APSA Paper, Thoughts

3 messages

John Freeman <freeman@umn.edu> Mon, Aug 9, 2021 at 9:54 AM
To: Logan Stundal <stund005@umn.edu>, Benjamin Bagozzi <bagozzib@udel.edu>

Hi Logan and Ben
GREAT progress! But glad the APSA meeting isnt until
end of September. This gives us some time to dig into this analysis
more deeply than if it was in several weeks.

Manuscript
I sent this separately, both the pdf .tex file.
a. Title page. Not sure what you two want on this We can say "Prepared for
presentation on Theme panel, Pluralistic Approaches to Complex Inferences" etc.
Not sure if we want to say this work is supported by old NSF grant. It is in one sense.
b. Happy to draft an abstract and findings summary (in Introduction)
once we settle on results.
c I realized last night looking at Logan's memo that we are using an AR(1) set up for the
GMRF, not a RWalk(1) set up; see pages 8-9. I wrote up both options but we can
omit one of them here. [Might be interesting to check how results change if we
impose rho=1 (RW1) as in Python et al. 2018 (with prior on RW variance).]
Note. With rho estimated to be negative for SIGACTS we have a damped
oscillatory pattern of autocorrelation Have to think about this (but might be
tied to army strategy somehow)
c. Ben's comments suggest we might want to break out SIGACTS-ICEWS-GED
comparison, maybe not including machine and human coded
results in first set of tables and figures. Also Ben wrote up nice summary
of three data sets. I have Ben's write up but its not dated. This write up should
be added in our appendix. Ben, can you send it to Logan?
d. Logan, please add Krainski et al book to references

Memo of yesterday
1. Summary data are for half-year time unit, correct?
2. Not sure about bias model. Interesting idea but cant remember any such set up
in literature
3. Maps are fascinating and seems to belie Silverman's and others' case for
localization; interesting "small scale" space time signatures (of diffusion, relocation
etc)
4. Also, need to study maps more closely re Donnay and Filmanov (2014) claim
 there are no meaningful dynamics pre 2006 and post 2008. Another analysis for
appendix might be break out of this period for estimation of main models.

Going forward
5. Logan, I think you used the default priors in R-INLA for the GMRF estimation (including
the prior |rho|<1?) Can you summarize those? Maybe you can just write this up in
the next version of the manuscript
6. You already did the Silverman replication, correct? Can you put that in the first table.
A vanilla Silverman replication without lagged DV but with GMRF instead of
fixed effects would be a useful (most straight forward) comparison here.
7. Weidman and Ward (2010) did compare a STAR model with time lag of DV only
to one without a lagged DV. But then they also include a full spatial weight plus
lagged DV set up (full STAR) as in our equation (2) in the manuscript. Can you
include this full STAR model going forward, maybe with a queen W matrix? You may
have already fit this model. I cant remember. DONT want to ask for too much but
it would be useful to compare a full STAR model with fixed effects(?) to a full
STAR model with GMRF?

ok. This is it for now.

thanks alot
John

Benjamin Bagozzi <bagozzib@udel.edu> Mon, Aug 9, 2021 at 10:39 AM
To: John Freeman <freeman@umn.edu>
Cc: Logan Stundal <stund005@umn.edu>

Thanks John,

Logan -- as John mentioned, I have attached the data write-up here, which we can add to the paper's appendix. If you'd like, you can perhaps send me the updated draft once you've combined your pieces with John's and I can add this in as an appendix. Though you're welcome to integrate this as well if you prefer (I'm just not sure we need all of the detail in section 2...so I can condense this if we'd like).

How about we add the NSF acknowledgement to the title page, since there is no cost to us in doing so?

I agree with Jon that it would be interesting to check how results change if we impose rho=1 (RW1), and also with his suggestion of incorporating the earlier Silverman replication table into these results and the combined paper.

I would say we scrap the current bias models for now. I will keep thinking about how to evaluate this across our models (beyond the additional quantities we currently report). If we held out the final half-year period we could try predicting that from each model to see how each model preferomed (maybe using mean squared prediction error or something akin to that, given our DV). But the challenge with that is this would mean the periods included in our main models do not match those of Silverman's. In-sample mean squared prediction error might be one alternative.

Logan, let's definitely try to get a handle on if the results from yesterday change when accounting for the Baghdad district in different ways. In addition to the condolence spending difference, the coefficient on Ruzicka Spending for the ICEWS model is massive (and positive), relative to SIGACTS & GED.

Cheers,
Ben

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Iraq_Write_up.zip
292K

Logan Stundal <stund005@umn.edu> Fri, Aug 13, 2021 at 5:43 PM
To: Benjamin Bagozzi <bagozzib@udel.edu>
Cc: John Freeman <freeman@umn.edu>

Hi John and Ben,

Thanks for your replies. I had to catch up on some things I postponed while completing the PA replication materials and this first pass through the SIGACTS analysis, so thanks for your patience on my followup! I should have some new results and tables by tomorrow that incorporate what you both have suggested here. Replies:

John
Manuscript
a - I'd be fine with a title page footnote indicating which panel it was prepped for.
c - Agh! AR1 for the GMRF is an oversight on my part. Let me revisit this. Changing from an AR1 to a RW1 is very easy in the code. I think the AR1 on the DV carried over into my mind into the GMRF and I forgot Python employed a RW.

d - I'll add Krainski et al.

Summary memo
1 - yes, these use the half-year to keep as close as possible to Silverman main article results
2 - Agreed. I'll dump these bias models on the next pass through constructing the tables.
3 - The fields also seem to imply the conflict diffusion / relocation theory implied by Schutte and Wiemann in their pol. geog. article.

Forward
5 - I realized I forgot to mention priors after hitting send! Here I used penalized-complexity priors and did perform a bit more calibration on the range prior for the SPDE. I'll write this into the tex file.
6 - Yes, the primary 1-to-1 Silverman replication (dropping Baghdad, analytic population weights) that perfectly matches his published results is done.
- I'll remove the ICEWS/GED models (and the bias ones) and add a first-column for Silverman as we have replicated, Silverman without FEs but with GMRF, with AR1 with spatial lag, and AR1 with GMRF.

7 - Yes! I have estimated the STAR model implied by eq. 2 and will include it in the model table with the other models (re: pt. 6 above).
- I have not fit a STAR model with a GMRF however. Rather the one I fit assumes the epsilon_it in equation 2 on page 5 is normally distributed iid. I can investigate if a STAR with GMRF is possible in INLA though.

Ben
Thanks for sending the data write-up. I'll create an Appendix tex file following the setup of our Colombia paper and when I send the revised documents along it'll be easier to make edits to. I'll add your data write-up to that appendix tex document. There are ways to make predictions using these INLA models that directly incorporate the field estimates. And, yes, I was not sure what was causing the Ruzicka spending divergence. I'll investigate in the next draft by both dropping Baghdad and including a Baghdad dummy.

I should have these changes incorporated into the draft in the next day or so.

Best,
Logan

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