Speaking notes for Gwyn Griffiths HamSCI Workshop 2025 presentation

Post-sunset Sporadic F propagation: A Sign of Electron Density Isopleth Convergence?

Slide 1

Good afternoon. Please feel free to give me quizzical looks over both parts of my title ”Sporadic F?” and what on earth are “isopleths”? This is work in progress and I welcome the opportunity to present these ideas in this HamSCI workshop’s supportive and constructively critical environment. I’m grateful to these individuals and organisations for the tools and data that have made this study possible.

Slide 2

I’ll start with the Ham part – with GPS aided transmitters and receivers using WSPR, automated Doppler measurements good to 0.1 Hz throw up fascinating puzzles. This is the story of post-sunset opening at 14 MHz across this continent that lasted about one hour.

WSPR is not the tool to answer ‘Why”. But this community has collectively built a great tool to help answer this and a myriad of other puzzles in HF propagation – the Grape network.

Building a picture of propagation modes and checking for a TID were straightforward, I felt on safe ground, but let’s see if I can make a convincing case once I get onto isopleths and the role they might play.

Slide 3

The Sacramento station of Carol KP4MD is typical of many that report WSPR spots using Rob Robinett’s WsprDaemon package. With WsprDaemon her GPS-aided KiwiSDR gives 0.1 Hz frequency resolution. I was looking for post-sunset variations in Doppler shift of the WsprSonde transmitter WW0WWV located on the WWV site at Fort Collins, and I was puzzled by this large variation – standard deviation – of Doppler shift in a two hour interval on the 26th – it’s an outlier. What was the cause?

Slide 4

With many WsprDaemon sites across North America it is easy to check whether the event was limited to the Fort Collins to Sacramento path or was it widespread?

Usefully, receiver W0DAS with line of sight to WWV at near zero Doppler rules out a transmitter fault, which could have been a cause of frequency shift common at all receivers.

Stations at four corners show a similar behaviour. Positive Doppler appearing then reducing, going negative before ending an hour or so after it started. Clearly this was real event across the continent.

The different times suggested a very roughly north east to southwest travelling disturbance.

But why was the opening only over a positive to negative half cycle?

Slide 5

My topic is the impact on propagation, not the source of the event, but for the record here is the timeline of geomagnetic conditions. There was a minor storm on the 25^{th}. Just prior to the 14 MHz opening there was a sharp rise in horizontal magnetic field anomaly at Fort Collins (3) and SuperMag Auroral electrojet indicies showed activity, although quite modest.

SMU is eastward, SML is westward https://supermag.jhuapl.edu/indices

Slide 6

To build a picture of propagation modes around the time of the 14 MHz opening I looked at what was going on at a lower frequency – 10 MHz.

Here I have used the Grape digital_rf data from the PSWS database to give this zoomed-in spectrogram for the path to John Ackerman's remote station at N8GA.

First, a point I really have to stress- there is no noise whatsoever in this plot. Everything that you see is a signal propagated from WWV. Let's look at the different modes. Here's our opening seemingly appearing out of nowhere large with a large positive Doppler shift, going negative and ending. And here's why I dropped to 10 MHz - to see what I expected from PyLap ray tracing -one hop propagation via the F2 layer. The thin line near zero is propagation via the E region. The fuzzy traces are from two-hop sidescatter, which, turns into two hop forward scatter immediately prior to the opening – giving me confidence that the high Doppler on this path was from two hop F2 propagation. Something must have caused the critical frequency to rise to produce this two-hop opening.

Slide 7

To tackle that question I need to step back and revisit what causes Doppler shift in these signals. Doppler is from rate of change of phase path. I have extracted just *part* of the determining equation from Chum et al.'s useful paper. Many studies of ionospheric Doppler to infer changes in height have focussed on the first term, the gradient term, and have assumed, implicitly or explicitly, that the other terms are small. That may not be the case. This slide is a caution – we need to keep the divergence term in mind, even though it may be difficult to identify. Here's why. On the right I have two simplified cartoons for a post-sunset TID showing three surfaces of equal electron density. There's the post sunset rise, and a periodic component from the TID. The top plot is for pure advection, the surfaces track each other – this is the assumption being made in only using the advection term. But what if the real situation is more similar to the bottom plot? Here we also have a divergence term. The surfaces diverge and converge – the second term comes into play and adds to the measured Doppler shift.

The third term, Production and Loss imbalance will not confuse us.

Slide 8

For it is very easy to spot and is most commonly due to the immediate effect of a solar flare. Here's an example in Grape data also from N8GA for an X8.79 flare last year. Doppler shift in magenta changed by about 1.2 Hz at 15 MHz but only seen for the few minutes between the X-ray level increasing and its ionising effect on D and E region absorption closing propagation.

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Slide 9

Returning to the second Doppler term – divergence – we clearly need to try and identify divergence, or compression and rarefaction, for which lines with equal values of a property – isopleths being the general term - are so useful. Isopleths of barometric pressure are so useful in meteorology that they have a name – isobars – and this plan view shows regions of compression, high wind, and rarefaction, lower wind. In oceanography isopleths of density are so useful they have a name – isopycnals – and ocean dynamics means that they are anything but boringly parallel. The ionosphere is anything but boring, so why would we expect parallel isopleths? Of course, they are not. Here's a real plot of isopleths of plasma frequency from an ionosonde in Spain from Reinisch et al. that inspired my earlier cartoons. There's clearly compression and rarefaction of the isopleths in this ionosonde data.

Slide 10

Am I able to determine isopleths from the Grape data? No. But, here's an attempt to get a flawed estimate that may be good enough to shed some light on why there was a post-sunset 14 MHz band WSPR opening with large Doppler. Step one is extracting as digital values the one-hop 10 MHz Doppler shift from the PSWS digital_rf data. Not as easy as it sounds given the multiplicity of propagation modes. But we do get a nice time series of one-hop Doppler, in orange, and I've added the two-hop Doppler in magenta with a right hand scale.

Now here's an interesting observation: the two-hop propagation started just after an inflexion in the one-hop Doppler. Given the previous two cycles, should the one-hop Doppler not have continued to reduce? Instead it increased slightly and stayed high. I think this is a valuable clue.

Slide 11

This is the flawed step – using the 10 MHz Doppler I derive a 'height of reflection' in the usual way – which assumes *all* of the Doppler is due to advection. The result is the brown trace.

From the Alpena ionosonde I've plotted the true height of half the maximum electron density. There's a peak in the correlation coefficient squared if I move forward the Alpena data by fourteen minutes.

The scatterplot of the two 'heights' shows this good correlation, but the slope is not one. It is one point five. Assuming the Doppler was 100% due to advection gives larger variations in height than the ionosonde height measurement I have chosen. One reason might be that my assumption of 100% advection was not right. Does this suggest that 'some' of the measured Doppler was from the divergence term?

Slide 12

I've yet to find a reference for what effect divergence during a TID might have on Doppler but Chum and coauthors have looked at the effect on Doppler of compression and rarefaction due to infrasound waves, admittedly with much shorter periods than TIDs. With a wave period T of fifty seconds the multiplier in their experiment was ten. I may have missed an equivalent for TIDs in the literature, please do let me know if that is so, if there isn't – it's a clear gap.

Slide 13

For the top graph I repeated the procedure to get height of reflection for the 5 MHz one hop Doppler to N8GA, the blue trace. Now I have two isopleths, but both likely flawed by the same incorrect assumption of being 100% due to advection. Nevertheless, lets calculate their height difference as an indicator of convergence and divergence. The smaller the difference, the greater the convergence, and the bottom plot does show variations.

The little map in top right is a reminder that the Doppler measurements are for the one hop path, with reflection around the yellow dot. On the bottom plot I have added the WSPR Doppler received during two hop propagation, hence reflected not at the same location, but near the white dots. And so back to my title – we did have a sporadic opening of F layer propagation that suggested a higher critical frequency – and my question was whether this was linked to convergence of electron density isopleths. I'm half convinced by this graph. There were two sets of WSPR spots, I certainly think the flawed isopleths show that both openings at least started when the isopleths were closest together.

Slide 14

A close look at this zoomed in plot of critical frequency isopleths from Reinisch et al. shows two small features that support my case. At A and B, just after peak heights, isopleths appear out of nowhere with higher critical frequencies than were present on the rising phase. I'd argue that it's convergence that led to those higher critical frequencies emerging.

Slide 15

While Ham WSPR reports are great for bringing propagation anomalies to our notice they may not tell us much about the physics. To investigate my proposition that the opening was due to convergence of isopleths Grape Doppler data certainly helped. But I faced two problems. First, I knew I should not ignore the contribution of divergence to the measured Doppler shift but I had no way of identifying its contribution. Second, the technical challenge of extracting Doppler time series when there were multiple co-propagating modes was not trivial. Nevertheless I have showed that convergence was likely present during the positive Doppler phase of the TID, that is during descent phase of the height of reflection and that the start of the WSPR spot opening coincided with greatest convergence. And finally, the prolonged positive Doppler, continued descent, of that one cycle may have been important.

Thank you.