Reviewers' comments:

Reviewer #1 (Remarks to the Author):

Summary:

The authors develop a new network measure to study the robustness of complex networks against targeted attacks. Their approach is inspired by established concepts in quantum statistical physics such as the density matrix and the Von Neumann entropy and it builds up upon recent works that have introduced, for the first time, a quantum-like network density matrix (Ref. 16).

The main theoretical result of the present work is the introduction of a network centrality measure named “network entanglement” (NE). This measure is essentially the Von Neuman entropy difference between the original network and the sum of two subnetworks of the original one, obtained by detaching a node x and its incidence edges from the original network (eq. 6 and Fig. 1). In a way, the entanglement measures the degree of subadditivity of the entropy for these networks. It is important to note that, as the Von Neumann entropy, the network entanglement depends on the diffusion time \beta.

After studying some limiting cases for the \beta parameter, the authors introduce the NE of a single node and the average NE of all nodes. The network robustness is controlled by the value of the largest connected component (LCC) of the network, as it is customary in the literature.

To proceed with network dismantling the authors follow a three steps procedure: 1) They compute the average NE for all nodes at different \beta values to find the critical value \beta_c at which the average NE is minimum; 2) They rank each node according to the single-node-NE score at \beta_c from the highest to the lowest score; 3) they remove the nodes from the network, one by one, from the highest to the lowest score and compute the new value of LLC at each removal. The method is compared to classic centrality measures used in the literature such as degree, page rank, betweenness, etc…

Remarks:

The method and the overall presentation have a series of weakness that I address in the following.

1. The method suffers of slow computation speed for two main reasons. First of all, the authors have to find numerically the value of \beta_c for which the average NE is minimum. This requires an extensive exploration of the \beta values numerically, and for each \beta value, the average NE has to be computed. The computation of the average NE involves the computation of several partition functions: the partition function of the whole network and the partition function of the perturbed network of each node \Delta G’ (eq. 7). For large networks, especially with large average degree, this computation is extremely expensive and had to be done for each \beta value in the whole explored range to find the value of \beta_c for which the average NE is minimum.

Once \beta_c has been determined, the authors have to compute again the single-node-NE for each of the nodes in the network at \beta_c and each one of these calculations involves the computation of one partition function (eq. 7). As a result, the method seems highly time consuming. The authors comment on this problem towards the end of the manuscript, yet they omit giving a quantification of the issue. I believe the authors should add a plot of the computation time vs size of the networks for their methods and the different ones used for comparison.

2. The NE approach for network dismantling is compared to only very dated methods in the literature and most recent and performing approaches are left out. The network robustness and dismantling literature has recently been enriched by several outstanding methods. The authors cite some of this literature, but then they avoid comparing their approach with the most recent and performing ones. For examples, the CI method of Ref. 12 is cited but not used as a comparison, as well as the method of Ref. 13 which proved to outperformed Ref. 13 in several cases. Several other methods of network dismantling are not even cited and used as a benchmark, as Mugisha, S. and Zhou, H-J, Phys. Rev. E 94, 012305, (2016); Zdeborová, L., Zhang, P. and Zhou, H.J., (2016), just to cite a few. All the above methods are not only very performative but also computationally very fast. For a solid publication, the author should consider comparing their method with existing literature. Indeed, their benchmarks are quite outdated.

3. A technique that has become customary in network dismantling is “adaptiveness” of the node-score calculation. Instead that calculating the score before any node removal once and for all, it has been proven to be more effective to recompute the node-score after each removal (i.e. in an adaptive way). Indeed, after each node removal, the adjacency matrix of the network changes as well as the network properties. For a more performative dismantling, it is therefore of crucial importance to compute the node-score adaptively. The method proposed by the authors could be used adaptively but since the approach is very expensive computationally, the adaptive feature would make the method even slower. The authors briefly comment on this limitation towards the end of the paper. I believe that this is an important drawback. Further, all the methods used as benchmark in the paper, even if outdated, can be used in an adaptive way and they would all be more performative than their non-adaptive counterpart. The authors omit to show the comparison with the adaptive version of these methods therefore it is hard to say if their approach is really outperforming the benchmark ones or not.

4. Another important limitation of the approach presented is scalability. The fact that the NE approach must compute several partition functions makes it very expensive for large size networks. All the numerical experiments are performed on network sizes of N = 256, whereas the existing literature attempt to dismantle networks of the size of N = 10^6. For a proper presentation of their method compared to existing works the authors should consider testing their method on larger networks (N = 10^5-10^6) and show the CPU time as a function of the network size.

5. From the theoretical point of view, the NE is introduced as ad-hoc measures to scores nodes for targeted attack. No mathematical nor physical justification on why NE should be used as centrality measure for network dismantling is given. The only minor justification is that the NE reduced to something proportional to the degree for \beta \to 0. This is not a big problem, several centrality measures are introduced in the literature as ad-hoc, but it would be great if the authors could give a more intuitive explanation of why NE should be used as a score for network dismantling. For instance, recently ‘F Morone, G Del Ferraro, HA Makse, Nature physics 15 (1), 95-102’ have shown that the kcore of a network is intrinsically related to the robustness of a complex network from a theoretical point of view. It would be worth to compare NE with the kcore and deepen the theoretical justification.

6. The terminology used in the paper is, at times, confusing. After introducing eq. 1 in analogy with

quantum mechanics, the authors call the \beta parameter “diffusion time” and, throughout the paper, they talk of “time scales” or “information transfer”. This language is appropriate in quantum mechanics where the equivalent eq. 1 would have the Hamiltonian on the exponent, which is the operator that controls the temporal evolution of the system. In the present work, the Laplacian in eq. 1 has no temporal encoding of the network, in fact it is a purely topological measure. Indeed, dynamics is never even introduced through the paper because the authors consider the case of static network dismantling. Nevertheless, to keep the analogy with quantum mechanics (which is, yet, intrinsically dynamic) they keep using terms as “time-scale”, “information-transfer” and similar. This is confusing and very misleading.

7. On the math: eq. 3, 4, 5, 16, 17 are all coherent among themselves but incoherent with eq. 10. Indeed, in the former 1/log2 multiplies both terms on the RHS, whereas in eq. 10 it multiplies only the first term on the RHS. One would think that eq. 10 has a typo but it seems to me that eq. 10 is consistent with eq. 7 of Ref. 16. The authors should clarify the derivation and double check their math because eq. 3,4,5 are important MF approximations of their theory.

8. The authors use a MF approximation for their approach and no control of the error is presented. It is hard to say how big are the fluctuations from the mean, i.e. to establish a regime where the MF approximation is valid and where it is not.

9. The method is highly based on the experimental investigation of the \beta value to find the \beta_c at which the average NE is minimum. The author hypothesis that a minimum exists throughout the paper but, to my understanding, there is no guarantee that the minimum is finite nor, more importantly, there is a recipe to investigate the \beta range where the minimum of NE would be confined. The absence of a prescription to search for the \beta range where NE is minimum makes, potentially, the search very time consuming.

Overall, I find that the paper lacks appropriate comparison with existing literature. The method presented has serious limitations in terms of size scalability and CPU time. Comparison with latest literature methods was not performed but, looking at the plots, my guess is that the method will not outplay existing approaches, neither in performances nor in CPU time. The presentation is sometimes misleading with misuse of terminology. I cannot recommend publication in Communication Physics.

Reviewer #2 (Remarks to the Author):

In general, the manuscript is well-written and addresses a timely, interesting problem. My concerns with the current submission are listed below:

-Page 3: How to choose \beta without full enumeration?
-Page 5: What is the time complexity of entanglement? It seems to be at least O(N**2), probably higher, making it hard to apply the method to larger networks

-Page 6: What is an "arbitrary" network? Be precise on its topology or exact network type and how it was generated (in case of being random)
-Page 8: Being different from betweenness (on its own) is not necessarily advantage. The authors should argue based on the grid example why attacking the neighbors of corners in the grid is indeed interesting. Moreover, the apparent similarity to PageRank should be explored further.
- Page 9: The comparison against other static methods (i.e., without recomputation of measures is potentially misleading). While it is nice to see that entanglement can indeed outperform these static methods in Figure 3, the trade-off in computation time needs to be considered. Particularly, it would be very interesting to see how some interactive/dynamic measures (especially interactive betweenness centrality) are ranked in this figure, compared to entanglement.
- Page 10: RO-Page 2: "It is however surpassed by the degree centrality in other cases10" <- very unlikely.
-Page 3: How to choose \beta without full enumeration?
-Page 5: What is the time complexity of entanglement? It seems to be at least O(N**2), making it hard to apply the method to larger networks
-Page 6: What is an arbitrary network? Be precise on its topology or exact network type and how it was generated (in case of being random)
-Page 8: Being different from betweenness (on its own) is not necessarily advantage. The authors should argue based on the grid examply why attacking the neighbors of corners in the grid is indeed interesting. Moreover, the apparent similarity to PageRank should be explored further.
- Page 9: The comparison against other static methods (i.e., without recomputation of measures is potentially misleading). While it is nice to see that entanglement can indeed outperform these static methods in in Figure 3, the tradeoff in computation time needs to be considered. Particularly, it would be very interesting to see where some interactive/dynamic measures (especially betweenness centrality) are ranked in this figure, compared to entanglement.
- Page 10: Report results on networks with 10000+ nodes. This is still feasible for degree+betweenness and alike, even in their interactive variants. Networks of size ~200 nodes are of interest; but the authors should make a step further, since the state of the art in netwowrk dismantling considers networks with up to one million nodes.
- Please eport results on networks with 10000+ nodes. This is still feasible for degree+betweenness and alike, even in their interactive variants. Networks of size ~200 nodes are of interest; but the authors should make a step further, since the state of the art in network dismantling considers networks with up to one million nodes.

Reviewer #3 (Remarks to the Author):

Authors propose a novel centrality measure, called network entanglement. It is based on different approximations of Von Neumann Entropy. Essentially, the measure is based on the ratio of the diffusion dynamics propagator and trace of the partition function. The free parameter beta encoded the rate of diffusion. The authors do provide several analytical approximations e.g. mean-field approximation of Von Neumann Entropy and describe the limiting behavior of the entanglement. Entanglement of certain nodes is defined as the difference in Von Neumann entropy after removing a specific node.
Additionally, the authors provide an approximation for the critical beta, for which average entanglement is minimized. It is easy to propose a new centrality measure, but it hard to show it is useful. At this stage, the author has shown promising results, but I have some additional questions that I would like you to address.

Points to address:
- In order to show that this measure is capturing different information than standard centrality measures, please make a quantitative comparison to existing measures e.g. check correlation and mutual information w.r.t existing centralities over different nodes and networks.
- Please specify the exact computational complexity for calculating this measure.
- Please evaluate your results on larger empirical networks. All your networks are quite small. You need to show that it is not a finite size effect.
- Provide more details on how you select critical beta for figure 3.
- Provide more intuition why this measure is different than other spectral centrality measures?
- Provide more intuition about the diffusion process that this measure is capturing
- Although the measure has motivation from quantum stat. phy. please provide more explanation are any quantum effects observable on real-world networks or you are just using mathematical formalism.

FONDAZIONE BRUNO KESSLER

Trento, 20 January 2021

We thank the referees for their insightful comments which undoubtedly enhanced the message of our manuscript. The resubmitted manuscript, entitled "*Unraveling the effects of multiscale network entanglement on empirical systems*", is extensively changed according to the comments of the reviewers. A brief summary of the revisions can be listed as:

- We added the discussion of computational complexity and provided a method to reduce the computational cost without losing accuracy. However, we certainly acknowledge that the entanglement centrality has the disadvantage of being computationally slow compared to a number of effective centrality measures.
- We investigated the correlations between entanglement and other centrality measures, across a variety of toy models. The result indicates that entanglement shows no particular similarity with any other specific measure, in general, and can not be reduced to existing measures.
- We investigated the adaptive version of all centrality measures to complement the static attack strategies, showing that entanglement performs very well in both types of strategy.
- We ran the analysis on larger networks, confirming that the effectiveness of entanglement is not merely a small size effect.
- We dedicated more space to explain the physical grounds of Gibbsian-like density matrices, describing the dynamics of information among the units of complex systems, citing the most recent articles.
- We doubled, with respect to the original manuscript, the number of centrality measures used in our comparative analysis.

We would like to further stress that the aim of this paper is to show how thermodynamic-like functions – which are very novel and based on a promising theoretical framework – describing the dynamics of information are sensitive to capture the nodes' importance for network integration, shedding more light on the relationship between structure and dynamics, providing a perspective based on concepts borrowed from quantum statistical physics.

As an application, we investigate the effectiveness of entanglement in disintegration of complex structures since it is a standard benchmark when one introduces a novel measure of node importance. We are aware that the challenge of network dismantling cannot be solved by entanglement, despite its excellent performance in this task, but we are very confident that with our work we have learned something more about this problem and we are moving a step forward. Yet, we certainly acknowledge that there are several and more scalable and highly specialized algorithms, with less computational cost than entanglement, because the main aim of our work was not to develop a better algorithm for network dismantling. In the following, we go through the comments one by one, providing our answers. All the changes made are highlighted in red in the revised manuscript.

Sincerely,

The authors

Referee #1

We thank the reviewer for the time he/she has spent and his/her effort in reviewing our manuscript. We appreciate the criticisms and suggestions, and we hope that the revised version of the manuscript will address his/her concerns.

1. The method suffers of slow computation speed for two main reasons. First of all, the authors have to find numerically the value of \beta_c for which the average NE is minimum. This requires an extensive exploration of the \beta values numerically, and for each \beta value, the average NE has to be computed. The computation of the average NE involves the computation of several partition functions: the partition function of the whole network and the partition function of the perturbed network of each node \Delta G' (eq. 7). For large networks, especially with large average degree, this computation is extremely expensive and had to be done for each \beta value in the whole explored range to find the value of \beta_c for which the average NE is minimum.

We thank the referee for this insightful comment. It is worth remarking that we fully agree with the referee here: our aim is to attack the problem of network dismantling as an application of our framework based on quantum statistical physics and information theory, and we are aware of its limitations in terms of computation speed. Nevertheless, as any application derived from a novel framework, we performed an exploratory investigation of the problem and we have compared our results against existing metrics to better understand the potential of our approach. In no way we think that, at this stage, our method is computationally competitive with the ones of the market which are tailored to tackle the specific problem of network dismantling: conversely, our methodology has the advantage of being part of a larger set of theoretical tools that allow ones to build a unified picture for the analysis of complex networks. To this aim, our work is surely novel.

We have tried hard to reduce the computational cost of our method in the light of the reviewer's comment, and we have been able to find the temporal scale effective for network disintegration without the full enumeration, thus reducing the overall complexity of a factor N. In fact, all results presented in the revised manuscript are obtained using the diffusion time – i.e. inverse of the second eigenvalue of the Laplacian matrix – as characteristic scale.

We hope that the referee will appreciate and positively assess the new lower complexity of our approach.

Once \beta_c has been determined, the authors have to compute again the single-node-NE for each of the nodes in the network at \beta_c and each one of these calculations involves the computation of one partition function (eq. 7). As a result, the method seems highly time consuming. The authors comment on this problem towards the end of the manuscript, yet they omit giving a quantification of the issue. I believe the authors should add a plot of the computation time vs size of the networks for their methods and the different ones used for comparison.

We thank the reviewer very much for this comment: we totally agree that discussing the computational complexity of our approach is necessary and the original version of the manuscript was not adequately covering this matter. In the light of this comment, and the similar ones we received from the other reviewers,

we added this discussion in the Methods. Furthermore, we developed a faster – and simultaneously – accurate way of approximating the entanglement. It is worth mentioning that even this new faster version of our approach cannot be considered as a valid alternative to a number of other effective centrality measures with respect to the computational cost. Nevertheless, we have gained new insights in the meanwhile, showing that the approximation is valid and we explain what is the physical implication of such a result.
We certainly hope that in the future there will be other opportunities to enhance the algorithmic side of our framework, e.g., based on other new analytical treatments at the meso-scale. We explain more in the discussion:

"Here, we used entanglement centrality to define static ---i.e., non-adaptive--- and adaptive attack strategies, demonstrating that under both setups, it is possible to efficiently dismantle the networks. It is worth mentioning that our approach requires solving the eigenvalue problem that has high computational complexity. Our derivations provide a fast and simple way to calculate entanglement at extremely large and small temporal scales. More theoretical investigations are still required, generalizing our derivations to the meso-scale and reducing the computational cost. We developed an approximated version of our algorithm showing that it is significantly faster and also highly accurate (See Methods). However, we acknowledge that this approximated version is still slower compared with a number of other effective centrality measures, especially in case of large networks. Nevertheless, our results clearly demonstrate the sensitivity of statistical physics of complex information dynamics, in detecting the nodes that are central for network integration, which is the main aim of the present study."

More specifically, at the extremely large temporal scale, the field reaches its equilibrium state where all information streams decay exponentially except for the first one corresponding to the smallest eigenvalue of the Laplacian. The stream corresponding to the second smallest eigenvalue determines the diffusion time--- i.e., controlling how fast or slow the field is distributed equally over the discrete space--- as its decay is slower than the others. Consequently, in relatively large temporal scales, one can approximate the entropy using only two streams, related to first and second smallest eigenvalues. We explain this in more detail in the Methods section:

"Our theoretical derivations and numerical results indicate that entanglement centrality is highly effective at relatively large temporal scales \beta. This fact can be exploited to provide an approximated version of entanglement centrality, which is more scalable relative to the accurate version … instead of computing the full spectrum, one can approximate the entropy using the first two eigenvalues of the Laplacian matrix."

We thank the reviewer once again for this important comment.

2. The NE approach for network dismantling is compared to only very dated methods in the literature and most recent and performing approaches are left out. The network robustness and dismantling literature has recently been enriched by several outstanding methods. The authors cite some of this literature, but then they avoid comparing their approach with the most recent and performing ones. For examples, the CI method of Ref. 12 is cited but not used as a comparison, as well as the method of Ref. 13 which proved to outperformed Ref. 13 in several cases. Several other methods of network dismantling are not even cited and used as a benchmark, as Mugisha, S. and Zhou, H-J, Phys. Rev. E 94, 012305, (2016); Zdeborová, L., Zhang, P. and Zhou, H.J., (2016), just to cite a few. All the above methods are not only very performative but also

computationally very fast. For a solid publication, the author should consider comparing their method with existing literature. Indeed, their benchmarks are quite outdated.

We thank the reviewer very much for this valuable comment. We completely agree that the original version of the manuscript was lacking an adequate comparison against very relevant methods used nowadays for network dismantling. We would like just to remark that it was not in our intentions to avoid a direct comparison against the best methods in the market, because we have tried to approach network dismantling as a benchmark for the overall framework we are proposing, not as its ultimate goal.

Nevertheless, the addition of more well-known and high performing centrality measures would enhance our results and we have accepted the suggestion of the referee with enthusiasm.

It is worth remarking first that while many measures already tested are really outdated, some others – such as the betweenness – are not. In fact, recent extensive studies assessing both static and adaptive attack strategies have indicated that betweenness centrality is still one of the most effective methods, even compared with more recent and important ones, like CI. We stated the same argument in the manuscript:

"A systematic evaluation of state-of-the-art methods reveals that one of the best approaches is based on adaptive betweenness, one of the oldest measures, while being computationally expensive [wandelt (2018)], compared with effective centrality measures, such as degree and CoreHD."

Of course, the CoreHD algorithm mentioned by the referee is a very fast and effective method that was not yet compared with betweenness in our work. Since this measure has been capable of outperforming CI and some other benchmarks mentioned by the referee, we certainly agree that our manuscript can benefit from including it. Therefore, we added the analysis of adaptive centrality measures to complement the static one. As represented in Fig.3 of the revised manuscript, results obtained from entanglement are interesting even in the adaptive scenario. However, we acknowledge that, as explained in the revised text, the computational cost of our method is much higher than measures like CoreHD that, in fact, should be used as a primary choice for network dismantling. We thank the reviewer once again for this comment that certainly enhanced our manuscript.

3. A technique that has become customary in network dismantling is "adaptiveness" of the node-score calculation. Instead that calculating the score before any node removal once and for all, it has been proven to be more effective to recompute the node-score after each removal (i.e. in an adaptive way). Indeed, after each node removal, the adjacency matrix of the network changes as well as the network properties. For a more performative dismantling, it is therefore of crucial importance to compute the node-score adaptively. The method proposed by the authors could be used adaptively but since the approach is very expensive computationally, the adaptive feature would make the method even slower. The authors briefly comment on this limitation towards the end of the paper. I believe that this is an important drawback. Further, all the methods used as benchmark in the paper, even if outdated, can be used in an adaptive way and they would all be more performative than their non-adaptive counterpart. The authors omit to show the comparison with the adaptive version of these methods therefore it is hard to say if their approach is really outperforming the benchmark ones or not.

We thank the reviewer for this comment. As explained in our answer to the previous comment, the revised manuscript now includes the analysis of adaptive centrality measures to compliment the static one. As represented in Fig.3 of the revised manuscript, entanglement keeps competitive even in the adaptive scenario: this result is not trivial and would not be unraveled without the request of the referee. We acknowledge that, as explained in the revised text, the computational cost of our method is higher than a number of other measures. As explained in our answer to the first comment of the referee, we added a fair discussion about computational complexity. Furthermore, we developed a faster and simultaneously accurate way of approximating the entanglement. It is worth mentioning that even the approximated version of our method cannot be used to replace other effective centrality measures, when the computational cost is an important metric. Nevertheless, the importance of our method relies on its explanatory power, which links the application of dismantling to the broader framework of statistical physics of complex information dynamics, which is the aim of our study.

We certainly hope that in the future, we will be able to enhance our algorithm, based on other new analytical treatments at the meso-scale. We explain more in the discussion:

"Here, we used entanglement centrality to define static ---i.e., non-adaptive--- and adaptive attack strategies, demonstrating that under both setups, it is possible to efficiently dismantle the networks. It is worth mentioning that our approach requires solving the eigenvalue problem that has high computational complexity. Our derivations provide a fast and simple way to calculate entanglement at extremely large and small temporal scales. More theoretical investigations are still required, generalizing our derivations to the meso-scale and reducing the computational cost. We developed an approximated version of our algorithm showing that it is significantly faster and also highly accurate (See Methods). However, we acknowledge that this approximated version is still slower compared with a number of other effective centrality measures, especially in case of large networks. Nevertheless, our results clearly demonstrate the sensitivity of statistical physics of complex information dynamics, in detecting the nodes that are central for network integration, which is the main aim of the present study."

4. Another important limitation of the approach presented is scalability. The fact that the NE approach must compute several partition functions makes it very expensive for large size networks. All the numerical experiments are performed on network sizes of N = 256, whereas the existing literature attempt to dismantle networks of the size of N = 10^6. For a proper presentation of their method compared to existing works the authors should consider testing their method on larger networks (N = 10^5-10^6) and show the CPU time as a function of the network size.

We thank the reviewer for this comment. In fact, as explained in the manuscript, our goal is not to outperform other centrality measures in every aspect, and therefore we cannot satisfy the request of applying the method to networks of size 10^5-10^6, as the referee is probably well aware of.
However, while research dedicated to efficient and highly specialized algorithms is surely crucial for applications to big social/biological/economical/ data, we are firmly convinced that a parallel, and equally important, research must be devoted to advance our theoretical insights and understanding of problems related to our complex Universe from a broader, and less specialized, perspective. The latter can open doors for the former in ways that one can not easily predict.

In this article, we use the framework describing information dynamics within complex networks and show that the effect of node removal on the mixedness of information streams (i.e. operators directing the flow of information), can be used to dismantle the structures quickly, across classes of networks. The fact that the nodes important for information dynamics are the ones that keep the network integrated is the key finding of this study, shedding light on the structure-dynamics coupling. Crucially, the fact that we have applied our framework to the problem of network dismantling is opening new interesting perspectives, since we provide a unified theoretical framework for the understanding of information dynamics which applies to this process but also to other ones, such as the enhancement of transport dynamics in multilayer systems [Ghavasieh and De Domenico], the identification of efficient topologies for information exchange [Ghavasieh, Nicolini and De Domenico (2020)] or the emerging macroscopic features of interactomes in virus-host interactions [Ghavasieh, Bontorin, Artime and De Domenico (2020)]. We hope that the referee will agree with us on this point, and will appreciate our work from another perspective, not strictly focused on the algorithmic performances of the proposed method. To show more manifestly our willingness to focus more on the understanding than the performance, we have removed the term "disintegration" from the title of our work, thus re-aligning the expectation of the reader with the main motivation of our study.

Nevertheless, we have enhanced our study with the analysis of larger networks (almost two orders of magnitude larger than the ones used in the original version of the manuscript), yet way smaller than million nodes, to ensure that the effectiveness of our approach is not merely a small-size effect.

5. From the theoretical point of view, the NE is introduced as ad-hoc measures to scores nodes for targeted attack. No mathematical nor physical justification on why NE should be used as centrality measure for network dismantling is given. The only minor justification is that the NE reduced to something proportional to the degree for \beta \to 0. This is not a big problem, several centrality measures are introduced in the literature as ad-hoc, but it would be great if the authors could give a more intuitive explanation of why NE should be used as a score for network dismantling. For instance, recently 'F Morone, G Del Ferraro, HA Makse, Nature physics 15 (1), 95-102' have shown that the kcore of a network is intrinsically related to the robustness of a complex network from a theoretical point of view. It would be worth to compare NE with the kcore and deepen the theoretical justification.

We thank the reviewer for this comment, and we blame ourselves if the original manuscript suggested somehow that the adopted measure was *ad hoc* and not mathematically or physically grounded. We reassure the referee that, indeed, our measure has a solid physical and mathematical justification.

We dedicated more space to explain the physical grounds of Gibbsian-like density matrices to clarify the ambiguity. Although the relationship between the Gibbsian-like density matrices and information dynamics has been only implicitly mentioned in the original studies [De Domenico and Biamonte], it became better clear in [Ghavasieh and De Domenico] until it was fully framed as a general statistical field theory describing short-to-long-range node-node interactions [Ghavasieh, Nicolini and De Domenico (2020)]. All these papers are now cited in our current manuscript and more theoretical context is provided.

In fact, the last reference shows that information dynamics within complex structures can be fully captured by an ensemble of operators acting as information streams [Ghavasieh, Nicolini and De Domenico (2020)], and their superposition form the density matrix. The mixedness of information streams can be obtained from the Von Neumann entropy, which is shown to be a measure of functional diversity of the system.

How does node removal affect the information dynamics within the whole network? We quantified it in terms of the change in the mixedness of information streams, and consequently, the impact of nodes on the system's functional diversity. The fact that the nodes important for information dynamics are the ones that keep the network integrated is the key finding of this study, shedding light on the structure-dynamics coupling. This is further stressed in the revised manuscript.

We certainly acknowledge that the previous version of the manuscript lacked a proper review of the Gibbsian-like density matrices. Therefore, we extended the Theoretical grounds section with:

"Information flow between components of complex systems can be mapped into the dynamics of a field on top of the network, governed by a general differential equation which after linearization reduces to a Schrodinger like equation with a quasi-hamiltonian H. It directly leads to the propagator, from which an ensemble of operators can be obtained acting like information streams, directing the flow of the field from unit to unit [Ghavasieh, Nicolini and De Domenico (2020)]. The superposition of the information streams weighted by their activation probabilities shapes a Gibbsian-like density matrix that has been used to describe the state of interconnected systems and investigate their macroscopic properties such as Von Neuman entropy."

We provided a deeper understanding of the Von Neumann entropy, as follow:

"The entropy measures the mixedness of the streams, quantifying the diversity of information dynamics in the system. Interestingly, the functional diversity of nodes, as sender or receiver of information, has been shown proportional to the Von Neumann entropy [Ghavasieh, Nicolini and De Domenico (2020)]."

From here, the meaning of entanglement will be more tangible:

"...the effect of node removal on the mixedness of information streams or, in other words, the functional diversity of the system defines the entanglement."

We thank the reviewer very much for this comment. We totally agree that this extended review of the theoretical grounds and the meaning of the mathematical objects were indeed missing in our previous version of manuscript.

6. The terminology used in the paper is, at times, confusing. After introducing eq. 1 in analogy with quantum mechanics, the authors call the \beta parameter "diffusion time" and, throughout the paper, they talk of "time scales" or "information transfer". This language is appropriate in quantum mechanics where the equivalent eq. 1 would have the Hamiltonian on the exponent, which is the operator that controls the temporal evolution of the system. In the present work, the Laplacian in eq. 1 has no temporal encoding of the network, in fact it is a purely topological measure. Indeed, dynamics is never even introduced through the paper because the authors consider the case of static network dismantling. Nevertheless, to keep the analogy with quantum mechanics (which is, yet, intrinsically dynamic) they keep using terms as "time-scale", "information-transfer" and similar. This is confusing and very misleading.

We thank the reviewer for this important comment. We fully agree with the reviewer that the previous version of the manuscript lacked a detailed description of the physical grounds of the Gibbsian-like density matrices and their relationship with information dynamics. Similar to what the referee correctly stated about quantum mechanics, the statistical physics of complex information dynamics is based on an operator controlling the temporal evolution of a field. In the revised manuscript, we mention and cite a recently published paper on the same matter:

"Information flow between components of complex systems can be mapped into the dynamics of a field on top of the network, governed by a general differential equation which after linearization reduces to a Schrodinger like equation with a quasi-hamiltonian H. It directly leads to the propagator, from which an ensemble of operators can be obtained acting like information streams, directing the flow of the field from unit to unit [Ghavasieh, Nicolini and De Domenico (2020)]."

Remarkably, this framework is general to include a variety of dynamical processes governing the information flow within complex networks. One of these classes of dynamics, observed in a multitude of systems and adopted to model the information dynamics, is diffusion and is governed by the Laplacian. The solution of this dynamical equation is a propagator, being an exponential function of the Laplacian multiplied by the propagation time which we indicated by \beta in this article. We certainly acknowledge that the previous version of the manuscript lacked the proper review of the relationship between Laplacian matrix, the diffusion dynamics and the statistical field theory. In the light of the reviewer's comment, we extend the Theoretical grounds section with:

"Remarkably, when the dynamical process governing the evolution of the field is continuous diffusion, the Von Neumann entropy of the ensemble coincides with the spectral entropy proposed to study the analyse the networks from an information-theoretic perspective [De Domenico and Biamonte] and the quasi-hamiltonian takes the shape of the combinatorial Laplacian matrix"

We thank the reviewer once again for this comment.

7. On the math: eq. 3, 4, 5, 16, 17 are all coherent among themselves but incoherent with eq. 10. Indeed, in the former 1/log2 multiplies both terms on the RHS, whereas in eq. 10 it multiplies only the first term on the RHS. One would think that eq. 10 has a typo but it seems to me that eq. 10 is consistent with eq. 7 of Ref. 16. The authors should clarify the derivation and double check their math because eq. 3,4,5 are important MF approximations of their theory.

We thank the reviewer for this comment. We understand that perhaps our notation has not been clear, and, therefore, we add a sentence to explain it better, after Eq.10. We thank the referee once again for this comment.

To better clarify on this point: let's assume the logarithm of x on the base of 2 is indicated as log_{2}(x). To change the base, one can write: log_{2}(x) = log(x)/log(2), where log without indicating the base is representing the natural logarithm. This is why we see 1/log(2) only before the first term in Eq.10.

8. The authors use a MF approximation for their approach and no control of the error is presented. It is hard to say how big are the fluctuations from the mean, i.e. to establish a regime where the MF approximation is valid and where it is not.

We thank very much the reviewer for this important comment. The MF approximation developed here is very similar to the one previously developed for random walk dynamics [Ghavasieh and De Domenico], differing only in the mean of the eigenvalues of the quasi-Hamiltonian. In the very same reference, it has been shown analytically and numerically that MF is not a very good approximation for small temporal scales, while it becomes quite accurate in the meso and macro-scale. Therefore, it is reliable to use MF in order to describe the larger scales, as done in our manuscript.

Since the concern of the referee is relevant, we have added a section dedicated to the precision of mean-field approximation, in order to better explain this point in the manuscript. The conclusion of this section, in the Methods, is as follows:

"According to Eq.14, at large propagation times \beta, the correction term decays exponentially with the exponent \bar{k} (average degree). Consequently, the precision of mean-field approximation is higher, when long range interactions between the nodes are under consideration. Similar results about the precision of mean-field approximation have been previously obtained for density matrices obtained from random walk dynamics [Ghavasieh and De Domenico] ."

We thank the reviewer once again for this important comment. Since we use the mean-field approximation to understand the behavior of entropy and derive the characteristic temporal scale, it is very fundamental for our study to ensure the reader about its precision.

9. The method is highly based on the experimental investigation of the \beta value to find the \beta_c at which the average NE is minimum. The author hypothesis that a minimum exists throughout the paper but, to my understanding, there is no guarantee that the minimum is finite nor, more importantly, there is a recipe to investigate the \beta range where the minimum of NE would be confined. The absence of a prescription to search for the \beta range where NE is minimum makes, potentially, the search very time consuming.

We thank the referee for this comment: in fact, we agree with the referee that there is no guarantee for the existence of a local minimum. As we explained in our response to the first comment, we have been able to find the temporal scale effective for network disintegration without the full enumeration, resolving this issue. In fact, all results presented in the revised manuscript are obtained using the diffusion time as a characteristic scale. We thank the referee once again for this comment.

Referee #2

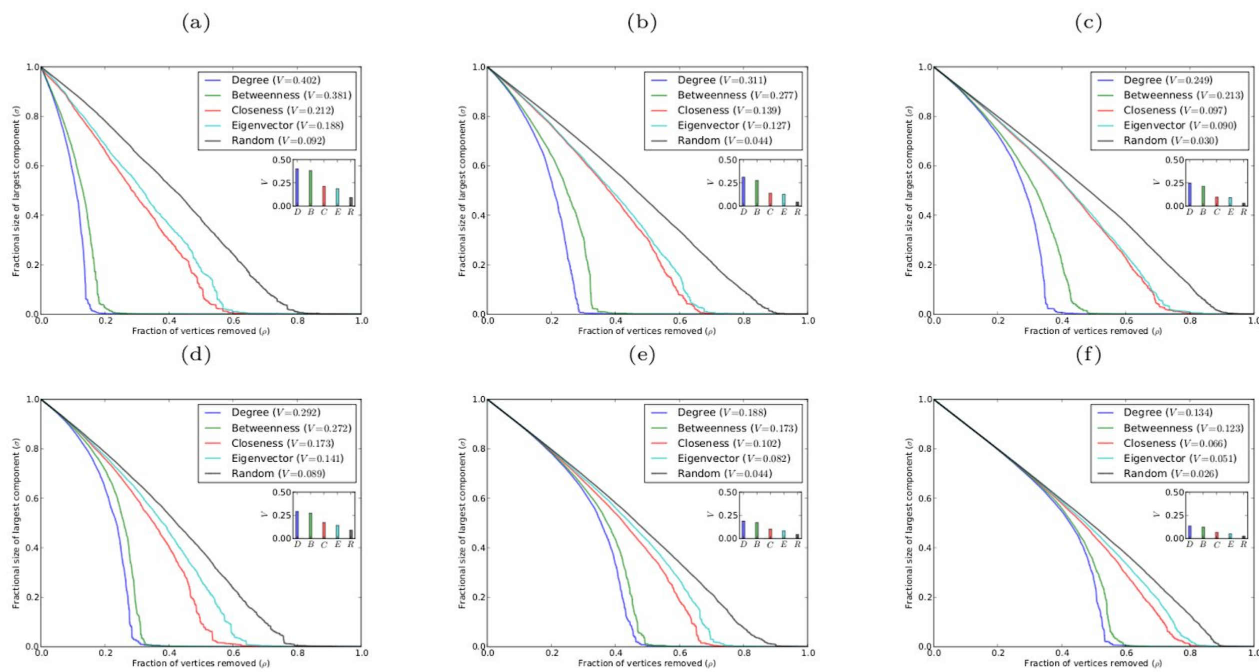
Firstly, we would like to thank the reviewer for his/her time and effort for considering our manuscript and the encouragement we received from him/her:

"In general, the manuscript is well-written and addresses a timely, interesting problem."

We also thank the reviewer for his/her insightful comments. We believe that the edited manuscript answers the concerns. In the following, we go through the comments one by one.

Page 2: "It is however surpassed by the degree centrality in other cases10" <- very unlikely.

We thank the reviewer for this comment. We attach a figure from our reference [Lyer (2013)] where, seemingly, degree centrality is performing slightly better than betweenness:



However, we completely understand the point raised by the referee highlighting the more extensive studies assessing both static and adaptive attack strategies, where betweenness centrality is shown to outperform others, including degree centrality. We refer to one of such recent studies in our manuscript as:

"A systematic evaluation of state-of-the-art methods reveals that one of the best approaches is based on adaptive betweenness, one of the oldest measures, while being computationally expensive [wandelt (2018)], compared with effective centrality measures, such as degree and CoreHD."

Page 3: How to choose \beta without full enumeration?

We thank the referee for this insightful comment. In the light of this comment, we have been able to find the temporal scale effective for network disintegration without the full enumeration, reducing the computational cost of the presented method of a factor N. In fact, all results presented in the revised manuscript are obtained using the diffusion time as characteristic scale- i.e. inverse of the second eigenvalue of the Laplacian matrix.

Page 5: What is the time complexity of entanglement? It seems to be at least O(N**2), making it hard to apply the method to larger networks

We thank the reviewer very much for this comment: we totally agree that discussing the computational complexity of our approach is necessary and the original version of the manuscript was not adequately covering this matter. In the light of this comment, and the similar ones we received from the other reviewers, we added this discussion in the Methods. Furthermore, we developed a faster – and simultaneously – accurate way of approximating the entanglement. It is worth mentioning that even this new faster version of our approach cannot be considered as a valid alternative to a number of other effective centrality measures with respect to the computational cost. Nevertheless, we have gained new insights in the meanwhile, showing that the approximation is valid and we explain what is the physical implication of such a result.
We certainly hope that in the future there will be other opportunities to enhance the algorithmic side of our framework, e.g., based on other new analytical treatments at the meso-scale. We explain more in the discussion:

"Here, we used entanglement centrality to define static ---i.e., non-adaptive--- and adaptive attack strategies, demonstrating that under both setups, it is possible to efficiently dismantle the networks. It is worth mentioning that our approach requires solving the eigenvalue problem that has high computational complexity. Our derivations provide a fast and simple way to calculate entanglement at extremely large and small temporal scales. More theoretical investigations are still required, generalizing our derivations to the meso-scale and reducing the computational cost. We developed an approximated version of our algorithm showing that it is significantly faster and also highly accurate (See Methods). However, we acknowledge that this approximated version is still slower compared with a number of other effective centrality measures, especially in case of large networks. Nevertheless, our results clearly demonstrate the sensitivity of statistical physics of complex information dynamics, in detecting the nodes that are central for network integration, which is the main aim of the present study."

To be more specific, we mention that the information dynamics within complex structures can be captured by a set of operators acting like information streams, directing the flow from node to node [Ghavasieh, Nicolini and De Domenico (2020)]. At the extremely large temporal scale, the field reaches its equilibrium state where all information streams decay exponentially except for the first one corresponding to the smallest eigenvalue of the Laplacian. The stream corresponding to the second smallest eigenvalue determines the diffusion time--- i.e., controlling how fast or slow the field is distributed equally over the discrete space--- as its decay is slower than the others. Consequently, in relatively large temporal scales, one can approximate the entropy using only two streams, related to first and second smallest eigenvalues. We explain this in more detail in the Methods section:

"Our theoretical derivations and numerical results indicate that entanglement centrality is highly effective at relatively large temporal scales \beta. This fact can be exploited to provide an approximated version of entanglement centrality, which is more scalable relative to the accurate version … instead of computing the full spectrum, one can approximate the entropy using the first two eigenvalues of the Laplacian matrix."

We thank the reviewer once again for this important comment.

Page 6: What is an arbitrary network? Be precise on its topology or exact network type and how it was generated (in case of being random)

We thank the referee for this comment. We completely agree that the previous version of the manuscript lacked a clear description of the network topology represented in Fig.1. In the revised version, the section (b) of the first figure represents the entanglement of nodes of a random geometrical network, with size 100 and radius 0.15. We added these details to the caption of the figure. We thank the referee once again for spotting this missing point.

Page 8: Being different from betweenness (on its own) is not necessarily advantage. The authors should argue based on the grid examply why attacking the neighbors of corners in the grid is indeed interesting. Moreover, the apparent similarity to PageRank should be explored further.

We thank the reviewer for this comment. We totally agree that being merely different from betweenness is not an advantage. The goal of that comparison is to visually show that entanglement of nodes can not be captured by other centrality measures, and, therefore, it is indeed a new measure. In the light of this comment and a similar one we received from the third referee, we adopt a more comprehensive and quantitative method to demonstrate this dissimilarity. In fact, in the revised manuscript, we use Spearman correlation to provide similarity heatmaps and hierarchical clusterings indicating that entanglement has no particular relation with other measures of centrality.

Regarding the example of grid, the only way to explain the behavior of entanglement is through its theoretical grounds. In fact, it has been shown that entropy measures the diversity of information dynamics in the system and the functional diversity of the nodes as senders [Ghavasieh, Nicolini and De Domenico (2020)]. Thus, those nodes with high entanglement are the ones that are important for information dynamics and their removal will damage the system's functional diversity.

Page 9: The comparison against other static methods (i.e., without recomputation of measures is potentially misleading). While it is nice to see that entanglement can indeed outperform these static methods in in Figure 3, the tradeoff in computation time needs to be considered. Particularly, it would be very interesting to see where some interactive/dynamic measures (especially betweenness centrality) are ranked in this figure, compared to entanglement.

We thank the reviewer for this comment. As we explained in our answer to the previous comments, we have added the discussion of computational complexity. Moreover, following this comment, we have improved our manuscript and included the comparison of centrality measures used in interactive fashion. As represented in Fig.3 of the revised manuscript, entanglement keeps its advantage even in the adaptive scenario. However we acknowledge that, as explained in the revised text, the computational cost of our method is higher than measures like degree and betweenness.

Page 10: Report results on networks with 10000+ nodes. This is still feasible for degree+betweenness and alike, even in their interactive variants. Networks of size ~200 nodes are of interest; but the authors should make a step further, since the state of the art in netwowrk dismantling considers networks with up to one million nodes.

We thank the reviewer for this comment. Accordingly, we have provided the analysis of larger networks in the revised manuscript to ensure that the effectiveness of this approach is not merely a small size effect. A description of the network considered in the revised manuscript is as follows:

"Data sets include the neural network of the nematode worm C. elegans (N=297) representing neurons linked by their neural junctions, the US airports network (N=500) representing the busiest commercial airports in the United States in 2002 with links encoding the flights between them, the protein-protein interaction network of yeast (N=1458), the network of internet pages of EPA (N=4253) and the citation network corresponding to the Digital Bibliography & Library Project (N=12495)."

Our results confirm that network entanglement is reliable even in the case of larger networks. We conclude the section "Entanglement analysis of the real-world networks" with:

"Interestingly, our analysis indicates that, for all the considered empirical systems, the entanglement centrality provides an effective dismantling strategy, comparable with the best measures, thus providing a promising candidate for such a universal attack strategy. "

We thank the referee for this important comment, which undoubtedly has improved the message of our manuscript. Note that elsewhere in the manuscript we have manifestly specified the limitations in terms of computational speed.

Referee #3

Authors propose a novel centrality measure, called network entanglement. It is based on different approximations of Von Neumann Entropy. Essentially, the measure is based on the ratio of the diffusion dynamics propagator and trace of the partition function. The free parameter beta encoded the rate of diffusion. The authors do provide several analytical approximations e.g. mean-field approximation of Von Neumann Entropy and describe the limiting behavior of the entanglement. Entanglement of certain nodes is defined as the difference in Von Neumann entropy after removing a specific node.
Additionally, the authors provide an approximation for the critical beta, for which average entanglement is minimized. It is easy to propose a new centrality measure, but it hard to show it is useful. At this stage, the author has shown promising results, but I have some additional questions that I would like you to address.

We would like to thank the reviewer for his/her insightful series of comments which helped us improve the paper. We believe that the revised manuscript answers the concerns, completely. In the following, we go through the point and address them one by one.

Points to address:
- In order to show that this measure is capturing different information than standard centrality measures, please make a quantitative comparison to existing measures e.g. check correlation and mutual information w.r.t existing centralities over different nodes and networks.

We thank the reviewer for this insightful comment. Following this comment and a similar one received from the second reviewer, we investigated the correlations between entanglement and other centrality measures, across four of topologies. The result indicates that entanglement shows no specific Spearman correlation with any other specific measure. According to the importance of this point raised by the referee, we updated our first figure and included the similarity heatmaps with hierarchical clustering, representing their correlations.

- Please specify the exact computational complexity for calculating this measure.

We thank the reviewer very much for this comment: we totally agree that discussing the computational complexity of our approach is necessary and the original version of the manuscript was not adequately covering this matter. In the light of this comment, and the similar ones we received from the other reviewers, we added this discussion in the Methods. Furthermore, we developed a faster – and simultaneously – accurate way of approximating the entanglement. It is worth mentioning that even this new faster version of our approach cannot be considered as a valid alternative to a number of other effective centrality measures with respect to the computational cost. Nevertheless, we have gained new insights in the meanwhile, showing that the approximation is valid and we explain what is the physical implication of such a result.
We certainly hope that in the future there will be other opportunities to enhance the algorithmic side of our

framework, e.g., based on other new analytical treatments at the meso-scale. We explain more in the discussion:

"Here, we used entanglement centrality to define static ---i.e., non-adaptive--- and adaptive attack strategies, demonstrating that under both setups, it is possible to efficiently dismantle the networks. It is worth mentioning that our approach requires solving the eigenvalue problem that has high computational complexity. Our derivations provide a fast and simple way to calculate entanglement at extremely large and small temporal scales. More theoretical investigations are still required, generalizing our derivations to the meso-scale and reducing the computational cost. We developed an approximated version of our algorithm showing that it is significantly faster and also highly accurate (See Methods). However, we acknowledge that this approximated version is still slower compared with a number of other effective centrality measures, especially in case of large networks. Nevertheless, our results clearly demonstrate the sensitivity of statistical physics of complex information dynamics, in detecting the nodes that are central for network integration, which is the main aim of the present study."

More specifically, at the extremely large temporal scale, the field reaches its equilibrium state where all information streams decay exponentially unless the first one corresponding to the smallest eigenvalue of the Laplacian. The stream corresponding to the second smallest eigenvalue determines the diffusion time--- i.e., controlling how fast or slow the field is distributed equally over the discrete space--- as its decay is slower than the others. Consequently, in relatively large temporal scales, one can approximate the entropy using only two streams, related to first and second smallest eigenvalues. We explain this in more detail in the Methods section:

"Our theoretical derivations and numerical results indicate that entanglement centrality is highly effective at relatively large temporal scales \beta. This fact can be exploited to provide an approximated version of entanglement centrality, which is more scalable relative to the accurate version … instead of computing the full spectrum, one can approximate the entropy using the first two eigenvalues of the Laplacian matrix."

We thank the reviewer once again for this important comment.

- Please evaluate your results on larger empirical networks. All your networks are quite small. You need to show that it is not a finite size effect.

We thank the reviewer for this comment. Accordingly, we have provided the analysis of larger networks in the revised manuscript to ensure that the effectiveness of this approach is not merely a small size effect. A description of the network considered in the revised manuscript is as follows:

"Data sets include the neural network of the nematode worm C. elegans (N=297) representing neurons linked by their neural junctions, the US airports network (N=500) representing the busiest commercial airports in the United States in 2002 with links encoding the flights between them, the protein-protein interaction network of yeast (N=1458), the network of internet pages of EPA (N=4253) and the citation network corresponding to the Digital Bibliography & Library Project (N=12495)."

Our results confirm that NE is reliable, even in case of larger networks. We conclude the section “Entanglement analysis of the real-world networks” with:

“Interestingly, our analysis indicates that, for all the considered empirical systems, the entanglement centrality provides an effective dismantling strategy, comparable with the best measures and outperforming the others, thus providing a promising candidate for such a universal attack strategy. ”

We thank the referee for this important comment, which undoubtedly has improved the message of our manuscript. Note that elsewhere in the manuscript we have manifestly specified the limitations in terms of computational speed.

- Provide more details on how you select critical beta for figure 3.

We thank the referee for this comment. In the previous version of the manuscript, critical beta was computed with full enumeration. However, in the revised version and thanks to a comment we received from the second referee, we have been able to find the temporal scale effective for network disintegration without the full enumeration, thus reducing the computational cost of our method. In fact, all results presented in the revised manuscript are obtained using the diffusion time as a characteristic scale- i.e. inverse of the second eigenvalue of the Laplacian matrix.

- Provide more intuition why this measure is different than other spectral centrality measures?

We thank the reviewer for this important comment. In contrast with most centrality measures which are ad-hoc, NE has a solid physical meaning and is derived from a general framework for information dynamics within complex networks. To provide a deeper understanding, we dedicated more space to explain the physical grounds of Gibbsian-like density matrices. Although the relationship between the Gibbsian-like density matrices and information dynamics has been only implicitly mentioned in the original studies [De Domenico and Biamonte], it became better clear in [Ghavasieh and De Domenico] until it was fully framed as a general statistical field theory describing short-to-long-range node-node interactions [Ghavasieh, Nicolini and De Domenico (2020)]. All these papers are now cited in our current manuscript and more theoretical context is provided.

In fact, the last reference shows that information dynamics within complex structures can be fully captured by an ensemble of operators acting as information streams [Ghavasieh, Nicolini and De Domenico (2020)], and their superposition form the density matrix. The mixedness of information streams can be obtained from the Von Neumann entropy, which is shown to be a measure of functional diversity of the system.

How does node removal affect the information dynamics within the whole network? We quantified it in terms of the change in the mixedness of information streams, and consequently, the impact of nodes on the system’s functional diversity. The fact that the nodes important for information dynamics are the ones that keep the network integrated is the key finding of this study, shedding light on the structure-dynamics coupling. This is further stressed in the revised manuscript.

We certainly acknowledge that the previous version of the manuscript lacked a proper review of the Gibbsian-like density matrices. Therefore, we extended the Theoretical grounds section with:

"Information flow between components of complex systems can be mapped into the dynamics of a field on top of the network, governed by a general differential equation which after linearization reduces to a Schrodinger like equation with a quasi-hamiltonian H. It directly leads to the propagator, from which an ensemble of operators can be obtained acting like information streams, directing the flow of the field from unit to unit [Ghavasieh, Nicolini and De Domenico (2020)]. The superposition of the information streams weighted by their activation probabilities shapes a Gibbsian-like density matrix that has been used to describe the state of interconnected systems and investigate their macroscopic properties such as Von Neuman entropy."

We provided a deeper understanding of the Von Neumann entropy, as follow:

"The entropy measures the mixedness of the streams, quantifying the diversity of information dynamics in the system. Interestingly, the functional diversity of nodes, as sender or receiver of information, has been shown proportional to the Von Neumann entropy [Ghavasieh, Nicolini and De Domenico (2020)]."

From here, the meaning of entanglement will be more tangible:

"...the effect of node removal on the mixedness of information streams or, in other words, the functional diversity of the system defines the entanglement."

We thank the reviewer very much for this comment. We totally agree that this extended review of the theoretical grounds and the meaning of the mathematical objects were indeed missing in our previous version of manuscript. We think that the revised version is clear about the novelty of NE and its fundamental difference with other centrality measures specifically designed for network dismantling.

- Provide more intuition about the diffusion process that this measure is capturing

We thank the reviewer for this important comment. We fully agree with the reviewer that the previous version of the manuscript lacked a detailed description of the physical grounds of the Gibbsian-like density describing information diffusion. In fact, the statistical physics of complex information dynamics is based on an operator controlling the temporal evolution of a field. In the revised manuscript, we mention and cite a recently published paper on the same matter:

"Information flow between components of complex systems can be mapped into the dynamics of a field on top of the network, governed by a general differential equation which after linearization reduces to a Schrodinger like equation with a quasi-hamiltonian H. It directly leads to the propagator, from which an ensemble of operators can be obtained acting like information streams, directing the flow of the field from unit to unit [Ghavasieh, Nicolini and De Domenico (2020)]."

Remarkably, this framework is general to include a variety of dynamical processes governing the information flow within complex networks. One of these classes of dynamics, observed in a multitude of systems and adopted to model the information dynamics, is diffusion and is governed by the Laplacian. The solution of this dynamical equation is a propagator, being an exponential function of the Laplacian multiplied by the propagation time which we indicated by \beta in this article. We certainly acknowledge that the previous version of the manuscript lacked the proper review of the relationship between Laplacian matrix, the diffusion dynamics and the statistical field theory. In the light of the reviewer's comment, we extend the Theoretical grounds section with:

"Remarkably, when the dynamical process governing the evolution of the field is continuous diffusion, the Von Neumann entropy of the ensemble coincides with the spectral entropy proposed to study the analyse the networks from an information-theoretic perspective [De Domenico and Biamonte] and... the quasi-hamiltonian takes the shape of the combinatorial Laplacian matrix..."

We thank the reviewer once again for this comment.

- Although the measure has motivation from quantum stat. phy. please provide more explanation are any quantum effects observable on real-world networks or you are just using mathematical formalism.

We thank the referee for this comment. We agree with the reviewer that the previous version of the manuscript was vague in explaining the physical grounds of our work, which can possibly give the wrong impression that our density matrix is only a mathematical formalism borrowed from quantum statistical physics. We thank the reviewer for pointing out this issue. In the extended version of the Theoretical grounds section, we explain the roots of this similarity between our framework and statistical quantum physics:

"Information flow between components of complex systems can be mapped into the dynamics of a field on top of the network, governed by a general differential equation which after linearization reduces to a Schrodinger like equation with a quasi-hamiltonian H. It directly leads to the propagator, from which an ensemble of operators can be obtained acting like information streams, directing the flow of the field from unit to unit [Ghavasieh, Nicolini and De Domenico (2020)]. The superposition of the information streams weighted by their activation probabilities shapes a Gibbsian-like density matrix that has been used to describe the state of interconnected systems and investigate their macroscopic properties such as Von Neuman entropy."

It is worth mentioning that the dynamical process considered in our work is classical diffusion, and therefore, we do not expect to see any quantum effect. However, as discussed in [Ghavasieh, Nicolini and De Domenico (2020)] and thanks to the generality of the framework, the dynamics of the field can instead be governed by quantum Hamiltonians, for specific systems. We will surely look forward to investigating the properties of complex quantum networks, adopting this field theory, in the future.

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have taken into account my remarks and changed the manuscript accordingly. In particular, there are a few changes in the paper that I found very interesting and that I think have improved the overall work. These are listed in the following.

The authors have discussed the algorithmic complexity of their approach from an analytical and numerical point of view. I believe that this discussion was essential to make the reader aware of the limitation of the presented method for large size networks.

For large temporal scale \beta the authors have developed an approximate method which reduces this complexity by a factor N and still makes accurate predictions. This method only computes the first two eigenvalues of the Laplacian matrix, instead that the full spectrum, and the entropy is approximated accordingly. In Section Discussion, the authors acknowledge the need of further studies to extend their approach at the meso-scale, where the aforementioned approximation fails.

The authors have included a discussion on the quantification of the error done through the mean-field approximation in their method.

Further, their numerical investigation now includes both static and adaptive approaches and comparison with a larger fan of methods.

With all the edits and new results I believe the paper has very much improved. I do have one more suggestion that I would like the authors to consider. The main results in term of mathematical formulas are now shown only in the Method section, with the risk of being missed by the reader. I believe that the paper would benefit if the final results for the entanglement and the large \beta approximation of the entropy were shown in the main text (as they were in the previous version of the manuscript). I think this would further help the reader interpreting the method and understanding the figures before even reading the Method Section.

Reviewer #2 (Remarks to the Author):

In the revised manuscript, the authors argue that network dismantling is just an application of their theoretical framework; and therefore, would be publishable despite other concerns. I do not follow this argument. If the authors think that other applications would be more appropriate for presentation of entanglement, I recommend to follow such a track.

The rebuttal repeatedly stresses that the authors’ goal was not to develop a better algorithm. I strongly believe that the goal of science should indeed be to derive better methods and advance the understanding, with *clear* references as a baseline. In recent years, several effective, scalable methods for network dismantling have been proposed and evaluated. If this submission on entanglement was published, it will be seen as latest reference by other authors for future submissions on network dismantling. Which, in turn will lead to more methods failing to improve the state of the art on *network dismantling*.

Given that all three reviewers have essentially raised the similar concerns, and since the application-specific insights I have obtained from the given manuscript are not sufficient to outweigh the absence of “being better”, I recommend to reject the submission.

Reviewer #3 (Remarks to the Author):

The authors have addressed the majority of my questions. I am not fully convinced of the superior performance of the proposed method w.r.t. all state-of-the art dismantling solutions. Still, the work provides new theoretical contributions with the network entanglement measure, that acts as an approximation of Von Neuman entropy.
The authors made comparisons with a large number of centrality measures, which has convinced me that net. ent. is capturing a novel property of a network.
The authors were honest about the limits of computational run time and made several approximations with the first two eigenvalues of combinatorial Laplacian. The authors have given a nice theoretical explanation of why Von-Neuman entropy is considered. Connection to the dynamics of fields, driven by differential equation and its linearization, which ends with propagators is described.

Trento, 11 March 2021

We thank the referees for their constructive comments and support for publication of our work, entitled "*Unraveling the effects of multiscale network entanglement on empirical systems*". The revised manuscript is changed according to the comments we received from the first reviewer.

Sincerely,

The authors

Referee #1

We thank the reviewer once again for his/her time and effort reviewing our manuscript, both in the first round and this round of review. We are glad that the revision has enhanced our work and resolved the raised issues. Especially, we are pleased to know that the changes mentioned by the reviewer has improved the overall work:

The authors have taken into account my remarks and changed the manuscript accordingly. In particular, there are a few changes in the paper that I found very interesting and that I think have improved the overall work. These are listed in the following.

The authors have discussed the algorithmic complexity of their approach from an analytical and numerical point of view. I believe that this discussion was essential to make the reader aware of the limitation of the presented method for large size networks.

For large temporal scale \beta the authors have developed an approximate method which reduces this complexity by a factor N and still makes accurate predictions. This method only computes the first two eigenvalues of the Laplacian matrix, instead that the full spectrum, and the entropy is approximated accordingly. In Section Discussion, the authors acknowledge the need of further studies to extend their approach at the meso-scale, where the aforementioned approximation fails.

The authors have included a discussion on the quantification of the error done through the mean-field approximation in their method.

Further, their numerical investigation now includes both static and adaptive approaches and comparison with a larger fan of methods.

With all the edits and new results I believe the paper has very much improved.

We thank the reviewer for his/her encouraging view of our revised manuscript.

I do have one more suggestion that I would like the authors to consider. The main results in term of mathematical formulas are now shown only in the Method section, with the risk of being missed by the reader. I believe that the paper would benefit if the final results for the entanglement and the large \beta approximation of the entropy were shown in the main text (as they were in the previous version of the manuscript). I think this would further help the reader interpreting the method and understanding the figures before even reading the Method Section.

We thank the reviewer for this comment. We agree that including the derivations of entanglement at extreme scales can increase the readability of the work. We moved the mentioned derivations to the main text.

We thank the referee once again for his/her valuable comments, which certainly enhanced our work.