**Referee 1**

* The Introduction says “Exploratory mathematical modelling into optimising NPIs has arisen from these retrospective analyses (9-15)” and the Discussion says “This work builds on previous epidemiological modelling (9-15)”. Can the authors be more specific about what it is in each of Refs 9-15 that the work builds upon?

*We agree that the sentences found in the Introduction and Methods sections were not specific enough with regards to these six references. We have since updated the revised manuscript to reflect the nature of this work which was conducted in March/April for SPI-M, and that the six referenced analyses (1-6) were conducted simultaneously or later than our original work. We have sought to clarify that while the other referenced authors have contributed to the existing preprint or published evidence base for optimal NPI interventions for COVID-19, only the Morris, et al (2) preprint contributed to the conceptual ideas presented in this study, specifically the concept of a “robust” intervention.*

*Therefore, we have altered the text on page 2-3, line 50-72 in the Introduction, to now more clearly describe the current evidence base for optimal NPIs and to reference the studies which contribute to our understanding of “robust interventions”. We have also altered the text, line 331 on page 11 in the Discussion to read: “This study adds to…” rather than “This study builds on…”.*

* What is the rationale for suboptimal interventions? Do the authors advocate that policy-makers should aim to achieve a gradual reduction in the contact rate rather than a faster one, or do they advocate that policy-makers accept that in reality a reduction in contact rates will be gradual and therefore they should plan for this, and trigger an intervention earlier than would be the case if a faster reduction could be achieved? The writing could also be tightened up: if these interventions are “not as obviously beneficial as an optimal intervention” then why do them? I presume the authors mean that achieving a theoretical optimum is impossible due to imperfect information and an inability to apply interventions perfectly and therefore one has to be pragmatic in designing policy. However, surely the “optimal” policy is the policy that works best in reality. If something works “in theory but not in practice” then the theory is wrong – or, at best, irrelevant.

*We thank the reviewer for identifying the lack of clarity regarding “suboptimal interventions” and the need for clarifying rationale behind this strategy.*

*We have now redefined what we previously termed “suboptimal interventions” in the original manuscript, as “robust interventions”. We have applied this change throughout the revised manuscript. We hope that by reframing the narrative, we can avoid a false initial impression that these interventions are merely inferior to optimal interventions, which was not the main message we wanted to convey to readers. By relabelling these interventions as “robust interventions”, we can refocus the narrative on the robust nature of these alternative interventions. However, we note it is important to not detract from suboptimal nature of these “robust interventions”. We therefore still accompany references of “robust interventions” with descriptions of their suboptimality throughout the revised and relabelled manuscript.*

*We have also sought to more clearly explain the rationale for robust interventions. We now introduce the reader to the possibility that due to imperfect information and/or application of NPIs, it may not be possibly to introduce a perfectly optimal NPI in practice. This is also now followed by an explicit description of the rationale of robust (but suboptimal) interventions, which are pragmatic reductions to Imax and Ic(tmax). Following this section, we now clearly define this alternative intervention strategy as “robust interventions”. These clarifications were made on page 2-3, line 58-72 in the Introduction.*

*We agree with the reviewer that optimality is defined in this study explicitly through maximising reductions to Imax or Ic(tmax). While it is beyond the scope of this paper to integrate the practical considerations of implementing public health policy into the model, we hope that by presenting the concept of robust interventions, we can encourage the reader to consider the practical limitations of theoretically optimal strategies and reflect on the idea of optimality within models. However, to further highlight the practical limitations of optimal interventions, we have added references to “theoretically optimal” interventions in the Discussion, rather than just “optimal”.*

* Regarding the statement “We note that for a single time limited intervention, the most effective suboptimal strategy to minimise Imax and Ic(tmax) can be achieved by intervening stronger and for longer than what is considered optimal”, why would intervening maximally not be “optimal” in this framework? Of course, in reality there is a trade-off between reduction in contact patterns and disruption to society but that trade-off is not in this model.

*We agree with the reviewer that the original sentence lacks clarity and we have now sought to further explain the unintuitive nature of optimal interventions in more granular detail. In particular, we note that all NPIs strategies were considered in the context of a time-limited intervention. As the reviewer highlighted, although this was not an explicit model feature, it was considered as part of our rationale to exclude indefinite interventions from the sensitivity analysis (tdur = ∞) due to the potential for unfeasible, extreme societal disruptions.*

*Therefore, even with interventions considered “maximal” in magnitude or duration, we note a severe rebound in prevalence following the cessation of the intervention, primarily due to the significant maintenance of population susceptibility during the intervention. It is for this reason that “maximal” time-limited interventions may result in suboptimal reductions to Imax and Ic(tmax).*

*Additionally, we note that for certain NPI scenarios (scenario 3 and 4) with a specific timing of the maximal reduction to transmission (cmin), increasing the tdur past the optimal value may “stretch” the timing of cmin past the epidemic peak. Therefore, even with theoretically maximal interventions (tdur = ∞) this may still result in suboptimal reductions to Imax. However, we note that if considered in the context of robust, but suboptimal interventions, maximising the intervention magnitude and duration can be considered the most efficacious robust strategy that is the least sensitive to implementation error.*

*We have sought to explain these concepts to the reader in more explicit detail, line 358-386 on page 11-12 in the Discussion. We also hope that by using descriptive examples of the unintuitive nature of “maximal” interventions in the Discussion, we have clarified this potentially misleading sentence highlighted by the reviewer.*

* However, the authors are clearly not trying to be realistic, anyway. They dismiss use of a more-realistic SEIR model on the grounds that they don’t really care about timing – despite the fact that timing is clearly critical in reality.

*We agree that it is important to consider the implications of a more realistic SEIR model structure and the inclusion of an incubation/exposed (non-infectious) period on the optimal timings and results obtained in the main results. We have therefore expanded the Discussion and the SEIR supplementary figure (Figure S17) to further reflect on the differences in optimal parameter space identified between SEIR and SIR model structures. We note that these reflections can be found in the Discussion, line 397-416 on page 13.*

*The use of an SEIR framework shifted the timing of the optimal parameter space to a later trigger point (Figure S17). However, we note that the qualitative pattern of the optimal parameter space remains unchanged relative to the original sensitivity analysis (Figure 2, 3). We note that if the aim of the study was to identify “realistic timings” for the optimal parameter space, factors additional to the Exposed compartment would also likely have to be modelled. This includes reporting or intervention implementation delays and heterogeneities in population structure, which would additionally alter the optimal parameter space and timings. The aim of this study is to serve as an exploratory analysis into the feasibility of NPI optimisation and to qualitatively describe the existence of optimal parameter spaces, rather than to predict or forecast the exact timing of the optimal parameter space. This point has been reinforced in the Discussion section on page 13, line 410-416. We note that the use of an SIR model structure can be justified in this case, with the simpler model framework capable of reproducing the qualitative patterns observed with a more realistic SEIR model.*

*We agree with the editor that an SEIIR or SIIR model (with an asymptomatic or symptomatic infectious state) is also a more realistic model framework to exactly model the dynamics of COVID-19 transmission. However, we note that use of an SIR model does not exclude the possibility of asymptomatic and symptomatic transmission, rather it does not distinguish between them and assumes a generalisability of transmission rates for these two states. We note that this model assumption should also be contextualised with the current uncertainty regarding the relative infectious potential of those who are either asymptomatically or symptomatically infectious, which may necessitate the use of simplifying assumptions. Therefore, a decision was made to aggregate transmission into a single I state.*

* Minor comment: p4 (of the pdf) line 22(ish): “economic” not “economical”.

*We thank the reviewer for pointing out this error. This has been corrected in the revised manuscript.*

**Referee 2**  
  
Comments to Author(s)

* This paper considers a SIR model framework to investigate how the peak prevalence and attack rate vary with the duration, magnitude and trigger point of five different non-pharmaceutical intervention scenarios. The baseline model parameter values are chosen to simulate the UK COVID-19 outbreak.  This paper demonstrates the use of models to identify regions of parameter space where peak prevalence or attack rate are minimised.  While the authors stress that the aim of the paper is not to provide specific policy advice for COVID-19 control, the conclusions of this paper have clear policy implications. In particular they show that the optimal intervention parameter values are very sensitive to model parameters. Therefore, sub-optimal strategies (earlier or stronger measures than would be optimal) are more robust to uncertainty in model parameters. This is particularly important in the case of an outbreak of a novel virus, where there is great uncertainty surrounding model parameter values
* I thought this was a very well written paper with thorough analysis, which support the findings and conclusions of the paper. I therefore have only a few comments, as well as a couple of minor typos.
* Page 8, line 38 – the authors say that the intervention parameters were optimised to minimise two outcomes (peak prevalence and attack rate). From the results it appears these parameter values are optimised to minimise the peak prevalence or attack rate, rather than minimising both simultaneously. Please can the authors clarify this.

*We thank the reviewer for identifying this. We have corrected this sentence to reflect that the analysis was to optimise the peak prevalence OR the attack rate, rather than AND. We note that after this was identified, we recognized similar instances elsewhere in the manuscript and these have all been amended in the revised version.*

* I appreciate that investigation of the impact of waning/partial immunity is beyond the scope of this study. However, it seems that these results depend significantly on the assumption of lasting immunity. Since there is great uncertainty over whether or not there is long lasting immunity to COVID-19 I think it is important that the authors mention how they think waning immunity would affect the results presented in terms of the optimal parameter space.

*We agree that it is important to reflect on the impact of waning immunity on this study. We have therefore altered the Discussion, line 400-416 on page 13, to include references to a new supplementary sensitivity analysis performed using an SIRS model with varying average durations spent in the recovered/immune compartment: 3, 6 and 12 months (Figure S18).*

*We identified that the optimal parameter space to control the peak prevalence (Imax) is qualitatively unchanged relative to the original SIR model (Figure 2, 3). We note that the initial epidemic wave has the potential for a much greater prevalence compared to subsequent epidemic waves in both SIR and SIRS model, with control of this initial wave greatly determining Imax. Therefore similar optimal parameters spaces to control Imax was observed for both SIR and SIRS model structures.*

*However, the use of a SIRS model altered the optimal parameter space to minimise Ic(tmax) relative to the original SIR analysis (Figure 2-3). Large increases in the attack rate were also observed across the observed parameter spaces, due to the replenishment of susceptibles and the long-term endemicity of COVID-19. Due to this discrepancy in optimal parameter space across model structures, we have highlighted in the Discussion, line 410-416, page 13, that care should be taken when interpreting the long-term dynamics of the model analysis with regards to the attack rate, especially due to the current uncertainty regarding the presence of long-term immunity following SARS-COV-2 infection.*

* This paper has focused on COVID-19, however they essentially provide a framework for considering optimal timing and duration of non-pharmaceutical interventions, which could equally well be applied to other outbreak scenarios. In particular I think it might increase the longer-term impact of the work if the authors briefly mention how this framework could be applied in the case of future pandemics.

*We thank the reviewer for pointing out the potential generalisability of the modelling framework used by this study. We have added a section to our Discussion, line 438-443 on page 14, discussing the applicability of this sensitivity analysis driven study to other viral immunising infections.*

* Figures 1B and S16B are not readable in black and white
* Page 17 line 35 – Figure S26 should be Figure S16
* Page 17 line 51 – Figure S27 should be Figure S17

*We thank the reviewer for identifying these errors and the lack of colour accessibility with regard to Figures 1B and S16B. We have now amended these figures, labelling and introducing a new colour scheme to increase accessibility to those who are visually impaired or viewing in black and white.*

**References**

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2. Morris DH, Rossine FW, Plotkin JB, Levin SA. Optimal, near-optimal, and robust epidemic control. *arXiv*. [Preprint]. 2020. Available from: arXiv:200402209.

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4. Miclo L, Spiro D, Weibull J. Optimal epidemic suppression under an ICU constraint. *arXiv*. [Preprint]. 2020. Available from: arXiv:200501327.

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