Review for "On the identifiability of interaction functions in systems of interacting particles"

Interacting particle systems naturally arise in various fields of science. It is often of interest to infer the interaction potential function. For such inference problem to be well-posed, it needs to be identifiable, which can be guaranteed by coercivity conditions. This manuscript (MS) defines coercivity conditions in several scenarios, and show that linear systems are coercive, and three particle systems are coercive if the interaction potential is polynomial-like. While these partial answers have some merits by themselves, I think the current version of MS is not suitable for publication in SPA. In below, I list some major issues that the authors can consider improving.

Math Rigor

The writing style in this MS seems not very "mathematical" or rigorous. Usually in math journals such as SPA, rigorous proofs need to be done carefully for theorems and other claims. This is not well done in this MS, as many arguments are conveyed through "semi-proofs".

For example, l-70 says "In the proving the consistency of the estimator,...." So is this part of a proof, or is it referring to an existing proof?

For another example, after (2.8) on page 5, it says "note that...

$$\langle Q_T h, g\rangle = \frac{1}{T}\int_0^T E[h(r_{12}, r_{13})\frac{\langle r_{12}, r_{13}\rangle}{|r_{12}||r_{13}|}dt]''$$

Q_T is defined through K_T in (2.7), which is a very long formula. How does the identity in above hold? I cannot see why this is true without some detail explanation.

As a last example, on page 11 there are 3 different "proofs". But I don't know what theorem or lemma are they referring to.

Problems such as these three appear in every page. The authors should consider making their statements and arguments rigorous and provide details.

On the other hand, these semi-proofs are quite tolerable and common in physics, nonlinear science or engineering literatures. The authors may want to consider submitting the MS to a different venue.

Identifiability

The title suggests this MS studies the identifiability of interaction functions. But identifiability is only discussed in Section 2.1 by some vague "proofs". In my opinion, identifiability is much more interesting than coerciveness. Is identifiability carefully proved in existing works like [11]? If yes, the associated theorems should be presented in the appendix. Otherwise, the authors should consider presenting the claim as a theorem. This result will be more significant than the other partial results.

The authors should also indicate/prove consequence of identifiability. That is, if the problem is identifiable, with what algorithm/method the true potential can be recovered.

The MS tries to argue coercive is important for identifiability. This again is done by some vague arguments after (2.4). The arguments need to be written more carefully.

Also it seems to me that coerciveness is only a sufficient condition. Why is (2.4) necessary to control h? Can't we define $\mathbb{E}\|\nabla J_h(X)\|^2$ as a norm for h? And by asking this norm going to zero, it seems that h also needs to go to zero? If this is true, all the discussion of coerciveness seem pointless to me.

Organization of settings/results

In most math papers, there are only 1 to 2 different problem settings. But this MS has four theorems, each has slightly different settings. (Again, many engineering papers contain several settings, so may be SPA is the wrong venue) For example, some results need Gaussian initialization, some need the covariance to follow technical assumption $cov(X_i) - cov(X_i, X_j) = \lambda_0 I_d$, some require the systems to be at equilibrium already. I understand the authors want to present the most general form of results, but these settings become very confusing. There is very little discussion of why they are appropriate assumptions. For example, is equilibrium a natural initialization? And if it is already natural, why do we consider other initialization? (I also believe by focusing on equilibrium state, you will only need one Q_t that is time independent, instead of 3 different ones, which cause additional confusion.)

Furthermore, the results are not organized. For example, it seems Theorem 4.12 gives stronger statements than Theorem 4.7, so why do you present the latter? Also, as an integral of Q_t, $\bar{Q}_t$ should be positive if Q_t is. So why is $\bar{Q}_t$ first introduced and analyzed, while the proof of Q_t refers to $\bar{Q}_t$?

As a summary, I prefer to read 1-2 main results and focus on understanding their proofs. Currently there are too many versions, and their proofs look largely the same. This is very confusing.

3 particles

I think it is fair to say that 3 particle is a quite restrictive setting. There should be explanation why extension to more particles is not doable. This is discussed in Remark 4.3. But Remark 4.3 is again very vague. What is “marginalization”? And what do you mean by ”except in the Gaussian case”? The authors may consider writing down the 4 particle scenario, and explain which terms prohibit the analysis.

Detailed comments

Here is a list of things I recommend changing. This is not an exhaustive list, since I fail to follow after page 7. Note that the line numbers are generated in a strange fashion for this MS, as multiple lines seem to be counted as one. So the line numbers in below are just indicating rough locations.

1. l24, “An exception” to the end of paragraph: the techdetails here are confusing. You haven’t shown what is your model yet, so I don’t understand what do you mean by “symmetric structure”.
2. l29, ”, and ” there is no comma here.
3. l31, ”we consider” seems better.
4. l37, identifiability is not defined here (or anywhere else)
5. (1.2) to the end of section 1. Usually introductions are not technical. They tend to give the background and intuition to attract further readings. Are there any physical intuition/literature related to (1.2) or (1.3)?
6. l61, the spaces in (H1) and (H2) are not defined. And why do you need finite moments up to power Nd in H2?
7. l63, what do you mean by ”the diffusion operator”, and how is it related to your system?
8. l65, exchangeability is used very often in this MS. There should be a definition, and some literature on its importance.
9. l67, ψ_{true}. Why do we switch from ϕ to ψ all of a sudden. And is $\phi = \psi_{true}$?
10. Ref [18] is a book, please give precise location of the reference.
11. Definition of $\mathcal{E}$, you defined $|\cdot|$ to be l_2 norm, now it is $\|\cdot\|$.
12. (2.3) The Dirac measure of writing is very ”physic” and may lead to confusion. Can you write it as a density?
13. l70-l75, this part should be formulated into a theorem or proposition. If it is already studied in [11], please write down the theorem.
14. l74, ”note that $I_{122} =$” what is U?
15. l74, ”Therefore, Eq. 2.4 is equivalent”, I don’t see the equivalence. Is it sufficiency instead? I also don’t understand the point of counting. Are you saying I_{123} (Not I(123) BTW) is significantly more than I_{122}, or I_{123} just need to be nonnegative? If I_{123} just needs to be nonnegative, why is (2.5) a strict inequality?
16. The r_{12} here is not defined. $C_{N,T}$ is independent of T.
17. ”Ensure the convergence of the estimator”, please show rigorous proofs.
18. P6, Prop 2.4, the ”operator Q_t associated with...”, such notion is not a common term, please define it.

19. $\Phi(r) = \frac{1}{2}\theta r^2$, please discuss the physic background of these potential.

20. Theorem 3.4, what is background of $\mathrm{cov}(X_i^0) - \mathrm{cov}(X_i^0, X_j^0) = \lambda_0 I_d$?

21. P11, there are three "Proofs" on this page. What are they referring to?

22. P19, usually you assign Appendix section section number A. Using "Theorem 5.6" in early part looks confusing, as I thought you have proved several new theorems in latter part of MS.