

Examiner's Report on
Three essays on regime change

Submitted as a PhD dissertation at CERGE-EI

January 29, 2008

1 Overall Summary

The dissertation examines 'regime change' through three distinct political economy applications. In each case it models the incentives of various players - politicians, central bankers, members of the public, owners of the media - trying to shed light on the dynamics of the economy and politics before and/or after the particular regime change.

The papers constitute a brave attempt to contribute to the respective topics, in which relatively little literature currently exists. They offer some interesting insights that provide solid basis for future work - that can develop them in various directions. While the modeling through which these insights are derived, and their presentation, are often unconvincing (and need to be improved in order to aspire for publication in tier 1 and 2 journals), the contribution of the papers is valuable.

I divide my comments into two main categories: those that I believe need to be incorporated in the dissertation itself; and those (more extensive ones) that the author can consider at a later stage to improve chances of publication. I label the former type as 'major' and the latter type as 'minor'. P followed by a number will denote the page number. A slash following that will report the line number.

2 Chapter 1

2.1 Summary

The chapter examines transition of a small open economy (either an inflation or an xchange rate targeter) to a membership in a monetary union in which monetary policy is no longer autonomous. It analyzes the response of private agents and the economy to the announced regime change, and reports the resulting welfare outcomes.

The quality of the chapter is clearly superior to the remaining two chapters. A micro-founded New Keynesian model common in the literature is used to generate impulse responses and improve our understanding of the various consequences of the announced regime change. While the conclusions do not deviate from conventional wisdom, they are relevant from the real world perspective in the light of the European Monetary Union.

Specifically, it is found that the economy starts to change immediately after the announcement of the regime change, i.e. well before the change itself takes place. This is unsurprising due to the forward looking nature of economic agents and the structure of the economy. From this it also follows that the behaviour of inflation and exchange rate targeting economies converges. Finally, and as expected, having a higher weight on inflation relative to the exchange rate will make inflation less stable and the exchange rate more volatile (and the other way round) in both the independent and the transition phases.

2.2 Major Comments and Suggestions

1. The choice of the weight on the various regimes is not motivated and seems peculiar. It further implies too many different cases that the reader will find difficult to keep track of (and the fact that there are often typos in describing the cases does not help, see below).

The impulse responses show that there is no major qualitative difference between the cases of weight 3 and 10 (ET3 and ET10). I therefore believe it would make much more sense to only include one type of the unified economy, e.g. ET5 (and only refer the reader to the more general working paper version as a robustness check). This would mean that the number of transition economies would reduce from 4 to 2 and that a lot fewer impulse responses would need to be reported. Further, this would allow the type of independent economy to be ET2 rather than ET1, which would make it comparable with IT2. The weights in the notation could then be dropped, ie we would have two economies, IT and ET, and three phases, *pre*-transition, transition, and *post*-transition. Finally, the unappealing transition notation ‘ \rightarrow ’ would no longer be needed.

2. An additional important advantage of this would be the ability to better focus on the comparison across economies (IT versus ET). The authors do not do so and only focus on comparisons within countries for an obvious reason (that they however do not spell out) - inflation targeting comes as a clean winner, i.e. it is never optimal in the model to target the exchange rate or join the monetary union. But as many countries (including the UK) are/were considering a membership, this is an important and policy-relevant question: Should a country join the monetary union in the first place?

The paper can contribute some insights into this. As it stands, the paper implies that transitory and unified economies feature more macroeconomic volatility than an independent economy (to see this clearly the loss for the independent economies should be reported in Table 1). This is not surprising, it is the price to pay for giving up autonomous monetary policy in the presence of idiosyncratic shocks.

Therefore, for the monetary union membership to be desirable, there has to be some other benefits (trade, reduction of transaction costs, more

efficient labour markets etc). While these are not modeled in the paper (and could be in an extension), I believe the authors should briefly discuss them. Importantly, the authors can report how large these unmodeled benefits would need to be for the membership to become optimal, since they can calculate the loss that happens as a consequence of adopting the regime (both during and after the transition). As various estimates of these benefits exist in the literature, this would be a valuable contribution to the debate on currency unions.

2.3 Minor Comments and Suggestions

1. The paper implicitly assumes that the announcement of the future regime change is a complete surprise. However, in the real world there exists some probability distribution that a country will join a monetary union even before the official announcement is made. For example, countries that aspire to join the EMU are commonly already a part of the EU. Similarly, there exist some probability that the country will fail (to satisfy the criteria) to join the monetary union even after the official announcement has been made.

Modeling this probability distribution explicitly would be an improvement. This could also be interpreted as the credibility of the announcement and of the union as such. Even if this is not done explicitly the predicted effect of this could be discussed. For instance, if agents place some probability on future regime change before the official announcement, the effect of the announcement itself would be less pronounced as they would have already taken some action.

2. The authors make a number of simplifying assumptions to make the model tractable. While these are justified, in future work I suggest to allow for the possibility of speculation and exchange rate crises in some way. The real world experience of the early 1990s Europe tells us that this can be an important factor in assessing the transition to a monetary union.
3. One point of debate (and a proposed advantage of a monetary union) has been the gradual synchronization of shocks amongst the union economies. I believe the framework would be capable of saying something about this and it would make a valuable extension.
4. There exists an emerging literature on learning. It is unlikely in the real world that the public knows the weights in the policymaker's loss function with certainty - in either the pre or post-transition phase. Therefore, the public's learning about the weights could be modeled.

3 Chapter 2

3.1 Summary

The chapter looks at a situation after the fall of the communist regime. It attempts to (i) understand the incentives of the communist party to transform itself into social democrats, and (ii) the resulting effect of this on the privatization method selected by the democratic party in power.

The fact that there may be a link between the two developments is an interesting and by no means conventional proposition. The analysis is set up in a way (arguably realistic) in which politicians prefer to steal for themselves through direct sales privatization (rent-seeking motive). If this is not possible they will hand out property to people (including themselves) through mass privatization to get reelected (office-seeking motive). Public goods provision effectively comes last on their list of priorities.

The author argues that incumbent (non-communist) democratic parties tended to use a method of private sales in countries with transformed communists and mass voucher privatization in countries in which communists had not transformed.

3.2 Major Comments and Suggestions

1. In a scientific paper it is imperative that the following three things be separated: (i) presentation of the model, (ii) additional assumptions imposed that do not directly follow from the model, and (iii) the solution/results of the model. This is unfortunately not the case in Chapters 2 and 3, which not only makes the papers hard to read, but casts doubt on the conclusions (as it prevents the reader to check the results).

See e.g. p75 where all three of these are mixed together and despite my best efforts I have been unable to determine what is purely assumed and what is claimed to follow as an outcome of the model. The fact that the author often neglects to justify his assumptions makes it even worse. For example, on that page it is stated that ...‘the incumbent party can no longer appropriate rent and provide thus lower level of public good...’. If it is an outcome of the model it needs to be backed up by algebraic derivations. If it is an assumption it needs to be supported by reference to some Bertrand type competition game in a duopoly (Cournot competition would not give you this outcome, there could still exist positive rents in equilibrium).

2. It is claimed on p74 that the probability of being elected ‘will be endogenous, depending on the preferences of voters.’ While the model allows for this, it is *not* done. The author simply assumes on p75 that reformed communist are ‘more likely’ to get elected than non-transformed ones. This claim should be derived, or yet better it should be shown under

what circumstances which party actually wins the election (since the voters' preferences are explicitly modeled, they optimal actions can be easily derived). This is essential since this probability directly determines the equilibrium outcomes. Unless this is fixed one has the impression that the results are merely assumed, i.e. following purely and directly from the assumptions made.

3. Even ignoring the above issues, the specification of the model seems inconsistent. If U^D features α as the relative weight, why doesn't U^C feature $(1 - \beta)$ in front of $(g_1 + g_2)$? And the same applies to the public. But in addition to this, none of these weights α, β, γ seem to play any role in the analysis - there is never any change in the equilibrium outcomes for different values of these. As such, they can be disposed of to streamline the notation.¹ Similarly, o_1^D can be normalized to zero as it plays no role.
4. In sections 2.3.1 and 2.3.2. the author writes out all the payoffs. However, when comparing them he does not write out the incentive compatibility conditions explicitly, nor does he refer to the various equations that feature the payoffs compared. This makes it more difficult to follow and check the claims.
5. The paper allows for the democrats to be either rent-seeking or office-seeking, but not both. Clearly, in the real world they are a combination of the two, and this can be easily incorporated. It suffices to place a relative weight on the r and o elements of the utility function (in the same way α is used currently). I will refer to this relative weight as κ (the degree of rent-seekingness). The analysis will then be greatly shortened since the rent and office-seeking cases will be considered jointly. The solution (in the sequential game under the communists' Stackelberg leadership) will then consist in deriving a threshold level $\bar{\kappa}$, above/below which a different Nash equilibrium obtains .
6. The author should use standard game theoretic terminology to better communicate the outcomes, e.g. normal vs extensive form games, simultaneous vs sequential moves, dominant strategy, Stackelberg leadership, first mover (dis)advantage etc.
7. Very little intuition is provided to accompany the results - this needs to be significantly improved to make the arguments more convincing. For example, the fact that voucher privatization is used by politicians to become more popular with the public, but only in countries in which they would not get away with stealing some money directly through private sales.
8. The actions available to player C is 'transform' and 'stay orthodox', not 'transformed' as used in Tables 2 and 3. An action's name cannot bear past tense as it needs to be available. In addition to that, the author

¹On p81 the author mentions that a certain result obtains 'for plausible parametrization', but I fail to see any such contingencies in the algebra.

should better explain the real world distinction between these two options - it seems plausible that in some countries ‘reforming’ took the form of simply changing the name of the party (i.e. reforming de facto or only de iure?)

9. The author finds that there may be two types of outcomes. (T, DS) obtains if the communists are the Stackelberg leader AND democrats are office-seeking. Under all other circumstances the other Nash (O, V) obtains. The author sells this result, e.g. in section 2.4 he argues that the outcomes may differ if ...‘the preferences of democrats differ across countries’. But there is no discussion of why the communists should move first or why the democrats may not care about rents. Neither is it explained whether the (O, V) countries feature office or rent-seeking democrats (it could be both, but the latter only under a simultaneous move).
10. Following from that, the author relates the results to the collected empirical evidence. While collecting the data is very valuable, without clarifying the issues from the previous two points little can be said about the differences between countries and the real driving forces of the observed outcomes. Put differently, we observe some correlation but cannot make conclusions about causality.
11. The research from development economics shows that an important aspect may be the quality of institutions in a country (legislation, police, or more broadly media etc). Solid institutions may disallow political parties to extract rents. This should be discussed (and could even be modeled: a punishment for extracting rents as a random variable whose mean is increasing in the size of the extracted rents. Then all democrats could be made homogenous, i.e. equally rent vs office seeking).
12. All players are assumed to be fully patient (not to discount the future). Given that this is a three period model, and that politicians are typically very impatient in some parts of the political cycle (before the election), discounting may refine or even alter some outcomes.
13. The discussion of the Hotelling (1929) product differentiation story on p68 is not accurate. The literature has identified that it may not only be optimal for the firms locate next to each other (minimal product differentiation), but also at the opposite extremes of the product space (maximal product differentiation). This depends on the ‘transport’ costs of the consumers (roughly speaking if the costs are linear we observe the former whereas under quadratic costs we observe the latter).

3.3 Minor Comments and Suggestions

1. It would be interesting to allow for the politicians’ budget constraint not to hold temporarily. This would model the realistic fact that governments tend to run deficits and accumulate debt (especially before elections).

2. The tax revenue τ is assumed to be exogenous. It would be interesting to consider a situation in which one of the privatization methods is superior and increases economic growth. Then this would endogenously increase future τ which the politicians would incorporate in their current decisions on the privatization method.
3. With reference to the suggestion above of postulating a parameter κ , it would be interesting to allow for incomplete information about this parameter (type of democrats) on the part of the public. This would make the analysis richer and the game theory a bit more sophisticated.

4 Chapter 3

Unlike chapter 2 that models a pre-election situation, this chapter examines a post-election one. As such, it is not really concerned with any ‘regime change’ that I can see.

Politicians want to extract rents as well as get reelected. Extracting rents is socially undesirable as it reduces the size of the public goods pie to be distributed. The public however has incomplete information about the rents extracted that it could use to punish the politician by voting him/her out of the office. This is where the media come in.

The media have the required information about the politicians’ behaviour, however they may not be willing to provide it to the public since it crowds out commercial content (paid advertising). The upshot is unsurprising: the more profit oriented the media owners are, they less they report, and the higher the politicians’ corruption. The author then conjectures that in countries with more concentrated media ownership (in which the commercial motive is stronger since the profits flow to a smaller number of owners), the level of corruption will be higher.

4.1 Major Comments and Suggestions

1. Many of the issues identified with chapter 2 apply here as well - most importantly, point 1 in Section 3.2 above. See p104 for an example of mixing assumptions and the solution. The problems get even worse in this chapter - the author postulates some new non-trivial assumptions even in the proof (e.g. $q_i^p = q_i^e = 1$ on p106, or $E[e_A^j = q_A^j]$ on the same page). Probably the most striking example is the proof on p109. In addition to the probability of voters setting \tilde{g}_o not being modelled properly (see point 2 in Section 3.2 above), some assumptions on its properties are first introduced in the proof without a justification. Several lines later the reader finds that these very assumptions bear the weight of the proof. This is very unsatisfying.
2. P107 claims that the $q_A^{p*} = q_B^{p*}$ and $q_A^{e*} = q_B^{e*}$ follow from equation (7). Well they do not - I find the proof incomplete.

3. Income of voters is postulated to be a function of work effort, but this does not play any role in the analysis and should be deleted for notational parsimony, (see point 3 in Section 3.2 above)
4. The author states that transforming private output into public good is costly. However, in equation (3) when θ is introduced, the appropriate assumption of $\theta > 1$ is not imposed.
5. Proposition 1 talks about ‘...the game of maximizing the expected utilities of all players’. This is a strange label, is it just a different way of saying that the players are rational? In general, the formulation of Proposition 1 needs to be improved.
6. Using words like hypothesize should be, where possible, replaced by words like ‘the model predicts...’
7. P100 contains a long paragraph about a subsequent (currently non-existing) paper. If anything, it should be only briefly mentioned, but focus should be on the work actually done in the thesis. Furthermore, the same paragraph (word for word) is pasted on p112. It should again be deleted.
8. p103/2 postulates that there are $M/2$ owners of both media A and B. At this point there is no reason for this constraint and it should be explained.
9. p103/15 states that ...‘the cost is a random variable, realized at the beginning of the period by the politician and the media owners.’ My understanding of a random variable is that it is given by the move of nature, not by somebody’s choice.
10. The author does not clearly distinguish between correlation, causation, and feedback. All claims should be clear about which of these is implied by the model and/or the data.

4.2 Minor Comments and Suggestions

1. The author assumes that (i) there is some probability (less than one) that a voter spots the news, but that (ii) if s/he does then with probability one s/he will correctly infer the cost. It would be more realistic to reverse these assumption, i.e. assume that the voter will surely spot the news if it appears in the newspaper, but that the voter may not be able to discern the cost from it correctly. This would also allow the author to consider situations in which the voter would ignore a noisy signal and in which the newspaper may engage in ‘cheap talk’.

5 All Chapters

5.1 Major Comments and Suggestions

1. Throughout all chapters, the authors should try to provide more intuition for their results from within their model and identify the driving forces.
2. Furthermore, while I do not mind the dissertation to consist of three separate papers, I do mind that the author did not provide a concluding section in which he would try to summarize the lessons learnt and relate/contrast the chapters. Given that the dissertation is about regime change, some conclusions about regime change should be drawn.
3. The literature review sections are detached from the specific focus of each paper. They should try to link the existing literature directly to what is done in the particular paper (and if no such link exists they should be deleted²). When doing so it is often beneficial to incorporate the references into other sections. For instance, in Chapter 1 the explanation on p8 why a simple instrument (Taylor type) rule is used instead of an optimal targeting rule would be better placed on p18 when the Taylor rule is postulated rather than in the literature review.
4. The notation in the thesis can be significantly improved. Some variables are undefined (e.g. ε in equation (13) or γ on p75), or only defined later (e.g. footnote 1 should move two sentences up; p15 $\varepsilon^{\pi,y,i}$ are only defined on pp17-18; MEBO on p66 is only defined on p72). Sometimes the same notation is used for different variables (in chapter 1 H denotes habit and home). Some variables are defined twice (e.g. W_t on p10; π_i on p106; and γ on p108).

In many equations there are brackets in which only one variable appears. It implies that the variable in bracket is a function of the preceding variable, rather than multiplication as intended by the author. For example in Chapter 2 see o_2^D and o_3^D in equation (1) and throughout, in chapter 3 see g and q_i^e in equation (1) and throughout.

The author should also be more precise in defining variables - some important information is missing. For example, on p20 the construction and intuition of the indicator v_t has to be better explained, it is not obvious; in the second column of Table 1 on p71 election year does not specify what year exactly it is; τ on p74 are lump sum taxes *per individual*; on p74 $\max(r_t)$ is not defined properly nor explained; γ on p104 is described before r , despite the fact that it measures the weight on r etc.

5. The thesis needs a very thorough round of editing, both factual and linguistic. This is to fix a number of typos and grammatical errors. In terms of typos, to name some examples:

²For example there is not a single mention of regime change in the literature review of any chapter.

In equation (13) the bracket of $(1 - \varepsilon)$; p15/3 good -> goods; p15/18 of of; p20/9 full stop missing (and several other places); transition economies notation incorrect at places (e.g. on p26/13, p27/2 and p27/7 ET3 appears twice where ER10 should appear); 65/last I -> in; p26 capital t; p27 reference format; p68/6 redundant text; p98/24 of -> on; footnote 11 refers to Appendix 5.1 but there is no such thing (it is 3.4); p108 equity -> equality. Also, the formatting can be improved, e.g. alignment on p72 etc.

In terms of grammar the main problem is the use of articles. There are too many for me to state (average 4-5 per page). Many countable nouns like rule, rate, era, regime, empire, public, party, method, seeker are used repeatedly without an article. Some non-countable nouns are used with an article and they should not be, e.g. corruption, politics etc. Finally, a definite article is often used instead of an indefinite one and vice versa.

6. The bibliography section must be significantly improved. Just to name a few problems:
 - It contains references to working papers that have been published many years ago (e.g. Clarida et al (2001)).
 - Some papers appear twice (or even three times, e.g. by Gali and Monacelli).
 - It should only report the URL for unpublished papers or those only published online.
 - Page numbers are often missing.

6 Conclusion

The dissertation examines three political economy situations in which regime change plays some role. The issues tackled are interesting, and upon improvement of the modeling and presentation aspects of the papers (see my suggestions above), they are likely to constitute valuable contributions to the literature.