

Referee report on the dissertation
“Economic Impact of Voting and Procurement Rules”
by Jan Palguta

Summary

The dissertation consists of three chapters. The first two chapters are empirical. The first chapter studies the effect of political competition on procurement spending. The second chapter studies the impact of regulation that allows restricting competitive in procurement bidding on the allocation of procurement contracts. The third chapter is theoretical/computational. The author studies the strategic voting behavior in small committees. He considers ten most common voting rules and calculates the probability of outcomes where voting results are manipulated.

In the first chapter, the author uses data on Czech municipal elections where the parties are elected based on proportional representation with a 5% threshold. The author uses the threshold for his regression discontinuity analysis to provide a source of exogenous variation in the number of parties in the legislature.

The main challenge is to construct the dependent variable, the “wasteful spending in procurement”. The author uses three indicators of inefficient procurement spending: (1) allocation of procurement contracts to the donors of political parties; (2) high prices given the content or overall level of procurement spending; (3) less competitive bidding for procurement contracts (measured as number of bidders). Each of these variables is just a proxy for “wasteful spending” and is subject to obvious criticisms (which the author recognizes) but the analysis of the three variables together presents a reasonably convincing body of evidence.

The author also studies the heterogeneity and finds that the effect is stronger in legislatures where the number of political party is small. This is intuitive as in this case each additional entrants matters more. He also finds that the effect is stronger in smaller municipalities and small legislatures.

The author also checks the impact of political competition on fiscal outcomes and interestingly finds not effect.

The author studies the validity of the empirical methodology and confirms that his research design is valid.

In the second chapter, the author uses the introduction of thresholds below which the procurement contracts do not have to be awarded through open auctions. The author studies the data on procurement contracts and shows that there are mass points of contracts below the threshold. He also studies the share of contracts awarded to “anonymously owned firms” (i.e. the firms that do not disclose their owners) and finds that the concentration of anonymous firms is significantly higher just below the threshold than just above (and increases in value of the contract below the threshold). The effects are especially strong in sectors where it is harder to evaluate the true value/quality of procurement: construction and services. There are no mass points or discontinuities in the years before the reform (that introduced thresholds); interestingly, in years after the reform, the discontinuities do not decline. This is especially impressive given non-trivial inflation that should have changed the nominal value of the contracts.

The anonymous companies are on average younger and smaller (in terms of both labor and capital). The author implicitly assumes that the anonymous companies may be conduits for politically connected businesses.

In the third chapter, the author simulates voting by looking at a uniform distribution over all possible strict preference orderings. The author considers committees with 2, 3, 5, 7, or 11 voters and 3 or 4 policy choices. He runs 100 000 simulations for the following 10 rules: plurality, Condorcet, approval voting, plurality with runoff, Borda's count, Black's procedure, Hare's single transferable vote procedure, Coomb's, Max-min, Copeland's.

In each simulation the author considers the following setting: one voter chooses whether it is better for him to vote strategically, all other voters vote sincerely. Then the author calculates the probability of strategy-proof outcomes for each voting rule. This is done for two settings: full information and partial information about the other voters. The findings are different for different settings and different number of players. But overall Black's, Copeland's and Condorcet's perform relatively well while plurality, Borda's, max-min and approval perform badly. The rest are in between.

Evaluation

The first two chapters are very solid pieces of empirical work. The author uses up-to-date empirical methodology and obtains convincing results. Both exercises are straightforward and results are intuitive which may make these two chapters harder to publish than they deserve. But as dissertation chapters they do demonstrate that the author is an accomplished empirical economist.

The third chapter is completely different. It is also straightforward and somewhat mechanical. It does demonstrate that the author understands modern voting theory and knows how to use computational methods. The results are interesting and the author has definitely invested a lot of effort. As I am not a specialist in the field it is hard for me to judge how well this chapter can be published.

All the three chapters are clearly written, the author connects well to the relevant literature, motivates the research question, presents the institutional context, describes empirical strategy and results. He also discusses and recognizes the potential implications of his analysis and limitations of empirical methodology.

Overall, this is a very good dissertation produced by an economist who knows how to collect data and how to use theory, modern empirical and computational methods.

Comments

I am not a specialist in the voting theory so I cannot provide specific comments on the third chapter.

As an overall comment I would like to mention that there are some typos throughout the dissertation which could have been identified by spellchecker.

Comments on Chapter 1

1. It would be nice to show the results of the OLS regressions as well.

2. The results for political donors are based on the assumption that each donor gives to one party. However, it is very likely that some donors support several parties. Also, even if Donor A supports party X only and party X does not get elected while a party Y with similar platform is elected, there may be an incentive to reward A with a contract to support the “close” party.
3. I am concerned by the lacks of significant results in Table 1.4. This implies that the “wasteful spending” does not have an effect on fiscal outcomes. This is a puzzle that has to be analyzed and explained. Is the impact of political competition on procurement too small? Or are there countervailing mechanisms restoring the negative impact of the “wasteful spending”? What are these mechanisms? Can they be described, qualitatively or, better yet, quantitatively?
4. I am also worried by the fact that some results do not hold together in the semi-parametric test (Table 1.5). This should be discussed and explained.
5. I think that the result that the effect is stronger in the smaller municipalities is counterintuitive. In such cases, as the author rightly argues, there greater opportunities for mutual monitoring. However, this implies that in such municipalities the effect of political competition should be smaller (as monitoring works anyway). I would check whether it is the size in terms of population or in terms of geographical distance that matters. In a less populous rural municipality that occupies a large geographical area monitoring is harder than in a densely populated city. Another variable to check is internet penetration and local newspaper subscriptions.
6. I would also like to see whether the effect is stronger in the cases where additional entrant is a “swing voter”, i.e. when this entry changes the majority within the legislature.
7. For both Chapters 1 and 2: there is a very similar paper by Adam Szeidl and co-authors (unpublished and even not really written up although being presented) on political connections and procurement in Hungary. There is also a directly related dissertation by a PhD student from IIES Stockholm, Audinga Baltrunaite, she looks at politics and procurement in Lithuania.

Comments on Chapter 2

8. The paper would benefit greatly from a case study which would discuss a case where the identity of anonymous company’s owner is explicitly or implicitly discovered.
9. Figure 2.1: it would be more informative in logs.
10. Overall, these results are striking but the authors could have done more work looking at heterogeneous effects: geographical, political orientation of politicians in charge of the contract, number of bidders etc.
11. The authors need to try to provide a calculation of potential losses due to manipulation (making assumptions about various parameters if necessary).