Review History

Dear Dr. Svalgaard:

Thank you for submitting the manuscript "Geomagnetic Semiannual Variation Is Not Overestimated and Is Not an Artifact of Systematic Solar Hemispheric Asymmetry" (2011GL048161) to Geophysical Research Letters. Based on the reviews, I believe that your article requires a major revision, and therefore I cannot accept this version of your manuscript for publication (please see Editorial Policies for major revisions at http://www.agu.org/pubs/pdf/Editorial_GRL.pdf).

You will see that reviewer #2 (reviews enclosed below) found promise in your study. Reviewer #1, however, mentions that significant additional work and explanations are required for the purported conclusions of the paper to be fully supported by the analysis. These issues are important and would need to be addressed in a substantive and constructive way. You may consider expanding your study and submitting a full-length paper to a more specialized journal such as JGR.

If you believe that a revised manuscript can account for these important comments, please include a point-by-point response to all reviewer remarks as well as a "tracked-changes" (changes highlighted or otherwise noted) version of your manuscript when resubmitting. Please also note that some supporting material may be added as auxiliary files, if appropriate.

Your revised manuscript will be treated as a resubmission and will be subject to re-review. A resubmit link will be available to you on your homepage. If you use this link, all of the information from the previous manuscript will be populated in your resubmission form. This link will be active for 6 months from the receipt of this letter, but if you need more time, please contact grlonline@agu.org.

Thank you again for your interest in GRL.

Sincerely,

Editor
Geophysical Research Letters

Reviewer #1 Evaluations:
Science Category: Science Category 4
Presentation Category: Presentation Category C

Reviewer #1 (Comments to Author):

I have three problems with the manuscript by Svalgaard:

1. There does not appear to be anything new in it. Instead, it is a restatement of previous work, mostly his own.

2. The author makes a dogmatic defense of the equinoctial hypothesis as an explanation for the semiannual variation. But the equinoctial hypothesis is not physical, but is, instead, simply a statement of an observation that magnetic activity seems to be a function of the angle between the dipole and the sun-earth line. There author promotes phenomenological descriptions of the data that he asserts supports the equinoctial hypothesis, but in reality the hypothesis does not have enough quantitative power to support such an exercise. Indeed, the Russell-McPherron hypothesis does not either, and without detailed understanding of solar-terrestrial interaction, there has been little progress in separating the two hypotheses. Still the role of the axial hypothesis needs to be considered, as Mursula et al have pursued.

3. Perhaps most importantly, Svalgaard has not looked at the data in the way presented by Mursula et al. This, I think, is crucial. Mursula et al are trying to unravel a mystery. Svalgaard is simply denying their results without actually investigating them. I encourage him to work constructively with other people's results, and to try to respectfully consider their methods. There is much to be learned.

Reviewer #2 Evaluations:
Science Category: Science Category 1
Presentation Category: Presentation Category A

Reviewer #2 (Comments to Author):

This paper is well-written and convincing and deserves publication with only minor modifications. It is important to place the Mursula paper in proper context so that it does not have undue influence.

This manuscript makes assumptions about the reader's knowledge that I suspect are not justified for GRL. The "axial hypothesis" and the "equinoctial hypothesis" are never defined except indirectly through citations. Several sentences of definition would help the general reader.

Line 47: Does the longitude dependence of Kp eliminate the semiannual variation or simply muddle and smooth it. Of course no maps in UT-season are possible, but why not the seasonal variation?

Again, IHV index is not well known and could use a sentence in addition to the citations. At line 53 it would be useful to explain why the IHV index eliminates the problem present in the range indices (K) discussed above.

Line 73: Why does the Mursula effect mean no UT variation?

Line 130: The quantity "A" does not seem to be defined anywhere that I could find.

Line 150: This claims that the Rosenberg-Coleman effect is evident in Figure 5. This certainly requires explanation.

My biggest objection to the manuscript is the lack of discussion of a third cause of the semiannual variation, the Russell-McPherron effect. This cause is never mentioned until Line 141. The results of this effect are not mentioned as one of the causes of semiannual variation. V and B alone are not the only drivers of geomagnetic activity; the clock angle of the IMF around the Earth-Sun line is also important. At equinox the GSEE (or approximately GSE) IMF By component will project onto the GSM Z-axis with a positive or negative component dependent on season. Since both toward and away sectors are nearly equally probable the correct orientation will enhance geomagnetic activity near the equinoxes through magnetic reconnection. Only if one removes this effect in the strength of the driver does it become apparent that there is still another effect related to the dipole tilt towards and away from the Sun.

There are a few typo problems that I noticed.

Line 37: "...indices respond (to) the same..."

Line 74: "has" should be "have"

=================== upon resubmission ========================

grlonline@agu.org ✆ 10:36 AM (21 hours ago) June 22, 2011

Thank you for your recent submission to Geophysical Research Letters (Geomagnetic Semiannual Variation Is Not Overestimated and Is Not an Artifact of Systematic Solar Hemispheric Asymmetry, [Paper #2011GL048616]).
However, we are unable to forward your paper to the Editor to start the peer review process until the following problems are corrected:

*******I apologize for the inconvenience; however, your manuscript exceeds the GRL by 1 page. I suggest that you cut text, reduce the number of figures, combine figures, or, if appropriate, consider moving material to the auxiliary option. Please visit: http://www.agu.org/pubs/authors/pub_fees/calculator.shtml to check the length of your manuscript once you've made modifications. This will give you the most accurate length count. Thank you for your cooperation.

*******GRL ask that not more than 4 figures be submitted. There are times that 5 are submitted.
=============

Leif Svalgaard ✆ to grlonline 1:19 PM (18 hours ago)
I have reduced the number of Figures to 5. [actually just combined some Figures] As regards the length, the manuscript was acceptable as far as length was concerned earlier. I have added half a page of new test as a response to a reviewer, and now your length calculator tells me to cut 5 pages of text. Is there a way you can check the actual file?

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Leif Svalgaard ✆ to grlonline 1:48 PM (18 hours ago)
As you have closed access to the manuscript I can't test if my article file is good. I therefore attached it as a TeX file. Driving blind like this is extremely frustrating.

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Leif Svalgaard ✆ to grlonline 2:38 PM (17 hours ago)
And I have gotten it further down. I can't see why it wont fit. In the two column format it takes up only 4 pages. I need help with this.
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Leif Svalgaard ✆ to grlonline 4:51 PM (15 hours ago)
With 4 Figures it all fits comfortably on 5 pages, see attached. I need help on this one. The text is only 2.5 pages. It cannot be cut anymore.

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grlonline ✆ to leif 3:45 AM (4 hours ago)
Dear Dr. Svalgaard:

I see how you made it fit but I cannot go by your 2 column manuscript. We calculate from a manuscript in draft form. You can submit this one in draft and we will check the calculator and see if it fits GRL requirement.

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Leif Svalgaard ✆ to grlonline 6:57 AM (1 hour ago)
I have attached the PDF of the article and the TeX file generating it. The abstract and the Figure captions come from GEMS I presume.
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grlonline ✆ to leif 7:22 AM (35 minutes ago)
Dear Dr. Svalgaard:
You need to ADD the abtract and figure captions. They do not come from GEMS. Once you do that, use the online calculator to determine the number of pages your manuscript will be.

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Leif Svalgaard ✆ to grlonline 7:34 AM (24 minutes ago)
Then why do I need to input them to GEMS? Attached is the PDF and the Tex file with Abstract and Captions included. It comes to 16 pages. The calculator says that I have to cut that to 10 pages. That is removing 6 pages. This is quite impossible. Also the first version of this paper was fine length-wise and I have only added half a page and removed 3 Figures. This does not make sense. What to do?

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grlonline ✆ to leif 7:41 AM (17 minutes ago)
This one looks fine. Submit it and I will move it on.

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Leif Svalgaard ✆ to grlonline 7:47 AM (12 minutes ago)
How do I submit it? You have to give me a link that allows that.

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grlonline ✆ to leif
show details 7:54 AM (5 minutes ago)
Never mind. You are already ok. The manuscript is with the editor to assign reviewers.

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Leif Svalgaard ✆ to grlonline 8:01 AM (0 minutes ago)
Thanks!

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Dear Dr. Svalgaard,

Regarding your recent resubmission, I would like you to provide two separate files; a "Letter to the Editor" and a "Reply to reviewers". Your analysis of the ratings and the request for an other reviewer should be suggested in the "Letter to the Editor".

Editor
Geophysical Research Letters

Leif Svalgaard
Jun 22
Since the double-spaced format [an obsolete thing from the old days where people would write comments and corrections in the blank space between lines] handles Figures and captions very poorly, and since I'm not sure what version AGU actually has because of the changes brought about with my discussions with Beverly, I'm attaching the correct version as a PDF in the double-spaced [unreadable] format and another reviewer-friendly 2-column version. It might be a good idea to open the resubmission link again so I can

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Leif Svalgaard
Perhaps the best thing to do is for AGU to process the TeX file to get something that is compatible with your process. It is attached. As are the Figure files it uses.

Leif Svalgaard
And the files in GEMS don't look too good. 'Failed to load PDF' and such.

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grlonline
If you could have waited until I finished before you checked them. I think they are ok now. If not, I am going to ask the editor that we return it to you to upload properly.

Leif Svalgaard

Sorry that I checked too early. Some comments: manuscript item 2 [the old response] should be deleted. The merged file has Figure captions that overflow out of the page and can't be read. The attached TeX file fixes that [makes the Figures smaller], also my PDF version of the result.

On Wed, Jun 22, 2011 at 10:47 AM
I have re-uploaded the files. The merged file is the most important because that is the one the reviewers see.

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grlonline
I have uploaded that file. Anything else will be on you to do because it should have been correct when you first submitted the paperwork. If you thought that changes were going to be needed, you should have waited until you finished everything.

Leif Svalgaard
Thanks for all your work. I, of course, thought things were OK, but did not foresee the length problem. Anyway, things look fine now.

Grlonline:
YEEPEE!!!!!!!!!!!!!!!!!!!

Letter to Editor: 22 June, 2011

Dear Editor,

With a spread in evaluation from 1A to 4C it is clear that the review process has failed. Either reviewer #2 [1A] displays gross error in judgment or reviewer #1 [4C] has not understood (or is biased) the paper in spite of the A rating from #2. As reviewer #2 has presented a thoughtful and useful review, I respond in specific details to reviewer #2.

Reviewer #1 has given the paper a 4C rating, where the C means that "The manuscript cannot readily be revised into Category A". I therefore confine myself to general remarks [as the review is not specific at all], and in keeping with the C-rating shall not attempt [unjustified] major revision.

I therefore request a third referee for arbitration, unless Reviewer #1 could provide a meaningful and specific review.

Response to Reviewers:
Reviewer two:

This paper is well-written and convincing and deserves publication with only minor modifications. It is important to place the Mursula paper in proper context so that it does not have undue influence.

This manuscript makes assumptions about the reader's knowledge that I suspect are not justified for GRL. The "axial hypothesis" and the "equinoctial hypothesis" are never defined except indirectly through citations. Several sentences of definition would help the general reader.

>>>I have inserted the required definitions as follows:
Several hypotheses have been proposed over the years to account for the semiannual variation. These fall in four broad categories. 1: The `Axial' hypothesis invoking driving forces varying with the heliographic latitude of the Earth; 2: The `Equinoctial hypothesis asserting that activity is modulated by a process sensitive to the angle between the geomagnetic dipole and the direction to the Sun; 3: The `Russell-McPerron' hypothesis leading to enhanced southward heliospheric magnetic field, and hence enhanced reconnection, in April and October; and 4: Lack of `Solar Illumination' of one polar ionosphere at each solstice, reducing geomagnetic activity. The mechanisms fall into two classes: `Excitation', where the controlling parameter directly `produces' geomagnetic activity (hypotheses 1 and 3), and `Modulation', where the parameter plays a role in modulating existing activity (hypotheses 2 and 4). It is likely that several of the mechanisms proposed will be operating at the same time to different degrees. The hypotheses predict very different Universal Time variations of the activity; in particular, there would be no UT-variation for the Axial hypothesis.

Line 47: Does the longitude dependence of Kp eliminate the semiannual variation or simply muddle and smooth it. Of course no maps in UT-season are possible, but why not the seasonal variation?

>>>The semiannual variation is not eliminated in Kp, only most of the UT-variation. I have clarified that as follows: The resulting Kp index should then have no UT variation at all, while still allowing for the semiannual variation. The ap index derived by linearizing Kp is therefore, by design, not suitable for a comprehensive study of the combined semiannual-UT variation of geomagnetic activity.

Again, IHV index is not well known and could use a sentence in addition to the citations. At line 53 it would be useful to explain why the IHV index eliminates the problem present in the range indices (K) discussed above.

>>>I have augmented the text as follows:
IHV measures geomagnetic activity for an interval around local midnight where the contribution (or interference) from the diurnal Sq variation is minimal, as the sum over

the 6-hour interval of the unsigned differences of one hourly average to the next. Calculating the curves using local nighttime `raw' K indices (converted to amplitudes), rather than IHV yields identical results, as expected, because both indices respond the same way to the same physical processes. The `raw' IHV has not been normalized or harmonized in any way, so any variation with longitude has not been purposely removed.

Line 73: Why does the Mursula effect mean no UT variation?

>>>Because if the axial effect were the only one [or the dominant one, as they claim] the solar wind driver is the same [with random fluctuations] at all times during the UT-day, as the heliographic latitude hardly changes in a UT-day.

Line 130: The quantity "A" does not seem to be defined anywhere that I could find.

>>>I have augmented the text as follows:
We consider four subsets of the series: rising and declining phases of the cycle and for each of those, positive and negative polarity (of the northern pole, often denoted as A > 0),

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Line 150: This claims that the Rosenberg-Coleman effect is evident in Figure 5. This certainly requires explanation.

>>>I have augmented the text as follows:
as is also evident from the disagreements between the calculated (blue curve) and observed values (red curve) in Figure 5 near 1977 and 1997

My biggest objection to the manuscript is the lack of discussion of a third cause of the semiannual variation, the Russell-McPherron effect. This cause is never mentioned until Line 141. The results of this effect are not mentioned as one of the causes of semiannual variation. V and B alone are not the only drivers of geomagnetic activity; the clock angle of the IMF around the Earth-Sun line is also important. At equinox the GSEE (or approximately GSE) IMF By component will project onto the GSM Z-axis with a positive or negative component dependent on season. Since both toward and away sectors are nearly equally probable the correct orientation will enhance geomagnetic activity near the equinoxes through magnetic reconnection. Only if one removes this effect in the strength of the driver does it become apparent that there is still another effect related to the dipole tilt towards and away from the Sun.

>>>Although the Russel-McPherron effect on rare occasions is visible in the data (as we point out for 1954 and 1996) the effect is on average very small and has a completely different UT-variation than that which is observed, hence constitutes only a minor, second-order effect. This was pointed out as early as 1974 by Mayaud (JGR, 70, 1131, 1974), 1976 by Berthelier (JGR, 81, 4546, 1976) and 1977 (Svalgaard, http://www.leif.org/research/suipr699.pdf , section 7). The Sun very efficiently removes the R-M effect by simply having approximate equal amounts of away/towards through the year. Only in rare cases (e.g. 1954 and 1996) where at the equinoxes there is a large imbalance between the two polarities (the Rosenberg-Coleman effect being large) can we see the R-M effect in the average semiannual variation. So, the R-M effect is a small, second order effect.

I have augmented the text as follows:
It is not the purpose here to discuss the various mechanisms of the semiannual variation, but to point out that the variation actually observed is in great quantitative agreement with an equinoctial hypothesis and that therefore all other proposed causes with a different UT-variation are not supported by the observations and can only be accommodated as small, second-order effects. As for the direct test of the MTL11 claim described below, which of the semiannual variation hypothesis one adheres to does not enter into the analysis at all.

There are a few typo problems that I noticed.

>>>Fixed

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Reviewer #1:
1. There does not appear to be anything new in it. Instead, it is a restatement of previous work, mostly his own.

>>> there are 36 references of which 6 are to own work. Not an unusual ratio, especially since this has been an active research area of mine for decades. The new part, is the specific test of MTL11's claim using both the aa index and the solar driver BV^2 directly. If someone else has already demonstrated that the specific claim in MTL11 is not supported by a much longer dataset than their 16 years, I would like to know about it.

2. The author makes a dogmatic defense of the equinoctial hypothesis as an explanation for the semiannual variation. But the equinoctial hypothesis is not physical, but is, instead, simply a statement of an observation that magnetic activity seems to be a function of the angle between the dipole and the sun-earth line. There author promotes phenomenological descriptions of the data that he asserts supports the equinoctial hypothesis, but in reality the hypothesis does not have enough quantitative power to support such an exercise. Indeed, the Russell-McPherron hypothesis does not either, and

without detailed understanding of solar-terrestrial interaction, there has been little progress in separating the two hypotheses. Still the role of the axial hypothesis needs to be considered, as Mursula et al have pursued.

>>> I show that the equinoctial hypothesis is the only one that quantitatively describes the observed variation, without taking sides as to mechanisms. I'm only dealing with the observational facts. As for not being physical, there are at least two proposed physical mechanisms (Boller and Stolov, JGR, 75, 6073, 1970; and Nagatsuma, JGR, 111, A09202, 2006; and the semiannual variation of solar luminosity of high-latitude conjugate ionospheres, Lyatskaya et al. AGU, abstract SM51A-05). Furthermore, which hypothesis one subscribes to has no bearing on the strictly observational test of MTL11 in section 5. The rationale for discussing the UT-variation is to point out that whatever the cause is, the UT-variation is an observational fact, not explained by the R-M and axial hypotheses. My paper simply shows that the axial hypothesis as proposed by MTL11 is not operating, neither when considering very many solar cycles or just a single one, or the much too short period of 16 years used by them.

3. Perhaps most importantly, Svalgaard has not looked at the data in the way presented by Mursula et al. This, I think, is crucial. Mursula et al are trying to unravel a mystery. Svalgaard is simply denying their results without actually investigating them. I encourage him to work constructively with other people's results, and to try to respectfully consider their methods. There is much to be learned.

>>> I have made a very specific and strict test of their definite claim, by simply going to the data and asking if their claim is substantiated, and found it is not. There is no mystery to unravel as far as this is concerned. The observations over many solar cycles do not support their specific claim. That conclusion stands on its own, regardless of the possible merits of Mursula et al's previous work. Looking at their specific very short 16-yr interval is inferior to looking at a ten times as long data set. Science progresses by independent analysis of new or different data.

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grlonline@agu.org
Thu, Jul 14, 2011 at 4:24 AM

Dear Dr. Svalgaard:

Thank you for the manuscript submission entitled "Geomagnetic Semiannual Variation Is Not Overestimated and Is Not an Artifact of Systematic Solar Hemispheric Asymmetry" [Paper #2011GL048616] to Geophysical Research Letters. I have now received 1 reviews of your manuscript, which are attached for your reference.

Based on the review comments, the manuscript may be suitable for publication after some revisions.

Editor note: The reviewer comment starting as "Line 93:" is an important comment. Please make sure to account for it in a constructive manner.

The feedback provided in the reviewer assessments of your manuscript is important and should be taken into account as you complete your revision. I encourage you to submit a suitably revised version of your manuscript within 14 days of receipt of this letter. […]

I look forward to receiving your revised manuscript. If you have any questions, please contact the editor's assistant at grlonline@agu.org.

Sincerely,
Editor
Geophysical Research Letters

Reviewer #2 Evaluations:
Science Category: Science Category 1
Presentation Category: Presentation Category A

Reviewer #2 (Comments to Author):

Second Review Svalgaard

The main point of this paper is to invalidate the results of Mursula et al. In my opinion the author has accomplished this objective and his paper should be published. I have my own issues, but they are mostly small ones related to improved communication. The one issue on which I partially support referee 1 is the strength of the assertion that the "equinoctial effect" is dominant over all others. I would like to know why the obvious UT-seasonal modulation by the solar wind driving electric field does not have an effect on the output. I agree that the UT-Season maps shown by the authors indicate the existence of the equinoctial effect. Why is there not a similar effect of ionospheric conductivity on the output?

My recommendation is for the author to make minor changes and for the editor to disregard the comments of the first referee.

Line 31:
Remind the reader that the Sun's rotation axis is tilted away from the Earth in spring placing it at low heliospheric latitude, and the opposite in fall. Otherwise the discussion of phase reversal and its correlation with season is not clear.

line 67.
The abbreviation IHV needs to be spelled out once (InterHourly Variability) so the novice will have some concept of its meaning besides the formal definition now added at this line.

Line 71-73:
In figure 1 there are no labels on either the left or right axes so the units are not evident. The caption and the text describe this plot as raw IHV data. As IHV is defined in the text I expect the units of this index to be nT, however, both families of curves have a mean value of 1.0. This fact suggests that the seasonal variation has been normalized by the long term means and that these are fractional seasonal variations and are dimensionless. However ,line 73 of the text says "The `raw' IHV has not been normalized". What have I missed here?

Line 82:
The Svalgaard function predicts two sinuous ridges of constant height centered on the equinoxes. The upper right panel of Figure 2 for raw IHV shows a pattern close to this function except the two ridges seem to be of different height. The O'Brien and McPherron plot in the upper left shows similar ridges but the contours in spring are peaked near 22 UT and in fall near 10 UT. These are the expected locations of maxima according to the Russell-McPherron (R-M) effect. The combination of the "hourglass pattern" and the imprint of the R-M effect makes it likely that both R-M and equinoctial (Svalgaard) effects are operating simultaneously (see comment on line 49).

Line 93:
The UT-Season map of GSM $Es = VBs$ has a pattern whose shape is very close to that predicted by the R-M hypothesis, although with reduced amplitude compared to their model. The Min, Mean, & Max of VBs are 0.8530, 1.0069, 1.2107 mV/m corresponding to a {plus minus}18% variation about the mean. The projection of this pattern onto season (average over UT) has a second harmonic fit to the seasonal variation with a normalized amplitude of 7.7%.The same projection of the AL index has an amplitude of 14.4%. This suggests that at least half of the semiannual variation in the westward electrojet can be explained by the seasonal modulation of the driver. Based on this argument I find the following statement too strong:
"...but to only point out that the variation actually observed is in close quantitative agreement with an equinoctial hypothesis and that therefore all other proposed causes with a substantially different UT-variation are not supported by the observations and can only be accommodated as small, second-order effects, ..."
If you insist on the truth of the above statement then explain why the modulation of the driver disappears in the output signal. The UT-Season map of AL (or AU) do not look like either the R-M or equinoctial patterns, and this fact is almost certainly an artifact of station distribution in the index calculation.

It seems likely that magnetic indices that depend on both electric field and auroral zone ionospheric conductivity have UT-Season patterns that are amalgams of patterns from all mechanisms.

Line 83:
In the text you should introduce the symbol psi used in the figure for the dipole tilt angle.

Line 85:

Explain "normalized by its average" There must have been multiple averages to get the curves for north and south Bz to lie on top of each other. Why are "odd and even" years treated separately? Is it odd and even solar cycles?

Line 90:
" ...the dominant semiannual-UT variation is a modulation of activity..."
According to figure 4 using southward Bz curves it appears that the psi modulation of the fractional variation in am is about {plus minus}15%. As noted above the fractional variation in VBs caused by the Russell-McPherron effect over a season has amplitude of {plus minus}7.7%. The output index am must be proportional to VBs and so must contain this variation unless removed, e.g. by using the quantity (am/VBs) as the quantity examined as a function of psi. Explain how you make a quantitative estimate of the relative effects of the two processes to justify the word "dominant".

Line 100 and the caption of Figure 5.
It would be helpful to indicate the solar cycle minima in the plot to explain what is meant by "Note the 22-year variation [Cliver et al., 2004] in geomagnetic activity resulting in calculated values that are slightly too high during the rising phase of solar cycles 21 and 23."

Lines 119-133:
This is the essence of this paper with Figure 6 clearly making the point that there is no significant difference in the seasonal variations of BV2 or aa index as a function of rising or declining phase of solar cycle or positive or negative polarity of the solar north polar field. I agree with the author that he has experimentally demonstrated that Mursula and coauthors are incorrect. Figure 7 also makes the same point with individual years.

Line 150:
You need to describe what it is in Figure 5 that makes the following statement true. "enhancement of geomagnetic activity is also confined to the same phase of the cycle as the Rosenberg-Coleman effect), as is also evident in Figure 5."

Response to Reviewer #2

Line 31:
MTL11 suggest a novel variation on the axial mechanism, namely that this purported 22-year variation in solar wind activity, coupled with the variation in heliographic latitude of the Earth during the year (between -7\deg in March and +7\deg in September), gives rise to an annual variation of geomagnetic activity with opposite phases between the declining phases of the cycles

Line 67:

variations at individual stations, using the `raw' {\it IHV} index for each station \citep{sva07}. ==The InterHourly Variation== {\it IHV} measures geomagnetic activity for an interval around local midnight where the contribution

Line 71-73:
The caption to Figure 1 says: "All values are normalized to the mean for the year", so I have changed the text to say:
==Apart from being expressed in units of the mean for the year,== the `raw' {\it IHV} has not been ==further processed== in any way, so any variation with longitude has not been purposely removed.

Line 82:
Yes, various estimates [see e.g. Cliver et al. 2000] allows for 20-30% of R-M effect, enough to distort the average 'hourglass' and for some rare cases [1954 and 1996] even dominate [Svalgaard & Cliver , Annales Geophysicae (2004) 22: 93–100], but the R-M effect remains of second order. The comment in line 49 notes the simultaneous operation of the several mechanisms to different degrees. I emphasize that by now saying:
"It is likely that several of the mechanisms proposed will be operating at the same time to different degrees==, slightly distorting the dominant signal==."

Line 93:
I would say that the GSM Es is just that of Bs as V does not vary in the R-M hypothesis. The variation of Bs when separated by IMF polarity is very substantial [60%] as pointed out in section 7 of http://www.leif.org/research/suipr699.pdf so the R-M effect is very much involved as an excitation mechanism [the most important systematic one]. However, as shown there and by Berthelier [JGR, 81, 4546, 1976] the R-M effect averages out to almost no variation for equal amounts of both polarities. See the full line curves on Figure 5a of Berthelier's paper. Both Am and AE are discussed, with the same conclusion: "We have calculated […] the effect of R-M and we show that it is responsible for a small part of the global annual variation and that it explains the equinoctial maxima observed when days are sorted according to the polarity". This has been clear for several decades now. To summarize: R-M is responsible for most of the excitation [variations of V and B for the rest], but for equal amounts of both IMF polarities, the seasonal/UT variations almost cancel out. The observed variations are due to a modulation depending on the dipole angle of the activity created by the R-M effect. I have included a reference to Berthelier:
"The hypotheses predict very different Universal Time variations of the activity, in particular, there would be no UT-variation for the Axial hypothesis. ==The Russell-McPerron mechanism is very effective in generating geomagnetic activity, but as already shown by \cite{ber76} ans \cite{sva77} produces very little combined semiannual variation for equal amounts of heliospheric magnetic polarities.==

Line 83 and later comments:
I think these are left over from a previous report and have been already answered and incorporated in the text as needed. And have been mooted by removal of some of the Figures.

Line 90: Explain how you make a quantitative estimate of the relative effects of the two processes to justify the word "dominant".
I refer to the paper by Berthelier above.

The following text as well as related references have been removed to conform to the length restriction:
Echer and Svalgaard [2004] showed that the Rosenberg-Coleman effrct is only prominent from minimum through the rising phase of the solar cycle, so the 22-year cycle is confined to such times (the conclusion of Echer and Svalgaard [2004] that the 22-year cycle must have another explanation is probably too simplistic as they did not appreciate that the enhancement of geomagnetic activity is also confined to the same phase of the cycle as the Rosenberg-Coleman effect), as is also evident from the disagreements between the calculated (blue curve) and observed values (red curve) in Figure xx near 1977 and 1997.

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grlonline@agu.org
Mon, Jul 18, 2011 at 9:13 AM

Dear Dr. Svalgaard:

I am pleased to accept "Geomagnetic Semiannual Variation Is Not Overestimated and Is Not an Artifact of Systematic Solar Hemispheric Asymmetry" (2011GL048616R) for publication in Geophysical Research Letters.

Please note that after a paper is accepted, no changes of a technical or substantive nature should be made without obtaining the Editor's approval.

The final acceptance date is the date on which final files are processed at AGU. If you have not yet provided your final publication-ready files, the Editor's Assistant will be in contact with you shortly to request them. Additionally, you will be provided an opportunity to preview your sized images prior to production.

Having gone through the review process, you are aware of the importance of the many scientist-volunteers who provide the independent evaluations of submitted manuscripts. We request that you make every effort to support GRL by agreeing to provide a review when called upon.

Editor
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