Reviewer 1

1. A general concern is regarding the originality of the work. While I think the work is original and will recommend it for publication, I don't know if the originality is clear to the reader. The authors argue that “most of the data regarding clinical and pathological variables that may predict prognosis and the benefits of adjuvant therapy are based on small retrospective studies”. First, I would encourage authors to reference some of these papers for their readers.

These references have been added to the quoted sentence, page 4, line 10.

2. Second, the authors reference Chan, Kawar, Shin et al (2008) Br J Cancer in their discussion, who considered the same potential prognostic factors, with the exception of tumor hormone receptor status, for ESS survival among 800+ participants in the U.S. There are some important differences between the studies that I think the authors could acknowledge. Besides the fact that the current study is smaller and represents less of the general U.S. population, there are some important strengths of the current study that could be highlighted. First, the current study includes much more recent follow-up (Chan et al. included diagnosis up until 2003, while the current study includes diagnoses up until 2012). Secondly, Chan et al. report on disease-specific survival, which have some inherent limitations. There is benefit to having information about overall survival and progression-free survival for ESS patients available.

We have added comments to the second paragraph of page 7, highlighting the above comments on the differences between this study and the one by Chan et al.

3. Fourth paragraph: I would recommend referring to Chan et al., 2008 and explaining how your current study can add further.

This reference and explanation has been added.

4. Paragraph 1: what city/town, country was this study conducted in? Please specify for international readers.

Tampa, FL USA has been added.

5. Paragraph 1: what ethical approval and, if required, consent process did you go through to obtain approval to conduct this study? Please specify.

The study was approved by the institutional IRB which was moved from the last sentence of the section to the first for clarification. As this was a retrospective review from data was that was already collected, the requirement for informed consent was waived.

6. Paragraph 1 and 3: you included patients diagnosed January 1990 – April 2012, when was follow-up ceased, when was vital status obtained, what was your censor date?
Vital status and follow-up data was censored December 2014, and this has been added to paragraph 2 of the methods.

7. Paragraph 1: please clarify what is meant by “earliest reported weight and height”. I would assume the record closest to diagnosis would be more ideal. Or did the patients not have medical records prior diagnosis (such as for other cancers, investigations, etc)?

This height and weight corresponded to their initial referral to our institution for EES.

8. Paragraph 2: I would not use the term “abnormally distributed”, I would say “Not normally distributed”.

Abnormally was changed to non-normally.

9. Paragraph 1: what information was typically missing?

Missing data included incomplete clinical or staging information or no follow-up data. This information was added to paragraph 1.

10. Paragraph 1: where those excluded different, on key variables, to those included, and/or do you think the exclusions could have resulted in any significant selection bias?

The majority of the exclusions (20/27, 74%) were based on an administrative database code corresponding to ESS, but a confirmed pathologic diagnosis other than ESS. The 7 patients excluded for incomplete records were primarily treated early in the study period before electronic medical records. Additionally, given that the center is a state and regional referral center, lack of follow-up is often related to significant distance from the center rather than outcome.

11. Paragraph 1: This may be my naivety being from outside the U.S. but what is “Caucasian” a category of? Please define this variable in the methods - what is means, how it was measured (how did the register ascertain this information - did patients self-report it or did clinicians report it), what other categories were reported.

Caucasian refers to white-skinned, of European origin race and this is typically self-reported. This was added to paragraph 1 for clarification.

12. Paragraph 4: I find the following sentence confusing; “Median PFS was 49 (range 24-99) months and OS was 238 months (range 56 months to alive at time of analysis).” I understand that while you censored patients at the end of the study period you may find it misleading to report a maximum survival time. However, you are reporting an average, which I assume is based on all patients, not just those who died? Also, you report a maximum of the progression-free survival – and I assume some patients never had recurrence?
The ranges which included “alive at the time of analysis” have been removed for clarity.

13. You are dealing with a 22 year study period. Some participants may have a very long follow-up period (22 years) and some may have very short follow-up periods (I assume follow-up ended on April, 2012, which means some people may have contributed only days to the study?). Therefore, giving the average survival time can be misleading. I think I would find it most meaningful for you to report the % who survived 1 year and 5 years post diagnosis.

Data was censored December 2014. This information has been added to the manuscript.

14. Paragraph 4: I understand it is not possible with small numbers to look at time trends, but I think some attempt to account for the time of diagnosis is warranted, given you are dealing with a 22 year study period and survival outcomes may have changed overtime. Some attempt to address this would be good - perhaps adjusting for diagnosis in 5 year blocks?

Given the very small numbers and long study time, we do not feel as if a time trend is possible.

15. Last paragraph: Did you not include all ESS cases diagnosed since 1990 at the one cancer center, in your study? Explain the limitation re nonrandom design

All cases were included, but the nonrandom design refers to the use of adjuvant therapy use since this was at the discretion of the treating physicians. The comment “with regards to adjuvant therapy” has been added to the discussion.

16. Last paragraph: I would add further discussion on the limitations – small numbers also mean you were not able to look at time trends, and recruitment from one center limits the generalizability of findings.

These limitations were added to the discussion.

17. Tables: Indicate you are reporting age using median, range/IQR. Indicate you are measuring your categorical variables using n (%) (include %). Make your category labels meaningful (e.g. does smoking history refer to current smokers, ever smoked, ex-smokers, never smoked, etc), and include other categories of variables if warranted (e.g. other categories of – what I assume is – ethnicity). Include referent groups or specify the referent groups in the footnotes of the table. I suggesting leaving out “Adjuvant chemotherapy” from the table and adding a footnote only to explain why it was not included in the analysis

These revisions have been made to the Tables as suggested.
Reviewer 2

18. The statistical method for disease progression or death (multivariate model) is not consistent with the method in results and tables.

   Multivariate results are reported in paragraph 4 of the results section.

19. Pathological diagnosis was based on pathology reports and was lack of pathological review by pathologists.

   Original pathologic diagnoses (and information obtained in reports) were based on original outside pathology (when applicable) and an expert pathology review within the institution. This information has been added to the Methods section.

20. There is no description regarding the statistical method for disease progression or death. Did authors use Cox proportional hazard model for univariate or multivariate analysis? No multivariate analysis was demonstrated in text or tables as described in the abstract.

   Cox proportional hazard models were used and this was added to the methods section. Multivariate results are reported in paragraph 4 of the results section.

21. Authors should provide updated literature for supporting your findings. Three references should be included in your article as those follows.

   These references have been added to the manuscript.

Reviewer 3

22. In table 1: the author show the median age of ESS patients was 51 years, however the range of age is from 19 to 41.

   This was a typo and has been corrected.

23. In table 1: the author introduced that 37 evaluable patients were analyzed who have complete records and confirmed ESS diagnosis. However, the total number of Clinical Stage(FIGO) is 36, one missed without any information.

   Staging could not be determined for one patient. This information has been added to the manuscript.

24. In table 1: the notes described "**" while there is not "**" in the table 1.

   This has been corrected in the Table.
25. In table 2: the numbers of FIGO stage II vs I, FIGO stage III vs I and FIGO stage IV vs I were all 36, which are not true. The numbers of FIGO I, II, III and IV is 18, 6, 3 and 9 respectively based on the information given in table 1. Then FIGO stage II vs I, FIGO stage III vs I and FIGO stage IV vs I should be 24, 21 and 36 respectively.

This was to represent the number of patients who had an evaluable variable, not to indicate the number of each specific stage. This column was deleted for clarity.

**Reviewer 4**

26. The authors must clarify better how they retrospectively selected the patients with ESS based on the pathology reports. How do they know for sure that no patients with undifferentiated sarcoma were included?

Original pathologic diagnoses (and information obtained in reports) were based on original outside pathology (when applicable) and an expert pathology review within the institution. This information has been added to the Methods section.

27. Please clarify why the 2009 FIGO endometrial carcinoma staging system was used and not the 2009 FIGO uterine sarcoma staging system?

This was a typo/oversight and has been corrected to read “staging system for uterine sarcomas.”

28. The authors wrongly conclude that adjuvant therapy might be of benefit for patients with ESS because as a basis for this conclusion they make use of literature data that also include high grade or undifferentiated sarcoma's (like references 21,22,25,26). This must be better discussed.

When making these citations we have noted that the studies include patients with histologies other than ESS, and that ESS specific results were not reported.

Thank you again for your consideration of our manuscript. Please let us know if we can provide further information.

Regards,

Lesly Dossett MD MPH