In this manuscript, the authors consider a two-dimensional fermionic superfluid with a spin-orbit coupling and the Zeeman interaction. Specifically, they regard the corresponding four-by-four Nambu kernel as the Hamiltonian of a four-level quantum mechanical system and solve its time dependence by a method called the dynamical matrix approach. From the solution, they numerically obtained the occupation probabilities of the four levels, which are shown in Figs. 3-11 as functions of the parameters. Although the original motivation of using interference among different quantum levels to facilitate gate operations, etc. seems potentially very interesting, the conclusions drawn from the main results (presented in section 4) are mostly speculative and remain at the level of mere observations. Also, the presentation of the manuscript is poor, and all these make me have a rather negative opinion on the manuscript. I believe that even if the manuscript will finally be accepted, the authors should make at least substantial revisions before.

Here are points which I think are worth serious consideration.

1. About the title: “A Non-Adiabatic SU(4) Symmetry Approach”. As far as I understand, “SU(4)” is just a buzz word and plays no essential role in deriving the main results. If the word “SU(4)” necessary, the authors should explain why SU(4) is crucial in their approach.

2. A lot of acronyms are used in the manuscript and I spent a hard time to decipher them. I recommend the authors minimizing the use of acronyms; when the terminologies appear only a few times or they are short enough, using acronyms for them just makes the manuscript harder to understand (we need to look for their definitions in the text). I do not think they need to use acronyms for words like “non-adiabatic” (NA), “quantum computing” (QC), “chemical potential” (CP), and “pair potential” (PP) as they are simple enough.

3. A typo. The $\epsilon_\sigma(k)$ [in (2)] should read as: ϵ_k [the Zeeman interaction is not included in $\epsilon_\sigma(k)$].

4. Misleading notations. Just above eq. (5), the authors define the four-component Nambu spinor as a ket $|\psi\rangle$, whereas it is in fact a vector of field operators. Also, I suspect that the third and fourth components should be interchanged. Below eq. (5), they define a new quantity (total fermion density): $n_\sigma = n_\uparrow + n_\downarrow$. However, they use σ to denote the spin components (↑/↓) and the subscript σ should be removed to avoid confusion. In eq. (5), they explicitly write the time dependence on the left-hand side while they drop it on the right-hand side. To make the time-dependent parameters clear, I recommend the authors writing, e.g., $\epsilon_{k\sigma} - \mu \to \epsilon_{k\sigma}(t) - \mu(t)$.

5. In section 3, the authors (implicitly) interpret the Bogoliubov-de Gennes (BdG) “Hamiltonian” (5) as the actual Hamiltonian of a four-level quantum mechanical system to investigate its time-dependence. However, the BdG Hamiltonian eq. (5) is just a *kernel* of the second-quantized (many-body) Hamiltonian, and its behavior as a four-level quantum mechanics has little to do with the dynamics of the original many-body fermionic superfluid (only when there is a single quasi-particle with momentum k, the BdG “Hamiltonian”’ becomes the actual Hamiltonian of the superfluid). The authors should clarify what situation they assume [or what the “wave function” φ in (9) actually means] in relating their analysis to physics of a 2D fermionic superfluid.

6. A related point. Just below eq. (15), they assume an initial condition for the four-level system without giving the definition of the states $|00\rangle, \ldots, |11\rangle$ in the language of the fermionic superfluid.

7. In section 3.1, the authors consider the case of linear sweep and discuss two limiting cases: $\epsilon_k - \mu(t) \to 0$ and $\epsilon_k - \mu(t) \gg 0$. However, ϵ_k is k-dependent and the conditions do not make any sense without specifying the value of k. They should clearly specify what value of k they assume here.

8. On the whole, the figure captions are poor. At least quick summaries of the main observations must be given in the captions (even if the full explanations are found in the main text). Other problems are:

   - The quantity r in Fig. 1 is not defined anywhere in the text.

- Clearly, all the plots are obtained for specific fixed values of t. However, the values are not shown.
- The initial conditions are not specified.

9. Two unnumbered figures appear between Fig. 1 and 2 (in page 12).

10. When the authors discuss the case of periodic sweep, they point out the occurrence of "a two-stage double-passage process" seen in Fig. 1b. However, they do not give any explanation which part of Fig. 1b can be interpreted as the above process.

11. Fix an equation overflow in page 10.

12. The figures 3-11 and the corresponding discussions in section 4 constitute the main part of the manuscript. On the whole, the physical interpretation of the results remains speculative or superficial lacking reasonable physical explanations, which makes this part far from satisfactory. I just list some (not all) examples:

    - In page 12, the authors claim: "The ripple-like interference patterns in Fig.3 indicate that the particles in the system exhibit wave-like behavior." quoting the color-map plot of the occupation probabilities of the level-1 and 2. However, what they plot is the parameter-dependence (the Zeeman field and the spin-orbit coupling, here) of the probabilities and is not the real-space (or momentum-space) profile which might reveal the wave nature. (As they work in the momentum-space, the wave nature, if any, must be seen in the k-space behavior.) They should give more detailed arguments why they arrive at this conclusion, which is not at all obvious to me, from the plot.
    - In page 14, they say: "At high frequencies ($\tilde{\omega} < 1$), individual multiphoton resonances are visible,...", whereas I could not figure out where I can identify in Fig. 4 the signature of the multiphoton resonances. They should add some arguments on how the multiphoton resonances lead to the structure seen in Fig. 4.
    - In page 16, they conclude that the ring-shaped structure seen in Figs. 5-7 is a manifestation of the Aharonov-Bohm (AB) effect and say: "This is a significant breakthrough in this work, as such phenomena have never been observed before in Fermi gases." However, I do not think the logic connecting this ring-like structure and the AB effect is obvious, as the AB effect needs non-zero gauge potential which is zero in the present setting (if it were non-zero, it would certainly affect ϵ_k). They should elaborate on this point.

      For the same reason, the discussion of the Landau level at the end of page 16 is not quite correct in the present setting (in which only the Zeeman field is taken into account).
    - In page 17, the authors interpret the results in Fig. 6 as a consequence of the resonance among different energy levels without giving any concrete physical arguments. If they argue that this sort of resonance helps us implement, e.g., gate operations, they should at least give a simple (semi-quantitative) explanation how the resonance among the levels affects the interference patterns.
    - I do not understand what "topological Fermi ring" in page 20 stands for.

    The authors must revise this section substantially.