

I am quite prepared to accept this paper provided the authors add some elements throughout the discussion of their results.

1) There are new books on organogelation that are more physics-oriented on this phenomenon. The authors should quote them:

Guenet, J.M. *Organogels: thermodynamics, structure, solvent role and properties*, **2016**, N.Y., Springer International Publishing

Weiss, R.G. Ed *Molecular Gels, Structure and Dynamics*, Monograph in Supramolecular Chemistry, **2018**, Royal Society of Chemistry, London

2) The problem of defining a gel. As we can see from figures 7, the morphologies of the different systems are not at all the same. Guenet in the above book has discussed in details a definition of a gel. The conclusion is that a gel is a network, and a network is “*a large system of lines, tubes, wires, etc. that cross one another or are connected with one another*” according to the definition given by any dictionary. Again, as further discussed in the above book what is needed is a near-1D crystallization to get eventually fibrillar systems that do obey the definition.

As a result, system in figures 7a, 7b, 7h, 7i do not comply to this definition they simply are 3D crystallization.

Now, the authors may argue that their rheological experiments always show $G' > G''$. As was shown by Collin et al. (*Soft Matter*, **2013**, 9, 2947) this can be deceiving because the explored range of frequency, especially within the low frequency range, is limited. Their experiments show that there is no modulus at infinite time, as required by a true rheological of a gel. Basically, each time they apply a new deformation, they retrieve the same value of G' , whereas this value should have increased.

This should be considered and discussed.

3) The Hansen parameter allows one only to decide whether the solvent is not too good, thus preventing crystallization, nor not too bad, thus preventing solubilization. It cannot predict whether the system will give 1D fibrils or 3D crystallization, which is what is eventually needed. Again, see the above book by Guenet.

Note that for polymers, scientists are rather using the Flory's parameter, which is experimentally obtained, rather than the Hansen parameter.

4) The gel formation and melting would be better studied by DSC. The inverted tube method is not very precise. Also, about the so-called “critical gelation concentration”, the authors should read the two above books above. As was also emphasized in a GELS article the T-C phase diagram by DSC is an important step in studying organogelation (R.G. Weiss, *Gels*, **2018**, 4, 2). This concentration depends upon the temperature at which it is measured. So, it is not a critical parameter in the physical sense. Only the temperature-concentration phase diagram tells you something about the system.