Reviewer 1:

General:

The manuscript provides an overview of the multiple potential local therapeutic options in the management of soft tissue sarcomas. The authors provided background and data within the limits of review. However, the review does not sufficiently describe the search for a rigorous systematic review. Nonetheless, it provides certain useful information.

Specific:

1. Under the methods, malignant fibrous histiocytoma is now largely replaced by UPS, undifferentiated pleomorphic sarcoma, search terms can be updated

2. Under metastectomy, the authors can perhaps make suggestions (based on the studies) on the population of patients who may successfully be considered for metastectomy (as pointed out by Abdalla et al. Curr Treat Options Oncol, 3 (2002), pp. 497 and Cardona et al. Curr Prob Cancer 2013; such as presence of controllable primary tumor, no multivisceral metastases, having completely resectable metastatic disease, etc)

Reviewer 2:

The authors review local treatment options for metastatic disease in patients with soft tissue sarcoma. Unfortunately, the review feels very superficial, beginning with simple mistakes - the authors state that the rate of distant metastasis ranges from 25-50% in their abstract and 25-30% in their introduction, without taking grading or the different biological behaviour of different histological subtypes into consideration. Further along in the introduction, the authors name lungs, soft tissue, bones and visceral organs as possible sites for metastases, ignoring regional lymph nodes. The authors also state that the mainstay of therapy for metastatic soft tissue sarcoma is chemotherapy. This statement does not reflect the current guidelines, which recommend chemotherapy as standard treatment for patients with synchronous lung metastases or extrapulmonary metastatic disease, whereas surgery is considered standard treatment for metachronous lung metastases (ESMO guidelines - http://annonc.oxfordjournals.org/content/25/suppl_3/iii102.full.pdf).

Far more limiting than the above mentioned errors, which can easily be amended, is, however, the fact, that the results of the presented studies on local treatment options are presented with no attempt to synthesize them into a greater whole or indeed any discussion whatsoever. The reader of the review cannot help but feel that the authors identified a number of studies on local treatment of metastatic disease and went on to simply list the results of each study.
Reviewer 3:

The topic is highly interesting and the method using a Pubmed and medline search to provide a good review is fine. However, a paragraph on the results of this literature search is clearly missing.

How many papers did the authors identify? How many metastatic patients were included? The authors should also score the quality of these papers? Which time period did these publications cover? Monocenter studies? Prospective trials, .....

Did the authors see a difference concerning the subentities of STS? E.g. myxoid liposarcoma + trabectidin?

In the end I would like to read some clear recommendations – at the moment it is more a collection of possibilities. What can the authors, based on the literature, clearly recommend in the treatment of metastatic STS?

(end)