Oral Discussions on Session: "Stability Assessment" – Part I

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Abstract

This paper contains the first part of the transcribed oral discussions of Session "Stability Assessment" of the 2013 IREP Symposium-Bulk Power System Dynamics and Control, held on Monday afternoon, August 26, 2013. Papers [1]-[4] were presented.

Discussion

Chair: I see some raised hands. I guess Bernie was first.

Bernie Lesieutre (University of Wisconsin): I have a question for the second paper presented by Sarai Mentoza [2] about operating the system with a certain amount of damping and redispatching generation to make that happen. Operating system with a certain amount of damping is an excellent idea. I think that is a great idea. My concern about the paper at this stage is that there are a lot of control systems on generators to achieve that level of damping. And I think it probably should be considered that different optimization variables, such as control systems settings and set-points could be tuned instead of doing redispatch, which has economic consequences. The decision variables maybe should be the control setpoints etc., in order to achieve that damping.

Sarai Mendoza-Armenta (Iowa State University): Sorry, did you make a comment or a question?

(Laughter)

B. Lesieutre: It was a comment, but I would like to hear your thoughts on it.

S. Mendoza-Armenta: OK. Thank you. In this moment we are thinking about generator redispatch. We are not thinking about another way of damping this kind of oscillations. But it could also be interesting to include the variables that you suggest in your comment.

Rodrigo Ramos (University of Sao Paolo): I have a comment, a question, for the last presenter [4] (Tilman Weckesser). In your method, you compared different generator models for transient stability assessment and from what I understood from what you presented, you used the same type of models for each generator in the system, is that correct?

Tilman Weckesser (DTU): Yes, we were representing all the generators by a second-order model or by a third-order model.

R. Ramos: So you had second-, third-, fourth- and sixth-order models. And the IEEE standard that you mentioned recommends uses of these models for certain types of generators, for instance, fourth-order model for round rotor generators, classical models for equivalents of large distant areas and systems, and others for salient pole. Is it realistic to consider all the generators with the same model in the system and wouldn't it be more fair assessment to consider the differences between the types of generators in the system?

T. Weckesser: Yes, that is right. From the information that we had on the generators, we had for all generators the sixth-order model representation, but that would definitely be an improvement if we have a specific generation type and we can decide that this one should be sufficient by representing it by a fourth-order model. That would be an improvement, yes.

R. Ramos: OK. And the last question. How do you know what to expect with respect to the critical generator. You mentioned that there is some behavior that would be expected from the critical generator. Did you get it by simulation or used real data for it? How do you know how the critical generator is supposed to behave to assess the correctness of your results?

T. Weckesser: We didn't have real data, a report or anything, so we assumed that the recommended model is of the sixth order, and we take that as a reference. If we see that the critical group in the sixth-order model simulation is Generators 6 and 7, we say that this is our measurement and when we reduce the order of the generator model we want that those generators again are the critical ones.

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R. Ramos: Ok. So a very quick last comment.

Voice off microphone: I thought you already asked the last question!

R. Ramos: I am sorry about that. The sixth-order model may not be your benchmark for all generators because not all of them have the damper windings associated with the extra equations.

T. Weckesser: In the test system that I have in the paper I had the data for all generators, so they were all represented with the data available for the sixth-order representation.

R. Ramos: Thank you very much. Sorry for the extra question.

Chair: No problem we have lots of time.

Misha Chertkov (Los Alamos National Labs): I have a question to the first author [1] (Jan Lavenius). My understanding is that you are computing expected security cost, stochastic optimal power flow. To me it sounds like you minimize expectation under certain conditions, which are probabilistic conditions. You used Monte Carlo for that, right?

Jan Lavenius (KTH): Yes, to construct it.

M. Chertkov: Yes, so this is about security, right? This is supposedly about something which happens very rarely. Doing Monte Carlo, standard Monte Carlo, typically you get zero in your expectations. I am guessing you are using some kind of importance sampling. My question is about what kind of techniques do you use. There are plenty of techniques in the Monte Carlo literature to access those rare event situations. My question is if this is a problem of this type, and if yes, how you do it.

J. Lavenius: First I think there is one problem involved in constructing this security boundary approximation and for that we use Monte Carlo. But then we have the stochastic optimal power flow, which personally I have not been doing, it is the application of this method that we will have a presentation on later in this IREP Symposium, then we try to minimize the expected security cost for contingencies. If we talk about the Monte Carlo sampling, in this case we used Gaussian distribution around it.

M. Chertkov: So you do not have this issue of a typical event giving you nothing about figuring out these boundaries. That's what you are telling, right?

J. Lavenius: Yes. So, if we extend this method, we should consider which contingencies we are investigating these boundaries for. We haven't really discussed this thing in this paper but it is something that should be done.

Thanos Koronidis (IPTO): I am from the Independent Power Transmission Operator of Greece, the local Transmission Operator and I have some comments on the second paper [2], presented by Ms. Mentoza-Armenta. I think you address a very realistic, pragmatic problem. As the systems grow larger you will see and we will see interarea oscillations. And this is the experience we had in Europe when we did the last big expansion to the East that was the connection of Turkey to the European system, since it is a big system of about 50,000 MW (40,000 MW now) connected through a rather weak interface to the Balkans and the European system. So, the analysis we did before the interconnection revealed that we would expect some interarea oscillations in the order of 0.15 Hz, which was confirmed after the interconnection. To go towards the direction of your suggestions, it was found by the analysis that depending on the direction of the flow and on the magnitude of the flow between Turkey and Europe, you would see more or less damping. So the conclusion was that when Turkey would be exporting to Europe, we would see less damping, worse conditions; the other way, things were better. Of course, it didn't cross our mind to try through redispatching to resolve the problem. First, we didn't know how to do that, and second it is quite serious to do redispatching, because you conflict with the interests of the market, and nobody likes to do that. So the measures that have been taken and have been proven quite successful so far (we have a parallel operation for three years now) was to try to retune the Power System Stabilizers of the units to the new frequency (although it is not easy to stabilize frequencies of 0.15 Hz; it's much easier to stabilize frequencies around 1 Hz), but it helps a little bit. The other thing was to try to add filters to all the SVCs in Turkey (and they have a lot of those because they have a lot of steel mills of 3,500 MW) and so these filters would trigger an operation of SVCs towards doing the damping of this 0.15 Hz oscillation that we call Turkish oscillation, if it shows up, and it shows up from time to time. And the third thing we did (we asked them to do and they did it quite successfully) they added a StatCom device in an area with mainly resistive load and this StatCom was also equipped with a filter, which would detect this frequency, when it would show up. So, if the frequency was going up, if this oscillation was triggered, it would increase the voltage, so a small amount of a few MW would be absorbed by the local loads in the area, where the StatCom was installed, and when the frequency was down the voltage was decreased. So, this was providing in reality adequate damping to solve the problem for three years. Although we have quite a lot of events triggering of this frequency, always the oscillations have been damped in a few cycles, and they disappeared. This was my comment. Thank you.

Chair: Would you like to respond to this? It is a supplement to your paper I guess…

(Laughter)

S. Mendoza-Armenta: Yes! No response.

Claudio Canizares (University of Waterloo): This is a question for the first paper [1] (by Jan Lavenius) regarding finding these boundaries. There was a work done by Prof. Annakkage at the University of Manitoba a few years back. He was looking at approximating boundaries, finding boundaries, using polynomials [5]. He used different types of polynomials. He was trying to obtain polynomials of higher order than the second degree because in some cases, particularly when you have limits, these boundaries start to have funny shapes, sort of sharp corners. My question is first regarding the fact that in your simulations you didn't consider you had those limits, you know having the system switching from one mode to another different mode of defining that boundary. The second question I have in comment is regarding… When we looked at this problem we proposed neural networks, because it was a fact that the polynomials were not doing a very good job, particularly in certain cases, as you increase the size of a system, these boundaries become much more complex. We had no problem with non-convex boundaries, we were able to find time. So what I was curious about was your experiences about those edges and the approximations that you have. And if you can comment on that, I would appreciate it.

J. Lavenius: Can you remind me again about the first comment?

C. Canizares: Second order polynomials were not very good approximation as demonstrated by Prof. Udaya Annakkage at the University of Manitoba. Probably you should read the paper.

J. Lavenius: Yes, very interesting. As you may have seen, one possible improvement of our method was to use higher order approximations. I think Camille Hamon when presenting his work on stochastic optimal power flow problem will discuss this a little bit more than I have done. That is true. But the work done by Prof. Annakkage was it on transient stability boundary?

C. Canizares: Yes and we did similar work with that. My question was regarding the model you presented. Can you describe the system a little bit?

J. Lavenius: Yes, it's a 9-bus system with the AVRs and excitation limiters implemented, so we ran the full time-domain simulation and during the simulation we checked for angle instability, to determine whether an operation point was stable or unstable. But there are of course many techniques that we could, or should, use to reduce the time, because the time required for these time-domain simulations is what takes most of the time …

Alex Papalexopoulos (ECCO International): My question is for the first paper by Jan Lavenius [1]. I really enjoyed it. Good presentation. The question is: when you approximate the security boundary using Monte Carlo, you showed us an example with one contingency. For practical systems, you have a series of contingencies, one after the other that are caused by the same event, let's say weather. So you have multiple contingencies, you have cascaded contingencies. The question is: have you thought in more complex cases, when you simulate realistic cases, how this technique, which I enjoyed, will apply, given that the use of Monte Carlo becomes much more complicated and the computational burden is also much more taxing… I don't know the details of your Monte Carlo, how you do this, but nevertheless that's an important area if you want to come up with some realistic results. Again, multiple contingencies, cascaded contingencies caused by the same event. Thank you.

J. Lavenius: Yes, since we have multiple contingencies that we need to consider, in principle we have to do one time domain simulation for each combination. If you have a sequence of cascading events, it becomes very time consuming. So, that is part of the rationale to do this off-line and possibly using super-computers or distributed computing, which is one of the topics that i-Tesla is researching. As for the practical applicability, I think it shows promise, but I don't think there are any guarantees when we are talking about very large systems, multiple contingencies and of course we would like to have as many sample points from this Monte Carlo technique as possible. That's why we discuss that the Monte Carlo technique has to be refined, so that we investigate the boundary more closely than other points in the parameter space. So, I agree.

Mani Venkatasoubramanian (Washington State University): A comment on the paper on the power redispatch and oscillation modes [2]. And I just wanted to comment following upon Bernie's question. Normally when we talk about improving a mode by some action, we broadly divide into whether the actions are meant for off-line (and that may include changing control parameters), or real time, which may be something like generation redispatch or tie-line redispatch, which is commonly what we preach. In terms of specific actions my question is: normally we say that this depends on the type of mode.

Whether it is a local mode. Typically when there is a problem with the local mode we recommend reducing the generation output of the plant to improve the damping, whereas when there is an interarea mode, it is more tricky. Such as the example that the gentleman from Turkey…

T. Koronidis: I am not from Turkey. I am from Greece!

M. Venkatasoubramanian: Sorry, from Greece. The example that you provided, the controls that you have implemented are ingenious controls. And we would like to see publication of these controls from industry. And the question I had is: is the method able to distinguish such as whether it is a local mode or an interarea mode, and thus come to the decisions and the redispatch that the method derives.

S. Mendoza-Armenta: Sorry, can you rephrase your question?

M. Venkatasoubramanian: The action that is needed may depend on whether the mode that is poorly damped is a local mode, which mainly involves one generating plant, or is an interarea mode or an intra-area mode, there are these different classifications that we use in the power literature. And my question is: in the examples you have mentioned so far, can you see the difference in the actions? Normally what we do in practice, if it is a local mode, we recommend reducing the power output of the generating plant, whereas if it is an interarea mode the Operator may have to adjust some tie-lines to improve the damping and it is still not clear which tie-lines to change and your work may provide some insight in that direction.

S. Mendoza-Armenta: Thank you. We are concentrated in this study on interarea oscillations. We are not treating local modes. Local modes, people know better than interarea modes. So in the case of interarea oscillations, as you pointed out, the formula is showing us which lines is good to try to use to make the redispatch. Just that.

M. Venkatasoubramanian: It would be good to see in some of your examples if you could come up with some recommendations, such as for the 0.2 Hz or the 0.3 Hz mode it would be good to lower this and that tie-line flow. I have another question on the formulation. You have assumed constant voltage behind the generators. Generally from experience we know that interarea modes are very sensitive to excitation controls that are behind the generator internal voltage. So by ignoring this, some of your results may not be accurate.

S. Mendoza-Armenta: We are assuming that the overall dynamics of the generators is included in the swing equation. And this is the first part of the work we are developing. We are still working on that equation. We would also like to include other things but this is future work.

Raphael Mihalic (University of Ljubljana): For the last presenter [4] (Tilman Weckesser). Perhaps it is a silly question. You have diagrams of stability margin vs. time. Which time is it?

T. Weckesser: Because we are using the SIME method we are acquiring new measurements or new data sets and with that our stability margin is refined. The time is the time in the simulations.

R. Mihalic: It is not the critical clearing time.

T. Weckesser: No, no, it is simulation time. Since we are trying to stop the simulation early, in each simulation step we are determining the transient stability margin. It just shows that it needs some time to go to constant values.

R. Mihalic: Ok, thank you. And you haven't tried with different models to determine critical clearing time to see the difference. Have you or not?

T. Weckesser: No, I haven't done that yet.

R. Ramos: I promise to be quick this time. The question is to Juan Munoz [3], the presenter of the third paper. Actually it is more of a comment than a question, but I would like to hear your thoughts on that. You are using your approach for voltage stability and transient stability assessment. The results that you get for the transient stability assessment are the time responses of the generators. Is that correct?

Juan Munoz (University of Waterloo): That's correct.

R. Ramos: So, is there anything that would prevent you from extending your work to the small signal stability framework by using modal analysis?

J. Munoz: In principle, the way that we define the differential-algebraic equations is a general way. So, I think there is no problem in extrapolating the application of this method in the way that it is formulated. But in terms of some limitations due to the method itself, there is not any work.

R. Ramos: That goes as a suggestion for further work. Thank you.

Chair: Any more questions? Maybe one question by myself then. The affine arithmetic [3] is it scalable to large systems and are there any benefits when you go to large scale systems? Is that for small systems?

J. Munoz: The main idea of this method is to have a computationally efficient method to estimate these bounds. As the size of the system increases, this time is critical too, because you see, for example, simple Monte Carlo simulations will take even more time as the system size increases. Of course we are working on that, applying this method to large-size systems and what we expect in terms of the behavior of the method, is that, as we see for example, when we increase the size of the intervals, of course we are expecting more errors when we compare it with Monte Carlo. So, as we increase the size of the system, the number of equations increase and that implies the number of the non-affine operations that needs to be approximated using affine force also increases. So the total number of additional noise signals increases and we are going to expect a higher error in the final estimations. But at the end of the day, what we are looking is to create a method that is computationally efficient as an alternative to commonly used sampling based approaches and we don't require assumptions regarding PDFs.

Chair: Thank you very much. There is another question. We have some more time. Go ahead.

Steven Low (CalTech): I have a question for the fourth speaker [4] (Tilman Weckesser) on the impact of different model details on stability. So, let's assume that the 6th order model is the most accurate in reality. Is it possible to systematically derive reduced order models in a way that is always more conservative, or more aggressive? For example, is it possible to develop from the sixth order model reduced order so that the stable region of the reduced model is always inside the stable region of the sixth order model, for example? In which case, if the reduced order is stable, then you know that the more accurate model is stable as well. Similarly, you can do it the other way. Unstable region is in the unstable region of an accurate model. Is it possible to develop such models? The low order models that you show, sometimes are more conservative, sometimes are less conservative and so on. Is it possible to systematically devise reduced order models from the sixth –order model so that the reduced order is always more conservative or less conservative?

T. Weckesser: As you said, for the low order models I was looking into there was no clear trend that it was always more conservative or more optimistic, and I agree it would be very interesting to find low order models which are always more conservative. I have to say I haven't looked into that, but I think it is a very interesting idea and I would like to think about it and look into it.

Chair: Any more questions? At this point I would like to thank the authors and the presenters.

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[3] J. Munoz, C. Canizares, K. Bhattacharya and A. Vaccaro, "Affine Arithmetic Based Methods for Voltage and Transient Stability Assessment of Power Systems with Intermittent Generation Sources," Bulk Power Systems Dynamics and Control – IX (IREP), August 25-30, 2013, Rethymnon, Crete, Greece.

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