Site-specific effects of transcranial direct current stimulation on motor function in Parkinson's disease: a systematic review and meta-analysis

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ABSTRACT AND KEYWORDS

We aimed to compare the effect of transcranial direct current stimulation (tDCS) compared with sham or conventional interventions on motor functions and activity in individuals with Parkinson Disease (PD). Two independent reviewers searched four databases (Pubmed, Embase, Scopus and Cochrane Library) from inception to April 2026. We only included RCTs comparing active tDCS (alone or with training) versus sham tDCS (alone or with training) on walking speed, functional mobility, Parkinson motor symptoms, activities of daily living (ADLs), quality of life (QoL), dropouts, and adverse events. Two authors independently extracted data including study source and design, participant and intervention characteristics, and outcomes. We computed a mean difference (MD) with a random-effects model. Subgroup analyses were performed for outcomes with ≥10 experimental study arms, accounting for intervention protocol and stimulation site. We assessed the risk of bias using ROB 2 tool, and the certainty of evidence using GRADE. Seventeen RCTs (n=553) were included. Pooled analyses showed little to no effect on walking speed (MD 0.04 ms^{-2}, 95%CI -0.03–0.11), functional mobility (MD 0.67 s gained on the Timed Up and Go test, 95%CI -0.64–2.02), and other outcomes. Subgroup analyses based on stimulation site and intervention protocol revealed no differences. Adverse events were minor and infrequent, and dropout rates did not differ between groups. The overall certainty of evidence ranged from very low to low. Current evidence suggests that the effect of tDCS on gait, ADLs, or QoL is probably not superior to other conventional interventions in individuals with PD.

Keywords: tDCS; Parkinson Disease; rehabilitation; meta-analysis

INTRODUCTION

Parkinson's disease (PD) is a progressive neurodegenerative disorder affecting more than 10 million people worldwide and commonly associated with gait and balance impairments that reduce activities of daily living (ADLs) and quality of life (QoL) [1–3].

Current treatments are mainly pharmacological and aim to compensate for dopaminergic loss. Although they can improve several motor and non-motor symptoms, their effectiveness may decrease over time and side effects may occur. Consequently, non-pharmacological interventions are increasingly used as complementary approaches. Rehabilitation is recommended to enhance independence and delay functional and cognitive decline in PD [4], typically including gait and balance training, physical activity, and occupational therapy [5].

In parallel, non-invasive brain stimulation techniques such as transcranial direct current stimulation (tDCS), have emerged as potential adjunctive therapies. tDCS is a non-invasive and painless brain stimulation treatment that uses direct electrical currents to stimulate specific parts of the brain by delivering a low-intensity electrical current through two electrodes placed on the head [6]. Anodal stimulation increases cortical excitability whereas cathodal stimulation decreases it [7]. tDCS is considered safe, with no serious adverse effects reported in healthy or neurological populations [8]. Randomized controlled trials (RCTs) suggest that repeated sessions of tDCS may enhance cognitive functions, particularly working memory and executive control, in individuals with PD [9,10].

Beyond cognition, a growing number of RCTs have also explored the effects of tDCS on motor outcomes in individuals with PD, including gait performance, balance and upper limb function [11–15]. While several studies report promising improvements, findings across trials remain inconsistent, with variable effect sizes and heterogeneous outcomes.

Recent systematic reviews have reported conflicting results, limiting clear clinical recommendations [12,16–19]. Most of these reviews [12,18,19] pooled heterogeneous protocols, despite evidence suggesting that stimulation site may influence treatment effect [20]. In addition, some did not perform meta-analysis and included heterogeneous neurological populations [16], while others relied on earlier risk of bias tools [12,16,17,19], and none assessed the certainty of evidence [12,16–19]. Moreover, outcomes such as treatment adherence, QoL, and ADLs have not been synthesized. Since the publication of the last meta-analysis in 2024 [18], several RCTs have investigated the effects of tDCS on motor function in individuals with PD, providing new insights that warrant systematic evaluation [21–23]. These newer studies [21–23] increased the number of available comparisons per outcome, allowing for more robust and statistically meaningful analyses, including subgroup analyses. In addition, they explored different stimulation sites, enabling comparisons of their relative effectiveness. Importantly, they also reported outcomes related to ADLs and QoLs, which were not examined in previous reviews, thereby offering a more comprehensive assessment of the clinical impact of tDCS.

Therefore, this systematic review and meta-analysis aims to determine whether tDCS, either as a standalone treatment or in combination with conventional rehabilitation therapies, improves motor function, ADLs and QoL in individuals with PD. In addition, we investigate whether stimulation site (anode location) and intervention protocol influence treatment effectiveness and safety.

METHODS

We conducted a systematic review with meta-analysis following the guidelines of the Cochrane Handbook and the Preferred Reporting Items for Systematic Reviews and Meta-Analyses [24]. Our study protocol was pre-registered in PROSPERO (CRD42023494856). The original protocol specified that this review would evaluate the effects of tDCS in individuals with major neurocognitive disorders. However, we identified that the vast majority of eligible RCTs were conducted in individuals with PD, with little to no eligible evidence available for other major neurocognitive disorder populations. To ensure methodological coherence and avoid an imbalanced and potentially misleading synthesis, we refined the review objective to focus specifically on individuals with PD.

Criteria for considering studies for this review

Types of studies

We only included RCTs. Cross-over RCTs were excluded due to the potential for carryover effects, particularly relevant for tDCS interventions where neuromodulatory after-effects may persist beyond the washout period, as well as possible period effects in PD. Cluster RCTs were excluded due to the need for adjustment for intra-cluster correlation coefficients, which were not consistently reported, and to avoid unit-of-analysis errors. We excluded studies if they were protocol trials, conference abstracts or if author retraction, fraud suspicion or limitations called into question the validity of these studies.

Types of participants

We included studies reporting on adults (aged > 18) who had been diagnosed with PD. There were no restrictions regarding age, sex, ethnicity, deficits, and time since diagnosis.

Types of interventions

We included studies in which the experimental group received a tDCS intervention targeting motor function improvement with no restrictions on dose, intensity, duration, mode

of administration, or intervention schedule, either as an addition to or a substitute for conventional treatment. We considered all types of coupling between tDCS and conventional treatment.

We included studies in which the control group received either usual care or sham tDCS. There were no restrictions on dose, intensity, duration, mode of administration, or intervention schedule, either as an addition to or a substitute for conventional treatment. We considered all types of coupling between sham tDCS and conventional treatment.

Types of outcome measures

We selected the studies that reported at least one of the following outcomes: functional mobility, walking speed, disease progression-related motor impairments, ADLs, QoL, treatment safety, and adherence to the intervention.

Search methods for identification of studies

We searched for RCTs in four databases: Cochrane Central Register of Controlled Trials in the Cochrane Library, MEDLINE PubMed, Embase Elsevier and Scopus Elsevier. The complete search strategies are detailed in Table 1. We completed all the searches up until April 7th, 2026.

Additionally, we manually reviewed the reference lists of all included studies, as well as relevant reviews identified during the search process. Furthermore, we conducted searches on Google Scholar to identify any studies that may have been overlooked or not indexed in the databases.

Data collection and analysis

Selection of studies

We retrieved all references from the databases and exported them to a reference management software (EndNote X9). After duplicate removal, we initiated the first screening based on titles and abstracts. Two independent authors (JT and MB) reviewed and selected the

trials according to the eligibility criteria. Any disagreements were resolved through mutual consensus with the assistance of a third author (GE) to reach a final decision. Full-text articles were then screened following the same procedure.

Data extraction and management

Two authors (JT and MB) independently extracted data from RCTs that met the inclusion criteria for full-text selection, including study source (authors, year, and country of publication), study design, participant characteristics (number of participants, mean and standard deviation of participant age, types of cognitive impairment), intervention characteristics (electrode placement, intervention frequency, duration and follow-up, treatment combination), and outcomes. When the information was not found in the full-text, authors were contacted. A third author (GE) exported all the data into RevMan 5.3. software.

Assessment of risk of bias in included studies

We used the ROB 2 tool to assess the risk of bias in the included trials. This version comprises five domains for evaluating the risk of bias for each variable: bias arising from the randomization process, deviations from intended interventions, missing outcome data, outcome measurement, and selection of the reported result. We focused on walking speed outcomes. However, when trialists did not measure walking speed, we considered their primary outcomes.

The risk of bias in the included studies was assessed by two authors (JT and MB), with discrepancies resolved through mutual consensus before reaching a final decision with the assistance of a third author (GE). We calculated an overall risk of bias using the excel tool (available at riskofbiasinfo.org).

Measures of treatment effect

To assess differences in treatment effectiveness between tDCS and control conditions, we compared post-intervention outcomes at the end of treatment. According to the Cochrane

Handbook (Chapter 10.5.2), in RCTs, post-intervention values and changes from baseline scores generally estimate the same underlying intervention effect, and change scores should not be preferred solely to adjust for chance baseline differences. For continuous outcomes, we reported treatment effects as mean differences (MD) with 95% confidence interval (CI) when outcomes were measured using the same scale. When different scales were used to assess a given outcome, we used a standardized mean difference (SMD) with 95% CI. To enhance clinical interpretability, in accordance with the Cochrane Handbook (Chapter 15.5.3.2), SMD estimates were additionally re-expressed as MD by multiplying the SMD and corresponding 95% CI by a representative standard deviation, calculated as the average of baseline standard deviations from included studies using a common scale for each outcome. For dichotomous outcomes, we reported the treatment impact using a risk difference (RD) and 95% confidence interval.

Unit of analysis issues

For multiple-arm studies with multiple relevant intervention groups, we used the splitting approach [25]. This method divides the shared control group's sample size equally. This ensures a direct comparison between each intervention and part of the shared group.

Dealing with missing data

When trialists reported standard errors or confidence interval, we converted them to standard deviations using the approach recommended in the Cochrane Handbook. In a similar way, we converted medians and quartiles to means and standard deviations using the approach of Wan et al [26].

Assessment of heterogeneity

From a statistical perspective, we assessed heterogeneity between trials using the I^2 test. Following the Cochrane Handbook recommendations, heterogeneity was considered non-

189 significant for $I^2<30\%$, moderate for $30\%\leq I^2<50\%$, substantial for $50\%\leq I^2<75\%$, and
190 considerable for $I^2>75\%$.
191 From a methodological perspective, we used sensitivity analyses (see section below).
192 ***Assessment of reporting bias***
193 To assess reporting biases, we compared the information in each trial's published
194 protocol, available in clinical trial registries, with the data reported in the methods and results
195 sections of associated publications.
196 If more than 10 studies reported the same outcome, we used a funnel plot to assess
197 small study effects and potential publication bias through symmetry analysis. If asymmetry or
198 outliers were detected, we performed the Egger regression test to evaluate publication bias
199 [27].
200 ***Data synthesis***
201 We used the statistical tool RevMan 5.3 to perform a quantitative synthesis of the
202 available data. All relevant studies with available results were included in this quantitative
203 synthesis. When at least two studies assessed the same construct using identical units and
204 comparable measurement methods, we conducted a meta-analysis using forest plots, which
205 allow for comparisons of results from studies on the same topic. For continuous outcomes, we
206 used a random-effects model as between-studies heterogeneity was expected. For
207 dichotomous outcomes with low event rate, we used a fixed-effects model, as recommended
208 by the Cochrane Handbook.
209 When the number of studies per variable was less than two, studies were not
210 sufficiently similar, or the heterogeneity was considerable (even after outlier removal), we
211 performed a narrative synthesis.
212 ***Subgroup analysis, meta-regression and investigation of heterogeneity***
213 If data from at least 10 experimental study arms (experimental intervention group
214 within a RCT) were available, we conducted subgroup analyses using the formal test for

subgroup differences in RevMan. Specifically, we aimed to examine (1) the influence of tDCS intervention protocol on outcomes, comparing its use as an adjunct to conventional therapy versus as a substitute, and (2) the influence of anode location on outcomes. In addition, if data from at least 10 experimental study arms were available, a meta-regression was conducted at the study level to examine whether total intervention dose (in min) was associated with treatment effect size (SMD). Inverse-variance weighted linear meta-regression was fitted, with study SMD as the dependent variable and total dose as the continuous moderator. For interpretability, regression slopes were expressed per 100 min dose units. Pearson's r and Spearman's ρ were reported as descriptive association measures.

Sensitivity analysis

When sufficient study arms were included in pooled analysis ($n > 3$), we performed sensitivity analysis to assess the robustness of the following analytical choices: (1) to include studies at low risk of bias, those with some concerns and high risk of bias, (2) to include studies with imputed data, (3) to exclude outlier studies, (4) to include studies in which the outcome was not defined as a primary outcome. A minimum of three study arms was required to avoid unstable or potentially misleading results that could arise from analyses based on very small numbers.

Assessment of the certainty of evidence

We applied the GRADE approach to assess the certainty of evidence for each analysis. This method assigns a level of certainty (very low, low, moderate, or high) to the results based on study limitations, risk of bias, inconsistency, indirectness, imprecision, and publication bias. For each domain, we downgraded the certainty of the evidence by one or two levels in case of serious or very serious concerns, respectively.

RESULTS

Description of studies

Results of the search

The initial database search enabled us to identify 3853 references, along with six additional studies retrieved via Google Scholar. During the duplicate's removal process, we eliminated 698 references. We then excluded 2835 articles during the title and abstract screening (Figure 1). After assessing the full texts of 227 articles, we excluded 209 additional studies. In total, our meta-analysis included 17 different RCTs, representing 18 records [21–23,28–42].

Participants

As presented in Table 2, the studies analysed in this meta-analysis were conducted between 2010 and 2025. In total, 553 patients were included, with sample sizes ranging from 17 to 73 participants. The mean (SD) age ranged from 50.1 (2.4) to 75.0 (4.6) years in the intervention group, and from 58.3 (8.0) to 73.7 (5.0) years in the control group.

Interventions

The tDCS administration protocol varied across studies. In most studies, the intensity was set at 2 mA throughout the treatment duration for the experimental group. The intervention duration ranged from a single session to 20 sessions (more details in Table 2).

In most cases, tDCS treatment was combined with another treatment such as walking (n=5 [one RCT, two records]) [21,32,33,37,39,41], various motor exercises (n=7) [22,23,31,34–36,42], and writing (n = 1) [30]. Training was designed for upper limbs in two studies [30,36], for lower limbs in six studies [21,31–33,39,41,42] and was general in five studies [22,23,34,35,37]. Four studies [28,29,38,40] administered tDCS alone.

The electrode placements also varied across studies. We grouped them into four categories based on the anode's location. In fourteen studies [22,23,28–37,39,41,42], the

anode was placed over the motor cortex, with the cathode placed over the mastoids (n=2) or on the contralateral (n=11) or ipsilateral (n=1) supraorbital region. Three studies [38,40,41] targeted the dorso-lateral prefrontal cortex (DLPFC) with the anode, with the cathode placement varying from the contralateral supraorbital region (n=2) to the bilateral leg region on M1 (n=1). Two studies [21,40] also targeted the cerebellum with the anode placed on the contralateral supraorbital region.

Comparisons

For control groups providing sham tDCS, the intensity was initially equivalent, but the current gradually decreased to 0 mA after ten, thirty, or sixty seconds, depending on the study.

Five studies (six records) combined sham stimulation with gait training [21,32,33,37,39,41], one with writing activity [30], seven with various motor activities [22,23,31,34–36,42] and four studies provided sham stimulation alone [28,29,38,40].

The number of sessions ranged from one to 20, distributed over 1 day to 7 weeks (more information in Table 2).

Outcomes

As presented in Table 2, twelve studies reported measuring walking speed [22,28,31,33–35,37–42]. Functional mobility was measured in eight studies using the Timed Up and Go test (TUG) [21,22,33,38–41] or the Korean version of the Functional Gait Assessment [37]. Parkinson motor symptoms were measured in nine studies using the Unified Parkinson's Disease Rating Scale (UPDRS) III [21,22,28,33–35,38,42] or its Korean version [37]. Activity of daily living outcomes was reported in two studies using the Functional Independence Measure [35] or the ADL test [21]. Quality of life was measured in four studies using the SF12-v2 [28] and the Parkinson’s Disease Questionnaire-39 (PDQ-39) [22,33,41]. Regarding treatment adherence, fourteen studies reported the number of participants who discontinued the intervention [21–23,28–33,37–42].

Ethics, funding and conflict of interest

All studies reported having obtained the approval from an ethical committee. Fundings were mentioned in twelve studies [21,22,28–30,35–37,39–42] and trialist of four studies [21,28,35,38] declared having a potential competing interest.

Risk of bias in included studies

We were able to assess the risk of bias for walking speed in eight studies [28,31–34,39–42]. For the remaining studies, we therefore assessed the risk of bias based on their primary outcomes. As presented in Figure 2, no study was at low risk of bias, nine had some concerns [23,28–31,34,35,39,41], and eight were at high risk of bias [21,22,32,33,36–38,40,42].

Synthesis of results

Walking speed

Ten studies reported measures of walking speed. Pooled analysis revealed little to no effect of tDCS on walking speed (MD 0.03 ms^{-2}, 95% CI -0.01 to 0.08 ms^{-2}; SMD 0.18, 95% CI -0.05 to 0.42; 14 arms; 303 participants; Figure 3), but the evidence is very uncertain. There is low heterogeneity (I^2=0%). We rated the certainty of evidence as very low due to risk of bias in included studies and very serious imprecision.

Sensitivity analyses

Exclusion of five studies with high risk of bias left five remaining studies. Pooled analysis of these studies showed no significant difference of effect between tDCS and other interventions (MD 0.04 ms^{-2}, 95% CI – 0.03 to 0.11 ms^{-2}; SMD 0.21, 95% CI -0.13 to 0.55).

Exclusion of two studies with imputed data left eight remaining studies. Pooled analysis of these studies showed no significant difference of effect between tDCS and other interventions (MD 0.04 ms^{-2}, 95% CI – 0.01 to 0.09 ms^{-2}; SMD 0.19, 95% CI -0.06 to 0.44).

Exclusion of four studies where walking speed was not listed as a primary outcome left six remaining studies. Pooled analysis of these studies showed no significant difference of effect between tDCS and other interventions (MD 0.05 ms^{-2}, 95% CI – 0.01 to 0.10 ms^{-2}; SMD 0.23, 95% CI -0.05 to 0.51).

Subgroup analyses

We compared the studies in which tDCS was the sole vs one of the treatments (5 vs 9 experimental study arms). The test for subgroup difference did not indicate a difference between subgroups (Chi^2=0.47; P=0.49; Figure 3(a)).

As presented in Figure 3(b), we also compared tDCS effect when the anode was placed over the motor cortex (11 experimental study arms) vs the DLPFC (2 experimental study arms) vs the cerebellum (1 experimental study arm). The test for subgroup difference did not indicate a difference between subgroups (Chi^2=0.97; P=0.62).

Meta-regression

As presented in Figure 4(a), the meta-regression slope was -0.04 SMD per 100 dose units (95% CI -0.15 to 0.07), suggesting no clear dose-response relationship. Correlation estimates were small and non-significant (Pearson r -0.18, P=0.533; Spearman ρ -0.16, P=0.585).

Functional mobility

Seven studies reported measures of functional mobility. All studies except one [37] used the TUG. The other scale used was the Korean version of the Functional Gait Assessment.

Pooled analysis revealed little to no effect of tDCS on functional mobility (MD 0.67 s faster TUG, 95% CI 0.64 s slower to 2.02 s faster; SMD 0.20, 95% CI -0.19 to 0.60; 9 arms; 166 participants; Figure 5), but the evidence is very uncertain. There is low heterogeneity (I^2=29%). We rated the certainty of evidence as very low due to risk of bias in included studies and very serious imprecision.

Sensitivity analyses

Exclusion of six studies with high risk of bias left one remaining study. This study showed no significant difference of effect between tDCS and other interventions (MD 0.68 s faster TUG, 95% CI 2.62 s slower to 3.98 s faster).

Exclusion of two studies with imputed data left five remaining studies. Pooled analysis of these studies showed no significant difference of effect between tDCS and other interventions (MD 0.67 s faster TUG, 95% CI 1.04 s slower to 2.35 s faster; SMD 0.20, 95% CI -0.31 to 0.70).

Exclusion of five studies where functional mobility was not listed as a primary outcome left two remaining studies. Pooled analysis of these studies showed no significant difference of effect between tDCS and other interventions (MD 1.45 s faster TUG, 95% CI 3.60 s slower to 0.71 s faster; SMD -0.43, 95% CI -1.07 to 0.21).

Parkinson motor symptoms

Nine studies reported measures of Parkinson motor symptoms. Pooled analyses revealed little to no effect of tDCS on Parkinson motor symptoms (MD +0.40 on the UDPRS III, 95% CI -2.54 to 3.34 on the UPDRS III; SMD -0.05, 95% CI -0.42 to 0.32; 10 arms, 264 participants; Figure 6), but the evidence is very uncertain. There is substantial heterogeneity (I^2=54%). We rated the certainty of evidence as very low due to risk of bias in included studies, very serious imprecision and serious inconsistency.

Sensitivity analyses

Exclusion of six studies with high risk of bias left three remaining studies. Pooled analysis of these studies showed no significant difference between tDCS and other interventions on Parkinson motor symptoms (MD +1.03 on the UPDRS III, 95% CI -3.50 to 5.49 on the UPDRS III; SMD -0.13, 95% CI -0.69 to 0.44).

Exclusion of three studies with imputed data left six remaining studies. Pooled analysis of these studies showed no significant difference between tDCS and other interventions on Parkinson motor symptoms (MD +0.32 on the UPDRS III, 95% CI –3.74 to +4.29 on the UPDRS III; SMD -0.04, 95% CI -0.54 to 0.47).

Exclusion of seven studies where Parkinson motor symptoms was not listed as a primary outcome left two remaining studies. Pooled analysis of these studies showed no significant difference of effect between tDCS and other interventions (MD -5.80 on the UPDRS III, 95% CI -12.90 to 0.48 on the UPDRS III; SMD 0.73, 95% CI -0.06 to 1.52).

Subgroup analyses

We compared the studies in which tDCS was the sole vs one of the treatments (3 vs 7 experimental study arms). The test for subgroup difference did not indicate a difference between subgroups (Chi^2=0.06; P=0.80; Figure 6(a)).

As presented in Figure 6(b), we also compared tDCS effect when the anode was placed over the motor cortex (8 experimental study arms) vs the DLPFC (1 experimental study arm) vs the cerebellum (1 experimental study arm). The test for subgroup difference did not indicate a difference between subgroups (Chi^2=3.60; P=0.17).

Meta-regression

As presented in Figure 4(b), the meta-regression slope was 0.35 SMD per 100 dose units (95% CI 0.04 to 0.65), indicating a positive dose-response trend in the weighted regression model. Correlation analyses were weaker and not statistically significant (Pearson r 0.57, P=0.086; Spearman ρ 0.27, P=0.443)

ADL

ADL was reported in two studies [21,35]. However, we did not pool studies together as there was considerable heterogeneity (I^2=91%). De Icco et al., showed that the combined effect of tDCS with intensive rehabilitation on the Functional Independence Measure was

superior to this of a sham stimulation combined with intensive rehabilitation (MD 4.80 points, 95% CI 0.74 to 8.86 points; 28 participants). Contrastingly, Pisano et al., showed that the effect of gait training (with sham stimulation) on ADL scale was superior to this of tDCS combined with gait training (MD -1.70 points, 95% CI -2.74 to -0.66 points; 17 participants).

tDCS has little to no effect on ADL but the evidence is very uncertain. We rated the certainty of evidence as very low due to risk of bias in included studies, very serious imprecision and serious inconsistency.

Quality of life

Four studies reported measures of QoL. Pooled analysis revealed little to no effect of tDCS on QoL (MD -1.57 points on the PDQ-39, 95% CI -8.36 to 5.23 points on the PDQ-39; SMD 0.09, 95% CI -0.30 to 0.48; 4 arms, 101 participants; Figure 7), but the evidence is very uncertain. There is low heterogeneity (I^2=0%). We rated the certainty of evidence as very low due to risk of bias in included studies and very serious imprecision.

Safety

Among the seventeen studies, thirteen documented the presence or absence of adverse events following the administration of tDCS. Three studies documented adverse events such as first-degree burns (likely due to improper electrode placement) [28,42], tingling sensations [39], and a brief visual flash lasting a few seconds [39]. However, most of these adverse events were minor and resolved spontaneously or within a short period. Notably, a stroke occurred in one participant, but the study stated that it was deemed unrelated to the tDCS intervention [42].

Fourteen studies reported the number of participants who discontinued the intervention [21–23,28–31,33,37–42]. The evidence suggests that tDCS results in minimal to no increased risk of dropouts (RD 0.02, 95% CI -0.03 to 0.08, 17 arms; 463 participants;

411 Figure 8). There is low heterogeneity (I^2=0%). We rated the certainty of evidence as low due
412 to risk of bias in included studies and serious imprecision.
413 *Sensitivity analyses*
414 Exclusion of seven studies with high risk of bias left seven remaining studies. Pooled
415 analysis revealed a similar safety effect (RD -0.00, 95% CI -0.06 to 0.06).
416 *Subgroup analyses*
417 We compared the studies in which tDCS was the sole vs one of the treatments (7 vs 10
418 experimental study arms). The test for subgroup difference did not indicate a difference
419 between subgroups (Chi^2=0.03; P=0.85; Figure 8(a)).
420 As presented in Figure 8(b), we also compared tDCS safety effect when the anode was
421 placed over the motor cortex (12 experimental study arms) vs the DLPFC (3 experimental
422 study arms) vs the cerebellum (2 experimental study arms). The test for subgroup difference
423 did not indicate a difference between subgroups (Chi^2=0.21; P=0.90).
424 **Reporting biases**
425 All included studies except five published their protocol on clinical trials register. Of
426 these, eleven were registered prospectively and one retrospectively. Differences between
427 trials' published protocol and publications are presented in Table 3. The most common
428 discrepancies concerned sample size (e.g., smaller than planned), outcome reporting (addition,
429 omission, or modification of outcomes), eligibility criteria, and intervention characteristics.
430 Less frequently, discrepancies were observed in assessment time points and number of study
431 arms. Only one study reported no differences between protocol and publication.
432 Funnel plots (for outcomes where n≥10) do not suggest potential publication biases
433 (see Supplemental Digital Content 1-3).
434

DISCUSSION

Summary of main results

The review included seventeen RCTs involving 553 individuals with PD. Overall, the evidence suggests that tDCS has little to no effect on walking speed, functional mobility, Parkinson motor symptoms, ADLs, or QoL. Subgroup analyses showed no differences in treatment effects based on stimulation site or intervention protocol. Meta-regression analyses did not support a clear dose–response relationship, although a positive trend was observed for Parkinson motor symptoms. Adverse effects were generally minor and transient, and tDCS does not seem to increase the risk of dropouts compared to control groups.

Overall completeness and applicability of the evidence

The included studies were generally small and highly heterogeneous in stimulation parameters, intervention protocols, and outcome measures, which limits the generalizability of our findings and prevents conclusions about optimal tDCS protocols. Short follow-up durations also preclude conclusions about long-term effects.

Limitations of the evidence included in the review

All included studies had unclear to high risk of bias, particularly in the domains of selection of the reported results. Trialists of five studies did not publish a prespecified protocol on clinical trials register and one protocol was published retrospectively. Among studies with available protocols, discrepancies between planned and reported methods were common. Such inconsistencies raise concerns about selective reporting and analytical flexibility, which may have influenced the reported findings and contributed to the overall risk of bias in this review. The small sample sizes and wide confidence intervals further contribute to reducing the certainty of evidence. There was also considerable heterogeneity in tDCS parameters (e.g., intensity, duration, electrode positioning), training modalities, and assessment tools. Only a limited number of studies reported on ADLs and QoL, and those that

did, produced conflicting results with very low certainty of evidence. In addition, for several outcomes (e.g., Parkinson motor symptoms and functional mobility), many included studies did not define these as primary outcomes, which may affect the reliability of effect estimates. However, sensitivity analyses restricted to studies reporting these outcomes as primary outcomes yielded consistent results, supporting the robustness of our findings.

Limitations of the review processes

Although this review followed Cochrane methodological standards, some limitations should be acknowledged. We excluded publications in languages other than English, French, or Spanish, which may have introduced language bias. Despite extensive database and manual searches, some relevant studies may have been missed. In addition, some study data could not be obtained from authors. Finally, we restricted inclusion to parallel-group RCTs, excluding cross-over and cluster RCTs. While this approach was chosen to ensure methodological consistency and avoid potential biases (e.g., carryover effects and unit-of-analysis issues), it may have limited the comprehensiveness of the evidence base.

Agreements and disagreements with other studies or reviews

At least five systematic reviews, published between 2019 and 2025, have examined the effect of tDCS on motor functions of participants with PD [12,17–19,43]. The results of our review are only partially consistent with prior reviews, with three reporting neutral effects of tDCS on motor function in PD, while two reported more favourable effects. Duan et al., and de Oliveira et al. both concluded that tDCS had no significant effect on UPDRS III scores, functional mobility, or gait performance [17,18], which align with our meta-analyses. Liu et al. similarly reported non-significant effects of tDCS on UPDRS III scores and gait speed, reinforcing the broader conclusion that current evidence does not support strong motor benefits in this population [19]. While these results were consistent with our current analyses, the inclusion of more recent RCTs [21–23] also enabled additional analyses that were not

undertaken in prior reviews. For instance, these studies also reported outcomes related to ADLs and QoL, allowing us to conduct both quantitative and qualitative syntheses on these outcomes that are clinically meaningful for individuals with PD. In addition, we were able to perform subgroup analyses and meta-regressions to explore potential sources of heterogeneity, such as intervention protocol and dose. We also performed a subgroup analysis based on stimulation site. Notably, several studies targeted the DLPFC [38,40,41], reflecting its proposed role in executive and attentional processes that contribute to motor control in PD. Modulation of the DLPFC has been hypothesized to enhance cognitive–motor interactions, particularly in tasks requiring dual-tasking, gait regulation, and executive function [44,45]. However, despite this theoretical rationale, our subgroup analyses did not demonstrate a consistent advantage of DLPFC stimulation over other cortical targets.

The other reviews did report more promising findings, though with notable caveats. Lee et al. found a significant effect of tDCS on locomotion, even when restricting analyses to gait speed or functional mobility outcomes alone [12]. However, they included both pre-post and between-group comparisons in their analyses, which may have inflated effect estimates. Similarly, the more recent review by Lee et al. suggested short-term benefits of tDCS combined with motor training on gait speed and motor performance. However, these effects were described as temporary. Furthermore, trialists seem to have reported multiple outcomes from the same studies in their pooled analysis, which may have introduced a risk of publication bias. In addition, the splitting approach, recommended for handling multi-arm trials, has not been considered, potentially compromising the validity of pooled estimates.

Implications for practice

While tDCS was generally well-tolerated with few reported adverse effects, the current evidence does not support a superior benefit compared to other interventions. Subgroup analyses did not reveal any consistent advantages based on stimulation site or

intervention protocol. These findings should be interpreted with caution given the small sample sizes, low to very low certainty of evidence, and methodological limitations of many included studies. As such, the routine use of tDCS in PD rehabilitation cannot yet be recommended.

Implications for research

Future research should prioritise large and methodologically rigorous RCTs to better determine the potential role of tDCS in PD rehabilitation. Greater consistency is needed in tDCS protocols, including stimulation parameters, intervention protocol and outcome measures, to allow for meaningful comparisons across studies. Studies should also ensure clear reporting of intervention details, adherence, adverse events, and long-term follow-up to evaluate sustained effects. In addition, researchers should explore potential moderators of treatment response, such as type and severity of disease to better identify responders to tDCS. Lastly, prospective registration and full publication of trial outcomes are essential to reduce reporting bias and strengthen the evidence base.

In conclusion, current evidence suggests that while tDCS appears to be safe, its effect of tDCS on motor function, ADLs, or QoL is probably not superior to other conventional interventions in individuals with PD. tDCS effects do not seem to be influenced by the stimulation site or the intervention protocol.

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686

687 **TABLES**

688 **Table 1.** Search strategies

689 **Table 2.** Characteristics of included studies

690 **Table 3.** Differences between prespecified protocols and studies

691

692 **FIGURES**

693 **Figure 1.** Flow chart diagram

694 **Figure 2.** Risk of bias results

695 **Figure 3.** Forest plot comparing the effect of tDCS and other sham interventions on walking
696 speed. (a) Subgroup analysis based on intervention protocol. (b) Subgroup analysis based on
697 anode location

698 **Figure 4.** Meta-regression of tDCS intervention dose and outcomes in Parkinson's disease.
699 (a) Walking speed: Meta-regression of standardized mean difference on total tDCS dose. (b)
700 Parkinson's motor symptoms: Meta-regression of standardized mean difference on total tDCS
701 dose

702 **Figure 5.** Forest plot comparing the effect of tDCS and other sham interventions on
703 functional mobility

704 **Figure 6.** Forest plot comparing the effect of tDCS and other sham interventions on Parkinson
705 motor symptoms. (a) Subgroup analysis based on intervention protocol. (b) Subgroup analysis
706 based on anode location

707 **Figure 7.** Forest plot comparing the effect of tDCS and other sham interventions on quality of
708 life

709 **Figure 8.** Forest plot comparing the risk of observing dropouts between tDCS and other
710 sham interventions. (a) Subgroup analysis based on intervention protocol. (b) Subgroup
711 analysis based on anode location

712

SUPPLEMENTAL DIGITAL CONTENT

Supplemental Digital Content 1.jpeg

Supplemental Digital Content 2.jpeg

Supplemental Digital Content 3.jpeg