Howell - Review of Don Scott's model for Birkeland current structures

www.BillHowell.ca 12Dec2014 initial draft

Summary :

Don Scott has developed a very creative and solid model of Birkeland current structure, yielding surprising results with extremely important potential relevance to a wide number of areas including [astronomy, Earth sciences, biological and evolutionary processes, human health (pandemics, accidents), history and war, engineering, alternative energy systems, etc].

- *Donald Scott 141114 "Magnetic Structure of Force-Free Currents" 14Nov2014* *http://electric-cosmos.org/FFCPaper.pdf*
- *Donald Scott 140320 "A New Model of Magnetic Structure in Space | EU2014" posted 09Sep2014 on* *https://www.thunderbolts.info/wp/2014/09/09/donald-scott-a-new-model-of-magnetic-structure-in-space-eu2014/*

I consider Donald Scott to be among the top [creative, conceptual, observational] solar/astronomy scientists, notwithstanding his amateur status! His ability to see the flaws of the "mainstream, overwhelming consensus, scientific thinking" is matched by only the rare professionals (to 10^-4 degree), and his ability to conceive of more powerful and creative new approaches is far rarer still.

On the basis of "a first pass through reading, but complete and in detail", with limited step-by-step re-derivations to check the mathematical work (p7), and relying on my very-rusty memory of Bessel functions and the Lorentz force law, this paper is serious, well thought out, and imaginative. The derivations that I have checked are competent, even though at present I have a few non-critical reservations about a few steps and assumptions.

I cannot say for sure at this stage of my review and work whether the model is correct, nor, given their ongoing and recurring failures, would I fully trust an expert (eg university professor in plasma physics). Again, I can say that the "step-by-step mechanics" of the model derivations are quite solid. But to me, that is already irrelevant - Scott's theory could hardly be as bad as a long precession of the "mainstream, overwhelming consensus scientific opinions" right across many, if not all, areas of science. At present, what counts are the questions - as a good question is worth a thousand good answers.

Furthermore, by trying to force myself into an attitude and approach of "multiple conflicting hypothesis", I automatically retain current and past scientific consenci, plus a "manageable sampling" of other diverse concepts. All the better to avoid becoming a disciple of a science [fashion-cum-cult-cum-religion], and being able to see and listen to the data.

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Introduction - Paper Review form

In this document, I've used my “standard” paper review form for doing peer reviews for scientific journals (these always vary a bit according to each journal's requirements). This is overkill, but it's comfortable for me to go through the usual sections to ensure that if I am skipping anything, it is not because I am forgetting it.

As always, I will have made errors throughout my review. My apologies to the innocent, if any.

NOT included in this special, unsolicited review

“Standard” review sections that are NOT included below are :

- ACTIONS REQUESTED OF THE AUTHORS - obviously this ISN'T a peer review for a journal, and there isn't a need for any changes by Don Scott!
- COMMENTS ONLY - actions by the authors are NOT required for the points listed below, to the end of the review. Perhaps some of these comments will be helpful in some way. - This comment traditionally introduces the rest of the review, keeping only one to 5 points if any as critical, recommended changes for the paper. Clearly not needed in this review.
- PERTINENCE of the paper for the <journal-name here> journal - normal focus and emphasis of journals (and conferences etc) isn't relevant here.
- C0. SPECIAL SECTION: COMMENTS FROM AN EXPERT IN THE AREA (if applicable to this review) - I just want to get this done, and while help from experts would certainly be welcome, this review is a great starting point for doing that (if ever...).
- C5a) Are references and citations in the standard format? - While Scott's paper was not for a journal, it is worthwhile to check references, to make sure that adequate information is provided so that readers can find the source material.
- CONFIDENTIAL COMMENTS for review chair / committee use only - clearly not applicable.

I have left in many comments here and there that are not relevant to the current review and situation, but they won't hurt anything either, and at least they leave some idea of the flavor of my “standard” peer reviews for journals.

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RATINGS

Comments on my paper ratings :

Howell 06Dec2014 - It is appropriate to explain my ratings below, so that they are not too grossly mis-interpreted. Overall, this is a very strong “conceptual” research paper compared to many that one sees in science and engineering. However, the ratings do have to some extent a “relative” basis, and are in comparison to ~4 journal papers per year, and 5 to 15 conference papers per year that I review, primarily in the area of Computational Intelligence (neural networks mostly). I am very lucky that in the last two to three years, almost of the papers that I review are of extremely high quality, far superior to essentially all that I ran into from the research groups and projects that I worked with over my career, and many of the reviews I had been doing prior to that. From that perspective, this paper is actually ranked very favourably, even though the numbers may at first sight appear to be low!! Furthermore, “Soundness of Conclusions” isn't a huge issue for the initial stages of very creative papers, although it can be a barrier for journal publication of breakthrough work.

My guess is that this review is about average length for my journal paper reviews, and almost twice the average length of my conference paper reviews. I would not be taking the time to review in this detail if Scott's paper didn't hold a special interest for me. In effect, I replaced a journal peer review request that I normally would have done, and for which I can only do so many in a year, in order to review this paper in detail.

To begin with, I am used to seeing VERY advanced concepts in the Computational Intelligence field (mostly Neural Networks, but I do like to follow Evolutionary Computation (including Particle Swarms etc)), that establish novel and creative mathematical theorems (lemmas, theorems, corollaries), including analysis of dynamical stability and control (NO - I am NOT an expert!!). Sometimes this includes NOVEL mathematics, or novel “treasure hunting” of sometimes distant past theorems combined with extensions to move forward.

But many Computational Intelligence methods are developing approaches to learn autonomously from the data, and one of the great strengths of these techniques is that they can sometimes work where the best of theories are inadequate, or where it is not possible to derive algorithmic solutions. They can therefore be tried to tackle challenges where traditional [phenomenological, algorithmic] approaches, and the associated [rational, logical, scientific] thinking fail, and are mostly, if not entirely, inadequate.

Don Scott's paper scores high for “seeing through the fog” (right or wrong), and for carrying it through with a strong, albeit traditional phenomenological basis, without the need for any mathematical theorem development. It's important to note that observation trumps math in the end (as per Vladimir Vapnik's statement circa ?2003? “... ?engineers and scientists, at some point all theories fail and you must model with the data? ...”).

Howell - Scott's Paper Rating compared to my "overall impression of other areas of science" :
10 Superior, 7 Above avg, 4 Average, 1 Poor, -1 Yuck, -4 Sucks, -7, Hugely net negative, -10
n/a Not applicable

Traditional criteria :	**Climate sciences**	**Environ - mental**	**Astro-physics**	**Scott**	**Advanced neural nets**
Quality of Methodology	1	3	6	6	9
Quality of Work	-3	-2	3	8	8
Soundness of Conclusions	-9	-5	-2	6	6
Significance of Subject	9	3	5	7	n/a
Clarity	4	4	5	4	7
Organization	4	4	6	7	7

Special criteria for this review :	**Climate sciences**	**Environ mental**	**Astro-physics**	**Scott**	**Advanced neural nets**
Resistance to political-correct	-10	-8	-2	8	2
Integrity of data	-5	-3	3	8	n/a
Integrity of analysis	-9	-5	-2	8	5
Experimental observation	-1	-3	1	7	1
Algorithm development	-8	-5	7	2	9
Math developments	n/a	n/a	n/a	n/a	9
Data-driven models	-7	-2	-7	7	10

Is the abstract, and are the figures, legends, and references acceptable? If not please explain:
>> Absolutely. The concepts are well illustrated and clarified. Don Scott's EU2014 presentation of 20-24Mar2014 also helps.

Please provide a brief and compelling argument supporting (a) your recommendations and (b) the above ratings:
>> See my comments to the authors.

Side comment on Science, Reviews, and Amateurs
Standards for conferences are typically lower than for journals - and for very good reason. Journals are often an impediment to getting extremely important ideas out, ideas that violate mainstream concepts, theories, experience etc. Conferences are a traditional mechanism to allow "crazy ideas" to be expressed and considered as they develop. In the last 20 years, the internet has allowed special interest communities to do a far superior job than traditional science at presenting and reviewing revolutionary ideas, to the point that the historical role of "scientists as rich amateurs" has returned - "rich" being an absolute rather than relative term, to describe the ability of anyone, for at least short periods of time, to focus major time and a keen interest on a theme, without starving to death. Retired people (me included, even with only a partial pension income).

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This reviewer's personal approach, nomenclature examples:
p1c1h0.8 = means page 1, column 1 80% of the way down the page (very approximately)
C2. = means Comment section #2 WEAKNESSES (note that actions by the authors are NOT required for the points)

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C1. STRENGTHS OF THE PAPER:

[Simple, elegant, inspiring] result
What really attracted me to this paper was the beautiful, thought-provoking result (irrespective of its veracity), given the lack of [comparable, reusable] work that I was aware of. Of course, it helps that I've been planning to do "Birkeland current end effects" analysis since ?2012?, and this was a very nice lead in!

Simple phenomenological basis
Scott's adoption of a "force-free, field aligned plasma" basis is a sensible approach that greatly simplifies the model and mathematics, which providing an intuitive benchmark model that can probably be adapted and perturbated for modeling modest dynamical effects such as [stability, phase and state transitions, turbulence and chaos, time series signatures (going beyond Fourier and wavelet spectra), control for alternative energy, etc].

Bessel functions
p4c2h0.8 Section "IV. Solution in closed form"
p12c1h0.85 Acknowledgement of Jeremy Dunning-Davis' recognition of Bessel formulation.
This was a pleasure to see for me, as it has been a while since I've run across Bessel functions!

Limited number of pre-conditions introduced at the start of the analysis
This is best illustrated by Scott's comment :

- p2c1h0.45 , p7c2h0.5 *"... Nor are any assumptions made about the distribution of the current density across the cross-section. ..."*
- ... oops, ran out of time to list other good examples...

Checks on modeling and assumptions :

- p5c2h0.45 Section "V. Euler method of solution" - Euler iterations with Runge-Kutta 4th order numerical integration to check on functional results of model
- ?Back-check on the veracity of the Peratt simplification using an alpha proportionality of the magnetic field as an approximation of the curl of the magnetic field?
- p9c1h0.8 through p10c1h0.25 Equation (57) - Double-check on solutions [(14), (22), (25)] of Equation (10) by inserting those back into (10)

Data-direct relation
Modern astronomy and physics claim a data basis, but often violate that to an extreme. Scott has made a superior effort to tie his model to observations, and avoid the "disease of theories and mathematical models".
p10c1h0.5 "... Fig. 7. North pole region of Saturn Infrared image.[15] ..."
p10c2h0.4 "... Fig. 8. Penumbra of a dense plasma focus discharge from a current of 174,000 A.

Credit A. L. Peratt ..."
Great illustrations from astronomy and the physics lab! This makes it very easy to relate to the author's results. Note that both figures lead one into dynamics, rather than a non-changing, "low-stress" ideal state (force-free, field-aligned).

Plasmas of partial ionisation
p1c2h0.85 "... Space plasmas consist of ionized and un-ionized gas, and dust. ..."
p10c2h1.0 "... It is well known that currents in plasma drag un-ionized (as well as ionized) matter along in their path . ..."
p11c1h0.75 "... Matter that is even partially ionized can become radially stratified in a Birkeland current. ..."
The author brings out the important point that only partial ionization is required to get plasma behaviour (as per Peratt's textbook - down to only 1% can be sufficient!). It was important to emphasize this by repeating the point, as the author has done.

Primary and induced fields (E)
p2c1h0.9 "... Explicitly, the current density vector, j, at that point creates a curl(B) vector, not the local B vector itself. ..."
This is an important emphasis and clarification.

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C2. WEAKNESSES:

(again, changes to the paper are not require for these comments)

Weaknesses that are less than strengths

Coverage of other "Birkeland current structure" models
I think that the biggest weakness of the paper is that it doesn't show the results of other Birkeland current models for a range of conditions, including the "force-free, field-aligned" situation addressed in this paper. Brief mention, but no model [assumptions, conditions, equations], are shown for Alfen's model p9c2h0.15 Figure 6, but no other efforts are described, and it's hard for me to imagine that there aren't a lot of them out there. (Yeah, I was too lazy to check...)

Reverse-direction currents & physical reality
Scott's model implies that there are current direction rotations, and even reversals, as a function of radial distance r. I have NO idea of whether that can actually happen, but my gfirst gut feel is "NO". This physical implication was not mentioned in the paper, nor was the issue of whether or not this is physically realistic either within the Birkeland current, and more important at the ends. This is NOT an issue if one STOPS at the first zero of J_0(αr), which in that case would also define R, the maximum diameter of the Birkeland current.

no estimate of R
(estimate of maximum plasma radius for Birkeland current) - This is important, as one has no idea of whether the sqrt(r) rule is applicable in even simple situations - for example with "Earth-Sun connections" as one approaches the poles, or at what point "Jupiter"sun connections" might influence Earth. Data of course can be referred to for this, but an initial theoretical guess would also be handy.

r^-0.5 force law
p5c2h0.1 applies as long as this is WITHIN the Birkeland current (r <= R) as stated by author p11c2h0.8 *"... For large radial distances, r, the amplitudes of those helical fields decay slowly in inverse proportion to the square root of r as far as the extent of the plasma r < R. ..."*. However, as there is no attempt to estimate R (maximum effective plasma radius), the readers are left with little guide them.

Aurora borealis (australis)
Scott does make allusions to auroral situations (p10c1h0.5 Figure 10, p11c1h0.15 Figure 9 - focus on dichotron instabilities), but it seems to me that there would be very good data from Earth's "boreali" which would be essential for commenting on the detailed structure of the endpoints of Birkeland currents as well as structure away from the Earth. Likewise, physics experiments are alluded to (p10c2h0.45 Figure 8), but what do lab measurements say of the [electric, magnetic] * [fields, currents, processes]? What models are being used, and how far are they away from observations?

Weaknesses that are strengths
As usual, many of my points below aren't really weaknesses of Scott's model, but are more issues and challenges that arise from its initial "success". It would have been counter-productive to cram the paper with too many distractions, and it would be a horrible waste to delay of hinder "publication" of results because one wants to address every issue!

"Roping helices"
- are not predicted by Scott's model, but are seen with many "glowing mode" Birkeland currents (or at least, in pictures that are often shown). Instead, the model depends on uniform current densities at radius r, so the "helical nature" of the currents and fields is in the form of uniform cylindrical shells. Perhaps "on average" this gives good results, but I suspect that the "rope-like" structures must be considered for some problems, plus it's nice to have models that predict that form. (see comment below on "Lucas' versus Maxwell's equations for electrodynamics")

Only a "steady-state, idealistic force-free, field-aligned" condition is modelled.
While this is an excellent base or benchmark, many of the practical events of interest are transitory, oscillatory, or result from much higher energy levels including :

- presumably Birkeland oscillations and rotations are not only common, but key to the induced behaviour and time series signatures of affected systems (such as Earth sciences : geology, biology, climate, etc, etc)
- disruptive or varying polar currents of planets and moons (these are sued by Scott as examples)
- dichotron instabilities and catastrophic discharges from approaching bodies at high potential differences),
- they may be the result of solar or galactic fluctuations of great magnitude (for example, having the solar system pass in and out of the spiral arms of the Milky way over say 70 to 140 My, or above and below the plane of the Milky way at ~15 to 40 My?).
- How realistic is the assumption that the partial derivative of electric fields can be ignored for many of the Birkeland current effects of interest? (see p2c1h0.6 Equation (2))

Solar processes and events
As a very important, special class of situations, Scott's model does not address the many solar processes and their effects on sun-planet, sun-galactic interactions. These include items below (and many others). Having said that, a natural next step would be for Donald Sott to combine the current

Birkeland current structure model with his own npn transitor model of the sun!

- coronal mass ejections (solar proton events), solar prominences
- Donald Scott's descriptions of effects explained by the npn transistor model of the sun
- First-principles approach to modelling and estimating the range of solar “pseudo-cycles” - ranging from David Thompson's 10 million mode of vibration of the sun below 1 second, plus all of the long-defined (including fuzzy) Solar and Earth

[Phase, state] transitions
Even more important than the “known and established psuedo-cycles” (?), is the modelling and prediction of , which routinely make garbage out of the best of “steady [phase, state] models” - and so famously and perenially for the sun. The reason that I am saying that transitions are “more important” than the “steady [phase, state] models” is that the latter have received a huge amount of attention, and the former are conspicuous for their near-complete ridicule of all scientists (de Jager and Duhau's “poloidal-toroidal” model for sunspot activity is a refreshing exception!).

Particle-in-cell calculations
- seem to be a REQUIREMENT for the modelling of more complex plasma processes (or at least, this is my impression from Peratt's plasma astronomy textbook). The field concept still exists in a sense, but the simple, pretty geometries that permit pretty algorithmic solutions simply don't apply to the fundamental modelling of complex situations for solid ! Having said that, simple field assumptions may be a great starting pointy for more powerful modelling, and can be double-checked after the fact. This is like the alpha-magnetic field proportionality in the current paper.

Z-pinch situations
- are mentioned by Scott in this paper (p12c1h0.6 Figure 10 is a beautiful example), but they are not specifically modelled. If I remember correctly, his npn transitor model of the sun does that, but it is not modelled here either (different situation, but a powerful framework to evoke questions and research!).

Lucas' versus Maxwell's equations for electrodynamics
According to Bill Lucas, Maxwell's original 20-or-so equations as well as Heaviside's 4 vector formulation of these, are [incomplete, inconsistent, based on incorrect approximations and assumptions]. Scott's analysis of giant astronomical Birkeland currents is missing a triple-cross-product “chiral” term that Lucas derived for his “Universal Force” (which accounts for the strong & weak forces and gravity as electromagnetic forces, thereby annihilating any need for them). This may be a critically-missing component to Scott's model.

“Fluid mechanics”
(Navier-Stokes, magnetohydrodynamics, equation as of motion of a general sort & beyond) are missing entirely. Peratt's textbook gives conditions where this is reasonable, and I think it's a good omission for the ideal situation analysed in this paper (at this stage). However, for “end effects” like interactions at polar regions, and for “Electric discharge machining” of craters and canyons, obviously a “fluid mechanics” component would be needed (plus [rock, liquid,etc] electro-mechanics...).

Other
p3c2h0.45 “... The purpose of introducing the parameter α is for none other than to ...”
This is a strange paragraph, given that alpha is persistent in the models, and has the important

implication of proportionality, which is non-trivial. I'm not entirely comfortable with this, but at least the author later back-checks results through independent numerical modelling.

p10c1h0.4 "... Whether or not the parameter α "varies with position" (or for any other reason) is a false concern ..."
Incorrectly stated, because if alpha WAS a function of [position, state, dynamics] this could have profound effects on the modelling and predictions! Perhaps a better way to state it would be that the approximation in equation (11) is a convenient simplification that makes it possible to develop a simple, clear model, and that it is a good approximation in the context of the current paper.

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C3. QUESTIONS:

(no need to answer)

Bill Lucas - Universal force chiral term
p2c1h0.2 paragraph beginning "... The driving source of the total current, I, within the plasma ..."
I've seen the comment (from Bill Lucas or perhaps another?) that there is a difference between primary E fields and secondary fields induced by B (or perhaps that was visa-versa?). Is that an issue, and how might that affect modelling in this paper?

Birkeland current end effects
What are Scott's thoughts on "end effects" of Birkeland currents? This would include the cartesian product of :

[torsion, tension, shear, flow]
[electrical, magnetic, fluid-gas, mechanical, chemical, etc]
[material properties, phenomena, processes, interactions, non-linearities]

This is an immediate interest of mine.

Dimensional analysis
While the author does bring up the important issue and context of dimensions (as per p2c2h0.35), and he "sort of uses" results in the form of dimensionless numbers (p6c1h0.0 Figure 2, p8c1h0.1 Figure 3, p8c2h0.3 Table I & Figure 4), it is strange and curious to see the author's sensitivity regarding the factor α, and in the end the author stops short of the broader context of dimensional analysis that could be very helpful in giving a feel for transitions from the "force-free, field-aligned" state as analysed in the paper, to more general and dynamic situations. While "dimensional analysis" has fallen out of favour with modern numerical techniques, the latter suffer from poor "conceptual understanding", in that important, and relatively simple relationships and trends of systems are hidden in the details. I personally find the dimensional analysis approach to be a great complement to numerical and phenomenological methods, both as a check on models, and often as yielding superior (human understandable) insight into the behaviour of systems.

Furthermore, if I remember correctly, Peratt makes extensive use of dimensional analysis in his plasma textbook, to help define and understand various [conditions, limits, transitions].

Other
p3c2h0.25 change to "... This leads to an expression of complexity and little usefulness for the simple situation addressed in this paper. ..."
It's best to qualify a statement that could get you into deep do-do...

p4c2h0.4 “... Following the Principle of Parsimony, no boundary condition at any non-zero value of r is introduced. ...”
One often sees mention of “Occam's razor”, but not so often the “Principle of parsimony”. Interesting way to put it.

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C4. DETAILS and GRAMMAR:

(again, changes to the paper are not required for these suggestions)

The paper is VERY well written and is easily understood, so I have few suggestions to make. These tend to be stylistic, and may not be better than the authors' original version, so they should only be considered if to the authors' liking. Quite frankly, the number of changes I have suggested below is far fewer than normal. This may indicate not only the author's writing talent, but insufficient attention on my part. Grammar is boring, but these simple checks have always helped to force my attention to the details, allows me to see things more clearly, and brings out questions. Maybe that's a bit like the contrast between writing with a pen as compared to typing on a keyboard, or at the extreme, speaking into voice dictation software that enters text into a word processor. The process of writing seems to affect to some degree the style of thinking?

p3c1h0.75 change to “... B_curl_??? = $(\nabla \times B) \times B = 0$ ...”
You allude to this equation at several points throughout the paper, and it is easy to forget it's significance and units.

p4c1h0.2 change to “... It follows from the absence of any externally applied forces (other than possibly a static axial electric field to maintain I) and any time-varying electric fields, that all partial derivatives of B w.r.t. θ and z are zero. Therefore, what remains of (11) after these simplifications in (12) are the following three equations: ...”
It seems to me that the phrase “... the absence of any externally applied forces ...” does NOT ensure “... that all partial derivatives of B w.r.t. θ and z are zero ...”. You would have to PROVE that initial minor mis-alignments wouldn't grow in a stable manner, and that hasn't been done in the paper.
... Rather, it is the lack of external forces PLUS the unproven ASSUMPTION of cylindrical symmetry and uniformity along the Birkeland current from one end to the other that ensures this. Otherwise, even minute initial misalignments could cause dispersion (widening) of the current over it's length, and there has been no confirmation that net magnetic fields aren't present to drive the dispersion. Actually, a better explanation later arises in the paper regarding self-induced current “guiding” within cylinders that provides a better explanation of the constant geometry and current density in the z direction! For now, this assumption can be made, then verified later. Also related is the problem of dertermining R, the maximum radius of the plasma Birkeland current.

p5c1h0.8 “... The Bessel functions can also be expressed in integral form as follows^7: ...”
What does the superscript '7' signify? You don't have footnotes in the paper, and reference [7] doesn't seem to fit.

p5c1h0.85 Equation (26) change to “... $\cos(x \sin \varphi)d\varphi$ ...”
This is strange - when I copy& past, the right symbol φ appears, but what I see on the page and in my pdf viewer is ϕ ?

p7c1h0.15 Equation (37) change to "... By definition the curl(B) in cylindrical co-ordinates is: (repeating Equation (12)) ..."
To me, this part of the derivation is confusing (repetition of (12) but one looks for some difference that is not there!), and is missing something to help the reader. I recommend adding the extra clarification.

p7c1h0.25 Equation (38) "... This confirms an initial assumption. ..."
No - this does NOT confirm the initial assumption as this satatement is not proven. It is important to nail this result down properly!

p7c2h0.05 change to "... the magnitude of both jz(r) and jθ(r) depend only on the values of Bz(0), μ. and r. ..."
The variable r is obviously missing in the "depends only on" phrase. Of course, that is implicitly understood, but it may lead to "loose thinking" and it would be dangerous to omit it.

p7c2h0.6 change to
"... 2. At every point j and B are collinear.
3. At every point μj = B. So μ is the only proportionality constant relating j and B. ..."
No specific point is referred to in the first numbered item, nor in the preceding paragraph. As such, "... there is no such ...".

p9c2h0.65 change to "... So in (50) the B_curl_???_F&Efree[2,2] element becomes as in (41) ..."
or "... So in (50) the B_curl_???_F&Efree(2 2) element becomes as in (41) ..."
The notation [22] is ambiguous and confusing for an array reference. It is best to name the array (as per my suggestion above), and use conventional notation (as per Matlab or whatever).

p9c2h0.65 Terms (51) (52) (53) (55) (56) are the same as the left-hand-side of equations (42) (44) (45) (47) (48), and (54) is a pure repeat of (48). This isn't wrong - it just causes the reader to spend a bit of extra time to double-check that there isn't something "extra" or "missing", and the way it is presented is more "complicated" than it needs to be. It does not make sense reporting (55), as that intermediate result collapses immediately to (56), as shown p7c1h0.95 (48).

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C5. REFERENCES

(using a quick web search, as opposed to checks using Scopus or standard indexes. I do not have access to CrossRefs "CrossCheck" via Elsevier's "iThenticate")

C5a) Are references and citations in the standard format?
One journal example :
Chun, M., Biglou, J., Lenard, J., & Kim, J. (1999). Using neural networks to predict parameters in the hot working of aluminum alloys. Journal of Materials Processing Technology, vol. 86, pp. 245-251

Reference and citation styles vary by journal, but in general much of the information required has been included in the references.

One special comment - the dates of the references are often missing, and sometimes page numbers are missing. Personal communications should be dated (earliest to latest dates would often be required), and websites continually change so it helps to put the date that the site was downloaded

or read.

Other suggestions : Include URL links for copies (hopefully free copies) and ISBN numbers

C5b) Are references legitimate (using a quick web search and personal familiarity with references)? By a spreadsheet random function, I randomly selected 5 references for checking from among those which are available :

[3] Mariner-Venus 1962 Final Project Report, NASA-SP59, Scientific and Technican Information Division, 1965, Washington, DC.
>> OK (available through amazon). Spelling mistake with “Technical”. Paperback date January 1, 1965

[7] T. Potemra, “Alfvén Waves and Birkeland Currents”, Physica Scripta Vol. T60, 107-112, 1995, The Johns Hopkins University, Applied Physics Laboratory, Laurel, MD.
>> OK http://www.plasmauniverse.info/downloads/Potemra-AlfvenW-BirkCurrents.pdf

[9] H. B. Callen, THERMODYNAMICS AND AN INTRODUCTION TO THERMOSTATISTICS (2nd Ed. ed.). New York: John Wiley
>> OK, September 12, 1985, ISBN-13: 978-0471862567
http://www.amazon.com/Thermodynamics-Introduction-Thermostatistics-Herbert-Callen/dp/0471862568

[10] Reference Data for Radio Engineers 4th edition (1956) ITT, New York, p. 1065, 1088.
>> OK (1962 listed for hardcover)
http://www.amazon.com/Reference-Data-Radio-Engineers-Edition/dp/B000TS5QGG

[14] H. Alfvén, “Evolution of the Solar System”, Washington. D.C., USA: Scientific and Technical Information Office, National Aeronautics and Space Administration. (1976).
>> OK http://www.amazon.com/Evolution-Solar-System-Hannes-Alfven/dp/1410218848
2004 paperback from Amazon.com mentions Gustaf Arrhenius as co-author

C5c) Is this paper significantly different from previous papers by the same authors? Is the work original? Normally, this is the hardest check to do thoroughly, especially to cover foreign-language reports.
I did not do this check properly. However, as I have [read, seen] several [papers, book, presentations] of the author, including within the last year (YouTube), the content is clearly a novelty compared to his work that I am aware.

C5d) Is this paper novel with respect to the literature?
As with the item C5c) above, I did not do this check, which I really should have. I would be very surprised if something similar had NOT been done in the past, as it seems like an obvious simple first approach for an ideal (force-free, field-aligned) situation. It would be the sort of thing one might seen in introductory courses for plasma physics for astronomy (maybe even an exercise in Peratt's book?).

C5e) Is the relevant literature well represented in breadth and Depth?
A range of good references are provided for key concepts, data, and situations. The practical examples from modern astronomy add a great deal to the paper.

****

C6. LIMITATIONS OF THIS REVIEW

Reviewer's expertise on the subject: Low
I have neither an astronomy nor plasma physics background, but these are active areas of reading and a limited amount of [concept, model development] for me. In addition, I have read fairly widely about specific challenges related to astronomy and physics, including some covered by the "Electric Universe" community.

Perhaps the most serious weakness of this review is that I have not (yet) taken the time to go through what must be a huge number of models and data for Birkeland currents approximating the "force-free, field-aligned" ideal situation modelled in this paper. But it was most important for me to go through the current paper in detail to better understand the [assumptions, limitations, strengths] of the work, and to gain confidence in the author and his associates (which I now have).

I have made no attempt to run matlab code to check results (assuming the author's code is available, and actually I have to convert to scilab code, which isn't always practical).

I'm VERY rusty with Bessel functions, albeit this paper's application is fairly straightforward.

Summary - Verifications of model derivations :

Step-by-step checks on the math were only performed for selected parts of the paper, as indicated in Section "C8. MATH CHECKS - step-by-step " : my checks are placed between +-----> and <-----+. The "mechanics" of the model derivations are quite solid. The only concerns I have are with respect to :

(14) $\alpha * B_r = 0$
(14) is probably OK, but perhaps I will revisit more thoroughly later. I'm not totally comfortable with this assumption, even if at first glance it looks reasonable. Stronger proof is required.

(25), (42) : There is a factor of alpha (α) involved in derivatives, whose treatment may be inconsistent.

(C38a) $0 = \mu * j_r$: stated (I'm not 100% comfortable)

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****

C7. THOUGHTS:

(again, changes to the paper are not require for these)
Here are some long-winded thoughts that are not really relevant to the paper review per se... For interest only, even if that.
These are separated from the "COMMENTS" above because they are less relevant to the actual paper.

Consequences and applications :

There are quite frankly too many themes to cover any but a random-scattered selection of a few. So here is a small sampling...

Climate - Planetary motions and solar activity
Of the end effects, the one with the most pertinence to climate discussions I have had over several years, is the "tension-repulsion" axial component, as this may provide a plausible mechanism for the repeated observations and claims that planetary motions correlate well with solar activity. These claims date right back ~150 years to the mid-1800's when the ~11.2 year sunspot half-cycle was first described. However, the suggestion that this correlation may be causitive has met with repeated attacks and "dis-proofs" that this cannot be so, often based on assuming gravitational models similar to those used to explain ocean tides.

By far the best long-term solar activity models I've seen are those of Ivanka Charvatova, upon whose work the 7.5 ky “naive, one-dimensional, lunatic” model of history (rise and fall of civilisations) of my father and I was built. (Our theory doesn't work, but it was awesome fun and arguments!) Ivanka, following in the footsteps of ?Theodore Landscheit?, presented a data-driven model of solar activity (grand solar episodes) based on planetary motions.

Galactic/cosmic rays (Svensmark)
Svensmark's extensions and data revelations for the galactic-cosmic ray theory of climate are possibly one of the best overall correlates of climate over all timescales up to the middle of the last glaciation period. What light can Birkeland plasma theories shine on this area?

Sun-Earth-Moon and Climate
Paul Vaughan is a friend in Vancouver BC who I consider to be the best "mid-time-scale" climate model that I've ever seen, although his work is only partially explained. Up until 2007, many of his models included planetary correlates such as [Jupiter-Earth-Venus, Neptune-Uranus?, etc - I forget details now!] , but he was severely attacked by physicists for making this suggestion. In 2007, Le Mouël etal published a key paper that showed the strongest climate correlation Paul had ever seen up to that point :

> *Jean-Louis Le Mouël, Vincent Courtillot, Elena Blanter, Mikhail Shnirman Jun08 “Evidence for a solar signature in 20th-century temperature data from the USA and Europe” C. R. Geoscience (2008), doi:10.1016/j.crte.2008.06.001*

Paul then posted (available somewhere on my website...) analysis showing why the paper by Le Mouel etal was successful, and Paul also improved on it. A year ago or so, Paul had basically dropped his work, waiting for another 2 years of data to provide a "proof" of the relationship, and also to take a break of several years of work in poverty as an amateur living in a very expensive city (he's gone back to [hiking, kayaking, etc]). He has no interest in climate predictions, feeling that even though the correlations are the best around, "no one can predict the sun".

Other consequences & applications
... I've run out of time to comment on many, many other consequences and applications of this type of work.

Evocations of other somewhat-related concepts and contexts

Bode's Law for [planetary, electron] orbitals
Bill Lucas

Rob Johnson's paper on "water bridges"
Rob's paper comes close to addressing the tension end-effect, but I don't see an analysis by Johnson like Scott has done, starting with Peratt's equation :

> *Robert J. Johnson 2012 "Plasma-like Behaviour of Partially-Ionized Liquids Part I – The Floating Water Bridge" Independent researcher, Oxford, UK, doi: 10.14294/WATER.2012.1*
> *http://www.waterjournal.org/volume-3/johnson*

There MUST be axial tension end effects for the water bridge (which is concave downwards).

Find a solution by starting with the answer
This is actually praise for the current paper, rather than a complaint! Advanced models of plasmas look to me to be massive challenges, likely suffering from the usual problems of :

- a lack of detailed [time, spatial, field] experimental data for the entire Birkeland current and the changes it undergoes. Even lab data will inherently suffer from this, whereas actual astronomical data is likely to be hugely incomplete, necessitating considerable “guesswork and inferences”.
- flaws in the fundamental theoretical physics and astronomy - in spite of longstanding claims to the contrary, many surprises likely remain for the coming decades if not centuries. Bill Lucas' re-visitation of Maxwell's equations are a great example, as is the ongoing (right from the start) failures with redshift, and to me - relativity etc, etc etc.
- False confidence arising from the “small-world universal function approximator” effect. As problems become more complex, so too do the tools, to the point where they become powerful approximation tools that perform some of the role of “universal function approximators” (eg [polynomial, Fourier, wavelet, neural network, evolutionary computation) which may be saying less and less about the veracity of the initial theorems other than they can at least accommodate the initial theory, as well as others that may be contradictory in a phenomenological sense.
- [Local, multiple] minima and global solutions - With complex problems, they is often no guarantee that multiple solutions may exists, or that one has obtained “good solutions” rather than “the actual answer”.

But Scott's paper is an example of where very simple approximations may yield good clues, but more than that they may be a great starting point to find approximations to feed into much more complex, problematic and time-consuming models so that they can converge to an appropriate result, and so that can be done orders of magnitudes faster than by inputting random or erroneous initial estimates.

This can be extended by having on hand a multitude of pre-calculated solutions for a wide variety of conditions (plasma conditions as per Peratt's dimensional analysis, for example, or initial and boundary conditions). If appropriate classifiers can be derived (as with neural nets or a variety of other approaches), this “solutions space” can provide good initial guesses. {Blending, morphing] techniques might also be possible to estimate good initial guesses on the basis of several diffent situations in the “solutions space”.

To me, this work serves to :

*Further strengthen the link between AND * [mythology, science, history, antiquity, religion]*
- as pursued by the “Electric Universe” community and its predecessors at least since the 1940's

Petroglyphs
This paper evokes concepts and imagery related to the astronomical inspirations of many symbols in ancient petroglyphs, as developed by David Talbot, tied to physics by Wal Thornhill, and provided with a solid experimental analogy and very extensive field data by Anthony Peratt. My gut field is that Peratt got a bit carried away, but he was right to shock me into a much larger vision than I could otherwise attempted.

Drive for corrections to wayward modern astronomy and physics
- which have let themselves be led by imaginative but arbitrary and often non-sensical theories with little or no ties to honest data. For example, think of one of the current science religions, that “CO2 has been the primary driver of climate change for the last 150 years”. This “kicking against the mainstream scientific pricks” has again been a key objective and result of the “Electric Universe” community, but also of the “John Chapell Natural Philosophy Society” (which itself split from the original “Natural Philosophy Society” which had been hijacked by strange egos). Of course, over the last 110+ years, many other groups and individuals have tried to do the same, but swaying the disciples (scientists) of the great new scientific religions is no different than with real religions, and this may take centuries.

****

C8. MATH CHECKS - step-by-step

The following note may be ignored by the authors, as it is simply a record of this reviewer's step-by-step check over a part of the paper.

As a reviewer, I find that a step-by-step re-typing of a part of the paper as I have done below forces me to pay attention to details that I might otherwise skim over. Even though this is perhaps too time intensive to apply to the full paper, by doing so over part of the authors' work, it gives me far grater confidence in the rest of the paper, which is read, but not analysed step-by-step. It also gives the authors a better idea of the weaknesses of the reviewer!

On the long term, the idea is to use software such as “High Order Logic” (HOL), Isabelle, or others to provide a more rigorous testing of theorems.

Not that although I redid the derivations independently, this check is NOT fully indep;endent of the author's work, as by following step-by-step, there is a high likelihood that I will repeat the same errors, and not see gaps and alternate ways of doing things. This is essentially a check on the “mechanics” of the model only.

+----+
Nomenclature :

** denotes [multiply of scalars, cross-product of vectors,] depending on arguments*

^ denotes [power, superscript] (eg a^b)
_ denotes subscript

3 3 reshape B - means to reform the data in an array of 3 rows and 3 columns.
This notation is used for arrays to avoid sophisticated formatting, and to permit the DIRECT use of expressions in an expression-processing system at some time in the future.

A denotes [scalar, vector, matrix]
A_T denotes transpose of A, also transpose(A
|A| denotes absolute value of matrix A (each element)
||A|| denotes spectral norm of A ||A||2
A_bar denotes an overscore on A
A_tilde denotes the authors' use of tilde over a Matrix symbol

******* *denotes start/end of topics & sub-topics*
+-----+ *denotes steps in a [proof, development]..*
//---/ denotes checks on specific steps by the reviewer (me)
>> short reviewer comments
(C21b) means an equation derived or introduced during the Check by the reviewer (me).

Greek and Latin symbols are written in short text form.
deriv(dx : y) means the derivative of y with respect to (wrt) x
derivp(dx : y) means the partial derivative of y wrt x
deriv2(dx : y) means the 2nd derivative of y with respect to (wrt) x
deriv2p(dx : y) means the 2nd partial derivative of y wrt x
integ(dx, from a to b : y) means the integral of y wrt x, from a to b

These substitutions were necessary given the limited number of useful ASCII characters, the lack of superscript and subscript with simple text editors, and to make the text easier to use in software for expression processing (eventually - not ready yet at this time).

Also, note that "" for (ambiguously) [scalar multiplication, vector crossProduct, vector dotProduct] are made EXPLICIT, in preparation for [simplifying, disambiguating] symbolic expression evaluation at some time in the future.*

Math reference for checks :

Erwin Kreyszig 1972 "Advanced engineering mathematics" Third edition, John Wiley & Sons, 1962,1967,1972 866pp ISBN 0-471-50728-8

Verification of Scott's model development

Summary of verification issues

I've missed a few checks below. However, the only concerns I have are with respect to :

(14) α*B_r = 0
(14) is probably OK, but perhaps I will revisit more thoroughly later. I'm not totally comfortable with this assumption, even if at first glance it looks reasonable. Stronger proof is required.

(25), (42) : There is a factor of alpha (α) involved in derivatives, whose treatment may be inconsistent.

(C38a) 0 = μ*j_r : stated (I'm not 100% comfortable)

****

Step-by-step derivations :

See the comments below regarding the issues for each equation derivation.

(1) F = q* (E + v*B)

(2) curl(B) = μ*(j + ε*derivp(dt : E))

+----->
Here I STRONGLY recommend clarifying/formalizing the assumption that is being made - of a steady state system where there are no time-varying quantities :

(C2a) curl(B) = μ*j

Such that curl(B) and J are co-linear.
<-----+

Key condition addressed : force-free, field-aligned :
(9) 0 = F = q*(v*B) = j*B

+----->
From (9), j & B are co-linear, and from (C2a) curl(B) and j are co-linear, therefore B and curl(B) are also co-linear, so their vector crossProduct is zero :

(C10) curl(B)*B = 0

which is the same as (10) below, where I've called the expression “CurlCrossVect
<-----+

(10) CurlCrossVect(B) = curl(B)*B = 0

(11) curl(B) = α*B

In polar coordinates :
(12) curl(B) = [1/r * derivp(dθ : B_z) - derivp(dz : B_θ) ,
derivp(dz : B_r) - derivp(dr : B_z) ,
1/r * derivp(dr : r*B_θ) - 1/r * derivp(dθ : B_r)

+-----> check on expression (12) :
From http://www.ittc.ku.edu/~jstiles/220/handouts/Curl%20in%20Cylindrical%20and%20Spherical%20Coordinate%20Systems.pdf

The image on that web-page has the same form as (12).

From http://hyperphysics.phy-astr.gsu.edu/hbase/curl.html#c2

curl(B) = determinant (3 3 reshape (
(1_r/r 1_θ k/r
curl_r(B) curl_θ(B) curl_z(B)
B_r r*B_θ B_z
))

I should derive this from 1st principles for practice - (nope, not today!).
<-----+

(13) α*B = (α*B_r, α*B_θ, α*B_z)

+----->

At this point, see my review comment “p4c1h0.2 ...” in the Section “C4. DETAILS and GRAMMAR: ” above, to augment the rational for the expressions (14) through (16).

Howell is WRONG!! with (14) “... Given the ASSUMPTION of cylindrical symmetry and axial uniformity along the Birkeland current from one end to the other(θ and z directions), there are no circulation patterns in the r direction, : ...”

(14) is probably OK, but perhaps I will revisit more thoroughly later.
<-----+

(14) α*B_r = 0
(15) derivp(dr : B_z) = -α*B_θ
(16) 1/r * derivp(dr : r*B_θ) = α*B_z

+-----> Combining (15) and (16)

(C15) B_θ = derivp(dr : B_z) / -α

(C16) 1/r * derivp(dr : r/-α*derivp(dr : B_z)) = α*B_z
derivp(dr : r/-α*derivp(dr : B_z)) = α*r*B_z

differentiating derivp(dr : r/-α*derivp(dr : B_z)) :
= -1/α * [derivp(dr : r)*derivp(dr : B_z) + r*derivp(dr : derivp(dr : B_z))]
= -1/α * [derivp(dr : B_z) + r*deriv2p(dr : B_z)]

Pulling all terms together :
-1/α * [derivp(dr : B_z) + r*deriv2p(dr : B_z)] = α*r*B_z
0 = r*deriv2p(dr : B_z) + derivp(dr : B_z) - α^2*r*B_z
multiply both sides by r, recognizing that the expression for B-z is no longer a partial derivative :

(C17) 0 = r^2*deriv2(dr : B_z) + r*deriv(dr : B_z) - α^2*r^2*B_z

which is the same as Scott's eqn (17) below.
<-----+

(17) 0 = r^2*deriv2(dr : B_z) + r*deriv(dr : B_z) + α^2*r^2*B_z

+----->
From Kreyszig p131, equation (1) is a Bessel equation of order zero (v=n=0) :

(K1) x*deriv(dx : derive(dx : y)) + deriv(dx : y) + x*y = 0

Kreyszig p133 Theorem 1 (General Solution)

(K9) y(x) = C_1*J_v(x) + C_2*Y_v(x)

This is the same as Scott p5c1h0.1 Equation (20) (see below)
<-----+

(20) y = A*J_0(α*x) + C*Y_0(α*x)

+----->
Kreyszig p127

(K13) J_n(x) = x^n*sum[m=0 to inf : (-1)^m*x^(2*m)/2^(2*m-v)/m!/(n+m)!]

For v=n=0 :
J_0(x) = x^0*sum[m=0 to inf : (-1)^m*x^(2*m)/2^(2*m-0)/m!/(0+m)!]
= sum[m=0 to inf : (-1)^m*x^(2*m)/2^(2*m)/(m!)^2]
(C21a) J_0(x) = sum[m=0 to inf : (-1)^m*(x/2)^(2*m)/(m!)^2]
(C21b) J_0(x) = 1 - (x/2)^2/(1!)^2 + (x/2)^4/(2!)^2 - (x/2)^6/(3!)^2 + ...

where (C21a) and (C21b) are the same as the corresponding expressions (21) in Scott's paper as shown below.

<-----+

(21) J_0(x) = 1 - x^2/2^2 + x^4/2^2/4^2 - x^6/2^2/4^2/6^2 + ...
= 1 - (x/2)^2/(1!)2 + (x/2)^4/(2!)^2 - (x/2)^6/(3!)^2 + ...

+----->
Now considering the plasma Birkeland current like a polar coordinate arrangement solution for membranes : (Kreyszig p450h0.7) "... Since the deflection of the membrane is always finite while Y_0 becomes infinite as s [x in the current case] approaches zero, we cannot use Y_0 and must choose C_2=0. ..."

(K8) W(r) = C_1*J_0(s) = C_1*J_0(kr)

Notice how similar this is to Scott p5c1h0.25 Equation 22 and its preceding paragraph: "... The function J0(αx) has the value unity at the boundary x = 0, and the function Y0(αx) has a singularity at this same boundary. Because reality dictates that the magnetic field be finite-valued, the value of arbitrary coefficient C must be set equal to zero. Thus, the solution to (17) is given specifically by ..."

Kreyszig p835 Appendix 4 Table A2 - confirms that Y_0(x) & Y_1(x) tend to infinity as x tends to zero.

As J_0(0) = 1.00, and as B(0)=B_z(0) (because B_θ(0)=0 and B_r=0 always), then C_1 = B_z(0), so in terms of the variable labels of Scott's paper, we arrive at the expression (22), with some concern for proper treatment of the proportionality constant alpha, α.
<-----+

(22) B_z(r) = B_z(0)*J_0(α*r)

+----->
Quick check - deriv(dx : J_0(x)) in equation (21) does yield (24) below.
Equation (24) below is the same as Kreyszig p127h0.8 Equation (13) for v=n=1.
<-----+

(23) J_1(x) = -deriv(dx : J_0(x))

(24) J_1(x) = x/2 - x^3/2^2/4 + x^5/2^2/4^2/6 - ...

+-----> From expressions (15), (22), and (23), we obtain

(C15) B_θ(r) = derivp(dr : B_z)/-α
(22) B_z(r) = B_z(0)*J_0(α*r)
(C22a) B_θ(r) = derivp(dr : B_z(0)*J_0(α*r))/-α
= B_z(0)/-α*derivp(dr : J_0(α*r))
where :
(23) J_1(x) = -deriv(dx : J_0(x)) substitute α*r for x, α*dr for dx
(C23) J_1(α*r) = -α*deriv(dr: J_0(α*r))
so :
(C25) B_θ(r) = B_z(0)*-α/-α*J_1(α*r)
= B_z(0)*J_1(α*r)

which is the same as Scott's (25) below
<-----+

(25) B_θ(r) = B_z(0)*J_1(α*r)

(26) J_0(x) = 1/π*integ(dφ from 0 to π : cos(x*sinφ))

(27) J_1(x) = 1/π*integ(dφ from 0 to π : cos(x*sinφ - φ))

+-----> Asymptotic forms
I didn't check these ....
<-----+

(28) J_0(x) = sqrt(2/π/x) * [cos(x - π/4) + O(1/x)]

```
          J_1(x) = sqrt(2/π/x) * [cos(x - 3π/4) + O(1/x)]

(29)=(16)  derivp(dr : r*B_θ) = r*α*B_z

simple derivative :
(30)       r*derivp(dr : B_θ) + B_θ = r*α*B_z

[Howell - curious why these are re-expressed as "state variables" in (33) and (34) ? May
be simple due to Matlab code convention?]

[Howell -  WARNING!  the term "+ ε*derivp(dt : E)" was dropped in the version (2a) given
below!!  That is OK as long as the reader remembers the simplification used (steady
state, non-changing current), but it would be best to repeat that assumption here. ]

(2a)       curl(B) = μ*j

(35)=(22) B_z(r) = B_z(0)*J_0(α*r)

(36)=(25)  B_θ(r) = B_z(0)*J_1(α*r)

(37)=(12)  curl(B) = [ 1/r * derivp(dθ :   B_z) -       derivp(dz : B_θ) ,
                             derivp(dz :   B_r) -       derivp(dr : B_z) ,
                        1/r * derivp(dr : r*B_θ) - 1/r * derivp(dθ : B_r)
                     ]

+-----> processings of terms in (37)
(C14)      α*B_r = 0 therefore derivp(d[r,θ,z] : B_r) = [0,0,0] (shorthand notation)

This reduces (37) to :
(C37)=(12) curl(B) = [ 1/r * derivp(dθ :   B_z) -       derivp(dz : B_θ) ,
                                             0    -       derivp(dr : B_z) ,
                        1/r * derivp(dr : r*B_θ) -       0
                     ]
restate (2a)
(2a)       curl(B) = μ*j = [ μ*j_r,  μ*j_θ,  μ*j_z ]

Therefore, for each direction, curl(B_i) = μ*j_i as they are all orthogonal and
don't affect one another as emasures (NEED to check this!!!), yielding :

(C38)      1/r * derivp(dθ : B_z) - derivp(dz : B_θ) =  μ*j_r
(C38a)     0 = μ*j_r    : stated (I'm not 100% comfortable)
           derivp(dθ : B_z) = R*derivp(dz : B_θ)  as a consequence (not used)

(C39)      derivp(dr : B_z) =  μ*j_θ

(C40)      1/r * derivp(dr : r*B_θ) = μ*j_z

These all agree with Scott's equations below, WITH THE CAVEAT THAT I'm not fully
confident that 0 = μ*j_r .
<-----+

(38)       0 = μ*j_r

(39)       - derivp(dr : B_z) = μ*j_θ

(40)       1/r * derivp(dr : r*B_θ) = μ*j_z

+-----> from (35) & (39)
(35)=(22) B_z(r) = B_z(0)*J_0(α*r)
(39)       - derivp(dr : B_z) = μ*j_θ

(C41)      - derivp(dr : B_z(0)*J_0(α*r)) = μ*j_θ
```

(C41a) -B_z(0)*derivp(dr : J_0(α*r)) = μ*j_θ
(23) J_1(x) = -deriv(dx : J_0(x)) substitute α*r for x
J_1(α*r) = -α*deriv(dr : J_0(α*r)) (since d(α*r) = α*dr)

(C42) -B_z(0)/α*J_1(α*r) = μ*j_θ

(C41) & (C42) are the same as (41) & (42) below, EXCEPT for the factor α !!!
<-----+

(41) - derivp(dr : B_z(0)*J_0(α*r)) = μ*j_θ

(42) B_z(0)*J_1(α*r) = μ*j_θ

+-----> from (36) & (40)
(36)=(25) B_θ(r) = B_z(0)*J_1(α*r)
(40) 1/r * derivp(dr : r*B_θ) = μ*j_z
B_θ + r*derivp(dr : B_θ) = μ*j_z*r
derivp(dr : B_θ) + B_θ/r = μ*j_z
derivp(dr : B_θ) + B_z(0)*J_1(α*r)/r = μ*j_z

(C42) derivp(dr : r*B_z(0)*J_1(α*r)) = μ*r*j_z

<-----+

(43) 1/r * [r*derivp(dr : B_θ) + B_θ] = μ*j_z

(44) derivp(dr : B_θ) + B_θ/r = μ*j_z

(45) B_z(0)* [derivp(dr : J_1(α*r)) + J_1(α*r)/α/r] = μ*j_z

+-----> check on (46)

(24) J_1(x) = x/2 - x^3/2^2/4 + x^5/2^2/4^2/6 - ...
so derivp(dx : J_1) = 1/2 - 3*x^2/2^2/4 + 5*x^4/2^2/4^2/6 - ...
J_1(x)/x = 1/2 - x^2/2^2/4 + x^4/2^2/4^2/6 - ...
derivp(dx : J_1) + J_1(x)/x
= 1 - 4*x^2/2^2/4 + 6*x^4/2^2/4^2/6 - ...
= 1 - x^2/2^2 + x^4/2^2/4^2 - ...
J_0(x) = 1 - x^2/2^2 + x^4/2^2/4^2 - x^6/2^2/4^2/6^2 + ...

so by a few terms (46) below is confirmed (I should have checked the general expression for each term! see (K13) above ...).
<-----+

(46) derivp(dx : J_1) = J_0 - J_1/x

+-----> (46) into (45), substituting α*r for x

(C47) B_z(0)* [J_0(α*r) - J_1(α*r)/α/r + J_1(α*r)/α/r] = μ*j_z

which is the same as Scott's (47) below
<-----+

(47) B_z(0)* [J_0(α*r) - J_1(α*r)/α/r + J_1(α*r)/α/r] = μ*j_z

simple elimination to :
(48) B_z(0)*J_0(α*r) = μ*j_z

+----->
Howell - The rest of the math is a check that [(14), (22), (25)] are solutions of (10).

As this is straightforward, and a fair amount is repeat, I did not go through these in detail.
<-----+

(49) B_curl_???_F&Efree = determinant (3 3 reshape (
(r θ z
curl_r(B) curl_θ(B) curl_z(B)
B_r B_θ B_z
))

(50) B_curl_???_F&Efree = determinant (3 3 reshape (
(r θ z
0 -derivp(dr : B_z) (1/r * derivp(dr : r*B_θ))
B_r B_θ B_z
))

where :
(51) -derivp(dr : B_z) = - derivp(dr : B_z(0)*J_0(α*r)) = μ*j_θ

(52) 1/r * [r*derivp(dr : B_θ) + B_θ] = μ*j_z = derivp(dr : B_θ) + B_θ/r = μ*j_z

(53)=(45) B_z(0)* [derivp(dr : J_1(α*r)) + J_1(α*r)/α/r] = μ*j_z

(54)=(46) derivp(dx : J_1) = J_0 - J_1/x

(55)=(47) B_z(0)* [J_0(α*r) - J_1(α*r)/α/r + J_1(α*r)/α/r] = μ*j_z
(56)=(48) B_z(0)*J_0(α*r) = μ*j_z

(57) B_curl_???_F&Efree = determinant (3 3 reshape (
(r θ z
0 B_0*J_1 B_0*J_0
B_r B_0*J_1 B_0*J_0
))
= 0

Calculation of total current in the Birkeland currents

While I did a preliminary integration of j(r), I did not complete this or go through iterative improments. This is an interesting integration, made relatively simple by hte properties of the Bessel function solutions (just as the relation between J_0 and J_1 is "clean").

A key issue is a pragmatic definition of R, the effective maximum diameter of the Birkeland current, and the round-off errors and the point at which r is sufficiently large that the inverse square root with distance" model result no longer applies, and the system behaves as the inverse of r (as with a copper wire).

Maybe at some later date ....

enddoc