Peer Review File

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Reviewer A

Comment 1: The first words of the title are "Short-term change", yet the definition of short-term was not given until line 125 of the manuscript, leaving readers to infer that post-operative evaluations were done later (3 and 6 months would have been common). It would be helpful if the Abstract said at least "This short-term study aimed…".

Reply: Thank you for the comment. It does need to be stated in the OBJECTIVE. We have modified the content as advised, which is highlighted in the revised manuscript (Page 1, line 5).

Comment 2: The Abstract Conclusion states that "…higher spasticity in the tibialis anterior and soleus muscles increase the risk of developing internal tibial torsion", but in Table 3, MAS scores preoperatively were 1's, insufficient to support the statement.

Reply: We have reviewed the data and redo the tables. The data indicate that limbs with tibial internal rotation have higher mAS scores for the tibialis anterior and soleus muscle groups, and this difference is statistically significant. In our clinical practice, it is rare to find mAS scores for tibialis anterior muscle tension greater than 2 points.

Comment 3: The conclusion that "SDR has the potential to improve tibial torsion in children with spastic CP" may be statistically true, but the result that 79% did not improve belies the validity of that conclusion.

Reply: Before SDR, 257 lower limbs in 148 cases were classified as internal tibial torsion. After SDR, 21% limbs changed into limbs with normal TMA angle. Though the other 79% (203/257) were still grouped as internal tibial torsion, 65% (166/257) of them showed an outward change in the TMA angle after SDR, indicating that the improvement of internal tibial torsion could be seen in a majority of limbs. Nonetheless, the content was not fully discussed in this manuscript,

which was caused by our negligence. These sentences are added to the part of results (Page 7, line 177-181).

Comment 4: The role of spasticity in the gluteus medius and minimus muscles (lines 54-55) as contributors to internal tibial torsion (ITT) is debatable. Do publications other than #2 in the bibliography support that statement?

Reply: Thank you for your suggestion. We agree that the role of spasticity in the gluteus medius and minimus muscles as contributors to internal tibial torsion (ITT) is debatable, for the reason that we could only find one publication as the bibliography. As a result, we have mofidied the sentence as "In children with spastic CP, tibial torsion may result from abnormality of multiple lower limb muscles, such as the hip adductor, medial hamstring, and tibialis muscles." (Page 2, line 32-34).

Comment 5: After the sentence in lines 63-64 about SDR reducing spasticity, consider citing a couple of publications that document the consistent, substantial, improvement in lower extremity spasticity after SDR.

Reply: We added some publications that document the consistent, substantial improvement after SDR. These publications confirm the surgical effect of SDR from long-term follow-up, and these publications were listed as follows:

1. Tedroff, K., G. Hägglund, and F. Miller, Long-term effects of selective dorsal rhizotomy in children with cerebral palsy: a systematic review. Dev Med Child Neurol, 2020. 62(5): p. 554-562.

2. Morota, N., [Functional posterior rhizotomy for treatment of spasticity]. No Shinkei Geka, 2010. 38(3): p. 209-28.

3. Dudley, R.W., et al., Long-term functional benefits of selective dorsal rhizotomy for spastic cerebral palsy. J Neurosurg Pediatr, 2013. 12(2): p. 142-50.

4. Nordmark, E., et al., Long-term outcomes five years after selective dorsal rhizotomy. BMC Pediatr, 2008. 8: p. 54.

5. Tedroff, K., et al., Does loss of spasticity matter? A 10-year follow-up after selective dorsal rhizotomy in cerebral palsy. Dev Med Child Neurol, 2011. 53(8): p. 724-9.

These publications were added to the manuscript with highlight in REFERENCE.

Comment 6: If a "few studies have investigated the specific effects of SDR in tibial torsion", line 65, they should be cited and their results presented.

Reply: Thanks for your reminding. We searched in PUBMED and found no publication focusing in the effect of SDR on tibial torsion, so we used the word "few" to express the negative result. In the revised manuscript, we have changed the expression as "However, very few studies have investigated the specific effects of SDR on tibial torsion." (Page 2, line 43).

Comment 7: Was only one two-week postoperative examination done by the physiotherapist? Have the authors considering re-evaluating recent patients in the study at longer intervals after SDR?

Reply: You are absolutely right. In this paper, we have chosen two-week interval as the time point to investigate the immediate change of TMA angle after SDR, which could eliminate the influence caused by the post-operative rehabilitation therapy. Long-term data follow-up is still being tracked. In subsequent studies we will focus on the mid-term and the long-term follow-up data to see whether the surgical effect on tibial torsion would last long. These contents were also listed as limitation (Page 9, line 246-248).

Comment 8: The authors state that ITT is related to spasticity in the tibialis anterior and soleus muscles". The soleus muscles were apparently not monitored during SDR (lines 86-87) nor graded postoperatively (lines 122-123) but in lines 194-195 "The muscle tone of the tibialis anterior and soleus was higher in limbs with ITT…" and in the discussion, "limbs classified as internal rotation had higher muscle tone in the soleus compared to normal limbs (lines 229-230). This needs to be clarified.

Reply: Thanks for the reminding, it does need to be clarified. The muscle tone of Soleus was evaluated both before and after SDR, which was mentioned in the previous version of manuscript and highlighted in the current manuscript (Page 3, line 61-63). Soleus is a wide flat leg muscle lays immediately deep to the gastrocnemius. Owing to the muscle position, it is hard for us to monitor Soleus precisely during SDR by inserting needle electrodes into the muscle.

We are seeking to find ways to monitor Soleus during SDR, it would definitely improve the surgical precision.

Comment 9: Who did the statistical analysis? Their work should be acknowledged in the manuscript, if not in the list of authors.

Reply: Dr. Wenbin Jiang and Dr. Rui Wang, both of whom are listed as authors, completed the statistical analyses.

Comment 10: The preoperative GMFM-66 score of the entire group is of questionable value.

Reply: We checked the original data of this manuscript, and there were no mistakes. To demonstrate the GMFM-66 score in a clearer way, we just sub-grouped the entire group into different GMFCS level. The average GMFM-66 score of the whole group seems to be rather high, as it might result from the rather high percentage of patients classified as GMFCS level 1 and level 2 (Table a). The rhizotomy protocol adopted in our center was designed to reduce muscle tone in the certain target muscle groups, and it was proved to have the ability to treat patients with focal spasticity.

GMFCS level	Gross motor function classification system (n, %)	Gross motor function measurement - 66 (mean ± SD)
Level 1	16 (10.8%)	81.4 ± 3.0
Level 2	50 (33.8%)	69.4 ± 3.9
Level 3	55 (37.2%)	58.2 ± 4.0
Level 4	25 (16.9%)	46.6 ± 5.1
Level 5	2 (1.4%)	26.3 ± 1.9

Table a. GMFM-66 for Different GMFCS Levels.

Comment 11: It does not seem appropriate to combine data about nerve roots and rootlets. Data from stimulation and division of a single nerve root are quite different than stimulation of its 3-5 branches. Does the statement in line 166 that "an average of 55.9 +/- 11.6 nerve roots/rootlets

were electrically stimulated and a total of 7.2 +/- 3.2 dorsal roots/rootlets …were partially transected…"" refer to the average of all 148 children, regardless of their topographical subtype and all their various MAS scores? If so, those data are of questionable value.

Reply: Thanks for pointing out this opinion. I can't agree more that stimulating the single nerve root would result in different EMG output when compared to stimulating the branches of the root. Nonetheless, the SDR performed in our center is a L2 single-level SDR (L2-L3 interlaminar approach). Through the single level exposure, we would test the L2 nerve roots and nerve rootlets below level L2, and we have no choice but to test all the nerve fibers divided by their natural boundaries. As a result, we use the statement "roots/rootlets". As for the number of transected nerves, we believed that it would be better if we demonstrate it by the classification of GMFCS level (we have added the content to supplementary data). As we can see that the number of transected dorsal roots would increase as the GMFCS level gets higher (Table b), which was similar to the results in publications written by Professor Nobuhito Morota and Professor Bo Xiao. To make the data clearer, we just add a new table here.

GMFCS level	Number of transected dorsal roots/rootlets (mean ± SD)
Level 1	3.3±1.2
Level 2	6.7±3.1
Level 3	8.2±2.6
Level 4	8.9±3.2
Level 5	10.0±4.2

Table b. Intraoperative Number of Transected Dorsal Roots/Rootlets for Different GMFCS Levels.

Comment 12: The average division of 11.9% of rootlets would be much lower than in most SDR publications and would not be expected to be associated with much clinical effect. The 21% improvement the authors report is more commensurate with the small proportion of rootlets divided than with the enormous statistical significance they report.

Reply: Thanks for raising a question that many readers might have. We mentioned an 11.9%

rootlet division rate, which refers to the overall nerve root division rate. Specifically, if a patient has a total of *x* sensory nerve roots/rootlets detected during SDR, and according to the rhizotomy protocol, *y* of them needs to be transected (with each sensory fiber cut 50%), then the proportion of sensory nerve division for that patient is calculated as *y/x*100%.* This might clarify that the 11.9% division rate is in reference to the total nerve roots. The specific data is listed in Table b.

Comment 13: Lines 239-241 state, "Our data suggest that internal tibial rotation does not improve spontaneously with time in children with spastic CP, emphasizing the importance of early intervention for tibial torsion." Data to validate that statement would require the serial examinations of children with tibial torsion over time. This study does not do that.

Reply: You are absolutely right. Indeed, our data does not support this viewpoint, and it is inappropriate to include this statement in the article. Therefore, we have deleted it in the revised manuscript (Page 8, line 224).

Comment 14: Reference 24 is cited to support the statement that SDR is a safe and effective surgical approach for reducing muscle spasticity in the lower limbs of children with spastic CP." That publication is a systemic review of complications of SDR. Better references are widely available.

Reply: Thank you very much for this useful feedback. we added some publications (Page 8, line 211).

Comment 15: It seems inappropriate to state that "SDR offers a promising approach for treating tibial torsion in children with spastic CP,' (line 283) based only on data obtained once, two weeks postoperatively, and then, with only a 21% improvement.

Reply: Thank you for your suggestion. We have carefully considered your advice and believe that the term "promising approach" is not precise enough. We have made the following modification to this sentence: "SDR has the potential to improve tibial torsion in children with spastic CP" (Page 1, line 18).

Comment 16: TABLE 1

The right column, "Normal range for tibial torsion" should be modified. First, because the numbers might be mis-interpreted as ranging from the degrees listed on the left side (e.g.1) to negative numbers in the right column (e.g., -2). More importantly though, there is no "normal range for tibial torsion"—there are normal TMA ranges but numbers out of those ranges reflect tibial torsion.

Reply: Thank you very much for your suggestion. We have made the following modifications to Table 1 in the manuscript, by replacing the symbol "-" with "~".

Age (years)	Normal range for TMA(°)
0 ~ 2 years	0° ~ 2°
2 ~ 4 years	2° ~ 4°
4 ~ 5 years	4° ~ 8°
5 ~ 6 years	8° ~ 13°
≥ 6 years	13° ~ 18°

Table 1. Normal ranges of TMA for children of all ages in the Shanghai Disabled Persons' and Federation.

Comment 17: TABLE 2 Should be modified. The data in it are all preoperative. None of the data would change postoperatively other than perhaps GMFCS. Unless GMFCS changed in some of the patients, the heading should omit the "Post-operational status"

Reply: To make the table clearer, we have removed the preoperative Demographic section as it has already been introduced in the text.

Comment 18: Data in the SPASTICITY table would usually be presented as number +/- standard deviation, and the statistical evaluation done on that data. Q1 and Q3 are defined but their interpretation is not described. What is known is that the small decreases reported in MAS scores would not be clinically consistent with the dramatically significant statistical numbers

Reply 18: In our previous manuscript, we chose to use the term [median, (Q1, Q3)] (Q1 representing the 25th percentile and Q3 representing the 75th percentile) as the data does not

conform a normal distribution (data conforming to normal distribution could be presented as mean ± SD). However, in order to more clearly visualize the significant changes between the data, we have made adjustments to the table.

Characteristics	Pre-operational status	Post-operational status	*p* value
Spasticity (Modified Ashworth Scale Score) （mean ± standard deviation）			
Hip adductors			
Left	2.9 ± 1.3	1.7 ± 0.9	<0.0001
Right	2.9 ± 1.2	1.7 ± 0.8	<0.0001
Quadriceps			
Left	0.9 ± 1.2	0.1 ± 0.4	<0.0001
Right	0.8 ± 1.2	0.1 ± 0.4	<0.0001
Hamstrings			
Left	3.2 ± 1.1	2.7 ± 0.9	<0.0001
Right	3.3 ± 1.0	2.8 ± 0.8	<0.0001
Tibialis Anterior			
Left	0.6 ± 0.5	0.3 ± 0.5	<0.0001
Right	0.6 ± 0.6	0.3 ± 0.4	<0.0001
Gastrocnemius			
Left	4.6 ± 0.7	3.9 ± 0.8	<0.0001
Right	4.8 ± 0.5	4.0 ± 0.7	<0.0001
Soleus			
Left	4.1 ± 1.0	3.0 ± 0.9	<0.0001
Right	4.2 ± 0.8	3.1 ± 0.9	<0.0001
Range of motion （mean ± standard			

deviation）			
Hip abduction			
Left	74.7 ± 12.5	82.3 ± 8.6	<0.0001
Right	75.0 ± 12.5	82.0 ± 9.3	<0.0001
Knee flexion			
Left	150.1 ± 4.3	151.4 ± 3.7	<0.0001
Right	150.1 ± 4.6	151.3 ± 3.8	<0.05
Ankle Dorsiflexion (Knee extended)			
Left	-6.3 ± 15.0	6.9 ± 10.6	<0.0001
Right	-6.7 ± 14.0	3.9 ± 14.8	<0.0001
Ankle Dorsiflexion (Knee flexed)			
Left	6.2 ± 12.8	15.8 ± 7.6	<0.0001
Right	5.5 ± 11.8	14.5 ± 7.8	<0.0001
Tibial torsion degree			
Left	3.1 ± 2.9	6.0 ± 2.1	<0.0001
Right	4.2 ± 3.5	6.7 ± 2.6	<0.0001

Table 2. Clinical details of patients before and after selective dorsal rhizotomy.

Reviewer B

Comment 1&Comment 2: 98: which method has been intraoperatively used for the section of the rootlets? Which percentage of rootlets has been cut on average?168: rhizotomy protocol: if you talk about a "protocol" you should explain how is it formed and which methods it includes.

Reply: Thanks for the suggestion. The rhizotomy protocol was previously published（Xiao, B., et al., The role of intra-operative neuroelectrophysiological monitoring in single-level approach selective dorsal rhizotomy. Childs Nerv Syst, 2020）, and the explicit description could be listed as:

Intraoperative nerve rootlet stimulation and EMG interpretation

EMG needle electrodes were placed in the bilateral hip adductors, rectus femoris, hamstring, gastrocnemius medialis, gastrocnemius lateralis, peroneus longus, tibialis anterior, and anal sphincter. Direct stimulation over those nerve rootlets was achieved through bipolar stimulation with rhizotomy probes. All stimulation and EMG recordings were conducted using Cadwell-Cascade Elite neurophysiological monitoring system.

Single pulse of electrical stimulation with duration of 0.2 ms and intensity started since 0.00 mA with step of 0.01 mA was applied to differentiate the following: (1) whether the rootlet stimulated was motor or sensory (differentiation of ventral and dorsal nerve rootlets was based on the threshold of stimulation, and the latency of EMG responses to the stimulus); (2) if it was sensory rootlet, whether stimulation initially innervated target muscle responses (among 15 channels monitored, EMG responses in which first reached 200 μV amplitude). If yes, then we perform train stimulation on the same rootlet for 1 s with stimulation duration of 0.2 ms, intensity of by which 200 μV EMG amplitude could be reached during single pulse stimulation, and in a frequency of 50 Hz, in order to decide whether we needed to section it in 50% or 75%.

New rhizotomy protocol

Our modified rhizotomy protocol was briefly graded into three levels (Fig.):

1. Dorsal rootlets that evoke EMG response in non-target muscles were left intact.

2. Dorsal rootlets that could evoke responses in a target muscle with NO EMG responses observed in the opposite extremities during train stimulation were transected 50%.

3. Dorsal rootlets that could evoke responses in a target muscle with EMG responses observed in the opposite extremities during train stimulation were transected 75%.

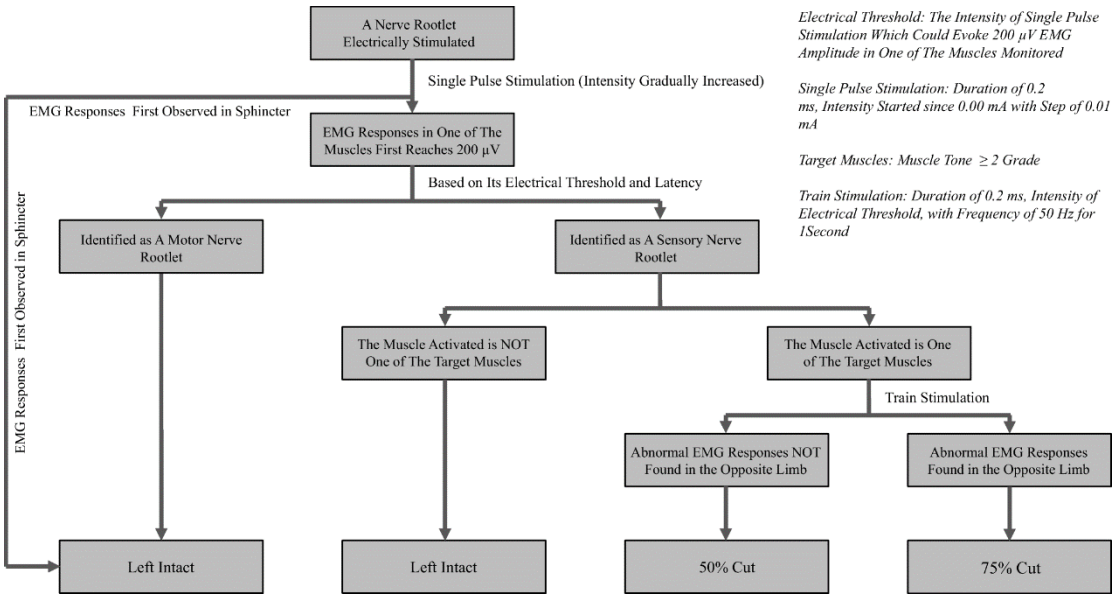

Figure a. The rhizotomy protocol we use in our center.

Comment 3: 125: could you better explain the concept of "target muscles"? Did you cut only the roots of these target muscles? Were all other roots left intact?

Reply: Based on the rhizotomy protocol, any muscle of hip adductor, hamstring, gastrocnemius with its pre-op muscle tone ≥ mAS grade 2 was marked as target muscle before SDR. We only cut the sensory nerve roots/rootlets that mainly elicited EMG responses in target muscle groups.

Comment 4: 177: it is not clear which results this p value refers to. How was this significant "difference in muscle tone" evaluated to calculate this specific p value?

Reply: This *p*-value represents the statistical significance of changes in quadriceps and hamstring muscle tone before and after the surgery. We have changed the expression "the value of MAS score for quadriceps decreased (left side: 0.86±1.24 vs. 0.13±0.39, $p < 0.0001$; right side: 0.83±1.21 vs. 0.13±0.43, $p < 0.0001$), and the muscle tone of bilateral hamstrings also decreased (left side: 3.23±1.08 vs. 2.69±0.86, $p < 0.0001$; right side: 3.28±1.03 vs. 2.77±0.79, $p < 0.0001$)", and was highlighted in the revised manuscript.

Comment 5: 199 and l. 264: it is not clear the aim of the inclusion of this study group of hemiplegic patients. Despite the idea of using the intact limbs as a control group, the absence of difference in TMA between intact and affected limbs seems a little bit confusing. Do these

compensatory mechanisms make the intact limbs somehow similar to the affected ones? On the other side in Table 4 one can see that all these hemiplegic patients were classified as GMFCS 1 and 2: this could suggest that even the affected limbs were just minimally deformed in terms of TMA. The compensatory mechanisms of the intact limbs, mentioned in l. 264, could thus be very questionable. The Authors could add a few sentences to make these paragraphs more clear.

Reply: Exactly, what you've said is correct. In clinical observation, it has been found that some children with cerebral palsy exhibit unilateral intracranial lesions in their MRI images, such as unilateral para-ventricular leukomalacia, and these children would present with focal spasticity in the affected sides. The rehabilitation therapist would considers these individuals as hemiparetic.

As for the compensatory mechanism, it has also been a significant challenge for us. We speculate that these children may undergo some unknown compensatory changes in the spinal cord neural network shaping process. However, the specific mechanisms behind this phenomenon remain unclear.

Comment 6: l. 232: typically: "normally" would sound better if it refers to to development of a child.

Reply: Thank you for your advice; we have changed the word into "normally". (Page 7, line 202)

Comment 7: Table 2: it is a pity that the postoperative GMFM-66 scores are not available.

Reply: We agree, but these children have not fully recovered two weeks after the surgery, so the GMFM-66 scores are not available.

Reviewer C

Comment 1: The main focus of the paper is the change of medial tibial torsion represented by TMA in the paper, not by the change of mAS or ROM. It would be fine to describe the change of mAS as a result of rhizotomy, but I am not sure if ROM measurement means any sense in the paper.

Reply: Indeed, the focus of this article is on the TMA. However, we believe that the TMA, especially internal tibial rotation, has something to do with the muscle and ROM. As a result, we studied the pre- and post-operational muscle tone, ROM. The results indicate a correlation between TMA and muscle tension but no significant relationship with ROM.

Comment 2&Comment 3: The Modified Ashworth Scale is scored from 0 to 4 with 6 levels, not scored 0 to 5. In addition, score 3 & 4 are highly spastic with moderate to severe joint contraction often associated, and usually observed in children with GMFCS level 4 to 5. It should not associate with children with GMFCS level 1 to 2. Considering the fact that about 45% of the patients in the paper are GMFCS level 1 & 2, mAS shown in table 3 are apparently too high in score. Furthermore, mAS in hamstring in the normal tibial torsion children is higher than those with internal tibial torsion. It seems it does not make sense. The authors should reevaluate the original data. In general, frequency of the internal tibial torsion correlates with severity of spasticity. Thus, classification based on GMFCS level is essential and critical. However, the authors analyzed the data as whole. The data based on GMFCS level is the must to be described in the paper and to lead the conclusion.

Reply: Indeed, the modified Ashworth Scale is scored from 0 to 4 with 6 levels. However, for statistical purposes, some scholars have proposed the concept of the modified Ashworth Scale Score, aiming to convert the muscle tension level of 1+ to a score of 2 that can be subjected to statistical analysis. The specific correspondences are as follows:

Modified Ashworth Scale	Modified Ashworth Scale score
0	Score 0
1	Score 1
1^{+}	Score 2
2	Score 3
3	Score 4
4	Score 5

Table c. Corresponding relation between mAS and mAS score.

As for the specific value in mAS score, we sub-grouped patients into different GMFCS levels,

and this might answer the question. As the elevation of GMFCS levels, the score of mAS just elevated.

Characteristics	GMFCS level 1 (n=16)	GMFCS level 2 (n=50)	GMFCS level 3 (n=55)	GMFCS level 4 (n=25)	GMFCS level 5 (n=2)
Hip adductors					
Pre-operational status	0.8±0.7	2.5±1.2	3.5±0.6	3.9±0.3	4.0±0.0
Post-operational status	0.9±0.6	1.5±0.7***	1.9±0.8***	2.4±0.7***	3.0±0.0*
Quadriceps					
Pre-operational status	0.0±0.0	0.1±0.5	1.0±1.1	2.4±1.3	2.5±0.6
Post-operational status	0.0±0.0	0.0±0.0**	0.1±0.3***	0.5±0.8***	0.5±0.6
Hamstrings					
Pre-operational status	1.7±1.0	3.0±1.1	3.6±0.6	3.9±0.6	3.8±0.5
Post-operational status	2.0±0.7**	2.5±0.9***	3.0±0.6***	3.2±0.6***	2.5±0.6
Tibialis Anterior					
Pre-operational status	0.2±0.4	0.4±0.5	0.7±0.5	1.0±0.4	0.5±0.6
Post-operational status	0.0±0.0*	0.1±0.3***	0.4±0.5***	0.6±0.5***	0.5±0.6
Gastrocnemius					
Pre-operational status	4.2±1.1	4.7±0.6	4.8±0.4	4.9±0.3	5.0±0.0
Post-operational	3.7±1.0**	3.9±0.7***	4.1±0.8***	4.0±0.7***	4.5±0.6

status					
Soleus					
Pre-operational status	3.4±1.6	4.1±0.9	4.3±0.7	4.3±0.6	4.5±0.6
Post-operational status	2.9±1.2**	3.1±0.9***	3.1±0.8***	2.9±0.8***	4.0±1.2

Table d. Clinical data of muscle tone in mAS score（mean ± standard deviation）for Different GMFCS Levels.

Statistical symbol:

*: $p < 0.05$, compared with pre-operational status

**: $p < 0.01$, compared with pre-operational status.

***: $p < 0.001$, compared with pre-operational status.

Comment 4: The change of the actual value of TMA is not shown at all in the text. The authors just mentioned improved or worsened. Even if it is shown in the table, it should be described in detail in the text. Actual data based on the each GMFCS level is required. Lack of the actual TMA data could be fatal as a scientific paper. Statistical analysis should be done on the change of TMA, not the number of patient who were normalized after rhizotomy.

Reply: Thanks for your recommendation, we have added the content to the text.

GMFCS level	Pre-operational status（mean ± standard deviation）	Post-operational status（mean ± standard deviation）	*p* value
level 1	5.8±4.3	8.3±3.3	<0.0001
level 2	3.7±3.3	6.4±2.3	<0.0001
level 3	3.5±2.7	6.0±2.2	<0.0001
level 4	2.6±3.0	5.7±1.8	<0.0001
level 5	2.5±1.3	4.8±0.5	/

Table e. TMA of limbs derived from different GMFCS Levels.

Comment 5: In order to investigate the factors, the authors used the data of normal side of hemiplegic children. As I mentioned above, those children with hemiplegia are usually associated with mild to moderate degree of spasticity like GMFCS level 1-2, and not appropriate to investigate the factors contributing the development of tibial torsion.

Reply: Indeed, we have a higher proportion of children with GMFCS levels 1-2, whereas most other articles primarily focus on children with higher GMFCS levels. However, we have found that among our patients with tibial torsion, there is a significant portion with milder symptoms, which is often overlooked.

Comment 6: Some results are described in the part of Conclusion. For example, line 246-257, and line 269-270. In addition, figure caption of Figure 4 should be moved in the Result part.

Reply 6: Thank you for your suggestion. We have removed the content that should belong to the results section. The figure caption for Figure 4 has already been placed in the results section, but it might not have been displayed due to formatting reasons.

Comment 7: Line 166-167: Number of nerve roots/rootles and the dorsal roots/rootles which were cut partially are compared. What is the total dorsal roots/rootlets among the total number of roots/rootlets? What is the cutting rate of the dorsal roots/rootlets?

GMFCS level	No. of sensory nerve roots/rootlets transected during SDR	Total number of non-sphincters related sensory nerve roots/rootlets	Average percentage of transected dorsal nerve roots/rootlets
level 1	3.3±1.2	44.5±12.3	7.3%
level 2	6.7±3.1	45.8±11.8	14.6%
level 3	8.2±2.6	44.6±10.0	18.4%
level 4	8.9±3.2	46.0±9.9	19.4%
level 5	10.0±4.2	48.5±7.8	20.6%

Comment 8: L191: What is the GMFCS level in those children with external tibial torsion?

Reply: Among 148 patients, 3 of them were children with external tibial torsion, and out of 296 limbs, 4 were limbs with external tibial torsion. Their GMFCS levels were as follows: 1 at level I, 2 at level III, and 1 at level IV. In Figure 2, we display the outcomes of this subset of patients.

Comment 9: Line 194-195 & 195-196: Please show the statistical data.

Reply: To visualize the data changes more intuitively, we made adjustments to Table 3.

Comment 10: Line 251-253: Please demonstrate GMFCS level to conclude that degree of spasticity correlates with tibial torsion. The description needs background data.

Reply: Thanks for your comments, we searched in PUBMED and found no publication focusing in the degree of spasticity correlates with tibial torsion

Comment 11: Line 255-256: Again, skeletomuscular deformity usually associate with GMFCS level 4-5 children. Please show the background data.

Reply: I feel that this comment somewhat differs from our article. The comment suggests that children with GMFCS levels 4-5 are more likely to experience skeletal muscle deformities. However, what we intend to convey is that the etiology of ITT may not be solely related to muscle tone factors but could also be associated with secondary skeletal muscle deformities. Nevertheless, we do not possess direct evidence to substantiate this viewpoint, and further research is needed.

Comment 12: Line 267: Does it mean that spasticity in the hamstrings is the cause of tibial torsion? Please show the actual data of the change of TMA and the change of mAS in the hamstrings.

Reply: Spasticity in the hamstrings is not the cause of tibial torsion. What we want to express is that we found that children with lower preoperative hamstring muscle tension showed more significant improvement under our surgical protocol.

Comment 13: Line 271-281: I simply think it caused by the level of cut roots/rootles and its cutting rate.

Reply: That's possible. We followed the protocol, but it doesn't rule out the possibility.

Comment 14: Line 283-286: It is the conclusion, not the discussion.

Reply: You're right, we made the necessary modifications.

Comment 15: Line 288-297: The stufdy limitation should be shown as separate part of the text as "Study limitation"

Reply: You're right, we made the necessary modifications (Page 9, line245).

Comment 16: Table 1: GMFM should be shown in each GMFCS level.

Reply: We have modified this table as requested.

	Gross motor function classification system (n, %)	Gross motor function measurement - 66 (mean ± SD)
Level Ⅰ	16 (10.8%)	81.4 ± 3.0
Level Ⅱ	50 (33.8%)	69.4 ± 3.9
Level Ⅲ	55 (37.2%)	58.2 ± 4.0
Level Ⅳ	25 (16.9%)	46.6 ± 5.1
Level Ⅴ	2 (1.4%)	81.4 ± 3.0

Comment 17: Table 1: Like in the text, spasticity and mAS should not be mixed. Here, it should be shown just as "Modified Ashworth Scale".

Reply: Thank you for your reminder, we have made the necessary adjustments.

Comment 18: Table 1: As I pointed out before, mAS 4 or 5 in this patients' population seems unrealistic. Q1 and Q3 are 4 or 5 in Gastrocunemius and Soleus, and they are unbelievable for children with GMFCS level 1 or 2 who accounts for nearly 45% of the patients.

Reply: We have previously discussed the evaluation of Soleus muscle tone both before and after SDR, as mentioned in the prior manuscript version and emphasized in the current one (Page 3, lines 61-63). Soleus is a broad, flat muscle situated deep beneath the gastrocnemius in the lower

leg. Due to its position, it poses challenges for precise monitoring during SDR, especially when attempting to insert needle electrodes into the muscle. We are actively exploring methods to enhance the monitoring of Soleus during SDR, as this would significantly enhance surgical precision.

Comment 19: Table 1: Knee flexton 150 (150, 150), is it true that all Q1, median, and Q3 are the same?

Reply: Yes, we just present the result calculated with the statistical software. However, to make the data more persuasive, we have changed the data form into mean ± SD.