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

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Abstract

This paper contains the second 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]-[3] were presented.

Discussion

Rodrigo Ramos (University of Sao Paulo): My question is to Mani Venkatasubramanian [3]. You mentioned at some point in your presentation that you use white noise, Gaussian white noise for modeling ambient data, ambient samples. How fair is it a model of the noise that really happens in real power systems, given that you have access to real data?

Mani Venkatasubramanian (Washington State University): This is a very good question, because we get this question from other researchers as well. I would start by saying that this is not an assumption that is exclusive to power systems. This assumption is commonly made in ambient model analysis. It is also used, for instance in civil engineering, where they study the structure of bridges. They have disturbances from cars and buses going over the bridge. And then they want to detect if any of the structural modes becomes affected because in that case the bridge will collapse. So that's a big concern for civil engineers. There is literature research in aeronautics, in airplanes, so this is a problem across several domains. Now specifically if we transfer it to power systems, we assume that fluctuations from the loads are random and it is true for a short period of time. Whereas if you look at long periods of time, such as 30 minutes or 20 minutes, there is a ramp also that goes with the load response. So, then it becomes more tricky to assume that perturbation is strictly Gaussian. So, there are some limitations to the assumption, but in reality we have seen that the assumptions work well, because we have this running in several companies and there are other researchers. Here we have Claudio who has looked at this problem, he wrote one of the early papers on subspace identification and then you know, Dan Trudnowski and John Hauer, They have been working on this one for many years. So, this is a well-known assumption and it appears to work well in the real systems, because we get excellent results. I showed some examples and we have written several other papers on such fields.

Sandro Corsi (Consultant): A question to Mani Venkatasubramanian [3] about the voltage instability indicator you use based on the sensitivity, the ratio between the reactive power variation and voltage variation. When this ratio approaches zero, you have a high risk of voltage instability. My question is on your experience on the risk value of this index, because, in my view, when the voltage instability risk increases, the voltage is reduced, but even if the reactive power injections of the generators are limited by the over-excitation limiters, the line discharge still continues, so the reactive power flow in the bus is still active at the collapse beginning. So, which are the values of this indicator that alarms on high voltage instability risk.

M. Venkatasubramanian: That's again a very good question and it is a tricky technical question. I would like to clarify that the sensitivities we talk about is not $\Delta Q/\Delta V$ with respect to the line equations. What we assume is that we are having the line perturbed from loads at the two ends of the line, then it is like a QV plot. The system is getting perturbed form various points in the system and then we try to calculate in the current system, how much is the variation, or how much is the sensitivity, of the line flow to the voltage and our aim or the motivation, which we describe in our paper [3], is to calculate the slope of the QV curve at each bus. And when that slope becomes zero then we know theoretically this is a problem for the system. But to do that we need to have access to several lines at the sub-station. Just looking at the value from one line is maybe misleading, because the problem may be tied to a different part of the system which this line is not connected to. And also I will give you the answer to the other question. We have seen that the approach works well in real systems. We had one case where we saw that the sensitivity drops significantly on one of the PMUs, whereas there was no indication of anything going on in

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the system. So we went back and checked with the company and the company later told us that operators reduced power from another part of the system, and in the part where we saw the sensitivity drop, they had increased the generation. So it was putting out more, so that part of the system became more stressed. And the good thing for us, the validation, was that we didn't know. We got this from the measurements and we were able to get some verification that our analysis was going in the right direction. And also there was another case where there was voltage collapse in one part of the system, which is traditionally considered strong, but they had two thermal units and it turned out that on that day one unit was off-line and the other unit had a fault and tripped out. And when the two generators which were supporting the bus tripped out, that bus became a weak bus. And we saw that sensitivity dropped significantly. So that is again an indication. Going back to what you were saying, it is sensitive to generators. You are absolutely right. And you cannot predict what is going to happen, which is one of the weaknesses of the approach. But when it becomes weaker in the system, we see that in the data.

Claudio Canizares (University of Waterloo): This is a comment and a question for the first paper by Marian Anghel [1]. My comment regards that transformation you mentioned about cosines and sines, which is interesting. It is something, if I recall correctly (I am getting older and my memory is getting weaker), that Thomas Overbye did in the late '80s, early '90s, when he was looking in a similar approach, basically a transformation of the power equations into polar coordinates, something similar to that. The other comment is regarding the issue of Lyapunov functions, when you have all these controls included in the model, which makes the Lyapunov functions to become much more, if you will, pessimistic, with respect to the actual behaviour of the system. Now, with that in mind, my question is regarding the separation of the system. When you separate your system based on Jacobians, it depends on which point you compute the Jacobian. The more stressed the system, you end up with different Jacobians. So I wonder how that change in the system as it evolves, how is that factor affecting the approach of dividing the system in different areas to basically combine this Lyapunov approach? Thanks.

Marian Anghel (Los Alamos National Lab): These sum of square techniques, these algebraic techniques are very flexible. And I can point you to a very nice review paper by Antonis Papachristodoulou, in which he looks at a number of problems that you can deal with using the sum of squares techniques, including models in which you have switching dynamics, between different dynamical descriptions of the same system. And if you add to that the system decomposition and the fact that you can analyze locally what happens to the switching dynamics and bound the disturbances introduced by your interacting neighbours, these techniques give you a lot of flexibility to deal with some of these issues. So, I am answering the question on the complexity of the model that you can handle. Of course you are limited by the size of the sum of square problems you can solve and right now I am moving away from using simple semi-definite programming solvers and I am trying to move towards using parallel semi-definite programming solvers. I am really curious to see how far I can push these techniques to deal with more complex dynamical models and to include even switching dynamics in some of these descriptions. I think you also had another question?

C. Canizares: About the evolution of the system…

M. Anghel: That's right. Essentially you may adapt, if you want, the decomposition as the system evolves, I am not so sure that this is strictly necessary though. Basically, there are lots of things you can do, which have been done in different context, to adapt the models that describe the system in order to better understand the transient stability limits of your system. So, I think there is potential in these techniques. The weakness is basically their scalability.

Janusz Bialek (Durham University): Again question to the first speaker [1]. So, if I understand correctly, the efficiency of your methodology depends on how well you decompose the system into parts, which are internally closely connected and weakly connected between them, and your spectral clustering technique which is a well-known technique. Now the problem with spectral clustering is that it divides the system into equal parts...

M. Anghel: You don't have to do that. In some sense you just define the size of the sub-systems. And you use that size to decide on your eigenvector, where you put the threshold for your cut-off.

J. Bialek: So, you don't divide it into equal parts?

M. Anghel: No, No. You can decide the size of the subsystems and because I had the goal to divide it into three equally sized sub-systems, the first cut was into two-bus model and a four-bus model for the second sub-system. And as I divided further the second sub-system into two…

J. Bialek: How big system? Was it a small system?

M. Anghel: It was a very small system and that choice was basically dictated because when we started this work, my intention was to compute a composite Lyapunov function which is what James Anderson and Antonis Papachristodoulou are doing in their original paper, but there are some issues with that. They impose a sparsity constraint on the coefficients of the Lyapunov function and that constraint impacts you very badly on the region of attraction estimate. Nevertheless, sparsity (reducing the number of monomials in your Lyapunov function) enables them to scale the SOS problem and to find a composite Lyapunov function which is a positive weighted sum of individual Lyapunov functions. When you squeeze the Lyapunov functions, you take the juice out of them and you find a composite one, but the region of attraction is so poorly estimated that is basically useless for transient stability analysis. So I decided that imposing sparsity doesn't make sense, because I am really interested in optimizing how well I approximate the region of attraction. Moreover, I realized that when I tried to build a composite Lyapunov function, I couldn't scale it up, that the problem was already too big. And then, as I was working more on it, I realized that I don't need to follow this approach. I don't care, because if you look at this decomposition, the stability check is of the order m, the number of subsystems, and if you are familiar with network small gain theorems, they basically look for all possible loops in your system that guarantee a small gain stability analysis. That scales very badly with the size of the system. Well, this analysis scales very nicely, if I am correct, it is just the size of the number of sub-systems.

J. Bialek: Well, you've interrupted my question; I didn't ask you the question. So this is a small system, seven-node system. So spectral clustering is basically overkill. But if you go to the real size systems, it is really very difficult to tell anything from the analysis of the seven-node system. If you go to decent-size systems, then you basically have to look at more sophisticated tools of traditional fiddler based clustering, not dividing into equal sizes, that is what I am saying, but using the information in the spectral distances, which is involved in that, into revealing the internal structure of the system. We have done some work recently that basically showed that you can use that information to reveal what the structure really is, how closely the different parts are connected rather than shoehorning them into given number of divisions, which is the basis of the fiddler method. But I think they will come up when you go to the next stage, when you go to the real size system, for instance one hundred nodes. Why don't you do something like that, rather than seven nodes?

M. Anghel: Sure, we can do it, but there is not just one technique, there are many available techniques out there to do system decomposition. I'm even working on the situation, in which the system is not naturally decomposable into weakly interacting sub-systems, and one way to deal with that problem is not to compute just local Lyapunov functions, you can import degrees of freedom from your neighbors to include in your stability analysis. So, there are ways to probably deal with some of these issues.

Thierry Van Cutsem (University of Liege): A question for Yusheng Xue [2]. Your presentation was dealing with the stability limit. I would like to know if you have considered the problem of the voltage dip that goes with the pronounced rotor oscillation. I mean, if you are located at the electrical center of the system, when you have very large angle deviation the voltage magnitude of that bus can go very low for a non-negligible amount of time, so If you have loads there, they are not in a very good shape. My question is do you have such situation in the Chinese system? And if so, can you incorporate this additional constraint in the analysis?

Yusheng Xue (State Grid EPRI): Thank you. Up to now we are testing mainly the transient angle stability. But voltage stability and acceptability are also concerns. Let's say we are doing the online simulations then we are supposed to find two elements acceptable. One is the dip of the voltage reduction and the other is its duration. During fault we cannot change the situation. But if the fault is cleared, then the oscillation in the system begins and then we may find some load nodes near the oscillation center, where they exceed the limitation. Then we can make some suggestion to change the situation but it does not always happen. If we correctly switch off the lines, there is no oscillation and then this won't happen.

Ian Hiskens (University of Michigan): I have a question regarding the equal area criterion idea [2]. It always sort of amazes me that you can take really large, very complex power systems and kind of condense them down to two-area, to a single machine-influence bus equivalent. So it seems to work, but there is an inherent robustness in the existing power systems, so I guess my comment-question is: I wonder how that would change, as we had a discussion yesterday about low-inertia generation, inverter-based generation, as there is more and more non-traditional generation that comes into the system, I wonder whether that robustness will remain and whether we will start to see other effects that are not captured at the moment.

Y. Xue: Thank you for the question. The integrated EAC is based on the actual simulation trajectory, so no matter what kind of modeling you use, and you can change the model, even switch off lines and generators and loads, and also if you have some wind power and anything, the method is still valid. Since you are dealing with this mathematical model, you will get the right trajectory. So the EAC itself does not change the trajectory but it's only a method to extract the information from the trajectory. So it's only starting from the trajectory you simulated. The modelling part difficulties and some time-varying effects they are already contained in the trajectory itself. So, it is independent of this.

Chair: Other questions? I have a question for Marian Anghel [1]. As I listened to your method, I got the impression that the method will perform well, if you can decompose into weakly coupled systems. Please verify this, or tell me if it is different. Then in this case, how much fidelity you are going to lose from this method, if you apply it to more strongly coupled systems. Of course in the US we have two very good examples. In the Western Interconnection you get islands that are weakly coupled, but then you go to the East and it is very difficult to do that.

M. Anghel: So, as you can probably guess I am just starting to play with this idea and trying to understand it better. So it is relevant to tell you how James Anderson and Antonis Papachristodoulou came up with this decomposition. It is more than just a spectral decomposition algorithm. When they apply that technique to linear systems, they observed that they could find Lyapunov functions for each sub-system, but they couldn't find a composite Lyapunov function only using structural information about the dynamics. The moment they added energy flows between the sub-systems, balancing both the strength of the structural interaction and the strength of energy flow, they were able to find a composite Lyapunov function. So there is some strength in the way they modify the traditional spectral decomposition algorithm. Also, what they did, when you do the decomposition, of course if the subsystems are not weakly interacting, you may not find a Lyapunov function for the individual sub-systems. And in that case, they propose to add degrees of freedom from the neighboring sub-systems and they do a search until they find a Lyapunov function. Of course the question now is how conservative is the estimate of the region of attraction when you do this decomposition and that is an interesting question and I don't know the answer. It is not obvious to me a priori that finding and doing a decomposition to estimate the stability region, is worse than having a global Lyapunov function for the entire system and trying to estimate the region of attraction. That's not at all obvious a priori to me. You notice that in this decomposition I don't give you an expression for the region of attraction. You give me a disturbance and I can estimate if that is stable according to the assumptions I am making. Each one of these Lyapunov functions in some sense is conservative. But it is not obvious to me how that translates into a global estimate of the region of attraction. It may be better than finding a global Lyapunov function, but I don't know that and it is not clear also in a generic sense if it is better or worse.

Chair: Thank you. Any more questions? Everybody is hungry, I guess. Oh there is one more from Ian.

Ian Hiskens (University of Michigan): A question for Marian Anghel [1]. I love the decomposition distributed ideas. In the control community they deal with ordinary differential equation (ODE) systems, whereas power systems are differential-algebraic structures and it is not clear that the ideas of being able to take some of the states and treat them as being disturbances from another region, as you have with ODE model, work. It doesn't seem to always follow through, when the couplings are via algebraic equations. There is a significant difference in having the couplings via algebraic equations, I think. Have you explored that?

M. Anghel: No, I haven't.

Chair: Let's thank the authors and presenters.

References

[1] M. Anghel, J. Anderson and A. Papachristodoulou, "Stability Analysis of Power Systems using Network Decomposition and Local Gain Analysis," Bulk Power Systems Dynamics and Control – IX (IREP), August 25-30, 2013, Rethymnon, Crete, Greece.

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