We regret to advise you that the Reviewing Committee is unable  
to accept the subject paper for publication as a PES  
Transactions paper.  
  
Enclosed please find the comments of the reviewers which should  
serve to explain the recommendation of the reviewing committee.  
I hope you will find the explanations satisfactory.  
  
Prof. Nikos Hatziargyriou, Editor in Chief,  
Transactions on Power Systems  
  
COMMENTS TO THE AUTHORS:  
Editor's Comments:  
  
Editor  
Comments to the Author:  
The authors are interested in switching actions within distribution systems and accompanying that action with an optimal voltage phasor regulation method. The editor has two primary comments: 1) how this work relates to previously published work. Note that one reviewer mentions that a part of this work is copied from a prior paper. If this is the case, the authors should note that the paper should be sufficiently distinct from prior work (and do not self-plagiarize). On that note, since at least one reviewer has expressed concerns over the clarity of the presented approach, the authors should consider rewriting it to be more direct and easier to understand. Also, at least one reviewer has commented on whether this work is a valuable contribution relative to how similar it is to prior work. 2) There are distinct characteristics for distribution systems. Multiple reviewers are concerned over the assumptions that have been established for this approach relative to the distribution system. Some of these assumptions go strongly against the characteristics of the distribution system. The authors must thoroughly justify their approach as being practical relative to realistic distribution systems within this paper for this work to be of value. There is potential in this work (and related work) associated to OPF formulations for the distribution system but there are key aspects that are lacking related to this work at this time.  
  
Reviewers' Comments:  
  
Reviewer: 1  
  
Comments to the Author  
This paper relates to distribution engineering.  The authors attempt to use elements of optimal power flow studies to make the operation of a distribution system better in some sense.  The system is assumed to have high levels of distributed generation.  
  
The real challenge in distribution systems with distributed generation is that most such resources are not dispatchable in the usual sense.  This is not well captured in the present paper.  There are controls which could be selected to optimize performance in some sense (e.g., economic operation, minimizing losses, maximizing resilience to outages of assets), but these seem to be missing from this paper.  Modelling of PWM inverters as well as other types of interfaces with distributed generation is also a challenge, and this is omitted from the paper.  
  
The paper reads like a treatise in constrained optimization, and this is a positive characteristic.  But the level of attention given to distribution engineering issues are not well done.  For example, most laterals in distribution systems are single phase, and this results in mixed single and three phase models.  The mutual coupling in the system must be modelled carefully.  Transformer connections need to include open delta connections as well as other types.  Modelling of the ground circuit is missing in the paper as is the treatment of the neutral conductor.  High values of R/X ratio need to be accommodated.  The presence of negative sequence voltages at the substation need to be included in the three phase primaries.  Voltage regulators must be included – not omitted as in the paper.  Fortunately, there are commercially available tools that accommodate many of these complexities (e.g., OpenDSS).  
  
For the reasons cited, the paper is found not to be of high value in distribution engineering.  However, there may be value in optimization theory.  
  
There are quite a few typos in the references (e.g., ‘Ieee’ vs ‘IEEE’ in [21];  repeated year of publication in several references;  journal titles written in reversed order as if copied and pasted from IEEE Explore).  The overuse of the word ‘we’ is tiresome.  
  
  
  
Reviewer: 2  
  
Comments to the Author  
This paper incorporates an existing linear power flow model, the LinDist3Flow, into an OPF formulation which is used to maintain the voltage phasor profiles throughout a feeder. Despite the fact that the LinDist3Flow which has been presented in a published paper can no longer be regarded as a contribution of this paper, it has a common drawback that exists in many so-called linear power flow models: some nonlinear terms in the original power flow formulations are simply omitted without showing the rationality.  
  
Main comments:  
1.      Whether the assumptions A1-A5 are reasonable has been shown in neither [15] nor [16]. Proofs are required to show that these assumptions are rational.  
2.      Sections II.A – II.C are simply copied from reference [16]. I don’t think this is necessary. Only some key statements and power flow formulations should be retained.  
3.      The optimization model presented in Section III is questionable: a) we do have magnitude references for bus voltages (it is generally 1 in p.u. and may be a little bit higher for some hub nodes), but we don’t have an angle reference for the voltage at a given bus which is not the slack bus; b) why are the weights 1000, 100 and 1 respectively? 3) I don’t think it is a good idea to control the active power output of DER which is a kind of sustainable resources. Conversely, we should maximize it.  
4.      The case study is weak. Simulations on the IEEE 13, 123-bus feeders are suggested. A test on a real-world feeder is also preferred if applicable. Switching actions are important in the proposed approach. However, only one switch is considered in the presented test case. Studies on larger systems, e.g. 123-bus feeder, with multiple switches are strongly recommended.  
  
Minor comments:  
1.      Page 1 Column 2 Line 56 and P3C1L40: reference [9] did not omit the loss term and the DistFlow model therein is nonlinear. Please check that.  
2.      P2C2L59 and P3C1L49: is reference [10] really about Dist3Flow? I think [10] should be [15].  
3.      P5C2L50: 755 should be 775.  
4.      A distribution system is not necessarily radial [A]. “Radial distribution systems” should be emphasized in both the title and the abstract section.  
  
Reference  
[A] G. Heydt, The next generation of power distribution systems, IEEE Transactions on Smart Grid, vol. 1, no. 3, pp.225-235, 2010.  
  
  
Reviewer: 3  
  
Comments to the Author  
  
This work develops novel linear approximations of multi-phase radial networks with the goal of bringing optimization methods (e.g. OPF) to the management of energy resources in distribution systems.  In the interest of linearity, the approximations adopt some typical assumptions, the network is losses [9], voltage magnitudes are 1.0, and sin(x) can be approximated by x.  
  
By-in-large I find that this is a strong paper with significant potential.  However there are 5 key points that require clarification.  
  
  
1) Contributions Relative to [15],[16]:  
  
From the reading of the paper it is not entirely clear how the contributions of this paper are related to [15],[16].  At the very least, the "angle equations", Section 2.D are new.  However, for Section 2.C, should we be viewing this paper as the Journal submission of [16]?  Or is there a separate submission focusing just on [16], and we should consider Section 2.C prior work?  
  
  
2) Validation by Simulation in Section 3  
  
At this point, it is not clear if a full non-convex multi-phase AC power flow simulation was used to validation the results presented in Section 3.  This is a critical point; If the results are simply the raw outputs of the linearized power flow, then they will be optimistic as to the quality of the control solutions produced by the linear approximation.  
  
If an AC simulation was used, this section would benefit from a details on which simulation tool was used.  
  
  
3) What Relaxations are For  
  
There are a number of points in the paper where the authors claim that SDP relaxation cannot be used for their problem of interest because it does not reliability return Rank 1 solutions.  I am sure this is true.  
  
For some time, relaxations have been miss-represented in the literature as alternatives to non-convex optimization of the power flow equations.  By-in-large this is not true, as the author's experience shows.  
  
We now know that even on radial topologies, the AC-OPF problem is NP-Hard [r3].  Furthermore, on realistic test cases, the optimality gap is often >1% [r4].  As noted in [r4], the primary value of relaxations is not to produce feasible globally optimal solutions, it is to produce guaranteed lower bounds on the objective value of the optimization problem.  Feasible solutions are better left to heuristics and approximations, such as the one developed in this work.  
  
I suggest framing the SDP relaxation in this work, to reflect these recent results.  
  
  
4) Radial vs Meshed Networks  
  
The paper should more clearly state that it focuses on radial systems.  This network structure has significant implications on the types of relaxations and algorithms can be used to optimize the network.  
  
For example, it is known that in a radial system, the SDP relaxation and the SOC relaxations are equivalent [r1].  This suggests that the much faster and more reliable SOC relaxation [r2] can be used in place of SDP relaxation, on these topologies.  
  
That said, the introduction of (32) does in-fact allow this work to extend to meshed networks, although that is not studied here.  Another option would be to extend this work to also consider meshed networks in the validation studies.  
  
  
5) Some Related Works  
  
There are a few related works that this work should be aware of.  [r5] proposed SDP relaxations on multi-phase networks based on the DistFlow/BranchFlow formulation as well as a simple linear approximation.  It would be helpful to understand this work in the context of [r5].  
  
Following the citations of [17],[18] in Section 1, [r6] represents a state-of-the-art approach for both reconfiguration and power supply restoration, in a single-phase context.  
  
[r1] S. Sojoudi and J. Lavaei, "Physics of power networks makes hard optimization problems easy to solve," 2012 IEEE Power and Energy Society General Meeting, San Diego, CA, 2012.  
  
[r2] R. A. Jabr, "Radial distribution load flow using conic programming," in IEEE Transactions on Power Systems, vol. 21, no. 3, pp. 1458-1459, Aug. 2006.  
  
[r3] K. Lehmann, A. Grastien and P. Van Hentenryck, "AC-Feasibility on Tree Networks is NP-Hard," in IEEE Transactions on Power Systems, vol. 31, no. 1, pp. 798-801, Jan. 2016.  
  
[r4] C. Coffrin, H. L. Hijazi and P. Van Hentenryck, "The QC Relaxation: A Theoretical and Computational Study on Optimal Power Flow," in IEEE Transactions on Power Systems, vol. 31, no. 4, pp. 3008-3018, July 2016.  
  
[r5] L. Gan and S. H. Low, "Convex relaxations and linear approximation for optimal power flow in multiphase radial networks," Power Systems Computation Conference (PSCC), 2014, Wroclaw, 2014, pp. 1-9.  
  
[r6] H. L. Hijazi and S. Thiébaux, "Optimal AC Distribution Systems Reconfiguration," Power Systems Computation Conference (PSCC), 2014, Wroclaw, 2014, pp. 1-7.  
  
Reviewer: 4  
  
Comments to the Author  
The paper develop an approximate model for distribution power flow that  
could be subsequently incorporated into optimal power flow problems. The  
work is well written, well organized, clear figures and tables, but there  
are several questions that the authors need to answer.  
(i) What are the main contributions of the work?, for this reviewer is not  
clear. The authors are expected to clearly identify and demonstrate the  
novel contributions of their paper.  
(ii) Discuss the studies cases, more details are necessary.  
(iii) Section III has to be expanded with new cases, preferentially using real  
systems.  
(iv) The authors should comment in more detail the limitations of their  
proposed methodology.  
(v) Discuss the difficulties of applying the proposed methodology in  
solving real systems.  
(vi) Concerning of an approximate model for distribution power flow, authors  
may wish to discuss [A].  
  
[A] J. F. Franco, et all, “A Mixed-Integer Linear Programming Model for the  
Electric Vehicle Charging Coordination Problem in Unbalanced Electrical  
Distribution Systems”, IEEE Transactions on Smart Grids, vol. 6, no. 5, pp.  
2200-2210, Sep. 2015.  
  
  
Reviewer: 5  
  
Comments to the Author  
Your nomenclature table is incomplete. It does not capture everything. Please update. This makes it difficult to read.  
  
The authors state that the substation control is completely independent of the DER control actions. They state, “While the substation voltage may evolve over time, we assume this evolution takes place independently of DER control actions in T.” Please clarify why this assumption is appropriate, what basis you have to make this assumption. The control actions of DERs can affect the voltage at the substation and, more importantly, much of the push in this research domain is to use DERs in such a manner and coordinate with the bulk transmission system. This would go against your assumption.  
  
The authors do not need to explain what the imaginary number is, the square root of -1.  
  
You should not use y and Y for your square magnitude of voltage and the vector of square magnitudes of voltages. We use y and Y related to admittances and then the admittance matrix. This is not just common for power engineers but in basic electrical engineering. This just makes your work confusing and harder to follow. Please rewrite.  
  
Before (9), it seems that you are saying that you will ignore losses. The first comment I have, the clarity of what you are doing could be improved. If you are going to ignore losses, just say so instead of the reader having to trace back what Lmn is, where it comes from, its relationship with other pieces, etc. Also, I am not sure what is the point of spending the prior time talking about Lmn in the preceding paragraphs and including it in the equations if you are going to ignore it in the end. I think being up front about the simplifications you wish to make would help. Finally, the question comes up regarding whether you should ignore losses (or assume they are constant) in the first place. The authors should clarify what is the impact of their approach based on the fact that you want to ignore losses? How will this approach vary from other approaches that do not have to ignore losses (or assume they are constant while you manage the DERs)?  
  
The authors are deriving well-known power flow equations and then linearizing them. They need to make an attempt to clearly explain how these linearizations related to linearizations of non-convex power flow equations that have existed for decades.  
  
On page 4 they first state that linearization can occur if they assume that the ratio among the phases stays constant (A3). So they want to assume that the voltages across the three phases move together. Distribution systems are often unbalanced and this assumption will be invalid. The voltages will not swing together as assumed by A3. The phases can easily feed different load types even (residential vs commercial) with different load profiles. This would be, for instance, to assume that if one phase experiences some form of uncertainty due to its load, so should the others. There are even resources that can act independently on a single phase to adjust voltage without the same adjustment happening on other phases. The authors need to address the validity of this assumption for the distribution system and its particular characteristics.  
  
Hmn(i), after (13), is later assumed to be zero. The authors have done a bad job describing the model and approach. It takes some effort to figure out what Hmn(i) is (the square of the individual phase voltage drops across the lines) to confirm whether this assumption is valid. Instead, they use notation that is not straightforward with equations and terms (not found in the nomenclature) that are not adequately described forcing the reader to dig through the various pieces and put everything together. Please work on being clear about what your assumptions are and whether they are valid. It is important for you to confirm that you have a model that accurately captures a distribution system and its characteristics.  
  
There is a push to include distributed resources within such models. Some of those distributed resources induce additional variability and uncertainty on operations. I am bringing this up because this approach makes various assumptions that not only go against distribution systems that do not have high levels of DERs but would further go against distribution systems with high levels of DERs. For instance, A3 goes against this assumption. A4 is against this assumption. And absolutely to assume that the voltage in the distribution system is 1 pu is not just strongly against actual operations within the distribution system but also against a distribution system that has high DERs.  
  
Beyond the prior comment, this approach does not include other aspects of the distribution system: voltage regulators, capacitor banks, transformers, - much of which could be used for this sort of application. There are other aspects to be considered, proper modeling of the DERs.  
  
Switching actions at the substation are common. For instance, there are automated special protection schemes that transfer load from one bus to another given a contingency. Switching in the distribution system also occurs. Of course, this doesn’t mean that all switching actions are not harmful. But the audience needs more insight to know when it is a concern and when such an action is not harmful to know when and where to use this technique and whether it does the job it is intended to do. The authors need to provide more information and insight as to when and why this is an issue. One fundamental piece that seems to be missing is the dynamic simulation of the distribution system during the switching action. You want to lessen the impact of the switching action. We need to have some form of analysis related to the impact of the switching action beyond the plots on page 6.  
  
Figure 2a and 2b: The authors should describe what is the benefit from moving from (a) to (b). Both plots stay within voltage bands. Your approach keeps all voltages closer to 1. There are bands on the voltage to maintain power quality. There is not a mandate to keep voltages as close to 1 as possible. The requirement is to stay within the band. Now, uncertainty can play a role in this (and other factors) but the authors should clarify the reasons why 2b is preferred. Some researchers support the idea of penalizing voltages that go away from 1pu in optimization problems for T&D. The research community varies on such an opinion (should we restrict voltages to stay within predefined lower/upper bounds or should there be a penalty on/bias against voltage deviations away from 1 pu eve when the voltage stays within the bounds). The authors’ perspective seems to make the assumption that we should maintain that voltage as close to 1 pu. This is debatable. I also suggest the authors look up IEEE standards for power quality and voltage for the distribution system to compare what they are doing to existing standards and requirements.  
  
Minor, Not sure why but page 5 has different paragraph spacing than does page 6. See below Fig 1. Try to check your formatting a bit more.  
  
Summary:  
I find it to be a must that the authors clearly describe their modeling assumptions. My suggestion: I think it would help them to have a section at the start of the methodology section that goes over the assumptions beforehand in summary and shortly state whether such assumptions are valid for this application and the distribution system. If your assumptions are problematic for a particular setting or for whatever reason, the reader is not going to want to read the entire paper and check every step and derivation tracking from assumptions back through your work to then find out that this approach won’t apply (as I had to do reading the paper as a reviewer).  
  
Second, this is on verification, the authors need to verify that the assumptions they make match well with distribution systems and distribution system characteristics. You should solve your model and compare its accuracy to that of a precise distribution system power flow model, with more features than what you are assuming. Also, I do not suggest the authors refer to some other work but do the verification themselves for this application. I understand that you have attempted some form of this in section III to prior work in [12]-[13] but really I doubt whether that comparison is appropriate. You should look into mixed phase systems with voltage ratios that do not go as you assume with voltages not a 1 pu as you assume, etc. to see when those assumptions breakdown and how that affects your approach.  
  
Third, on alternatives, there are various other existing control capabilities in the distribution system that exist. You need to communicate why such control mechanisms are not sufficient, why other approaches would fall outside existing power quality and voltage standards, and why this work would be of use.