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Discussion of "An epidemiological forecast model and software assessing interventions on the COVID-19 epidemic in China"

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I would like to commend the authors for their fast development and clear presentation of an important model for studying the effect of a temporally varying transmission rate modifier on the trajectory of COVID-19. The paper extends work by Osthus et al. (2017), modeling noisy observed proportions of infected and removed cases as coming from an underlying state-space SIR model.

As noted in the paper, parameter identifiability issues pose a severe challenge to predicting the peak or end of an epidemic during the exponential growth phase. This concept was well illustrated in Osthus et al. (2017) for influenza prediction; combinations of parameters in the model that agree with influenza data early in the season will not necessarily result in reasonable late season forecasts (see Figure 3 for an example). Of course, Osthus et al. [2017] could only designate one set of predictions as reasonable and another as unreasonable because the seasonal flu has a set of historical data from which one can learn about "typical" patterns. Wang et al. do well to note that with COVID-19, "whichever the chosen model is used used, the model itself will dictate prediction results."

I mention the Osthus et al. (2017) illustration because it came to mind when I saw the severity of the predicted infectious proportion with the basic SIR model (i.e., that having $\pi(t) \equiv 1$) in Wang et al.'s Figure 6. My instinct was to think "Well, I'm sure this result is quite sensitive to initial conditions" and pick apart the authors' priors and assumptions. I then decided that, while sensitivity analyses showing this predicted proportion under various prior/hyperparameter specifications would have been appreciated, the star of this figure and the paper in general is the contrast between the basic SIR model and the models incorporating some time-varying intervention. In a sense, it is comforting that the models run with a transmission modifier or quarantine process show relatively optimistic predictions even when the basic SIR model looks so catastrophic.

The authors' do well to show results under a handful of different $\pi(t)$ functions, but uncertainty about $\pi(t)$ is not reflected in individual model predictions. To propagate this uncertainty about $\pi(t)$, an option would be to have this function be another model parameter, informed by data, rather than a fixed quantity. To do so, one would need to link $\pi(t)$ to some real measurement of the reduction in the chance of a susceptible person meeting with an infected person. Of course, measuring this quantity is nontrivial, particularly in places where macro isolation measures are either not in place or not universally enforced. Proxies could include mobility data, self-reported surveys, school/workplace closures, etc. But, given the general issue of parameter unidentifiability, I worry that even if $\pi(t)$ is measured (or approximated) well, we may still not be able to tell if we're hitting the threshold at which the true transmission rate modifier is too high (i.e., if in Figure 6 we're going to end up nearer to column 1 than the latter three columns).

Similarly, I think the message regarding the effect of loosening restrictions is more impactful in spirit than in exact measure. That is, I find the authors' Figure 7 a good reminder that too much loosening of transmission modification measures can lead to poor outcomes; I also don't

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think the model as-is can be used to guide policy on what exact value of $\pi(t)$ we need to shoot for to avoid the red scenario in Figure 7. This issue is particularly salient right now as government officials and citizens debate when/how/what reopening should take place (I am U.S.-based, so have spent many hours in various levels of disbelief as I catch up on the latest news). I think a lot of people would appreciate a tool allowing them to play with step timing/values for $\pi(t)$ and run the model with their local data to get a handle on the possible outcomes of various choices. This thought leads nicely to my next set of comments...

I appreciate how the authors emphasized the potential for their model to be used by health professionals, who may have better or more relevant/current data to use as inputs. I think their creation of an R Shiny App was a step in the right direction but that it still has some work to be done in terms of realizing this potential. Specifically, I wish the app had the capability for users to change more model settings and see the resulting output change, and to input new data. I trust the authors will expand on the App in the coming weeks/months for use in other COVID-19 modeling efforts. In playing with the App, I appreciated the ability to see how the forecasts changed with each new day of data, and the chance to see the paper results come to life. I personally will now consider creating R Shiny Apps to accompany my own papers, since I love the idea so much!

References

Osthus D, Hickmann KS, Caragea PC, Higdon D, Del Valle SY (2017). Forecasting seasonal influenza with a state-space SIR model. *The Annals of Applied Statistics*, 11(1): 202–224.