Dear Yi,

Thank you so much for your feedback, which has greatly contributed to improve the quality of my thesis. In the following, I shall address your comments (in blue), by outlining where changes have been made within the thesis.

Sincerely,

Marco Saldutti

Chapter 1

I like the introduction, especially the first section. It is up-to-date, sets out the issues, and motivates the work, e.g., reduce energy cost, in a clear and simple fashion, and supplements with a few detailed numbers. My criticism is that the transition from the first to the second section is not apparent. I would like to see a few more arguments of why targeting the rate equations of PhC lasers is natural and essential. Section 2 covers general semiconductor laser characteristics, e.g., threshold, output power, 3-dB bandwidth, etc., which are organized in a way inconsistent with section 1. Also, since the laser model is quite general, with the only "PhC specialty" perhaps being the small active (and mode) volume, it would be more appropriate to name section 2 as micro- or nano- (cavity) lasers instead of "PhC lasers", with relative merits for this choice of addressing challenges in optical links. In the end, the candidate could highlight the validity and limitations of the model when applying for PhC lasers. By doing so, I think, it will naturally bring out the motivations for establishing more sophisticated models as presented in the following chapters.

The title of Sec. 1.2 has been changed into "Rate equation analysis of microcavity lasers". In the beginning of Sec. 1.2, a paragraph has been added serving as a bridge between Sec. 1.1 and Sec. 1.2. In the end of this paragraph, reference to Sec. 1.3 for detailed motivations is made explicitly.

Chapter 2

This is my favorite chapter. Centered on the space harmonics of Bloch waves, the candidate well illustrates the physical picture of the transition from the FP-like to the DFB-like resonant mode as the cavity length increases and the wavenumber approaches the band edge. The candidate also does a great job of linking the interference between the Fourier components of harmonics to the radiation loss of the PhC cavity and providing an intriguing insight of disorder-induced disruption of side lobe destructive interference within the light cone. I agree that these interesting issues need more investigation. I would have liked to see some performance as the LN cavity becomes shorter (L3 cavity, for example), i.e., how large the simple semi-analytical approach the candidate uses for the analysis can deviate from the full numerical one for even smaller nanocavities. Otherwise, this is a very nice chapter.

In Fig. 2.16, I have added a data point for the L4 cavity. Regarding the L3 cavity, the resonance frequency would unfortunately fall outside the range of frequencies of available MPB simulations. This is the reason why I could not add a data point for the L3 cavity as well.

In addition, for the sake of completeness, I have added further comments in the end of Sec. 2.2.2 on the physics of b_{-1}approaching to b_0 as the wavenumber moves towards the band edge (see Eq. 2.6). In short, this signifies a transition from a travelling wave to a standing wave. These comments will serve as a basis to further address your feedback on Chapter 3.

Chapter 3

Thanks to the thorough investigations, I enjoyed very much the way the candidate presented the theoretical models. The candidate well describes many interesting phenomena associated with the slow-light and strongly supports them with precise mathematical equations. However, I would have liked to get some more physical pictures.

For example, what is the physics of coinciding the self and cross-coupling coefficients at the band edge? What does this imply (e.g., a transition from a moving wave to a standing wave)?

In the end of Sec.3.2.1, I have added a detailed, analytical derivation showing that the physics of coinciding self- and cross-coupling coefficients at the band edge is indeed a transition from a moving to a standing wave. These considerations offer a simple and intuitive understanding of the results shown in Fig. 3.13.

What governs the oscillation period shown in Figs. 3.26-3.28?

In Sec.3.5.1, just before the beginning of the subsection “Real refractive index perturbation”, I have clarified that the period of the spatial oscillations of the forward- and backward-propagating power (Fig.3.28-3.29) is determined by the effective detuning. This is the reason why, for instance, no oscillations are seen in Fig.3.26, as now clarified in the subsection “Real refractive index perturbation”. The oscillations in Fig.3.28-3.29 originate from the beating between the forward- and backward Bloch modes of the perturbed waveguide. Regarding Fig.3.27, the oscillations are, again, controlled by the effective propagation constant, with the maxima corresponding to the condition that $\beta_{\text{eff}}L/\pi$ is an integer. These maxima can be understood as a form of tunneling.

Since the peaks observed in the gain spectrum correspond to the poles of the scattering matrix, reflecting the onset of lasing, then why are they still finite?

In Sec.3.6, I have clarified that the finite height of the peaks in Fig.3.32 is simply due to the limited resolution in the values of wavelength and modal gain which I have considered. To further elaborate on this point, I have also added other comments and figures in Sec. 4.4.2 (in Chapter 4) on feedback-sustained lasing. As shown therein, the position of the peaks in the transmission and reflection spectra is consistent with the numerical solution of the oscillation condition $r_+ r_- e^{+2i\lambda_+ L} = 1$. This outlines that the peaks in the reflection and transmission spectra do correspond to the onset of feedback-sustained lasing.

Chapter 4

I also like this chapter. The introduction is complete and extensive, although with some overlaps with the other chapters. The candidate clearly describes the laser model by combining the slow-light effect discussed in chapter 3 with boundary conditions brought by the laser mirrors. Criteria of simplifying the model into the FP case have been well discussed, i.e., when the gain is small enough, or the oscillation frequency is sufficiently detuned from the band edge.

But I would have liked to see some exact numbers, e.g., estimations based on standard InP quantum well PhC L7 lasers. Say if restricting the deviation of the results of the simplified FP model from the complete one to less < 10%, what should the detuning be? It is intriguing to find the resonances in the internal reflection coefficient as a function of wavenumber by tuning the modal gain and left mirror phase. I agree that the mechanisms of these resonances, depending on the interplay between the distributed feedback and gain, are complicated. Still, it would be perfect if the candidate can provide a more intuitive explanation about the physical origins, e.g., the oscillation period and the difference between the major resonance and minor ones, as exemplified in Fig. 4.8.

Unfortunately, it does not seem to be possible to come up with a better physical explanation for the oscillation period and various resonances of r_{int}. Similarly, it is not possible to identify a simple criterion to simplify the full model. However, in Sec. 4.3.2 I have added and discussed Fig.4.9. The figure shows conditions under which the relative error in the magnitude of r_{int} is smaller than 10%. The figure highlights not only the impact of detuning from the band edge and modal gain, but also the impact of the cavity length and linewidth enhancement factor.

The discussions about the M0 mode close to the band edge are interesting. However, the evidence supporting the absence of such mode in LN PhC cavities, due to the nonzero mirror phase (of r_int) as claimed by the candidate, is not convincing enough. One may argue that by changing the mirror phase, e.g., shifting the mirror holes, it should be possible to observe the M0 mode, which is, however, not the case experimentally.

It should be emphasized that the reason for the absence of lasing close to the band edge in the presence of distributed feedback is not simply the fact that the phase condition cannot be fulfilled. Instead, the reason is the impossibility to simultaneously and efficiently fulfil the phase and magnitude condition, owing to their strong coupling caused by the distributed feedback. In the revised manuscript, this point is now thoroughly discussed in Sec.4.4 and further supported by Fig. 4.14 and Fig. 4.15. In these figures, it is shown how, in the presence of distributed feedback, lasing close to the band edge cannot simply be achieved by tuning the mirror phase. It is also noted, though, that in practice disorder due to unavoidable fabrication imperfections would also impact the possibility of lasing in the slow-light region. From an experimental point of view, distinguishing between the detrimental effects of disorder and gain-induced distributed feedback would probably not be trivial.

Chapter 5

This chapter further extends the model by including the field and carrier dynamics. The field rate equation is based on the conventional transmission-line model, so the fast dynamics is limited by the round-trip time resolution, in contrast to multi-section models. This may not be a significant issue for ordinary FP laser since its round-trip time is usually very short on fs, which is, however, not the case if working deep into the slow-light regime of the PhC laser (DFB case). Therefore, it would be better if the candidate can provide some discussions in this respect, preferably by comparing it with the multi-section model if available.

Just before Sec.5.1.1, a detailed discussion has been added on limitations of the model for resolving large-signal and ultrafast dynamics. It is also explained why a multi-section scheme as proposed in [1] (see reference below) cannot be applied if one accounts for slow-light and gain-induced distributed feedback. In this case, one should come up with other discretization schemes, a task which is beyond the scope of this thesis (but could be the subject of future work). Therefore, it has been specified that the model as presented in Chapter 5 is only applicable to determine the small-signal response and, in general, dynamics with limited deviation from the stationary solution.

[1] T. S. Rasmussen et al., SPIE, 2019

Chapter 6

I am impressed by the well-established multi-physics laser model and wonder if this can also be applied for electrically injected nanolasers, e.g., lateral current injection scheme.

This is a very interesting, open issue. It would definitely require further investigations, to assess if and how electronic transport in the presence of a lateral injection scheme can be approximated with a one-dimensional transport model.