.6$, $p < .001$, $BF_{SAT} = 3.40$; Amsterdam: $F(1, 42) = 22.4$, $p < .001$, $BF_{SAT} = 11.9$; Leiden: $F(1, 39) = 32.1$, $p < .001$, $BF_{SAT} = 11.5$; over all data sets: $BF_{SAT} = 6186$).

However, there was again no effect of stimulation on nondecision time, with mean values of 373 and 374 msec for the anodal and sham stimulation conditions, respectively (Berkeley: $F(1, 42) = 0.4, p = 1.0, BF_{tdcs} = 0.34$; Amsterdam: $F(1, 42) = 2.7, p = .963, BF_{tdcs} = 0.35$; Leiden: $F(1, 39) = 0.6, p = 1.0, BF_{tdcs} = 0.37$; over all data sets: $BF_{tdcs} = 0.22$), nor was the instruction by stimulation interaction significant (Berkeley: $F(1, 42) = 0.08$, $p = 1.0$, $BF_{tdcs \times SAT} = 0.28$; Amsterdam: $F(1, 42) = 0.02$, $p = 1.0$, $BF_{tdcs \times SAT} = 0.43$; Leiden: $F(1, 39) = 0.5$, $p = 1.0$, $BF_{tdcs \times SAT} = 0.29$; over all data sets: $BF_{tdcs \times SAT} = 0.16$.

Drift Rate (v)

As has been shown before (Forstmann et al., 2008; Ratcliff & McKoon, 2008), drift rate was similar in the speed and accuracy conditions. The mean drift rate was 1.46 for the accuracy-stressed regime and 1.34 for the speed-stressed regime. The drift rate values did not differ in any of the three data sets (Berkeley: $F(1, 42) = 0.06$, $p = 1.0$, $BF_{SAT} = 0.26$; Amsterdam: $F(1, 42) = 1.7$, $p = 1.0$, $BF_{SAT} = 0.29$; Leiden: $F(1, 39) = 2.7$, $p = .936$, $BF_{SAT} = 0.38$; over all data sets: $BF_{SAT} = 0.22$).

Drift rate was also not influenced by tDCS, with mean values of 1.37 and 1.44 for the anodal and sham stimulation conditions, respectively (Berkeley: $F(1, 42) = 1.5, p = 1.0$, $BF_{tdcs} = 0.28$; Amsterdam: $F(1, 42) = 0.03, p = 1.0, BF_{tdcs} = 0.26$; Leiden: $F(1, 39) = 0.203, p = 1.0, BF_{tdcs} = 0.28$; over all data sets: $BF_{tdcs} = 0.18$). There was no interaction between instruction and stimulation on drift rate (Berkeley: $F(1, 42) = 0.003, p = 1.0, BF_{tdcs \times SAT} = 0.35$; Amsterdam: $F(1, 42) = 0.051, p = 1.0, BF_{tdcs \times SAT} = 0.34$; Leiden: $F(1, 39) = 0.03, p = 1.0, BF_{tdcs \times SAT} = 0.36$; over all data sets: $BF_{tdcs \times SAT} = 0.22$).

Exploratory Analyses

Block Analysis

Studies involving tDCS suggest that anodal stimulation of 1 mA for 13 min should produce physiological changes up to 60 min (Nitsche & Paulus, 2001). Participants completed the experiment in approximately 45 min, a duration falling within this window. Nonetheless, we were concerned that the effects of tDCS might be more effective in the beginning of the experiment. To examine this question, we split the data into four blocks of 150 trials each, asking whether there was an interaction between stimulation type (anodal vs. sham) and block on RT or accuracy.

This interaction was not evident in any of the data sets for the RT data (all Fs < 0.48, $p > .05$; Bayesian ANOVA on combined data showed that the model not including this interaction factor is 49.3 times more likely than the full model) and for the accuracy data (all Fs < 0.12, $p > .05$ Bayesian ANOVA without interaction factor is 39.7 times more likely than full model). The three-way interaction Stimulation × Cue × Block was also not significant for any of the conditions for RT (all Fs < 0.118, $p > .05$; Bayesian ANOVA without interaction factor is 23.7 times more likely than full model) and accuracy (all Fs < 0.177; $p > .05$; Bayesian ANOVA without interaction factor is 25.2 times more likely than full model). These results clearly indicate that effects of tDCS were not selectively effective at the beginning of the experiment.

Cluster Analysis

A second post hoc hypothesis was that the effect of tDCS might be overshadowed by individual variability in sensitivity to electrical stimulation (Labruna et al., 2015). As estimated by the DDM, 25 of the 44 participants (57%) showed a lower response threshold following anodal stimulation compared with sham stimulation. It is possible that the participants in this subgroup were more sensitive to effects of tDCS. Factors such as hair thickness,

skin condition, and the shape of the skull can influence the effectiveness of tDCS (Rossini et al., 2015; Opitz et al., 2013), and it has recently been shown that sensitivity to anodal tDCS is related to sensitivity to TMS (Labruna et al., 2015).

To examine this issue, we divided the participants into a group of "responders" and "nonresponders" based on the effect of tDCS stimulation condition on the DDM response threshold parameter. We did so using a Gaussian mixture model, fit with either one or two clusters on these effect sizes. We compared the two models using the Bayesian Information Criterion (BIC) and Schwartz weights to see which was most likely given the data (Vandekerckhove, Matzke, & Wagenmakers, 2015). If there exists a subset of nonresponders, one would expect the two-cluster model to explain the data better than the model with only one cluster.

The results showed that the one cluster model outperformed the two-cluster model with a BIC of −44.5 vs. a BIC of −33.6. These two BICs correspond to a Schwartz weight of 9.95E−13, which means that the model with one cluster is approximately 1,000,000,000,000 times more likely than a two-cluster model (see also Figure 5). In summary, the results provide no evidence for responder and nonresponder subgroups.

Test–Retest Reliability

The primary dependent variable in this study was the threshold parameters, estimated with the DDM. To assess the reliability of this measure, we correlated the estimated values for Session 1 and Session 2. Test–retest reliability was high: For the accuracy-stressed condition, the correlation was .72; for the speed-stressed condition, the correlation was .68. Divided further into the three data sets (and two instruction conditions), the test–retest correlations ranged from .40 to .83. A Bayesian linear model with data set, instruction, and participant as random factors suggested that a model with a linear relationship of the threshold parameter between the two sessions is more than 21 million times more likely than a model without this relationship. These results show that it is unlikely that a possible effect of tDCS was overshadowed by excessive intraindividual variability.

Session and Order Effects

We also assessed if the effect of tDCS might be overshadowed by a session or order effect. Independent from the other factors, participants' performance improved in Session 2 compared with Session 1. In the accuracy-stressed condition, participants were on average 57 msec (SD = 73 msec) faster in the second session and made 1.2% less errors (SD = 7.4%). In the speed-stressed condition, they were 32 msec faster (SD = 40.0 msec) and made 0.4% (SD = 6.9%) less errors. The effect of session on RT was significant in two data sets (Berkeley: $F(1, 42) = 4.0$, $p = .0517$; Amsterdam: $F(1, 42) = 9.5$, $p = .004$; Leiden: $F(1, 39) = 5.6$, $p = .023$) and a Bayesian ANOVA favored a model including this effect with a factor of 7.1. However, accuracy was not significantly different across sessions (all ps > .05; Bayes factor is 2.8 in favor of the null model).

The lower RTs, coupled with minimal change in accuracy, led to lower estimates of the response threshold in Session 2. In the accuracy-stressed condition, the threshold dropped from 1.16 (SD = 0.20) in the first session to 1.09 (SD = 0.23) in the second session. In the speed-stressed condition, the decrease went from 0.90 (SD = 0.12) to 0.83 (SD = 0.14). This main effect of session on threshold was significant in the Amsterdam, but not the other data sets (Berkeley: $F(1, 42) = 2.41$, $p = .128$; Amsterdam: $F(1, 42) = 6.48$, $p = .015$; Leiden: $F(1, 42) = 2.65$, $p = .111$). The Bayesian ANOVA favored a model that includes session, with a Bayes factor of 3.0. However, there were no interactions between session and stimulation type for RT, accuracy, and threshold (all ps > .05, all BFs < 0.4).

In a second-order analysis, we also considered whether the first session involved anodal or sham stimulation. Here we used thre-way ANOVAs with the factors stimulation order, session, and instruction, with the first factor between participants (anodal first or sham first) and the other two factors, within participants. Importantly, there was no significant effect of order on the threshold estimates (all ps > .45; Bayes factor of 4.9 in favor of a model without a main effect of order), nor was there a significant Order × Session interaction (all ps > .17; Bayes factor of 3.4 in favor of a model without this interaction). There were also no interactions involving stimulation order on the RT (all ps > .27; Bayes factor 4.5 in favor of no interaction) and accuracy data (all ps > .32; Bayes factor 2.8 in favor of no interaction).

In summary, even when the improvement in performance across sessions is taken into account, there is no significant effect of tDCS condition in any of the data sets.

DISCUSSION

A growing body of empirical results from multiple methods as well as computational models, have articulated a critical role for the pre-SMA in decision-making (Frank et al., 2015; Boehm et al., 2014; Mansfield et al., 2011; van Maanen et al., 2011; Forstmann et al., 2008, 2010). These findings led us to ask if anodal tDCS stimulation over the pre-SMA would influence performance on a perceptual discrimination task. Specifically, we sought to provide causal evidence for the role of this brain region in modulating the decision threshold as people varied their performance to emphasize speed or accuracy in judging the direction of a moving dot pattern. We chose this task because it is very amenable to both performance- and model-based analyses. Moreover, it allowed us to compare two versions of the threshold hypothesis, one in which pre-SMA neurons

directly instantiates a decision threshold and the other in which pre-SMA modulates downstream areas that instantiate a threshold (van Maanen et al., 2011; Bogacz et al., 2010).

In three experiments, conducted with different samples and in different laboratories, we failed to find any consistent change in performance following anodal tDCS over pre-SMA, relative to sham stimulation. Although caution is always required when drawing conclusions about the lack of an effect, the results of our Bayesian analyses provide substantial evidence in favor of the null hypothesis. We failed to observe a main effect of tDCS stimulation on DDM parameter estimates of the response threshold. More concretely, the null hypothesis was 5.9 times more likely than the alternative hypothesis across the three experiments. A similar result was obtained in the interaction test where we ask whether tDCS had a differential effect on the speed or accuracy condition (instruction by stimulation interaction). Here the null hypothesis was 4.2 times more likely than the alternative hypothesis. Additional analyses showed that this null result could not be explained by a decrease of tDCS efficacy over the experimental session or due to changes in performance from practice.

One possible explanation for these null findings is that the pre-SMA is not causally involved in modulating the response threshold in perceptual decision-making tasks. This conclusion would stand in contrast to the impressive evidence from fMRI studies. Although fMRI is not well suited for drawing causal inferences—and indeed, one of the motivating reasons for our study—the BOLD response in these studies has been shown to vary with response threshold, both in comparisons between individuals (Mansfield et al., 2011; Forstmann et al., 2008) and in trial-by-trial fluctuations (Boehm et al., 2014; van Maanen et al., 2011). Moreover, these findings are in line with prominent neurocomputational models that make specific predictions for the role of the pre-SMA in speeded decision-making tasks (Bogacz et al., 2010; Frank, 2006).

Another possibility is that the tDCS protocol employed in this study was not effective in modulating cortical activity in the pre-SMA; as such, our experimental manipulation was not sufficiently sensitive to induce behavioral changes. This insensitivity may reflect a general limitation with tDCS in altering cognitive processes. This would be consistent with the results of two recent meta-analyses (Horvath et al., 2014, 2015); on the other hand, several critiques of such meta-analyses have been published (Antal, Keeser, Priori, Padberg, & Nitsche, accepted; Nitsche et al., 2015), and it seems premature to dismiss tDCS as a tool for manipulating brain function.

More specific limitations may be related to our particular protocol. The placement of the active electrode at FCZ was informed by neuroimaging (e.g., Forstmann et al., 2008) and EEG studies of the SAT (Boehm et al., 2014). Boehm and colleagues (2014), using a near-identical task as that employed here, reported that electrode FCZ was most related to trial-to-trial variability in response threshold. Other tDCS studies that have been successful in modulating behavior have targeted pre-SMA with either identical electrode placements (Spieser et al., 2015) or with the anode in a neighboring location (Liang et al., 2014; Kwon & Kwon, 2013; Hsu et al., 2011). Given the spatial resolution of tDCS (Tremblay et al., 2014; Wassermann, Epstein, & Ziemann, 2008; Miranda, Lomarev, & Hallett, 2006), the differences between these locations are likely negligible. Most important, with these configurations, anodal tDCS has been found to modulate performance on tasks putatively linked to pre-SMA (see Table 2). For example, participants were more efficient in inhibiting a prepotent response after anodal stimulation over pre-SMA compared with sham stimulation (Hayduk-Costa et al., 2013; Kwon & Kwon, 2013).

Because research resources are limited, we limited our study to anodal tDCS and did not test the effect of cathodal tDCS. We opted for this approach given that cathodal tDCS is generally not as effective as anodal tDCS in modulating behavior (Jacobson, Koslowsky, & Lavidor, 2011). Indeed, in tDCS studies targeting pre-SMA, behavioral effects were limited to anodal stimulation; cathodal produced similar effects as sham stimulation (Hayduk-Costa et al., 2013; Hsu et al., 2011). However, one recent tDCS study by Spieser et al. (2015) showed the exact opposite pattern, with cathodal stimulation over pre-SMA enhancing performance on a measure of cognitive control. The improvement here might be related to a decrease in motor excitability given that the benefits were limited to trials in which participants were less likely to make fast, prepotent responses. Given these results, it would be useful to revisit our task and examine the effects of cathodal stimulation.

Another important methodological issue to consider are the choices made concerning the timing (online or offline), intensity, and duration of stimulation. The effects of increased stimulation intensity and duration on brain function are nonlinear, and there are even reports that the effects can be nonmonotonic, reversing at high levels of stimulation (Batsikadze, Moliadze, Paulus, Kuo, & Nitsche, 2013). In terms of timing, we opted to use an offline procedure, completing the tDCS phase before participants started the task. Our protocol is similar to that employed in other studies of frontal lobe function (Tremblay et al., 2014), including pre-SMA (Liang et al., 2014; Hayduk-Costa et al., 2013; Kwon & Kwon, 2013; Hsu et al., 2011). Nonetheless, it is possible that we would have observed a different pattern of results with another protocol. Of course, it is not practical to do parametric manipulations in every study. We need to recognize that, as with any new procedure, there will be a "learning period" in which researchers explore different protocols. This point also underscores the importance of publishing null results, allowing the scientific community to be aware of the conditions that produce both positive and negative effects.

Table 2. Stimulation Protocols and Behavioral Results of tDCS Studies Targeting the Pre-SMA

Article	*n*	*Within/ Across Subjects?*	*Area*	*Task*	*Stim*	*Sponge Size*	*Ref*	*Stim Intensity*	*Online/ Offline*	*Stim Duration*	*Result*	*Anodal Stimulation Pre-SMA Functioning*	*Cathodal Stimulation Pre-SMA Functioning*
Spieser et al. (2015)	24	within	SMC	Simon task	FCZ	7 × 5 cm	Left cheek	1 mA	Combined	20 min	Cathodal decreased fast overt errors. Anodal did nothing.	No effect	Up
Hsu et al. (2011)	14	within	pre-SMA	Stop signal	FZ	4 × 4 cm	Left cheek	1.5 mA	Offline	10 min	Anodal increased stopping efficiency. Cathodal impaired.	up	Down
Kwon and Kwon (2013)	40	within	pre-SMA	Stop-signal	4 cm anterior to CZ	35 cm^2	"contralateral" Supraorbital area	1 mA	Offline	10 min	Anodal increased stopping efficiency.	up	n.a.
Hayduk-Costa et al. (2013)	12	within	SMA	Anticipation stop signal	1.8 cm anterior to CZ	7.8 cm^2	"centrally on the forehead directly above the eyebrows"	1 mA	Offline	10 min	Anodal decreased stopping efficiency. Cathodal did nothing Anodal stimulation also reduced 'GO-trimes'/ response intitation.	down	No effect
Liang et al. (2014)	18	Within	pre-SMA	Stop signal	FZ	4 × 4 cm	Left cheek	1.5 mA	Offline	10 min	Anodal stimulation increases stopping efficiency.	up	n.a.

Conclusion

We report three independent experiments that examined the effects of anodal tDCS over pre-SMA on performance in a speeded perceptual decision-making task. We expected that stimulation would influence how the participants alter their response threshold when balancing demands on speed and accuracy. However, across all three studies, the results provide strong evidence that anodal tDCS had no consistent impact on performance in this task. Our findings are particularly interesting in light of the recent concerns with the efficacy of tDCS to modulate cognitive function (Horvath et al., 2014, 2015; Tremblay et al., 2014). The utility to tDCS as a tool for cognitive neuroscience will surely benefit from the dissemination of reports, both positive and negative, concerning studies that look at a range of task domains and stimulation protocols.

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