Review of: "Synthesis of Nickel Nanoparticles Using Ionic Liquid-Based Extract from Amaranthus viridis and Their Antibacterial Activity"

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Potential competing interests: No potential competing interests to declare.

The work by Ullah et al. entitled "Synthesis of Nickel Nanoparticles Using Ionic Liquid-Based Extract from Amaranthus viridis and Their Antibacterial Activity" discusses the formation of biogenic nickel nanoparticles, their characterization, and antibacterial activity.

After reviewing the manuscript, I consider that the work cannot be accepted for publication due to various issues related to the manuscript that are described below:

1. The English used for the presentation of this work is poor, with grammatical errors and paraphrasing errors, which makes it difficult to understand the work. For example, in the section on manipulation and seeding of bacteria, the authors indicate the following: “Petri plates were evenly coated with nutrient agar before being infected with Staphylococcus aureus, Aeromonas hydrophilia, and Escherichia coli”. This is very serious, as this is a very serious grammatical and technical error, since the bacteria are sown in petri dishes that previously have a specific culture medium; the word infect cannot be used in this sense.

2. The introduction is very poorly described. Firstly, it does not give a well-described background on why to use nickel nanoparticles instead of another metal. On the other hand, two strains of gram-positive bacteria and one gram-negative are used to test the antibacterial capacity of the biogenic nanoparticles. Given this, the authors do not indicate the justification for why they use these 3 strains, nor the clinical importance of targeting these 3 strains specifically. Nor do they indicate what new knowledge will be obtained with the creation of these biogenic nanoparticles using amaranth extracts and how the problem of pharmacological resistance of bacteria will be solved. Have these bacteria you used been evaluated for resistance to conventional antibiotics? The authors do not give details about it either.

3. Material and Methods section: Due to the concentration of metal salts being of utmost importance in a biosynthesis, since it determines the concentration, size, and morphology in relation to the concentration of reducing agent, the authors of this work do not indicate the final amount in volume of the stock solution to carry out the biosynthesis. The physicochemical characterization tests are very poorly described. On the other hand, the antimicrobial analysis does not describe the concentrations of nanomaterials implemented in the sensor disks used to determine the susceptibility of bacteria. Nor is any analysis of mean inhibitory concentration on bacterial growth described.

4. In the results section, regarding the UV-Vis spectroscopy analysis (3.1), issues of antimicrobial activity are described, but not even Figure 1 is described. Could any parameter be observed that would indicate that there is formation of nanomaterials? For example, surface plasmon resonance? A color change in solution that would indicate reduction from metal to nanometal? None of this is described. In the XRD analysis, the particle size is indicated using these analyses in conjunction with the formula, but what is this? Since it is not described in the manuscript, nor what parameters were used to predict the crystal size. There is no agreement between particle sizes, since the analysis done by SEM shows spherical or hemispherical particles in a size range of approximately 280 nm to 350 nm. But DLS analysis shows that there are particles with an average size of 23 nm. What is the real size of the nanoparticles formed? The authors indicate that the zeta potential value of the nanoparticles is around -41 mV; however, the graph shows that the curve is close to the value of 0 mV, which would indicate almost synergetic particles. Why present two different values by the authors? The antibacterial activity section is not clear. The figure does not manage to show the inhibition zones described by the authors in any of the concentrations used. Regarding this, the authors indicate that they used 10, 20, and 30% nanomaterials, but with respect to what? What was the real concentration of the nanomaterials implemented in these analyses? If the authors indicate 30% diluted of the stock solution of nanomaterials formed, this is a serious error made by the authors, since it is not actually known how much nanomaterials are in each dilution, and this parameter is of utmost importance to identify the real amount of nanoparticles that could exhibit antibacterial activity. Likewise, the authors do not indicate what the minimum inhibitory concentration of the nanomaterials used is.

5. The conclusions are very poorly described, since they only give a summary of the results that have been described previously. What new knowledge has been created with this application?

Due to everything described above, I recommend that this work cannot be accepted for publication in its current state.