X-Sender: earscott@apollo.ssl.berkeley.edu
Date: Mon, 30 Oct 2000 15:16:21 -0800
To: mcollier@pop600.gsfc.nasa.gov
From: jgr@ssl.berkeley.edu
Subject: JGR MS #20.0382: major revision requested

10/30/00 (a hard copy of the following is in the mail)

Dr. Michael R. Collier
NASA Goddard SFC
Code 692
Bldg 2, Rm S103
Greenbelt, MD 20771

Manuscript Number: 20.0382
Manuscript Title: Observations of Neutral Atoms from the Solar Wind

Dear Dr. Collier,

Enclosed are comments on the above paper that you submitted to our editorial office. While both referees are enthusiastic about the general content, they agree that the presentation requires considerable improvement. Carefully consider the referees' reviews, and submit four double-spaced (including references and figure captions) copies of the revised paper before 01/08/01, together with a detailed response. If you need an extension, contact the editorial office and it will almost certainly be granted. If, however, I do not hear from you by two months after the deadline, I will consider your paper withdrawn. Let me know if you decide against revising the paper.

Prior to publication of any paper, AGU requires that you submit a copyright transfer agreement and a publication options form. Please return the enclosed forms and the author's checklist to JGR editorial office with your revision. You will also find included instructions for submitting an electronic abstract-the GAP abstract. The best time to submit the GAP abstract and the request for index terms is after an accepted paper has been copyedited.

Thank you for choosing the Journal of Geophysical Research-Space Physics.

Sincerely yours,

Janet G. Luhmann
Senior Editor

Enclosures:
Referee Reports
Reviewer Form
Author's Checklist
AGU Forms
==========
Referee #1
Manuscript No.: 20.0382
Manuscript title: Observations of Neutral Atoms from the Solar Wind

1) Yes. This paper reports on possibly the first detection of H atoms of energy ~1 keV at the edge of the magnetopause during the arrival of a CME. If true, two important facts should be established:
a) The detection technique is now proven viable for the imaging of space ion populations containing H atoms of energies around 1 keV; and
b) There is a flux of neutral atoms, about 10-4 of the solar wind flux, in the magnetosphere.

Both facts would impact space physics in opening a new window for observation and a new topic for investigation in the role of low energy

neutral atoms in space plasma.

2) One of the prime purposes of observation is to check earlier speculations against most recent observations. This paper missed the opportunity to carry out this task in two areas in view of the observations reported in this paper:
a) Implications of Akasofu (1964) and Brant and Hunten (1966) on the effect of solar wind neutrals on geophysical phenomena are missing. The latter reference, which is missing, is given below.

Brandt, J. C. and D. M. Hunten, "On ejection of neutral hydrogen from the sun and the terrestrial consequences" Planetary and Space Science, 14, 95-105, 1966.

b) Implications of Gruntman (1994) and Hsieh et al. (1992) on using neutral atoms generated in CME for early warning against geomagnetic storms. The latter reference, which is missing, is given below.

K. C. Hsieh, K. L. Shih, D. J. McComas, S. T. Wu, and C. C. Wu, "Forecasting the arrival of fast coronal mass ejecta at Earth by the detection of 2-20 keV neutral atoms," in Instrumentation for Magnetospheric Imagery, Proc. SPIE International Symposium on Optical Applied Science and Engineering, San Diego, 19-24 July, 1992, Vol. 1744, 72-78 (1992).

3) No. The paper seems to be written in great haste. The paper appears to be written while still thinking through the arguments. Aside from some minor revisions - to be listed separately - two major arguments need careful revision:
a) The ratio of H counts to O counts ([H]/[O]): If locally produced O is directly linked to incident H of higher energies (~1 keV), as indicated by Figure 4, and if this fact is used as argument for the presence of ~1 keV H at the arrival of a CME, as shown in Figures 5 and 6, two questions must be answered.
i) In the lower panel of Figure 4, the ratio of H peak to O peak is about 50 to 1, while that in Figure 5 is about 6 to 1. If we take the total counts (area under the curves) into account the discrepancy is even greater. A more quantitative explanation is needed.
ii) Going from the absence of O counts to the presence of O counts, i.e. an increase in O counts, should not the ratio [H]/[O] decrease from almost infinity to a smaller finite number? Why is the ratio increasing in Figure 6?
b) The arrival direction of the observed H flux: The text, accompanied by Figure 1 through Figure 6, gives the impression that the H flux arrives from the sun-ward direction within a 90° FOV centered about 180° in the spin plane. Figure 7, however, shows a 90° FOV that does not even include the sunward direction. Some clarification seems needed. This may affect the presentation and conclusion of the paper.
It should be noted that a discussion on the rise in the neutral flux at 09:11 UT in spin angles out side of the "sun-pulse" is needed in order to make the paper complete.

4) Please see answer to Question 3).

5) Yes, if the paper is revised according to the comments and recommendations made above.

6) Aside from the above comments and recommendations, here is a list of minor changes.
a) Introduction, 2nd paragraph:
1st sentence: Replace "neutral particles" by "neutral atoms". The former includes neutrons and neutrinos, which are not of concern to this paper.
2nd sentence: Rewrite this sentence. "∑neutral atoms, originate from∑" gives the impression that the LISM neutral atoms originate from dust grains.
Last sentence: Give reference to charge exchange in the atmosphere of Venus; e.g.,
b) Observations:

1st paragraph: Should the space mission Wind be represented by WIND () as SOHO and ACE are presented throughout the paper? Please cite references from which the reader could find the observation of the same CME by ACE, SOHO and WIND.
2nd paragraph: Please rearrange the sentences in order to make the identification of the "bright streak" in Figure 1 in a more coherent manner.
3rd paragraph: Please rephrase the parenthetical remark. Natural activities cannot be associated with detector signals, but detector signals can or cannot be associated with natural activities.
5th paragraph: The description would be more lucid, if the ACE/EPAM counting rate as a function of time is also shown in Figure 3. By the way, the "data" does not "peak". It is the counting rate, contained in the data, that peaks.
6th paragraph: Please make it clear that the distance, which is being compared to the ~40 RE scale length, is the distance between WIND and IMAGE, which is only a few RE from Earth at the time. IMAGE is a relatively new mission. Many readers may not be familiar with its orbit around Earth.
7th paragraph: Please read the comments on [H]/[O] ratio under Questions 3). The dependence of the [H]/[O] ratio on the incident energy of H is a crucial piece of information lacking. The discrepancy in the ratio mentioned under Question 3) makes the identification of ~1 keV as the incident energy of H very uncertain. Picking ~1 keV for the possible energy needs a bit more justification.
8th paragraph: The peak to the left of the H peak in Figure 5 needs a little more discussion. The authors should suggest some possible causes of this "artifact". In addition, please unify the name referring to the "bright streak", "sun pulse", "sun signal", and "enhancement in the solar direction", etc.

9th and 10th paragraphs: Please read the comments on [H]/[O] ratio in the response to Question 3).

c) Discussion:
2nd paragraph: The reference to the observation prior to 26 may 2000 needs more clarification.
3rd paragraph: Please see the response to Question 3, concerning the arrival direction of the signals and the FOV shown in Figure 7.
4th paragraph: How would the geocoronal H density based on Wallace et al. (1970) compare to that from the later model of Rairden et al. (1986)?

d) Conclusion: This section has to be rewritten after the above-recommended revision.

Summary Evaluation: The paper should be revised.

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Referee #2 Review of "Observations of Neutral Atoms from the Solar Wind"
Author: Collier et al.
Manuscript Number: 20.0382

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In your opinion, does this paper describe interesting and substantial new results? If yes, briefly describe their nature and potential impact.
Yes. The paper describes the probable detection, for the first time, of the neutral particles in the solar wind.

****

In your opinion, does this paper adequately put the progress it reports in the context of previous work? (This includes both representative referencing as well as introductory discussion.)
Improvements are needed. See the detailed comments

****

Is the paper clearly and concisely written? (Note it is not necessary to include every detail to be "clear".)
No. The presentation requires serious improvements.
See detailed comments

****
Are the conclusions and potential impacts of the paper made clear? If no, how can the author make them clearer?
Yes.
****
Will readers outside of the specialty of this paper be able to appreciate at least the motivations and general conclusions of the reported work?
(Almost) yes. Strengthening of the discovery aspect of the results is advisable.
****
Is the paper up to JGR standards in terms of:
Writing style: No
Graphics: Yes
Professionalism: Yes
Apparent Accuracy: No
Explanations:
see detailed comments
****
Summary Evaluation
Reviewer recommendation:
Recommend revising
****

Additional comments:
"Observations of Neutral Atoms from the Solar Wind" by M.R. Collier et al.
MS # 20.0382

The manuscript describes interesting results, a possible detection of the neutrals in the solar wind. The presentation requires significant improvement. There are three major flaws in the manuscript. First, the authors identify the higher-energy neutrals by considering the detailed characteristics of the instrument output. This may be understood only by the readers intimately familiar with the LENA instrument design and performance. A description of what exactly the instrument measures and how these count rates relate to the incoming ENA fluxes is needed. The major instrument performance characteristics, including the field of view (see next paragraph), could be presented in a tabular form.

Second, the observation geometry (fig.7, etc.) should be presented before the measurement results are shown and discussed. The observation geometry does not seem to support the identification of the observed signal, as presented in the article.

Third, the neutral component should also be present in the quiet solar wind. The article focuses on the essentially disturbed conditions. Are there any signatures in the quiet solar wind? Would such signatures be below the instrument sensitivity?

Other specific comments:

p.2
The references to two major review papers on magnetospheric ENAs (Williams et al., Rev. Geophys., 30,183, 1992; Gruntman, Rev. Sci. Instrum., 68, 3617, 1997) will help the readers to place the presented work in the context.

The reference to Akasofu, 1964 is somewhat misleading because it was soon shown by Brandt, Hunten, and Cloutier that the Akasofu's mechanism would probably be insignificant. Fahr (Astrophys. Space Sci., 2, 496, 1968) and Holzer, 1977 should be credited for clear identification of the processes leading to the neutral component in the solar wind. In addition, Dessler et al. (JGR, 66, 3631, 1963) were the first to invoke solar wind charge exchange on the exospheric neutrals. The solar wind ENA story is described by Gruntman, 1997.
"Σ by the fact that the geocoronal density at the magnetopause is comparable to that of the solar wind Σ"

Ambiguous statement. The authors probably mean the importance of charge exchange on the exospheric neutrals. This effect requires further discussion.

p.3
The statement on the possible importance of the atmosphere of Venus should be supported by (simple) numerical estimates.

p.3
The count rate dependences should be preceded by a description of what exactly the instrument measures and how these count rates relate to the incoming ENA flux. Such a discussion is especially important for the reliable identification of the signal origin.

p.4 The authors convincingly demonstrate that the solar EUV radiation is not responsible for the observed counts. There is another potentially important and much brighter source of photons that may be capable of triggering the detectors, namely the solar Lyman-alpha line.

p. 6
The time-of-flight spectrum would be understood only by the readers intimately familiar with the LENA instrument design and performance. Explanations are needed.

p.6 (last paragraph)
The narrowing of the energy range to > 1 keV is not justified in the text. The calibration, as described, provides only the threshold of 0.3 keV.

p..8
A detailed description of the instrument field of view and its angular sensitivity is needed because of the (unfavorable, fig.7) observation geometry.

p.8.
How do the Wallace's results compare to those of Rairden et al. on DE-1?