**Reviewer I:**

The paper presents an interesting study of the confinement effects on the structure and stability of the blue phases of chiral liquid crystals. The paper combines experiments with high-level numerical simulations of the structures and properties. An excellent feature is the application of confocal laser scanning microscopy. The paper is richly illustrated and well written.

Minor comments:

1. *The references are not in order.*

**Response:** We thank the Referee. The corrections have been implemented in the revised manuscript.

1. *It is not clear how Figure 3e could illustrate the reduction in phase transition temperatures (page 5, left column), as it shows a simulated structure. Apparently, the authors meant to refer to Fig.2e*

**Response:** We thank the Referee for this comment. The figure number is corrected in the revised version.

1. *The funding source is duplicated and there is a remnant of a template in Funding Sources.*

**Response:** We are grateful to the Referee for this observation. The comment was addressed in the revised manuscript.

**Reviewer II:**

Sadati et al. report the switching between blue phase I (BP-I) and blue phase II (BP-II) as a result of temperature changes to the system, where the chiral liquid crystal is confined within spherical droplets, and the blue phase is made more stable through photopolymerization, as established in bulk films in the previous work of Castles. The switch between different blue phases found in their system has not been achieved experimentally before and is a significant finding for the fields of liquid crystals and self-assembly, especially since the disclinations of the blue phases are populated by a polymerized material. The work is fascinating, and the findings should be of interest to the broad ACS Nano readership. However, the following points should be addressed before I can fully support the publication of this work:

1. *The authors use super-resolution confocal laser scanning microscopy to capture high resolution micrographs of the blue phases. Yet, the system conditions necessary to successfully perform such high-resolution confocal measurements are usually stringent, where index matching of the surrounding solvent is necessary to prevent noise from scattering, and a precise thickness of the coverslip and the type of glass used are all important details. No such details are mentioned in Methods, which makes me wonder exactly how their imaging works… The authors note that their samples are label-free. Does this mean that their confocal images are just a result of scattering of the confocal laser light, since the structures of the material have periodicities that* *are on the order of the wavelength of visible light…? Discussion of the their imaging technique is necessary, especially if the technique is relatively new and has only been used once before (as the authors reported on pg. 6). Does their captured data change if the index of the surrounding water is altered? Furthermore, many of the experimental images also lack scale-bars. If the authors are obtaining high-resolution images, then scale-bars for the magnified images is needed. Additionally, which laser wavelength is used for which images? Without these details, it would be difficult for others to reproduce their work with the information presented in the manuscript…*

**Response:** We agree with the Referee and are grateful for this observation. The Super-Resolution Confocal Laser Scanning Microscopy (CLSM) was performed on the label-free polymer-stabilized BP droplet dispersed in 3% PVA aqueous solution. We added 5 wt% Borax to the aqueous solution to crosslink the PVA and prevent movement of the polymerized droplets during the confocal imaging process. The sample cell was covered with a cover glass (NO. 1) and a 100X oil objective lens was used to image the periodic structure of the BPs confined in the spherical droplets at room temperature. For imaging, the label-free polymer-stabilized BP droplet was illuminated with He-Ne laser at the wavelengths of 543 nm and 633 nm, as well as Argon laser at the wavelengths of 458, 488, and 514 nm. To observe the periodic lattice structures, the CLSM was operated in the reflection mode with a narrow pinhole. When the periodic lattice structure of BPs matches the wavelength of the laser beams, it leads to strong Bragg reflection pattern, which was detected by a photomultiplier tube. He-Ne laser at the wavelengths of 543 nm and Argon laser at the wavelengths of 458 match the internal periodic structure of BPI and BPII, respectively leading to strong reflections. Following the Referee’s comments, the main text has been revised; more details have been provided for the CLSM technique and the wavelengths and scale bars are included in the revised versions.

*Does their captured data change if the index of the surrounding water is altered?* This is indeed a very interesting question. It can definitely affect the scattering resolution. However, to maintain the anchoring condition on the surface of the polymerized droplets, we have not attempted this experiment yet and will certainly consider it in a future study.

1. *On pg. 5, the authors also describe the energy contribution from polymerization of the monomers within the disclination lines. They use this to determine that the phase transition temperatures of their system is size-dependent, due to confinement within droplets. Since this is one of the major findings of this work, the assumptions used for the energy contribution from polymerization is necessary within the main text, instead of only placed within supplemental materials. I do not doubt their conclusion from this calculation, but one assumption is that the polymerization is considered only to the extent of polymer cylinders, when indeed polymers must also be populating the junctions. Their energetic contribution from the junctions should have an impact on whether it is possible for BP-I and BP-II to switch between one another (detailed further in the point 3).*

**Response:** We agree with the Referee, we have moved from the SI to the main manuscript and also expanded the section related to the energy contribution of the monomer aggregation.

1. *The most exciting discovery of this work is that, for “larger droplets,” the authors find a reversible transformation between BP-I and BP-II, even though the disclination lines are polymerized. This is quite incredible, if true. The polymerization should result in some elasticity in the disclination network. Does the shift from BP-I to BP-II require topological changes to the disclination network? That is, are tears in the network structure necessary to shift from BP-I to BP-II? If so, that would make this shift extremely mysterious, as surely temperature changes alone should not be enough to create tears, if the monomers are truly cross-linked… If tears are not needed, are some of the disclination lines forced to buckle and fold up? If this is the case, then these folded areas would likely introduce imperfections in the crystal structure, that could also show up in their confocal imagining at high enough resolutions… Further discussion of the possible mechanisms behind this shift is needed, and would further highlight the significance of their findings.*

**Response:** We acknowledge the Referee for rising these observations. In a previous work, we showed the martensitc nature of the BPI-BPII transformation (Li et al., PNAS, 114 (2017), Jin et al., Sci. Adv. 6, eaay5986 (2020)), i.e., there is no diffusion of the double twisted cylinders (DTCs) which are the building units of the BP crystals. In the course of such crystal-crystal transformation there is a reconfiguration of DTCs which is accompanied by smooth changes of the disclination lines as can be appreciated in the following video (0:45):

<https://movie-usa.glencoesoftware.com/video/10.1073/pnas.1711207114/video-3>

With respect to the topological changes of the disclination lines, during the crystal-crystal transformation the disclination lines of the BPI connect to produce a BPII and vice versa.

Now, this movie corresponds to a polymer-free BP but it illustrates how the topological changes of the disclination lines are possible when considering polymerized and flexible disclination lines. ~~In previous works, the polymer template of the BP is frozen and prevents any crystal transformations~~.

We consider several mechanisms that may facilitate the rearrangement of BP arrays of disclination lines. First, we recall that the UV irradiation occurs under conditions favorable to BPI formation. Computer simulations show that the disclination lines from BPI change shape, touch and recombine to produce the BPII defect structure. This can be achieved if the polymers follow the defect rearrangement but do not produce new cross-links at the recombination points: this would leave the BPII defect structure free to revert back to the earlier configuration without tearing. Second, polymer structures formed by UV irradiation have been characterized as semi-interpenetrated polymer networks (semi-IPN) [Joo 2007, Athawale 2003]. For such networks, the conditions of irradiation can affect the ability of polymer to form a fully defect-spanning scaffold: for instance, for long irradiation conditions it has been shown by Johansson et al (2020) that semi-IPN display a decrease in their elastic moduli, which they attribute to chain scission. In addition, semi-IPN can display phase incompatibilities between their components, with one of them being more mobile than the other [Athawale 2003]. This could lead to a situation where the more mobile component could migrate to the disclination-line recombination zones, thus reducing the free energy of the whole assembly without tearing. These possible mechanisms may be explored and tested experimentally in subsequent work.

We have included a paragraph in the manuscript where we refer to the previous movie to explain possible mechanisms on how the polymerized networks of the BPs can change their topology in a reversible way.

1. *On page 3, the authors write, “… when low chirality liquid crystals are confined within microdroplets, no droplet size-dependency has been reported on the optical properties of the polymerized LC droplets and instead the reflected color changes have been achieved by [tuning] the amount of chiral dopant.” This statement is too simplified, as it only considers low chirality droplets with planar anchoring, where defects from planar droplet geometries are highly localized and do not span the system. Comparing only this simple planar arrangement to BP’s that are filled with defects is too basic of a comparison, especially when past work has indeed demonstrated that low-chirality cholesterics have altered defect structures with changes in their geometrical confinement. This has been seen for cholesteric droplets with homeotropic anchoring (see Sec, Copar, Zumer, Nature Communications, 2014; Posnjak, Copar, Musevic, Scientific Reports, 2016; amongst others) as well as for cholesteric double emulsions with homeotropic anchoring (see Tran, et al., Phys. Rev. X, 2017; Durey, et al., Soft Matter, 2020; Tran, et al., ACS Nano, 2020). The comparison would be more accurate and complete when considering, more broadly, cholesterics with defects that change their structure with system size, especially systems where defects span the system (i.e., when they have homeotropic anchoring). This statement should be modified to reflect this, and studies of cholesterics emulsions with more diverse defects should also be cited.*

**Response:** We thank the Referee for their very useful comments. The recent citations on the effect of confinement on the defect structures of chiral liquid crystals have been included in the revised manuscript.

1. *At the end of pg. 3, the authors write “With longer UV exposures, the centers of the distributions of the droplets… were slightly shifted to larger diameters.” The data for this should be included, either within Fig. 2 or in supplementary materials.*

**Response:** We thank the Referee for this comment. The data for longer UV exposure time, was included in the Supporting Information (Fig. S3).

1. *Twice in the manuscript, the authors state that simulation results are in “excellent agreement” with the experimental data. “Excellent” is too strong of a word, especially since the authors do not state a quantitative comparison from simulations to experimental data. It is more accurate to state that the simulations are in “qualitative agreement” instead.*

**Response:** We agree with the Referee: the statements has been modified in the revised manuscript.