ABSTRACT

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Essay 1 investigates an empirical link between institutional variables and the performance of firms based on cross-country firm-level survey data. Current empirical evidence based on this type of data is unsatisfactory because employing survey responses as direct measures of institutional concepts and using those to analyze the effects of institutions at the firm level would limit the researcher to findings only *within* countries effects. This happens at the expense of losing inherent cross-country variation in institutions. Essay 1 offers a simple conceptual framework that decomposes survey responses for each firm into the average of their country and a residual firm-specific component. Importantly, the estimation results indicate that both variations have clearly different effects on growth of sales of firms.

Essay 2 estimates the causal effects of economic shocks on the incidence of politically destabilizing events. The estimation is difficult due to the joint endogeneity between economic growth and events related to the political environment, which is

addressed by the instrumental variable method. The variation in oil prices is used as an instrument for economic growth in the sample of small oil importing economies during 1960 – 1999. In contrast to a common belief and OLS estimates, the most striking finding of the IV estimation is that higher economic growth has a strong and robust positive effect on the incidence of relatively peaceful unrest such as demonstrations, strikes and riots.

Essay 3 studies the question of differences in economic growth rates between Democratic and Republican governorships in the United States. The question is difficult to answer by simply comparing growth rates because the party affiliation is not randomly selected during elections. The empirical analysis employs the Regression Discontinuity Method to address the endogeneity in the party control variable. Focusing on very close elections permits the generation of quasi-experimental estimates of the impact of a "randomized" change in party control at the 50 percent threshold. When comparing Democratic with Republican governorships, the results are suggestive about the possibility of slightly worse performance of Democratic governors but the lack of statistical significance does not fully support this evidence.

EMPIRICAL ESSAYS IN COMPARATIVE INSTITUTIONAL ECONOMICS

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Chapter 1: Disentangling the Effects of Institutional Perceptions of Firms in Transition

1.1 Introduction

In recent years, the role of institutions for development has attracted a considerable attention. It has been argued that institutions and institutional mechanisms for growth and development provide the "missing link" that explains differences in growth rates and development paths across countries. Institutions are generally defined as "constraints that human beings impose on themselves" (North 1990).[1] Most of the recent articles define institutions in a broader sense, linking various different measures of institutional quality to development outcomes from various angles and disciplines.

There is a growing consensus regarding the importance of institutions for development, with many studies providing strong evidence at the macro level using such economic outcomes as growth and levels of income.[2] The rapidly developing set of available institutional indexes for many countries has made it possible for such empirical efforts to achieve meaningful results. Cross-country comparisons have focused on determinants of growth and levels of income. Despite some weaknesses related to problems of definition, causalities and proper interpretation of findings, there is little doubt that cross-country variation in institutions is crucial for economic outcomes.

[1] Other scholars include in their definition of institutional organizational units, procedural devices, and regulatory frameworks (Williamson 2000).

[2] Starting with early contributions by Knack and Keefer (1995) and Mauro (1995) and more recent work of Hall and Jones (1999), Acemoglu, Johnson and Robinson (2001, 2002), Easterly and Levine (2003), Dollar and Kraay (2003), and Rodrik, Subramanian, and Trebbi (2002) have suggested the effects of good institutions on economic growth.

Further insights can be gained if one focuses on microeconomic channels linking institutions to macroeconomic outcomes. Why various countries' aggregate economic outcomes are so closely linked to their levels of institutional development? One obvious candidate for this transmission is the performance of firms and their behavior, which result from a particular institutional setting and related business environment.

The micro-economic empirical evidence is limited not only in terms of the amount of the evidence produced but also in terms of the reliability of the data and, more importantly, of the interpretation of the results. The evidence is limited because quantifying institutions and their interactions with economic agents is an inherently difficult task. Available data is often of dubious quality or nonexistent, and many important aspects are not quantifiable. In order to obtain needed information, it has become increasingly popular to ask the general public, managers of firms, and officials their views about the performance of public institutions and levels of corruption. Such data are easy and relatively inexpensive to obtain, particularly in comparison with detailed information on the actual day-to-day operation of state institutions.

Although the use of survey data has improved radically the potential of studying institutional influences at the micro level – putting aside issues related to the quality of collected data – there are significant methodological gaps in the empirical literature. Overall, the evidence is scattered and chaotic, very little attention is paid to conceptual basics about how to systematically approach the micro-level survey data and how to interpret its results. Typically, studies employ individual responses as direct measures of institutional concepts addressing, to a certain extent, the potential perception bias which

is considered to be one of the major issues. However, other important conceptual problems in some of the well cited literature need to be addressed. In particular, country dummies are often used to address problems of omitted unobservable effects. This happens at the expense of losing inherent cross-country variation in institutions[3]. On the other hand, the interpretation of regressions without country fixed effects is unsatisfactory because these studies typically claim to capture "full" variation across countries.[4] However, the analysis in this chapter shows that these regressions still capture "within" effects. Furthermore, this chapter argues that institutional measures drawn from surveys combine objective variation at the national level (countries) and variation in responses of managers conditional on the situation at the firm level. One would expect these sources of variation to have different effects and policy implications. This chapter offers a simple conceptual framework that decomposes survey responses for each firm into the average of their country and a residual firm-specific component.

Importantly, the results show that both variations have clearly different effects on firms' performance variable in regressions. The estimation procedure uses the firm-level survey data (BEEPS, 2002) to examine the effects of the different institutions and specific characteristics of enterprises on the economic performance of the firms in transition measured by growth of sales. Institutional variables are *quality of courts, regulatory burden, corruption, and obstacles to financing*. The estimates of firm-level assessments of institutions on growth of firms can only provide *within* country effects: damaging effects of higher levels of *regulatory burden*, *corruption*, and *obstacles in financing*, beneficial effects of better *quality of courts* and more frequent bribing of

[3] Hellman et al. (2003) is one of the most representative examples
[4] For instance, see Johnson et al. (2002)

legislators to influence laws and regulations (state capture). In contrast to firm-level estimates, we found a significant evidence of nonlinear effects of country-level *corruption* and *regulatory burden* on growth of sales of firms. The country-level *regulatory burden* measure displays a Laffer-type effect with respect to growth of sales while *corruption* appears to "ease" the impacts of higher regulation levels. *State capture* helps individual firms for a given country while such corruption has sizeable but less robust negative effects at the national level, which is a relatively scarce quantitative result in the literature that has only conjectured the negative country effects of capturing politicians.

Overall, this chapter offers a conceptual framework to analyze firm-level institutional survey data in transition. Analyzing the experience of transition countries offers an additional advantage because it allows studying the behavior of economic agents in the rapidly changing institutional environment.[5]

The analysis offers a straightforward framework in Section 1.2 and links it to existing literature; this model is also used as guidance to initial estimations steps. Section 1.3 discusses some specification concerns. Section 1.4 describes the data and identifies the relevant variables and their measurement. Initial results in Section 1.5 show symptoms that induced more careful reconsideration of the functional form, while Section 1.6 provides robustness tests and Section 1.7 concludes.

[5] See Murrell (2003) for detailed analysis on evolution of institutions in transition. Ayyagari, Demirguc-Kunt and Maksimovic (2006) also found empirically that institutional factors explaining property rights protection in former Socialist countries are different from institutional factors in other countries.

1.2 Analyzing Survey-Based Institutional Micro Data

The cross-country studies mentioned in the introduction arrive at a consensus that institutional quality matters for growth. Yet, some researchers question the relative importance of "institutions" versus other factors, like geography (Sachs 2003) and trade (Dollar and Kraay 2003).[6] Whereas the earlier literature used variables such as political violence and civil liberties to proxy for institutions, the more recent literature focuses on measures that capture institutional quality by referring to the risk of expropriation, degree of corruption, quality of bureaucracy and strength of the rule of law (Kaufmann and Kraay 2002, Rodrik, Subramanian and Trebbi 2002). In this chapter, we will use variables similar to these last three. This is because they seem not only to be important in the cross-country studies, but also appear to have the most significant effects on the performance of firms in the data under consideration. Furthermore, some authors point to the existing shortcomings of cross-country studies. The identified weaknesses relate to problems of definition, causalities and proper interpretation of findings (Glaeser et al. 2004). Nevertheless, there is little doubt that cross-country variation in institutions is crucial for economic outcomes.

The data on such variables as corruption, rule of law, and quality of bureaucracy is not easily available, given the fact that these concepts are very hard to quantify. Therefore, as institutional topics became very popular in the mid and late 1990's, the use of micro surveys has become increasingly popular. Such surveys typically ask the general public, managers of firms, and officials to evaluate the performance of public institutions and levels of corruption. International organizations and development agencies, such as

[6] See footnote 3 in Introduction section for additional references.

the World Bank, EBRD, and USAID, have been putting significant resources into conducting such surveys. In particular, since the mid-1990s the World Bank has been very active in conducting business surveys worldwide, especially in transition countries. For example, their publicly available databases include World Development Report 1997 Business Survey, World Business Environment Survey 2000/01, Business Environment and Enterprise Performance Survey (BEEPS) 1999 and 2002. The surveys in similar fashion ask firms about key aspects of state institutions, such as business regulation and taxation, law and judiciary, as well as infrastructure and finance.

Before going into the review of the existing evidence on cross-country micro institutional surveys, it is helpful to introduce a simple empirical framework for analyzing such data that is thought to be applicable for studying institutional effects on economic outcomes for and behavior of firms.

1.2.1 Conceptual Framework

There are several factors that may affect individual responses to questions evaluating institutions in a survey. Fist of all, a situation at the country level should determine variation across countries. If the number of respondents is sufficient, a simple averaging of individual scores in a particular country should provide a reasonable measure of country-level institutions that can be compared across countries.

Within a country, individual answers are determined not only by the national level but may also significantly deviate from it because of specific situation at a given firm, available information set and individual respondent's characteristics. Situations at the

firm may vary and depend upon the firm's specific characteristics, such as industry, ownership structure and history, location, size, and age of the firm. These can be included as controls in our model. In addition, individual responses may be affected by any new relevant information on the basis of which the preexisting opinion is updated. New information may be the result of a very recent experience or can be based on other trusted sources; it may be common and shared among everybody in the same country, but it can also be specific to an individual. Moreover, the way information is processed may vary between managers due to their personal characteristics, such as analytical abilities, age, education, political beliefs, and personality in terms of the level of skepticism, criticism, or cynicism. In sum, at any given point in time, the respondents in the same country may have different opinions about the same institution. As one can see in Appendix 1.1, Table A1.2, averages of standard deviations of institutional measures within countries are much higher than averages of standard deviations of these variables across countries in BEEPS 2002 data.

It is important to examine the effects of individual valuations of institutions, since they certainly affect a firm’s investment and restructuring decisions, as well as other strategic business decisions, such as product innovation, hiring policies, and so forth. Therefore, both country level measure of institutions and an individual manager's valuation of institutions directly determine the firm's business outcomes. One does not want to count the effect of the national variation twice. Thus, the individual effects can be measured as deviations from the national level. If the outcomes of the firm's business behavior and strategies are observed through its performance, we can write a simple equation describing the effects of institutions:

$$Y_{is} = \alpha + \beta \overline{X}_s + \gamma (X_{is} - \overline{X}_s) + Z_{is}\theta + u_{is}, \quad s=1,\ldots, S \quad i = 1,\ldots, I_s \qquad (1.1)$$

where *i,s* are firm and country subscripts, Y_{is} is the measure of the performance of firms. X_{is} represents the firm-level measure of institutions, $\overline{X}_s$ represents the national level of the quality of the institution measured as an equally weighted average of all individual responses within the country, and Z_{is} is the vector of specific characteristics of firms. In equation (1.1), β represents the cross-country effects of institutions while γ indicates the effect of the firm-level institutional variable measured by manager's opinion in the survey. The latter reflects varying degree of institutional constraints for different firms within countries. For instance, bigger firms may have easier access to finance (Beck et al. 2005).

To attain further insights, it is useful to see the connection between equation (1.1) and the country fixed effects model that is widely used in the literature. From (1.1),

$$Y_{is} = \alpha + (\beta - \gamma)\overline{X}_s + \gamma X_{is} + Z_{is}\theta + u_{is} \qquad (1.2)$$

By averaging equation (1.2) over *i's* within *s,* one obtains cross-country equation, which only uses variation between countries:

$$\overline{Y_s} = \alpha + (\beta - \gamma)\overline{X_s} + \gamma \overline{X_s} + \overline{Z_s}\theta + \overline{u_s}, \qquad (1.3)$$

Subtraction of equation (1.3) from equation (1.2) for each *i* results in the fixed effects transformed equation, also called the *within* estimator as it uses the variation of firms within countries:

$$Y_{is} - \overline{Y_s} = \gamma(X_{is} - \overline{X_s}) + (Z_{is} - \overline{Z_s})\theta + u_{is} - \overline{u_s} \qquad \text{or}$$

$$\widetilde{Y}_{is} = \gamma \widetilde{X}_{is} + \widetilde{Z}_{is}\theta + \widetilde{u}_{is} \qquad (1.4)$$

Estimating equation (1.4) is equivalent to estimating:

$$Y_{is} = \alpha_s + \gamma X_{is} + Z_{is}\theta + v_{is}\,, \qquad (1.5)$$

Different intercepts α_s are capturing systematic differences in the dependent variable and explanatory variables across countries and $\gamma's$ represent within country firm-level effects. For example, country dummies would control for cross-country differences in institutions. Note that estimating the effects of deviations of individual measures from the national level in equation (1.1) is equivalent to capturing the within country effects of individual measures in country fixed effects specifications (1.4) and (1.5).

Equation (1.1) will become the main equation of interest. This simple framework will also allow one to see drawbacks in existing literature that deal with cross-country micro-level data. In particular, the country fixed effects specification (1.5) will only provide estimates of γ or within country effects of the variation in measures of institutions at the firm-level, and the researcher does not obtain any information on β, i.e. missing completely cross-country differences. On the other hand, exclusion of country-level controls $\overline{X}_s$ in estimation implies that the $(\beta - \gamma)$ term in (1.2) is restricted to zero, effectively imposing the condition that cross-country impacts are equal to within country effects, which may not be true.

1.2.2 Existing Evidence

The styles and approaches of the research work analyzing the aforementioned cross-country surveys can be summarized by some examples in the literature.

The work of Johnson et al. (2002) uses the perception survey in five transition countries to show that perceived weak property rights discourage firms from reinvesting their profits irrespective of whether the external finance is available or not. The authors use both specifications with and without country dummies. They acknowledge that much of the variation in the security of property rights is across countries rather than within country; therefore, specification without country dummies captures the full effects of property rights. However, according to the logic introduced earlier in the previous section, such interpretation of the results is misleading since the model without country dummies would still capture the variation in perceptions within country imposing the restriction that cross-country effects are the same as individual effects. Consider a simple bivariate regression model:

$$Y_{is} = a + bX_{is} + \varepsilon_{is}$$

If the correct model is

$$Y_{is} = \alpha + \beta \overline{X}_s + \gamma (X_{is} - \overline{X}_s) + u_{is}$$

then the expected value of estimated *b* will be

$$E(\hat{b}) = \gamma + \frac{\text{cov}(X_{is}, \overline{X}_s)}{\text{var}(X_{is})}(\beta - \gamma) \qquad (1.6)$$

Johnson et al. (2002) did not find much difference in the estimates for regressions with and without country dummies, which led them to conclude that within country coefficients are not different from cross-country effects, $\beta=\gamma$. However, the size of the ratio $\frac{\text{cov}(X_{is}, \overline{X}_s)}{\text{var}(X_{is})}$ is rather small for all institutional measures in the data used in our work. For instance, the value of this ratio for state capture variable is 0.026, so that the term $(\beta - \gamma)$ is deflated by 36 times.[7] We suggest that they found in both cases within countries firm-level effects of perceived insecurity of property rights.

Hellman et al. (2000) review the results of a subset of questions from the 1999 BEEPS relating to governance and corruption, as well as detailing the sample structure and methodology. The results are reported at the country level and several governance indices are also constructed on the basis of different dimensions of governance. On the basis of this data, Hellman et al. (2003) studied state capture and influence in transition economies. They first analyzed the determinants of being a captor or an influential firm then try to find the effect of these two on the firm's performance controlling for country fixed effects and the firm characteristics. In their study, state captors are "firms that make private payments to public officials to affect rules of the game," while influential firms are those "that have influence on those rules without recourse to private payments to public officials".[8] The relevant survey question about state capture was: "How often do firms like yours need to make extra, unofficial payments to public officials to influence

[7] Smallest inverse value of the ratio was 16.7 for the quality of courts, 18.2 -- for regulatory burden, and about 19 -- for corruption and state capture measures

[8] Although such distinction could be applied to the early years of the transition, but after almost a decade such difference seems not to be relevant. Nowadays, as the author's personal observations based on anecdotal evidence suggest that influential firms typically reward officials in one way or another for received favors. Moreover, the "influence" question in the survey did not specify whether the influence was accompanied with private payments, so one cannot really distinguish between the two.

new laws, decrees and regulations?" The authors claim that they found that state captors enjoy higher growth rates of sales. Finally, all specifications use country-fixed effects and; therefore, cross-country effects are not available.

Carlin et al. (2001) investigated factors that influence restructuring by firms and their subsequent performance by using BEEPS 1999 data. They specifically analyzed the impact of the competition, ownership, soft budget constraints, general business environment, and a range of measures about the intensity of competition as perceived by managers of firms. The institutional influences were not a major concern of their study and were summarized as a business environment measure by using a principal component method combining the different perceptions of obstacles in corruption, macroeconomic instability, tax administration, business licensing, financing, and crime. They found that the competition and restructuring effects on performance are very strong and robust, and privatization is not significant. Yet, they caution that such results may be affected by the selection bias. The business environment measure becomes less significant after controlling for endogeneity by applying the method of instrumental variables to a country fixed-effect model. As instruments they use the interaction of a firm-level competition measure with the country dummy, claiming that in this way the differences across countries in business environments are modeled. The validity of such an approach is not very well explained in the study, so that immediate doubts arise since the competition measure itself enters directly into the performance equation. The fact that they validate their instruments by the test of overidentifying restrictions is not convincing, as the test can only suggest that instruments may not be valid, something that cannot validate their use.

Fries et al. (2003) provided an overview and summarized the main findings of the 2002 BEEPS survey. They divided measures of the business environment into qualitative and quantitative measures. By comparing qualitative measures with objective statistical measures and quantitative survey measures, they concluded that these appear to provide reasonably accurate measures across various dimensions of the business environment and countries. Among other things, they examine the effects of country-level aggregate demand growth, quality of the business environment, and firm-level factors on enterprise performance measured by real growth in fixed assets, trend changes in productivity, and real sales growth. Results show that firms engaged in state capture enjoy higher investment rates and revenue growth rates while firms that report being affected by state capture have slightly lower estimated parameters due to external costs of capture. In relation to our analysis, the state capture effects are estimated only at the firm-level but not at the national level. Fries et al. (2003) also found that the country-level index for the quality of the business environment is positively associated with higher investment rates but not with growth of sales, which is attributed to multicollinearity with the aggregate output growth term. In contrast with other studies reviewed here, Fries et al. (2003) considered only the variation across countries in aggregated measure of the business environment but not the individual level effects on performance of firms.

Beck, Demirguc-Kunt, and Maksimovic (2005) study how growth of firms is related to financial, legal and corruption obstacles and whether these effects vary with the size of the firms. Similarly, Ayyagari, Demirguc-Kunt and Maksimovic (2005) compare the effects of a broader set of institutional obstacles on growth of firms. Both studys use the World Business Environment Survey 2000 (WBES). It is very similar to BEEPS survey

but covers more countries. Authors examine the effects of firm-level measures of institutional obstacles on growth of sales. Beck et al. (2005) do control for country-level inflation, income per capita, income growth and external to survey measures of financial and legal development while Ayyagari et al. (2005) control only for levels of GDP per capita. The estimation framework is country random effects model. However, the random effect model assumes that unobserved country-level error component is not correlated with observed explanatory variables, which is a very strong assumption. If such correlation is present then random effect estimator is inconsistent (Wooldridge 2001, p. 257). Beck et al. (2005) found that perceptions of managers about financial, legal and corruption obstacles are significantly related to the firm's growth. They also found that smaller firms are more constrained while country's financial and legal development weakens these constraints especially for smaller firms[9]. Ayyagari et al. (2005) find that most important obstacles for growth of firms are obstacles related to financing, political instability and crime. Separate effects of country averages in obstacles are not considered in Beck et al. (2005). However, country averages of obstacles measures are used as instruments for firm-level variables.

Among other empirical studys, Kaufmann and Wei (1999) and Gaviria (2002) use business survey databases to investigate the effects of corruption and regulation. Kaufmann and Wei (1999) separately ran cross-country and within country regressions to find that bribery increases the time spent with public officials, arguing that this result is inconsistent with the enhancing role of corruption. Gaviria (2002) also used a similar dataset to investigate how corruption impacts the growth of sales and investment in firms.

[9] Country-level institutional variables are interacted with the firm's sizes.

He abstracts from cross-country differences and finds that at the firm-level, the corruption measure negatively affects economic performance.

In summary, cross-country studies show that institutional differences across countries matter a lot. Furthermore, most of firm-level studies use country-fixed effects estimation and ignore cross-country variation in institutions. Finally, often cited study of Johnson et al. (2002) claims to capture "full" effects by not using country dummies, which is misleading as their regressions, in fact, capture only within country individual firm's effects.

1.3 Specification and Estimation Issues

To avoid the problem of missing cross-country variation, we chose a specification that uses both within and between countries variations in institutional variables, as in equation (1):

$$Y_{is} = \alpha + \beta \overline{X}_s + \gamma (X_{is} - \overline{X}_s) + Z_{is}\theta + u_{is} \qquad s=1,\ldots, S \;\; i = 1,\ldots, I_s$$

The major caveat with estimated γ's is the reverse causation problem. The manager, who is unhappy with the current and past performance of the firm, is likely to report more negative assessments of existing aspects of institutions as opposed to the reports of a happier manager whose firm performs better (perception bias). For instance, if the measure of corruption negatively affects growth of sales in OLS regression, the feedback effect from growth is likely to induce downward bias in OLS estimates so that $\gamma_{OLS} < \gamma$.

The explicit and accurate correction for this type of endogeneity is beyond the scope of this study but Appendix 1.4 provides a crude assessment of the problem. It has been

found that the problem is likely to be small in our sample. In addition, all the sensitivity tests in subsequent sections of the chapter display a remarkable robustness of the firm-level estimates. Moreover, Fries et al. (2003) also conclude that the extent of the perception bias in the BEEPS 2002 data is not significant. Beck et al. (2005) in their robustness tests found that their γ's are not very sensitive to employing IV estimation. Nevertheless, one needs to be careful with the interpretation of the firm-level estimates and evaluate whether the signs of estimated coefficients agree with the possible direction of expected bias.

Feedback effects are unlikely to be sizeable and systematic from an individual firm's performances to country measures of institutions; therefore, β's are likely to be unaffected by reverse causation.

The country-fixed effects framework suggests that γ's are unbiased and consistent with respect to a country's unobserved variables in the absence of other sources of biases.

Other problems may arise in estimating the model. One concern is that error terms are correlated within each country, if one expects that firms share unobservable characteristics. It would be reasonable to assume that observations are independently distributed across countries. However, error terms within countries may not be independent. The OLS estimators are still unbiased but not efficient (Wooldridge 2001, Moulton 1990). The standard procedure in the applied work is to use clustering methods to correct estimated standard errors. Statistical packages, such as Stata, allow for the computation of standard errors that are robust to arbitrary within cluster correlation as well as arbitrary heteroskedasticity. However, the asymptotic theory of clustering

methods justifies using clustering methods when the number of groups is infinite. The robust variance matrix for a pooled OLS estimator with a small number of clusters *S* can behave poorly (Wooldridge 2001, p.331, 2003, 2006, Donald and Lang 2005, Cameron et al.2006). When *S* is very small, Wooldridge (2003) notes that estimating β relying on a large *S* asymptotic can be misleading, often resulting in a downward bias in estimates of standard errors. In our study, there are 25 countries and 140-333 firms within countries. There is no explicit recommendation in the literature about how large the number of groups should be in order to get reliable standard errors estimates. In panel data, Kezdi (2004) uses Monte Carlo simulations to show that the fixed effects estimator (γ), adjusted for clustering, works reasonably well when the number of cross-sections (*S* between 10 and 50) is not large relative to the time dimension. Cameron et al. (2006) show in their simulations that for S=25 standard cluster method performs reasonably well and estimates of standard errors are more accurate than those obtained from the unadjusted OLS estimator. Hence, we chose to use a standard clustering adjustment in our regressions.

1.4 Survey and Institutional Measures

1.4.1 Sample

The survey covers over 6000 firms surveyed in 2002 in the following 25 countries:

Albania, Armenia, Azerbaijan, Belarus, Bosnia & Herzegovina (BiH), Bulgaria, Croatia, Czech Republic, Estonia, Georgia, Hungary, Kazakhstan, Kyrgyzstan, Latvia, Lithuania, Macedonia (FYROM), Moldova, Poland, Romania, Russia, Slovakia, Slovenia, Ukraine, Uzbekistan, Yugoslavia.

The survey included a minimum of 170 firms from each country, with larger samples in Poland, Ukraine, and Russia (around 500 firms). Firms were sampled randomly from business directories, although minimum quotas were imposed for state-owned firms, size of firms, and industries.[10]

About 62 percent of the firms in the sample were newly established private firms, 13.8 percent were state-owned (more than 50 percent of shares), and 15.2 percent of the firms were privatized, having zero or minor state ownership. The sample is dominated by small and medium-sized enterprises: 68 percent of the sampled firms employed fewer than 50 persons, 19 percent employed between 50 and 250 people, and 14 percent were large firms. 39 percent of the firms were in industry while the rest were in services. About 32 percent of the firms were located in the capital, 20 percent were in large and medium sized cities (>250000), 23 percent were in small cities (50000-250000) and the remaining 25 percent in towns and rural areas. (see Table 1.1)

Table 1.1 Basic firm characteristics of the sample

Characteristic	Sample share (in per cent)
Sector	
Industry	38.7
Services	61.3
Size (Number of employees)	
Small (2 to 49)	67.6
Medium (50 to 249)	18.5
Large (250 to 9,999)	13.9
Ownership	
New private	61.9
Privatized	15.2
State owned	13.8
Location	
Capital	31.9
Large cities (not capital)	19.6
Small cities	23.4
Rural areas	25.1

[10] A very detailed description of the BEEPS 2002 data can be found in Fries et al. 2003

1.4.2 Performance of Firms – Growth of Sales

The growth of sales was used as the dependent variable reflecting the firm's performance. Entrepreneurs were asked for the real growth of sales over the previous three years. The concern here is the accuracy of the survey measures. It is quite possible that not all managers knew the exact economic meaning of the term "real." In addition, responses critically depend upon translations and the ability of interviewers to explain the question. Nevertheless, in the absence of more precise measures from accounting records or firms' annual reports, survey-based measures do provide useful information on the performance that can be compared among firms.

In the sample, 30.4 percent of firms reported negative growth rates of in sales per worker and 61.5 percent reported positive growth.

1.4.3 Measuring Institutions

The survey asks the managers of firms their views about the performance of public institutions and the incidence of corruption. If the goal is to assess the actual performance of institutions, one must first know the degree to which the public's image reflects what the function of an institution and how well it accomplishes this function. Unfortunately, there is a substantial risk that such an opinion tells us less about the supposedly sad state of an institution and more about the sad state of the opinion, or worse, public misinformation that may originate in an incomplete information problem. There are important differences in how an institution is perceived depending on whether that perception is based on personal experience or indirect sources such as rumor and news reports. Although managers in the BEEPS survey are less likely to be biased in

comparison to an average citizen, as their perceptions are formed in the firm's day-to-day interactions with institutions, the above concerns still remain. There is also formal evidence that perception bias is not significant problem in BEEPS survey (Fries et al. 2003) and in a very similar WBES survey (Beck et al. 2005).

Table 1.2 shows correlations among survey country measures used in the study and the 2002 World Bank's index of regulatory quality, and the Transparency International (TI) corruption index (see definitions of the variables in Appendix 1.5).

Table 1.2 Correlations of country averages with external to survey measures

External Survey	WB Regulatory Quality	TI Corruption Index	State Capture 2
Regulatory Burden	-0.39***	-0.47***	0.09***
Corruption	0.48***	-0.75***	0.13***
State Capture	-0.01	-0.07***	0.72***

*** significant at 1 percent level

Overall these correlations show consistency among measures. There is a high correlation between corruption and state capture measures while the regulatory burden correlation coefficient is lower. This is because the World Bank's measure of regulatory quality has broader coverage of related issues than our variable measured as time tax with officials. In addition, the better regulatory quality does not necessarily imply that the time spent with officials to comply with regulations should be less. In the hypothetical extreme, the absence of regulations implies zero time spent with public officials but this certainly does not indicate that the regulatory quality is high.

1.4.4 Institutions and Survey Questions

The survey offers a wide scope of questions for institutional variables. We can roughly divide them into several broad categories shown in Table 1.3.

Our goal is to find measures for institutions that best explain their economic performance. Many raw institutional measures are highly correlated to each other so picking the most powerful explanatory variables is not an easy task.[11] We used Hendry's approach (Hendry 1979), which starts with a very general model that is over-parameterized, and which is subsequently reduced on the basis of significance tests. By using Hendry's "general to specific" approach, all raw measures that can potentially affect the firms' performance were included in the equation, which created a great deal of noise in the regression coefficients, so the specification was progressively simplified by eliminating variables with low significance to arrive at a final specification that included only one or two variables from each broad category[12].

In particular, the business regulations category was left with "time tax" variable, which we thereafter refer to as "regulatory burden." "Corruption" and "state capture" categories retained the percentage of sales paid in unofficial payments and frequency of bribing of legislators respectively. The rule of law group retained the quality of courts variable which was constructed with the help of the factor analysis discussed in the Appendix 1.2. The financial system was left with the "obstacles in financing" measure in obstacles for business environment question.

[11] See Appendix 1.3 for correlations
[12] These variables are shown emboldened in Table 1.3.

Table 1.3. Questions and grouping for institutional variables[13]

Business regulations	• Consistency and predictability of laws and regulations; Q46, 49 • Customs and trade regulations (# of days for custom clearance); Q25 • Ability to get correct treatment from another official; Q51 • **Time tax (% of time spent with officials); Q50**
Corruption	• Public procurement kickbacks (% of contract value); Q57 • Tax compliance (% of sales reported to authorities); Q58 • **Unofficial payments (**how common, Q54; how often Q56, Q54**; as % of sales, Q55**)
State Capture	• **Payments to influence laws &regulations, how often Q56j** • State capture's impact on the business, Q59;
Rule of Law	• **Quality of the courts, Q41** • Security and protection payments, Q44 • Security of property and contract rights, Q42
Financial system	• Use of accounting standards and external audit, Q73-74 • Use of Collaterals (value; requirements) and loan terms, Q65 • Sources of Finance, Q64 • **Obstacles to Financing, Q80a** • Prevalence Government subsidies, Q79

More specifically, the time tax question supposedly measures the regulatory burden, and is given by responses to the question about the percentage of the senior management's time spent in 2001 in dealing with public officials about application and interpretation of laws and regulation and to acquire or maintain access to public services. The responses to this question are perhaps less sensitive to subjectivity relative to another survey question about government quality, as we believed that managers reported their actual proportion of devoted time, although individual mistakes in reports may be present. Our corruption measure seems to capture the more general level and is given by the reported percentage of total annual sales firms typically pay in unofficial payments/gifts to public officials. The measure of the state capture is given by responses to the question about how often firms would make unofficial payments/gifts to influence

[13] Emboldened text indicates preferred measures. Notation Q# refers to the question number in the actual survey. Exact formulation of questions and correlations within groups can be found in the Appendix 1.3. Full survey questionnaire is available at http://www.ebrd.com/country/sector/econo/surveys/beeps.htm

the content of new laws, decrees, etc. (from 1="never" to 6="always"). Finally, the financial aspect of the business environment includes the measure of obstacles for operation and growth in access to financing with scales from 1 (no obstacle) to 4 (major obstacle). Our institutional measures are summarized in Table 1.4.

Table 1.4. Descriptive Statistics: $Y_{is,}\ \overline{X}_s, (X_{is} - \overline{X}_s)$ *and* Z_{is}

Variable	Level	Mean	Std. Dev	Min	Max	Obs
Growth of Sales, *Y*	Firms	25.20	79.05	-600	990	5736
Courts Quality	Country	0.00	0.22	-0.40	0.43	5977
	Firm	0.00	0.88	-1.93	3.03	4766
Regulatory Burden	Country	7.88	2.72	2.79	12.41	5977
	Firm, %	0.00	11.55	-12.4	78.4	5656
Corruption	Country	1.59	0.78	0.34	3.70	5977
	Firm,%	0.00	3.20	-3.7	48.5	5455
State Capture	Country	1.40	0.17	1.13	1.95	5977
	Firm	0.00	0.93	-0.95	4.8	5166
Financial Obstacles	Country	2.31	0.27	1.62	2.80	5977
	Firm	0.00	1.14	-1.79	2.38	5637
Firms' Age		14.68	18.71	3	202	5977
Ln(Size)		3.19	1.75	0.69	9.21	5948

Firm-level variables are deviations of survey scores X_{is} from the mean $\overline{X}_s$

Note that many survey questions use ordinal scales in the questionnaire, and many studies treat these variables as continuous in the models. However, as the distance between the response categories of an ordinal variable are generally unknown, assuming the continuity is possibly inappropriate. On the other hand, one can consider scales to be ordinal with approximately equal intervals between categories. We can examine this simplifying assumption by entering these variables in the model as category dummies, to

test if differences between subsequent categories are the same. All the variables with ordinal scales have passed this test (results are not shown).

1.4.5 Controls

The firms' specific characteristics need to be included to assess the determinants of the firms' performance.

The age of the firm is included as a control variable since firms are likely to enjoy higher returns and be more flexible for restructuring in the earlier stages of operation. The logarithm for the size of the firm is also included as a control measured by the number of employees. Large firms may have different market conditions or enjoy economies of scale or monopoly power. Also, Beck, Demirguc-Kunt and Maksimovic (2005) find that the effects of corruption, financial and legal constraints on the growth of firms depend on firm sizes.

The firms in the sample can be categorized as being state-owned (more than 50 percent of ownership) and private. Moreover, private firms can be divided into old firms that have been privatized from the state, and new private firms. It seems to be a general observation in the literature that public enterprises are less efficient than private ones (Megginson and Netter 2001). Djankov and Murrell (2002) use the statistical technique of meta-analysis to synthesize the empirical results of over 100 studies and found that the effects of ownership are very important for firms in transition. The regressions below include the dummies *new private* and *state* to control for these differences so the omitted category will be *privatized* firms.

Dummy variables for the different categories of city sizes are also included as controls, since clearly there are larger markets in bigger cities. Sector dummies are included as well.

Another important variable is the degree of market power that firms face in the market. The firms were asked what the result would be had they raised real prices by 10 percent with the scale of 1 – "customers would continue to buy in the same quantities", and 4 – "many customers would switch to competitors".

1.5 Results

Table 1.5 presents the main results of the chapter. It contains four specifications that outline and support the main ideas introduced in previous sections. The results of the regressions are very interesting and provide important insights into what perception-based data can tell about a firm's economic outcomes. First, column (1) shows estimates of the regression that does not include country dummy variables while column (2) controls for country-fixed effects. The estimates of these specifications can be compared to see whether there are substantial differences in estimates. If our model is correct and differences are substantial, then this may indicate that the coefficients in (1) are biased because of omitted country-level variables. To account for cross-country differences, column 3 includes country-level measures of institutional variables, as well as deviations of individual observations from country averages. In comparison to firms–perceptions effects, column (3) estimates show that three out of five country level institutional variables have opposite signs, although large standard errors make these coefficients

insignificant. Subsequently, Specifications 4 is an attempt to resolve these paradoxical opposite signs.

1.5.1 Specifications 1 vs. 2

It seems that within country variation of firm-level measures of institutions is very important for their performances, and evidence is generally consistent with other researchers' findings.[14]

The specification (1) in Table 1.5 does not control for country differences while (2) includes country dummies so that coefficient estimates represent within country effects of the firms' perceptions of the institutions on their performances. We disagree here with Johnson et. al (2002) that estimates in column (1) indicate the full effects of institutional indices that combine both across and within country variation. Rather, they show the influence of variation in firm-level measures of institutions within countries. Note that the estimates of γ's in column (2) are not subject to the country level omitted variable bias and the noticeable differences in estimates between columns (1) and (2) should tell us whether the coefficients in (1) are sensitive to omitted country level influences because estimates in (1) can be viewed as γ's in (2) affected by the omitted cross-country variation.

Both (1) and (2) show that *quality of courts* variable at the firm level has a positive sign and is marginally significant in (2) at 10 percent level. This result is consistent with already existing evidence that better legal institutions promote economic growth and

[14] Hendley et al. (2000, 2001), Murrell (2003), Johnson et al.(2002), Murrell (2005), Beck et al. (2005), Ayyagari et al. (2005) Hellman et al. (2003), Fries et al. (2003)

better trust in courts improves performances (Hendley et al. 2000, 2001, Murrell 2003, Johnson et al. 2002, Murrell 2005, Beck et al. 2005).

The *corruption* and *regulatory burden* coefficients in column (1) are almost two times smaller in absolute value than within country estimates in (2), which implies that there are important omitted cross country effects for these variables. This becomes evident in columns (3) and (4) where country level variables are included in the estimation. *State capture* is advantageous, which is consistent with existing evidence found by Hellman et al. (2003) and Fries et al. (2003) analysis of BEEPS1 and BEEPS2 surveys. The estimated influence of perceived *obstacles in financing* is negative and significant at the 10 percent level.

1.5.2 Specifications 3 and 4

In specification (3), Table 1.5, note that firms' specific coefficients are very similar to within countries estimates in column (2), which is not surprising as they should both be asymptotically equal to γ's in the model.

One can see that country-level effects are much bigger in absolute values although the coefficients are imprecisely estimated. Three institutional variables constructed as countries' averages have opposite signs as compared to firms' perception effects. These variables are *regulatory burden, corruption,* and *state capture* measured respectively as time tax with officials, bribing as percentage of sales, and frequency of bribing politicians.

The negative sign of the *state capture* variable is not surprising and is consistent with the hypothesis that state capture at the country level is associated with external negative spillovers on other firms. This is consistent with literature that suggests negative country effects of capturing politicians (Hellman et al. 2003 and 2002, Fries et al. 2003).

However, country level estimates of the effects of *regulatory burden* and *corruption* show positive effects on firms' performance with the former being statistically significant at only 10 percent. Does this really tell us that more regulations and corruption improve economic outcomes or that relationships are more complex than simply adding effects separately in the estimated regression? [15]

The idea of the implausibility of the simple additive functional form in our model is evident by looking more carefully at regulatory burden impacts. It is important to note that the time burden spent with officials may be correlated with the quality of regulatory environment but does not directly measure the latter. Clearly, there are benefits to imposing regulations, which can not be underestimated. A simple textbook interpretation of such benefits would be the correction of market failures. Creation of antitrust laws and regulations is one of the most cited examples. A very low level of regulatory requirements may be harmful for competition: it may create room for favoritism and discretion, and more generally reflect a weak level of institutional development, whereas excessive regulations would create significant obstacles in the business environment and subsequent waste of resources. Guriev (2003) also notes that the socially optimal level of red tape is not trivial and a zero level is not desirable. Therefore, we suggest a "Laffer"

[15] The importance of non-linear effects of country-level institutional variables on firm's perceptions of security in property rights are also found by Ayyagari, Demirguc-Kunt and Maksimovic (2006).

type of relationship between regulations and the performance of firms so that the appropriate functional form for regulatory burden is quadratic. To our knowledge, such an empirical test has never been performed in the existing literature.

Furthermore, the models from (1) to (3) in Table 1.5 assume that the impact of corruption is the same for all levels of the regulatory burden and vice-versa. This condition seems to be restrictive since the change in response with one variable may depend on the other or the effects of corruption may be modified by levels of the regulatory burden. The "efficient grease" hypothesis (Huntington 1969, Krueger 1974, and Lui 1985) suggests an efficiency enhancing role of corruption. Lui (1996) argued that corruption may be efficient under some circumstances, but it reduces growth because of misdirection or misallocation of the human capital towards rent-seeking activity. In our case, corruption may help firms to avoid excessive regulation at a given point of time; therefore, the use of the interaction term for corruption and regulatory burden is needed if the former is modified by the latter.

Consider a modified fragment of the regression equation (1.1):

$$Y_{is} = \alpha + \beta_2 \overline{R}_s + \gamma_2 (R_{is} - \overline{R}_s) + \beta_3 \overline{R}_s^{\,2} + \beta_4 \overline{C}_s + \gamma_3 (C_{is} - \overline{C}_s) + \beta_5 \overline{R}_s \overline{C}_s + \ldots \quad (1.7)$$

where $\overline{R}_s$ *and* $\overline{C}_s$ are regulatory burden and corruption country-level variables so that partial effects of regulatory burden $\overline{R}_s$ and corruption $\overline{C}_s$ can be presented by :

$$\frac{\partial Y_{is}}{\partial \overline{R}_s} = \beta_2 - \gamma_2 + 2\beta_3 \overline{R}_s + \beta_5 \overline{C}_s \quad (1.8)$$

$$\frac{\partial Y_{is}}{\partial \overline{C}_s} = \beta_4 - \gamma_3 + \beta_5 \overline{R}_s \qquad (1.9)$$

Under the "efficient grease" hypothesis, $\beta_5 = \frac{\partial}{\partial \overline{R}_s}\left(\frac{\partial Y}{\partial \overline{C}_s}\right)$ is expected to be positive. The negative effect of corruption is modified by the level of regulations so that at very high levels of regulations the net effect of $\overline{C}_s$ can be positive. The inverted U-shape relationship between Y_{is} and $\overline{R}_s$ implies that the coefficient β_3 is expected to be negative.

If we add both nonlinear terms into a regression equation, as in column (4), the estimated coefficients show that the above hypotheses about nonlinear relationships are supported by the data.

The data shows a negative and significant β_4 coefficient, which is related to the separate effects of countries' corruption. Based on the coefficients in column (4), the marginal effect of corruption becomes positive at the regulatory burden levels greater than $\overline{R}_s$ = 5.55. Seven countries in the sample have levels of $\overline{R}_s$ below this value: Czech Republic, Armenia, Azerbaijan, Bulgaria, Estonia, Slovenia, and Croatia.

The negative estimate of β_3 makes regulatory burden harmful for firms at levels greater than a certain point, $\overline{R}_s^*$. In order to assess whether the sizes of coefficients are meaningful, it is informative to relate the resulted country-level regression lines to actual data points. If the curvature and peak point for Y ($\overline{R}_s$) depend on the value of $\overline{C}_s$ and β

estimates as in $\overline{R}_s^* = -\frac{\beta_2 - \gamma_2 + \beta_5 \overline{C}_s}{2\beta_3}$, then $\overline{R}_s^*$ for Russia, for instance, would be 7.52[16] while the actual data point is $\overline{R}_s = 9.79$. Figure 1 shows several country profiles where firm's growth of sales is a function of the country's level of regulatory burden. The fitted lines are drawn on the basis of the coefficients in column (4), Table 1.5, where firm-level variables are used as countries' means. The actual data points seem to lie close to plotted lines which indicate that the sizes of estimated coefficients are reasonable, at least not too big or too small. Although the magnitude of the results should be treated with caution due to the possible omitted variable bias, the sign and significance of the coefficients are informative and support the idea of the non-trivial quadratic functional form for the regulatory burden variable and short-term modifying effects of corruption for higher levels of regulatory burden.

1.5.3 Controls

Controls of firms' seem to be very significant and robust in all specifications. The ages of firms have a slightly negative effect on performance. Bigger firms, private start-ups, and firms located in big cities perform better according to all specifications. However, one has to take into consideration the feedback effect from performance to the size of the firm and ownership structure. Better performance would tend to increase the size of the firm and may have an effect on the ownership structure. Accounting for this bias is beyond the scope of this study.

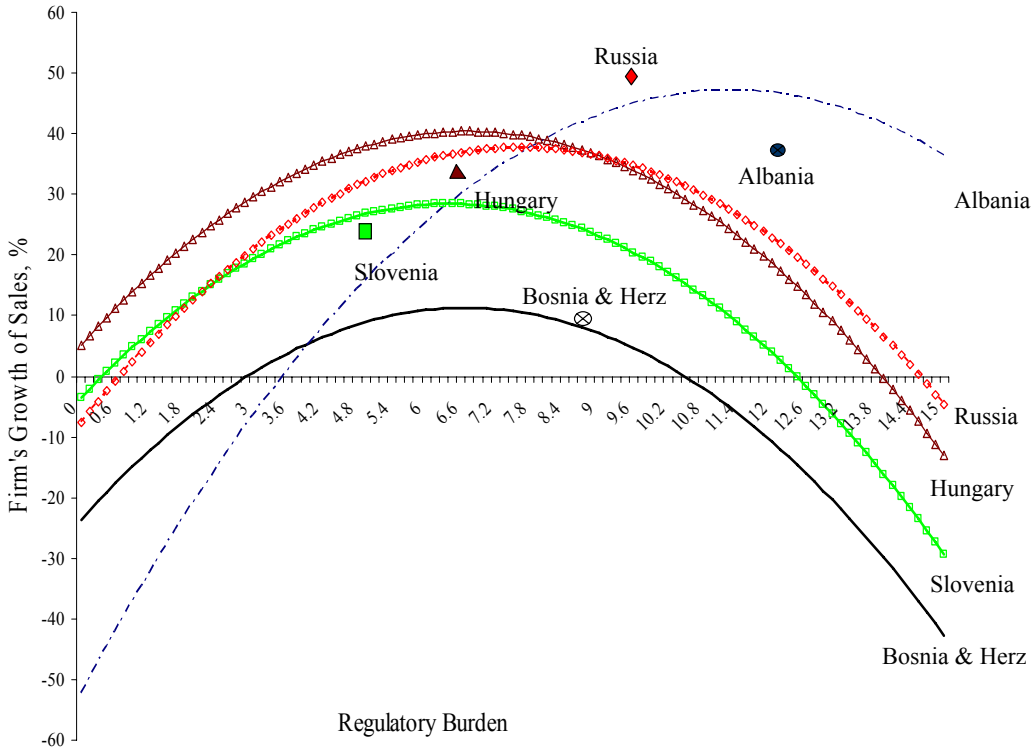
[16] Corruption index in the data is 1.43, see Appendix 1.1.

Table 1.5 Regression results for Firms' Growth of Sales

	Firm-level effects and country FE's		Separating Country Effects		Non-Linear Model in Regulatory Burden and Corruption	
Variables	(1)	(2)	(3)		(4)	
	No FEs	FEs included	Country Effects β's	Firm-level Effects γ's	Country Effects β's	Firms-level Effects γ's
Courts' Quality	2.1	2.23	13.97	2.03	7.94	2.07
	[1.51]	[1.15]*	[14.79]	[1.17]*	[14.94]	[1.15]*
Regulatory Burden	-0.13	-0.24	1.80	-0.23	7.57	-0.23
	[0.10]	[0.08]***	[0.94]*	[0.08]***	[3.43]**	[0.08]***
(Regulatory Burden) ^ 2					-0.78	
					[0.29]**	
Corruption	-0.49	-0.7	5.00	-0.7	-16.88	-0.70
	[0.30]	[0.31]**	[4.26]	[0.31]**	[8.24]*	[0.31]**
Reg. Burden X Corruption					3.04	
					[1.07]***	
State Capture	4.17	4.38	-16.18	4.55	-16.16	4.50
	[1.94]**	[1.97]**	[15.38]	[1.95]**	[13.17]	[1.95]**
Obstacles in Financing	-2.48	-2.17	-14.05	-1.95	-5.97	-2.03
	[1.37]*	[1.10]*	[10.04]	[1.08]*	[8.66]	[1.09]*
Firms' Age	-0.43	-0.32		-0.38		-0.37
	[0.05]***	[0.06]***		[0.05]***		[0.05]***
Ln(Size)	7.36	6.67		7.28		7.21
	[0.89]***	[0.84]***		[0.85]***		[0.86]***
State	-12.3	-11.09		-12.15		-11.42
	[3.84]***	[3.53]***		[3.83]***		[3.93]***
New Private	7.23	9.79		8.22		9.02
	[3.17]**	[3.12]***		[2.85]***		[2.87]***
Lack of Market Power	-5.28	-4.37		-5.23		-5.08
	[1.42]***	[1.16]***		[1.39]***		[1.38]***
Big City	37.75	25.94		34.15		35.17
	[5.49]***	[6.09]***		[6.50]***		[6.82]***
Medium City	-7.92	-8.21		-9.39		-9.15
	[7.19]	[5.81]		[5.76]		[5.83]
Small City	-4.96	-5.91		-5.27		-6.94
	[3.44]	[3.32]*		[3.20]		[3.11]**
Town/ Village	-0.91	-0.78		0.12		-1.02
	[4.40]	[4.31]		[4.18]		[4.08]
Constant	6.65	-9.54		37.86		23.44
	[11.12]	[10.62]		[25.35]		[25.64]
Obs	3499	3499		3499		3499
R-squared	0.06	0.08		0.06		0.07

Robust and Robust Clustered standard errors are in brackets. Sector controls are included in all specifications. *Significant at 10%; ** significant at 5%; *** significant at 1%

Figure 1. Countries profiles based on estimates in column (4), Table 1.5



1.6 Robustness Tests

If the country-level estimates in column (4) of Table 1.5 are prone to an omitted variable bias, we can use relevant country-level variables to check whether the results in (4) are robust to inclusion of such variables.

1.6.1 GDP Growth Rates

Some groups of countries in the sample have experienced different timings for transition paths in outputs. Countries can be roughly divided into three regional groups: Central

Eastern Europe and the Baltic countries (CEB), South-Eastern Europe (SEE) and Commonwealth of Independent States (CIS). In terms of economic progress, CEB countries experienced the lowest and a shorter initial decline during 1989-1992 and the fastest recovery and growth by the end of 1990s. On the other hand, CIS countries suffered longer and more severe declines until the late 1990s. Only in recent years did they display stable and high growth rates exceeding those in CEB and SEE since 1999 (see EBRD 2004). As the survey was conducted in 2002, aggregate growth rates and reported firms' growth of sales rates as well as institutional scores can be correlated. So column (2) in Table 1.6 includes countries' GDP growth rates in 2000 to test the sensitivity of the results. An increase in countries' GDP growth rate of 1 percent is associated with a 2.07 percent growth in sales of firms. In comparison to column (1), signs of estimates do not change, country-level coefficients for *Regulatory Burden* and *Corruption* are now higher in absolute value, but the estimated effects of *State Capture* and *Financial Obstacles* are smaller in absolute values.

1.6.2 Infrastructure

From the survey, we also construct the index *Lack of Infrastructure*. BEEPS 2002 survey provides ratings of the quality and efficiency of public services related to infrastructure like roads, postal service, power, telephone and water. This index was constructed by calculating the equally weighted mean ratings of telecommunication, electricity, and transportation services at the individual firm level, which was then averaged over firms within countries. Both country level ratings and firm-level deviations from a given country's averages are used in the column (3) of Table 1.6. Using the firm-level deviations offers the additional advantage of controlling for respondents' "propensity to

complain". Since the objective ratings of these services should not differ much across firms within a specific country, actual differences in the ratings' deviations from country means can be explained in part by differences among managers in their tendency to complain about all aspects of the business environment. In column (3), *Lack of Infrastructure* at the country level has a strong negative effect on firms' performance. However, "propensity to complain" does not seem to be related to growth of sales.

1.6.3 Labor Regulations

Column (4) in Table 1.6 tests the robustness of the results with respect to obstacles in labor regulations. The index is given in the World Bank's Development Indicators database but, in fact, it is constructed from the BEEPS 2002 survey. The measure is calculated as a percentage of managers surveyed that rank labor regulation as a major business obstacle (Question 80k in questionnaire). The estimate in column (4) indicates that the more problematic are the labor regulations, the worse firms' sales are. Again the coefficients of *Regulatory Burden* and its square, "grease" coefficient and *Corruption* separate estimate have the same signs, similar magnitudes and statistical significance as in column (1).

1.6.4 Inflation

We found in the data that inflation is positively correlated with individual firm growth of sales (9 percent correlation). The correlation coefficient between countries' average growth of sales and inflation is 46 percent. First, the positive relationship may indicate that higher inflation is associated with higher economic activity and rising nominal

incomes of the population. Second, the positive relationship may also indicate that managers systematically overestimate the performance in real terms so that higher inflation is positively related to "overestimation", which is in turn positively related to average growth of sales numbers. The measurement error in the dependent variable should not bias the coefficients unless it is correlated with a measurement error in independent variables. Therefore, it is important to check the sensitivity of results when the inflation term is included. Column (5) reports the results if the inflation is included in the model. Inflation is positively and significantly related to growth of sales; a 10 percent increase in inflation is associated with an increase of 4.4 percent in sales. The signs of the coefficient of the linear and non-linear terms related to *Regulatory Burden* and *Corruption* are the same. The size of the squared term is smaller but the "grease" coefficient is still significant at a 5 percent level.

1.6.5 Political Freedom

Because economic and political freedoms in countries may be related to each other, it is important to check the robustness of the results when the measurement of political freedom is added. In addition, the survey responses may be correlated with political freedoms as managers may be reluctant to reveal their perceptions about state institutions in authoritarian countries, fearing possible negative consequences of criticizing the government. We draw measures of political freedom from Freedom House data. In particular, countries are categorized as free, partially free, and not free. In the regression, dummies for these categories are included ("not free" category is omitted), and results are reported in column (6). In addition, we use *Legal Regulations Obstacles, Inflation* and

GDP Growth as country controls. The latter two are especially important to control for because during 2000-2002 economic growth was on average higher in CIS region than CEB while the political freedom was worse in CIS.

Column (6) shows that results related to *Regulatory Burden* and *Corruption* are robust to the inclusion of all country controls. Firms grow much faster in politically free countries than not free countries.

In summary, the robustness tests show that some coefficients of country-level variables are very sensitive to the inclusion of relevant country-level controls. The concerns about omitted country level unobservables are important in our analysis. All firm-level estimates γ's are very stable in all specifications and very similar to fixed effects "within" estimates in Table 1.5, column (2). The robustness tests reinforce our major findings. The squared term of *Regulatory Burden* and its "grease" interaction with *Corruption* are most robust among the country-level coefficients. They always retain sign and are statistically significant in almost all specifications.

Courts' Quality and *Obstacles in Financing* country-level variables are not very robust in all specifications as signs and magnitudes of coefficients change dramatically across specifications. The effect country's *State Capture* is negative except in column (5) but in general it is imprecisely estimated because standard errors are quite large.

Table 1.6 Sensitivity tests for Firms' Growth of Sales Equation

Variables	Column (4) in Table 1.5		Adding country-level controls to check for robustness			
	(1)		(2)		(3)	
	Country Effects	Firm-level Effects	Country Effects	Firm-level Effects	Country Effects	Firm-level Effects
Courts' Quality	7.94 [14.94]	2.07 [1.15]*	7.7 [11.67]	2.17 [1.14]*	10.33 [16.32]	1.95 [1.22]
Regulatory Burden	7.57 [3.43]**	-0.23 [0.08]***	9.74 [4.05]**	-0.24 [0.08]***	3.48 [3.98]	-0.23 [0.08]***
(Regulatory Burden)^2	-0.78 [0.29]**		-1.08 [0.37]***		-0.55 [0.27]*	
Corruption	-16.88 [8.24]*	-0.70 [0.31]**	-26.46 [9.40]***	-0.68 [0.31]**	-19.93 [8.96]**	-0.73 [0.31]**
Reg. Burden X Corruption	3.04 [1.07]***		4.23 [1.18]***		3.66 [1.26]***	
State Capture	-16.16 [13.17]	4.50 [1.95]**	-11.41 [11.01]	4.51 [1.95]**	-9.48 [14.64]	4.64 [2.00]**
Obstacles in Financing	-5.97 [8.66]	-2.03 [1.09]*	1.98 [8.14]	-2.06 [1.08]*	-5.3 [8.56]	-2.11 [1.16]*
GDP Growth Rates in 2000			2.07 [1.20]*			
Lack of Infrastructure					-21.82 [10.98]*	-0.52 [2.93]
Observations	3499		3499		3450	
R-squared	0.07		0.07		0.07	

Robust-clustered standard errors in brackets, Sector controls and firm's specific control from Table 1.5 are included in all specifications. *Significant at 10%; ** significant at 5%; *** significant at 1%

Table 1.6 (cont.) Sensitivity tests for Firms' Growth of Sales Equation

	Adding country-level controls to check for robustness					
Variables	Labor Regulations Obstacles (4)		Inflation (5)		Political Freedom (6)	
	Country Effects	Firm-level Effects	Country Effects	Firm-level Effects	Country Effects	Firm-level Effects
Courts' Quality	13.22 [10.26]	2.01 [1.15]*	-1.65 [13.69]	2.03 [1.15]*	8.23 [7.92]	2.17 [1.15]*
Regulatory Burden	9.5 [3.43]**	-0.23 [0.08]***	2.44 [3.98]	-0.23 [0.08]***	9.52 [2.97]***	-0.24 [0.08]***
(Regulatory Burden)^2	-0.78 [0.32]**		-0.41 [0.35]		-0.84 [0.32]**	
Corruption	-17.31 [9.71]*	-0.73 [0.31]**	-15.5 [8.43]*	-0.71 [0.31]**	-14.86 [10.75]	-0.71 [0.31]**
Reg. Burden X Corruption	2.63 [1.23]**		2.53 [1.09]**		2.82 [1.21]**	
State Capture	-12 [12.43]	4.49 [1.95]**	1.72 [12.25]	4.5 [1.94]**	-11.87 [10.37]	4.5 [1.96]**
Obstacles In Financing	6.8 [6.71]	-2.15 [1.11]*	-14.62 [7.63]*	-2.09 [1.11]*	3.95 [8.27]	-2.2 [1.10]*
Labor Regulations Obstacles	-1.23 [0.24]***				-1.16 [0.29]***	
Inflation			0.44 [0.19]**		0.39 [0.15]**	
Partially Free Country					12.39 [8.28]	
Free Country					21.51 [6.16]***	
GDP Growth 2000					2.41 [0.69]***	
Observations	3499		3499		3499	
R-squared	0.07		0.07		0.08	

Robust-clustered standard errors in brackets, Sector controls and firm's specific control from Table 1.5 are included in all specifications. *Significant at 10%; ** significant at 5%; *** significant at 1%

1.6.6 Alternative Dependent Variable: Investment

The model should be checked against alternative measure of performance of firms. We use the investment variable. The relevant survey question asks managers about the percentage change in fixed assets such as land, buildings, machinery and equipment. Results are reported on Table 1.7. Among firm-level perception variables in column (1), only *Corruption* and *Obstacles in Financing* are significant at 5 percent level with expected signs. In column (2), country-level estimates display similar coefficients to estimates in models for growth of sales. Note that non-linear terms are highly significant. Thus, the use of the alternative measure of firms' performance leads to very similar predictions lending more credibility to the empirical model and results.

Table 1.7 Alternative dependent variable: Investment

	Firm-level effects and country FE's	Non-Linear Model in Regulatory Burden and Corruption	
Variables	(1)	(2)	
	γ's	Country Effects β's	Firms Perceptions γ's
Courts' Quality	0.08 [1.16]	8.22 [6.93]	0.16 [1.15]
Regulatory Burden	0.03 [0.05]	4.61 [2.47]*	0.03 [0.05]
(Regulatory Burden) ^ 2		-0.62 [0.22]***	
Corruption	-0.49 [0.19]**	-19.58 [5.36]***	-0.47 [0.19]**
Reg. Burden X Corruption		2.9 [0.61]***	
State Capture	2.02 [1.33]	8.24 [8.72]	1.98 [1.34]
Obstacles in Financing	-1.98 [0.76]**	-1.39 [5.29]	-1.94 [0.76]**
Inflation		0.17 [0.10]*	
Partially Free Country		6.65 [5.51]	
Free Country		8.58 [5.08]*	
GDP Growth 2000		2.11 [0.62]***	
Firms' Age	-0.15 [0.05]**		-0.17 [0.05]***
Ln(Size)	1.83 [0.48]***		1.94 [0.49]***
State	-6.65 [2.52]**		-6.13 [2.57]**
New Private	7.77 [2.71]***		7.23 [2.53]***
Lack of Market Power	-1.09 [0.73]		-0.96 [0.78]
Constant	4.71 [10.24]	-17.84 [23.05]	
Obs	3383	3383	
R-squared	0.06	0.05	

Robust-clustered standard errors in brackets, Sector controls are included in all specifications
*Significant at 10%; ** significant at 5%; *** significant at 1%

1.7 Concluding Remarks

Decomposition of institutional survey measures for each firm into the average for their country and a residual firm-specific component used in this study is the nested version of the country fixed effects model. This procedure allows us to differentiate between cross-country and firm-specific effects, and these effects are found to be strikingly different. The most significant findings of this chapter are the nonlinear effects of country-level corruption, regulatory burden on growth of sales for firms. The regulatory burden variable displays a Laffer-type relationship with respect to the performance of firms. The separate effect of corruption is negative. However, at high levels of regulatory burden, higher corruption in the country helps to improve the performance of firms. This implies that excessive liberalization of regulations should be treated with caution. However, excessively high levels of regulatory burden may encourage corruption in the country. We also find some indication in the data that state capture helps individual firms for a given country, yet such corruption has greater negative effects at the national level. Previous studies were able to identify firm-level positive effects, while country-level negative impacts were only suggested on a hypothetical basis.

The major issue with our approach is that average country indices may be correlated with unobserved country variables that are important for the economic outcomes of firms. Our sensitivity tests with respect to measures of GDP growth, infrastructure, labor regulations, inflation and political freedom show that the coefficients of non-linear terms are quite robust to inclusion of the country level variables.

The broad range of questions in the BEEPS survey is very valuable, yet desired concepts are difficult to measure so that the questions in the questionnaire may not accurately reflect the information to meet objectives of such research. In this study, choosing questions for the institutional measures and corruption has been done in a somewhat ad hoc manner, and, perhaps, more analysis is required when assessing the potential subjectivity and minimizing the measurement errors. For this purpose, the two BEEPS data can be analyzed together to quantify specific sources of measurement errors and indicate where improvements should be sought for future surveys.

Chapter 2: How economic shocks affect the incidence of politically destabilizing events: an instrumental variable approach

2.1 Introduction

One of the obvious impressions drawn from news headlines is that socio-political instability is sparked by worsening economic conditions. Indeed, the world has seen many episodes in which a country's economic turmoil was accompanied by radical political changes. For instance, the Asian crisis of 1997-1998 in Thailand, Korea, Indonesia and Malaysia was followed by political unrest and discontent in these countries. In Thailand and Korea, democratic elections were held and the opposition came to power. In Indonesia, Suharto's long dictatorship ended when riots and demonstrations finally forced him to resign. Malaysia has also faced a significant rise in political activism. South America provides another recent example of this phenomenon: Argentina's leaders resigned under increasing political pressure following the rapid deterioration of economic conditions during the crisis in 2001-2002.

However, there are also many episodes when destabilization has occurred during economically good times. Following the painful transition in the 1990s, most of the former Soviet Union countries during the early years of the new century were enjoying high growth rates. Yet in some countries serious political events took place when living standards seem to be improving at a fast pace. Ukraine's "Orange Revolution" was a series of mass protests that took place throughout the country in response to allegations of massive corruption, voter intimidation, and elections fraud during the end of the 2004

Ukrainian presidential election. Notably, growth in Ukraine was a strong 9.3 percent (10.22 percent in per capita terms) in 2003 and a remarkable 12 percent (13 percent per capita) in 2004. One year earlier, the end of 2003, Georgia's "unfair" parliamentary elections also sparked massive anti-governmental demonstrations which led to the so called "Rose Revolution" and a change of presidential power with new elections in January 2004. Interestingly, growth rates in 2002 and 2003 were 5.5 percent (6.63 percent per capita) and 11.1 percent (12.3 percent per capita) respectively. In Kazakhstan during the period of booming resource based economy, 2005 and 2006 seem to see the rise not only of the peaceful political activism of the opposition but also of several assassinations of key political figures which rumors attributed to the political struggle among the elites. These examples show that political mobilization against existing regimes starts during the time of high contemporaneous growth rates.

Studying the relationship between economic conditions and political instability has attracted the attention of many researchers. This study is an empirical contribution to understanding the related question: to what extent can the incidence of politically destabilizing events be explained as a result of economic shocks?

The importance of the question is related to the determination of the political stability that is commonly seen as one of the major determinants of economic growth (Gupta 1990, Barro 1991, Roubini 1991, Fosu 1992). In general, political stability is perceived to be a longer term phenomenon that does not vary much in the short run. In a secure political environment, one observes very few destabilizing political events, such as peaceful and violent protests of the masses, revolutions, political assassinations, military

coups, and civil wars. Researchers would generally characterize such a state as a long run equilibrium. However, as with any equilibrium, political equilibrium may be affected by shocks in the short run. In particular, economic shocks may disturb the equilibrium resulting in short term disequilibrium or instability, which can be manifested in the above mentioned destabilizing events. However, short term instability or uncertainty in turn affects economic growth making it harder to use statistical analysis to predict to what extent and in which direction economic shocks are affecting political instability. This is a standard joint endogeneity or two-way causation problem in the econometric analysis.

In my opinion, this problem of endogeneity has not been satisfactorily addressed in existing empirical literature studying the relationship between economic shocks and political instability variables. As a result, there is no agreement in economic and political science literature as to whether economic conditions matter for political destabilization[17]. We believe that such disagreement mainly comes from weaknesses in the methodologies used, from different definitions applied to instability, and differences across specifications. Methodological weaknesses are due to the failure to address the endogeneity between these two variables: studies either do not account for endogeneity at all or use inadequate instruments in the form of lagged endogenous variables, which is inappropriate if variables under consideration are autocorrelated (Green 2000, Mauro 1995, Brunetti 1997, Miguel et al 2004, Angrist and Krueger 2001). In addition, if the causal link between economic conditions and instability is found in some studies, then lower economic growth rates would increase the incidence of destabilizing events (coups in Lodgregan and Poole 1990, civil conflicts in Miguel et al. 2004). In contrast, the data

[17] Examples include Lodgregan and Poole (1990), Alesina et al. (1996), Gasiorowski (1998). The next section discusses the evidence in more detail.

and analysis in this study shows that the political unrest is more likely to happen during higher contemporaneous income per capita growth rates.

We use the instrumental variable approach to study the effects of income changes on the probability of politically destabilizing events. The identification strategy in this study allows for focusing on short-term economic fluctuations that "trigger" political events, but it does not intend to explain the duration in political cycles. We use exogenous variation in world primary commodity prices as instrumental variables for contemporaneous GDP growth. It is argued that fluctuations in the world primary commodity prices play an important role for economic fluctuations around the world. In particular, oil price is an arguably plausible instrument for GDP growth in economies that largely rely on oil imports. The approach is similar to the strategy in Miguel et al. (2004) who use changes in rainfall as an instrument for income growth variation in Sub-Saharan Africa to study the incidence of civil wars. This study covers a greater number of countries and a longer time period. We also use the larger set of political variables that allows investigating whether economic shocks affect in a different way various political events.

In the next section, we will review the existing literature that is most relevant to the question studied. In section 2.3, there is a discussion about the empirical strategies of how we are going to deal with the endogeneity problem. Section 2.4 describes the data sources and variables. Section 2.5 explains the choice of the instrument while section 2.6 presents the results and robustness checks. The comparison to Miguel et al. (2004) study is made in section 2.7 followed by concluding remarks in section 2.8.

2.2 Some Theoretical Background and Empirical Evidence

This section examines the main theoretical views and reviews relevant empirical studies about the relationship between economic growth and political instability. We have found several alternative explanations and disagreements both in theory and empirical evidence.

Theoretical considerations are mainly dominated by the literature that provides arguments showing that a lower economic growth can increase the probability of a change in government, political unrest, and violence (Brunetti 1997). Rapid and sustained growth increases the popularity of the government, thus reducing the probability that the existing government is thrown out by regular or irregular transfers. Lower growth has similar destabilizing effects on all types of government changes, perhaps with different intensities. The existing government is either punished or rewarded according to its economic performance. For instance, Drazen (2000) summarizes that several studies find that pre-election economic conditions are directly related to election outcomes and the popularity of incumbent politicians in the US and other OECD countries with established democracies. Brender and Drazen (2006) find that higher growth rates raise the probability of reelection in less developed countries and in new democracies. Gasiorowski (1998) sums up the idea in the literature that adverse economic conditions generate widespread frustration, increasing support for opposition movements.

An alternative view is that instability may occur during good times in the economy. This view is rarely referenced in the literature. The idea of "revolutions of rising expectations" initially attributed to Alexis de Tocqueville suggests that anti-government

protests and unrest occur when people begin to expect more, owing to improvements in their lives. Olson (1963) also supported the idea that economic growth stimulates "rising expectations" about what the government should do and it "can awaken people to the possibilities of further improvement and thereby generate additional discontent" (p. 542). Olson goes further and considers the sociological roots of protests concluding that "a rapid economic growth is a major force leading toward revolution and instability." Olson argues that people, who participate in revolutionary mass movements, tend to lack close attachments to social subgroups ("extended families", professional groups, or social classes). To Olson, "it is not those who are accustomed to poverty, but those whose place in the society is changing, who resort to revolution (p.531)." During rapid economic growth, there are vast changes in the methods of production, importance of industries, distribution of income, so that some people gain a great deal and others lose a great deal.[18] Both gainers and losers from rapid growth can be destabilizing forces. Gainers can use their increased economic power to change political order in their interest while losers are resentful of their newly decreased position in society.

Huntington (1968) is concerned that, as a result of economic development, political mobilization will increase faster than the appropriate institutions can arise, thus leading to instability. Huntington suggested that social instability was due precisely because growth and industrialization put pressure on existing institutions. Conflict was a result of the inability of institutions to adjust, which required strong political institutions to promote stability.

[18] Many peoples' previous ties with class and caste are weakened because their income brackets are changed and this may keep them from belonging to the group (class) in which they were born. Rapid industrialization and modern business institutions may also break traditional social family groupings (tribes, guilds, rural villages) that existed before rapid economic changes took place.

Overall, the empirical evidence disagrees as to whether changes in economic growth are important at all for political instability. If the relationship is found significant, studys suggest that lower growth increases instability. However, Campus and Nugent (2002) find that if the measure of institutional development is controlled for, the effect of growth on their measure of socio-political instability becomes positive in Latin America. They do not pay much attention to this result leaving investigation for "future work".

The empirical literature can be broadly divided into studys looking at the long run relationship between growth and political instability and articles that estimate the short run dynamics of the annual frequency data. The "short run" studys have more relevance to our research agenda of identifying the effects of economic conditions on political changes, and; therefore, we will focus more on these studies below.

The majority of studys addressing joint endogeneity between growth and political instability use the instrumental variable approach in estimating the relationship. Typically, authors employ earlier values of the endogenous variables (instability measures and growth rates) as instruments, but the validity of such instruments is questionable if the variables under consideration are autocorrelated.[19] More specifically, Miguel et al. (2004) argue in the case of civil wars that using lagged GDP growth values implicitly assumes that economic actors in previous periods did not anticipate the instability and that they did not adjust economic activity accordingly, which is a very

[19] Green (2000, p.689, 701) notes that using lagged endogenous instruments results in misspecification if disturbances are autocorrelated.

strong assumption, and lagging economic variables may not be a convincing solution to the endogeneity problem.

Cukierman et al. (1992) recognize that political instability is affected by economy in the short run and estimate the equation for probability of changes in government as a function of inflation and consumption's share of GDP using annual panel data in 79 countries. However, they do not account for feedback effects from changes in government on the economy in the short run. They find weak evidence that higher inflation and lower growth of private consumption make government changes more likely to happen.

Lodgregan and Poole (1990) also argue that if an economic crisis occurs or economic growth turns into stagnation or decline, then a change in executive is more likely to emerge. Their major objective is to study the short run relationship between economic performance and political instability in the form of coups d'etat – the irregular transfer of executive power. They first construct the autoregressive probit model of coups with economic performance and regional variables and find negative insignificant effect of last period growth on the incidence of coups in the data with annual frequency. The authors further address the issue of joint endogeneity by estimating a system of two equations. Similarly, Alesina et al. (1992, 1996) use a simultaneous equations framework with a somewhat different model specification including broader definition of government changes and updated determinants of growth. Their study defines political instability as "the propensity of a change in the executive either by constitutional and unconstitutional means". Acknowledging endogeneity in instability and economic performance, both

studys suggest simultaneous equation method at annual frequency in the following form (from Alesina et al. 1996):

$$c^* = \alpha_c X_c + \beta_c X + \gamma_c y + u_1 \quad (2.1)$$

$$y = \alpha_y X_y + \beta_y X + \gamma_y c^* + u_2 \quad (2.2)$$

where c^* is an indicator of a government change, y is an annual rate of income growth, X are exogenous variables that determine both c^* and y, X_c are instruments for instability, and X_y are instruments for growth. The identification of this system requires that at least one of each of the X_c and X_y variables exist so that the exogenous variable(s) in the growth equation does not belong to the equation for instability and vice-versa. Both Londregan and Poole (1990) and Alesina et al. (1992) use earlier values of variables as instruments, $X_y = y_{t-1}$ and $X_c = c^*_{t-1}$. However, it is questionable that y_{t-1} does not belong to (2.1) and c^*_{t-1} is not in (2.2). In other words, there is no reason to believe that current occurrence of the change in executive is independent of past economic performance, and even more questionable is the assumption that current economic performance is not affected by past instabilities. In addition, Alesina et al. (1992, 1996) use education as an instrument for growth and the occurrence of an executive adjustment in previous period as an instrument for the government change. These two instruments may also not be valid as education is likely to be correlated with changes in executives and lagged executive adjustment may be the part of lagged government change that is already being used as an instrument. Alesina et al. (1992) found that economic performance does not significantly influence changes in government. In contrast, Lodgregan and Poole (1990) found that lower growth increases the incidence of the coups. Thus, these two studies, while using a very similar methodology

and datasets, reach different conclusions about whether the variation in economic growth rates matters in determining the likelihood of government changes. Alesina et al. (1996) claim that such disagreement comes from differences in defining government changes and growth equation specifications. However, we think, in addition to these differences, both studies fail to reach agreement due to the use of bad instruments.

Some of the more recent attempts in the literature employ Granger causality tests to investigate relationships between instability and growth. However, Granger tests may be uninformative if these two variables are affected by a third, unless the latter is controlled for. In cross-country data the possibility of omitted variables is very high; hence the usefulness of the Granger test may be limited. Gasiorowski (1998) uses Granger causality tests in annual frequency data and concludes that the incidence of unstable events has more of an effect on economic growth than the latter do on the former. Campos and Nugent (2002) constructed two indexes of socio-political instability (SPI) for non-overlapping 5-year periods, between 1960 and 1995, for 98 developing countries. In general, they found no evidence of the causal long run relationship between political instability and economic growth in either direction. In addition, accounting for institutional development measured by legislative effectiveness makes growth effect on SPI in Latin America positive. This positive effect of growth on instability is consistent with theories of Olson (1963) and Huntington (1968) but authors do not make a connection to such interpretation and leave explanations for future work.

One study deserves more careful attention since it uses an interesting choice of instrument for economic shocks that distinguishes this study from others reviewed here.

Miguel et al. (2004) studied the effects of economic shocks on the likelihood of civil conflict. The sample included 41 African countries during 1981-99. They use exogenous variation in rainfall as an instrument for income growth in order to estimate the impact of economic growth on the civil conflict. Authors argue that these weather shocks make a good case for the instrument for GDP growth in countries that largely rely on rain fed agriculture, such as sub-Saharan Africa, but this identification strategy is inappropriate for other regions in the world. Also their dependent variable is the incidence of civil wars, which is the most horrible type of instability, and their study does not examine how economic shocks affect other types of the political events, such as mass protests, demonstrations, and riots against existing governments and regimes. They find a strong negative causal effect of GDP growth on the incidence of civil wars. In our study, we use the similar approach of using instruments for economic fluctuations, but our choice of the instrument allows studying a larger set of countries, and we also examine a more broad set of political unstable events.

In sum, we find that the alternative theories suggest that there may be positive and negative effects of growth on politically unstable events in the short run. Furthermore, the empirical literature is not able to reach an agreement as to whether economic shocks are important for the incidence of politically unstable events. We believe that a major weakness of the existing literature is a failure to address the endogeneity between these two variables: studies either do not account for endogeneity at all or use bad instruments in the form of lagged endogenous variables. The use of problematic instruments causes the instrumental variable approach to fail to serve its purpose in correcting biases coming from the joint endogeneity. The relevant empirical evidence is summarized in Table 2.1.

If one believes that political uncertainty negatively effects growth as it is consistently documented in the literature[20] then the standard OLS or Probit estimation of the effects of economic growth changes on instability will result in downward biased estimates. Because IV method with problematic instruments results in inconsistent estimator, the estimates are certainly not reliable. Miguel et al. (2004) seems to use a valid instrument but the sample is limited only to a subset of African countries. Also civil wars as a measure for political conflict may not capture variation in other more moderate types of politically unstable events such as mass protests, coups and revolutions. Thus, we are convinced that further work is needed covering other regions of the world and using alternative sets of dependent variables.

[20] see the summary of the literature in Persson and Tabellini (1999).

Table 2.1 Existing studies about effects of economic conditions on political instability variables.

Study	Data and Political Instability Measure	Countries and period	Specification and Method	Result
Cukierman et al. (1992)	Taylor&Jodice (1983) Government changes both regular and irregular (coups)	79 countries, 1948-1982 annual	Probit Gov Change = f(consumption growth, inflation, riots, repressions, democracy)	Negative effects of consumption growth on probability of government change
Lodgregan and Poole (1990)	Taylor&Jodice (1983) Coups	119 countries 1970-1982 annual	Probit Coups = f(change in GDP per capita, past coups, region) Granger causality, Simultaneous equations Instruments: lagged growth	Income changes negatively affect probability of coups
Alesina et al. (1992, 1996)	Taylor&Jodice (1983) Government changes both regular and irregular (coups)	113 countries 1950-1982 annual	Probit Gov Change=f(change in GDP per capita Simultaneous equations Instruments: lagged growth and education	Growth does not significantly influence probability of government change (1992) Growth negatively affect incidence of coups (1996)
Campos and Nugent (2002)	Principal component construction of instability variables: Barro&Lee (1993) for "severe SPI" index and Polity III (1996) for "less severe" SPI[21]	98 developing countries, 1960–1995 5 year averages	Granger-causality framework between SPI index and growth rates, legislative effectiveness, regions as controls Instruments: the twice-lagged levels of growth	No evidence of negative and causal long run relationship between political instability and economic growth in either direction. Latin America region displays positive effect of growth on SPI if institutions are controlled for.
Gasiorowski (1998)	Banks (1996) Peaceful Unrest, Violent Unrest, Coups, Changes in Government	110 countries, 1960-1992 annual	Granger-causality tests between instability measures and inflation & growth variables	Economic conditions do not have an effect on political conditions
Miguel et al. (2004)	Armed Conflict Data, PRIO/Uppsala;	41 African countries 1981-1999 annual	Civil Conflict = f(growth, ethno-linguistic and religious fractionalization, democracy, population, terms of trade growth) IV-2SLS Instrument: rainfall	Growth shocks strongly and negatively affect civil conflict incidence

[21] SPI – Socio-Political Instability

2.3 Estimation Strategy

In this study, the major focus is placed on finding whether short-term economic fluctuations trigger political events that may disrupt overall stability. These events are discrete and may signal the imminence of a political crisis. The dummy variable, which takes the value of one if the event is observed, should measure the underlying propensity of the occurrence of such an event. The estimated equations follow models typically used in the reviewed literature (Alesina et al. 1996, Lodgregan and Poole 1990, Gasiorowski 1998, Miguel et al. 2004). It describes the probability of the occurrence of destabilizing political events as a function of economic and political variables:

$$Y_{it} = \alpha + \beta X_{it} + C\gamma + \varepsilon_{it} \qquad (2.3)$$

where i = country index, t = time index

Y_{it} = incidence of political destabilizing event (1 if destabilizing event was observed, 0 if otherwise)

X_{it} = measure of economic shocks (growth of GDP per capita)

C = is vector of controls which are important for political environment

The equation (2.3) may not be correctly estimated if X_{it} is also affected by Y_{it}, and $\hat{\beta}_{OLS}$ or $\hat{\beta}_{PROBIT}$ are biased downwards if the short-term political instability negatively affects growth. The identification strategy is to find the instrument Z_{it} that affects the incidence of instability only through changes in economic growth. If such an instrument is found, then equation (2.3) can be estimated by the instrumental variable method, where the first stage is:

$$X_{it} = a + bZ_{it} + C_{it}c + e_{it} \qquad (2.4)$$

In this study we use exogenous variation in oil prices as an instrumental variable for economic shocks. Oil shocks arguably have significant impacts on GDP growth in all countries. While oil exporters clearly benefit from the direct impact of higher oil prices, lower domestic demand and substantial second round effects cause the overall activity to decline in oil importing countries (see IMF 2000 for a detailed analysis). Various economic channels include the impacts of oil prices on the cost of production of goods and services, domestic aggregate demand, inflation, corporate earnings, etc. As the oil intensity of production in advanced countries has fallen over the past three decades, the supply side impact for a given increase in oil prices can be expected to have been less in recent years than in the past episodes of oil shocks taking place in the 1970's and 1980's followed by the global economic slumps (IMF 2000). Overall, the empirical evidence is focused on OECD countries and it shows that oil shocks have been important for economic activity in recent years as well (Mork and Olson 1994, Lee and Ratti 1995, Hamilton 1996, Ferderer 1996, Davis and Haltwinger 2001).

If the effects of oil price fluctuations on economic conditions do exist, the remaining question is whether oil shocks are plausible instruments in our setting. The key issue in justifying the validity of oil price fluctuations being an instrument is that oil shocks should affect political environment only through economic channels. However, political variables in oil exporting countries are potentially linked to changes in oil prices through other channels. Political science literature points to several possible effects of oil wealth on political stability and the democratization process in oil rich countries (Smith 2004, Ross 2001). The government can use oil wealth on patronage spending to reduce social pressures for political changes. The repression of opposition forces may occur since the

government has extra resources from oil wealth to spend on military forces supporting the existing powers. Finally, the mere existence of oil wealth, accessible through possession of the executive power, creates additional incentives to seek oil rents in the society so that political tension arises between incumbent and other potentially powerful forces in the country. So, the argument that oil prices affect political instability only through economic channels is much weaker in oil exporting countries. Therefore, our identification strategy may be inappropriate for oil exporting countries.

In contrast, oil importers do not depend on oil wealth eliminating those potential effects of oil prices on politics. We think that world oil prices are truly exogenous to a small oil importing country in the sense that its economy and internal, as well as external, politics do not affect world markets for oil. Furthermore, we do not see possibilities where changes in oil prices might directly affect political instability independently of changes in measures of income in an oil importing country. For instance, one could possibly argue that higher oil prices are associated with higher gasoline prices and this may add more anger and frustration to the public; however, we think that this kind of discontent is not independent of income changes since dissatisfaction arises from the perception of people that higher gas prices result in a decrease in real incomes. Therefore, such unhappiness is effectively linked to changes in real incomes.

In sum, the identification strategy leaves us with a sample consisting of countries that are oil importers and not big enough to affect world oil markets. On this ground we exclude from the sample: major industrialized countries -- US, Germany and Japan, oil

producers[22], developing China and India, and countries from the former communist block[23].

2.4 Variables and Data

Sanders (1981) proposes a breakdown of political changes into four indicator events that destabilize political stability: regime changes (changes in party system, in military-civilian status), government changes (changes in chief executive, in cabinet), violent events (guerrilla attacks, riots, deaths from political violence, attempted coups), peaceful events (strikes, demonstrations). Such categorization in our view will help to identify whether economic changes may affect differently the occurrence of various political events. Gasiorowski (1998) uses a very similar classification but narrows it to the available data which is Banks' Cross-National Time-Series Data Archive in 1996 (Banks-CNTS). This study utilizes updated Banks-CNTS (2000) data for political events, so we will follow Gasiorowski's classification: unrest, violent unrest, government changes and political assassinations. More specifically, the first category is a measure of the extent of unrest, which is a combination of demonstrations, general strikes, and riots. The second is a measure of violent unrest, which brings together guerrilla warfare and revolutionary activity. The third category includes changes in government such as successful coups d'etat, changes in effective executive and cabinets. The data also provides the measure of government crises which excludes revolutions but covers the occurrence of any other

[22] We exclude oil exporters on the basis of oil production. The sample contains countries that do not produce oil at all or produce very little relative to oil consumption and GDP.
[23] The former Soviet Union countries' sample is limited only to post-1990 period. Prior to collapse, the politics and economy were related in a very special way among countries in the socialist block, and oil prices were heavily subsidized by the Soviet Union in intra-group trade. However, one can argue that the collapse of the system occurred mainly due to poor economic performance.

"rapidly developing situations that threaten to bring the downfall of the present regime" (Appendix 2.1). In fact, it is not clear exactly from the data which situations are considered so we do not assign a specific category to this measure. We also separately consider the incidence of political assassinations. Appendix 2.1 contains definitions of the political event variables in Banks-CNTS dataset.[24] The time period is chosen from 1960 to 1999 due to data availability and also because many developing countries gained independence during the 1950s.

The most widely used economic data are Penn's World Tables (Heston et al. 2002), where data is adjusted for differences in purchasing power parity. We obtain the real GDP per capita figures from this data. Oil prices are given in BP Statistical Review of World Energy June 2004.

The exclusion of big countries and oil producers leaves the sample consisting of 67 countries during the period 1960-1999. The panel is unbalanced as some observations are missing. Summary statistics are given in Table 2.2.

[24] Although the original data provides the frequency of events, we convert these frequencies into dummy variables to evaluate the probability of the occurrence of events.

Table 2.2 Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Real GDP Growth per capita	2600	0.012	0.068	-0.72	0.437
Real GDP per capita	2669	4640	5533	281	41383
Δ + Ln(Poil)	2680	0.087	0.214	0	1.154
Δ - Ln(Poil)	2680	-0.076	0.126	-0.67	0
$[\Delta + \text{Ln (Poil)}]^2$	2680	0.053	0.218	0	1.33
Political Events Dummies:					
Unrest	2468	0.276	0.447	0	1
General Strikes	2468	0.084	0.277	0	1
AntiGov. Demonstrations	2469	0.192	0.394	0	1
Riots	2468	0.165	0.371	0	1
Violent Unrest	2465	0.249	0.427	0	1
Guerrilla Warfare	2465	0.153	0.360	0	1
Revolutions	2468	0.165	0.371	0	1
Government Changes					
Successful Coups	2503	0.040	0.196	0	1
Change in Executive	2503	0.161	0.367	0	1
Cabinet Changes	2490	0.416	0.493	0	1
Other:					
Government Crises	2468	0.12	0.325	0	1
Assassinations	2468	0.102	0.303	0	1
Democracy	2347	0.377	0.485	0	1
Anocracy	2347	0.233	0.423	0	1
Autocracy	2347	0.391	0.488	0	1
Ethnic Fractionalization	2680	0.470	0.266	0	0.93
Military Regime	2503	0.161	0.368	0	1
Urbanization, % of population	2680	0.371	0.238	0.022	0.97
Legislative Effectiveness	2503	1.675	1.041	0	3
Party Coalitions	2498	1.567	1.291	0	3
Rival Party Legitimacy	2498	0.495	0.500	0	1
Log of Real GDP per capita, MA(5)	2612	7.846	1.025	5.73	10.49
Purges, MA(2)	2403	0.116	0.415	0	5

2.4.1 Controls

Miguel et al. (2004) studying the incidence of civil conflicts use controls for a country's political and social characteristics which are drawn from Fearon and Laitin (2003). Both studies include measures of ethnic or religious diversity on grounds that these measures may be associated with political instability. Miguel et al. (2004) find that higher ethnic fractionalization increases the probability of conflict but their coefficient is not statistically significant. We use measures of ethnic fractionalization from Alesina et al.(2003)[25]. The index reflects the probability that two randomly selected individuals from a population belonged to different ethnic groups.

Democracy measures are drawn from Polity IV Project (2002). The original polity scores range is from -10 (fully autocratic) to 10 (fully democratic). It is very common in empirical literature to use the index scores as a continuous variable in regressions. However, political processes are likely to be different in autocratic countries and democratic countries. Therefore, a democratic change in index from -5 to -4 is likely to have different implications on political instability as compared to the impact of a democratic change from 4 to 5. In our view, it is more appropriate to use Polity IV scores to categorize regimes into democratic, autocratic, and "in-between" regimes dummies like in Gurr and Marshall (2003). "Anocracies" are regimes where countries are "in-between" (Polity scores range from -5 to +5) and have a mix of democratic and autocratic features.

[25] Data is posted by Wacziarg at http://www.stanford.edu/~wacziarg/

Auvinen (1997) also implements controls for urbanization because political demands are typically higher in urban areas and political mobilization is easier simply because of higher population concentration; therefore, highly urbanized countries are likely to experience more protest than less urbanized countries.

Banks-CNTS data provides other variables that characterize political institutions. We use these variables in the way that other scholars studying political instability have done. Campos and Nugent (2002) argue and show that institutional development should be negatively correlated with the level of socio-political instability (SPI). They use the index of legislative effectiveness from Banks-CNTS dataset (1984) as a measure of institutional development.

Feng (1997) estimates logit equation where the dependent variable is the probability of government change as a function of economic and political variables. The latter set includes variables from Banks-CNTS that describe political systems of countries in terms of regime type (civilian or military), election processes (rival party legitimacy, party coalitions). These variables are described in Appendix 2.1.

Political institution variables (measures of democracy, regime type and nature of elections) are possibly endogenous as they can be affected by the political events under consideration. For instance, in a politically unstable environment, ruling authorities may respond to political destabilization by initiating political reforms that would alter the political process in the country toward a more democratic or authoritarian government so

that the democracy measure would change. To reduce such a possibility of endogeneity, we use lagged values of political variables that characterize political institutions.[26]

2.5 Oil Shocks and GDP Growth in the First Stage: Choosing Instruments

The effects of oil price shocks on growth and economic activity have been empirically documented relatively well for the US economy and selected OECD countries.

Early empirical studies generally regressed GDP on oil prices and found that oil shocks negatively affect economic activity and were an important cause of recessions in the 1970s and early 1980s (Hamilton 1983, Burbidge and Harrison 1984, Gisser and Goodwin 1986).

Since the early 1980s, however, oil prices began to fall, which historically had rarely happened and the estimated relationship between growth and oil prices became less significant. Mork (1989) suggested testing for asymmetric effects of oil price increases and decreases. The motivation is that any oil price movement creates costly reallocation of resources but in the case of oil price increase such costly reallocation work in the same direction with harmful supply-side effects of oil price increases on growth. However, frictions associated with negative changes in oil prices make the benefits of an oil price

[26] However, this approach does not guarantee that the issue of endogeneity is resolved because political discontent may still be present in the previous period, but did not materialize in the form of actual destabilization events, so that present events may be correlated with past changes in political variables that describe a political system. Therefore, the coefficients of lagged political variables may be biased and should be treated with caution

decline smaller. Mork's study found that the real effects of oil price increases are different from oil price decreases, and the latter do not have a statistically significant impact on US economic activity. These issues were further explored by other authors from various angles, and they found that oil prices have asymmetric effects on output, inflation, employment, and wages (Mork and Olsen 1994, Lee and Ratti 1995, Hamilton 1996, Ferderer 1996, Davis and Haltwinger 2001).

Here, we use three different forms for oil price shocks that are typically used in the literature and can be potentially applied as instruments in IV method. Table 2.3 presents the first-stage relationship between oil shocks and income growth. All columns include country fixed effects, which permits one to account for unobservable country-specific time invariant characteristics that are important for growth and reducing the possibility of an omitted variable bias. Furthermore, we use country specific time trends along with country fixed effects to capture additional time variation. Finally, political controls are included because these are used in the main equation of IV estimation.

It is found in the data that lagged percentage changes in oil prices have biggest and most precise estimated negative effects on GDP growth rates. In column (1), the oil shock variable is a simple percentage change in oil prices or $\Delta Ln(Poil)_{t-1} = Ln(Poil)_{t-1} - Ln(Poil)_{t-2}$. The coefficient is negative and significant at the ten-percent level. F test of excluded instruments is rather low, 2.87 (p-value=0.09), suggesting that the instrument is weak.

Column (2) in Table 2.3 distinguishes between positive and negative changes, as in Mork (1989) , which are defined as $\Delta^{+} Ln(Poil)_{t-1}$ for positive values of $\Delta Ln(Poil)_{t-1}$,

and $\Delta^{-} Ln(Poil)_{t-1}$ for negative values of $\Delta Ln(Poil)_{t-1}$.[27] Note that both positive and negative oil price growth rates have different signs, and the effect of positive shocks is about two times greater in magnitude than in column (1). F statistics shows a higher joint significance of instruments but is still relatively low.

In addition, column (3) adds a dummy for the positive changes in oil prices allowing different intercepts for our two types of shocks. Omitting this dummy would "force" regression lines for positive and negative oil shocks to have a common intercept, which will in turn, bias both slope coefficients if the intercepts are different. The coefficient on negative percentage changes in oil prices is close to zero, suggesting that the harmful effects of oil price declines due to frictions arising from the economy's adjustment are about the same as benefits due to lower oil prices.[28] The dummy for the positive oil shock appears positive and significant. The slope coefficient for positive percentage changes in (3) is now larger in absolute value than in (2). Consequently, the F statistics show slightly better correlation of excluded instruments with the endogenous income growth. Looking forward to IV-2SLS results, one also needs to consider the overidenticiaftion test in the IV model. However, the reported overidentification test is very close to rejection of the joint null hypothesis of instruments validity at the ten-percent level.

[27] Zeros are absent in the data.
[28] This is similar to conclusions reached in Mork and Olsen (1994) and Jimenez-Rodrıguez and Sanchez (2005) who find, for some OECD countries that oil price increases have a greater impact on real GDP growth than that of oil price declines, with the latter being insignificant in most cases.

Table 2.3 Choosing Instruments for IV method: First Stage Regressions

	(1)	(2)	(3)	(4)
Δ *Ln(P oil)*$_{t-1}$	-0.008 [0.005]*			
Δ^{+} *Ln(P oil)*$_{t-1}$		-0.018 [0.007]***	-0.026 [0.008]***	
Δ^{-} *Ln(P oil)*$_{t-1}$		0.013 [0.011]	0.0007 [0.011]	
Dummy for Δ^{+} *Ln(P oil)* $_{t-1}$			0.009 [0.004]**	
Δ^{+} *Ln(P oil)*$_{t-1}^{2}$				-0.018 [0.006)***
Country dummies	Yes	Yes	Yes	Yes
*Time Trend*Country*	Yes	Yes	Yes	Yes
Political Controls	Yes	Yes	Yes	Yes
Observations	2223	2181	2181	2223
Partial R-squared	0.0014	0.0038	0.0061	0.0042
F test	2.87	3.83	3.97	8.08
p-value	(0.0903)	(0.0219)	(0.0079)	(0.0045)
Overid test, χ^2	--	0.78	4.48	--
p-value	--	(0.38)	(0.11)	--

Robust clustered standard errors in brackets. *F* test is test of joint significance of excluded instruments
* significant at 10%; ** significant at 5%; *** significant at 1%

Our main interest, however, is not in testing the relationship between oil prices and growth. Although the model in column (3) appears to describe the relationship between oil shocks and income growth reasonably well, the first stage functional form does not have to be right in order to get consistent estimates in the second stage (Angrist and Krueger 2001). It is more important to find out which functional form of oil prices provides better instruments for our model. Asymmetric effects of oil prices on growth may suggest that the quadratic term for positive oil price growth rates may be relevant for estimating the effects of oil shocks on GDP growth rates. Intuitively, negative effects of positive oil price shocks can be higher when these shocks are larger. Column (4) shows that $\Delta Ln^{+}(Poil)_{t-1}^{2}$ is indeed correlated with income growth with the expected negative sign. F statistics is 8.08, which is more than two times bigger than the statistics in (3).

Angrist and Krueger (2001) suggest that the bias of IV-2SLS estimates is proportional to the degree of overidentification if instruments are weak, and the use of fewer instruments reduces bias. Hence, our preferred candidate for an excluded instrument is the one in column (4).

To summarize, there is evidence of an asymmetry in oil price effects with respect to GDP per capita growth. In comparison to the case when symmetry is assumed, allowing for asymmetry in first stage regressions provides a better choice of instruments for the IV model because correlations are stronger and more statistically significant.

2.6 Main Empirical Results

The major focus is on finding whether economic shocks have an effect on the incidence of politically destabilizing events. As it was argued in section 2.3, this task is complicated by feedback effects from political instability to economic conditions. We have also argued that feedback is most likely to be negative as it is consistently documented by a very large volume of the empirical literature. If one does not take into account the endogeneity in the growth rates in simple OLS or Probit regressions, the estimated growth coefficient β in equation (2.3) is biased downwards.

In this section, we present our main estimated impacts of economic shocks on the incidence of various events. One of the key tasks is to assess whether the bias in OLS and Probit specifications is present, and whether the instrumental variable approach (IV) is able to correct the endogeneity bias. If the above proposition is true then the OLS and

Probit estimated coefficient should be smaller than the IV coefficient, $\beta_{OLS|PROBIT} < \beta_{IV-2SLS|IV-PROBIT}$.

2.6.1 Unrest

In Table 2.4, columns (1) through (3) show OLS and Probit estimated effects of per capita income growth rates on the incidence of Unrest which is measured as a combination of indicators for General Strikes, Anti Government Demonstrations, and Riots. Columns (1) and (2) do not include country dummies while column (3) includes country fixed effects to capture time invariant country characteristics that may be related to political events under consideration.

The first thing to note is that the OLS and Probit regression coefficients in (1)-(3) display a negative relationship between growth rates and the probability of the occurrence of Unrest. The size of the estimated impact of economic growth on Unrest in (3) indicates that a one-percentage-point decrease in the contemporaneous growth rate increases the likelihood of Unrest by 0.50 percentage points.

These results differ dramatically if the endogeneity is taken into account in IV regressions in columns (4)-(7), Table 2.4. Columns (4) and (5) show coefficient of IV-Probit estimation while columns (6) and (7) display IV-2SLS results. The IV coefficients of $Growth_t$ term in all of these columns are similar. It is striking that we find that the signs of coefficients in IV columns are positive. Therefore, in contrast to most of the existing empirical evidence, it is not the deterioration but the improvement in economic conditions that increases the likelihood of unrest, further supporting the idea of "rising

expectations". The correction of the endogeneity seems to be consistent with the fact that $\beta_{OLS|PROBIT} < \beta_{IV-2SLS|IV-PROBIT}$ if there is a negative feedback effect from *Unrest* to *Growth*. The size of the estimated impact of economic growth is quite large: significant marginal effect in column (4) implies that one-percentage-point rise in GDP per capita increases the likelihood of the *Unrest* by 3.41 percentage points. Therefore, if GDP goes up by seven-percentage-point, which is about one standard deviation in annual per capita income growth (Table 2.2), then the probability of *Unrest* will increase by about 24-percentage-point. This increase is nearly equal to the average incidence of *Unrest* in the data (Table 2.2).

Regressions in Table 2.5 estimate the impact of *Growth* on *Unrest* by region. The impacts are still positive in all regions except Europe and South East Asia. However, the estimated positive effects of Growth are significant in Latin America, Sub-Saharan Africa, and South East Asia. Shown results are estimated by the IV-Probit regressions. The IV-2SLS estimates of growth effects are similar in magnitudes and the signs of coefficients, but significance levels are lower (Table A2.3 in the Appendix 2.1). In addition, the oil shock instrument in IV-2SLS regressions is very weak in all regional regressions except for Western Europe, as shown by the F tests in Table A2.3 in the Appendix 2.1. Thus, the evidence is not completely convincing, but suggestive about some of the hypotheses. In particular, the interpretation of differences in the signs of coefficients across regions may be consistent with the views of Olson (1963) and Huntington (1968) as outlined in section 2. Huntington argued that economic development puts pressure on existing institutions and that

Table 2.4 Effect of Economic Growth on Probability of Unrest

	Unrest						
Explanatory variables	OLS (1)	Probit (2)	Probit (3)	IV-Probit (4)	IV-Probit (5)	IV-2SLS (6)	IV-2SLS (7)
Growth $_t$	-0.296 [0.122]**	-0.43 [0.173]**	-0.508 [0.190]***	3.41 [1.48]**	2.76 [1.30]**	3.72 [2.21]*	3.51 [2.19]*
Democracy$_{t-1}$ *(Anocracy omitted)*	-0.121 [0.060]**	-0.120 [0.054]**	0.007 [0.055]	-0.08 [0.05]*	0.01 [0.04]	-0.12 [0.07]*	0.004 [0.07]
Autocracy$_{t-1}$	-0.083 [0.040]**	-0.099 [0.038]**	-0.049 [0.045]	-0.05 [0.04]	-0.02 [0.04]	-0.06 [0.05]	-0.04 [0.06]
Ethnic Fractionalization	-0.061 [0.190]	-0.053 [0.185]		0.03 [0.15]		0.01 [0.20]	
Military Regime$_{t-1}$	-0.069 [0.041]*	-0.085 [0.042]*	-0.056 [0.049]	-0.08 [0.03]**	-0.05 [0.04]	-0.09 [0.05]*	-0.08 [0.06]
Urbanization	0.482 [0.231]**	0.516 [0.218]**	-1.139 [1.154]	0.40 [0.15]***	-0.93 [1.01]	0.53 [0.23]**	-1.18 [1.27]
Legislative Effectiveness$_{t-1}$	-0.034 [0.031]	-0.043 [0.034]	-0.029 [0.037]	-0.04 [0.03]	-0.03 [0.03]	-0.04 [0.04]	-0.05 [0.04]
Party Coalitions$_{t-1}$	0.021 [0.019]	0.025 [0.018]	0.002 [0.024]	0.002 [0.02]	-0.002 [0.02]	0.01 [0.02]	0.005 [0.03]
Party Legitimacy$_{t-1}$	-0.016 [0.053]	-0.025 [0.054]	-0.003 [0.062]	-0.005 [0.04]	-0.014 [0.05]	0 [0.06]	-0.01 [0.08]
Regional Dummies	Yes	Yes	No	Yes	No	Yes	No
Country dummies	No	No	Yes	No	Yes	No	Yes
*Trend*country*	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.24						
Observations	2223	2166	2166	2223	2223	2223	2223
F test of excl. instruments	---	---	---	---	---	7.90	8.08
p- value	---	---	---	---	---	(.0050)	(.0045)

Robust clustered standard errors in brackets, p-values in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%. Marginal effects (dy/dx) at sample averages are reported in Probit and IV-Probit regressions

political mobilization will increase more quickly than the appropriate institutions can accommodate.

Our results show that, with increased growth, political mobilization and activism increase in regions like Latin America and Sub-Saharan Africa where countries are less developed, and, therefore, institutions are not strong enough to promote stability.

On the other hand, our European sample includes exclusively developed countries (see Appendix 2.1.G) where institutions are strong and developed, so people are happy with governments when growth rates are high, but unhappy with bad economic conditions as suggested by the negative sign of the growth coefficient in column (6), Table 2.5.

Table 2.5. Effect of Economic Growth on Probability of Unrest by Regions

	Unrest by Different Regions, IV-Probit					
Explanatory variables	All Regions Full Sample	Latin America	S.-S. Africa	S.East Asia	Middle East	W. Europe
	(1)	(2)	(3)	(4)	(5)	(6)
Growth $_t$	3.41	6.94	4.42	-7.14	8.08	-2.16
	[1.48]**	[1.37***	[0.97]***	[7.41]	[1.11]***	[2.68]
Democracy$_{t-1}$	-0.08	-0.06	-0.063	-0.17	-0.12	-0.52
(Anocracy omitted)	[0.05]*	[0.10]	[0.067]	[0.15]	[0.25]	[0.20]**
Autocracy$_{t-1}$	-0.05	0.11	-0.07	-0.02	0.30	0.57
	[0.04]	[0.10	[0.06]	[0.19]	[0.18]*	[0.27]**
Ethnic	0.03	-0.06	0.37	-0.62	-1.41	-0.43
Fractionalization	[0.15]	[0.469]	[0.24]	[0.20]***	[1.43]	[0.42]
Military Regime$_{t-1}$	-0.08	-0.07	-0.09	0.21	-0.48	-0.19
	[0.03]**	[0.16]	[0.04]**	[0.13]*	[0.02]***	[0.03]***
Urbanization	0.40	0.53	0.25	-0.09	-0.03	1.02
	[0.15]***	[0.48]	[0.27]	[0.45]	[0.43]	[0.36]***
Legislative	-0.04	-0.002	-0.06	-0.03	0.16	-0.14
Effectiveness$_{t-1}$	[0.03]	[0.088]	[0.03]**	[0.05]	[0.08]**	[0.08]**
Party Coalitions$_{t-1}$	0.002	-0.06	-0.01	0.12	0.04	-0.21
	[0.02]	[0.05]	[0.03]	[0.07]*	[0.12]	[0.04]***
Party Legitimacy$_{t-1}$	-0.005	0.05	-0.05	-0.15	-0.31	0.42
	[0.04]	[0.09]	[0.06]	[0.07]**	[0.29]	[0.02]***
Const						
Regional Dummies	Yes	---	--	--	--	--
Country dummies	No	No	No	No	No	No
*Trend*country*	Yes	Yes	Yes	Yes	Yes	Yes
R^2						
Observations	2223	453	1002	311	155	296

Robust clustered standard errors in brackets, p-values in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%. Marginal effects (dy/dx) at sample averages are reported in Probit and IV-Probit regressions

In addition, Olson's story suggests that instability arises during *rapid* growth because there are vast changes in methods of production, importance of industries, and distribution of income. One could argue that there is simply more room for these changes

in less developed countries than in developed countries for a given increase in growth rates. As changes have greater magnitudes, there will be more gainers and losers, so pressure to change political order by the gainers and the political discontent of the losers will be much higher in developing countries.

The *Unrest* variable can be broadly interpreted as the initial political mobilization of masses of populations against current governments or regimes. According to the definitions of the variables (Appendix 2.1), these events are relatively peaceful. Conceptually, they can eventually just stop or be suppressed by the military and police, so that stability returns to the country without radical political changes in the existing political order. On the other hand, peaceful protests can grow into something bigger like revolutions, peaceful or violent, government crises, and reelections with changes in political power, and the worst, civil wars. In Table 2.4, the data and IV results seem to indicate that with rising incomes the incidence of relatively peaceful protests increases. Whether and how changing incomes are associated with more radical political events in our data, we will investigate in next subsections.

2.6.2 Violent Unrest and Government Crises

Table 2.6 shows the results of estimated *Growth* effects on probability of *Violent Unrest*, which is the dummy equal to one if revolutions or guerilla warfare occurred in the given year. Estimation order and procedures are similar to those in Table 2.4.[29] Overall, the size and significance of the effects of economic shocks on the probability of V*iolent Unrest* is lower than the impacts of *Growth* on *Unrest* in Table 2.4. Note that the

[29] The difference between the number of observations used in columns (1), (5)-(6) and probit columns (2)-(4) is because probit regressions discard country observations where Violent Unrest did not occur in the sample.

sign of *Growth* coefficients switches to positive in the IV regressions as well. The estimate of growth impacts is significant at 5 percent in IV-Probit specification (4) and one-percent increase in growth raises probability of Violent Unrest by almost three-percents. The IV-2SLS estimates in (5) are similar to the IV-Probit coefficients but are not statistically significant.

Table 2.6 Effect of Economic Growth on Probability of Violent Unrest

Dependent Variable	Violent Unrest						
Explanatory variables	OLS (1)	Probit (2)	Probit (3)	IV-Probit (4)	IV-2SLS (5)	IV-2SLS (6)	IV-2SLS (7)
Growth $_t$	-0.202	-0.269	-0.236	2.79	2.88	2.18	1.80
	[0.130]	[0.149]*	[0.153]	[1.55]*	[2.01]	[1.81]	[1.81]
Democracy$_{t-1}$	-0.163	-0.126	-0.036	-0.18	-0.22	-0.16	-0.05
(Anocracy omitted)	[0.065]**	[0.064]**	[0.070]	[0.05]***	[0.07]***	[0.06]**	[0.08]
Autocracy$_{t-1}$	-0.153	-0.126	-0.073	-0.12	-0.17	-0.14	-0.09
	[0.060]**	[0.055]**	[0.051]	[0.05]**	[0.07]**	[0.06]**	[0.05]*
Ethnic	0.1	0.063		0.01	0.016	0.14	
Fractionalization	[0.189]	[0.193]		[0.10]	[0.109]	[0.20]	
Military Regime$_{t-1}$	-0.023	-0.019	0.0004	0.04	0.05	-0.04	-0.02
	[0.043]	[0.041]	[0.048]	[0.06]	[0.07]	[0.05]	[0.05]
Urbanization	-0.104	0.036	-2.55	-0.15	-0.15	-0.08	-1.81
	[0.138]	[0.150]	[1.544]*	[0.13]	[0.12]	[0.14]	[1.18]
Legislative	-0.031	-0.022	-0.047	-0.02	-0.03	-0.03	-0.06
Effectiveness$_{t-1}$	[0.035]	[0.034]	[0.035]	[0.03]	[0.04]	[0.04]	[0.04]
Party Coalitions$_{t-1}$	-0.003	-0.005	0.023	-0.007	-0.008	-0.013	0.02
	[0.016]	[0.017]	[0.020]	[0.02]	[0.024]	[0.02]	[0.02]
Party Legitimacy$_{t-1}$	-0.062	-0.06	-0.033	-0.03	-0.04	-0.05	-0.03
	[0.068]	[0.062]	[0.068]	[0.05]	[0.06]	[0.07]	[0.07]
Regional Dummies	Yes	Yes	No	Yes	Yes	Yes	No
Country dummies	No	No	Yes	No	No	No	Yes
*Trend*country*	Yes	Yes	Yes	No	No	Yes	Yes
Observations	2223	1928	1928	2106	2223	2223	2223
R^2	0.25	---	---	---	---	---	---
F test of excl. instr.					6.66	7.90	8.08
p- value					(0.0099)	(0.0050)	(0.0045)

Robust clustered standard errors in brackets; p-values in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%; Marginal effects are reported in probit regressions

In Table 2.7, the effects of growth on the likelihood of government crises are estimated. Similar to the Table 2.4 results, columns (1)-(3) display negative and significant growth effects. In the IV regressions, the sign becomes positive and only

significant at a 10 percent level in the IV-Probit column (4). The IV-2SLS columns (6)-(8) indicate positive estimates but are not statistically significant. It is also clear that the *Growth* coefficient is not robust when country fixed effects and country specific time trends are included.

Regional differences in the estimates of Table 2.7 are not reported as the estimates are not informative, because the standard errors are quite large. For the rest of the variables (Successful Coups, Assassinations, Changes in Executive, and Cabinet Changes, results are shown in Appendix 2.1, Table A2.4), the data does not show a significant relationship between growth and the likelihood of these events in the IV regressions. However, Growth coefficients are negative but insignificant in the IV regressions.

In sum, when the joint endogeneity between contemporary growth rates and political instability measures is not taken into account, the data displays negative association between these variable. Once we use the instrumental variable approach, growth rates are positively and significantly related to the incidence of some political events. The IV results strongly suggest that higher growth rates of income increase the incidence of relatively peaceful protest. The evidence that the likelihood of violent unrest and government crises also increases is less significant and robust. Our interpretation is that it is much easier to attend peaceful demonstrations and express political demands whereas it is much more costly and risky to participate in revolutions and armed violent activities where, not only freedom, but also lives are at stake.

Table 2.7 Effect of Economic Growth on Probability of Destabilizing Political Events: Government Crises

Dependent Variable	Government Crises				
	OLS	Probit	Probit	IV-Probit	IV-Probit
Explanatory variables	(1)	(2)	(3)	(4)	(5)
Growth $_t$	-0.278	-0.296	-0.336	3.04	1.72
	[0.129]**	[0.120]**	[0.125]***	[1.74]*	[1.73]
Democracy$_{t-1}$	-0.049	-0.03	-0.005	-0.06	-0.03
(Anocracy omitted)	[0.042]	[0.028]	[0.033]	[0.04]*	[0.03]
Autocracy$_{t-1}$	-0.094	-0.077	-0.087	-0.10	-0.05
	[0.029]***	[0.018]***	[0.022]***	[0.04]**	[0.02]**
Ethnic Fractionalization	0.051	0.06		0.04	0.09
	[0.171]	[0.122]		[0.08]	[0.11]
Military Regime$_{t-1}$	-0.005	0.002	0.02	-0.001	-0.01
	[0.032]	[0.030]	[0.034]	[0.04]	[0.03]
Urbanization	0.108	0.01	-0.913	0.02	0.03
	[0.170]	[0.106]	[0.265]***	[0.10]	[0.09]
Legislative	-0.002	-0.005	0.007	-0.003	-0.005
Effectiveness$_{t-1}$	[0.022]	[0.018]	[0.019]	[0.025]	[0.017]
Party Coalitions$_{t-1}$	-0.004	0.002	0.007	-0.03	-0.006
	[0.012]	[0.011]	[0.011]	[0.015]**	[0.011]
Party Legitimacy$_{t-1}$	0.064	0.049	0.052	0.05	0.05
	[0.040]	[0.028]*	[0.030]*	[0.04]	[0.03]
Regional Dummies	Yes	Yes	No	Yes	Yes
Country dummies	No	No	Yes	No	No
*Trend*country*	No	Yes	No	No	Yes
Observations	2223	2001	2001	2223	2067
F test of excl. instrument					
p-value					

Robust clustered standard errors in brackets; p-values in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%; Marginal effects are reported in probit regressions

2.6.3 More on Controls

With few exceptions, control variables that are prominently cited in the literature show varying statistical predictive powers in different regressions, as shown in Tables 2.4-2.7.

Table 2.7 (cont.) Effect of Economic Growth on Probability of Destabilizing Political Events: Government Crises

Dependent Variable	Government Crises		
Explanatory variables	IV-2SLS (6)	IV-2SLS (7)	IV-2SLS (8)
Growth $_t$	2.02	1.93	1.36
	[1.50]	[1.44]	[1.35]
Democracy$_{t-1}$	-0.06	-0.05	0.02
(Anocracy omitted)	[0.05]	[0.04]	[0.06]
Autocracy$_{t-1}$	-0.1	-0.08	-0.08
	[0.04]**	[0.04]**	[0.04]*
Ethnic Fractionalization	0.02	0.09	
	[0.08]	[0.17]	
Military Regime$_{t-1}$	0	-0.02	0.01
	[0.04]	[0.04]	[0.04]
Urbanization	-0.01	0.13	-2.86
	[0.11]	[0.17]	[1.15]**
Legislative	0.01	-0.005	0.01
Effectiveness$_{t-1}$	[0.03]	[0.023]	[0.03]
Party Coalitions$_{t-1}$	-0.03	-0.01	-0.01
	[0.01]*	[0.01]	[0.01]
Party Legitimacy$_{t-1}$	0.04	0.07	0.07
	[0.04]	[0.04]*	[0.05]
Regional Dummies	Yes	Yes	No
Country dummies	No	No	Yes
*Trend*country*	No	Yes	Yes
Observations	2223	2223	2223
F test of excl. instrument	6.66	7.90	8.08
p-value	(0.0099)	(0.0050)	(0.0045)

Robust clustered standard errors in brackets; p-values in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%; Marginal effects are reported in probit regressions

Democracies and Autocracies are more stable than Anocracies in Tables 2.4, 2.6, and 2.7. *Urbanization* appears to be important for *Unrest* (Table 2.4, columns (4)-(5)) and *Violent Unrest* (Table 2.6, column (3)). However, unlike the effect on *Unrest*, the impact of *Urbanization* on *Violent Unrest* is negative which is consistent with the fact that most rebellions and civil wars are fought in rural environments (Auvinen (1997).

The additional support for the importance of institutions is found in estimates of some control variables although some estimates are potentially biased due to endogeneity. Column (4) in Table 2.4 indicates that *Unrest* is less probable in regimes controlled by the military and in countries with established democracies. Note that in Table 2.5, regional differences in size and significance of some control variables have interesting implications as well. For example, lower incidence of unrest in Western Europe is associated with democracies, military regimes, lower urbanization, more effective legislature, and greater political competition measured by the party coalition variable (column (6), Table 2.5). In Sub-Saharan Africa, a lower incidence of Unrest is related to greater legislative effectiveness, lower ethnic fractionalization, and military regimes. Thus, the significance of institutional variables for instability seems to vary across regions. These differences also imply that the importance of the strength of institutions for political stability in less developed countries. Why would legislative effectiveness and regime type be important in Sub-Saharan Africa, but not levels of democracy and the variation in political party coalitions? Western Europe and Sub-Saharan Africa represent regions of two extremes in institutional development. One of the possibilities is that in the region where the level of institutional development is very low, the variation in democracy levels and political competition provides a poorer measure of institutional strength than measures of legislative effectiveness and the type of regime.

2.6.4 Robustness Checks and Alternative Instruments

Additional robustness checks are shown in Table 2.8. Our major finding is that economic growth is directly related to probability of *Unrest*. In Table 2.4, it is clear that the IV estimates are robust to inclusion of country fixed effects and country specific time trends. The IV-Probit and IV-2SLS coefficients are also very similar. We use columns (4) and (5) in Table 2.4 as benchmark specifications to evaluate the robustness of the results. In Table 2.8, one should examine whether differences in samples and the use of alternative explanatory variables, as well as changes in estimation methods, significantly change the IV coefficient of $Growth_{t-1}$ variable.

Column (2) adds country fixed effects and excludes all other controls, which increases the sample size by almost 200 observations. We see that the growth coefficient is slightly lower and still significant at the five-percent level.

Furthermore, we tested the robustness of the results by adding the level of economic development. According to Huntington (1968):

> "The relation between the rate of economic growth and political instability varies with the level of economic development. At low levels, a positive relation exists, at medium levels no significant relation, and at high levels a negative relationship" (p. 53).

This may suggest that instability is a quadratic function of the level of economic development variable. We used the log of moving averages of five observations before the current observation of real GDP per capita. The reason for using five-year averages

was to reduce the possibility of feedback effects from *Unrest* to income level. However, there is still a possibility that persistent instability may affect income levels in return, but our identification strategy is not able to control that. The results are reported in column (4), Table 2.8. Both terms in the quadratic form of the economic development variable are highly significant and indicate the inverted U shape relationship between economic development and *Unrest*. Growth IV coefficient is again robust to inclusion of the level of economic development. Note that exclusion F test in column (4) indicates a great improvement in F statistics, which is now equal to 12.62. Thus, control for the level of economic development improves the predictive power of the instrument.

In addition, the lagged two-year moving average of *Purges* variable is included as a control for political repression to test the robustness of the results. Moreover, we will be able to test an alternative channel of why lower growth can result in lesser unrest. One could argue that lower growth may induce the government to increase the political repression to prevent unrest. The repression variable has a positive but insignificant impact on the probability of *Unrest* in column (4) of Table 2.8. Growth coefficient does not change dramatically either. Moreover, in separate regression (not shown) we do not find that *Growth* variable has a significant impact on *Purges* in the IV regression, where *Growth* is instrumented by oil shocks. Therefore, this result does not agree with the explanation that lower growth affects unrest through changes in political repression.[30] Freedom house indexes for period 1972-1999 were also tried in the place of *Purges* variable but coefficients are found to be very small and insignificant in regressions (results are not shown).

[30] However, higher unrest may in turn cause more repression, which will cause the estimate of the effect of repressions on unrest to be biased upwards.

Another objection to results may arise because secular time trends in oil price variation affecting G*rowth* may be correlated independently with the time variation in the incidence of *Unrest* compromising the causal inference of the effects of *Growth* on *Unrest*. To account for this possibility, in addition to country-specific time trends, we include country-specific time trends squared. Column (6) in Table 2.8 shows that the positive sign of *Growth* coefficient remains and the absolute value of it increases although the significance level is slightly lower due to the larger standard error, which can be explained by weaker instruments in first stage regression.

Finally, one can also argue that changes in the world oil prices may not provide enough variation to explain changes in growth rates across countries since the former only varies with time but not across countries. We address this concern by using the alternative form of instruments – country-specific oil shocks, measured as the interactions of $\Delta^{+} Ln(Poil)_{t-1}{}^{2}$ with country dummies. IVprobit results are reported in column (7) of Table 2.8 and IV-2SLS results are reported in column (8). The marginal effect of *Growth* on *Unrest* is positive and significant at 1 percent level in column (7). However, it is still positive but much smaller in absolute value in column (8) than in (7). Such difference between IVprobit and IV-2SLS estimates are puzzling. The IV-2SLS coefficient is 1.54 and significant at 5 percent level if sample is restricted to Latin America and Sub-Saharan Africa. Note that the results of the first stage regression, reported in Appendix 2.1, Table A2.5, indicate that increases in oil prices are not always associated with lower growth in some countries. For such countries as Gambia, Jordan, Paraguay, Uruguay and Zimbabwe, coefficients are positive and significant.

Table 2.8 Effect of Economic Growth on Probability of Unrest: Robustness Checks

Instrument	Δ^{+} Ln(P oil)$_{t-1}^{2}$					
Explanatory variables	IV-Probit (1)	IV-Probit (2)	IV-2SLS (3)	IV-2SLS (4)	IV-Probit (5)	IV-2SLS (6)
Growth $_t$	3.41	3.17	3.72	3.1	2.97	4.59
	[1.48]**	[1.65]**	[2.21]*	[1.94]	[1.50]**	[2.87]
Democracy$_{t-1}$	-0.08		-0.12	-0.12	-0.09	0.03
(Anocracy omitted)	[0.05]*		[0.07]*	[0.07]*	[0.05]*	[0.07]
Autocracy$_{t-1}$	-0.05		-0.06	-0.05	-0.04	-0.03
	[0.04]		[0.05]	[0.04]	[0.04]	[0.05]
Ethnic Fractionalization	0.03		0.01	0.15	0.16	0.12
	[0.15]		[0.20]	[0.20]	[0.15]	[0.40]
Military Regime$_{t-1}$	-0.08		-0.09	-0.07	-0.06	-0.09
	[0.03]**		[0.05]*	[0.05]	[0.04]*	[0.07]
Urbanization	0.40		0.53	0.4	0.27	0.5
	[0.15]***		[0.23]**	[0.27]	[0.26]	[0.34]
Ln (Income MA(5))$_{t-1}$				1.18	1.17	
				[0.53]**	[0.61]**	
[Ln (Income MA(5))] $^{2}_{t-1}$				-0.07	-0.07	
				[0.04]**	[0.04]**	
Purges MA(2)$_{t-1}$				0.03	0.02	
				[0.02]	[0.02]	
Regional Dummies	Yes	No	Yes	Yes	Yes	Yes
Country dummies	No	Yes	No	No	No	No
Trend X *Country*	Yes	Yes	Yes	Yes	Yes	Yes
Trend ^{2} X *Country*	No	No	No	No	No	Yes
Observations	2223	2411	2223	2151	2151	2223
F test of excl. instrument			7.90	12.62		5.53
			(.0050)	(0.0004)		(0.0188)
Overid test , χ^{2}						

Robust clustered standard errors in brackets, p-values in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%. Marginal effects (dy/dx) are reported in probit regressions; *Legislative Effectiveness, Party Coalitions, Party Legitimacy* are included but not reported

Table 2.8 (cont.) Effect of Economic Growth on Probability of Unrest: Robustness Checks

Instrument	$\Delta^{+} Ln(P\ oil)_{t-1}^{2}$ X Country	
	IV-Probit	IV-2SLS
Explanatory variables	(7)	(8)
Growth $_t$	6.01	0.78
	[0.48]***	[0.92]
$Democracy_{t-1}$	-0.001	-0.12
(Anocracy omitted)	[0.04]	[0.06]*
$Autocracy_{t-1}$	0.03	-0.08
	[0.04]	[0.04]*
Ethnic Fractionalization	0.13	-0.04
	[0.10]	[0.19]
$Military\ Regime_{t-1}$	-0.04	-0.08
	[0.03]	[0.04]*
Urbanization	0.07	0.49
	[0.09]	[0.23]**
Regional Dummies	Yes	Yes
Country dummies	No	No
Trend X *Country*	Yes	Yes
Trend ^{2} X *Country*	No	No
Observations	2223	2223
F test of excl. instrument		13.72 (0.0000)
Overid test , χ^2		65.8 (0.31)

Robust clustered standard errors in brackets, p-values in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%. Marginal effects (dy/dx) are reported in probit regressions; *Legislative Effectiveness, Party Coalitions, Party Legitimacy* are included but not reported

2.7 Comparison with Miguel et al. results

With the exception of Campos and Nugent (2002), why do other empirical studies not achieve similar results to those obtained in this study? One could argue that this happens because other studies did not address the joint endogeneity or used invalid instruments. However, Miguel et al. (2004) used a very similar method. They estimated the effect of economic shocks instrumented by rainfall changes on the incidence of civil conflict. Their estimates are not directly comparable to results of this study because our main result is more about peaceful protests against existing governments and regimes.

Nevertheless, some comparison can be made, and differences in the IV results are striking. In Miguel et al. study, the OLS/Probit estimates are very similar to our results in Tables 4, 6, and 7, which are close to -0.3. However, the Miguel et al. IV-2SLS results are very different from our findings. They have more negative IV coefficients and

$$\beta_{2SLS} < \beta_{OLS} < 0$$

Miguel et al. (2004) acknowledged that the IV coefficient should be greater than the OLS coefficient in the presence of the downward bias in OLS estimation. They suggest that the attenuation bias in the OLS regression may arise from measurement errors in the GDP variable, which would bias the OLS estimates towards zero. As a result, if the attenuation bias is greater than the downward bias from joint endogeneity, the IV-2SLS estimates would be more negative than the OLS estimates. Some of the OLS estimates in their study, however, have a positive sign, which casts doubt on this explanation. Another explanation is given in the footnote:

> "An alternative explanation for the divergence between OLS and instrumental variable estimates is that the first stage systematically underestimates the impact of rainfall shocks on economic growth—because of extensive nonclassical measurement error in national accounts data—leading instrumental variable estimates to exaggerate the impact of economic growth on conflict…"

However, further explanation and investigation of this statement is not given in their study.

The replication of the results in Miguel et al.(2004) can shed some light on the differences in IV results[31]. Oil shock instruments are statistically rather weak in their sample.[32] We used rainfall instruments as in Miguel et al. in columns (1)-(6), where (1)-

[31]Thanks to Edward Miguel, the data is available at http://elsa.berkeley.edu/~emiguel/data.shtml

[32] The original sample consisted of 41 countries in Sub Saharan Africa but we only used countries that are in our sample. This will reduce the number of observations to 501 in 27 countries as opposed to 743

(3) are compared to (4)-(6) and (7)-(9) in Table 2.9. Column (1) presents results corresponding to the model used in Miguel et al. (2004). Column (2) separately estimates the effect of growth rates at time *t* while in column (3) the impact of the lagged growth is estimated separately. The dependent variable is Civil Conflict, and it is equal to one if there was any of the following: Internal Minor Armed Conflict, Internal Intermediate Armed Conflict, and Internal War (see Appendix 2.1 for details). In columns (4)-(7), the dependent variable is from the Banks-CNTS data used in this study, Violent Unrest and Unrest.

The lagged growth coefficient is negative in (1), (4) and (7) with the greatest magnitude in (1), which is the main result in Miguel et al. The effect of current growth on Violent Unrest is positive in (2) and (5), but more sizable in (5), which is similar to what we find in this study.

Therefore, differences in the IV results may arise because Miguel et al. used a lagged growth variable in addition to the current economic growth variable. In our sample with oil shocks instruments, the use of lagged growth rates together with contemporaneous growth rates in our model does not provide meaningful results because of the very large values of standard errors (not shown). In addition, the effect of lagged growth on *Unres*t in our data is also positive and significant at the ten-percent level (not shown). Thus, the differences arise because of the distinct nature of

observations in the Miguel et al. study, but replicated results are very similar to the original reported results.

Table 2.9 Economic Growth and Instability in Sub Saharan Africa: Alternative measures and IV specifications using rainfall instruments

Dependent Variable	Civil Conflict ≥ 25 deaths (Prio/Uppsala data in Miguel et al.) S. S. Africa			Violent Unrest (Banks: revolutions & guerilla warfare) S. S. Africa			Unrest (Demonstrations, Strikes, Riots) S. S. Africa		
Instruments	Rainfall Shocks			Rainfall Shocks			Rainfall Shocks		
Explanatory variables	IV-2SLS (1)	IV-2SLS (2)	IV-2SLS (3)	IV-2SLS (4)	IV-2SLS (5)	IV-2SLS (6)	IV-2SLS (7)	IV-2SLS (8)	IV-2SLS (9)
Economic Growth $_t$	-3.95 [4.28]	0.48 [0.88]		1.53 [2.52]	3.07 [1.96]		-1.28 [2.56]	-0.69 [1.55]	
Economic Growth $_{t-1}$	-4.27 [2.69]		-3.29 [1.99]	-1.48 [2.17]		-1.91 [1.46]	-0.57 [2.42]		-0.16 [1.99]
Country Fixed Effects	Yes	Yes	Yes						
Country-specific time trends	Yes	Yes	Yes						
Observations	491	491	494	490	490	493	490	490	493
F test of excl. instruments:									
Growth $_t$ *equation*	5.44 (.0046)	9.90 (.0018)		5.44 (.0046)	9.90 (.0018)		5.44 (.0046)	9.90 (.0018)	
Growth $_{t-1}$ *equation*	6.95 (.0011)		8.66 (.0034)	6.95 (.0011)		8.66 (.0034)	6.95 (.0011)		8.66 (.0034)

the dependent variable and samples. Political events variables in Banks-CNTS data are targeted specifically to reflect the activity against existing governments and regimes. However, the PRIO/Uppsala database measure of the conflict in Miguel et al. records any internal armed activity which involved more than 25 deaths. The conflict may involve, for instance, two different ethnic or religious groups but not necessarily existing regimes. The correlation between Civil Conflict and Violent Unrest variables in the replication data is not very high, 0.56. Therefore, "revolutions of rising expectations" motive may be

stronger in Banks-CNTS data especially for variables measuring political mobilization of masses against existing governments and regimes.

8 Concluding Remarks

In this study, we have studied the effects of economic shocks on the incidence of politically destabilizing events. The OLS regressions show that income growth exhibits a strong negative correlation with the occurrence of all politically unstable events. In contrast, it is found here that if the joint endogeneity is addressed by the Instrumental Variable method, higher growth rates are associated with an increased likelihood of some events, especially those that indicate relatively peaceful mass protests against existing governments and regimes. However, it is important to note that our findings do not apply to the whole range of the politically unstable events in the data such as coups d'etat, changes in effective executive, cabinet changes, and political assassinations.

Nevertheless, the finding is very controversial as previous empirical evidence suggests either that there is no causal effect of growth on political instability or that it is negative. On the other hand, theories by Olson (1963) and Huntington (1968) argue that changes in growth rates may be directly related to the incidence of political instability. Olson, at the same time, does not deny that the sudden decrease or stagnation in income level could also be destabilizing so both *rapid* economic growth and *rapid* economic decline would tend to increase instability. Perhaps, further research is needed to build an empirical model that would be able to examine both types of effects at the same time.

In addition, our interpretation of Huntington suggests that the effects of economic shocks depend on the level economic development. We find some support of the Huntington's idea that positive relationship is likely in less developed countries, while it may be absent or slightly negative in developed countries. We also found that the relationship of level of development with respect to unrest display the inverted U shape association. However, the level of economic development is itself may be endogenous variable and our empirical strategy is not suited to account that. Moreover, studying the relationship of economic development and political instability requires approach that takes a longer run perspective, which is different from the short run horizon used in this study.

The implications of the results obtained in this study are striking. In fast growing countries, it is possible to affect the probability of political unrest by both domestic and foreign policies. For instance, an income redistribution providing some compensation to losers from the rapid growth will alleviate their discontent and reduce the likelihood of protests. Likewise, an improvement of antitrust laws and regulations enhancing the competition and reducing the concentration of economic powers will reduce the opportunity of organizing mass protests by gainers.

Chapter 3: Estimating Partisan Effects on Income per Capita Growth in U.S States: a Regression Discontinuity Approach

3.1 Introduction

Whether political affiliations really matter when considering economic policy outcomes, has been the subject of long-standing debates within political science and economic literature.

This study explores the question of whether the differences in Democratic and Republican governorship have an impact on economic performance measures such as state's economic growth. The credible answer to this question would offer an interesting contribution to the existing huge body of the empirical literature relating partisan differences and economic outcomes[33]. The credibility, however, is difficult to establish due to the well-known issue of endogeneity in the party control variable. This is one of the reasons why studies reach such differing conclusions on the significance of party control for various policy outcomes.

When examining studies of the United States, surprisingly, there are no studys analyzing the partisan effects on economic growth using the data from individual states. Yet, some studys employ aggregate macroeconomic data for the US and selected OECD countries, using such variables as income growth, unemployment and inflation to analyze the partisan differences[34]. It has been found that there are partisan differences for

[33] Imbeau et al. (2001) in their meta-analysis identify over 600 studies containing empirical results relevant to left-right party ideology and government policies.
[34] Tufte (1978), Alt (1985), Hibbs (1987), Alesina, Roubini and Cohen(1997), Faust and Irons (1999)

economic outcome variables for the US as well as OECD countries[35]. On the other hand, research has been done on income convergence using the data from individual states without exploring the aspect of partisan differences. [Barro and Sala-i-Martin (1992)]

Many studys, however, utilize data from the states and test the effects of partisan differences on such policy variables as taxes, expenditures, and welfare transfers but not income growth[36]. A survey article by Besley and Case (2003) finds that studys come to very different conclusions on the significance of party control for policy outputs due to differences in estimation strategies, outcome variables, time periods, and measures of party controls. In particular, they report that much of the earlier literature reveals a very little evidence that party control matters for these policy variables[37] while some of the more recent work[38] finds support for party control being important for the various policy variables mentioned above.

Some authors acknowledge that endogeneity, in the party control variable, may not be addressed properly in cross-country studies [Faust and Irons (1999), Drazen (2000)]. The endogeneity of voters' choice, during the elections, makes estimation of causal effects difficult. Voters might be affected by expected economic conditions if they can forecast future outcomes with some accuracy and, they believe that parties are suited differently to specific economic conditions [Leigh (2005)]. Similarly, the desire for particular economic outcomes may affect partisan electoral victories, rather than elections causing

[35] see details in Drazen (2000, 2001), and literature survey in Franzese (2002)
[36] Besley and Case (1995) tests party control effects on income per capita and find overall no differences. However, Democrats that cannot run for reelection show negative and significant effect.
[37] Winters (1976), Dye (1984), Plotnick and Winters (1985), Garand (1988), Gilligan and Matsusaka (1995).
[38] Knight (2000), Grogan(1994), Besley and Case (1995), Rogers and Rogers (2000), Alt and Lowry (1994, 2000), Clarke(1998).

economic outcomes [Drazen (2000)]. Studys previously cited by Besley and Case (2003) have not discussed the potential endogeneity in the party control variable.

By contrast, this study tests directly the effects of the party control on income growth in the states and also addresses the endogeneity in the variable related to election outcomes by employing the Regression Discontinuity (RD) method. In the context of elections, the identifying source of information comes from the bi-partisan feature of the US election institutions: the candidate wins only if 50 percent of the vote has been obtained[39]. At this threshold value, the assignment of the party control could be seen as a random "treatment" in very close elections. The treatment in this case is assigned to those candidates whose vote share crosses the 50 percent threshold. Outcomes for candidates whose value of vote share is just below the threshold should represent a valid counterfactual for the treated group just above the threshold. Why so? Because states where, for instance, Democrats just barely won an election are likely to be comparable in all other ways to states where Democrats just barely lost the election. Applying the Regression Discontinuity method allows estimation of the unbiased effect of partisan differences on economic growth in the US states.

In the next section we will discuss how the RD method is used in the literature within the context of elections. Section 3.3 outlines the empirical framework and section 3.4 outlines the empirical strategy. The results and testing of empirical strategy are presented in section 3.5.

[39] 50 percent is the threshold when there are only two parties. In the study, the focus is only on election races between Democrats and Republicans only.

3.2 Regression Discontinuity in the Elections Context

The application of the regression discontinuity approach in the context of elections is used by Lee et al. (2004), Lee (2005), Leigh (2005) and Pettersson-Lidbom (2006).

The first two studys, cited above, study the empirical questions related to U.S. Congressional House elections, and, their research questions are different from the one posed in this study.

Lee et al. (2004) study the role of elections in policy formation of politicians in the U.S. House. They argue if electoral strength is high then a politician can afford to behave in more partisan way while a weaker candidate would be forced to choose more moderate policy. In the case of the full policy divergence, the relative electoral strength is irrelevant for politician's behavior measured by subsequent roll-call voting records. Thus, policy formation should be tested against changes in electoral strength. However, electoral strength is endogenously determined. Focusing on very close elections between Democrats and Republicans allows them to generate quasi-experimental estimates of the effects of a "randomized" change in electoral strength. According to authors, the initial "random assignment" of who wins previous elections generates random assignment of greater electoral strength for next elections because incumbents are known to posses an electoral advantage. They find no evidence in shifting politicians' behavior in either party when there is an exogenous increase in electoral strength for Democrats.

Lee (2005) focuses more on validity of using RD method in analyzing the electoral advantage to incumbency in the U.S. Congressional House elections. His study argues

that RD analysis for this particular data can be seen as credible as the evidence from randomized experiment. Lee (2005) also verifies if the randomization worked by examining whether treated and control groups have the same distribution of baseline characteristics. Author found that there is the advantage to incumbency, and also verified that in close elections districts where party wins or loses are similar in pre-determined characteristics.

Both Lee et al. (2004) and Lee (2005) compare bare democratic winners and losers during previous Congressional House elections in various districts. The outcome variable in Lee et al. (2004) is politicians' roll-cast voting behavior in the next period while in Lee (2005) the outcome is the probability of winning next elections by the democratic candidate[40]. In this study, bare democratic winners and losers in gubernatorial elections are compared. The outcomes are subsequent income growth rates in states. Therefore, the estimated discontinuity (if any) can be interpreted as the difference in subsequent growth performance between states where Democrats barely win elections and states where Democrats barely lose elections (Republicans win). However, Lee and coauthors are able to use substantially greater number of observations in house elections by districts.

Pettersson-Lidbom (2006) looks at Swedish local government elections to study the differences in fiscal policy and unemployment rates between left-wing and right-wing governments so his study employs different country-data and dependent variables. The results show that left-wing governments spend and tax 2-3 percent more than right-wing governments. Left-wing governments also have lower unemployment rates, which is

[40] Whether or not the candidate runs for re-election.

partly due to that left-wing governments employ 4 percent more workers than right-wing governments.

Finally, the major focus of Leigh (2005) is to test partisan differences on a very large set of policy settings and outcomes (32 measures) using the data from U.S. gubernatorial elections. One of the variables in Leigh's study is the level of income per capita and it claims to study a very similar question with the same methodology and data. The scope of the study is very broad. Moreover, the major focus is not the application of regression discontinuity method (despite of the title of the article) but rather a small element of the RD design is used as a robustness check for results by controlling for vote share variable (linear form of control function). The model regresses a given policy or outcome on an indicator of whether the governor is a Democrat. Author claims that the endogeneity in party control is accounted for by inclusion of the vote share received by the Democratic candidate. Effectively, such an approach uses control function in the linear form, and is likely to result in misspecification and inconsistent estimates[41]. It is not clear from the results and not discussed in the study in which way controlling for vote share variable affects the estimates. Leigh (2005) does not check changes in results by restricting sample to close elections. None of the RD method's validity and credibility checks in the context of U.S. gubernatorial elections is done in Leigh (2005). Consequently, it is questionable that such empirical strategy exploits to a full degree (if at all) advantages offered by RD method and it is not clear if the RD approach is applicable and useful in the analysis.

[41] see below Section 3.3 for more discussion and common approach used in the literature.

In my opinion, the application of the RD method in U.S. gubernatorial elections deserves greater attention and more analytical efforts. First, the focus is on growth in income. Second, there is implementation of the RD design by using both more flexible control function approach and restricting the sample to close elections. Finally, there is verification of the validity of the RD method by checking the distribution of the covariates around discontinuity point. The analysis will hopefully help future researchers to decide whether the RD approach is a valid and credible identification strategy in the context of the U.S. gubernatorial elections

3 Empirical Framework

3.3.1 The Model

A primary focus is estimating the causal impact of a Democratic victory in a U.S. gubernatorial race on the measure of the subsequent economic performance measure, which is in the form of *growth* in personal per capita income.

In particular, the starting point can be presented in the following simple equation:

$$Y_{it} = \alpha + \beta D_{it} + \varepsilon_{it} \qquad (3.1)$$

where Y_{it} is a measure of income per capita growth in state i at time t, and D_{it} measures a party control variable. In this case, it is a dummy variable equal to 1, in time periods subsequent to the year when Democrat won the election, until next election year.

The parameter β should represent the average difference in Y between Democrats and Republicans if $E[\varepsilon_{it}| D_{it}] = 0$. However, voters' choice D_{it} may be correlated with an error

term because of omitted unobservables such as quality of candidates, the expected economic environment and other unmeasured characteristics of voters, states and the candidates that are relevant for economic performance measures. If the error term is correlated with the party control variable, $E[\varepsilon_{it}| D_{it}] \neq 0$, due to the presence of unobserved heterogeneity, the estimated parameter β would be biased. Following the notation used in Lee (2001), and applying to our case, we can express this by

$$E[Y_{it} \mid D_{it} = 1] - E[Y_{it} \mid D_{it} = 0] = \beta + BIAS_{it} \qquad (3.2)$$

where β is the true party effect and $BIAS_{it} = E[\varepsilon_{it} \mid D_{it} = 1] - E[\varepsilon_{it} \mid D_{it} = 0]$.

The identification of β parameter can be achieved by using regression discontinuity design, which under certain conditions will allow us to generate quasi-experimental estimates of the effects of "randomized" change in party control on subsequent economic performance. The victory or party control is a function of the vote share received by Democrat during the elections, V_{it},

$$D_{it} = D(V_{it}) = \begin{cases} 1 \text{ if } V_{it} > 0.50 \\ 0 \text{ if } V_{it} < 0.50 \end{cases} \qquad (3.3)$$

D_{it} is a *deterministic* function of V_{it}, and, therefore, for given V_{it} the conditional mean of the error term will not depend on D_{it} or

$$E[\varepsilon_{it}| D_{it}(V_{it}), V_{it}] = E[\varepsilon_{it}| V_{it}] \qquad (3.4)$$

In RD method, by comparing bare losers and winners we obtain

$$E[Y_{it} \mid 0.5 < V_{it} \leq 0.5 + \nu] - E[Y_{it} \mid 0.5 - \nu \leq V_{it} < 0.5] = \beta + BIAS^{*}_{it}(\nu) \qquad (3.5)$$

where $BIAS^{*}_{it}(\nu) = E[\varepsilon_{it} \mid 0.5 < V_{it} \leq 0.5 + \nu] - E[\varepsilon_{it} \mid 0.5 - \nu \leq V_{it} < 0.5]$ and ν > 0 represents the closeness of the elections. As elections get closer or ν gets smaller, the bias term goes to zero and one can obtain unbiased estimates of β [Lee (2001)].

This strategy is valid if vote shares are randomly assigned to Democrats and Republicans. It is true that candidates may have some control over votes they receive during the elections but there is an element of chance especially in close races that determines the realization of the vote share. For instance, such uncertainty may be related to voters' turnout or delivery of postal ballots. In this case, whether the Democrat wins or looses a very close election, would be determined randomly. If one compares average Democratic vote shares between states in which they narrowly win and lost elections, one can obtain credible estimates of effects of a governor's party affiliation on subsequent economic growth in these states.

The crucial implication of the RD design is that all observable and unobservable pre-determined (relative to election at *t*) characteristics that could influence growth per capita in $t + s$, are not systematically different between the winning (treatment group) and losing (control group) candidates of election at *t*. Therefore, by checking whether close winners and losers are comparable along observable dimensions, one can add some credibility to the identification strategy.

From an implementation point of view, one way is to compare outcomes in very close elections where the margin of victory is between 1 and 5 percent. This approach,

however, requires large sample sizes because in smaller samples it can result in large standard errors and lead to imprecise estimates. The estimates will be reported for different winning margins to see how the estimates change as these margins get closer and also to compare with results another approach which is described below.

The other approach to estimate the treatment effect is to specify and include the conditional mean function $E[\varepsilon_{it} \mid D_{it}(V_{it}), V_{it}] = E[\varepsilon_{it} \mid V_{it}] = k(V)$ as a control function [Heckman and Robb (1985), van der Klaauw (2002)]:

$$Y_{it} = \alpha + \beta D_{it} + k(V) + u_{it} \qquad (3.6)$$

For given V_{it}, the conditional mean of the error term will not depend on D_{it} since V (vote share) is a systematic determinant of treatment status D. Thus, by entering the correct specification of control function in (6), β can be estimated consistently.

In the literature, typically the control function is specified as a higher order polynomial [Angrist and Lavy (1999), Lee et al. (2004), DiNardo and Lee (2004), Lee (2005), van der Klaauw(2002), Black, Galdo, Smith (2005), Lemeiuex and Milligan (2004)]. However, it is impossible to test the assumption whether control function will capture any correlation between treatment variable and unobservables affecting outcomes [Black, Galdo, Smith (2005)]. Moreover, misspecified control function is likely to produce inconsistent estimates [van der Klaauw(2002)]. That is the reason why the empirical literature considers a wide range of alternative polynomial specifications for $k(V)$. Angrist and Lavy (1999) use linear and quadratic controls for selection variable. Lee et al. (2004), DiNardo and Lee (2004), Lee (2005) assume that all control functions

are fourth order polynomials interacted with the treatment variable. DiNardo and Lee (2004) also report results for lower order polynomials. Lemeiuex and Milligan (2004) show linear, quadratic, and cubic functions, as well as linear and quadratic splines (separate regressions on both sides of the discontinuity). van der Klaauw (2002) and Black, Galdo and Smith (2005) select the order of the polynomial is selected with cross-validation method, which is used as a tool to measure and compare the predictive ability of given models.

In this study, the estimation will follow the more common approach in the literature, and report regressions which include various forms of control function, polynomials up to the fourth order.

First, the threshold value is subtracted from each Democratic vote share value.

$$\overline{V}_{it} = V_{it} - 0.5 \qquad (3.7)$$

This is done in order to set the intercept equal to the cutoff/threshold value of the vote share.

Second, one should specify the most general model, which estimates both main effect of vote share in the form of the polynomial of sufficiently high order and interactions of the polynomial with our treatment variable (Democratic governor dummy). The interactions are used to account for differences in slopes between lines of the two groups. The interactions are used in Lee et al. (2004), DiNardo and Lee (2004), Lee (2005). Trochim (1984) recommends the multiple criterion approach to select the control function by series of steps until likely function is overfitted by several terms. For

example, the first step may fit the linear term of the assignment variable $\overline{V}$, proceeding with adding the first-order interaction of $\overline{V}$ and D. In subsequent steps, higher-order terms can be added one-by-one until the researcher is fairly confident that model is overspecified. Model selection criteria mentioned include: R-squares, significance of added coefficients and minimum residual mean squares. Several can be used to make a best judgment in the selection process. In addition, we also use cross-validation method described below because van der Klaauw (2002) and Black, Galdo and Smith (2005) mentioned that the order the polynomial is selected by the cross-validation model selection method.

The method was introduced by Allen (1974) in context of regressions[42]. Let $M = \{M_\lambda, \lambda \in \Lambda\}$ be a collection of candidate models from which one will select a model for the observed data. λ is the model index belonging to a set Λ which may be finite, countable or uncountable. Let $f(\cdot)$ be a function representing the right hand side of the equation (6). The leave-one-out cross-validation selects a model as follows. Suppose *n* is number of observations in the sample. The method splits the whole data into *n* sub-samples so y^{-j} is the vector with *j*th observation removed from the original outcome vector *y*. Let $\hat{f}_\lambda^{-j}$ be fitted values of the model M_λ on *n-1* observations. The cross-validation estimate of the prediction error is

$$CV(\lambda) = \frac{1}{n}\sum_{j=1}^{n}(y_j - \hat{f}_\lambda^{-j})^2 \qquad (3.8)$$

[42] More details on pros and cons of using cross-validation method in linear model selection see Shao (1993). The introduction to the topic can be found in Gentle (2002), p.74-76.

The cross-validation model selection criteria is λ that minimizes (8). To compute *CV* scores, one needs to fit model M_{λ} n times, once for each delete-one data y^{-j}.

However, any of statistical criteria can not guarantee that optimum function will be selected. Therefore, one needs to rely on statistical indicators and analytic judgment to select a model that is efficient and minimizes the chance of bias.

Thus, the suggested approach is to check steps up to the fourth order polynomial, which is commonly used in the recent literature. The final step will look like the following equation:

$$Y_{it} = \alpha + \beta D_{it} + \gamma_1 \overline{V}_{it} + ... + \gamma_4 \overline{V}_{it}^{\,4} + \gamma_5 D_{it} \overline{V}_{it} + ... + \gamma_8 D_{it} \overline{V}_{it}^{\,4} + \varepsilon_{it} \qquad (3.9)$$

α is both the Y-intercept for control (comparison) group regression line and the predicted Y-value for the control group at the election threshold. Therefore, β can be interpreted as the main treatment effect, which represents the difference between the two group regression lines at the intercept and at the threshold value. The regression coefficient β associated with the Democratic governor dummy *D* is the RD method's estimate of the effect of Democratic governorship on economic growth.

3.3.2 Controls

The state fixed effects are typically employed in the literature on grounds of taking into account unobservable characteristics relevant for economic growth. However, the use of state fixed effects results in the estimates explaining within states variation at the expense of losing the variation between states. Such approach is still very useful in the

specifications where there is a greater chance of omitting relevant variables and obtaining biased coefficients. But in the valid RD design, there is no need to include additional covariates except for control function in equation (8). Thus, in RD method our preferred specifications are without fixed effects, although we use them as robustness checks for results.

In practice, it may still be useful to include control variables since the randomization may not be perfect. In addition, checking the distribution of the control variables around discontinuity point will provide a validity check of how well the randomization worked.

It appears that population characteristics are commonly used both in economic growth literature[43] as well as literature that studies partisan differences in fiscal policies[44]. These variables typically include: population growth, percentage of youth and elderly population, percentage of black population, percentage of urban population, and college education attainment. In addition, one of the major interests of the growth literature is to study convergence among economies in the sense that poor economies tend to grow faster than rich ones [Barro and Sala-i-Martin (1992), Mankiw, Romer and Weil (1992), Durlauf and Quah (1999), Islam (1995), Lee et al. (1998)]. In case of the panel data estimation, Durlauf and Quah (1999) and Islam (1995) report the growth equation where the lagged log of income per capita level is used on the right hand side of the equation. If the growth rate is used as a dependent variable, the speed of convergence can be calculated as $\lambda = -\ln(\theta + 1)$, where θ is the estimated coefficient of the lagged log income per capita term.

[43] See survey in Durlauf and Quah (1999)
[44] See survey in Besley and Case (2003)

3.4 Data

The panel data is constructed for U.S. gubernatorial elections and state income data for the period 1928-1991. State election data comes from ICPSR, 1995, Candidate Name and Constituency Totals, 1788-1990 (ICSPR Study No. 2). The data was processed to keep elections where two top candidates are either Democrat or Republican. There are overall 951 of such elections in the sample. For any year between consecutive elections, observations are used for candidates and their received vote shares during previous elections held at *t*. During the election year, the data records received vote shares and party affiliation of the elected governor in this year. The elections are typically held in November and a governor assumes responsibility in the beginning of the following year. Therefore, regressions use lagged values of the Democratic governor dummy. Suppose the election takes place in 2000 then the relevant observations for income changes for newly elected governor will be 2001 through 2004. In 2004, new elections will be held in November and the name of the new governor is recorded in the data. However, the relevant observations for economic outcomes in 2004 will be the observations corresponding to the governor whose name is recorded in the data in 2003. Lagged values are used also for all controls and polynomial in vote share variable.

Per capita personal income data by states are taken from the Bureau of Economic Analysis available for 1929 -present. Income numbers are deflated by the US Consumer Price Index.

Data for population variables was obtained from the U.S. Census Bureau publications. The data is available for every 10 years of the population census so it was

interpolated for intervening years. However, education data is only available by decades starting from 1940, in the Decennial Census of Population database. Main regressions do not include education variable but it is included when distributions of the covariates around discontinuity are checked. The variable represents the percent of the total population 25 years and over with a Bachelor's degree or higher.

Table 3.1 Summary Statistics

Variables	Number of Observations	Mean	SD
Democrat	2839	0.575	0.494
Vote Share of Democratic Candidate	2839	0.532	0.118
Per Capita Personal Income (PCPI)	2805	14407	6464
Log PCPI	2805	9.45	0.535
PCPI Growth (change in logs)	2732	0.022	0.069
Population Growth	2720	0.012	0.012
Proportion of young, 0-15	2793	0.276	0.055
Proportion of old, over 65	2793	0.091	0.026
Proportion of black population	2793	0.075	0.087
Proportion of urban population	2793	0.6025	0.234
Proportion of population over 25 with a Bachelor's Degree or Higher	2361	0.0956	0.049

3.5 Results

3.5.1 Simple Comparison of Growth Rates under Democrats and Republicans

This subsection presents results of an OLS "naive" comparison of the performance measures under Democrats and Republicans, which does not take into account the potential endogeneity in party control variable.

Table 3.2 presents the estimates where the economic growth measures are regressed on Democratic governors' dummy like in equation (1). Column (1) does not include any

control variables. Column (2) includes only demographic and lagged income controls, while column (3) controls separately for state and time fixed effects and column (4) includes all control variables. Column (1) indicates that under Democrats per capita income growth rates are about 0.8 percent higher than under Republicans. The difference in the effects of democratic governorship on growth disappears, however, when fixed effects are used in (3) and (4). R-squared statistics indicates that fixed effects account for about 60 percent of the sample variation. As mentioned before, one cannot exclude the possibility that when using within variation under fixed effects specifications, tests may not have enough statistical power to reject the null of no relationship between democratic governorship and economic growth measures. Therefore, there is a need to look at the regression discontinuity method results to see if partisan differences matter for growth when endogeneity is addressed under alternative method.

Table 3.2 OLS Estimates of the effect of democratic governor on Income per Capita Growth

	1	2	3	4
Democrat $_{t-1}$	0.0083	0.0072	0.0021	0.001
	(0.003)***	(0.0031)**	(0.0015)	(0.0019)
Log (Income) $_{t-1}$		-0.022		-0.135
		(0.0058)***		(0.025)***
State Demographics	No	Yes	No	Yes
State and Time FE	No	No	Yes	Yes
Constant	0.0174	0.23	-0.1147	2.02
	(0.0017)***	(0.049)***	(0.009)***	(0.35)***
Observations	2732	2732	2732	2720
R-squared	0.0036	0.0175	0.63	0.65

Standard errors clustered at the state level are in parentheses. Each column is a separate regression.
* Significant at the 10 percent level, ** Significant at the 5 percent level , *** Significant at the 1 percent level

3.5.2 RD estimates – close elections

Table 3.3 and 3.4 show the RD results by looking at close elections sub samples. In column 1, elections are very narrow so that the candidate wins with a margin of less than 1 percent. In next columns the winning margin is gradually increased and results are reported for winning margins less than 3, 6, 10 and 20 percent in Table 3.3 while in Table 3.4 control variables are added.

One can see in Table 3 and in columns 1-4 of Table 3.4 (without fixed effects controls) that coefficients of the democratic governor dummy tend to become more negative as we look at closer elections while the OLS "naive" comparison coefficient was positive and significant in Table 3.2, column 1. This suggests that the RD method is getting rid of an upward bias, which was present in OLS estimates in Table 3.2.

Table 3.3. RD Estimates of the Effect of Democratic governor on Income per Capita Growth: Close Elections

	winning margin < 1%	winning margin < 3%	winning margin < 6%	winning margin < 10%	winning margin < 20%
	1	2	3	4	5
Democrat $_{t-1}$	-.022	-.008	-0.004	-0.0007	0.0039
	(0.018)	(0.01)	(0.006)	(0.0043)	(0.0034)
Constant	0.047	0.028	0.025	0.022	0.020
	(0.01)***	(0.006)***	(0.004)***	(0.003)***	(0.002)***
Observations	142	402	743	1212	1989
R-squared	0.021	0.038	0.0008	0	0.0009
# of elections	52	137	261	415	674

Standard errors clustered at the state level are in parentheses. Each column is a separate regression.
* Significant at the 10 percent level, ** Significant at the 5 percent level , *** Significant at the 1 percent level

Table 3.4. RD Estimates of the Effect of Democratic governor on PCPI growth: Close Elections. Adding Controls

	winning margin < 1%	winning margin < 3%	winning margin < 6%	winning margin <10%	winning margin < 1%	winning margin < 3%	Winning margin < 6%	winning margin <10%
	1	2	3	4	5	6	7	8
Democrat $_{t-1}$	-0.04	-0.012	-0.006	-0.0014	0.037	0.001	-0.002	0.0019
	(0.02)**	(0.0096)	(0.0049)	(0.0042)	(0.022)	(0.005)	(0.004)	(0.0025)
Log(Income) $_{t-1}$	-0.053	-0.017	-0.021	-0.023	-0.63	-0.18	-0.17	-0.14
	(0.023)**	(0.014)	(0.011)*	(0.01)**	(0.137)***	(0.060)***	(0.037)***	(0.027)***
State Demogr.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State and Time FE	No	No	No	No	Yes	Yes	Yes	Yes
Constant	0.661	0.217	0.229	0.238	4.82	1.74	1.72	1.30
	(0.188)***	(0.130)	(0.092)**	(0.078)***	(1.08)***	(0.56)***	(0.38)***	(0.21)***
Observations	140	397	736	1201	140	397	736	1201
R-squared	0.20	0.03	0.02	0.02	0.95	0.75	0.70	0.65

Standard errors clustered at the state level are in parentheses. Each column is a separate regression.
* Significant at the 10 percent level, ** Significant at the 5 percent level , *** Significant at the 1 percent level

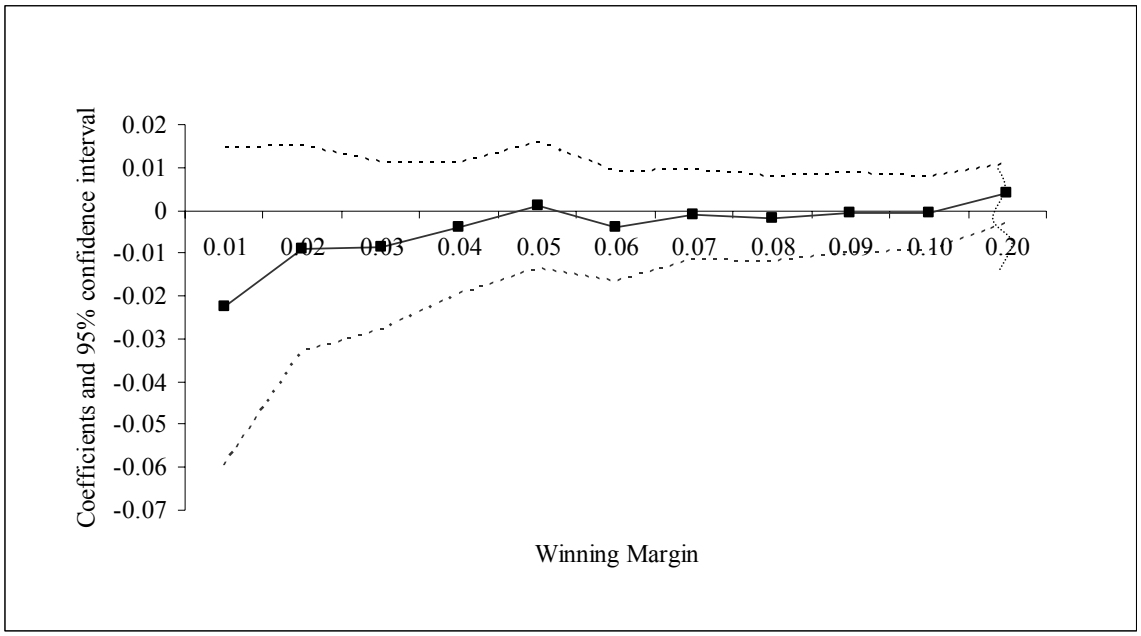
The pattern of changing coefficients towards negative values, as we restrict sample to closer elections, can also be seen in Figure 1, which is constructed based on regressions of Table 3.3. The graph shows how coefficients and 95% confidence interval change as a function of winning margins. One can notice that the confidence interval and standard errors get larger as a sample and closeness of elections get smaller. In addition, Figure 2 shows how standard errors (SE's) of estimated coefficients change relative to $1/\sqrt{N-2}$, where N-2 is the degrees of freedom[45]. In close elections, the slopes of SE's curves are noticeably steeper than the slope of $1/\sqrt{N-2}$ curve. The standard deviation in Democrat dummy does not change much with N. Therefore, increases in SE's take place because unexplained variation in Y's is increasing as sample gets smaller and elections get closer.

[45] Standard errors of estimated β in the simple two-variable equation (1) can be expressed as

$$SE(\beta) = \frac{1}{\sqrt{N-2}} \sqrt{\sum \left(Y_{it} - \hat{Y}_{it}\right)^2} \Big/ \sqrt{\sum \left(D_{it} - \overline{D}_{it}\right)^2} = \frac{1}{\sqrt{N-2}} \frac{\sqrt{RSS}}{SD(D_{it})}$$

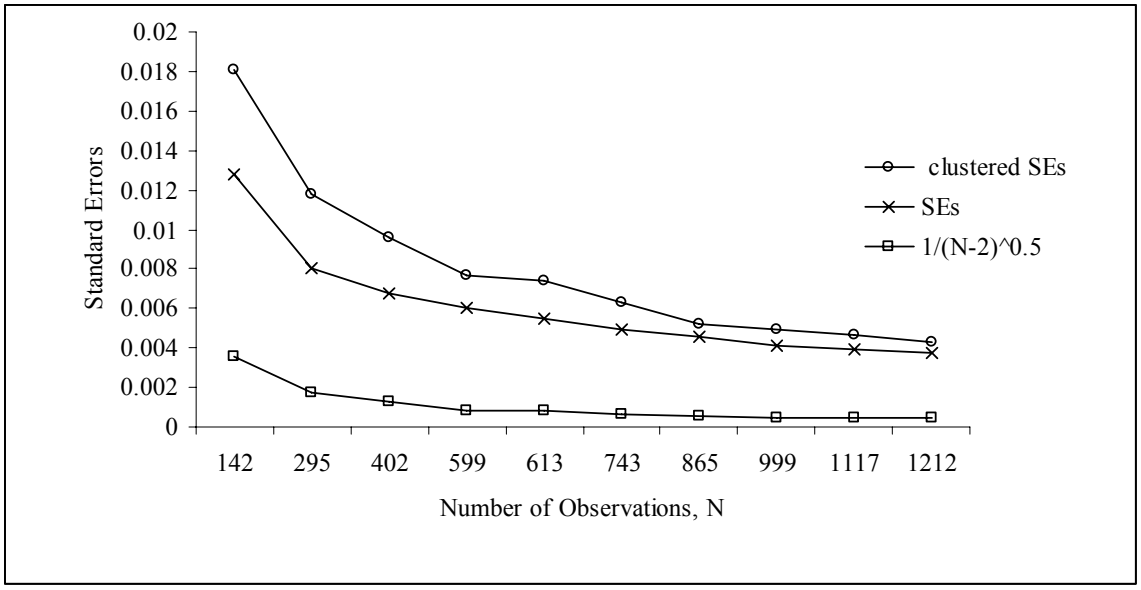
This casts some doubt on the reliability of the RD's close elections approach in our study because in close elections we are left with too few observations.

Figure 3.1 RD Estimates: Close Elections Margins and Changes in Estimates of the Effect of Democratic Governor on Income per Capita Growth



Graph is constructed based on results of Table 3.3. When Winning Margin is between 0.10 and 0.20, X-axis scale is reduced to include the estimate in the sample where Winning Margin is 0.20.

Figure 3.2 Standard Errors (SEs) and Sample Sizes (Table 3.3 results)



All reported democratic dummy coefficients are not statistically significant except in column 1, Table 3.4. We do not think that this estimate is reliable because the sample size is too small and the reported difference of about 4 percent in growth rates is unrealistically high. The reported difference of about 1 percent in column 2 of Table 3.4 seems more reasonable despite statistical insignificance due to relatively large standard error value[46]. As state and year fixed effects are added in columns 5-8 of Table 3.4, all reported coefficients become insignificant.

Based on significant estimates of the coefficients of lagged log income in Table 3.4, the income convergence across states is supported by the data and the implied[47] convergence rate ranges from 1.7 percent to 19.8 percent in columns 2 and 8 respectively. Durlauf and Quah (1999) report that estimates in the literature from panel-data analyses have varied significantly from 3.8 percent to 30 percent. Differences are due to finite sample biases, different estimation methods and datasets.

To sum RD's close elections approach in Tables 3 and 4, results for democratic governors' effect on income growth show that the bias presented in OLS estimates is being corrected by looking at close elections samples. There is some tendency of the estimates towards negative 1 percent differences in growth rates but coefficients are not statistically significant. These results show that the RD estimates are subject to a large degree of sampling variability. Also, there may not be enough observations to estimate more precisely coefficients by restricting the sample around discontinuity point.

[46] When standard errors are not clustered, this coefficient is significant at 10 percent level.

[47] Using formula $\lambda = -\ln(\theta + 1)$, where θ is the estimated coefficient of the lagged log income per capita term.

3.5.3 RD estimates – Control Function Polynomial

This section presents the evidence obtained from the RD estimation using the control function approach with full sample. As in the literature mentioned in section 3.1, the assumption for control function is that it belongs to the class of polynomial functions.

Using the stepwise procedure explained in section 3.1, the results are presented step-by-step in Table 3.5 to select the optimum specification for control function. Thus, eight columns in Table 3.5 present eight different specifications of the control function. The selection of the model is based on several statistical criteria: R squared, residual mean square values and statistical significance of added terms measured by t statistics or by F test of whether the additional term should be added to a model. In particular, one should pay attention to the greater statistical significance of added polynomial terms, sizable upward jumps and subsequent leveling-off in R squares, downward jumps and leveling-off in residual mean squares.

Cross-validation method in model selection is also implemented (Shao 1993, Gentle 2002, p.74-76). Leave-one-out algorithm is used. Each model (step) is fitted *n* times leaving out one observation from estimation. Root mean square error (RMSE) is recorded each time. The sum of all estimated RMSE's for one specific model would represent the model selection criteria reported in Table 3.5. Lower value of the criteria would indicate better predictive ability.

The statistical indicators' are in favor of models in step 5 and 7. However, the preferred model is in step 5. Note that coefficient sizes prior to step 5 in Table 3.5 change

more dramatically, from -0.17 (step 1) to -0.56 (step 2) then to -0.24 (step 3) and to -0.06 (step4). After step 5, coefficients display less variability, they change very little in step 6, from -1.1 to -1.0, and then to -0.5 in step 7 and -0.7 in step 8. Recall, however, that estimates are prone to bias when the control function is "underspecified" and ideally should not be biased if the control function is "overspecified".

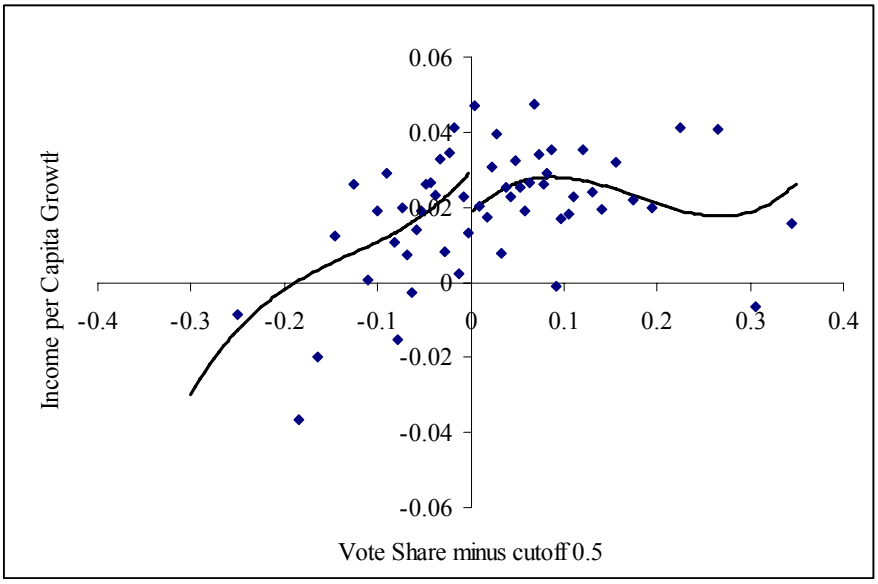
Table 3.5 Income Growth and RD Estimates: Using Control Function Polynomial in Vote Shares, 8 step analysis.

	1	2	3	4	5	6	7	8
Democrat $_{t-1}$	-0.0017	-0.0056	-0.0024	-0.0006	-0.011	-0.010	-0.005	-0.007
	(0.0042)	(0.0045)	(0.0050)	(0.0070)	(0.0083)	(0.0089)	(0.01)	(0.011)
Vote Share $_{t-1}$	0.062	0.16	0.225	0.176	0.291	0.206	0.282	0.515
	(0.02)***	(0.05)***	(0.055)***	(0.119)	(0.126)**	(0.258)	(0.261)	(0.443)
Vote Share^2 $_{t-1}$			0.293	0.063	1.384	0.409	1.912	6.504
			(0.135)**	(0.452)	(0.661)**	(2.504)	(2.627)	(7.281)
Vote Share^3 $_{t-1}$					3.585	0.940	10.40	39.29
					(1.305)***	(6.230)	(7.774)	(41.92)
Vote Share ^{4} $_{t-1}$							18.08	73.29
							(8.22)**	(76.15)
Democrat X		-0.122	-0.293	-0.252	-0.039	0.054	-0.384	-0.605
Vote Share $_{t-1}$		(0.05)**	(0.098)***	(0.129)**	(0.148)	(0.292)	(0.374)	(0.479)
Democrat X				0.250	-3.292	-2.372	0.378	-4.355
Vote Share^2 $_{t-1}$				(0.480)	(1.407)**	(2.634)	(2.962)	(7.892)
Democrat X						2.732	-22.60	-50.97
Vote Share^3 $_{t-1}$						(6.393)	(13.84)	(41.82)
Democrat X								-55.81
Vote Share^4 $_{t-1}$								(77.47)
Observations	2732	2732	2732	2732	2732	2732	2732	2732
R-squared	0.0094	0.0127	0.0147	0.0148	0.0196	0.0197	0.0217	0.0220
Residual Mean Squares	.0047228	.0046680	.0046601	.0046613	.0046404	.0046417	.00463	.00463
p value of F test		0.0026	0.0179	0.5804	0.0003	0.6247	0.0168	0.1422
Cross-validation criteria	.0684241	.0683227	.06826499	.06827368	.0681206	.06813011	.06807	.06808

Standard errors clustered at the state level are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. F test of whether the additional term should be added to a model.

In addition, Figure 3 shows a graphical analysis of the polynomial fit in step 5. To obtain meaningful scattered plots, all values for income growth are averaged within certain intervals of vote shares[48]. The plot provides a visual representation about the estimated discontinuity at the cutoff point of vote shares.

Figure 3.3 Effect of Democratic Governor on Income per Capita Growth. Step 5 – 3rd order polynomial.



Each scatter point represents the average growth within certain interval of Democrats' vote share. Intervals equal to 0.5% when vote share is between 40% and 60% (between -0.1 to 0.1 on the graph). Solid lines represent fitted values from polynomial regressions in vote share and interactions of democratic governor dummy with polynomial in full sample from Table 3.5, column 5.

Model in step 5 of Table 3.5 shows that democratic governors' performance is worse by 1.1 percent but the coefficient is not significant. When adding population and income convergence controls in Table 3.6 using step 5 and step 7 models, the estimates in Table 3.6 are very similar to estimates in columns 5 and 7 of Table 3.5. Implied convergence rate is about 2.2 percent in columns 1-2, and 14 percent in columns 3-4 where fixed

[48] The intervals are chosen such that they contain at least 30 observations of growth rates at tails of the distribution of vote shares. Then intervals are gradually reduced such that they are equal to 0.5 percent where vote shares are in between 40 percent and 60 percent. Then fitted polynomial line is drawn from regression 5 in Table 3.5.

effects are added. In fixed effects models in 3 and 4, the difference between Democrats and Republicans becomes smaller in terms of sizes of the coefficients.

Table 3.6 Income Growth and RD Estimates: Using Control Function Polynomial in Vote Shares, Adding Controls.

	Step 5	Step 7	Step5	Step 7
	1	2	3	4
Democrat $_{t-1}$	-0.013	-0.006	-0.0001	-0.001
	(0.008)	(0.009)	(0.004)	(0.005)
Vote Share $_{t-1}$	0.324	0.381	0.021	0.287
	(0.128)**	(0.257)	(0.060)	(0.144)*
Vote Share^2 $_{t-1}$	1.442	2.822	0.193	3.508
	(0.703)**	(2.600)	(0.320)	(1.427)**
Vote Share^3 $_{t-1}$	3.610	13.66	0.811	12.27
	(1.410)**	(7.55)*	(0.471)*	(3.71***
Vote Share ^{4} $_{t-1}$		21.08		8.382
		(7.596)***		(3.532)**
Democrat X Vote Share $_{t-1}$	-0.060	-0.522	0.031	-0.411
	(0.151)	(0.356)	(0.074)	(0.163)**
Democrat X Vote Share^2 $_{t-1}$	-3.453	-0.007	-0.700	-1.994
	(1.499)**	(2.913)	(0.540)	(1.731)
Democrat X Vote Share^3 $_{t-1}$		-28.31		-18.89
		(12.81)**		(5.52)**
Log (Income) $_{t-1}$	-0.022	-0.022	-0.134	-0.137
	(0.004)***	(0.004)***	(0.019)***	(0.020)***
State and Year Fixed Effects	No	No	Yes	Yes
State Demographics	Yes	Yes	Yes	Yes
Const	0.224	0.224	1.329	1.346
	(0.037)***	(0.041)***	(0.191)***	(0.196)***
Observations	2693	2693	2693	2693
R-squared	0.0320	0.0346	0.65	0.65

Standard errors clustered at the state level are in parentheses. Each column is a separate regression.
* significant at 10%; ** significant at 5%; *** significant at 1%

Overall, the evidence from RD estimation is suggestive about the slightly worse performance of democratic governors but it is not strong in terms of statistical significance across all specifications. The polynomial fit results, using the full sample, are also very similar to close elections results in Tables 3 and 4. The RD method does its

job in terms of reducing the upward bias resulting from simple OLS comparisons between Democrats and Republicans. Next section will look at the distribution of predetermined characteristics around the discontinuity point.

3.5.4 Checking Empirical Strategy

The empirical test within the RD framework importantly relies on the assumption of random assignment of the winner in close elections [Lee (2004)]. One of the implications from the analysis: when comparing closer and closer elections, all predetermined characteristics of Republican and Democratic states become more and more similar. Consider this in Table 3.7. In rows, the averages of variables, which should be determined before time of elections, are first calculated for the whole sample. Then the sample averages are calculated as elections get closer. Like in Lee (2003), we finally do the parametric polynomial fit.[49] The coefficient in column 13 can be interpreted as a predicted difference between Republicans and Democrats with respect to predetermined characteristic at 50 percent of vote share.

First consider geographical locations. States are classified into 4 regions according to US Census classification.[50] Averaging over entire sample of all elections, Democrats, on average, win more elections in the South while losing more in the Northeast and Midwest (columns 1-3).

[49] The dependent variable is a predetermined characteristic which is regressed on Democrat dummy, the 4th order polynomial of democratic vote share and interactions of the polynomial with democratic dummy.

[50] Northeast: ME, NH, VT, MA, RI, CT, NY, NJ, PA. South: DE, MD, VA, WV, NC, SC, GA, FL, KY, TN, AL, MS, AR, LA, OK, TX. Midwest: MN, IA, MO, ND, SD, NE, KS, OH, IN, IL, MI, WI. West: MT, ID, WY, CO, NM, AZ, UT, NV, WA, OR, CA.

Table 3.7. Difference in Regional and Population Characteristics during elections. Democratic candidates, Winners vs. Losers

	All elections			margin < 10%			margin < 5%			margin < 2%			Polynomial fit
	Win	Lose	Diff.	Win	Lose	Diff.	Win	Lose	Diff.	Win	Lose	Diff.	Dem. Dummy coefficient
	1	2	3	4	5	6	7	8	9	10	11	12	13
Northeast	0.19	0.29	-0.10 (0.027) ***	0.22	0.26	-0.04 (0.04)	0.25	0.30	-0.05 (0.06)	0.36	0.29	0.07 (0.09)	-0.08 (0.08)
South	0.36	0.11	0.25 (0.027)***	0.18	0.15	0.03 (0.04)	0.11	0.16	-0.05 (0.05)	0.06	0.15	-0.09 (0.06)	-0.05 (0.07)
Midwest	0.21	0.37	-0.16 (0.029)***	0.32	0.34	-0.02 (0.05)	0.34	0.32	0.03 (0.06)	0.34	0.29	0.05 (0.09)	0.06 (0.08)
West	0.244	0.237	0.017 (0.028)	0.28	0.25	0.03 (0.04)	0.30	0.23	0.07 (0.06)	0.24	0.27	-0.03 (0.09)	0.07 (0.08)
#of elections	951			415			216			102			951
Population:													
$Education_{t-1}$	0.090	0.083	0.007 (0.003)**	0.088	0.086	0.001 (0.004)	0.085	0.088	-0.003 (0.007)	0.089	0.087	0.002 (0.01)	-0.005 (0.01)
# of elections	707			319			160			73			707
Old_{t-1}	0.087	0.095	-0.008 (0.002)***	0.087	0.094	-0.007 (0.002)***	0.086	0.095	-0.009 (0.003)***	0.090	0.089	0.001 (0.005)	-0.006 (0.005)
$Young_{t-1}$	0.28	0.272 4	0.0076 (0.003)**	0.282	0.273	0.0096 (0.006)	0.286	0.270	0.016 (0.008)**	0.275	0.274	0.001 (0.013)	0.017 (0.011)
$Black_{t-1}$	0.08	0.04	0.04 (0.005)***	0.049	0.047	0.002 (0.006)	0.037	0.053	-0.015 (0.008)**	0.034	0.049	-0.015 (0.009)*	-0.014 (0.013)
$Ln(Income)_{t-1}$	9.34	9.43	-0.09 (0.04)***	9.38	9.45	-0.07 (0.05)	9.33	9.49	-0.16 (0.07)**	9.40	9.43	-0.03 (0.09)	-0.12 (0.10)
# of elections	878			395			203			97			878

Standard errors are in parenthesis. "Diff." shows the difference between means. In the parametric fit, the dependent variable is regressed on democrat dummy, the 4th order polynomial of democratic vote share and interactions of the polynomial with democratic dummy. * significant at 10%; ** significant at 5%; *** significant at 1%

Next we look at the population characteristics. The differences are smaller for percentage of people with higher education as elections get closer. For percentages of an older population, the difference does not get smaller for sample of elections where margins are less than 10 percent and 5 percent but for the margin less than 2 percent it is smaller. In the polynomial fit, the difference is small and not statistically significant. For the percentage of black, the difference is smaller in closer election sample and not statistically significant in polynomial fit.

We finally test the identifying assumption by looking at the differences in pre-election log income levels. Log income averages are bigger in the sample with margin less than 5 percent and smaller in the other two sub samples. It is insignificant in the polynomial fit.

Although not perfect, overall the randomization around discontinuity point is supported by data. This lends more credibility to the RD results obtained in this study.

6 Conclusions

The analysis in this study shows that the RD design is a powerful tool to address the endogeneity in party control variable using the data from US gubernatorial elections. The RD method does its job in addressing the upward bias in OLS estimates. In OLS regressions, a naïve comparison of growth rates shows slightly better (about 1 percent) performance of Democratic governors.

However, the RD estimates suggest that under Democratic governors the growth rates may be lower by about 1 percent but the evidence is not very strong because of the low statistical significance level. Alternatively, one could suggest with greater confidence that

the relationship between Democratic governorship and economic growth is at least not positive as a simple correlation may suggest first.

Addressing the endogeneity in party control variable with RD method offers a great scope for the analyses of the relationships between election and economic outcomes. However, the main challenge would be the availability of the data. RD method requires very large sample sizes to obtain more credible results. Further analysis can examine the effects of partisan differences on fiscal policies in US states. An even more ambitious project would be to evaluate the differences in economic performance measures and policies between right and left governments in cross-country analysis.

Appendices

Chapter 1

Appendix 1.1 Table A1.1 Country Level Survey Measures and External Indexes

Country	Growth 2000-2002	Survey Measures							External Measures		
		Growth of Sales	Courts' Quality	Regulatory Burden	Corruption	State Capture	State Capture 2	Obstacles Financing	WB Reg. Quality	TI Corrupt. Index	WB Corrupt. Control
Albania	6.6	36.29	-0.13	12.41	3.31	1.61	0.48	2.07	-0.37	2.50	-0.85
Armenia	9.5	4.43	0.12	2.80	0.92	1.13	0.11	2.34	0.13	Na	-0.72
Azerbaijan	10.3	10.31	0.03	2.94	2.74	1.24	0.30	2.16	-0.82	2.00	-1.07
Belarus	4.83	35.39	0.25	7.16	1.49	1.19	0.17	2.47	-1.67	4.80	-0.78
BiH	5.17	9.53	-0.07	8.81	0.95	1.95	0.84	2.53	-0.93	Na	-0.60
Bulgaria	5.03	8.43	0.05	3.74	1.95	1.58	0.70	2.80	0.62	4.00	-0.17
Croatia	4.43	31.86	-0.09	5.88	0.64	1.55	0.33	2.18	0.19	3.80	0.23
Czech	2.87	15.94	-0.03	2.79	0.92	1.24	0.29	2.45	1.12	3.70	0.38
Estonia	5.8	37.88	0.41	4.06	0.34	1.49	0.22	1.94	1.35	5.60	0.66
FYROM	0.43	1.51	0.06	8.99	0.79	1.48	0.81	2.08	-0.10	Na	-0.73
Georgia	3.5	14.80	-0.37	11.60	2.74	1.61	0.49	2.21	-0.82	2.40	-1.03
Hungary	4.17	30.95	0.43	6.89	0.97	1.38	0.18	2.22	1.21	4.90	0.60
Kazakhstan	10.8	37.57	-0.15	9.08	2.10	1.24	0.13	2.00	-0.74	2.30	-1.05
Kyrgyzstan	3.3	13.27	-0.40	6.90	3.70	1.59	0.42	2.24	-0.46	Na	-0.84
Latvia	6.57	24.59	0.08	8.47	0.93	1.42	0.44	1.85	0.86	3.70	0.09
Lithuania	5.47	20.30	-0.03	7.49	0.74	1.24	0.31	1.62	0.98	4.80	0.25
Moldova	4.53	24.63	-0.14	7.21	2.07	1.25	0.35	2.49	-0.17	2.10	-0.89
Poland	2.17	0.27	0.09	9.54	1.22	1.41	0.21	2.65	0.67	4.00	0.39
Romania	3.67	41.15	0.07	9.26	2.57	1.51	0.37	2.55	0.04	2.60	-0.34
Russia	6.07	49.39	-0.36	9.79	1.43	1.34	0.15	2.31	-0.30	2.70	-0.90
Slovakia	3.2	26.86	0.03	7.03	1.45	1.41	0.40	2.50	0.76	3.70	0.28
Slovenia	3.53	21.38	0.09	5.08	0.80	1.27	0.29	1.82	0.81	6.00	0.89
Ukraine	6.53	42.46	-0.10	11.21	2.19	1.40	0.33	2.44	-0.62	2.40	-0.96
Uzbekistan	3.73	37.67	0.43	7.23	1.45	1.22	0.34	2.45	-1.44	2.90	-1.03
Yugoslavia	4.83	12.06	0.15	11.52	1.52	1.61	0.51	2.43	-0.60	Na	-0.80
Min	0.43	0.27	-0.40	2.79	0.34	1.13	0.11	1.62	-1.67	2.00	-1.07
Max	10.80	49.39	0.43	12.41	3.70	1.95	0.84	2.80	1.35	6.00	0.89
St. Dev	2.41	14.08	0.22	2.82	0.89	0.19	0.19	0.28	0.85	1.20	0.64

Table A1.2 Comparison of within and between country standard deviations of X_{is}

	Standard Deviations of X_{is} in country s, $SD(X_{is})$				
Country	SD(Courts' Quality)	SD(Regulatory Burden)	SD(Corruption)	SD(State Capture)	SD (Obstacles to Financing)
Albania	0.74	11.56	3.62	1.03	1.10
Armenia	0.97	5.34	2.89	0.53	1.18
Azerbaijan	1.37	5.58	4.44	0.79	1.06
Belarus	0.84	11.16	4.01	0.73	1.17
BiH	0.86	11.58	2.12	1.22	1.12
Bulgaria	0.91	7.65	3.10	1.00	1.18
Croatia	0.71	9.33	1.78	0.98	1.24
Czech	0.77	5.75	2.13	0.62	1.16
Estonia	1.04	8.42	1.02	0.88	1.05
FYROM	1.12	12.91	1.07	0.94	1.08
Georgia	1.00	16.12	3.72	1.11	1.03
Hungary	1.06	10.11	2.15	0.80	1.20
Kazakhstan	0.91	13.18	3.89	0.84	1.07
Kyrgyzstan	0.73	11.57	5.85	1.21	1.11
Latvia	0.81	11.29	1.99	1.00	1.03
Lithuania	0.77	10.65	1.83	0.77	0.96
Moldova	0.64	6.94	3.70	0.79	1.14
Poland	0.72	13.32	2.61	0.97	1.17
Romania	0.87	10.72	4.19	1.17	1.20
Russia	0.80	14.45	2.50	0.90	1.14
Slovakia	0.71	9.09	2.57	1.03	1.21
Slovenia	0.76	9.50	3.56	0.74	1.00
Ukraine	0.89	14.69	4.18	1.11	1.19
Uzbekistan	1.19	11.50	3.04	0.74	1.19
Yugoslavia	1.03	13.70	3.24	1.08	1.16
Average of $SD(X_{is})$	0.89	10.64	3.01	0.92	1.13
St. Dev ($\overline{X}_s$)	0.22	2.82	0.89	0.19	0.28

Appendix 1.2

Measuring Institutions – the Quality of Courts

The *courts' quality* variable measures the overall quality of the court system in solving business disputes. The respondents had to evaluate on a scale of 1("never") and 6("always") how often the court system is fair, uncorrupted, quick, affordable and able to enforce its decisions. These attributes seem all to contribute to firms' perception of the effectiveness of courts but each of them also captures a unique aspect of an underlying quality. The first two categories, "fair" and "uncorrupted", are highly related to each other. Corrupted court cannot be fair and vice versa. Furthermore, these two seem to be more important for court effectiveness. If the probability of the court being unfair or corrupted were high then entrepreneurs would consider the system to be less effective and less likely to use it in business disputes even if courts are affordable, quick and able to enforce its decisions. The latter two attributes would be also important for measuring the quality of the system since the "fair" decision must be enforceable in timely manner if system is indeed effective.

One approach to analyzing subjective perceptions, to gain insights from survey responses, is through factor analysis which is a statistical technique applied to a single set of variables to discover which groups of variables in the set form coherent subsets that are relatively independent of one another. Variables that are correlated with one another and are also largely independent of other subsets of variables are combined into factors. Factors are thought to be representative of the underlying processes that have created the correlations or covariances among variables. This framework fits nicely into the discussion of how the court quality is determined by the survey measures.

The factor analysis of the dataset (Table A1.3) supports the intuition developed in the previous paragraph. The data reveals only one unobservable factor, the underlying courts' quality, which accounts for variation in observed characteristics provided by survey. Factor loadings show the specific association between factors and original variables. As expected, factor loadings for fair and uncorrupted courts are highest indicating the first-degree significance of those for courts' quality. The remaining three observable characteristics are also important as indicated by their factor loadings. Since the results of the factor analysis are consistent with the intuition developed, variables can be transformed into linear combinations of an underlying unobserved *Quality of Courts* variable.

Table A1.3 Factor Analysis for Courts Quality

Component	Eigenvalue	Difference	Proportion	Cumulative Proportion
1	2.39890	2.25390	1.1154	1.1154
2	0.14500	0.24193	0.0674	1.1828
3	-0.09693	0.03354	-0.0451	1.1377
4	-0.13048	0.03525	-0.0607	1.0771
5	-0.16572	.	-0.0771	1.0000
How often do you associate the following descriptions with the court system in resolving business disputes? 1=never, 6=always				Eigenvector of 1st component (weighting)
Honest/Uncorrupted				0.79127
Fair and impartial				0.78224
Able to enforce its decisions				0.63183
Quick				0.62304
Affordable				0.61115

Appendix 1.3

Survey Questions and correlations for Business Regulation Questions

Observations with "don't know" answers were recoded as missing observations. However, omitting "don't know" answers may create a selection bias in corruption variables as those firms that are reluctant to confess to corrupt behaviors are more likely to respond with a "don't know". If these firms are relatively poor or good performers and they are heavily engaged in corrupt activities, the corruption coefficients (γ) would be biased. Moreover, averaging responses may understate or overstate the country's level of corruption. These concerns have to be kept in mind when interpreting the results.

Business Regulations

Q.25 What was the average and the longest number of days in 2001 that it took from the time your goods arrived in their point of entry (e.g. port, airport) until the time you could claim them from customs?

Q.46a. Information on the laws & regulations affecting my firm is easy to obtain

Q.46b. Interpretations of the laws and regulations affecting my firm are consistent & predictable
Strongly disagree=1 → Strongly agree=6

Q.49 How likely do you think it is that an unforeseen change in laws or regulations will occur in 2003 and have a significant impact on your business?
Extremely unlikely =1 → Extremely likely = 6

Q.50 What per cent of senior management's time in 2001 was spent in dealing with public officials about the application and interpretation of laws and regulations and to get or to maintain access to public services? ...%

Q.51 How often is the following statement true? "If a government agent acts against the rules I can usually go to another official or to his superior and get the correct treatment without recourse to unofficial payments/gifts."
Never =1 → Always=7

Rule of Law

Q.41 How often do you associate the following descriptions with the court system in resolving business disputes? a)Fair and impartial; b) Honest/uncorrupted; c) Quick; d) Affordable; e) Able to enforce its decisions
Q.42 To what degree do you agree with this statement? "I am confident that the legal system will uphold my contract and property rights in business disputes".
Q.43e How many cases in civil or commercial arbitration courts have involved your enterprise either as a plaintiff or defendant since January 2000?
Q.44 Do you pay for security (equipment, personnel, etc. excluding "protection payments") or protection payments? If yes, what per cent of your total annual sales do you pay?

Influence and Lobbying

Q.51a Is your firm a member of a business association or chamber of commerce?

Q.51b What services do you receive from the association or associations to which you belong, and what is the value of these services to your firm? "Critical value to your firm" should be placed on services that significantly reduce the costs of your firm or significantly increase the sales of your firm.

Q.52 Did your firm seek to influence the content of laws or regulations in 2001 affecting it? Yes/No

Q.53 How much influence do you think the following groups actually had on recently enacted national laws and regulations that have a substantial impact on your business?
Your firm, Your domestic competitors, Other domestic firms, Foreign firms, A business association to which you belong, Other business associations, Dominant firms or conglomerates in key sectors of the economy, Labour unions, Organised crime, Regional or local government, Military, Individuals or firms with close, personal ties to political leaders, International development agencies or foreign governments
No impact =0 → Decisive Influence =4

Corruption and State Capture

Q.54a It is common for firms in my line of business to have to pay some irregular "additional payments/gifts" to get things done " with regard to customs, taxes, licenses, regulations, services etc Never→Always
Q.54b Firms in my line of business usually know in advance about how much this 'additional payment/gifts' is. Never→Always
Q.55 On average, what percent of total annual sales do firm's like yours typically pay in unofficial payments/gifts to public officials? ...%
Q.56 Thinking now of unofficial payments/gifts that a firm like yours would make in a given year, could you please tell me how often would they make payments/gifts for the following purposes: (a)To get connected to and maintain public services (electricity and telephone); (b) To obtain business licenses and permits; c) To obtain government contracts ; d) To deal with occupational health and safety inspections; e) To deal with fire and building inspections; f) To deal with environmental inspections; g) To deal with taxes and tax collection; h) To deal with customs/imports; i) To deal with courts; j) To influence the content of new legislation rules decrees etc.
Never =1 → Always =6
Q.57 When firms in your industry do business with the government, how much of the contract value would be typically paid in additional or unofficial payments/gifts to secure the contract?
……%
Q.58 Recognising the difficulties that many firms face in fully complying with taxes and regulations, what per cent of total annual sales would you estimate the typical firm in your area of business reports for tax purposes? …%
Q.59 It is often said that firms make unofficial payments/gifts, private payments or other benefits to public officials to gain advantages in the drafting of laws, decrees, regulations, and other binding government decisions. To what extent have the following practices had a direct impact on your business? a) Private payments/gifts or other benefits to Parliamentarians to affect their votes; b) Private payments/gifts or other benefits to Government officials to affect the content of government decrees; c) Private payments/gifts or other benefits to judges to affect the decisions of criminal court cases
d) Private payments/gifts or other benefits to judges to affect the decisions in commercial cases; e) Private payments/gifts or other benefits to central bank officials to affect central bank policies and decision; f) Illegal contributions to political parties and/or election campaigns to affect the decisions of elected officials
No impact=0 → Decisive Impact=4

Financial System

Q.64 What proportion of your firm's working capital and new fixed investment has been financed from each of the following sources, over the past 12 months?
Q.65a Thinking of the most recent bank loan or overdraft you obtained, did the financing require collateral?
Q.65b What kind of collateral (e.g. land, buildings, machinery, your home) was required?
Q.65c What was the approximate value of the collateral required as a percent of the loan value?
Q.65d What is the loan's annual cost (i.e. rate of interest)?….%
Q.65e What is the duration of the loan in months?
Q.65f How many days did it take to agree the loan with the bank from the date of application?

Q.65h Now I would like to ask you a hypothetical question. If your firm were to fall behind in its bank repayments, which of the following would best describe how you would expect the bank to react?
Q.66 Now I would like to ask you a hypothetical question. Suppose that your firm has to obtain a loan from a commercial bank. How easy would it be for your firm to obtain a short-term working capital loan on commercial terms. And how easy would it be for your firm to obtain a longer term banking loan for new investment
Q.72b Now I would like to ask you a hypothetical question: Suppose that the incoming cash flow to your firm for the next quarter is 10% lower than you had expected. This cash flow is not permanently lost, but merely unexpectedly delayed. However, your working capital needs and level of production remain the same. Please look at this list and select at most 4 sources from which you would finance this gap. For each of the source selected, please indicate its importance.
Q.73 Does your firm use international accounting standards (IAS)?
Q.74 Does your establishment have its annual financial statement reviewed by an external auditor?
Q.75 How long does it take to clear (in days) the following payments through your financial institution and what is the charge you pay as a per cent of the transaction?
Q.76 What share of your sales to your customers (in per cent) in 2001 was ultimately settled by:
Q.77 What share of your purchases from suppliers in 2001 was ultimately settled by:
Q.78 Do you currently have any payments overdue (by more than 90 days) to each of the following. If yes, what is the amount of the overdue payments as a % of your total annual sales?
Q.79 Since 1998, has your firm received any subsidies from the national government, regional/local governments or any other sources? If yes, on average since 1998, what was the amount of these subsidies as a per cent of average annual sales.

Business Environment

Q.80 Can you tell me how problematic are these different factors for the operation and growth of your business: a) Access to financing (e.g., collateral required) or financing not available from banks; b) Cost of financing (e.g., interest rates and charges); c) Telecommunications; d) Electricity; e) Transportation; f) Access to land ;g) Tax rates; h) Tax administration; i) Customs and trade regulations; j) Business licensing and permits; k) Labour regulations; l) Skills and education of available workers; m) Economic policy uncertainty; n) Macroeconomic instability (inflation, exchange rate); o) Functioning of the judiciary; p) Corruption; q) Street crime/theft/disorder; r) Organised crime/Mafia; s) Anti-competitive practices of other producers; t) Contract violations of by customers and suppliers; u)Title or leasing of land

Table A1.4 Regulations

	Regulatory Burden	Easy to obtain regulatory info	Consistency and predictability of laws and regulations	Ave # of days for custom clearance	Ability to get correct treatment from other official
Regulatory Burden	1.00				
Easy to obtain regulatory info,	-0.06***	1.00			
Consistency and predictability of laws and regulations	-0.11***	0.51***	1.00		
Ave # of days for custom clearance	0.06**	-0.01	0.01	1.00	
Ability to get correct treatment from other official	-0.05***	0.19***	0.21***	-0.02	1.00

** significant at 5%; *** significant at 1%

Table A1.5 Corruption and State Capture related ratings correlations

	Corruption, as % of sales	State Capture	How common is bribing?	How often is bribing for:									State Capture 2
				Utility services	Licensing	Gov. contracts	Fire inspect.	Envir. inspect.	Tax inspect.	Customs	Courts	Average	
Corruption, as % of sales	1.0												
State Capture	0.18	1.0											
How common is bribing?	0.41	0.27	1.0										
How often is bribing for:													
Utility services	0.23	0.33	0.31	1.0									
Licensing	0.31	0.40	0.49	0.48	1.0								
Gov. contracts	0.29	0.46	0.39	0.35	0.53	1.0							
Fire inspections	0.28	0.39	0.44	0.43	0.57	0.50	1.0						
Env. inspections	0.27	0.43	0.38	0.42	0.52	0.52	0.72	1.0					
Tax inspectors	0.35	0.41	0.51	0.45	0.56	0.43	0.56	0.54	1.0				
Customs	0.25	0.47	0.43	0.35	0.49	0.49	0.45	0.47	0.59	1.0			
Courts	0.25	0.58	0.37	0.38	0.50	0.55	0.48	0.50	0.55	0.63	1.0		
Average	0.38	0.59	0.56	0.62	0.79	0.74	0.78	0.78	0.79	0.75	0.76	1.0	
State Capture 2	0.16	0.37	0.24	0.25	0.32	0.33	0.27	0.28	0.34	0.36	0.42	0.45	1.0

All significant at 1% level

Appendix 1.4

Evaluating Reverse Causality Problem in Estimation of γ's

The important caveat to the results at the firm level may be the feedback effect from firms' performances to perceptions of institutional variables. If the firm is not doing well, there may be a tendency to give qualitatively lower scores to such variables as corruption, courts' quality and regulatory burden.

Take for instance corruption, unhappy manager is likely to complain about the higher extent of corruption so there is a negative feedback effect from performance to reported corruption measure. If the measure of corruption negatively affects growth of sales in OLS regression, the feedback effect from the latter is likely to induce downward bias in OLS estimates.

Similar logic applies to regulatory burden and obstacles to financing variables in this study. The situation is somewhat different with the state capture. As before, with better firms' performance, the respondent is likely to complain less about state capture activities so OLS estimates are biased downwards. If the state capture benefits the individual firm, the true effect is bigger than the OLS estimate. Likewise, courts' quality effect is likely to be underestimated by OLS in the presence of reverse causation problem.

To see this, consider a simple example. The estimated equation is

$$Y_{is} = \gamma X_{is} + u_{is} \qquad (1)$$

and suppose that X_{is} is function of Y_{is},

$$X_{is} = \theta\, Y_{is} + e_{is} \qquad (2)$$

Then applying OLS to (1) will result in the following expression

$$\hat{\gamma} = \gamma + \frac{\text{cov}(u_{is}, X_{is})}{\text{var}(X_{is})} = \gamma + \frac{\theta}{1-\theta\gamma}\frac{{\sigma_u}^2}{{\sigma_x}^2}$$

Therefore, the asymptotic bias has the same sign as $\frac{\theta}{1-\theta\gamma}$. If higher Y_{is} leads to smaller X_{is} ($\theta < 0$) and $1-\theta\gamma > 0$ then estimated γ will be biased downwards.

In case of corruption, if the true effect of corruption γ is negative then the sign of $\frac{\theta}{1-\theta\gamma}$ will depend on whether the term $1-\theta\gamma$ is greater or smaller than zero. The firm-specific effect of corruption γ on growth of sales of firms is likely to be smaller than one in absolute value because corruption is measured as a percentage of sales. One percent increase in corruption payments will not likely to decrease sales by more than one percent. Likewise, drop in sales will not likely to induce the respondent to report two times more than the firm actually gives in corruption payments so θ is likely to be less than one in absolute value either. If $1-\theta\gamma$ is greater than zero then the estimated effect of corruption will be biased downwards.

Considering *State Capture*'s influences on growth of rims, the conjectured true γ for state capture is positive. If the hypothesized feedback effect θ is negative then $1-\theta\gamma$ is positive and estimated γ will be biased downwards without ambiguity.

To evaluate the extent of the joint endogeneity problem in firm-level estimates, we use the propensity of complaining measure from the data.

If this feedback effect is present, the data should show at least some statistically significant negative correlation between propensity to complain and growth of sales.

One way to control the propensity of managers to complain in the surveys that are similar to BEEPS is to use the average rating of quality and efficiency of public services

related to infrastructure like roads, postal service, power, telephone and water to approximate a respondent's tendency to complain. The idea comes from Hellman et al. (2000) who compare country averages of survey responses about infrastructure to external objective measures. We further argue that the subjective individual variation in ratings of these services should be smaller than the individual variation in ratings of corruption, for example. Actual differences in the ratings of infrastructure partly can be explained by differences among managers in their tendency to complain about everything.

The propensity to complain variable was constructed by calculating the individual deviations of equally weighted mean ratings of telecommunication, electricity, and transportation services. We first construct the lack of infrastructure variable, which is calculated as the average of individual responses to the question about how problematic are telecommunication, electricity, and transportation services for operations and growth of firms. We measure the propensity to complain as individual deviations of constructed measure from country's average.

The variable is not significantly correlated with growth of sales (-0.9 percent), negatively correlated with firms' deviations in ratings of courts' quality while positively correlated with firm-level measures of regulatory burden, corruption, state capture variables and obstacles to financing (Table A1.6).

Table A1.6 Simple correlations of Propensity to Complain and Firms' Deviations in Ratings of Institutions

Variables	Sales Growth	Courts Quality	Regulatory Burden	Corruption	State Capture	Obstacles to Finance
Propensity to Complain	-0.009	-0.055***	0.076***	0.0594***	0.0973***	0.2149***

*** significant at 1%

Appendix 1.5 Variables and Sources

Variable	Definition	Source
Regulatory Burden	Percent of senior management's time in 2001 was spent in dealing with public officials about the application and interpretation of laws and regulations and to get or to maintain access to public services, %	BEEPS2
Corruption	On average, percent of total annual sales do firm's like yours typically pay in unofficial payments/gifts to public officials, %	BEEPS2
State Capture	In a given year, how often a firm like yours would make payments/gifts to influence the content of new legislation rules decrees etc. Never=1, Seldom=2, Sometimes=3, Frequently=4, Usually=5, Always=6	BEEPS2
Obstacles in Financing	How problematic is the access to financing or financing not available from banks for the operation and growth of your business. No obstacle=1, Minor obstacle=2, Moderate obstacle=3, Major obstacle=4	BEEPS2
Firms' Age	2002 minus year the firm began operations in the country	BEEPS2
Ln (Size)	Log of the number of employees	BEEPS2
State	Dummy equal to 1if the legal organization is state/municipal/district-owned enterprise, and state ownership is share is more than 50%	BEEPS2
New Private	Dummy equal to 1 if firm was originally established as private, from time of start up	BEEPS2
Privatized	Dummy equal to 1 if the firm was privatized and state share now is less than 50%	BEEPS2
Lack of Market Power	If you were to raise your prices of your main product line or main line of services 10% above their current level in the domestic market (after allowing for any inflation) which of the following would best describe the result assuming that your competitors maintained their current prices? Our customers would continue to buy from us in the same quantities as now = 1 Our customers would continue to buy from us, but at slightly lower quantities = 2 Customers would continue to buy from us, but at much lower quantities = 3 Many of our customers would buy from our competitors instead = 4	BEEPS2
Big City	Dummy equal to 1: if over 1 million	BEEPS2
Medium City	250 000 – 1 000 000	
Small City	50 000 – 250 000	
Town/Village	under 50 000	
Capital	Capital	

Lack of Infrastructure	How problematic are these different factors for the operation and growth of your business: Telecommunications, Electricity and Transportation. No obstacle=1, Minor obstacle=2, Moderate obstacle=3, Major obstacle=4. Average of ratings for all three factors is calculated.	BEEPS2
Labor Regulations Obstacles	Percent of managers surveyed rank this as a major business constraint	BEEPS2
WB Regulatory Quality	World Bank Index in 2002: "Regulatory Quality focuses more on the policies themselves, including measures of the incidence of market-unfriendly policies such as price controls or inadequate bank supervision, as well as perceptions of the burdens imposed by excessive regulation in areas such as foreign trade and business development."	World Bank Governance Indicators Database
TI Corruption Index	Transparency International ratings in 2002	Transparency International
State Capture 2	Unweighted average of responses for the question: To what extent have the following practices had a direct impact on your business: a) Private payments/gifts or other benefits to Parliamentarians to affect their votes; b) Private payments/gifts or other benefits to Government officials to affect the content of government decrees; c) Private payments/gifts or other benefits to judges to affect the decisions of criminal court cases d) Private payments/gifts or other benefits to judges to affect the decisions in commercial cases; e) Private payments/gifts or other benefits to central bank officials to affect central bank policies and decision; f) Illegal contributions to political parties and/or election campaigns to affect the decisions of elected officials No impact=0, Minor Impact=1, Moderate impact=2, major impact=3, decisive impact=4	BEEPS2
Not Free Partially Free Free	Dummies based on categories made by Freedom House	Freedom House
Growth	GDP growth rate in 2000	EBRD
Inflation	CPI based inflation in 2001	EBRD

Chapter 2 Appendices

Appendix 2.1

Variables Definitions and Sources

A. Real GDP per capita (Laspeyres): RGDPL from Penn World Tables

RGDPL is obtained by adding up consumption, investment, government and exports, and subtracting imports in any given year. The given year components are obtained by extrapolating the 1996 values in international dollars from the Geary aggregation using national growth rates. It is a fixed base index where the reference year is 1996, hence the designation "L" for Laspeyeres.

B. Political Events: From the Banks Cross-National Time-Series Data Archive (Banks-CNTS):

(Moderate) Unrest: aggregated measure is a combination of Demonstrations, General Strikes, and Riots defined below.

1. **General Strikes**: The number of any strike of 1,000 or more industrial or service workers that involves more than one employer and that is aimed at national government policies or authority.
2. **Anti Government Demonstrations**: Any peaceful public gathering of at least 100 people for the primary purpose of displaying or voicing their opposition to government policies or authority, excluding demonstrations of a distinctly anti-foreign nature.
3. **Riots:** Any violent demonstration or clash of more than 100 citizens involving the use of physical force.

Violent Unrest: aggregated measure is a combination of guerrilla warfare and revolutions.

4. **Guerrilla Warfare:** Any armed activity, sabotage, or bombings carried on by independent bands of citizens or irregular forces and aimed at the overthrow of the present regime.
5. **Revolutions:** Any illegal or forced change in the top governmental elite, any attempt at such a change, or any successful or unsuccessful armed rebellion whose aim is independence from the central government.

Government Changes – aggregated measure is not used.

6. **Coups d'Etat:** The number of extra constitutional or forced changes in the top government elite and/or its effective control of the nation's power structure in a given year. The term "coup" includes, but is not exhausted by, the term "successful revolution". Unsuccessful coups are not counted.
7. **Changes in Effective Executive**: The number of times in a year that effective control of the executive power changes hands. Such a change requires that the new executive be independent of his predecessor.
8. **Number of Cabinet Changes:** The number of times in a year that a new premier is named and/or 50% of the cabinet posts are occupied by new ministers.

Other:

9. **Government Crises:** Any rapidly developing situation that threatens to bring the downfall of the present regime - excluding situations of revolt aimed at such overthrow.

10. **Assassinations**: Any politically motivated murder or attempted murder of a high government official or politician.

C. Political System Measures (Banks-CNTS 2002).

1. Type of Regime (Banks-CNTS measure)

(1) Civilian. Any government controlled by a nonmilitary component of the nation's population.
(2) Military-Civilian. Outwardly civilian government effectively controlled by a military elite. Civilians hold only those posts (up to and including that of Chief of State) for which their services are deemed necessary for successful conduct of government operations. An example would be retention of the Emperor and selected civilian cabinet members during the period of Japanese military hegemony between 1932 and 1945.
(3) Military. Direct rule by the military, usually (but not necessarily) following a military coup d'état. The governing structure may vary from utilization of the military chain of command under conditions of martial law to the institution of an ad hoc administrative hierarchy with at least an upper echelon staffed by military personnel.
(4) Other. All regimes not falling into one or another of the foregoing categories, including instances in which a country, save for reasons of exogenous influence, lacks an effective national government.

Military Regime Indicator (used in the regression analysis)
= 1 if the above CNTS's Type of Regime = (2) or (3) and zero otherwise.

2. Legislative effectiveness:

(0) None. No legislature exists.
(1) Ineffective. There are three possible bases for this coding: first, legislative activity may be essentially of a "rubber stamp" character; second, domestic turmoil may make the implementation of legislation impossible; third, the effective executive may prevent the legislature from meeting, or otherwise substantially impede the exercise of its functions.
(2) Partially Effective. A situation in which the effective executives power substantially outweighs, but does not completely dominate that of the legislature.
(3) Effective. The possession of significant governmental autonomy by the legislature, including, typically, substantial authority in regard to taxation and disbursement, and the power to override executive vetoes of legislation.

3. Party Legitimacy (Banks-CNTS Measure):

(0) No parties, or all but dominant party and satellites excluded
(1) Significant exclusion of parties (or groups)
(2) One or more minor or "extremist" parties excluded
(3) No parties excluded

Measure below is constructed and used in the regression analysis:

Party Legitimacy = 1 if Banks-CNTS Measure = (2) or (3), and zero otherwise.

4. Party Coalitions (Banks-CNTS measure)**:**

(0) No coalition, no opposition
(1) More than one party, government coalition, no opposition
(2) More than one party, government coalition, opposition
(3) More than one party, no coalitions

D. Democracy: POLITY2 variable

Combined Polity Score: The POLITY score is computed by subtracting the AUTOC score from the DEMOC score; the resulting unified polity scale ranges from +10 (strongly democratic) to -10 (strongly autocratic). POLITY2 is Revised Combined Polity Score. It modifies the combined annual POLITY score by applying a simple treatment, or ""fix," to convert instances of "standardized authority scores" (i.e., -66, -77, and -88) to conventional polity scores (i.e., within the range, -10 to +10).

Regime Dummies (Marshall and Gurr (2003)):

Democracy =1 if Polity2 score $\geq$ 6, and 0 otherwise

Autocracy =1 if Poltiy2 score $\leq$ -6, and 0 otherwise

Anocracy =1 if -5 $\leq$ Polity2 $\leq$ 5, and 0 otherwise

E. Ethnic Fractionalization (from Alesina et al. 2003)

The variable is computed as one minus the Herfindahl index of ethnic group shares, and reflected the probability that two randomly selected individuals from a population belonged to different groups.

E2. Urbanization

Urban population (% of total), from World Development Indicators

F. Data from Miguel et al. (2004).

Civil Conflict ≥25 deaths

Any Internal War or Any Internationalized Internal War. Dichotomous variable. PRIO/Uppsala define Minor Conflict, Intermediate Conflict, and War as follows:

- Minor Armed Conflict: At least 25 battle-related deaths per year and fewer than 1,000 battle-related deaths during the course of the conflict.
- Intermediate Armed Conflict: At least 25 battle-related deaths per year and an accumulated total of at least 1,000 deaths, but fewer than 1,000 per year.
- War: At least 1,000 battle-related deaths per year.

G. Countries by region

Latin America & Caribbean	Sub-Saharan Africa	South-East Asia & Pacific	Middle East& North Africa	Europe
1. Barbados	14. Benin	45. Fiji	54. Turkey	58. Austria
2. Chile	15. Botswana	46. Korea, Rep.	55. Israel	59. Belgium
3. Costa Rica	16. Burkina Faso	47. New Zealand	56. Jordan	60. Cyprus
4. Dominican Republic	17. Burundi	48. Philippines	57. Morocco	61. Iceland
5. El Salvador	18. Cape Verde	49. Thailand		62. Luxembourg
6. Guatemala	19. Central African Rep.	50. Bangladesh		63. Ireland
7. Guyana	20. Chad	51. Nepal		64. Switzerland
8. Haiti	21. Comoros	52. Pakistan		65. Sweden
9. Honduras	22. Congo, Dem. Rep	53. Sri Lanka		66. Greece
10. Jamaica	23. Equatorial Guinea			67. Portugal
11. Nicaragua	24. Ethiopia			
12. Paraguay	25. Gambia, The			
13. Uruguay	26. Ghana			
	27. Guinea			
	28. Guinea-Bissau			
	29. Madagascar			
	30. Malawi			
	31. Mali			
	32. Mauritania			
	33. Mauritius			
	34. Mozambique			
	35. Namibia			
	36. Niger			
	37. Rwanda			
	38. Sierra Leone			
	39. South Africa			
	40. Tanzania			
	41. Togo			
	42. Uganda			
	43. Zambia			
	44. Zimbabwe			

Table A2.1 Political Events Correlations: measured as number of events in a given year

	Gen.Strikes	AntiGovDem	Riots	GuerrillWar.	Revolutions	Gov. Crises	Assassin.	Coups	ChangeExec.	CabChanges
General Strikes	1									
Anti Gov. Demonst.	0.2810*	1								
Riots	0.2598*	0.6881*	1							
Guerrilla Warfare	0.0577*	0.0563*	0.1051*	1						
Revolutions	0.1089*	0.1131*	0.0912*	0.3246*	1					
Government Crises	0.1485*	0.1906*	0.2567*	0.2202*	0.1708*	1				
Assassinations	0.1208*	0.1248*	0.1236*	0.2265*	0.1978*	0.1503*	1			
Successful Coups	0.0579*	0.0159	0.0525*	0.0192	0.3561*	0.1407*	0.0365	1		
Change in Executive	0.0734*	0.0626*	0.1063*	0.0433*	0.2063*	0.2712*	0.1008*	0.4265*	1	
Cabinet Changes	0.0653*	0.1075*	0.1179*	0.0704*	0.2432*	0.2836*	0.0541*	0.2758*	0.4648*	1

* significant at 5%

Table A2.2 Political Events Correlations: measured as dummy variables, 1 if events happened in a given year

	Gen.Strikes	AntiGovDem	Riots	GuerrillWar.	Revolutions	Gov. Crises	Assassin.	Coups	ChangeExec.	CabChanges
General Strikes	1									
Anti Gov. Demonst.	0.3280*	1								
Riots	0.2909*	0.4965*	1							
Guerrilla Warfare	0.0627*	0.1356*	0.1504*	1						
Revolutions	0.0743*	0.1220*	0.0850*	0.4047*	1					
Government Crises	0.2077*	0.2448*	0.2493*	0.1275*	0.1384*	1				
Assassinations	0.1490*	0.2573*	0.2035*	0.3024*	0.2107*	0.1474*	1			
Successful Coups	0.0569*	0.0214	0.0479*	0.0569*	0.3874*	0.1402*	0.0670*	1		
Change in Executive	0.0680*	0.0856*	0.1176*	0.0585*	0.2158*	0.2762*	0.1240*	0.4330*	1	
Cabinet Changes	0.0424*	0.0832*	0.0987*	0.032	0.1792*	0.2009*	0.1027*	0.2259*	0.4627*	1

* significant at 5%

Table A2.3 Effect of Economic Growth on Probability Unrest by Regions: OLS and IV-2SLS

Dependent Variable	Unrest, OLS					Unrest by Different Regions, IV-2SLS				
Explanatory variables	Latin Amer. (1)	S.-S. Africa (2)	S.East Asia (3)	Middle East (4)	W. Europe (5)	Latin America (6)	S.-S. Africa (7)	S.East Asia (8)	Middle East (9)	W. Europe (10)
Growth $_t$	-0.564	-0.102	-0.547	-1.938	-2.443	12.14	5.46	-6.38	133.2	-1.70
	[0.298]*	[0.122]	[0.764]	[0.665]*	[0.520]***	[9.32]	[4.27]	[10.60]	[796.3]	[2.45]
Democracy$_{t-1}$	0.04	-0.123	-0.078	0.278	-0.47	-0.1	-0.16	-0.16	-2.05	-0.44
(Anocracy omitted)	[0.105]	[0.062]*	[0.121]	[0.133]	[0.194]**	[0.22]	[0.12]	[0.21]	[14.8]	[0.18]**
Autocracy$_{t-1}$	-0.172	-0.112	0.053	-0.19	0.539	0.17	-0.16	-0.03	5.02	0.49
	[0.079]*	[0.048]**	[0.096]	[0.095]	[0.180]**	[0.23]	[0.10]	[0.21]	[31.28]	[0.27]
Ethnic Fractionalization	0.378	0.293	-0.509	1.586	-0.118	-0.06	0.64	-0.63	-24.02	-0.12
	[0.451]	[0.202]	[0.164]**	[0.311]**	[0.458]	[0.90]	[0.44]	[0.37]	[155.3]	[0.44]
Military Regime$_{t-1}$	-0.336	-0.053	0.153	-0.296	-0.352	-0.18	-0.13	0.21	0.47	-0.35
	[0.183]*	[0.037]	[0.102]	[0.073]**	[0.154]*	[0.30]	[0.10]	[0.19]	[4.92]	[0.15]**
Urbanization	1.37	0.594	-0.256	-0.91	0.922	1.16	0.57	-0.14	0.45	0.91
	[0.208]***	[0.144]***	[0.294]	[0.085]***	[0.430]*	[0.45]**	[0.28]**	[0.40]	[9.39]	[0.41]*
Legislative	-0.128	-0.026	-0.044	0.033	-0.207	-0.03	-0.08	-0.04	2.73	-0.21
Effectiveness$_{t-1}$	[0.092]	[0.028]	[0.063]	[0.091]	[0.110]	[0.16]	[0.07]	[0.06]	[16.3]	[0.10]*
Party Coalitions$_{t-1}$	-0.028	0.038	0.134	-0.093	-0.147	-0.11	0.001	0.12	0.63	-0.15
	[0.042]	[0.015]**	[0.033]***	[0.106]	[0.051]**	[0.11]	[0.03]	[0.03]***	[5.11]	[0.05]**
Party Legitimacy$_{t-1}$	-0.037	-0.095	-0.111	0.462	1.145	0.09	-0.08	-0.13	-5.16	1.10
	[0.138]	[0.068]	[0.047]**	[0.104]**	[0.091]***	[0.20]	[0.10]	[0.06]*	[34.7]	[0.17]***
Country dummies	No	No	No	No	No	No	No	No	No	No
*Trend*country*	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	453	1002	317	155	296	453	1002	317	155	296
R^2	0.15	0.23	0.28	0.38	0.25					
F test of excl. instr.	---	---	---			1.52	1.32	2.82	0.01	7.76
p-value	---	---	---			(0.2179)	(0.2516)	(0.0941)	(0.9371)	(0.0057)

Robust clustered standard errors in brackets, p-values in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A2.4 Effect of Economic Growth on Probability of Other Events: OLS and IV-2SLS

Dependent Variable: Events	Coups	Changes in Executive	Changes in cabinets	Assassinations	Coups	Changes in Executive	Changes in cabinets	Assassinations
Explanatory variables	OLS (1)	OLS (2)	OLS (3)	OLS (4)	IV-2SLS (5)	IV-2SLS (6)	IV-2SLS (7)	IV-2SLS (8)
Growth $_t$	-0.28	-0.27	-0.6	-0.22	-0.28	-1.21	-1.92	-3.06
	[0.11]**	[0.13]**	[0.16]***	[0.13]*	[1.14]	[1.98]	[2.40]	[2.01]
Democracy$_{t-1}$	0.01	-0.02	-0.1	-0.05	0.01	-0.02	-0.1	-0.05
(Anocracy omitted)	[0.02]	[0.04]	[0.05]*	[0.04]	[0.02]	[0.04]	[0.05]*	[0.05]
Autocracy$_{t-1}$	-0.04	-0.15	-0.08	-0.02	-0.04	-0.15	-0.08	-0.04
	[0.02]**	[0.03]***	[0.04]*	[0.04]	[0.02]*	[0.04]***	[0.04]**	[0.04]
Ethnic Fractionalization	0.06	0.11	0.15	0.08	0.06	0.1	0.14	0.03
	[0.06]	[0.13]	[0.18]	[0.10]	[0.06]	[0.14]	[0.19]	[0.13]
Military Regime$_{t-1}$	-0.05	0.01	0.05	-0.04	-0.05	0.02	0.06	-0.02
	[0.02]**	[0.04]	[0.05]	[0.03]	[0.02]**	[0.04]	[0.05]	[0.03]
Urbanization	-0.01	0.03	-0.09	0.07	-0.01	0.02	-0.11	0.03
	[0.05]	[0.13]	[0.18]	[0.13]	[0.05]	[0.15]	[0.19]	[0.13]
Legislative	-0.04	-0.01	-0.02	-0.01	-0.04	-0.01	-0.02	-0.01
Effectiveness$_{t-1}$	[0.01]***	[0.03]	[0.03]	[0.02]	[0.01]***	[0.03]	[0.03]	[0.02]
Party Coalitions$_{t-1}$	0	-0.03	-0.04	0.01	0	-0.02	-0.04	0.02
	[0.01]	[0.01]**	[0.02]**	[0.01]	[0.01]	[0.01]*	[0.02]*	[0.01]
Party Legitimacy$_{t-1}$	-0.01	-0.01	0.09	-0.02	-0.01	-0.01	0.08	-0.03
	[0.02]	[0.04]	[0.05]*	[0.05]	[0.02]	[0.04]	[0.05]*	[0.05]
Regional Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country dummies	No	No	No	No	No	No	No	No
*Trend*country*	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2251	2251	2246	2223	2251	2251	2246	2223
R^2	0.065	0.085	0.138	0.199	--	--	--	--
F test of excl . instr.					7.73	7.73	7.73	7.73
p-value					(0.0055)	(0.0055)	(0.0055)	(0.0055)

Robust clustered standard errors in brackets, p-values in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A2.5 First Stage Regression: Effect of Δ^{+} Ln(P oil)$_{t-1}$[2] on growth rates by countries

Country	Coefficient	Stand. Err		Country	Coefficient	Stand. Err	
Bangladesh	-0.053	0.021	**	Madagascar	-0.010	0.007	
Belgium	-0.028	0.012	**	Malawi	-0.034	0.018	*
Benin	-0.064	0.012	***	Mali	0.031	0.029	
Botswana	-0.009	0.018		Mauritania	-0.057	0.011	***
Burkina Faso	0.007	0.019		Mauritius	-0.031	0.026	
Burundi	-0.030	0.027		Morocco	0.010	0.010	
Centr. Afr. Rep.	-0.031	0.017	*	Mozambique	-0.335	0.054	***
Chad	0.034	0.038		Namibia	0.985	0.694	
Chile	-0.148	0.025	***	Nepal	0.012	0.035	
Comoros	-0.041	0.031		New Zealand	-0.048	0.005	***
Congo, Dem. Rep.	-0.038	0.016	**	Nicaragua	0.021	0.023	
Costa Rica	-0.017	0.012		Niger	-0.041	0.038	
Cyprus	-0.160	0.022	***	Pakistan	0.007	0.016	
Dominican Republic	-0.011	0.007		Paraguay	0.022	0.006	***
El Salvador	-0.033	0.041		Philippines	0.014	0.008	*
Equatorial Guinea	-0.103	0.027	***	Portugal	-0.077	0.014	***
Ethiopia	-0.018	0.014		Rwanda	-0.008	0.027	
Fiji	-0.039	0.017	**	Sierra Leone	0.010	0.035	
Gambia, The	0.046	0.011	***	South Africa	0.013	0.010	
Ghana	-0.113	0.006	***	Sri Lanka	-0.006	0.008	
Greece	0.003	0.013		Sweden	-0.008	0.004	**
Guatemala	0.009	0.009		Switzerland	-0.068	0.020	***
Guinea	0.016	0.016		Tanzania	-0.031	0.031	
Guinea-Bissau	0.110	0.092		Thailand	-0.004	0.009	
Guyana	-0.044	0.014	***	Togo	-0.023	0.077	
Haiti	-0.047	0.024	*	Turkey	-0.006	0.027	
Honduras	0.018	0.014		Uganda	0.000	0.008	
Ireland	-0.017	0.009	**	Uruguay	0.032	0.011	***
Israel	-0.015	0.005	***	Zambia	-0.015	0.014	
Jamaica	-0.049	0.015	***	Zimbabwe	0.025	2.76	
Jordan	0.033	0.016	**				
Korea, Rep.	-0.023	0.028					

significant at 10%; ** significant at 5%; *** significant at 1%;
all control variables from column (8) in Table 8 are included in regression

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