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Dissertation Review

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Dissertation Title: "Essays in Applied Economics"

This dissertation consists of three separate research papers each addressing a question in applied economics. I am providing a separate comment for each of the chapters. My general assessment is that **this dissertation fulfills the formal and content requirements for a PhD thesis in economics**, and I recommend the dissertation for a defence. The following comments are only suggestions for the author to develop the papers further. **None of my comments should be viewed as binding for the dissertation defence.**

Chapter 1: Under Pressure? Performance Evaluation of Police Officers as an Incentive to Cheat: Evidence from Drug Crimes in Russia

This paper explores the impact of performance evaluation of Russia's enforcement agencies dealing with the drug offences on the amount of estimated manipulation of confiscated drugs. Because according to the Russian law the drug offence is characterised as more serious (and the sentence length is increased) if the among of drugs involved is higher than a certain threshold, the discontinuous jump in the probability mass in the area just above such thresholds is indicative of manipulation by law enforcement officers. In 2013/2014, two Russian agencies were responsible for the law enforcement with respect to drug-related offences: FSKN and MVD. MVD had incentives to beat the known previous-year benchmark, while FSKN officers are implicitly competing with the police stations in other areas. Because the benchmark is unknown for FSKN officers, the author argues, the incentives to cheat are weaker for FSKN. The paper finds, using the bunching estimator, that, in the drug seizures by FSKN the amount of manipulation is smaller. The author also presents set of related results on the social costs of manipulations, such as who suffers the most, and what are effects on sentencing.

1. I suggest the author clarifies the research question earlier in the chapter. From the first couple of pages, it looks like the question is "do people respond to incentives in the public sector?", which is fine but may not be as exciting for the potential audience as a more specific question about the incentives to cheat and distributional aspects of such cheating. For example, I think that the finding about persons with prior convictions is potentially quite interesting.

2. The section describing the method of identifying victims of manipulation should be expanded with more intuition and motivation about why the method is valid. I appreciate that the author uses the method developed by Diamond and Persson (2016), but the familiarity with Diamond and Persson (2016) should not be a prerequisite for reading this paper.

3. The finding that the amount of manipulation along the major drug trafficking routes Is higher than the manipulation away from the routes is quite interesting. But for now, it is quite unrelated to anything else and seems a bit arbitrary. I would recommend motivating it in a

more thorough may and maybe include more ways to split the samples geographically while retaining statistical power: maybe looking at the difference in national republics or using Petrov & Titkov subregional democracy indices.

4. Is it possible that the higher manipulation along the major drug trafficking routes can be explained by higher numbers of officers? If MVD and FSKN send more people to those areas, then they would need more manipulation to achieve desired numbers of serious offences per officer.

5. I believe that the most important challenge for the interpretation of the results as pertaining to the effect of the incentives of law enforcement agencies is that the officers are not assigned to the agencies randomly and might differ in many ways – both observable and unobservable. For example, training, competence, socio-economic status etc. I appreciate that it might be impossible to observe those characteristics, but the paper might benefit from the discussion about why the results are not driven by these potential differences. Most importantly, I would expect an explanation about why the results are not driven by the differences in potential punishments for manipulation.

6. It is possible that I missed it in the paper, but I have not found a regression equation for the event study depicted on Figure 1.3.

7. I am not sure I fully understand why FSKN is considered to have "weaker" incentives to manipulate. As I understand the explanation in Section 1.4.2, MVD has to beat the previous-year benchmark (but not by too much because of the concern for the potential ratchet effect), but FSKN officers are implicitly competing with the other stations. The author writes that because the benchmark for FSKN is unknown, the incentives to manipulate are weaker. I am not sure I completely agree. This explanation implies the FSKN situation is more like a game, where strategic officers from different locations compete with each other. Such a competition should drive manipulation to very high amounts, constrained only by the costs of manipulation (potential punishment, for example). In light of this concern, I would expect some explanation about why FSKN has weaker incentives to manipulate seized amounts.

<u>Chapter 2: For God, Tsar, and Fatherland? The Political Influence of Church: The Case of Russia</u>

This chapter studies the impact of Russian Orthodox church on voting and popularity of government in Russian regions. The author constructs a Bartik-style instrument based on the pre-revolution density of Orthodox monasteries and finds that the Church positively influences presidential approval and election results in favour of the incumbent. The main mechanism suggested is the implied association between Orthodox faith and Russian identity.

1. I suggest that the author discusses the empirical work by Maria Ukhvatova who has a series of papers and a Political Science Ph.D. dissertation on Russian Orthodox believers and the support for the Russia's political regime.[1] She finds several things of relevance to this paper: first, that the Orthodox believers can be divided into two categories: conservative activists (for whom the ideological aspects of the Orthodox faith are of primary relevance) and ritual followers (for whom the ritualistic aspects of the Orthodox faith are of primary relevance), the ritual followers being the most reliable supporters of the regime; secondly, she argues in her qualitative work that "ritual followers" support the Russian political regime precisely because it has been so supportive of the Church: allowing it to influence cultural sphere and education. This is an important argument because it suggests a relevant mechanism of the connection between the Church and the support for the regime: Orthodox believers support the regime

[1] https://www.hse.ru/en/staff/ukhvatova#sci

not because they are brainwashed by the Church through media, but because they feel grateful to the government which permitter Church to become such a prominent cultural force.

2. I suggest that the author includes a discussion of potential endogeneity problems that the instrumental variable strategy is supposed to solve. For example, I would imagine that underlying conservative/nationalist values might drive both the support for the regime as well as the orthodox density. But Table 2.1 has the 2SLS coefficient larger than the OLS coefficients. This suggests that, assuming the instrument is valid, the naïve OLS estimation is biased downwards. Given the author's meticulous data collection effort, it appears unlikely that measurement error in orthodox density can account for such a difference.

3. Regrettably, I find very little discussion of the instrument validity. For the instrument to be valid, several assumptions should be satisfied: exogeneity (no backdoor path between the instrument in the outcome), full mediation (the instrument influences the outcome only through the treatment), monotonicity (not in a single observation treatment goes up because the instrument goes down, or vice-a-versa), and relevance (the association between the instrument and the treatment should be strong enough). I do believe in the relevance (the author showed the first stage in Table 2.1), and in monotonicity (since it is very difficult to come up with the story violating the assumption). But the exogeneity assumption and the full-mediation assumption seem much more problematic. For example, historical piety of the local population can influence both the old monasteries the support for the regime (given Yeltsin's and Putin's performative Christianity) thus violating exogeneity. Also, higher historical density of monasteries could have invited more repression during the post-Revolutionary period influencing the bias against the Communist party (the main opposition party) thus violating the full-mediation assumption.

Out of the three papers, I find that this one potentially is the most important in terms of its contribution, but also might require the most work to reach its full potential.

<u>Chapter 3: Economic Disruption, Life Satisfaction, and Political Attitudes</u>

This paper uses the data from Survey of Health and Retirement in Europe to estimate an association between working in a sector that experienced high unemployment rate during the post-communist transition and life-satisfaction and an array of economic and political outcomes. The paper finds a negative association with life satisfaction, perceived control over one's life, and marriage, and no negative association with economic outcomes. The paper also finds that people from sectors that experienced disruption are more likely to be on the ideological right. The paper addresses endogeneity of sectoral choice by using an instrumental variable: predicted probability of a respondent's sector being disrupted with a set of demographic and socio-economic covariates (with the respondent omitted from the calculation).

1. As in the previous paper, I would suggest expanding the section about the research design. For example, I find it possible that the instrumental variable strategy violates the exogeneity assumption. The socio-economic characteristics used as predictors are obviously related to the outcomes (such as life-satisfaction). The fact that the individual *i* is omitted from the calculation is of little solace given the potential homophily effects in occupational choice.

2. The potential channels are so abundant that it is quite difficult to make sense of them without some kind of theoretical expectations. Also, for the mechanism I suggest exploring the possibility of finding contemporaneous outcomes: what happens with the individuals at time of disruption that might have long-lasting effects. This might help assess the plausibility of the long-run mechanism.

3. In general, I feel like that framing could be tightened. Yes, indeed, there is little evidence about the effects of the sectoral disruption on life satisfaction. But is there theoretical expectation for it to be non-existent? Or even positive? Or maybe the magnitude is of relevance and unknown? Some argument about the epistemic uncertainty that necessitates this study will help readers appreciate its contribution.

In sum, I find all three chapters important, innovative, and competently executed. I congratulate the author with such an achievement.

Date: January 28, 2022 Dr. Maxim Ananyev *Maxim Ananyev*