

Comparative Effectiveness of Muscle Energy Technique versus Exercise Therapy, Both Combined with Transcutaneous Electrical Nerve Stimulation, on Pain, Disability, and Lumbar Range of Motion in Sacroiliac Joint Dysfunction: A Randomized Controlled Trial

Dr. Lokesh Nandi

Physiotherapist

Abstract

Background. Sacroiliac joint dysfunction (SIJD) is a common but frequently under-recognized contributor to low back and pelvic girdle pain. Muscle energy technique (MET) and structured exercise therapy are both used in its conservative management, but head-to-head evidence comparing the two, with both arms receiving an identical transcutaneous electrical nerve stimulation (TENS) adjunct, is limited.

Objective. To compare the effects of MET combined with conventional TENS against structured exercise therapy combined with conventional TENS on pain intensity, functional disability, and lumbar range of motion (ROM) in adults with SIJD presenting with biomechanical stress patterns.

Methods. In this prospective, two-arm, parallel-group randomized controlled trial, 67 adults were screened and 50 were randomized (computer-generated sequence; allocation concealment by sequentially numbered opaque sealed envelopes) to Exercise Therapy + TENS (Group A, n = 25) or MET + TENS (Group B, n = 25). SIJD was diagnosed clinically on a cluster of three or more positive findings on a six-test provocation battery (thigh thrust, compression, distraction, Gaenslen's test, sacral thrust test, and FABER test). Both groups received 12 sessions over 4 weeks (3 sessions/week). Outcome assessors were blinded to group allocation; participants and treating therapists were not blinded, given the physical nature of the interventions. The Numeric Pain Rating Scale (NPRS), Oswestry Disability Index (ODI), and lumbar flexion and extension ROM were recorded at baseline, Week 2, Week 4, and Week 6. NPRS was the outcome underlying the a priori sample-size calculation and is treated as the principal outcome measure, although a formal primary/secondary outcome hierarchy was not prospectively designated. Between-group differences in change from baseline to Week 6 constituted the confirmatory analysis; a mixed-design (Group $\times$ Time) analysis of variance and a sensitivity analysis adjusting for a baseline imbalance in pelvic-tilt direction were performed as post hoc additions. The trial was not prospectively registered in a clinical trial registry.

Results. The two arms were balanced at baseline on age, sex, number of positive provocation tests, and biomechanical pattern (all $p > 0.05$), but differed significantly in the categorical distribution of pelvic-tilt direction ($\chi^2 = 9.73$, $df = 3$, $p = 0.021$; Group A weighted toward posterior tilt, Group B toward anterior

tilt). Both groups improved significantly from baseline to Week 6 on every outcome (paired t-tests, all $p < 0.001$). At Week 6, Group B showed significantly greater improvement than Group A in NPRS (mean difference 0.80 points, 95% CI 0.11–1.49; $p = 0.023$; $d = 0.66$), ODI (mean difference 9.28 points, 95% CI 7.07–11.49; $p < 0.001$; $d = 2.39$), lumbar flexion (mean difference 6.12° , 95% CI 4.50 – 7.74 ; $p < 0.001$; $d = 2.15$), and lumbar extension (mean difference 3.00° , 95% CI 1.91 – 4.09 ; $p < 0.001$; $d = 1.57$). A post hoc mixed-design ANOVA confirmed significant Group $\times$ Time interactions for all four outcomes ($p \leq 0.0004$). A post hoc sensitivity analysis adjusting for baseline pelvic-tilt category attenuated the NPRS between-group effect to non-significance ($p = 0.067$), while the ODI, flexion, and extension effects remained robust (all $p < 0.001$).

Conclusion. Both exercise therapy and MET, each combined with conventional TENS, produced large within-group improvements in pain, disability, and lumbar ROM over 6 weeks. MET + TENS was associated with significantly greater improvement than Exercise Therapy + TENS in disability and lumbar ROM, and with a significant unadjusted advantage in pain that was attenuated after adjustment for a baseline pelvic-tilt imbalance. These findings support MET as a promising adjunct to TENS for SIJD but, given the absence of prospective registration and of participant/therapist blinding, should be confirmed in an adequately powered, prospectively registered trial that stratifies randomization by pelvic-tilt direction.

Keywords: Sacroiliac joint dysfunction; Muscle energy technique; Exercise therapy; Transcutaneous electrical nerve stimulation; Randomized controlled trial; Oswestry Disability Index; Lumbar range of motion; Low back pain.

Introduction

Sacroiliac joint dysfunction (SIJD) is a recognized contributor to low back and pelvic girdle pain, arising from abnormal mobility or malalignment of the sacroiliac joint and producing pain, functional limitation, and altered biomechanical load transfer between the spine and lower limbs (Barros et al., 2019; Gartenberg et al., 2021). Estimates of its contribution to chronic low back pain vary across the literature, and diagnosis in clinical practice relies heavily on clusters of manual provocation tests rather than a single definitive reference standard (Laslett, 2008; Telli et al., 2018).

Conservative physiotherapy is the first-line management approach for SIJD. Structured exercise therapy — encompassing stabilization, mobility, and self-correction exercises — and muscle energy technique (MET), a manual therapy approach that uses targeted isometric muscle contractions to influence joint position and periarticular soft-tissue tone, are both widely used (Chaitow, 2013; Greenman, 2016). Transcutaneous electrical nerve stimulation (TENS) is frequently added as an adjunct for short-term pain modulation through gate-control and descending inhibitory mechanisms (Vaidya, 2018).

Several trials have examined MET or exercise therapy separately against various comparators. MET has been reported to produce significant pain and disability improvements when compared with Mulligan mobilization, kinesiotaping, or core-strengthening exercise (Mohammed et al., 2019; Joshi et al., 2017; Khalid et al., 2022), and combining MET with targeted strengthening has been shown to augment outcomes beyond strengthening alone (Soomro et al., 2024). Exercise-based approaches directed at lumbopelvic stabilization have independently demonstrated benefit for SIJD-related pain and disability (Nejati et al., 2019). However, head-to-head evidence comparing MET with structured exercise therapy, with both arms receiving an identical TENS adjunct, is limited, which motivated the present trial.

The objective of this study was to compare the effects of MET combined with conventional TENS against structured exercise therapy combined with conventional TENS on pain intensity (NPRS), functional disability (ODI), and lumbar flexion and extension range of motion in adults with clinically diagnosed SIJD presenting with biomechanical stress patterns, over a 6-week observation period.

Methods

Study design and setting

This was a prospective, two-arm, parallel-group randomized controlled trial conducted at the Physiotherapy Outpatient Department of the Tripura Institute of Paramedical Sciences (TIPS), Agartala, and AGMC & GBP Hospital, West Tripura, India, over approximately three months encompassing recruitment, intervention, follow-up, and analysis.

Participants

Adults aged 18–65 years were eligible if they presented with chronic symptoms of more than three months' duration, baseline pain intensity of at least 4 on the NPRS localized to the pelvic girdle or lower back (with or without referred pain to the lower limb), a demonstrable biomechanical stress pattern on physical examination (pelvic asymmetry consistent with anterior or posterior innominate rotation, inflare, outflare, or sacral torsion), and no prior treatment for the current episode. SIJD was diagnosed clinically on the basis of a cluster of three or more positive findings on a six-test provocation battery comprising the thigh thrust test, the compression test, the distraction test, Gaenslen's test, the sacral thrust test, and the FABER (Patrick's) test — the same six tests whose intra-examiner reliability for SIJD assessment has been reported as good to very good (Telli et al., 2018). Exclusion criteria were red-flag pathology (malignancy, spinal or systemic infection, fracture, inflammatory arthritis such as ankylosing spondylitis, or pregnancy), neurological deficit (radiculopathy, nerve root compression, intervertebral disc herniation, spinal canal stenosis, or a positive straight-leg-raise below 60°), other identifiable sources of lumbopelvic pain, and comorbidity precluding participation in the intervention protocol.

No imaging-based or injection-based reference standard was used to confirm the diagnosis; classification relied exclusively on the clinical provocation-test cluster described above. This reliance on a clinical rather than an imaging-based reference standard is a recognized feature of the SIJD trial literature more broadly and is addressed further under Limitations.

Sample size

Sample size was calculated in G*Power (version 3.1.9.7) for an independent-samples t-test, based on a large pain-reduction effect size (Cohen's $d = 1.05$) reported in a previous MET trial in SIJD (Mohammed et al., 2019), with $\alpha = 0.05$ (two-tailed) and power $(1 - \beta) = 0.80$. This yielded a minimum requirement of 32 participants (16 per group). To allow for an anticipated 20% attrition, this was inflated to 40 participants (20 per group); to improve the precision of the repeated-measures design across four assessment points and enhance external validity, a final pragmatic sample of 50 participants (25 per group) was recruited, exceeding the calculated minimum.

Randomization and blinding

Participants were allocated 1:1 using a computer-generated randomization sequence prepared by an investigator independent of assessment and treatment delivery, with allocation concealment maintained through sequentially numbered, opaque, sealed envelopes that were opened only after baseline assessment. Given the physical nature of both interventions, blinding of participants and treating therapists was not

feasible. Outcome assessments were performed by an assessor independent of treatment delivery and blinded to group allocation, and data were analyzed using coded group labels.

Interventions

Both groups received 12 treatment sessions over 4 weeks (3 sessions per week), delivered by a qualified physiotherapist under standardized conditions of session duration, treatment frequency, progression criteria, and monitoring.

Group A (Exercise Therapy + TENS).

Participants received a structured exercise program targeting sacroiliac mobility and lumbopelvic stability, comprising posterior-innominate self-mobilization (supine, knee-to-chest technique to facilitate posterior pelvic rotation), sacroiliac joint stretches performed side-lying with combined trunk rotation and resisted hip abduction/internal rotation, and a four-phase progressive spinal-stabilization program. Phase 1 comprised foundational activation exercises (abdominal draw-in, bridging variations, prone and side-support positions, quadruped opposite arm–leg lifts); Phase 2 introduced unstable surfaces and resisted movement (medicine-ball exercises, single-leg bridging); Phase 3 progressed to dynamic stabilization using foam rollers and medicine balls with functional trunk movement; and Phase 4 incorporated dynamic postural control during external perturbation (catching and throwing a medicine ball). Progression between phases was governed by the participant's ability to perform each exercise with correct control and without pain.

Group B (MET + TENS).

MET was applied using a pattern-specific approach guided by the participant's identified biomechanical dysfunction (anterior innominate, posterior innominate, inflare, outflare, or sacral torsion), following the principle of post-isometric relaxation: the target muscle was positioned at its restrictive barrier, an isometric contraction was performed at approximately 20–30% of maximal voluntary effort and held for 5 seconds against therapist resistance, followed by complete relaxation and passive advancement of the segment to a new barrier, repeated for 3–5 cycles per technique. For anterior innominate dysfunction the hip was positioned in flexion with a resisted hip-extension (hamstring) contraction; for posterior innominate dysfunction the hip was positioned in extension with a resisted hip-flexion (iliopsoas) contraction; for inflare the hip was positioned in abduction with a resisted adduction contraction; for outflare the hip was positioned in adduction with a resisted abduction contraction; and for sacral torsion, MET was applied prone or side-lying according to the direction of sacral rotation, using a gentle isometric contraction of trunk or hip rotators (piriformis, multifidus) to guide the sacrum toward a neutral position. All applications followed a uniform procedural framework for contraction intensity, hold time, and repetitions, applied consistently across participants irrespective of the specific pattern treated.

TENS (both groups).

Conventional TENS was applied immediately after the primary intervention in every session, with electrodes placed over the region of pain around the sacroiliac joint, typically near the posterior superior iliac spine. Standardized parameters were used (frequency 80–100 Hz; pulse duration 50–100 μ s), with intensity adjusted to produce a strong but comfortable sensory stimulation without visible muscle contraction, for approximately 20 minutes per session.

Outcome measures

Pain intensity was measured with the Numeric Pain Rating Scale (NPRS, 0–10) and treated as the principal outcome measure on the basis of the sample-size calculation, though a formal primary/secondary outcome hierarchy was not prospectively designated. Functional disability was measured with the Oswestry

Disability Index (ODI, 0–100%) (Copay & Cher, 2015; Hawker et al., 2011). Lumbar flexion and extension range of motion were measured goniometrically in degrees. All four measures were recorded at baseline, Week 2, Week 4, and Week 6 (2 weeks after the end of treatment) by the blinded assessor.

Statistical analysis

Analyses were performed in JASP (version 0.19.x); statistical significance was set a priori at two-tailed $p < 0.05$. Normality of each continuous outcome was assessed by the Shapiro–Wilk test and visual inspection of histograms and Q–Q plots. Continuous baseline characteristics and Week-6 change scores (baseline to Week 6) were compared between groups using independent-samples t-tests, which constituted the confirmatory between-group analysis; within-group change from baseline was assessed by paired-samples t-tests. Categorical baseline variables (sex, biomechanical pattern, pelvic-tilt direction) were compared using chi-square tests. Cohen's d (pooled SD) and 95% confidence intervals were calculated for all between-group comparisons. Percentage-improvement values were calculated as the mean of each participant's individual percentage change from baseline, rather than as the percentage change between group means; this convention is applied consistently throughout and should be noted when comparing these figures with studies using the alternative convention.

A one-way ANOVA comparing percentage NPRS improvement across biomechanical-pattern subgroups, and a Pearson correlation between baseline NPRS severity and absolute point-reduction, were performed as exploratory, hypothesis-generating analyses given the modest size of several pattern subgroups. Two further analyses were performed on the participant-level dataset as post hoc additions to the original analysis plan: (1) a mixed-design (between-subjects Group $\times$ within-subjects Time) analysis of variance across all four assessment points for each outcome, providing a single omnibus test of the treatment-by-time interaction; and (2) a linear-regression sensitivity analysis of each Week-6 change score, adjusting for baseline pelvic-tilt category, to evaluate whether the observed baseline imbalance in pelvic-tilt direction (see Results) could account for the between-group effects. Both are identified as post hoc throughout the Results.

Ethical considerations

Ethical approval was obtained from the Institutional Ethics Committee prior to the commencement of the study. Written informed consent was obtained from all participants before enrolment, participant confidentiality and anonymity were maintained throughout, and participants retained the right to withdraw at any stage without consequence.

Trial registration

This study was conducted as a postgraduate (Master of Physiotherapy) dissertation project and was not prospectively registered in a clinical trial registry (e.g., CTRI, ClinicalTrials.gov).

Results

Participant flow

Sixty-seven individuals were assessed for eligibility. Seventeen were excluded prior to enrolment — for not meeting the eligibility criteria, declining to participate after receiving detailed information about the study, or for medical/clinical reasons — without a further numeric breakdown across these categories recorded in the source documentation. Fifty eligible participants were enrolled and provided written informed consent, and were randomized 1:1 to Group A ($n = 25$) or Group B ($n = 25$). All 50 participants received their allocated intervention, completed all four assessment points (baseline, Week 2, Week 4,

Week 6), and were included in the final analysis, with no losses to follow-up, no discontinuations, and no missing data.

Baseline characteristics

Table 1. Demographic and Baseline Clinical Characteristics

Variable	Group A (n = 25)	Group B (n = 25)	Test statistic	p-value
Age, years (mean $\pm$ SD)	39.44 $\pm$ 12.06	38.60 $\pm$ 11.85	Independent t-test	0.805
Sex, n (male/female)	13 / 12	13 / 12	Chi-square	1.000
Positive SIJ provocation tests (mean $\pm$ SD, out of 6)	4.56 $\pm$ 1.08	4.80 $\pm$ 1.04	Independent t-test	0.428
Biomechanical pattern — AS ilium, n (%)	5 (20%)	3 (12%)		
Biomechanical pattern — Inflare, n (%)	5 (20%)	8 (32%)		
Biomechanical pattern — Outflare, n (%)	7 (28%)	7 (28%)		
Biomechanical pattern — PI ilium, n (%)	7 (28%)	6 (24%)		
Biomechanical pattern — Other (sacral torsion), n (%)	1 (4%)	1 (4%)	Chi-square	0.867
Pelvic tilt — Anterior, n (%)	5 (20%)	12 (48%)		
Pelvic tilt — Lateral, n (%)	6 (24%)	9 (36%)		
Pelvic tilt — Posterior, n (%)	13 (52%)	3 (12%)		
Pelvic tilt — Complex/combined, n (%)	1 (4%)	1 (4%)	Chi-square	0.021*

SIJ = sacroiliac joint. * $p < 0.05$.

Age, sex distribution, the number of positive SIJ provocation tests, and biomechanical-pattern classification did not differ significantly between groups, confirming successful randomization on these variables. The categorical distribution of pelvic-tilt direction, however, differed significantly between groups ($\chi^2 = 9.73$, $df = 3$, $p = 0.021$): Group A was weighted toward posterior-tilt presentations (13/25, 52%), whereas Group B was weighted toward anterior-tilt presentations (12/25, 48%), with lateral tilt present in 24% of Group A and 36% of Group B, and the Complex/Combined category equally represented in both arms (1/25 each; two participants whose clinical presentation combined features of lumbopelvic instability with a sacral-torsion character were classified into this category). Because pelvic-tilt direction was not a stratification variable in the randomization scheme, this imbalance is interpreted as having arisen by chance; its implications are examined further in the Discussion and Limitations.

Baseline outcome comparability

Table 2. Baseline Comparison of Outcome Measures Between Groups

Outcome	Group A (n = 25)	Group B (n = 25)	p-value
NPRS (0–10)	6.00 ± 1.44	5.76 ± 1.01	0.499
ODI (%)	49.96 ± 8.73	52.00 ± 10.76	0.465
Lumbar flexion (°)	44.64 ± 6.01	43.40 ± 4.85	0.426
Lumbar extension (°)	16.80 ± 4.57	17.40 ± 4.55	0.644

No significant baseline between-group difference was found for any of the four continuous outcomes, confirming comparability at study entry despite the categorical pelvic-tilt imbalance described above.

Within-group change

Table 3. Within-Group Change from Baseline to Week 6

Outcome	Group A: baseline → Week 6	Group A % improvement	Group B: baseline → Week 6	Group B % improvement
NPRS (0–10)	6.00 → 2.44	57.8%	5.76 → 1.40	75.3%
ODI (%)	49.96 → 28.76	44.1%	52.00 → 21.52	61.0%
Lumbar flexion (°)	44.64 → 58.88	32.6%	43.40 → 63.76	47.4%
Lumbar extension (°)	16.80 → 26.16	59.2%	17.40 → 29.76	75.9%

Percentage improvement is the mean of each participant's individual percentage change from baseline (see Statistical analysis).

Both groups improved significantly on all four outcomes from baseline to Week 6 (paired t-tests, all $p < 0.001$), with within-group effect sizes (Cohen's d_z) ranging from 2.62 to 5.52 in Group A and from 4.21 to 7.76 in Group B.

Between-group change, baseline to Week 6 (confirmatory analysis)

Table 4. Between-Group Comparison of Change Scores, Baseline to Week 6

Outcome	Group A Δ (mean ± SD)	Group B Δ (mean ± SD)	Mean difference (95% CI)	p-value	Cohen's d
NPRS (points)	3.56 ± 1.36	4.36 ± 1.04	−0.80 (−1.49 to −0.11)	0.023	0.66
ODI (points)	21.20 ± 3.84	30.48 ± 3.93	−9.28 (−11.49 to −7.07)	<0.001	2.39
Lumbar flexion (°)	14.24 ± 2.82	20.36 ± 2.87	−6.12 (−7.74 to −4.50)	<0.001	2.15
Lumbar extension (°)	9.36 ± 1.78	12.36 ± 2.04	−3.00 (−4.09 to −1.91)	<0.001	1.57

Independent-samples t-test on baseline-to-Week-6 change scores. Negative mean differences indicate greater improvement in Group B (MET + TENS).

Group B showed significantly greater improvement than Group A on all four outcomes. Effect sizes were medium for NPRS and very large for ODI, flexion, and extension by conventional benchmarks. Effect sizes of this magnitude ($d > 2.0$) are unusual for a 6-week conservative-therapy comparison and are interpreted cautiously in the Discussion.

Post hoc Group $\times$ Time analysis

A mixed-design analysis of variance (between-subjects Group; within-subjects Time: baseline, Week 2, Week 4, Week 6) was performed for each outcome as a post hoc addition to the confirmatory change-score analysis above, providing a single omnibus test of differential trajectory across all four assessments.

Table 5. Post Hoc Mixed-Design (Group $\times$ Time) Analysis of Variance

Outcome	Main effect of Group (F, p)	Group $\times$ Time interaction (F, p)
NPRS	F(1,48) = 1.62, p = 0.209	F(3,144) = 6.49, p < 0.001
ODI	F(1,48) = 0.88, p = 0.353	F(3,144) = 70.08, p < 0.001
Lumbar flexion	F(1,48) = 1.65, p = 0.205	F(3,144) = 55.97, p < 0.001
Lumbar extension	F(1,48) = 2.66, p = 0.110	F(3,144) = 27.13, p < 0.001

Sphericity was violated for ODI, flexion, and extension (Mauchly's $p < 0.001$); the time and interaction effects remained significant under Greenhouse–Geisser correction.

The non-significant main effect of Group, combined with a significant Group $\times$ Time interaction for every outcome, is consistent with two groups that began statistically equivalent and diverged progressively over the intervention period.

Post hoc sensitivity analysis for baseline pelvic-tilt imbalance

Because pelvic-tilt direction differed significantly at baseline, each Week-6 change score was re-examined in a linear regression model of the form $\text{change} \sim \text{Group} + \text{Pelvic-tilt category}$ (excluding the two Complex/Combined cases, $n = 48$), to assess whether this imbalance could account for the observed between-group effects.

Table 6. Post Hoc Sensitivity Analysis Adjusting for Baseline Pelvic-Tilt Category

Outcome	Unadjusted effect	Adjusted group effect (controlling for pelvic-tilt category)
NPRS	p = 0.023	p = 0.067 (no longer significant)
ODI	p < 0.001	p < 0.001 (robust)
Lumbar flexion	p < 0.001	p < 0.001 (robust)
Lumbar extension	p < 0.001	p < 0.001 (robust)

The ODI, flexion, and extension effects were not substantially explained by the baseline pelvic-tilt imbalance, whereas the NPRS effect was attenuated to non-significance once pelvic-tilt category was accounted for. This distinction is carried through to the Discussion and Conclusion.

Exploratory Week-4 finding

An unplanned comparison of raw (non-change) NPRS scores at Week 4 showed a significant difference favoring Group A: 2.36 ± 1.04 versus 3.04 ± 0.93 (independent t-test, $p = 0.019$; Mann–Whitney U, $p = 0.020$) — the inverse of the Week-6 direction, in which Group B was favored (group means: baseline 6.00 vs. 5.76; Week 2 4.44 vs. 4.16; Week 4 2.36 vs. 3.04; Week 6 2.44 vs. 1.40, Groups A and B respectively). Because the corresponding Week-4 change-score comparison (baseline to Week 4) was not statistically significant ($p = 0.067$), this pattern is most parsimoniously described as a difference in raw trajectory shape rather than a genuine treatment-effect reversal. It is reported as an exploratory, hypothesis-generating observation: it was not part of a prespecified analysis plan and was not corrected for multiple comparisons across time points.

Exploratory subgroup and correlation analyses

Mean percentage improvement in NPRS did not differ significantly across the five biomechanical-pattern subtypes (AS ilium $67.1 \pm 15.3\%$, $n = 8$; Inflare $64.5 \pm 19.5\%$, $n = 13$; Other/sacral torsion $65.0 \pm 21.2\%$, $n = 2$; Outflare $67.0 \pm 11.5\%$, $n = 14$; PI ilium $68.0 \pm 13.1\%$, $n = 13$; one-way ANOVA $F = 0.098$, $p = 0.983$). This analysis is descriptive and hypothesis-generating given the small size of several subgroups, particularly Other/sacral torsion ($n = 2$), and should not be interpreted as evidence that biomechanical pattern is irrelevant to treatment selection.

A significant positive Pearson correlation was found between baseline NPRS severity and absolute point-reduction by Week 6 across the combined sample ($r = 0.79$, $p < 0.001$). This is consistent with a scale-boundary (floor/ceiling) artifact common to bounded rating instruments, in which participants starting closer to the upper end of the scale have more numerical room to show a large absolute reduction, and should not be over-interpreted as evidence that baseline severity causally predicts treatment responsiveness.

Adherence and adverse events

Participant compliance and adverse events were recorded on the case-record form at each of the 12 treatment sessions. All 50 participants completed the full course of treatment and all follow-up assessments, with zero losses to follow-up and zero discontinuations in either arm; a separate summary tabulation of session-level adherence and adverse-event data beyond this complete-follow-up record was not part of the reported results.

Discussion

Principal findings

In this randomized comparison of two TENS-adjunct conservative interventions for SIJD, both MET and structured exercise therapy produced large, statistically significant within-group improvements in pain, disability, and lumbar ROM over 6 weeks. Between groups, MET + TENS produced significantly greater improvement than Exercise Therapy + TENS on all four outcomes in the confirmatory change-score comparison, and this pattern was corroborated by a Group $\times$ Time interaction that was significant for every outcome in a post hoc mixed-model analysis, evidence against the between-group differences being an artifact of the specific pairwise test used. A post hoc sensitivity analysis, however, indicated that the NPRS advantage for MET did not survive adjustment for a baseline imbalance in pelvic-tilt direction, while the

ODI, flexion, and extension advantages did. This distinction — a functional/ROM advantage for MET that appears robust to the baseline imbalance, alongside a pain advantage that is more fragile to it — tempers the strength of the pain-related conclusion specifically and is carried through to the Conclusion below.

Interpretation of pain findings

The unadjusted between-group NPRS difference favored MET + TENS and is consistent with prior reports of MET producing meaningful pain reduction in SIJD (Mohammed et al., 2019; Joshi et al., 2017). However, because this advantage was attenuated to non-significance after adjustment for the baseline pelvic-tilt imbalance, it should be regarded as suggestive rather than conclusive evidence of a pain-specific benefit of MET over exercise therapy in this sample. The Week-4 reversal favoring Group A, discussed above, further indicates that the two interventions may differ in their early time-course of pain relief, which may in part reflect a more immediate gate-control contribution from TENS combined with active exercise relative to MET, though this explanation is speculative and was not directly tested.

Interpretation of disability findings

The substantially larger ODI improvement in Group B (61.0% vs. 44.1% mean improvement; $d = 2.39$), which remained robust after adjustment for the pelvic-tilt imbalance, suggests that the observed functional benefit of MET is unlikely to be explained by this baseline difference alone. This is consistent with prior reports of significant disability reduction following MET in SIJD populations (Mohammed et al., 2019; Sonam et al., 2025). Exercise therapy also produced substantial ODI gains, consistent with the established role of lumbopelvic stabilization training in improving force-closure mechanisms and load transfer across the sacroiliac joint (Nejati et al., 2019; Khalid et al., 2022).

Interpretation of lumbar range of motion findings

Lumbar flexion and extension ROM improved progressively in both groups, with significantly greater and adjustment-robust gains in Group B (flexion $d = 2.15$; extension $d = 1.57$). Because the sacroiliac joint is a key mechanical link between the spine and pelvis, restriction at this joint may produce compensatory limitation of lumbar movement; a direct effect of MET on periarticular musculature and accessory joint motion, together with normalization of pelvic mechanics, is a plausible explanation for the larger ROM gains observed, consistent with prior reports of mobility gains following MET-based manual therapy (García-Peñalver et al., 2020).

Comparison with previous literature

The overall direction of the present findings — a functional advantage for manual correction over exercise alone — is broadly consistent with prior small trials reporting benefit from MET in SIJD, including the trial whose pain effect size informed this study's sample-size calculation (Mohammed et al., 2019). Direct numerical comparison across studies is limited, however, by differences in comparator arms, outcome instruments, treatment dosage, and diagnostic criteria across the SIJD literature, and by the fact that several previously published trials in this area are themselves small and not prospectively registered.

Possible mechanisms

Several mechanisms have been proposed for MET's effect on SIJD, including transient correction of joint positional fault via post-isometric relaxation and reciprocal inhibition, modulation of periarticular muscle tone and proprioceptive input, and short-term neurophysiological pain-gating effects analogous to those proposed for manual therapy generally. Exercise therapy is thought to act through progressive loading, motor-control retraining of lumbopelvic stabilizers, and gradual desensitization. These mechanisms are

plausible explanations for the relative difference between arms observed here, but they are not established causal pathways confirmed by this trial, and should be read with that caveat.

Role of TENS

Both groups received identical conventional TENS, which isolates the differential effect of MET versus exercise therapy against a common TENS background but means the trial cannot speak to the independent contribution of TENS itself, nor exclude an interaction between TENS and either manual intervention. The between-group findings should be read as “MET + TENS versus Exercise + TENS,” not as evidence for or against MET or exercise therapy in the absence of TENS.

Baseline pelvic-tilt imbalance

Despite computer-generated randomization, the categorical distribution of pelvic-tilt direction differed significantly between arms, with Group A weighted toward posterior tilt and Group B toward anterior tilt. This variable was not included in the randomization stratification scheme, and the imbalance is interpreted as having arisen by chance. Because anterior and posterior pelvic tilt reflect different directions of sacroiliac loading, a differential response to MET or exercise by tilt direction cannot be excluded. The post hoc sensitivity analysis suggests the ODI and ROM findings are not substantially explained by this imbalance, whereas the NPRS finding may be; this should be regarded as hypothesis-generating pending a prospectively stratified, adequately powered confirmatory trial.

Clinical interpretation

The statistical significance of the ODI, flexion, and extension differences, together with their large effect sizes and robustness to the pelvic-tilt sensitivity analysis, suggests these differences may be clinically meaningful. However, effect sizes exceeding $d = 2.0$ for a 6-week conservative-therapy comparison are larger than typical in this literature; very large effect sizes observed in single, small, non-blinded, unregistered trials are, on average, more likely to shrink than to hold on replication, and independent confirmation is warranted before these magnitudes are taken as representative. The NPRS finding, nominally significant in the unadjusted analysis but attenuated after adjustment, should be presented with this caveat explicit rather than omitted.

Implications for future research

Future trials should be prospectively registered, adequately powered for stratified subgroup analysis, and should stratify randomization by pelvic-tilt direction given the imbalance observed here. Multi-center designs across diverse geographic and clinical settings would improve generalizability, and longer follow-up (6–12 months or beyond) is needed to establish the durability of the observed between-group differences. Incorporation of objective biomechanical assessment (three-dimensional motion analysis, surface electromyography, pressure mapping) and, where feasible, imaging-based confirmation of SIJD would strengthen diagnostic classification. Broader outcome domains — quality of life, fear-avoidance beliefs, patient satisfaction, and global perceived effect — would provide a more comprehensive evaluation of treatment response.

Limitations

Several limitations should be considered when interpreting these findings. The trial was not prospectively registered in a clinical trial registry, and a formal primary/secondary outcome hierarchy was not prespecified; NPRS is treated here as the principal outcome on the basis of the sample-size calculation alone, a retrospective interpretive convention rather than a prospectively registered designation. Participants and treating therapists could not be blinded given the physical nature of the interventions, which leaves the trial open to performance bias, although outcome assessment was independently blinded.

SIJD diagnosis relied on a clinical provocation-test cluster rather than an imaging-based or injection-based reference standard, and the diagnostic threshold of three or more positive findings on the six-test battery, while supported by reliability data for these tests (Telli et al., 2018), does not eliminate the possibility of diagnostic misclassification.

A significant baseline imbalance in pelvic-tilt direction was identified despite computer-generated randomization; the post hoc sensitivity analysis addressed this only partially, since it could not fully disentangle a potential pelvic-tilt-by-treatment interaction within a between-groups design. The mixed-design Group $\times$ Time analysis and the pelvic-tilt sensitivity analysis were both performed post hoc on the participant-level dataset rather than as part of a prespecified analysis plan, as was the Week-4 exploratory comparison; none of these should be treated as confirmatory. Multiple outcomes, time points, and subgroup and correlation analyses were examined without a formal multiplicity-correction strategy, and the biomechanical-pattern subgroup analysis in particular was underpowered for definitive subgroup inference, with the collapsed Other (sacral torsion) category ($n = 2$) being descriptive only. Session-level adherence and adverse-event data were collected on the case-record form but were not summarized beyond the complete-follow-up record reported in Results.

The trial was conducted at two affiliated centers in a single region of India using consecutive sampling, which may limit generalizability to other populations and settings; the sample size ($N = 50$), while adequate for the primary between-group comparisons, may be insufficient for robust subgroup or interaction analyses. Follow-up was limited to 6 weeks, restricting conclusions about the durability of the observed differences. Finally, several effect sizes — particularly for ODI and lumbar ROM — were unusually large for a 6-week conservative-therapy comparison and should be interpreted cautiously pending independent replication.

Conclusion

In this randomized trial, both Exercise Therapy + TENS and MET + TENS produced substantial within-group improvements in pain, disability, and lumbar range of motion over 6 weeks in adults with sacroiliac joint dysfunction. MET + TENS showed significantly greater improvement than Exercise Therapy + TENS in functional disability and lumbar range of motion, and this advantage was robust to post hoc adjustment for an observed baseline imbalance in pelvic-tilt direction. The unadjusted between-group NPRS difference also favored MET + TENS, but this pain finding was attenuated to non-significance after adjustment for the same imbalance and should therefore be interpreted more cautiously than the disability and ROM findings. Given the absence of prospective trial registration, the lack of participant/therapist blinding, the single-region setting, and the unusually large effect sizes observed for some outcomes, these results should be regarded as encouraging but preliminary evidence for an incremental benefit of MET over exercise therapy as a TENS adjunct in SIJD, warranting confirmation in an adequately powered, prospectively registered trial that stratifies randomization by pelvic-tilt direction.

Declarations

Ethics approval and consent to participate: Ethical approval was obtained from the Institutional Ethics Committee prior to the commencement of the study, and the study was conducted in accordance with the approved protocol. Written informed consent was obtained from all participants prior to enrolment, and participants retained the right to withdraw at any stage without consequence.

Consent for publication: Not applicable. This manuscript does not contain identifiable individual participant data or images.

Trial registration: Not registered. Prospective trial registration was not undertaken for this postgraduate dissertation study.

References

1. Barros, G., McGrath, L., & Gelfenbeyn, M. (2019). Sacroiliac joint dysfunction in patients with low back pain. *Federal Practitioner*, 36(8), 370–375.
2. Chaitow, L. (2013). *Muscle energy techniques* (4th ed.). Churchill Livingstone Elsevier.
3. Copay, A. G., & Cher, D. J. (2015). Is the Oswestry Disability Index a valid measure of response to sacroiliac joint treatment? *Quality of Life Research*, 25, 283–292. <https://doi.org/10.1007/s11136-015-1095-3>
4. García-Peñalver, U. J., Palop-Montoro, M. V., & Manzano-Sánchez, D. (2020). Effectiveness of the muscle energy technique versus osteopathic manipulation in the treatment of sacroiliac joint dysfunction in athletes. *International Journal of Environmental Research and Public Health*, 17(12), 4490. <https://doi.org/10.3390/ijerph17124490>
5. Gartenberg, A., Nessim, A., & Cho, W. (2021). Sacroiliac joint dysfunction: Pathophysiology, diagnosis, and treatment. *European Spine Journal*, 30(10), 2936–2943.
6. Greenman, P. E. (2016). *Greenman's principles of manual medicine* (5th ed.). Wolters Kluwer.
7. Hawker, G. A., Mian, S., Kendzerska, T., & French, M. (2011). Measures of adult pain. *Arthritis Care & Research*, 63(S11), S240–S252. <https://doi.org/10.1002/acr.20543>
8. Joshi, R., Rathi, M., Khandare, S., & Palekar, T. (2017). Effect of muscle energy technique on pain and function in patients with sacroiliac dysfunction — experimental study. *International Journal of Scientific Research and Education*, 5(6). <https://doi.org/10.18535/ijser/v5i06.01>
9. Khalid, A., Bashir, M., Ikram, S., Ahmad, J., Athar, M., & ul Mustafa, A. (2022). The comparison of muscle energy techniques with core muscle strengthening exercises in subjects with sacroiliac joint dysfunction. *Pakistan Journal of Health Sciences*, 3(4), 198–202.
10. Laslett, M. (2008). Evidence-based diagnosis and treatment of the painful sacroiliac joint. *Journal of Manual & Manipulative Therapy*, 16(3), 142–152.
11. Mohammed, S., Kamel, R., Abutaleb, E., Lasheen, Y., & Alshaarawy, F. (2019). Effect of muscle energy technique in chronic sacroiliac joint dysfunction. *South Valley University International Journal of Physical Therapy and Sciences*, 1, 8–19. <https://doi.org/10.21608/svupts.2019.230984>
12. Nejati, P., Safarcherati, A., & Karimi, F. (2019). Effectiveness of exercise therapy and manipulation on sacroiliac joint dysfunction: A randomized controlled trial. *Pain Physician*, 22(1), 53–61.
13. Shimpi, A., Hatekar, R., Shyam, A., & Sancheti, P. (2018). Reliability and validity of a new clinical test for assessment of the sacroiliac joint dysfunction. *Hong Kong Physiotherapy Journal*, 38(1), 13–22. <https://doi.org/10.1142/S1013702518500026>
14. Sonam, S., Naseer, A., Ramzan, F., Iqbal, M., & Shehzadi, K. (2025). Effects of muscle energy techniques in addition to routine physical therapy on pain and functional disability in post-partum females with sacroiliac joint dysfunction. *Health Journal of Physiotherapy and Rehabilitation Sciences*, 4(4), 25–33.
15. Soomro, R. R., Karimi, H., & Gilani, S. A. (2024). Comparative efficacy of quadratus lumborum muscle energy technique with gluteus medius strengthening versus gluteus medius strengthening alone in sacroiliac joint dysfunction: A randomized controlled trial. *Diagnostics*, 14(21), 2413. <https://doi.org/10.3390/diagnostics14212413>

16. Telli, S. H., Telli, H., & Topal, M. (2018). The validity and reliability of provocation tests in the diagnosis of sacroiliac joint dysfunction. *Pain Physician*, 21(4), E367–E376.
17. Vaidya, S. (2018). Sacroiliac joint mobilisation versus transcutaneous electrical nerve stimulation for pregnancy induced posterior pelvic pain — a randomised clinical trial. *Journal of Clinical and Diagnostic Research*, 12, YC04–YC07. <https://doi.org/10.7860/JCDR/2018/26696.10781>