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Dissertation Review

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Title of the PhD Dissertation: Essays in Applied Economics

The PhD dissertation consists of three distinct and independent chapters. Each chapter addresses a research question in the domain of applied political economy and law and economics. In the following report, I will comment on each of the chapters. At the end of the report I will provide a general comment on the PhD dissertation.

Chapter 1 Ranking of Candidates on Slates: Evidence from 20,000 Electoral Slates – co-authored with Klara Svitakova

This is a chapter on applied political economy. The authors exploit data from municipal election in Czech Republic and conduct an empirical analysis that finds some regularities in the way in which candidates are ranked by political parties in the electoral slates. Then the authors provide a theoretical model of demand and supply of political candidates that tries to explain the results. This chapter is a nice piece of research. I encourage the authors to qualify better their contribution to the literature. In the current version, it is difficult to understand what we learn relative to previous findings and it is problematic to put the analysis of the authors in a broader context of the literature. In addition, the first part of the introduction should be more clear in defining the context and the focus of the chapter.

As a general point, I would put more attention on shocks (like the shocks on popularity pointed out by the authors) that move party leaders' incentives on the demand side and on shocks that manipulate selection of candidates with different characteristics on the other side. In fact, in the absence of these type of shocks, it is not easy to interpret the results. First, while valence can be considered as a pre-determined characteristic in the context of this application, party membership and value is endogenous to valence and potentially to other unobservable factors related to valence and normalized rank. Second, for the very fact that the outcome depends on the supply and the demand of the political arena (as put forward by the model), we do not know if some of the results are determined by the relative movements of the demand or movements of the supply.

I am not entirely convinced that the model should be placed at the end of the chapter. A simplified version of it could be a conceptual framework after the description of the institutional background that guides the empirical analysis. In conclusion, I encourage the authors to move to a less descriptive empirical analysis. In this way, we could learn much more from their paper.

Specific points

- Is there any difference between parties?
- The conditional rank in section 1.3.2 should be explained better. I guess it is the residual from a regression of actual rank on observables.
- Please write down the regression model in the chapter.

- Section 1.3.4 is interesting but rather obscure. The authors should elaborate more the issue stated in the section and provide additional evidence on the existence of the trade-off faced by the party leaders.

Chapter 2. Sentencing Decisions Around Quantity Thresholds: Theory and Experiment, co-authored with Jakub Drapal

In this chapter the authors investigate the behavioral mechanisms that drive prison sentences given by judges in the context of quantity thresholds and subsections in which a crime fall depending on its seriousness. In particular, using an experimental design they try to isolate a “severity effect” and a “reference effect” deriving from the presence of thresholds in the sentencing regime. They find evidence for both the effects which have opposite sign. The severity effect dominates the reference effect.

This chapter is intriguing and the research question is potentially very relevant. Also in this case, however, the authors should try to work more on the motivation and the clarity of the research question. Why is this research question relevant? What do we learn from this in the context of the literature? How do we advance our knowledge on the way in which judges formulate sentences? This is particularly important if the authors, as I suggest, would like to publish the paper in top-tier law & economics journal.

Specific points:

- I am not sure that the model in the current version is very useful. I have the impression that it is rather complicated for the ideas that the authors would like to convey. I found much more useful table 2.1 to understand the main ideas that the authors would like to test.
- In terms of the experimental design, it should be clarified what the author manipulates, namely assignment to different scenarios. I found the first table reporting the results not very easy to read, especially regarding of what the treatment and the control was.
- I could not find any evidence on individual characteristics being balanced across treatments and control groups.
- The implications of the rather low participation rates (about 200 out 1257) should be discussed.
- I am not sure that in the theft offence, the comparison between A and B reveals the effect of the 50k threshold only. It could also capture or be contaminated by the effect of having in B with respect to A a higher upper limit of the sentencing range.
- In the drug possession case, the substantially equal results between scenario A and D should be emphasized because they support the idea that ceteris paribus a higher upper limit of the sentencing range does not lead to a higher sentence.

Chapter 3: Consequences of Inconvenient Information: Evidence from Sentencing Disparity

The last chapter explores the effect of revealing “inconvenient” – yet not deceptive – information about the functioning of the judicial system (e.g. sentencing disparity) on individuals’ trust on formal institutions (e.g. judicial system, police, government and broadcasting), intention to apply to a court, and willingness to learn about alternative dispute resolution. Finally, the chapter investigates the effect of revealing inconvenient information on the propensity to sign a petition addressing sentencing disparity. The author does not find compelling evidence that revealing information has an effect on all the outcomes. A main implication of the results of the paper is that revealing inconvenient information about the functioning of institutions is not harmful from a public perspective.

This chapter is in my view the best one of the thesis. I congratulate with the author for addressing a quite novel research question and for conducting an intriguing experiment. The chapter is well motivated and it is clear why the research question is relevant from a policy point of view. Of course, a drawback of the chapter, given the existence of “null results”, is to convince the reader that the individuals trusted the information provided in the context of the experiment and therefore the absence of statistically significant results does not depend on the experiment but rather from a genuine absence of effects. To be clear, we would like to have an experiment that somehow replicates a scenario in which individuals really understand and internalize the information on sentencing disparity provided by a third party. Perhaps, a new experiment can be designed in way in which these concerns are addressed. In this case, I believe, the paper would have a high chance to be published in top-field journal in law and economics.

- The treatment and control slides should be included in the main text.
- The methodological aspects of the survey experiment should be discussed in detail following the standard in the literature.
- The null results may mask interesting pattern of heterogeneity. Can you split the sample and report Figure 3.1 for individuals with high and low initial trust in the judicial system?
- On the basis of standard errors, the author can exclude the existence of an effect larger than the upper limit of the confidence interval. There are now several papers in top journals presenting these kinds of results. It could be useful to give a look on how the authors of these papers present the (absence of) results.
- In a follow-up study, I would somehow measure initial beliefs (priors) and after treatment beliefs (posteriors). It would be nice to have an idea of how and whether the piece of information provided confirmed or changed initial beliefs on the functioning of the justice system priors.
- In general, it is not easy to measure initial and after-treatment beliefs because the researcher risks to generate framing effects in addition to “teaching” the results. Hence, an ingenious design should conceptualize the way in which this is possible.

- A nice example on possible useful designs comes from Barrera et al. in the Journal of Public Economics in which the authors study the effect of fact checking in countervailing “alternative facts”. One way would be to first ask individuals to provide an idea of how sentencing disparity looks like in their region/province and then revealing the true measure.

In my opinion, the dissertation satisfies formal and content requirements for a PhD thesis in economics in a department such as the one at CERGE. The average quality of the chapters is high. As most of the PhD theses, also this one needs to be revised to be submitted to journals. However, the work done is a lot and it is very good, and I congratulate with the author for putting this amount of research together during these years. I am sure that the author will do a great job in publishing his chapters as single papers. Overall I do recommend the dissertation for a defense as it is without any binding recommendations about things that should be changed or added.

Yours sincerely,

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