



Referee report on the Dissertation Thesis of Ekaterina Travova (CERGE-EI)

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I have read the whole thesis with interest and ease. In my opinion, all three chapters definitely constitute a solid contribution (not only) to the field of public economics. My good impression from reading can only be validated by the fact that the first chapter has been selected as the best paper at the 2019 Young Economist Meeting. The author in the thesis investigates important economic phenomena using advanced econometric methodology, with results drawing potentially important policy implications.

The first chapter investigates the effects of incentive structure on the malpractice of police officers in Russia. Using a bunching estimator and an event study, Ms. Travova reveals a consistent upward manipulation of the amount of heroin seized by the police officers that results in crossing the two thresholds set by law and, eventually, means a harsher punishment of the criminal and likely a reward for the involved officer.

Comments: The author mentions the anecdotal evidence that police officers commonly use heroin to fabricate evidence; I just wonder if there has ever been a court case of a police officer caught for this misconduct. The main analysis in my opinion needs no further improvements. I also like the assessment of how much the malpractice makes sentences longer, and the discussion of the associated social costs. I see that the exact calculation is rather difficult due to many indirect effects on the victims and entire society, but a rough estimation of the direct costs (incarceration, benefits to police officers, ...) might, however, be a feasible exercise that would deliver the lower bound of the excessive costs of this incentive structure. Apart from the drug enforcement units, I wonder if similar incentives based on past performance are set in other police departments. Effectively, such incentives distort the official crime statistics and may keep crime existent where there is in reality none (for example, in case of the drug enforcement units, if no heroin is supplied to a certain region, what will the local officers do to fulfill their forecast objectives of seized heroin?). Lastly, the conclusion might include more general implications for optimal incentive scheme structure when individual productivity is unobservable.

The second chapter uncovers that the denser network of the Orthodox Church of Russia improves the approval ratings of the president of the federation, and also the share of votes for the governmental candidate. The causal effect is found with a Bartik-style instrument. Interestingly, the trust to the president and the approval ratings of other authorities are not influenced by the church. The mechanisms of the effect are still not clear as the number of believers attending church to whom the propaganda might be directly served does not increase over time, and the analysis of media also does not deliver clear answers.

Comments: Again, the main analysis with all the robustness checks and the analysis of channels are in my opinion well done. I could not come up with another channel that would be possible to test on this data. For a better clarity, I would only welcome the interpretation/discussion of the magnitude of the coefficients presented in the regressions (similarly for the third chapter). How economically meaningful are these? How many more percentage points of approval ratings does the Church on average add?

The third chapter shows that the negative experience from unemployment during the transition period after the 1989 affects the long-run life satisfaction and shifts the political preferences to the right.

Comments: Before 1989 there existed substantial overemployment in many sectors and, therefore, many people were not really used for their full capacity in their jobs while they still were well paid. The unemployment resulting from the opening of the market competition during the transition period might, therefore, be also seen as a loss from the point of view of a reference point being the easy employment before the transition (using the prospect-theory parlance). I wonder if there might be certain selection effects into the SHARE survey – motivated, capable, and skilled people (that will also be happier) might have left the country during the transition period and this would result in overestimating the real effect in the author's analysis. Another concern comes with the sector disruption calculations based only on SHARE data. The authors state the reason for not using the official labor force statistics to be the lack of clean data. SHARE is representative to population but there might be some departures with respect to the sectoral employment (if I am not mistaken). To be more persuasive with this method, I would like to see a comparison of the disruption/unemployment calculated on the SHARE data with the data on unemployment in the sectors obtained from official sources from one or two countries for which the data is available.

To summarize, I could come up with only small remarks that may improve the chances of publishing the papers, but the thesis can be definitely defended in the current shape. Because the thesis in my opinion fully satisfies the formal and content criteria for a PhD thesis in economics, I can absolutely recommend it for the defense.

With best regards,
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