Reviewer #1:   
*Summary:*   
This is a very well written manuscript with significant contribution to both clinical and research communities. The author proposed a novel breathing driven stimulation strategy based on the new findings of intrinsic physiological coupling activated during voluntary breathing (from the research group led by the author). The clinical efficacy of this novel strategy was clearly demonstrated in this study for management of neuropathic pain and post-stroke spasticity. There are many other potential clinical applications of this novel electrical stimulation strategy for patients with neuromusuclar disorders or impairments.  
  
*Minor Concerns:*  
Page 3: second paragraph, line 10, typo, "inexpensice"  
REPLY: I appreciate the Reviewer’s encouraging comments. The sentence has been deleted.  
  
Reviewer #2:   
*Summary:*   
The paper presents a unique premise but organization, multiple grammatical errors, insufficient documentation of results and claims that exceed data distract from the overall message. As an example of organizational difficulties, the author, in the Introduction, introduces recovery of hand function in one paragraph and breathing in another without any transitional sentences or ideas. The paper will need editing.  
Reply: a transitional sentence has now been added.

*Minor Concerns:*  
Abstract:   
grammar- 3rd and 5th sentences; also coupling between what?

Reply: the word “coupling” has been changed to “interactions”.  
  
Page 3:   
last sentence, 1st para - grammar correction needed here and throughout

Reply: I have carefully checked grammar throughout and have corrected what I could.

2nd para - reference needed for claim that acupuncture is widely accepted

Reply: a reference has been added

2nd para - integration of which coping mechanisms?  
Reply: The mechanisms (acupuncture, electrical stimulation, aversiveness) have been specified.

last para - reference needed for statement that "this intervention protocol superior to NMES?"  
Reply: references have been added.

Page 4:  
1st para - brief description of MRI results would be helpful

Reply: brief description of MRI results has been provided.

2nd para - More detail is needed regarding comparison and contrast between Brestim and NMES. Author states that Brestim is superior but why would the addition of a breathing trigger result in totally differing results from estim alone? If, as author suggests, breathing augment the effects of stimulation, then wouldn't results be similar?  
Reply: Comparisons between BreEStim and NMES are found in the Discussion section, not in the Introduction section.

Case presentation - For case studies it is typical to have much more patient information than is provided here. (e.g. MRIs, structures involved in stroke, rehabilitation hx more detailed clinical exam, etc.)  
Reply: the stroke patient was discharged from outpatient PT/OT programs because he was stable and plateaued in therapy. MRI results were not available at the time of experiments. These have been clarified in the text. See page 4, last para for details.

Page 5:  
Author reports MAS of 1+ pre-test but only says minimal for post test. What was the actual score. Also does the change exceed the published minimum clinical significance change? Doubtful. Author talks about finger movements but provides no data. Was something like a Fugl-Meyer test used?  
Reply: The post-test assessment of finger flexor tone does not qualify for MAS=1, but not completely normal. This could be due to the MAS measurement itself that is not sensitive enough to detect minor difference, and/or assessment was masked by tone from other muscles, such as wrist flexors or intrinsic muscles. On the other hand, it was a surprise to the author as well that a single session of treatment could lead to such long-lasting reduction in muscle tone and significant improvement in hand function. No Fugl-Meyer tests were used pre- and post-treatment.

Results 1st para - How can author make the claim that Breestim success depends on severity of pretreatment conditions when only one subject was tested?

Reply: This is a method-based paper. Case presentation showcases how the method could be applied for. It does not mean that only one subject was tested. As stated in Results, finger flexor spasticity was reduced from MAS=3 to MAS=1 in another patient.   
  
Discussion, 1st para - "unique approach" This Reviewer agrees but wonders whether it really differs from any other technique that requires voluntary effort to initiate estim. For instance, EMG or electrogoniometry. Perhaps results are due solely to attentional demands of task. Brief discussion  
comparing and discussing these possibilities would be helpful.  
Reply: a sentence has been added:” EMG-triggered EStim also involves active participation33. But BreEStim integrates other additional mechanisms.”

Page 8:  
4th para - "extensive cortical?" Describe or list and reference. What is meant by "short window"  
Reply: references for extensive cortical/subcortical activation during voluntary breathing has been provided. Short window refers to “short window of breathing-associated cortical and subcortical activation”

Page 9:  
1st para - earlier author states that it is not effective with severe spasticity

Reply: In the Results section, the author mentioned that “It is important to note that other patients may not have the amount of spasticity reduction”. Spasticity reduction from MAS=3 to MAS=1 in a patient with severe spasticity does not mean it is not effective. The author does not want to overestimate the effect of treatment. This sentence has been revised further to “It is important to note that other patients may not have the same degree of spasticity reduction and functional improvement”.

4th para - study does not permit the author to conclude "excellent clinical efficacy?"  
Reply: It is been changed to “clinical efficacy”.