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To Whom It May Concern,

I was asked by Professor Slobodyan to submit an external examiner's report on the dissertation by Batlome Janjgava, entitled "Essays on Economics of Adaptive Learning and Imperfect Monitoring". In my opinion, this is an outstanding thesis. It is ready to be defended.

The thesis consists of three essays. The first essay studies a version of the the Green-Porter (1984) model with parameter uncertainty and adaptive learning. The second essay extends the analysis of the first essay to the case of oligopoly, and revisits the issue of the welfare effects of free entry. The third essay studies adaptive learning in a repeated Prisoner's Dilemma game. It can be interpreted as a discete-action version of the first essay. A unifying theme in all three essays is the assumption that agents cannot observe their rivals' actions, which makes explicit coordination difficult to enforce. From a methodological perspective, the essays are also unified by the use of large deviations methods to characterize endogenous regime switching dynamics. I think all three essays will ultimately be published in top field journals.

Since the essays share a common methodology, and most of my comments and suggestions are methodological, I will simply provide a single list, and refer as needed to each essay.

1. As the author notes, the idea of using escape dynamics to characterize switches between noncooperative and collusive pricing in a duopoly is based on the previous work of Williams (2003, which is the last version that contains this example) and Ellison and Scott (2013). The key contribution here is to use the recent results of Kolyuzhnov et. al. (2014) to simplify the analysis. It might be worth noting, however, that the original idea goes back to a paper by Alan Kirman entitled, "Mistaken Beliefs and Resultant Equilibria", which appeared in the famous 1983 Phelps/Frydman volume "Individual Forecasting and Aggregate Outcomes". The reason I mention this is that as I recall (I no longer have the book, since I lent it to a student who never returned it!) Jerry Green was the discussant, and he raised some objections to the idea that a firm neglects its rivals' output when estimating its own demand curve. Even if you can't observe their output, it seems like you could do (a lot) better than simply throwing it into the intercept! Is it really plausible that firms are so unaware of their rivals' actions? For example, consider the following passage on page 29, which explains the intuition behind the escape dynamics: "..the increase of the mean actual price leads firms to update their beliefs accordingly and to increase the mean forecasted price by contracting the supply". Is this reasonable? Do firms respond to higher prices by *reducing* supply? On the following page, when comparing the dynamics to Green-Porter (GP), it's stated: "In our context the mean forecast price serves as the trigger price, and the

unanticipated fall of the mean actual price below the mean forecasted price causes the price war." I think I see what's going on, but it seems like there's an important difference from GP. In GP, a price war occurs because you think the *other guy* is cheating, and so you punish him. Here it happens because lower actual prices call for a lower forecasted price, which requires you to increase supply (despite the lower prevailing prices). Maybe one way to interpret this would be to assume that firms think other firms know something they don't know, so that lower prices are interpreted as a decline in demand, rather than as cheating. Either way, it seems like a sensible interpretation requires some awareness and assessment of what other firms are doing. As one final example, consider the following passage from the second essay, on page 48: "When firm i observes higher average price than he expected, $y - \hat{y} > 0$, he interprets it as a lower demand and reduces output". This seems really counterintuitive to me. Wouldn't higher prices signal *higher* demand?

2. It seems like there is one missing condition from the definition of an SCE, namely, would firms have an incentive to switch to a different gain parameter? This issue is discussed by Evans and Honkapohja (1993) and Evans and Ramey (2006). They discuss a simulation strategy to compute a Nash equilibrium gain. Related to this, remember that a constant gain is premised on the hypothesis of slow parameter drift. Wouldn't this hypothesis be rejected during escape episodes? There is a recent paper by Kostyshyna (MD, 2012), which addresses this issue. It applies a version of Kushner and Yang's (1995) adaptive step-size algorithm. With this algorithm, the gain would endogenously increase during an escape. As far as I know, no one has looked into the LD properties of this kind of algorithm.

3. In general, deriving analytical results on escape dynamics is extremely difficult. First you have to compute the log moment generating function of the LS orthogonality conditions, which defines the so-called 'H-functional'. Then you have to solve a nonlinear control problem to compute the rate function. The key idea in this thesis is to apply the ingenious approximation results of Kolyuzhnov et. al. (2014). These results are based on three kinds of approximation. First, the non-normal, discrete-time, dynamics are approximated by a diffusion (remember, the LS orthogonality conditions are quadratic forms). Next, a couple of shrewd dimensionality reductions are exploited. The first uses the fact that estimates of 2nd moments are driven by the first moments, so we only need to consider escapes of the parameter estimates. The second is a little more case-specific, and exploits the idea that under certain parameterizations, there is a dominant eigenvalue, which means that (with overwhelming probability) escapes take place along a certain subpace of the parameter space. (In the essays, this is a *one-dimensional* subspace). Finally, the third approximation is to neglect the drift term in the diffusion approximation, and just approximate the escape dynamics using the dominant eigenvector of the volatility matrix (which, as noted, is just a scalar here). The miracle is that all these approximations seem to work! Still, it would be nice to know just how general they are. It might be worth noting that the results of Bryc and Dembo (1997, "Large Deviations for Quadratic Functionals of Gaussian Processes") could be used to at least obtain an analytical expression for the H-functional, without resorting to the diffusion approximation. It is noteworthy that this H-functional will be logarithmic. Unfortunately, one still needs to solve the control problem using this function as flow cost. However, I suspect that the dimensionality reductions could still be used, which might make this tractable.

4. Finally, the only glitch I came across is that the equation numbering was screwed up in the first essay. There were many references to equations (2.x), which seemed to have no counterparts in the current version of the essay.

Sincerely,

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