Referee report
Title: Flare Energy Build-Up in a Decaying Active Region Near a Coronal Hole

The article describes the evolution of a small active region and presents arguments for changes in magnetic field topology leading to a B1.7 flare. In general, a better understanding of these evolutionary changes may shine light on causes of solar flares, and therefore, the results deserve publication. However, the article is poorly written: it is often hard to follow the description given by the authors; in some cases, the description seem to deviate from what one can see in the figures, some statements are given without any supporting argumentation, etc. Examples are provided below, but this list is not all inclusive.

Thus, I believe that the article needs a major re-writing to fit the standards of the ApJ publication.

We thank the referee for the comments. We tried our best to improve the writing of the paper. For example, we clarified many issues, made many minor modifications, investigated the GONG magnetograms, and moved the descriptions of the ambiguity resolution to the appendix. Our responses to the referee’s comments are inserted below.

1. Although the description of data sets is incomplete, it appears that the authors use 90-minute cadence MDI full disk magnetograms to study the evolution of magnetic fields prior to the flare. If so, the cadence could be insufficient for this type of study, because as the authors point out "the photospheric magnetic field on the region evolves rapidly." Insufficient time cadence may be a reason why no significant evolution of photospheric magnetic field associated with flare was found. More appropriate data sets are available, e.g. full disk GONG magnetograms taken with 10-minute cadence.

Thanks for the comments. Higher resolution and higher polarimetric sensitive magnetograms would be certainly useful to clarify these points. The time scale of the evolution of magnetic field triggering flares is commonly hours and not minutes. Unfortunately, THEMIS was not observing on May 17 due to clouds. We examined the GONG magnetograms. Indeed, we found small flux emergence and cancellations before the flare. The text in section 2.4 (p. 6) and the second para. in Section 4 (p. 11) have been revised accordingly. In addition, two GONG magnetograms before the flare have been added to Figure 5 in the revised manuscript.

2. Some examples of statements not supported by any arguments:

a. Pg. 2 "It is well accepted that solar flares [...] are different manifestations of a single physical process [...]"
The reader is left wondering about "what is that single physical process".

Since the meaning of “this single physics process” is not the point of this introduction, the sentence has been revised to:
It is well accepted that solar flares [...] are powered by the release of magnetic free energy [...]"

b. Pg. 4 "The two sets of J-shaped loops are highly sheared."

Similar statements of "highly sheared" loops are repeated throughout the text, but no arguments are given why one concludes that the loops are highly sheared. It is well-known that even potential fieldlines may appear as J-shaped in projection to the image plane, and so, a simple appearance is not sufficient to make a conclusion about the presence or absence of shear.

Using these unsubstantiated descriptions, the author later concludes that the magnetic field is not potential (pg. 6 "The observations discussed in previous section indicate that the coronal magnetic field [...] deviate significantly from a potential field.")

Again, this conclusion hinges on the assumption that the presence of J-shaped loops is an indicator of shear, which is not always correct.

We state that the two sets of coronal loops are highly sheared, because these loops are nearly parallel to the underlying polarity inversion line (i.e., the angle between these loops and the polarity inversion line is very small). We agree that even potential loops may appear as J-shaped in projection to the image plane. However, the comparison of the observed loops with a potential field model also shows that these loops deviate significantly from a potential field.

We have modified the text in the second paragraph of section 2.2 (p. 4).

c. On pg. 11, the authors find a significant difference between observed and modelled magnetic field. They conclude that this difference may be due to uncertainties of observations. There is also an alternative explanation that if the model does not agree with the observations, the model or model assumptions may be incorrect.

We admit that we did not use the right word. What we really want to say is that there is a clear difference between observed and modeled magnetic field. However, this difference is no more than 50 gauss, which is close to the measurement uncertainties. This difference is less likely caused by the fact that the model or model assumptions are not correct. The text in section 3.2.3 (p. 11) has been modified for clarification.
3. Some references in the text and reference list do not much match. Also, references in the text are given to wrong figures e.g. "S1 and S2 in Figure 6a" (pg. 11), but Fig 6a does not have S1 or S2.

We examined the references very carefully, and fixed all of the errors that we found.

4. Generic statements that are not always correct:

Pg. 6 "The force-free approximation is reasonable for active regions because the magnetic pressure is much larger than the gas pressure."

This is not true everywhere in the solar atmosphere.

The force-free approximation is indeed not true everywhere in the solar atmosphere, but it is valid in active regions, as stated. The coronal magnetic field strength in active regions B > 20 G, so the magnetic pressure $B^2/(8\pi) > 16$ dyne/cm$^2$, while the plasma pressure $\sim 1$ dyne/cm$^2$.

Pg. 7 "This method assumes that the magnetic field is on the Sun is more or less radial, which is reasonable outside sunspots."

Vector magnetic field observations indicate that horizontal magnetic fields are present even in quiet Sun areas. Also, field is not vertical in plages.

It is true that horizontal fields have been found in high-resolution vector-field measurements with Hinode/SOT-SP on the quiet Sun (Lites et al. 2008; ApJ, 672, 1237) and in plage areas (Ishikawa et al. 2008; A&A 481, L25). However, it is not clear that such small-scale horizontal fields have much of an effect on the magnetic structure at larger heights. Also, the THEMIS instrument used here cannot observe such fields. Therefore, we have chosen to ignore these horizontal fields reported in the literature, and to focus on the larger magnetic elements (flux $> 10^{19}$ Mx), which are well-observed by THEMIS.

The space-based Hinode/SOT-SP measurements show that the larger magnetic elements in plage regions have structure on sub-arcsecond spatial scales. The field has both vertical and horizontal components, but the vertical component dominates when averaged over spatial scales of 1 or 2 arcseconds, which is the resolution obtained by the ground-based THEMIS instrument. Therefore, the THEMIS data used here seem consistent with the Hinode/SOT-SP measurements when the difference in spatial resolution and sensitivity are taken into account.

Finally, remember that the assumption of vertical field is made only as an intermediate step in resolving the 180-degree azimuth ambiguity, not for the final magnetic field used in NLFFF modeling.
Moreover, the authors contradict themselves. On page 7, they say that for B-total larger than 50 gauss they use full vector. On page 8, however, the reference field is computed from B-longitudinal under the assumption that the field is vertical everywhere. Which of these statements is correct?

Both statements are correct. The statement on page 8 refers to the so-called reference field, which is computed in the first phase of processing BEFORE the 180-degree ambiguity has been resolved. The statement on page 7 refers to the vector field in the second phase of processing AFTER the 180-degree ambiguity has been resolved (using the reference field as a guide).

The reference field is a 3D model field that extends from the photosphere into the corona; the RADIAL component of the reference field ON THE PHOTOSPHERE is computed as B-longitudinal divided by cos(THETA), where THETA is the heliocentric angle. This is only an approximation of the true flux distribution, but that does not matter because the reference field is used only for ambiguity resolution, not for any NLFFF or potential-field modeling.

In order to clarify this issue, we have moved the descriptions (3 paragraphs in Section 3.1) of the ambiguity resolution to the appendix.

5. Figures are not well-marked, which makes it difficult to follow the authors description. For example, on pg. 4, the authors say that "No corresponding dark lane is seen in XRT image at 07:57UT." Contrary to that, I do see a dark feature in XRT frame at 07:57 UT at about the same location as in TRACE. I could be looking at a different feature, so a proper marking would help.

We agree that we did not describe the observation very accurately. Probably we should say that there is a dark feature in XRT frame at 07:57 UT, but this dark feature is very faint in comparison to dark lane in the TRACE images. The text in the second para. of Section 2.2 (p. 4) has been modified.

Also, some features that the authors refer as "loops" in TRACE images seem to correspond to filaments in H-alpha images. A traditional wisdom is that filaments overlie magnetic neutral lines, while loops connect oppositely directed fields across neutral lines. Again, a proper marking would clear this misunderstanding.

In TRACE images, we see a mixture of bright and dark features that correspond to filaments in H-alpha images. We call the dark features filaments, and the bright features are called loops (or hot filaments). The text in the first para. of Section 2.3 (p. 5) has been modified to make this clarification.

It would be useful for the reader to show boxes 1-3 on all panels in Figure 5. Otherwise, the connection between panels is not immediately clear.
Figure 5 has been modified. Boxes have been marked on all panels.

6. Jump in direction of transverse azimuths (pg. 8) is not an artifact, but simply a consequence of a field diverging with height.

Let us first illustrate the problem of the “jumps” more clearly. Figure 1 shows results obtained with the “non-potential magnetic field calculation” technique by Georgoulis (2005; ApJ 629, L69) for resolving the 180-degree ambiguity in the azimuth of the measured magnetic field. We applied this method to the THEMIS vector magnetogram used in the present paper. The lower part of the figure shows the RADIAL component of magnetic field as derived from the vector magnetogram using Georgoulis’ disambiguation method. The region shown is 35 degrees east of disk center, so disk center is on the right-hand side of the figure. Note that most plage elements have a sharp edge on the disk-center side; we believe these edges are artifacts of the disambiguation method.

The upper part of the figure shows why these edges occur. This panel shows the magnetic vectors in a vertical plane through one magnetic element (indicated by the small red line in the center of the lower panel). The direction to the observer is
upward and to the right. In the center of the magnetic element, the magnetic vectors are more or less vertical, but just to the right of center the vectors suddenly "jump" to a much more horizontal orientation. The vectors on either side of the jump lie on opposite side of the line-of-sight to the observer, so this jump is due to the choice made by the disambiguation method. The jumps are too sudden to be simply a consequence of the divergence of the magnetic field with height. In the present paper we argue that the correct solution is more or less vertical throughout the magnetic element, and we adopt a different disambiguation method that does not produce such jumps.

Ideally, the azimuth ambiguity should be done in a coordinate system with the active region "rotated" to a disk center. Since the authors resolve ambiguity in the image plane, the azimuths may appear as changing the direction in a "wrong" place (not at the center of flux element as in Figure 6 example, but at disk center side of it).

The 180-degree ambiguity is with respect to the azimuth angle of the magnetic field in the IMAGE plane (perpendicular to line of sight). Therefore, the ambiguity should be resolved in the image plane, not the horizontal plane on the Sun, as the referee seems to suggest. The difference is quite important when the active region is away from disk center, as is the case here.

In fact, our disambiguation method uses both the "observer" reference frame (longitudinal and transverse fields) and the "heliocentric" frame (B_r, B_theta, B_phi). Vectors are rotated between these frames as needed. The THEMIS measurements are given in the observer's frame, and the 180-degree azimuth ambiguity is defined in that frame. However, we also use a "reference model" which is defined in the heliocentric frame. Therefore, the magnetic vectors of the reference model are projected onto the image plane for comparison with the observed vectors ("acute angle method"). We believe this is the correct approach.

Note that Figure 6 had been changed to Figure 12, in accordance with the moving of the ambiguity resolution descriptions to the appendix.

Furthermore, the authors arguments that making fields vertical somehow improves the ambiguity resolution is at odds with their own finding of "no significant difference between the potential field model and the NLFFF model at photosphere." (pg. 12)

In fact, these findings are perfectly consistent with each other, and are both related to the non-force-free nature of the photosphere. As discussed in section 3.1, the "reference model" used for ambiguity resolution includes upward forces in the photosphere to simulate the effects of magnetic buoyancy on photospheric flux elements. Therefore, the photospheric field in this model is closer to vertical than it would otherwise be (also see Figure 12b in the revised manuscript). This improved the ambiguity resolution. The same upward forces are used in the potential field model and the NLFFF model, so the photospheric fields in these models are more vertical than they would be without such buoyancy effects. The main difference
between the NLFFF model and the potential field model is that one contains a coronal flux rope and the other does not. However, this difference manifests itself only in the corona; at the photospheric level the two models are very nearly the same. This is a consequence of the buoyancy forces - they prevent the horizontal field of the flux rope from penetrating down to the level of the photosphere.

7. Pg.12 "In our case we find no evidence if flux emergence and cancellation that directly related to the flare."

As I have indicated above, the magnetic data used by the authors, may have insufficient cadence to detect flux emergence and cancellation. Also, MDI magnetograms have 30s or 300s "exposure" time. 30s magnetograms are much nosier and may not be show small-scale flux emergence very well. It is not clear what type of MDI magnetograms was used in this article.

The referee is right, some MDI magnetograms we examined are noiser than the others. Therefore, we also investigated the GONG magnetograms, which did show flux emergence and cancelations which may be relevant to the flare. The text has been modified.

8. Pg. 14 "We found no significant difference between the potential field model and the NLFFF model at photosphere. [...] Therefore, NLFFF extrapolation based solely on the photospheric vector magnetogram may not be reliable for this type of active region."

I do not understand the meaning of this "conclusion". Potential and non-potential fields are compared with observed field, and that both agree relatively well with the observations at photospheric level. At the same time, no measurements are taken at the chromospheric level, and so, the above authors' conclusion is based on a trivial fact that potential and non-potential extrapolations produce different extrapolated fields.

The last sentence “Therefore, NLFFF extrapolation based solely on the photospheric vector magnetogram may not be reliable for this type of active region.” has been removed, since we did not perform the test to confirm this implication yet. We also added a reference to the work of DeRosa et al. (2009) (see the first para. of p. 7) to distinguish our “flux rope insertion” method from the “vector field extrapolation” method used by other researchers.

9. (a minor point) pg. 3. "The dispersion (spectral resolution) was 0.0125 A/pixel"

I suggest using "spectral sampling' instead. Traditionally, dispersion is expressed in physical units (e.g., A/mm).

We agree. The text has been modified.