“Essays in Political Economy”

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1. Introduction

This thesis consists of five essays in the field of political economy. The first part of the thesis includes three essays covering various aspects of the political economy of globalization and economic reforms, which are linked in several ways. The second part of the thesis includes two essays on the political economy of development in India. The aim of this introductory section is to give a brief and non-technical overview of the essays, as well as to explain the links between them. The discussion of the contribution of the research to the existing literature will be carried out separately in each essay.

Globalization and economic reforms are two important concepts in the international political economy field. Explaining the social effects of both has been at the center of the international political economy literature for an extensive period of time. Much has been written about the effects of globalization on social outcomes (Dreher, Gaston, and Martens 2008, Schneider, Barbieri, and Gleditsch 2003) and there are at least two distinct trains of thought in academia and the public discourse. One view is that the structural changes towards more liberal economic policy can positively transform the economy and polity, increasing economic growth and welfare, as well as bringing much required modernization (Bhagwati 2004, Wolf 2004, Friedman 1999). On the other hand, more pessimistic voices, which include mainstream

economists such as Nobel laureate Joseph Stiglitz, and Marxist critical theorists, believe that globalization can simultaneously promote a 'race to the bottom' in social standards that degrades communitarian bases of social stability and welfare (Rodrik 1997, Stiglitz and Charlton 2006). In fact, the global financial crisis of 2008, where people took to the streets in both developing and developed countries, has made the issue of whether or not globalization and economic reforms create socially undesirable consequences, ever more crucial.

The first part of the thesis sheds new light on various aspects of the social effects of globalization and economic reforms, reviewing findings in the literature to date and extending existing theories. A major contribution of the thesis is the rigorous and thorough empirical evaluation of the human rights effects of economic reforms, and to what extent globalization has induced a race to the bottom in labour standards, while also contributing new empirical findings to extend the research surrounding the side effects of participating in IMF programs.

The thesis is structured as follows. In chapter 1, we focus on the impact of economic reforms and economic freedom on human rights. It is argued that economic policy reforms will benefit most people in terms of better access to goods, lower inflation, and better economic opportunities (Murphy et al. 1991). However, critics of market reforms see the majority as losers from such reforms, expecting resistance that would lead to political repression (Przeworski 1991). Using the change in the Index of Economic Freedom as a measure of market liberalization reforms, employing data from a panel of 117 countries for the 1981–2006 period, the results show a strong positive association between reforms towards more free markets and governments' respect for human rights, controlling for a host of relevant factors, including the possibility of endogeneity. These results lend support to those who argue that freer markets generate better

economic conditions and higher levels of social harmony. In fact, halfhearted measures at implementing reforms could be dangerous to human rights.

After exploring the impact of economic reforms on human rights, we continue by linking economic liberalization policies prescribed by international organizations such as the IMF to the outbreak of civil war in chapter 2. As the global economic downturn has heightened concerns over intervention by international financial institutions, as well as political stability, a prominently published work by Hartzell, Hoddie and Bauer (2010) purports to show that signing on to an IMF Structural Adjustment Program (SAP) increased the risk of civil war during the 1970–1999 period. They claim that the IMF's SAPs push economic liberalization to the point where some people are so negatively affected as to foment civil war. We advance this debate by critically examining their theoretical and empirical evidence, particularly questioning their crucial assumptions about the impact of IMF programs on the economic environment in terms of the actual winners and losers from economic liberalization, and who might be in a position to rebel. Separating the effects of crises from IMF interventions is crucial since crises also generate losers in their own right. With only minor adjustments to their study, we find the exact opposite of what they conclude. We show that their measure of signing on to an IMF program remains consistently insignificant in explaining the outbreak of civil war, using the threshold of 25 battle deaths when defining the onset of a civil war. These results suggest that their operationalization of the IMF variable, as well as the utilization of large-scale civil wars (1000 deaths and above), captures the effect of ongoing war rather than the effects of liberalization. After extending the time period under study from 1970–1999 to 1970–2008, as well as making some minor changes to operationalization, again we find that IMF involvement is at worst a poor predictor of conflict, and at best, an alleviator of the risk.

The next chapter turns to a topic which is currently the focus of both politicians and the labour unions in general. We analyze whether inter-country competition for investment has adverse effects on labour standards. Among the many concerns over globalization is that as nations compete for international firms, they will relax labour standards as a method of lowering costs and attracting investment. Using spatial econometric estimation on panel data for 148 developing countries over 18 years, we find that the labour standards in one country are indeed positively correlated with labour standards elsewhere (i.e., a reduction in the labour standards of other countries reduces the labour standards of the country in question). This interdependence is more evident in labour practices (i.e., enforcement) than in labour laws. Furthermore, competition is most fierce in those countries which already have low standards. Since there has been a decline in the labour practices and laws across all three groups, this is possible evidence of a race to the bottom as nations compete for investment.

The second part of the thesis focuses on essays on the political economy of development in India. India is often hailed as one of the success stories of globalization (Basu 2008). Indeed, after the inception of market economic reforms in 1991, economic growth has been both sustained and impressively robust in terms of national economic indicators (Basu 2008). However, despite rapid economic growth during the post-1990 period, the benefits of economic growth are unevenly distributed, and some areas and groups of people have seen their living standards decline (Banerjee 2010). One could argue that this is somewhat surprising given the rapid surge in economic growth in recent years (Bardhan 2010). In connection with this, two critical issues have attracted a lot of attention, both within and outside India. First is the issue of

corruption which is seen as a hindrance to prosperity and development, and the second is the emergence of India as one of the major donors of development aid.

In chapter 4, we focus particularly on the influence of the timing of elections on controlling corruption. Firstly, we develop a conceptual framework that extends theories of political budget cycles to corruption, where an incumbent government considers controlling corruption based purely on political considerations. More specifically, we investigate whether the timing of elections affects the responsiveness of the incumbent government to control corruption. Secondly, we empirically test the predictions of the conceptual framework using 30 Indian states during the 1988–2009 period. Consistent with the conceptual framework developed, i.e., an incumbent politician might exert greater effort in an election year to control corruption, the findings show that scheduled elections (and not unscheduled elections) are associated with an increase in the number of corruption cases registered. In addition, we find that corruption cases registered tend to increase as a scheduled election year draws closer. Furthermore, the effects are found to be stronger in 'swing states' (where the margin of victory of the incumbent in the previous election was 5% or less), and in state scheduled election years which coincide with national elections. On the other hand, there is no effect of scheduled elections on corruption cases being investigated by anti-corruption agencies.

The following chapter examines a puzzling question about India. Here, we analyze what determines Indian development aid. It is indeed puzzling to note that India, which has a large domestic population suffering from underdevelopment, chronic poverty and mal-governance, is emerging as an important aid donor. With the intension of learning why poor countries provide foreign aid, this is the first work to econometrically analyze India's aid allocation decisions. We utilize cross-sectional data on aid commitments to 128 developing countries by the Ministry of

External Affairs and the Export-Import Bank of India, obtained in US dollars from AidData for the 2008-2009 period. We then compare India's bilateral aid allocation with that of other donors to examine if India is any different regarding the motivation behind its allocation decisions. The findings show that India's aid allocation decisions are largely driven by commercial and political self-interest. While recipient need does not seem to be a key determinant, neighboring countries receive considerable attention.

The final section of this work briefly summarizes the main findings.

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2. Chapter 1

Do Pro-Market Economic Reforms Drive Human Rights Violations?[1]

2.1 Introduction

There is little consensus on whether or not economic policy reforms towards freer markets lead to violations of human rights (see Abouharb and Cingranelli 2009, de Soysa and Eriksen 2009).[2] This issue resurfaces in many of the debates related to growing globalization and the spread of neo-liberal policies and values (Martell 2010; Stilwell 2006). Several prominent observers hail the victory of free-market capitalism over its rivals as progress that promotes prosperity, social peace and democracy (Friedman 1962, Hayek 1944, Bhagwati 2004, Fukuyama 1991). Thus, countries at higher levels of economic freedom and change at higher rates are expected to have less social dissatisfaction and less state repression of dissent (Sen 1999). Skeptics of market

[1] Coauthored with Indra de Soysa, Norwegian University of Science and Technology (NTNU henceforth).

[2] We use the terms *political repression* and *violations of human rights* interchangeably because they essentially mean the same thing. In fact, the two most widely-used empirical indicators, the CIRI human rights database and the Political Terror Scale, are essentially based on information obtained from agencies such as the US State Department and Amnesty International reports. Thus, conceptions of political terror and the degree of respect for human rights are analytically and empirically very similar.

reform, comprised of scholars of critical political economy, neo-Marxist scholars, a large portion of the NGO community, the anti-globalization movement, as well as some orthodox economists, voice concern over capitalistic policy choices for social justice and stability (Root 2008, Stiglitz 2002). They claim that free-market reforms are mostly undertaken by ruling elites for narrower aims, in which the rich reap all the gains and displace the pain of austerity and budget cuts on the poor and politically powerless. Therefore, the question of whether economic reform policies towards more free markets cause social dissent and disarray, measured as the level and degree of state violations of human rights, is an empirical one which we address by examining the association of the levels and rates of change towards more free-market policies and respect for human rights of individual citizens by their own states.

We employ a widely-used measure of economic policies that capture broad-based reform towards free markets, rather than only assessing whether or not there was intervention by International Financial Institutions (IFIs hereafter), which has generally been the norm for addressing this question (Abouharb and Cingranelli 2006, 2009). The problem with such an approach is the rather strong assumption that governments actually do implement market-oriented reforms suggested by the IMF, which some suggest is not the case (Boockmann and Dreher 2003). Our approach is a direct test of the mechanism that purportedly explains why interventions by IFIs increase human rights violations, which is that free-market economic policies that these institutions push lead to state repression of human rights. Our measure of "economic opening" therefore captures both endogenous economic changes, with and without interventions from outside and regardless of the severity of political and economic crisis that generally require the IMF to be involved. Many countries, notably China and India, undertook far reaching economic reforms without much external intervention.

This study contributes to the existing literature in at least two very important ways. Firstly, most studies simply address the issue of liberalization and human rights by looking at the level of economic openness, which is typically measured as openness to trade and foreign investment (FDI) (see Hafner-Burton 2005). We make use of a comprehensive measure of the Fraser Institute's Economic Freedom Index as a proxy for the extent of market-economic policy reforms. Secondly, we explicitly address a number of causality issues because of possible endogeneity between the respect for human rights and free market reforms by estimating dynamic models, employing the GMM estimation method, and addressing the issue of direction of causality using Granger Causality test. We employ panel data for 117 countries (95 Least Developing Countries and 22 OECD countries) over the period from 1981–2006 (26 years). The rest of the paper is structured as follows: Section 2.2 presents the various theoretical discussions around how market economic reforms and its progress can affect human rights within countries. Section 2.3 introduces the two dimensions of market reforms, human rights and their measurement. Section 2.4 discusses the method of estimation and data. We report empirical results in Section 2.5, and then conclude.

2.2 Why might reforms matter for Human rights?

Economic reforms are usually wide-ranging changes to the existing regulatory, institutional and structural make up of an economy, and are aimed at increasing economic efficiency by promoting the privatization of markets, free competition and the strengthening of property rights. Reforms generate winners and losers, and the causes of reforms and their consequences are functions of the "political economy" within these societies and not entirely due to economic factors alone, or any particular direction of the reforms being undertaken (Alesina and Drazen

1991, Rodrik 1997). It is empirically and theoretically recognized that economic policy reforms may require strong political authority since losers from reform are likely to resist them, whether they are directed towards more free markets or away from them (Alesina, Ardagna and Trebbi 2006, Haggard and Kaufman 1992). On the other hand, political rulers who generally control *de facto* political power are likelier to repress rather than reform if reforms towards more free markets, particularly granting stronger property rights, might undermine access to power and privilege in the future (Acemoglu and Robinson 2008). Others show that reforms were undertaken in many cases because there was general consensus about the necessity for reform because of the widespread recognition that the previous policies had failed (Armijo and Faucher 2002). Indeed, many suggest that the recent spread of neoliberal policies has occurred because of emulation (Simmons, Dobbins and Garrett 2004). Clearly, the view on whether market reforms necessitate human rights violations or causes it is mixed, and it is not immediately obvious how the degree of human rights in a country relates to economic policies. As studies of human rights violations show, the rights of people are violated by governments when they are faced with serious social dissent (Poe 2004). Consequently, repression is one tool from a "menu of choices" available to policy makers for dealing with serious threats to socio-political stability. The degree of human rights violations in a country is therefore a relatively good proxy for capturing the level of social dissent serious enough to threaten the incumbents in power, and in many ways, an indirect measure of the degree and extent of societal grievances.[3]

The causal link between the economic reforms and human rights are offered by two competing theoretical traditions in political economy. First, the direct effects of economic

[3] Of course, we also acknowledge that mass violations of human rights may occur without the threat of serious dissent, but this is likely to be the exception rather than the rule.

liberalism on human rights can be traced back to Adam Smith's "The Wealth of Nations," which argued that people who are free from economic regulations and restraints will naturally solve collective dilemmas such as peace and security as if by use of a "hidden hand" (Smith 1776). Free markets encourage voluntary exchange and the allocation of goods according to supply and demand, whereas success and failure in the market is based on effort and talents rather than by privilege. These processes obtain harmony because the power to determine social outcomes largely rests with individuals and communities rather than states—in other words, respect for property rights leads to the dilution of state power and the empowerment of citizens, who are free to choose or exit with their assets (Stilwell 2006).

Freer markets, rather than privilege, help disperse economic resources, allowing those with economic power to offset the influence of those with political power (Iversen 2008). Political competition and market competition are complements because they both prevent monopoly (Hayek 1944, 1994, Friedman 1962). In Friedman's (1962: 10) words, "the kind of economic organization that provides economic freedom directly, namely, competitive capitalism, also promotes political freedom because it separates economic power from political power and in this way enables the one to offset the other." Moreover, markets are viewed as superior at allocating scarce resources relative to states, and the incentives operating in markets act powerfully to raise individual productivity and wealth (Bjørnskov and Foss 2008). Liberals argue that economic and political pathologies related to rent-seeking can have severe negative consequences for society, whereas competitive capitalism should bring wealth, justice and social harmony (de Soysa and Fjelde 2010, Mousseau and Mousseau 2008). As a result, countries engaged in economic policy reform are likely to be on the right path to greater affluence, more

democracy and conditions generally preferred by ordinary people (Murphy et al. 1991, Berggren 2003, Gans-Morse and Nichter 2008).

Liberals also argue for indirect effects through positive outcomes of reforms on social and human capital development (Goldsmith 1997, Dawson 1998, Norton 2003, Gerring and Thacker 2008). Likewise, there is much evidence to suggest that internal conflicts in their various forms occur as a result of underdevelopment and lower economic growth (Collier et al. 2003, Hegre and Sambanis 2006). Amartya Sen (1996) contends that it is the friendlier economic policies, and not repressive political systems *per se*, that provide economic growth and development. Countries that make use of the opportunities provided by economic policy reforms will gain economically and solve problems related to underdevelopment and the lack of industrialization, which are factors that promote respect for human rights and the empowerment of people. However, if opening up to free markets requires repression, then economic growth might be still-born because instability and political repression could scuttle growth. Although there is quite a bit of evidence that shows a positive relationship between economic policy reforms towards more free markets and economic growth[4], several scholars, including some prominent economists, voice concern about the redistributional effects of economic policy reforms. They argue that reforms imposed by elites impose costs on the poor that can lead to "race to the bottom" effects which would increase social tensions, undermine democracy and entrap states in cycles of poverty and repression (Przeworski 1991, Rodrik 1994, Armijo, Biersteker and Lowenthal 1994, Haggard and Kaufman 1995). Nevertheless, others argue that

[4] Prominent studies such as: Easterly and Levine (1997), Ayal and Karras (1998), Gwartney et al. (1999), de Haan and Sturm (2000), Carlsson and Lundstrom (2002), Scully (2002), Dawson (2003), de Haan et al. (2006), Doucouliagos and Ulubasoglu (2006), Justesen (2008) have all examined the impact of economic freedom and economic growth. All the studies, including the meta-data study by Doucouliagos and Ulubasoglu (2006), find strong positive effects between economic freedom and economic growth with different samples, countries and periods. See Rodriguez and Rodrik (2001) for the other side of the debate.

bad policies are purposely followed by political elites because bad policies are good politics that help the elites maintain their powers and privileges, which can lead to self-sustaining poverty traps (Acemoglu and Robinson 2008).

There is no doubt that reforms create winners and losers, and who actually dissents and for what reasons—whether socially beneficial or harmful—is generally unclear, but one might assume that repression is highest where the losers are many and the winners are small. Rodrik (1994) argues that the consequences of neoliberal policies often involve a redistribution of income among different groups. If the efficiency gains from neoliberal policies are not substantial and income is not redistributed properly, this may lead to widespread agitation for resisting the required policy changes. Others see economic liberalization causing problems due to the short-term hardships borne by some (Gans-Morse and Nichter 2008). Those directly hurt by austerity measures may take their anger to the streets, often resulting in strikes and riots that states respond to with repressive measures. Many scholars argue that these destabilizing effects pose a threat to democratic institutions, thereby ultimately leading to human rights repression (Przeworski 1991). Clearly, the net effect of economic policy reforms on human rights violations is theoretically and empirically ambiguous. Thus, we test the following hypothesis:

Ceteris paribus, countries moving towards freer economic policies will experience higher violations of human rights.

2.3 Measuring human rights and market-economic policy reforms

We use the Cingranelli and Richards (2006) Human Rights Data (CIRI) measuring the degree of government respect for Physical Integrity Rights (PIR hereafter). These data contain annual coverage from 1981 to 2008 for 195 countries. The sources of information used for coding the

index are from both the US State Department's annual country reports on human rights practices and from Amnesty International's annual reports.[5] The "integrity of physical rights" deals with the abuses that physically harm people such as torture, disappearances, imprisonment for political beliefs and political murder. The PIR index contains information about the pattern and sequence of government respect for physical integrity rights in addition to the level of severity. Here, the pattern is defined as "the association of different levels of government respect for several physical integrity rights with a single, overall scale score" (Cingranelli and Richards 1999). Sequence is defined as "the order in which governments have a propensity to violate particular physical integrity rights" (Cingranelli and Richards 1999). Naturally, some forms of violations, such as political murder and torture, are more serious than others, such as imprisonment. The PIR index is based on the human rights practices of governments and any of its agents such as police or paramilitary forces. The index is an additive index constructed from observations on torture, extrajudicial killing, political imprisonment and disappearances. It ranges from zero, meaning no government respect for these four human rights to eight, or full government respect for physical integrity rights.

Our main independent variable is market reforms. Previous studies addressing the issue of market economic policy reforms have used proxies, such as a dummy variable capturing the date on which the country liberalized its economy, or a composite index made from several dummy variables on capital account and current account restrictions (Asiedu and Lien 2004). Others have used trade openness and the level of FDI in an economy. These single indicators only capture very specific aspects of economic policy reforms. Major upheavals to the social and

[5] For more on the construction of the dataset and coding rules, see the CIRI Human Rights Data project at: http://ciri.binghampton.edu/documentation.asp

economic order of a country possibly occur because of broader changes that include the privatization of business and regulatory laws covering labour and wages, the state of property rights enforcement, real and potential trade openness and the extent of government involvement in the economy.

Following Dreher, Lamla, Lein and Somogyi (2009), we consider the Fraser Institute's Economic Freedom Index (EFI hereafter) constructed by Gwartney and Lawson (2008) as a proxy for policy reforms. These data are available in five-year intervals for the period from 1970 – 2000, and on a yearly basis thereafter. The EFI is a comprehensive measure comprised of five sub-indices capturing: expenditure and tax reforms; property rights and legal reforms; trade reforms; reforms related to access to sound money; labour, business and credit reforms. These five sub-indices are roughly comprised of 35 components of objective indicators. Each variable in the respective sub-indices was transformed to an index on a scale from 0 to 10. When higher values of the original variable indicated a higher degree of freedom, the formula $[(V_i - V_{min}) / V_{max} - V_{min})] \times 10$ was used for the transformation. Conversely, when higher values indicated less freedom, the formula was $[(V_{max} - V_i) / V_{max} - V_{min})] \times 10$. The sub-component indices were then averaged to determine each component. The component indices within each area are averaged to derive the indices for each of the five sub-indices. In turn, the five sub-indices are averaged to derive the summary index for each country. The final index is then ranked on a scale of 0 (not free) to 10 (totally free). Another way of interpreting this would be that the value of 0 denotes the absence of state regulations, or a state failure to provide these public goods, while 10 denotes the highest level in a highly competitive market economy. Hence, a higher index implies a higher degree of market conformity. Finally, the missing years between the reported quintiles for this variable are interpolated. Since the score on EFI changes slowly between the five-year

periods, the interpolated values should not be problematic. The detailed description on EFI is captured in exhibit 1 in appendix. However, unlike Dreher et al. (2009) and Bjørnskov and Foss (2008), we also make use of **Δ** reforms (henceforth) denoting the year-to-year growth in the overall index of market-economic conformity (i.e. EFI), which in fact captures the degree to which reforms occurred. A positive value of **Δ** reforms indicates a movement towards more free market policies, whereas a negative value would be a move towards more autarky. In other words, the **Δ** reforms capture the new policy decisions taken by the state and not necessarily the accumulation of reforms over the years, which we also control for in our models because countries at very low levels are more likely to have higher rates of change and vice versa.

2.4 Data and method

We analyze a time-series cross-section dataset (TSCS) containing 117 countries covering the years from 1981–2006 (see table 1.6 in appendix). The available number of countries and the coverage over time are entirely dependent on the availability of the EFI measure and CIRI data on human rights. Since the EFI is not available for all countries for all years, our dataset is unbalanced. The model to be estimated is specified as:

$$PIR_{it} = \phi_1 + \psi_2 PIR_{it-1} + \psi_3 H_{it} + \psi_4 Z_{it} + \upsilon_t + \omega_{it} \qquad (1)$$

Where, PIR_{it} is the Physical Integrity Rights index for country ***i*** at year ***t***. H_{it} captures the hypothesis variable(s), namely **Δ** reforms and EFI, and Z_{it} includes the vector of control variables. υ_t are time dummies and ω_{it} is the error term for country ***i*** at time ***t***. Following others, we also include lagged dependent variables (PIR_{it-1}). There are two reasons for the inclusion of a lagged dependent variable. First, it fixes problems associated with autocorrelation and model

dynamic effects of the X variables on Y (Poe and Tate 1994, Beck and Katz 1995, Wilson and Butler 2007). Second, it is theoretically plausible that bureaucratic decisions associated with the organs of state repression use past decisions to decide whether to repress or not in present circumstances, so this behavior can be quite sticky (Poe, Tate and Keith 1999, Neumayer 2005). Hence, we also estimate all our models with a lagged dependent variable.

The vector of control variables (Z_{it}) includes other potential determinants of human rights which we obtain from the extant literature on the subject. We follow the pioneering studies of Poe and Tate (1994), Poe, Tate and Keith (1999) and other comprehensive evaluations of these early studies on the determinants of repression (Landman 2005). Accordingly, the models control the effects of development by including per capita income (logged) in US$ constant prices and the economic growth rates obtained from the Economic Research Service (ERS) International Macroeconomic 2008 dataset, Washington DC.[6] Since economic reforms are more likely to be peaceful when countries are richer and enjoying higher growth rates, we control for this. Following others (Landman 2005), we include the log of total population (ERS 2008). Large countries will generally have more dissent and are harder to govern. As a matter of fact, country size is one of the most robust predictors of civil war and political repression (Hegre and Sambanis 2006, Landman 2005). To measure the nature of the political regime in power, we include a measure of regime type using the Polity IV data (Marshall and Jaggers 2002). Democracies are less likely to use repression as a policy tool. We subtract the autocracy score from the democracy score, as is standard when using the Polity data. The democracy score ranges from +10 (full democracy) to –10 (full autocracy).

[6] We use the ERS dataset to maximize the number of observations in our dataset. The results do not change dramatically when using the per capita GDP data sourced from the World Development Indicator 2010 (World Bank 2010).

Additionally, we account for the degree of ethnic fractionalization sourced from Fearon and Laitin (2003) since some claim that ethnic difference can lead to social frictions that generally discourage good economic policies (Easterly 2006).[7] Naturally, an ongoing civil war is likely to affect the degree of state repression. We include a variable measuring civil war that takes the value 1 if there is armed conflict between an organized group and a state in which at least 25 deaths have occurred in a single year and 0 otherwise (Gleditsch et al. 2002). We also include the count of the number of years of civil peace so as to distinguish between immediate post-war situations and others (Gleditsch et al. 2002). In addition to these variables, we include oil export dependency, which is independently related to repression due to the so-called "resource curse" (Ross 2001, de Soysa and Binningsbo 2009). Oil wealth is a dummy taking the value 1 if oil exports exceed one-third of export revenue, and 0 if not. Finally, the legal heritage of countries is likely to matter. We include dummy variables, which take the value 1 separately if the country's legal heritage originates from one of the following: British, Socialist, French, German and Scandinavian, and 0 otherwise (La Porta et al. 1998). For more details on the data, see the data sources in table 1.7 in appendix. The descriptive statistics are presented in table 1.8 in appendix. The baseline models are estimated using ordered probit with time dummies, in addition to the pooled OLS method (POLS henceforth). The pooled data are susceptible to having highly correlated data between and across panels that could lead to highly optimistic standard errors (Beck and Katz 1995). We use the Newey-West method which allows us to compute an AR1 process for autocorrelation and obtain Huber-White corrected robust standard errors that are robust to heteroscedasticity (Newey and West 1987). With both these methods (Ordered probit and Newey-West POLS), we do not include any country fixed effects because

[7] For dissenting views on ethnic fractionalization and economic outcomes, see Collier (2001) and de Soysa (2011).

some of the variables (ethnic fractionalization and legal heritage, etc.) are "time invariant." The usage of two-way fixed effects will not only be collinear with time-invariant or largely time-invariant regressors, but will also generate biased estimates (Beck 2001). However, we drop the time-invariant variables from our models and perform two-way OLS fixed effects because accounting for unit (country) heterogeneity is an additional robustness check since TSCS results can be sensitive to specification (Wilson and Butler 2007). We estimate the Huber-White corrected robust standard errors, which is a method robust to heteroskedasticity and serial correlation (Wiggins 1999).

2.4.1 Endogeneity concerns

It is quite possible that our key explanatory variables – **Δ** reforms and EFI – are endogenous to having less human rights violations. That is, it might be governments committed to respecting human rights that reform in the first place. For example, the expectation of political or regime instability arising out of dissent and uprising could deter new policy initiatives to be introduced by the government that, among other things, eases restrictions on economic freedom. Improvement in human rights performance of the states may also spur increases in economic policy reforms initiated by the government. Not taking this endogeneity into account would induce bias in our estimate of the effect of policy reforms and EFI on human rights. This issue is not trivial because those who argue that repression is required for free-market reforms make causal claims about reforms leading to repression. Nevertheless, to determine the direction of causality we use a dynamic model of Granger Causality (Granger 1969). Accordingly, the variable *x* is said to "Granger cause" a variable *y* if the past values of the *x* help explain *y*, once

the past influence of *y* has been accounted for (Engle and Granger 1987). We follow Dreher and Siemers (2009) to account for Granger Causality in a panel setting as:

$$y_{it} = \sum_{j=1}^{\rho} \psi_j \, y_{i,t-j} + \sum_{j=1}^{\rho} \xi_j \, x_{i,t-j} + \delta_i + \zeta_t + \omega_{it} \qquad (2)$$

Where, the parameters are denoted as: $\psi_{i,t}$ and $\xi_{i,t}$ for country *i* during the year *t*, the maximum lag length is represented by ρ. While δ_i is unobserved individual effects, ζ_t is unobserved time effects. ω_{it} denotes the error term. Under the null hypothesis, the variable *x* is assumed to not Granger cause *y,* while the alternative hypotheses allow for *x* to Granger cause *y* after controlling for past influence of the variable *y*. Joint F-Statistic is used to gauge the joint significance of PIR on **Δ** reforms and EFI.

After rejecting the null hypothesis of no endogeneity, we control for this by replicating the baseline models using the system-GMM estimator as suggested by Arellano and Bond (1991), Arellano and Bover (1995) and Blundell and Bond (1998). We are not aware of an IV estimator for an ordinal score dependent variable when the error term is serially correlated and heteroscedastic. We therefore follow Miguel et al. (2004) and Eichengreen and Leblang (2008) in estimating linear ordered probability models, which provide consistent estimates. The dynamic panel GMM estimator exploits an assumption about the initial conditions to obtain moment conditions that remain informative even for persistent data, which is considered appropriate in the presence of endogenous regressors. The results are based on the two-step estimator implemented by Roodman (2006) in Stata 11. The two-step GMM estimator weights

the instruments asymptotically by efficiently using the first-step estimates. Because our sample is large enough, we do not face the problem of under estimated standard errors (Arellano and Bond 1991). We apply the Sargan-Hansen test on the validity of the instruments used (amounting to a test for the exogeneity of the covariates) and the Arellano-Bond test of second order autocorrelation, which must be absent from the data in order for the estimator to be consistent. We treat the lagged dependent variable and our measures of **Δ** reforms and EFI as endogenous and all other variables as strictly exogenous. Thus, EFI and **Δ** reforms are lagged by two years.[8] As before, we include time dummies in all the GMM regressions. In order to minimize the number of instruments in the regressions, we collapse the matrix of instruments as suggested by Roodman (2006).

2.5 Results

We begin with the Granger Causality tests (tables 1.1 and 1.2). The null hypothesis in set 1 can be easily rejected at lag lengths from one to three (see table 1.1). However, at lag two, the joint F-statistics, though significant at the 5% level, is down to the 10% level at three lags. The results do remain constant at the 10% level when we introduce one more lag structure into set 1. The joint F-statistic is strongly significant at the 1% level in column 1 of set 1 in table 1.1. In set 2, we clearly find that there is reverse causality flowing from PIR to EFI at all three lag structures, with joint significance at the 1% level (see table 1.1). The null hypothesis that human rights have no effect on economic freedom can therefore be rejected for lag length one to three. In table 1.2, we find that at lag one and three there is a significant positive impact of changes in the economic

[8] As a further robustness check, we used different versions of lagged values for EFI and **Δ** reforms. We lagged both variables by one and three years for both the global sample and the sample of LDCs, and the results remained unchanged.

freedom index (**Δ** reforms) on human rights. However, the null hypothesis that human rights have no effect on economic reforms cannot be rejected for lag length one and two, but only at lag length three. Consequently, the purely statistical test addressing causality suggests that positive changes in economic reforms "cause" human rights to improve but reveals little effect from the other way around. In fact, the result from higher human rights to higher economic freedom is weakly negative.

Next, we move to dynamic models that allow us to control for other relevant intervening factors. In table 1.3, column 1, we find that **Δ** reforms (annual change in the economic freedom scores) have a statistically significant positive impact on PIR, net of all the controls. The positive effect suggests that movement towards more free markets reduces the level of violations of physical integrity rights. These results are robust to the inclusion of a lagged dependent variable. The positive and statistically significant effect of free market economic reforms is robust to entering region dummies (see column 1 in table 1.3). These results are upheld when including regional dummies in column 2. When we drop the time-invariant variables in column 3 by including country and time effects, the relationship between **Δ** reforms and PIR is still positive. Notice that the level of economic freedom (accumulated reforms) shows a robustly positive association with rights across the columns, thereby signifying that even when change towards more free markets are accounted for, the level of free market policies has a strong positive effect on rights, net of several important control variables, such as income per capita and the level of democracy. Thus, even if countries may have short-term pain from reforms in some cases, they may be worth undertaking in order to reach higher levels of economic freedom for obtaining social peace (de Soysa and Fjelde 2010, Mousseau and Mousseau 2008). Overall, the results are in line with the basic argument put forth by Amartya Sen (1999) and Hayek (1974) on the

importance of economic freedom for social progress, although our results also show no contradiction between moving towards more free markets and the deterioration of human rights.

The results of OLS regressions with Newey-West standard errors and two-way OLS fixed effects are presented in columns 4–6 in table 1.3. As seen there, the results remain the same as those reported for ordered probit regressions. The effect of free market reforms is positive on the government respect for human rights. A standard deviation increase in reforms would raise respect for human rights by roughly 39% of a standard deviation of the PIR index (columns 4 and 5). The impact from a maximum change of 180% on reforms could increase the PIR by 160% of a standard deviation, or 3.84 points, which is close to half the scale and certainly no small matter. The accumulated level of economic freedom also has a non-negligible effect, in which a standard deviation increase in the level of economic freedom would increase respect for human rights by roughly 19% of a standard deviation, and by almost 63% of a standard deviation of the PIR at the maximum value of economic freedom, net of all the other variables in the model. The somewhat lower impact of the level is not surprising since changes from lower to higher levels of economic freedom should yield a much higher impact than what might be expected when countries are already at high levels. In any case, because of the close association of wealth and democracy with economic freedom levels, the indirect effects are likely to be large. These results remain robust to the inclusion of regional dummies and two-way fixed effects estimations, in which the time-invariant variables drop out (columns 5 and 6 in table 1.3).

Table 1.4 demonstrates that the effect of reforms and EFI also matters when we only test a sample of 87 developing countries. In all columns, irrespective of the estimation techniques, both **Δ** reforms and EFI are significantly different from zero at the 1% level (see table 1.4). In the developing countries only sample we find very similar substantive effects, which are in fact

slightly increased in magnitude. These results remain robust to the inclusion of two-way OLS fixed effects regressions (see column 6 in table 1.4). Given that the results in both tables are net of the indirect effects through per capita income and the level of democracy, the total substantive impact of free market economic reforms on human rights seems quite substantial.

Interestingly, with the ordered probit, the Newey-West OLS and two-way fixed effects regressions, the control variables are consistent with those reported by others. There is a positive relationship between economic development (per capita GDP) and human rights. Increases in the level of income raises the cost of dangerous dissent and reduces the power of states to use repression as a viable policy tool. Although the results on the rate of economic growth are positive, they remain largely insignificant. Like others, we find that large countries have higher violations of rights. This effect is consistent across the methods displayed in all models in both samples. Contrary to many arguments about the effects of high ethnic fragmentation on social friction, we find significant positive effects of ethnic fractionalization on human rights in all the methods, which produces results that are consistent with those who argue that high fractionalization makes countries safer (de Soysa 2009, 2011, Landman and Larizza 2009). Conflicts cause higher violations of human rights, as others also report (Poe and Tate 1994, Poe, Tate and Keith 1999, Dreher, Gassebner and Siemers 2010).

Likewise, the greater the years of civil peace, the lower the incidence of human rights abuse. With respect to legal heritage relative to Scandinavian legal origin, we find that other legal systems display negative signs. Oil exporters show higher levels of human rights abuses than non-oil exporters. This result is consistent with the findings of Ross (2004) and de Soysa and Binningsbø (2009), who argue that oil exporters have high levels of social dissent and are likely to maintain autocratic regimes. As expected, democracy proved very important for human

rights. In all the models irrespective of estimation technique, democracy is positively associated with less human rights violations. Interestingly, our main results on **Δ** reforms and EFI demonstrated a net positive effect on basic human rights despite the inclusion of several statistically significant controls.

Next, we examine our models, controlling for possible endogeneity between human rights, **Δ** reforms and EFI. As discussed earlier, we make use of the System-GMM method to control for reverse causality (table 1.5). The Hansen test and the Arellano-Bond test do not reject the GMM specifications at conventional levels of significance across the columns. The Hansen J-statistic clearly shows that the null-hypothesis of exogeneity of the instruments cannot be rejected at the conventional level of significance. In table 1.5, EFI is significantly different from zero at the 1% level in all the columns (including developing countries). A noteworthy point is that after controlling for potential feedback from PIR, the value of the coefficient of EFI in column 1 of table 1.5 has more than doubled – from 0.15 to 0.40.

Changes in economic freedom, however, are now robustly significant on human rights only in the case of the LDCs sample and when regional dummies are not in the estimations for the global sample. This suggests that for the full sample, the result on reforms causing better human rights is not as robust, but given the stringency of the tests there are still statistically significant effects, particularly in the LDCs sample. For this reason, there is good evidence showing that reform efforts, even after controlling for endogeneity, improve human rights conditions among those countries already at lower levels of human rights. Both variables, for example, remain significant when we repeat the exercise in column 5 and 6 in table 1.5 by dropping the time-invariant variables. At no time, however, is there any evidence to suggest that that reforms lower human rights conditions as skeptics have argued. Our results demonstrate that

on average countries attain higher levels of economic freedom without the need for repressive measures, and the bulk of the evidence suggests that reforming more comprehensively might in fact improve existing levels of government respect for human rights, not social breakdown.

2.5.1 Further checks on robustness

We examine the robustness of our main findings in the following ways. First, the EFI measure is linearly interpolated by us for the years in-between the quintiles. We now run the original data only on the reported quintiles so as to remove any biases stemming from the interpolation. Hence, our total number of observations drops to 1130. Our results, for both global and developing countries, show that the level of economic freedom (EFI) is positive and significantly different from zero at the 1% level. However, the **Δ** reforms measure is now statistically insignificant across the board (including GMM). This change is likely due to the five-year data setup. Second, following Dreher and Boockmann (2010), we run all our previous estimations with the Political Terror Scale (PTS hereafter), which is an alternative measure of human rights violations coded differently but using similar empirical material, namely Amnesty and the US State Department reports (Gibney and Dalton 1996). The PTS is coded by the US State Department and Amnesty International on a scale of 0 – 5, with the highest value representing worse human rights conditions. Nonetheless, for easy interpretation we reverse the coding with a higher value implying full respect for human rights. The results show that EFI decreases political terror, although the results with respect to **Δ** reforms are not as robust. These results are essentially the same for both the full sample and the sample consisting of 87 developing countries only. It should be noted again, however, that the results on **Δ** reforms are never close to being negative, which the skeptical arguments expect to see empirically.

Third, instead of the percentage change of EFI, we take the first difference as our reforms variable. The results do not show any major change from those reported earlier. Alternatively, we also experimented with a dummy variable which obtains a value of 1 if the EFI score for an i^{th} country in the year t scores 6 or above and 0 otherwise. We find that our EFI dummy is significantly different from zero at conventional levels of significance. Fourth, we check for sensitivity by including a host of different control variables. We drop all our controls to estimate the impact of EFI and Δ reforms on human rights. We still find that both variables remain significant at conventional levels of significance. Even if we drop only those variables which remain statistically insignificant, we still find robust evidence for positive effects of reforms on human rights. Fifth, we consider five-year averages for 1981–1985; 1986–1990; 1991–1995; 1996–2000; 2001–2006 periods and replicate our baseline estimates as reported in table 1.1. EFI and change in EFI remain positive and significant for explaining better human rights in estimates using ordered probit, pooled OLS and fixed effects.[9]

Next, we examine the sensitivity of our main variables (i.e. EFI and **Δ** reforms) on various permutations and combination of controls by employing Extreme Bounds Analyses[10] (EBA hereafter) proposed by Leamer (1983) and Levine and Renelt (1992). The EBA enables us to examine whether the proposed variables are robust as determinants of human rights, independently of which of the additional variables are in the set of control variables. Due to limitations of space we do not discuss the technicalities of EBA here, but instead refer the reader to Leamer (1983) and Sala-i-Martin (1997). We follow a less stringent EBA test proposed by

[9] We also replicate the baseline estimations by splitting our sample into regions. America (North and South), Europe (including transition countries), East Asia and the Pacific, Sub-Saharan Africa, the Middle East and North Africa and South Asia. We find very similar results within all the regions except for South Asia. This is not surprising given the N of 5 for South Asia region.

[10] It is noteworthy that Hafner-Burton (2005) finds only patchy support using trade and FDI as measures of globalization on the level of human rights within countries.

Dreher et al. (2009). We find that EFI and **Δ** reforms are both significantly different from zero at the 5% level in 90% of regressions when testing the global sample and 92% of regressions for the developing countries sample only, with CDF(0) being almost equal to one in both samples. In addition, most of the control variables are strongly related to PIR, as in our baseline regressions. The EBA results provide strong additional support for the robustness of the relationship between EFI level and the rate of change of EFI on the levels of human rights. The results of all of the robustness checks (including that of EBA) are not reported because of space considerations, but they are replicable using our data and do files. Given the weight of this evidence, we can safely reject the hypothesis that free market economic reforms increase the incidence of human rights violations.

2.6 Conclusion

The founding father of Singapore, Lee Kuan Yew, is fond of saying, "economic development requires discipline," which is a sentiment echoed by many rulers who try to cling to power with the promise of bringing development (Sen 1999). Many think Lee has a point because it is commonly understood that people will resist free-market economic reforms even if they will benefit from such reforms in the long run. According to many, turning to the market is like "swallowing the bitter pill" (Weyland 1998). The question of whether free-market reforms cause declines in human rights is an issue that is extensively debated in the literature and among policy circles, particularly in the debates on globalization and the effects of IMF interventions. The skeptics of market-oriented economic policy reforms contend that reforms reflect the interests of the haves and that they come at the expense of the many. Under such conditions, a market opening can lead to mass dissent and the violations of human rights. Liberals, on the other hand,

see economic reforms as the antidote to crises that can lead to social breakdown and that various aspects of a market opening can be healthy for economics and politics. Despite much anecdotal evidence pointing both ways, there has been little systematic empirical research that addresses this issue and takes the question of the direction of causality seriously because even if levels of open market policies are good for societies, changing towards them might cause problems that may destroy the foundations of future progress.

Our findings are easily summarized. Using the best available data and empirical methods, we find positive effects of market-economic policy reforms on government respect for human rights. We control for potential feedback effects, running from human rights to increased market-economic policy reforms using the GMM method of estimation. Even after controlling for endogeneity, market-economic policy reforms seem to predict better human rights, a result that is robust to changes in specification and testing method, as well as in a sample of 87 developing (non-OECD) countries only. These results support liberal optimism about market economic reforms and vindicate those who find positive effects of free markets on economic development and other measures of social welfare, including the public good of peace and decent governance. Furthermore, if IMF interventions lead to human rights violations as some find, then the mechanism leading to such an outcome might clearly not be a market opening as they assume. Perhaps interventions allow bad governments to defer opening as some have claimed (Boockmann and Dreher 2003), which may raise levels of dissent due to the continuation of cronyism. Despite our tests controlling for endogeneity, it might still be true that there is widespread consensus that reforms are necessary when they are in fact undertaken so that social dissent is lower before reforms occur (Armijo and Faucher 2002). Regardless, our results show that the level of economic freedom and movement towards greater economic freedom both

reduce violations of human rights. Future studies might look more carefully at the differential effects of change towards freer markets and changes away from them, and more clearly identify the winners and losers of free/market economic reforms and the conditions under which high dissent is defused or exacerbated, taking into consideration the exact nature of market opening policies.

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Table 1.1: Granger Causality tests on Economic Freedom Index and Human rights

	(1)	(2)	(3)		(1)	(2)	(3)
Variables	**PIR**	**PIR**	**PIR**	**Variables**	**EFI**	**EFI**	**EFI**
Physical Integrity Rights (t-1)	0.487***	0.395***	0.352***	Economic Freedom Index (t-1)	0.948***	1.263***	1.181***
	(0.0196)	(0.0224)	(0.0234)		(0.00753)	(0.0375)	(0.0399)
Physical Integrity Rights (t-2)		0.153***	0.108***	Economic Freedom Index (t-2)		-0.333***	-0.0959
		(0.0223)	(0.0247)			(0.0356)	(0.0630)
Physical Integrity Rights (t-3)			0.120***	Economic Freedom Index (t-3)			-0.171***
			(0.0224)				(0.0390)
Economic Freedom Index (t-1)	0.233***	0.261*	0.159	Physical Integrity Rights (t-1)	0.0105***	0.00709***	0.00579**
	(0.0465)	(0.156)	(0.159)		(0.00219)	(0.00242)	(0.00242)
Economic Freedom Index (t-2)		-0.0838	0.0900	Physical Integrity Rights (t-2)		0.00207	0.00126
		(0.153)	(0.242)			(0.00255)	(0.00272)
Economic Freedom Index (t-3)			-0.133	Physical Integrity Rights (t-3)			0.000996
			(0.157)				(0.00228)
Joint F-Statistics	25.03***	7.36***	2.51*	Joint F-Statistics	22.91***	7.57***	3.26***

Table 1.2: Granger Causality tests on Market-Economic Policy Reforms and Human rights

Variables	(1) PIR	(2) PIR	(3) PIR	Variables	(1) Reforms	(2) Reforms	(3) Reforms
Physical Integrity Rights (t-1)	0.507***	0.400***	0.356***	Economic Reforms (t-1)	0.380***	0.311***	0.285***
	(0.0194)	(0.0224)	(0.0233)		(0.0397)	(0.0436)	(0.0442)
Physical Integrity Rights (t-2)		0.166***	0.111***	Economic Reforms (t-2)		0.116***	0.0572
		(0.0224)	(0.0247)			(0.0426)	(0.0504)
Physical Integrity Rights (t-3)			0.122***	Economic Reforms (t-3)			0.0946**
			(0.0224)				(0.0482)
Economic Reforms (t-1)	1.632*	0.571	0.433	Physical Integrity Rights (t-1)	0.000351	0.000444	0.000340
	(0.854)	(0.885)	(0.860)		(0.000386)	(0.000484)	(0.000495)
Economic Reforms (t-2)		1.178	-0.0946	Physical Integrity Rights (t-2)		-0.000686	-0.000376
		(0.859)	(0.880)			(0.000504)	(0.000559)
Economic Reforms (t-3)			2.467***	Physical Integrity Rights (t-3)			-0.00102**
			(0.850)				(0.000464)
Joint F-Statistics	3.65***	1.65	3.34**	Joint F-Statistics	0.83	0.95	2.41*

Note: *** p<0.01, ** p<0.05, * p<0.1

Table 1.3: Effects of EFI and Market-Economic Reforms on Human Rights, 1981–2006 (Global sample)

Variables	(1) PIR Ordered Probit	(2) PIR Ordered Probit	(3) PIR Ordered Probit	(4) PIR Newey-West	(5) PIR Newey-West	(6) PIR Fixed Effects
Constant				1.731***	2.121***	3.615
				(0.265)	(0.340)	(4.445)
Lagged Dependent Variable	0.546***	0.517***	0.378***	0.620***	0.590***	0.408***
	(0.0173)	(0.0177)	(0.0201)	(0.0163)	(0.0175)	(0.0303)
Economic Freedom Index	0.144***	0.160***	0.171***	0.146***	0.167***	0.183***
	(0.0311)	(0.0319)	(0.0452)	(0.0312)	(0.0321)	(0.0573)
Economic Reforms	1.803***	1.879***	1.840***	2.062***	1.975***	1.878**
	(0.682)	(0.681)	(0.714)	(0.742)	(0.743)	(0.847)
Per capita GDP (log)	0.128***	0.0595**	0.0445	0.126***	0.0634**	-0.0278
	(0.0249)	(0.0298)	(0.150)	(0.0247)	(0.0307)	(0.220)
Per capita GDP growth rate	0.00090	0.0018	0.0011	0.00136	0.0016	0.0016
	(0.0066)	(0.0065)	(0.0066)	(0.0078)	(0.0077)	(0.0084)
Population (log)	-0.0995***	-0.123***	0.284	-0.114***	-0.123***	-0.104
	(0.0138)	(0.0157)	(0.287)	(0.0149)	(0.0170)	(0.401)
Democracy	0.0227***	0.0300***	0.0498***	0.0203***	0.0279***	0.0394***
	(0.00403)	(0.00486)	(0.00739)	(0.00411)	(0.00512)	(0.0113)
Oil Exports/GDP dummy	-0.160**	-0.0847	-0.142	-0.230***	-0.189***	-0.0865
	(0.0683)	(0.0694)	(0.155)	(0.0704)	(0.0724)	(0.218)
Conflicts	-0.755***	-0.860***	-1.009***	-0.988***	-1.066***	-1.176***
	(0.0740)	(0.0753)	(0.0944)	(0.0806)	(0.0827)	(0.161)
Civil Peace Years	0.00324**	0.000730	0.00226	0.00295**	0.00131	0.00576
	(0.00144)	(0.00152)	(0.00283)	(0.00147)	(0.00156)	(0.00377)
Ethnic Fractionalization	0.341***	0.172		0.339***	0.185*	
	(0.102)	(0.118)		(0.101)	(0.111)	
British Legal Heritage	-0.792***	-0.549***		-0.289***	-0.143**	
	(0.141)	(0.144)		(0.0662)	(0.0660)	
Socialist Legal Heritage	-0.358**	-0.114		0.158*	0.225*	
	(0.155)	(0.184)		(0.0946)	(0.136)	
French Legal Heritage	-0.707***	-0.429***		-0.223***	-0.0621	
	(0.140)	(0.144)		(0.0647)	(0.0632)	
German Legal Heritage	-0.596***	-0.476***		-0.0787	-0.0217	
	(0.176)	(0.178)		(0.111)	(0.108)	
OECD region dummy		0.775***			0.461***	
		(0.133)			(0.131)	
MENA region dummy		0.305**			0.267**	
		(0.122)			(0.130)	
Europe region dummy		0.317**			0.301*	
		(0.156)			(0.165)	
South East Asia region dummy		0.254*			0.128	
		(0.134)			(0.136)	
South Asia region dummy		0.0563			-0.103	
		(0.158)			(0.164)	
Sub-Saharan Africa region dummy		0.266**			0.185	

		(0.126)			(0.131)	
Latin America and Caribbean dummy		-0.0868			-0.144	
		(0.106)			(0.110)	
Pseudo R2	0.327	0.3351	0.3717			
Log Pseudo likelihood	-3911.7	-3864.4	-3651.9			
R-Squared						0.7503
F-Statistics				330.6***	390.5***	30.44***
Time Dummies	YES	YES	YES	YES	YES	YES
Country Dummies	NO	NO	YES	NO	NO	YES
Number of Countries	115	115	115	115	115	115
Total Observations	2746	2746	2746	2746	2746	2746

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 1.4: Effects of EFI and Market-Economic Reforms on Human Rights, 1981–2006
(Developing countries sample)

Variables	(1) PIR Ordered Probit	(2) PIR Ordered Probit	(3) PIR Ordered Probit	(4) PIR Newey-West	(5) PIR Newey-West	(6) PIR Fixed Effects
Constant				1.978***	2.102***	-0.332
				(0.339)	(0.385)	(5.409)
Lagged Dependent Variable	0.516***	0.505***	0.379***	0.606***	0.591***	0.418***
	(0.0183)	(0.0186)	(0.0211)	(0.0182)	(0.0189)	(0.0326)
Economic Freedom Index	0.118***	0.121***	0.152***	0.138***	0.146***	0.189***
	(0.0322)	(0.0334)	(0.0468)	(0.0343)	(0.0357)	(0.0613)
Economic Reforms	2.056***	2.099***	1.977***	2.396***	2.347***	2.097**
	(0.699)	(0.705)	(0.746)	(0.793)	(0.805)	(0.920)
Per capita GDP (log)	0.0860***	0.0842***	0.00829	0.0926***	0.0833**	-0.0194
	(0.0277)	(0.0310)	(0.161)	(0.0294)	(0.0333)	(0.242)
Per capita GDP growth rate	0.00205	0.00157	0.000683	0.00252	0.00194	0.00128
	(0.00674)	(0.00671)	(0.00672)	(0.00842)	(0.00833)	(0.00881)
Population (log)	-0.104***	-0.104***	0.311	-0.123***	-0.120***	0.306
	(0.0156)	(0.0165)	(0.345)	(0.0183)	(0.0191)	(0.519)
Democracy	0.0154***	0.0256***	0.0400***	0.0160***	0.0265***	0.0358***
	(0.00408)	(0.00532)	(0.00784)	(0.00434)	(0.00584)	(0.0120)
Oil Exports/GDP dummy	-0.126*	-0.140*	-0.0539	-0.193**	-0.213***	-0.00369
	(0.0696)	(0.0722)	(0.163)	(0.0771)	(0.0803)	(0.211)
Conflicts	-0.864***	-0.882***	-1.032***	-1.078***	-1.090***	-1.223***
	(0.0766)	(0.0781)	(0.0969)	(0.0885)	(0.0899)	(0.171)
Civil Peace Years	0.000325	0.000959	0.00121	0.00163	0.00234	0.00369
	(0.00172)	(0.00173)	(0.00332)	(0.00189)	(0.00191)	(0.00445)
Ethnic Fractionalization	0.334***	0.195		0.364***	0.233*	
	(0.109)	(0.131)		(0.121)	(0.138)	
British Legal Heritage	-0.0695	-0.142**		-0.0813	-0.147**	
	(0.0538)	(0.0622)		(0.0589)	(0.0694)	
Socialist Legal Heritage	0.381***	0.168		0.397***	0.162	
	(0.0922)	(0.130)		(0.104)	(0.148)	
French Legal Heritage						
German Legal Heritage						
OECD region dummy						
MENA region dummy		0.265*			0.216	
		(0.138)			(0.155)	
Europe region dummy		0.305*			0.281	
		(0.161)			(0.179)	
South East Asia region dummy		0.290**			0.179	
		(0.147)			(0.158)	
South Asia region dummy		0.0378			-0.0763	

		(0.167)			(0.182)	
Sub-Saharan Africa region dummy		0.231*			0.140	
		(0.135)			(0.148)	
Latin America and Caribbean dummy		-0.0836			-0.169	
		(0.115)			(0.127)	
Pseudo R2	0.2735	0.2761	0.3089			
Log Pseudo likelihood	-3206.7	-3194.9	-3050.2			
R-Squared						0.379
F-Statistics				173.3***	160.7***	29.92***
Time Dummies	YES	YES	YES	YES	YES	YES
Country Dummies	NO	NO	YES	NO	NO	YES
Number of Countries	87	87	87	87	87	87
Total Observations	2074	2074	2074	2074	2074	2074

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 1.5: Effects of EFI and Market-Economic Reforms on Human Rights, 1981–2006 (GMM)

Variables	(1) PIR Full sample SGMM	(2) PIR LDCs SGMM	(3) PIR Full sample SGMM	(4) PIR LDCs SGMM	(5) PIR Full sample SGMM	(6) PIR LDCs SGMM
Constant	1.915	2.021	1.322	1.903	1.896*	2.079
	(1.246)	(1.648)	(1.665)	(5.293)	(1.066)	(1.540)
Lagged Dependent Variable	0.237***	0.221***	0.209***	0.202***	0.256***	0.239***
	(0.0339)	(0.0455)	(0.0367)	(0.0454)	(0.0368)	(0.0467)
Economic Freedom Index	0.358***	0.386***	0.398***	0.405***	0.280***	0.344***
	(0.0845)	(0.0991)	(0.0899)	(0.120)	(0.0867)	(0.106)
Economic Reforms	3.427	6.531**	2.531	5.302	3.970*	6.669**
	(2.094)	(2.703)	(2.191)	(3.662)	(2.082)	(2.674)
Per capita GDP (log)	0.204***	0.164	0.138	0.208	0.186***	0.122
	(0.0664)	(0.102)	(0.110)	(0.136)	(0.0619)	(0.0971)
Per capita GDP growth rate	-0.00373	-0.00066	-0.00458	-0.00278	-0.00200	-0.00101
	(0.0095)	(0.0122)	(0.00982)	(0.0124)	(0.0099)	(0.0123)
Population (log)	-0.259***	-0.311***	-0.263***	-0.273***	-0.218***	-0.229**
	(0.0763)	(0.0966)	(0.0817)	(0.0996)	(0.0841)	(0.106)
Democracy	0.046***	0.032***	0.0523***	0.053***	0.046***	0.0325**
	(0.0109)	(0.0119)	(0.0134)	(0.0179)	(0.0110)	(0.0129)
Oil Exports/GDP dummy	-0.241	-0.291	-0.230	-0.257	-0.235	-0.312
	(0.193)	(0.231)	(0.216)	(0.258)	(0.193)	(0.238)
Conflicts	-1.383***	-1.368***	-1.322***	-1.314***	-1.405***	-1.389***
	(0.147)	(0.202)	(0.152)	(0.206)	(0.150)	(0.211)
Civil Peace Years	0.0128***	0.0146**	0.0098**	0.0160**	0.0135***	0.0140**
	(0.00400)	(0.00610)	(0.00402)	(0.00633)	(0.00410)	(0.00625)
Ethnic Fractionalization	0.518	0.708**	0.183	0.163		
	(0.318)	(0.350)	(0.263)	(0.314)		
British Legal Heritage	-0.555**	-0.0954	-0.219	-0.0863		
	(0.247)	(0.188)	(0.212)	(0.269)		
Socialist Legal Heritage	0.307	0.803**	0.626	0.953		
	(0.351)	(0.393)	(0.464)	(0.683)		
French Legal Heritage	-0.405		-0.174			
	(0.268)		(0.235)			
German Legal Heritage	-0.476		-0.184			
	(0.420)		(0.420)			
OECD region dummy			1.318*			
			(0.709)			
MENA region dummy			1.034	-0.508		
			(0.711)	(4.311)		
Europe region dummy			0.851	-0.427		
			(0.672)	(4.198)		
South East Asia region dummy			0.654	-0.375		
			(0.691)	(4.204)		

South Asia region dummy			1.080	-0.108		
			(0.716)	(4.335)		
Sub-Saharan Africa region dummy			0.422	-0.600		
			(0.784)	(4.290)		
Latin America and Caribbean dummy			0.374	-0.920		
			(0.641)	(4.303)		
Arellano-Bond test for AR(1): p-value	0.00	0.00	0.00	0.00	0.00	0.00
Arellano-Bond test for AR(2): p-value	0.452	0.719	0.634	0.827	0.359	0.607
Hansen test: p-value	0.259	0.859	0.393	0.908	0.263	0.753
Number of instruments	113	111	120	117	108	108
Wald chi2	2414.9***	1441***	3829.9***	1278.2***	2127.9***	1403.1***
Time Dummies	YES	YES	YES	YES	YES	YES
Number of Countries	115	87	115	87	115	87
Total Observations	2620	1978	2620	1978	2620	1978

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

2.8 Appendix

Exhibit 1: Components of the Fraser Economic Freedom Index (EFI)

Area 1: Size of Government: Expenditures, Taxes, and Enterprises
- A General government consumption spending as a percentage of total consumption
- B Transfers and subsidies as a percentage of GDP
- C Government enterprises and investment
- D Top marginal tax rate
  - i Top marginal income tax rate
  - ii Top marginal income and payroll tax rates

Area 2: Legal Structure and Security of Property Rights
- A Judicial independence (GCR)
- B Impartial courts (GCR)
- C Protection of property rights (GCR)
- D Military interference in rule of law and the political process (CRG)
- E Integrity of the legal system (CRG)
- F Legal enforcement of contracts (DB)
- G Regulatory restrictions on the sale of real property (DB)

Area 3: Access to Sound Money
- A Money Growth
- B Standard deviation of inflation
- C Inflation: Most recent year
- D Freedom to own foreign currency bank accounts

Area 4: Freedom to Trade Internationally
- A Taxes on international trade
  - i. Revenues from trade taxes (% of trade sector)
  - ii Mean tariff rate
  - iii Standard deviation of tariff rates
- B Regulatory Trade Barriers
  - i Non-tariff trade barriers (GCR)
  - ii Compliance cost of importing and exporting (DB)
- C Size of the trade sector relative to expected
- D Black-market exchange rates
- E International capital market controls
  - i Foreign ownership/investment restrictions (GCR)
  - ii Capital controls

Area 5: Regulation of Credit, Labor, and Business
- A Credit market regulations
  - i. Ownership of banks
  - ii Foreign bank competition
  - iii Private sector credit
  - iv Interest rate controls/negative real interest rates
- B Labor market regulations
  - i Minimum wage (DB)
  - ii Hiring and firing regulations (GCR)
  - iii Centralized collective bargaining (GCR)
  - iv Mandated cost of hiring (DB)
  - v Mandated cost of worker dismissal (DB)
  - vi Conscription
- C Business Regulations
  - i Price controls
  - ii Administrative requirements (GCR)
  - iii Bureaucracy costs (GCR)
  - iv Starting a business (DB)
  - v Extra payments/bribes (GCR)
  - vi Licensing restrictions (DB)
  - vii Cost of tax compliance (DB)

Source: Gwartney and Lawson (2008), www.freetheworld.com

Table 1.6: List of countries under Study

Albania	Denmark	Kuwait	Senegal
Algeria	Dominican Republic	Latvia	Singapore
Argentina	Ecuador	Madagascar	Sirrea Leone
Australia	Egypt	Malaysia	Slovakia
Austria	El Salvador	Mali	Slovania
Bahamas	Estonia	Malwai	South Africa
Bahrain	Fiji	Mauritius	Spain
Bangaldesh	Finland	Mexico	Sri Lanka
Barbados	France	Morocco	Sweden
Belgium	Gabon	Myanmar	Switzerland
Benin	Germany	Namibia	Syria
Bolivia	Ghana	Nepal	Tanzania
Botswana	Greece	Netherlands	Thailand
Brazil	Guatemala	New Zealand	Togo
Bulgaria	Guinea-Bissau	Nicragua	Trinidad & Tobago
Burundi	Guyana	Niger	Tunisia
Cameroon	Haiti	Nigeria	Turkey
Canada	Honduras	Norway	Uganda
Central African Republic	Hungary	Oman	United Arab Emirates
Chad	India	Pakistan	United Kingdom
Chile	Indonesia	Panama	United States of America
China	Iran	Papua New Guinea	Urkraine
Colombia	Ireland	Paraguay	Uruguay
Congo Democratic Republic	Isreal	Peru	Venezuela
Congo Republic	Italy	Philippines	Zambia
Costa Rica	Jamaica	Poland	Zimbabwe
Cote d'Ivoire	Japan	Portugal	
Croatia	Jordon	Romania	
Cyprus	Kenya	Russian Federation	
Czech Republic	Korea Republic	Rwanda	

Table 1.7: Data sources and definitions

Variables	Data description	Data Sources
Economic Freedom Index (EFI)	EFI is made up of five sub-indices capturing: expenditure and tax reforms; property rights and legal reforms; trade reforms; reforms related to access to sound money; labour, business and credit reforms. These five sub-indices are made up of 35 components of objective indicators. The final index is ranked on the scale of 0 (not free) to 10 (totally free)	Fraser Institute
Economic Policy reforms	Reforms denote year-to-year growth in the overall EFI.	Own construction
PIR index	The index range from 0, no government respect for human rights to 8, full government respect for human rights on torture, extrajudicial killing, political imprisonment, and disappearances.	CIRI dataset
Log (Per capita GDP) and growth rate	Per capita GDP (logged) in US$ 2000 constant prices and rate of growth of per capita GDP.	Economic Research Service (ERS), Washington DC
Log (Population)	Count of total population (logged)	ERS dataset, Washington DC
Political regime (Polity IV)	Polity IV index captures the nature of political regime ranging from +10 (full democracy) to –10 (full autocracy).	Marshall and Jaggers (2002)
Conflicts dummy	Dummy coding 1 if there is a civil conflict and 0 otherwise.	UCDP dataset (Gleditsch et al., 2002)
Number of peace years	Count of Number of civil peace years since the end of a conflict.	UCDP dataset (Gleditsch et al., 2002)
Ethnic Fractionalization	Index of Ethnic Fractionalization raging from 0 - 1	Fearon and Laitin (2003)
Legal heritages	Dummies for British, Socialist, French, German and Scandinavian legal origins.	La Porta et al. (1998)
Oil Exports dependency	Dummy taking the value 1 if oil exports exceed 1/3rd of export revenue, and 0 if not.	Fearon and Laitin (2003) augmented with fuel export data from the World Bank (2008).

Table 1.8: Descriptive Statistics

Variables	Mean	Median	Maximum	Minimum	Standard Deviation	Observations
PIR	4.902	5	8	0	2.368	2839
EFI	6.393	7	10	0	3.01	2839
Δ Economic Reforms	0.05	0.05	1.806	-5.391	0.447	2839
Democracy	2.834	6	10	-10	7.126	2839
log (Per capita GDP)	7.629	7.563	10.72	4.368	1.628	2839
GDP growth rate	3.201	3.6	4.25	-5.03	5.103	2839
Oil exports dummy	0.156	0	1	0	0.363	2839
log (Population)	9.393	9.229	14.076	5.849	1.505	2839
Conflicts dummy	0.192	0	1	0	0.394	2839
Ethnic Fractionalization	0.404	0.356	0.925	0.004	0.291	2839
Socialist legal heritage	0.064	0	1	0	0.245	2839
British legal heritage	0.339	0	1	0	0.473	2839
Instrument variable	0.004	0.061	0.651	-3.78	0.368	2839

3. Chapter 2

Bad Medicine? Intervention by the International Monetary Fund and the Risk of Civil War, 1970-2008[11]

3.1 Introduction

Intervention by the International Monetary Fund (IMF hereafter) is likened to "swallowing the bitter pill" because IMF structural adjustment programs (SAP henceforth), designed to produce macro-economic stability, require deeply unpopular austerity measures coupled with a package of broadly neoliberal reforms often referred to as "the Washington consensus" (Dreher and Rupprecht 2007, Easterly 2005, Stiglitz 2002, Vreeland 2003, Woods 2006).[12] The current economic crises have brought people into the streets in protest, leading to violence even in some Western capitals, which makes the issue of whether or not the IMF dispenses 'bad medicine' ever more prescient. Indeed, Hartzell, Hoddie and Bauer (HHB henceforth) in an article published by *International Organization* argue that liberalization through IMF SAPs is like "sowing the seeds of war" because economic liberalization generates 'losers' who organize violence. HHB's arguments are in many ways exemplary of

[11] Coauthored with Trude M. Midtgaard and Indra de Soysa, (NTNU).

[12] See New York Times (NYT) 30thApril, 2010 The Bitter Pills in the Plan to Rescue Greece. New York Times, B1. The 'Washington consensus' is a term coined for a specific policy package of liberalization, which pushes the opening up of markets, privatization, the removal of price supports, the balancing of budgets etc. Williamson, John (1994) *The Political Economy of Policy Reform*, Washington, DC: Institute for International Economics.

other critical arguments about the involvement of the IMF as a cause of adverse social consequences, such as civil violence and the violation of human rights. To put it simply, HBB claim that liberalization generates losers, who will have low opportunity costs for organizing armed violence against a state. They do not specify the nature of opportunity costs associated with no reforms. Without knowing theoretically who these losers from liberalization are, and how exactly they foment violence, it is hard to imagine that there would be no losers willing to fight if economic crisis continued without reforms. From their empirical analysis, it is difficult to separate the effects of the immediate crisis from the liberalizing effects of SAPs, if indeed they actually occur when countries sign an agreement.

Studies such as HHB's should be commended for focusing on this important issue and improving on existing studies on IMF programs and armed conflict, a topic that deserves far more attention than has been the case hitherto[13]. They take particular care in solving issues related to biases stemming from endogeneity and selection. Using their dataset and operationalization (making minor changes in addition), however, we find results at odds with their conclusions. For example, their positive and significant effect on the risk of civil war becomes statistically insignificant with a one year lag and becomes negative and statistically highly significant if lagged by three years, which is the time that most stand by agreements typically end and, presumably, when liberalization should have fully set in.[14] Thus, assuming that signing on to an agreement signifies that a country stays in the program for three years and is 'liberalized' by the IMF, the risk of civil war onset is significantly negative. More interestingly, switching their dependent variable, which measures large-scale civil war (1000 battle death threshold), to onsets of conflicts determined at 25 deaths and above consistently

[13] Abouharb and Cingranelli (2007) present comparable results in a study using a similar design. This study covers the years 1981-1999, with the same variable for IMF participation and no time lags. On the other hand, Rowlands and Joseph (2003) find a negative and significant relationship between IMF programs and civil conflict. This study is also based on a limited sample covering the years between 1985 and 1995, and the authors find it is not robust to alternative specification. Previous studies are therefore both limited and ambiguous.

[14] For details on IMF lending programs, see http://www.imf.org/external/np/exr/facts/howlend.htm.

yields negative effects that are statistically highly significant, whether the main variable is contemporaneous or lagged three years. These results hold up when we extend the data by 11 years (until 2008). Once we employ several different operationalizations, however, we find for the most part that the IMF does not matter either way. The rest of this article will examine the theoretical and empirical issues addressed by HHB, present our objections, our alternative empirical strategy and data, our results, and conclude.

3.2 Blaming Doctors for Death?

The IMF's role has developed increasingly from a lender of last resort to a development bank akin to the World Bank. The IMF's lending activities in poor countries have mushroomed in the last three decades (IMF factsheet, 2012)[15]. In 2009, the G20 summit committed to tripling the IMF's funding from about 250 to 750 billion US$ as a part of the official crisis management strategy, thereby increasing their leverage substantially (IMF, 2009). The IMF lends money to countries in trouble on concessional and non-concessional terms well below market rates. In fact, many countries that do sign agreements with the IMF are simply not creditworthy and would be at the mercy of international capital markets with high risk premiums. To ensure that the borrowing country is able to meet payments the IMF gives technical advice and primarily tries to stabilize the macroeconomic environment (Woods, 2006). If the content of IMF programs causes civil wars, however, the international community is complicit in causing underdevelopment since civil war would set the borrowing country back further. However, if the problems associated with the IMF are simply that SAPs are unsuitable, then the advice, as HHB suggest, is simply to tweek SAPs so that the losers are compensated and would therefore be content enough not to organize violence.

[15] http://www.imf.org/external/np/exr/facts/howlend.htm

If, however, the problem of conflict lies in the deeper causes of the structural crises themselves, then a different set of policy priorities must take effect, with reforms pushed far enough so that long-term gain might still be worth the short-term pain. Giving in to the demands of those able to organize costly conflict because they might not like the immediate changes that would increase the gains for all is surely not advisable. There should be no moral dilemmas associated with policies that hurt corrupt generals who steal the public purse, or rich elites who are likely to be behind crises in the first place. HHB argue that liberalization through IMF programs produces winners and losers and lowers the opportunity costs for the losers to participate in civil war, while at the same time reducing the government's ability to compensate them. There are several reasons to be skeptical about these conclusions on theoretical and empirical grounds.

First, it is not at all clear from the main explanatory variable used by HHB (i.e. signing on to a SAP) that liberalization occurs when countries borrow from the IMF because of the problem of moral hazard. Governments that get bailout loans can renege on the implementation of reforms, prolong decisions regarding difficult political choices or getting economic fundamentals right, leading to stalled reforms and further crises (Bird et al. 2004, Collier and Gunning 1999). Indeed, many find that countries receiving IMF loans do not necessarily liberalize after receiving the money (Boockmann and Dreher 2003). Thus, the assumption that SAPs lead to liberalization that causes mass dissent and generates losers is rather unfounded and not clearly demonstrated by HHB. There are several recent papers that show that more liberalized economies avoid civil and ethnic war and generally have less political repression (de Soysa 2011, de Soysa and Fjelde 2010, Mousseau and Mousseau 2008, Steinberg and Saideman 2008). Nor are states liberalizing at greater rates exposed to higher political dissent and political repression (de Soysa and Vadlamannati 2012). If it is not liberalization that matters, then it might be that the IMF, who is the 'doctor', is being blamed

for death, particularly in instances where patients refuse the medicine. Studies looking at the compliance rate of IMF SAPs by countries borrowing from the IMF suggest that, on average, over half of the patients do not take their medicine (Vreeland 2006). HHB anticipate this criticism and suggest that the IMF may cause losers regardless, but as we suggested earlier, governments in crisis are already generating large numbers of losers. On the other hand, what would happen in a country in crisis that has no access to cheap IMF loans?

Secondly, who loses out from SAPs and whose opportunity costs for engaging in costly conflict are affected is not clear at all, which has a direct bearing on the assumption about liberalization's effect. Indeed, several political leaders who implemented SAPs, such as Gerry Rawlings in Ghana and the United National Party in Sri Lanka, have gone on to win popular elections immediately afterwards. In other words, those who dissent against SAPs cannot be readily identified as representative of popular opinion about the implementation of reform and getting economic fundamentals right. Indeed, most people are hurt by economic crisis because of high prices, inflation, resource shortages, and general economic insecurity—HHB remain silent on how some groups may overcome collective action problems in organizing violence relative to others. Stylized-theory in terms of winners and losers in the developing world, such as the Heckhscher-Ohlin-Samuelson and Ricardo-Viner type models, suggest that workers and farmers in poor countries will gain from the opening up and liberalization of markets, whereas capital and domestic rent-seeking forces could lose. Thus, workers and consumers in poor countries could gain when goods become cheaper and access to better quality goods increases. Export-oriented businesses are likely to gain relative to inward-oriented ones. Scrutiny by the IMF is also likely to hurt those rent-seeking coalitions that spend heavily on the military, for example. Privatization and increased competition is also likely to give people access to better quality goods and services at better prices. It is

highly unclear from the theory, however, as to who foments civil war. Generals in the military might organize violence because they lose rents (Collier and Hoeffler 2005).

Ordinary people will experience a very high level of collective action problems for organizing violence. Theoretically, HHB simply suggest that people will lose jobs and government budgets will be cut due to SAPs. This could have been directly modeled quite easily, but they fail to do this. Identifying who actually rebels and the exact reasons for their actions is no easy task, so we do not fault HHB for this, but inferring from a positive effect of signing on to a SAP with increased risk of civil war as the effects of liberalization is completely unwarranted unless one thinks that narrowly-based rent seekers who have access to the appropriate infrastructure to cause violence actually organize costly violence as a public good that brings redress to others. Highly distorted economic fundamentals might also often be the reason why civil war breaks out, particularly at a time when a bad government tries to reform itself. Distorted economies also generate losers. Contrarily, if conflict is driven by the underlying issues that have also caused the crisis, then ignoring the IMF and choosing not to liberalize is not a solution to peace. There is also a danger that governments will use the argument of stability to increase military spending to try and increase security, if reforms require strong states capable of insuring stability.

We also question HHB's empirical analyses on the following two issues primarily: first, if economic liberalization through IMF programs increases the likelihood of civil war by reducing the opportunity costs of losers, we would expect the effect to unfold over some time. A standard IMF program lasts between one to three years, and loans are disbursed in tranches based on how satisfied the IMF is with the reforms undertaken, or when the borrowers reach particular reform and policy benchmarks set by the IMF (Kahn and Sharma 2006). If liberalization or the content of SAPs are bad for people, then we should observe rising grievances and lowered opportunity costs over time. The Liberian case, as presented by HHB,

illustrates this point well because the standby arrangements, which the authors suggest influenced the onset of war, were signed by Liberia between 1980 and 1984, while the actual war did not break out until 1998. If their reasoning is right, then this is because Liberia implemented SAPs according to IMF wishes over those years. Indeed, the evidence from the case they discuss suggests otherwise. Liberia was denied all further credit from the IMF and World Bank in 1985 because the government refused to accept the conditionality after signing (Claessen and Salin 1991). The Bank and Fund left the country the same year (Claessen and Salin 1991: 136). Did Liberia, then, suffer civil war because the country was liberalizing because of the IMF? With this question in mind, we test the proposition that signing only matters for conflict after a period of time has lapsed between signing and proper implementation of reforms. By doing this, we can assess the argument that the content of SAP, rather than the nature of the crisis, matters.

Second, the HHB study is limited to full-scale civil wars covering only armed conflicts with a threshold of 1000 battle related deaths, with at least 100 killed on each side every year. This threshold excludes all but a few of the most brutal conflicts and comprises no more than 74 conflicts. It would be difficult to argue that that the effect of IMF programs is limited to the most brutal intrastate conflicts. Indeed, the various definitions of a full-scale civil war usually include an internal conflict between a state and an organized armed group with a political agenda, with at least 1000 battle related deaths. However, the onset of a particular civil war is often hard to pinpoint, and different scholars use different coding rules to define when a conflict started. The measures vary on how many deaths are needed for a conflict to be counted as active, how multiple civil wars in one country are treated, and whether the starting point of the conflict is the year the number of deaths surpasses the 1000 death threshold, or the year the first killings took place (see e.g. Sambanis (2004) for a thorough review).

In this way, a country could have experienced several years of devastating conflict before it is recorded as a full-scale civil war. Using the 1000 death threshold therefore excludes many relevant conflicts, and has some additional unfortunate consequences. For instance, Chad has suffered several rebellions, insurgencies and struggles for power among various factions, warlords and rebel groups since 1965. Yet, it is not recorded as an onset of civil war until 1994 by the coding rules of Fearon and Laitin (Fearon and Laitin 2003, Hartzell et al. 2010). This is the same year the central government signed up to a program with the IMF with the intention of rebuilding the economy that had been ravaged by roughly three decades of war. It does not make much sense to use a program signed up to in 1994 to predict the onset of a conflict that had been ongoing and caused more than 40000 deaths at the time of signing an IMF agreement (See also Buhaug 2010). By utilizing the PRIO/Uppsala Armed Conflict Dataset (ACD) (Gleditsch et al. 2002, Harbom and Wallensteen 2009), which uses the much lower threshold of 25 battle related deaths in a single year, the onset of civil war in Chad is coded for 1976, 18 years before it is recorded as a civil war by the coding rules of Fearon and Laitin (2003).

This difference in coding rules is also important because civil war destroys infrastructure and the economy in general, which has a direct impact on independent variables such as GDP per capita, economic growth, and external trade. At the same time, the IMF formally included post conflict assistance in the IMF emergency assistance arrangement in 1995, but had been providing technical advice and programs for reconstruction of war-ravaged economies before that through other facilities, as well as in countries where conflict is still active (Gupta 2005). Mozambique, for instance, signed its first economic rehabilitation program with the IMF in 1987 during the civil war. This was as a part of the transformation from a socialist planning economy to a market economy (Michailof et al. 2002). In this case too, HHB recorded the conflict 10 years after relatively large-scale organized violence had

started, and five years before it officially ended. For all of these reasons, it is important to identify the onset of the conflict as early as possible since the onset of conflicts and economic crises are likely to be related. To analyze the relationship between IMF programs and civil war, after replicating HHB results, we use the 25 deaths threshold, which allows greater variance and is rapidly becoming the standard in the field.

3.3 Data and method

Initially, we use the data collected by HHB, which is based largely on Fearon and Laitin's (2003) replication dataset (see table 2.5 in appendix for summary statistics). HHB conduct bivariate probit analysis in order to mitigate problems associated with endogeneity and omitted variables bias. This is because the conditions causing conflict and the conditions that determine why IMF agreements are signed might not be independent of each other. They employ the strategy which accounts for selection effects, or the non-randomness of the chances of being selected into an IMF program, which is a vast improvement on existing studies on the IMF and civil war. Thus, we make no alterations to either their data or method, which makes our analysis directly comparable with theirs. After performing a strict replication of HHB, we then lag their main independent variable to ascertain the effects stemming from the period after signing and then introduce our own variations of IMF signing using a variable indicating whether a country has been in an IMF program for more than five months during a specific year. We do this so that a reasonable amount of time is given for liberalization to take place, which can then be recorded (Dreher 2006).

Our second innovation is to switch the dependent variable from large-scale civil war to insurgencies that already develop after 25 battle deaths in a single year, as well as at least two years of 'no fighting' having taken place. A new onset of war is only coded once these conditions are fulfilled (Gleditsch et al. 2002, Harbom and Wallensteen 2009). After the

initial replication, we push matters further by extending the period of study from 1999 to 2008. First we run HHB's model specification on the extended data, to make sure the models are compatible. We find the same results in all specifications as in the initial analysis.[16] Then we employ variables in the first and second-stage equations that fit more with the standard literature on why the IMF lends, as well as capturing several important correlates of civil war. Our argument is that our extended analysis is more refined in terms of the variables employed. For example, in order to measure regime type HHB use Polity data that are highly sensitive to ongoing violence (Cheibub et al. 2010). They also square the polity measure to model the quadratic effect of democracy on civil war, which is not generally recommended given problems with massive measurement bias associated with such measures (Treier and Jackman 2008) and employing squared term and interpreting it in a non-linear model is not straight forward (Ai and Norton 2003). We simply use a measure of democracy presented as a dummy that is less likely to be influenced by violence as the Polity index is (Cheibub et al. 2010). Another innovation is to add conflict in the neighborhood because of the geostrategic reasons attached to Western intervention in bad neighborhoods (Balla and Reinhardt 2008). For example, historically, Pakistan has received many IMF bailouts due to its strategic importance to the US. Furthermore, we employ a variable measuring the number of years since previous civil war as recommended by Beck, Katz and Tucker (1998) that has become the standard for controlling for time-dependency in civil war models since.

Our extended dataset contains annual observations on all countries for which data are available from 1970 to 2008. In the civil war equation (second stage), we include the standard controls: population size logged, GDP per capita logged, trade to GDP, regime type, conflict history, oil exporting country, and mountainous terrain, for comparability. In order to control for spatial dependence and the fact that the IMF might be influenced by geopolitical factors,

[16] The results from these analyses are presented in table 2.6 in the appendix.

we include a variable capturing whether or not a neighbor is in conflict. In the IMF equation (first stage), we include economic indicators that are known to influence the decision to apply for IMF assistance (Barro and Lee 2005, Bird and Rowlands 2006, Dreher 2004).

The IMF provides loans to countries in crises, and one crucial issue is whether the outcome of conflict is the result of IMF programs or the conditions of the crisis itself. We include measures of both a low economic standing and more sudden economic crises. Countries with a low economic standing sign up to programs with the IMF more often than countries that are better off because they are less creditworthy on open capital markets. Following others, we use the availability of foreign reserves, measured as the total amount of reserves as the number of months of exports of a country, which also doubles as a measure for the extent of a country's vulnerability to external shocks (Barro and Lee 2005). Healthy economic growth reduces the need for IMF assistance, thus we include the economic growth rate. Trade as a percentage of GDP (Trade-to-GDP ratio) is used for the degree of openness because closed economies are known to approach the IMF more often (Bird and Rowlands 2006). Following others, we also include three dummies indicating whether a country has experienced a currency crisis, a debt crisis or a systemic banking crisis (Laeven and Valencia, 2008). Additionally, previous participation in IMF programs also increases participation in new programs (Bird et al. 2004, Vreeland 2003). Thus, following others, we include the number of years a country has been under an IMF program in the equation (Abouharb and Cingranelli 2007).

In order to control for endogeneity, we need an instrument that influences IMF lending but has no independent effect on civil war. Several studies have found that voting patterns in the United Nations General Assembly (UNGA) is related to support from the IMF (Dreher et al. 2009). Indeed, US support is likely to be a key determinant of why a country receives IMF assistance because of the abnormal voting power wielded by the US (Stone, 2004). As we

consider voting patterns in the UN to be unrelated to the likelihood of civil war, we employ Kegley and Hook's measure of voting in line with the US in the United Nations General Assembly (UNGA) as an instrument in the analysis (Kegley and Hook 1991). The Kegley and Hook (1991) measure codes votes in agreement with the US as 1, votes in disagreement as 0, and excludes the abstentions[17]. The resulting numbers are then divided by the total number of votes in UNGA each year (Dreher et al. 2009).

3.4 Results

Table 2.1 reports our main results. In column 1, we are able to replicate the positive and statistically significant effect reported by HHB for their variable measuring signing up to an IMF agreement. The coefficient we obtain of 1.89 is extremely close to that reported by HHB (1.91). In fact, the difference is because of 1 missing observation which occurred due to merging our data with theirs. In column 2, we lag their IMF variable by one year, which makes the result statistically insignificant. Quite surprisingly, lagging the IMF variable by three years (column 3) leads to the reversal of the sign to negative, a result that is statistically highly significant. In other words, three years after signing up to an IMF agreement, a country exhibits a much lower chance of an onset of civil war, ceteris paribus. It is hard to believe that the contemporaneous term of signing up to an IMF program causes war if it is the liberalization process itself that produces the conditions of civil war. It seems far more reasonable to think that the conditions of the crisis, rather than liberalization, are what matters. Given that these tests were based on the 1000 battle deaths threshold, which should take much greater organization and time to take effect, the argument that liberalization

[17] As a further robustness check, we replace Kegley and Hook's (1991) measure of UNGA voting with Thacker (2006), who codes votes in agreement with the US as 1, votes in disagreement as 0, and abstentions as 0.5. The results are the same if we use Thacker's data Thacker, Strom. (2006) The High Politics of Imf Lending. In *Globalization and the Nation State: The Impact of the Imf and the World Bank*, edited by Gustav Ranis, James R. Vreeland and Stephen Kosack, pp. 111–142, London: Routledge.

matters is rather suspect. Indeed, when we enter a term measuring whether a country has been in a program for at least five months of that year, the significant effect of signing up to an IMF program vanishes (column 4).

In columns 5, 6, and 7 of table 2.2, we switch the 1000 battle death threshold to 25 and above using the PRIO-Uppsala armed conflicts data. As seen here, both the contemporaneous term and the three year lagged term are both negative and statistically significant. In other words, there is a much lower chance of an onset of civil war causing more than 25 battle deaths in a single year when countries sign up to an IMF program, as defined and tested by HHB both contemporaneously and with reasonable time lags. As in the case of using the 1000 battle death threshold, again, gauging whether a country was in a program for at least five months during a given year did not have a statistically significant effect when testing for the onset of smaller wars. Additionally, we tried to see whether we could improve the specification of the models and the proxies used. For example, we switched the Polity democracy measures used by HHB with different operationalization of democracy, including a measure of democracy developed by Cheibub et al (2010). We also dropped some variables in the model and added others. At no time did we find a positive and statistically significant effect of signing up to an IMF program on the risk of an onset of civil war. If HHB have the right specification of a model of civil war, then IMF involvement is better associated with peace rather than civil war.

Next, we examine our updated data, including various forms of crises that elicit IMF interventions and support from the US, proxied by UNGA voting in the first stage. As seen in table 2.3, none of the IMF variables significantly predict the onset of a civil war after estimating why some countries are chosen for IMF programs. Notice that our UNGA variable is robustly related to why countries get chosen for IMF programs, as are the variables measuring crisis. Most importantly, our extended data and models show highly similar results

to others, particularly Fearon and Laitin´s (2003) findings where per capita income, population size and oil exports are properly signed and statistically significant (Hegre and Sambanis 2006, Ward et al. 2010). We think our results are generally more comparable to other studies in the field than HHB´s findings. Given that IMF interventions are more likely where crises are present, it is probable that less people suffer because of intervention that may alleviate some pain, given that crises themselves would lead to hardship and austerity by default.

3.4.1 Further Checks on Robustness

We examine the robustness of our main findings using the extended dataset in several ways. First, we use alternative measure of civil war, wherein we use civil war incidence variable coded 1 for each year if a country experiences conflict and 0 otherwise. Although it is equivalent to Fearon and Laitin civil war measure, in this version it records every year if the death toll surpasses 1000 deaths. Replicating the HHB models with their explanatory variables and replicating the extended dataset models with this alternative measure of civil war yields consistent results, i.e. no association between IMF program participation and outbreak of civil war.

Second, we control for country fixed effects along with the time fixed effects (which are already controlled for) using two-stage least squares method (2SLS-IV hereafter), thus estimating linear probability models. We estimate the 2SLS-IV models on our extended dataset in which we regress outbreak of civil war measure on IMF program participation and other explanatory variables. In doing so, we use UNGA voting index as our instrumental variable. Note that we estimate 2SLS-IV models by including time dummies only in one set of models and including time and country dummies in another set of models. The validity of the selected instruments depends on, firstly, instrument relevance, i.e., the instrument must be

correlated with the explanatory variable in question. Bound, Jaeger and Baker (1995) suggest examining the F-statistic on the excluded instruments in the first-stage regression. The selected instruments would be relevant when the first stage regression model's F-statistics meets the thumb rule threshold of being above 10 (Staiger and Stock 1997). Secondly, the instrument variable should not vary systematically with the disturbance term in the second stage equation, i.e. $[\omega_{it} \mid IV_{it}] = 0$ meaning, the instruments cannot have independent effects on the dependent variable. The Hansen J-test, developed by Hansen (1982) is employed to check whether the selected instruments satisfy the exclusion restriction. The joint F-statistic in our models is always above 10 with statistical significance of 1% level[18] and the Hansen J-test of over identifying restrictions accepts the null hypothesis that the instruments are valid. After controlling for endogeneity associated with IMF program participation, and for country fixed effects, we do not find any difference in relation to the effects of IMF program participation. The impact of IMF programs on both civil war measures (Fearon and Laitin 2003 and ACD) remains statistically insignificant. Our extended data show that IMF involvement is a poor predictor of conflict, results supported by others who have used other data and methods examining this crucial question for African countries (Bussmann et al. 2005). However, if HHB´s data and models are more representative, then perhaps IMF involvement does alleviate the risk of civil war.

3.5 Conclusion

Despite the massive importance of IMF lending, few studies have looked at the impact of the IMF on the risk of civil war. Recently, Hartzell et al (2010) provided an interesting study on

[18] Apart from joint F-statistics, we also use Cragg-Donald statistic test, developed by Cragg and Donald (1993), of the null that the model is under identified, i.e. that UNGA voting index does not sufficiently identify IMF program participation. However, the Cragg-Donald test allows us to overwhelmingly reject the null hypothesis that the model is under identified.

the effects of IMF lending on the risk of civil war. Indeed, their study, which takes the issue of non-randomness between IMF programs and civil war seriously, is a massive improvement on previous empirical studies on the subject. Using this study as a benchmark, we investigate the issue further by critically examining some fundamental assumptions in their study, namely that the IMF actually liberalizes countries and that liberalization is what causes the conditions of conflict, rather than crisis itself. Unfortunately, their explanation of the connection is neither grounded well in theories explaining who wins and who loses from liberalization, nor who is in a position to rebel when IMF programs take effect.

Given their explanation, the use of an empirical strategy that employs a contemporaneous term for signing on to an IMF program is also suspect. With only minor adjustments to their own data, particularly the use of a dependent variable measuring the onset of a civil war at a much lower threshold for deaths, we find ample evidence suggesting that the relationship they report may in fact be the opposite. Our extended data, as well as a better operationalization of a model of civil war and the endogeneity between conditions of crisis and IMF involvement, found no connection—either positive or negative—between participation in IMF programs and the risk of civil war. Further research is clearly needed on the subject, particularly the use of alternative operationalizations of IMF involvement and data measuring the degree to which governments liberalize. Indeed, two variables, namely per capita income and population size, seem to explain most of the variance in the onset of civil wars (Ward et al. 2010). Thus conclusively, explaining how economic crises, as well as the conditions of rent-seeking and other maladies that lead to crises also generate conditions conducive to civil war, might be where scholars should look in future studies in order to find out how the IMF impacts upon civil war.

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Table 2.1: Bivariate Probit Estimations Replicating Hartzell et al. (2010)

	Fearon & Laitin >1000 Model 1	Fearon & Laitin >1000 Model 2	Fearon & Laitin >1000 Model 3	Fearon & Laitin >1000 Model 4
Civil war equation				
Signed IMF program	1.885***			
	(0.593)			
Signed IMF program (t-1)		0.113		
		(1.089)		
Signed IMF program (t-3)			-1.917***	
			(0.240)	
IMF participation > 5months				0.282
				(0.711)
Per capita GDP (log)	-0.069	-0.091	-0.166***	-0.077
	(0.043)	(0.063)	(0.023)	(0.058)
Economic growth (log)	-0.002	-0.005	0.003	-0.005
	(0.007)	(0.008)	(0.003)	(0.008)
Foreign reserves	-0.027	-0.033	-0.046**	-0.030
	(0.028)	(0.031)	(0.021)	(0.034)
Democracy	0.004	0.004	0.003	0.003
	(0.014)	(0.016)	(0.012)	(0.016)
Democracy squared	-0.007***	-0.007***	-0.003	-0.007***
	(0.002)	(0.002)	(0.003)	(0.002)
Population size (log)	0.053	0.048	-0.010	0.052
	(0.040)	(0.049)	(0.030)	(0.048)
Oil	0.397*	0.495**	0.284	0.482**
	(0.225)	(0.230)	(0.228)	(0.223)
Previous civil war	-0.090	-0.154	-0.207**	-0.147
	(0.112)	(0.123)	(0.101)	(0.119)
Years under IMF program	0.006	0.011	0.025***	0.008
	(0.008)	(0.010)	(0.005)	(0.013)
Mountainous terrain	0.101**	0.114**	0.039	0.114**
	(0.048)	(0.055)	(0.056)	(0.054)
Constant	-2.239***	-2.141***	-0.357	-2.275***
	(0.387)	(0.562)	(0.319)	(0.522)
Signed IMF program equation				
GDP per capita (log)	-0.133***	-0.127***	-0.118***	-0.194***
	(0.018)	(0.017)	(0.017)	(0.027)
Economic growth (log)	-0.011*	-0.014**	-0.010	-0.007
	(0.006)	(0.006)	(0.009)	(0.005)
Foreign reserves	-0.073***	-0.039**	-0.040**	-0.063**
	(0.024)	(0.018)	(0.017)	(0.025)
Democracy	0.015*	0.008	0.002	0.003
	(0.008)	(0.008)	(0.007)	(0.009)
Democracy squared	-0.000	0.000	0.000	-0.002
	(0.002)	(0.002)	(0.001)	(0.002)
Population size (log)	-0.006	-0.022	-0.028	-0.041
	(0.030)	(0.028)	(0.028)	(0.040)
Oil	0.032	0.106	0.118	0.003
	(0.102)	(0.101)	(0.095)	(0.178)
Previous civil war	0.028	-0.093	-0.134	-0.120
	(0.093)	(0.097)	(0.124)	(0.170)
Years under IMF program	0.015**	0.020***	0.024***	0.033***
	(0.006)	(0.006)	(0.006)	(0.007)
Number countries under IMF program	-0.007**	0.000	0.003	0.015***

	(0.003)	(0.004)	(0.002)	(0.005)
Constant	-0.120	-0.430	-0.515*	-0.187
	(0.307)	(0.325)	(0.288)	(0.451)
Number of Observations	2404	2404	2158	2404
Log likelihood	-1103.683	-1129.373	-1017.780	-1275.498
rho	-0.889	-0.031	0.997	-0.168

Robust standard errors in parentheses. Independent variables in civil war equation lagged following Hartzell et al. (2010). Independent variables in signed IMF equation lagged according to the lags in the IMF variable * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.2: Bivariate Probit Estimation Replicating Hartzell et al. with Civil wars above 25 Battle deaths as Dependent Variable

	ACD >25 battle deaths	ACD >25 battle deaths[19]	ACD >25 battle deaths[20]
	Model 5	**Model 6**	**Model 7**
Civil war equation			
Signed IMF program	-1.050**		
	(0.454)		
Signed IMF program (t-1)		-0.309	
		(2.695)	
Signed IMF program (t-3)			-1.430***
			(0.190)
Per capita GDP (log)	-0.108***	-0.086	-0.119***
	(0.033)	(0.063)	(0.022)
Economic growth (log)	0.003	0.008	0.005
	(0.007)	(0.012)	(0.004)
Reserves	-0.027	-0.003	-0.022
	(0.023)	(0.037)	(0.016)
Democracy	0.031***	0.032***	0.023**
	(0.010)	(0.011)	(0.009)
Democracy squared	-0.004*	-0.005*	-0.003
	(0.002)	(0.002)	(0.002)
Population (log)	0.140***	0.160**	0.087*
	(0.052)	(0.064)	(0.046)
Oil	0.321*	0.378**	0.295*
	(0.174)	(0.189)	(0.152)
Previous war	-0.207	-0.198	-0.201*
	(0.149)	(0.165)	(0.112)
Years under IMF program	0.008	0.004	0.017**
	(0.007)	(0.017)	(0.007)
Mountainous terrain	0.008	0.017	0.012
	(0.042)	(0.048)	(0.032)
Constant	-2.249***	-2.886***	-1.493***
	(0.704)	(1.092)	(0.476)
Signed IMF program equation			
Per capita GDP (log)	-0.133***	-0.125***	-0.122***
	(0.018)	(0.017)	(0.015)
Growth (log)	-0.011*	-0.014**	-0.008
	(0.006)	(0.006)	(0.006)
Reserves	-0.073***	-0.039**	-0.045**
	(0.024)	(0.017)	(0.019)
Democracy	0.014*	0.008	0.009
	(0.008)	(0.007)	(0.007)
Democracy squared	-0.000	0.000	0.000
	(0.002)	(0.002)	(0.002)
Population size (log)	-0.003	-0.020	-0.024
	(0.028)	(0.028)	(0.027)
Oil	0.055	0.101	0.148
	(0.099)	(0.102)	(0.113)
Previous war	-0.035	-0.083	-0.152
	(0.101)	(0.107)	(0.100)
Years under IMF program	0.016**	0.020***	0.034***

[19] Dependent variables in civil war equation t-1, in IMF equation t-2
[20] Dependent variables in civil war equation t-1, in IMF equation t-4

	(0.006)	(0.006)	(0.005)
Number countries under IMF program	-0.008**	0.000	-0.003
	(0.003)	(0.004)	(0.004)
Constant	-0.111	-0.433	-0.357
	(0.301)	(0.362)	(0.334)
Number of Observations	2404	2391	2145
Log likelihood	-1254.497	-1272.738	-1153.294
rho	0.650	0.056	0.892

Robust standard errors in parentheses. Independent variables in signed IMF equation lagged according to the lags in the IMF variable * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.3: IMF Interventions and the Onset of Civil War using Extended Data, 1970-2008

	ACD >25	ACD >25	ACD >25
	Model 8[21]	**Model 9[22]**	**Model 10**
Civil war equation			
IMF signed (t-1)	0.295		
	(0.725)		
IMF signed (t-3)		-0.158	
		(0.490)	
IMF participation > 5months			0.628
			(0.391)
Population (log)	0.193***	0.196***	0.189***
	(0.042)	(0.043)	(0.041)
Per capita GDP (log)	-0.190***	-0.217***	-0.119
	(0.068)	(0.072)	(0.077)
Trade (% of GDP)	0.002	0.002	0.002
	(0.002)	(0.002)	(0.002)
Democracy	0.126	0.073	0.134
	(0.123)	(0.129)	(0.123)
Peace years	-0.009*	-0.010	-0.012**
	(0.006)	(0.006)	(0.006)
Oil	0.388***	0.378**	0.414***
	(0.148)	(0.153)	(0.142)
Neighbor at war	-0.015	-0.090	-0.005
	(0.135)	(0.142)	(0.131)
Mountainous	-0.001	0.000	0.000
	(0.003)	(0.003)	(0.003)
Years under IMF	-0.000	-0.001	-0.007
	(0.008)	(0.011)	(0.008)
Constant	-8.234	-8.018	-8.689
		(6.654)	(12.025)
Signed IMF program			
Per capita GDP (log)	-0.251***	-0.224***	-0.571***
	(0.031)	(0.032)	(0.030)
GDP Growth	-0.014***	-0.014***	-0.004
	(0.005)	(0.005)	(0.004)
Trade (% of GDP)	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)
Foreign Reserves	-0.067***	-0.071***	-0.035***
	(0.015)	(0.015)	(0.012)
Currency crisis	0.349**	0.350**	0.175
	(0.140)	(0.140)	(0.145)
Debt crisis	0.388*	0.348	1.211***
	(0.212)	(0.223)	(0.260)
Banking crisis	0.253	0.263*	0.236
	(0.157)	(0.159)	(0.155)
Ongoing civil war	-0.197**	-0.173**	-0.370***
	(0.087)	(0.085)	(0.077)
UNGA voting	1.895***	1.688***	3.493***
	(0.554)	(0.588)	(0.453)
Years under IMF	0.036***	0.040***	0.049***
	(0.004)	(0.004)	(0.003)
Constant	-0.221	-0.357	1.256**

[21] Independent variables in civil war equation t-1, in IMF equation t-2
[22] Independent variables in civil war equation t-1, in IMF equation t-4

	(0.576)	(0.570)	(0.612)
Number of Observations	2769	2572	2853
Log likelihood	-1346.603	-1284.185	-1628.340
Rho	-0.214	0.238	-0.434

Standard errors in parentheses; Time dummies not shown; Independent variables in signed IMF program equation lagged according to the IMF variable in the civil war equation. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.7 Appendix

Table 2.4: List of countries

Afghanistan	Colombia	Honduras	Mauritania	Singapore
Albania	Comoros	Hungary	Mauritius	Slovak Republic
Algeria	Congo. Dem. Rep.	Iceland	Mexico	Slovenia
Angola	Congo. Rep.	India	Moldova	Solomon Islands
Argentina	Costa Rica	Indonesia	Mongolia	Somalia
Armenia	Cote d'Ivoire	Iran. Islamic Rep.	Montenegro	South Africa
Australia	Croatia	Iraq	Morocco	Spain
Austria	Cuba	Ireland	Mozambique	Sri Lanka
Azerbaijan	Cyprus	Israel	Myanmar	Sudan
Bahamas. The	Czech Republic	Italy	Namibia	Suriname
Bahrain	Denmark	Jamaica	Nepal	Swaziland
Bangladesh	Djibouti	Japan	Netherlands	Sweden
Barbados	Dominican Republic	Jordan	New Zealand	Switzerland
Belarus	Ecuador	Kazakhstan	Nicaragua	Syrian Arab Republic
Belgium	Egypt. Arab Rep.	Kenya	Niger	Tajikistan
Belize	El Salvador	Korea. Dem. Rep.	Nigeria	Tanzania
Benin	Equatorial Guinea	Korea. Rep.	Norway	Thailand
Bhutan	Eritrea	Kuwait	Oman	Timor-Leste
Bolivia	Estonia	Kyrgyz Republic	Pakistan	Togo
Bosnia and Herzegovina	Ethiopia	Lao PDR	Panama	Trinidad and Tobago
Botswana	Fiji	Latvia	Papua New Guinea	Tunisia
Brazil	Finland	Lebanon	Paraguay	Turkey
Brunei Darussalam	France	Lesotho	Peru	Turkmenistan
Bulgaria	Gabon	Liberia	Philippines	Uganda
Burkina Faso	Gambia. The	Libya	Poland	Ukraine
Burundi	Georgia	Lithuania	Portugal	United Arab Emirates
Cambodia	Germany	Luxembourg	Qatar	United Kingdom
Cameroon	Ghana	Macedonia. FYR	Romania	United States
Canada	Greece	Madagascar	Russian Federation	Uruguay
Cape Verde	Guatemala	Malawi	Rwanda	Uzbekistan
Central African Republic	Guinea	Malaysia	Saudi Arabia	Venezuela. RB
Chad	Guinea-Bissau	Maldives	Senegal	Vietnam
Chile	Guyana	Mali	Serbia	Zambia
China	Haiti	Malta	Sierra Leone	Zimbabwe

Table 2.5: Descriptive Statistics

HHB data	Mean	Standard Deviation	Minimum	Maximum	Observations
Civil war onset > 1000 battle deaths	0.017	0.130	0.000	1.000	4238
Signed IMF program	0.141	0.348	0.000	1.000	4226
GDP per capita (log)	4.259	4.730	0.196	41.021	4094
Economic growth (lagged)	1.634	6.331	-41.900	63.630	3303
Foreign reserves (months of exports) (lagged)	3.479	2.988	0.000	25.000	2760
Polity2, Index of democracy	-0.329	7.572	-10.000	10.000	4219
Polity 2 squared	57.435	30.890	0.000	100.000	4219
Population (log, lagged)	9.079	1.481	5.403	14.030	4238
Oil	0.151	0.358	0.000	1.000	4238
Previous civil war	0.146	0.353	0.000	1.000	4238
Number of countries under IMF program	6.037	7.297	0.000	35.000	4225
Mountainous terrain	2.090	1.431	0.000	4.557	4238
Years under IMF program	44.550	16.016	21.000	72.000	4078
Index of economic freedom (interpolated)	5.733	1.111	2.300	8.800	2836
IMF program > 5 months	0.197	0.398	0.000	1.000	5760
Civil war onset >25 deaths (ACD)	0.036	0.185	0.000	1.000	4622
Democracy dummy (Cheibub et al.)	0.409	0.492	0.000	1.000	4832
Extended analysis					
ACD onset >25 battle deaths	0.033	0.179	0.000	1.000	6013
Signed IMF program	0.127	0.333	0.000	1.000	6013
IMF program >5 months	0.255	0.436	0.000	1.000	6013
Population (log)	15.763	1.707	11.620	20.999	5874
GDP per capita (log)	7.389	1.588	4.046	11.686	5521
Trade (% GDP)	74.557	43.266	0.309	438.091	5372
Democracy (Cheibub et al)	0.429	0.495	0.000	1.000	5985
Peace years	11.598	10.978	0.000	38.000	6013
Oil	0.147	0.354	0.000	1.000	5974
Neighbor at war	0.156	0.363	0.000	1.000	6013
Mountainous terrain	17.347	21.659	0.000	94.300	5470
Years under IMF	7.768	8.778	0.000	42.000	5869
Growth	4.016	10.049	-88.086	446.865	4311
Foreign reserves (months of exports)	3.634	3.293	-0.092	43.694	4384
Currency crisis	0.035	0.183	0.000	1.000	5767
Debt crisis	0.011	0.102	0.000	1.000	5767
Systemic banking crisis	0.021	0.144	0.000	1.000	5767
Ongoing civil war	0.170	0.375	0.000	1.000	6013
Voting in line with the US in the UNGA	0.194	0.116	0.000	0.734	5581
Anocracy	0.206	0.405	0.000	1.000	6013
Signed SAP program with the IMF	0.006	0.077	0.000	1.000	6013
Civil war onset (Fearon & Laitin 2003)	0.017	0.131	0.000	1.000	4086

Table 2.6: Further checks on Robustness

Civil war equation	Fearon Laitin >1000 deaths	ACD >1000 deaths	ACD[23] >1000 deaths	ACD >1000 deaths	ACD >25 deaths	ACD >25 deaths
Signed IMF agreement	2.098***	1.120			-0.255	
	(.531)	(1.787)			(0.509)	
Signed IMF agreement (t-1)			-1.443***			
			(.353)			
IMF participation >5 months				-1.152**		-0.041
				(0.420)		(0.474)
Per Capita GDP (lagged)	-0.097	-0.245*	-0.334***	-0.427***	-0.241***	-0.233**
	(0.134)	(0.130)	(0.065)	(0.074)	(0.065)	(0.091)
Economic growth (lagged)	0.015	-0.016	-0.016**	-0.018**	0.006	0.007
	(0.010)	(0.022)	(0.008)	(0.009)	(0.005)	(0.005)
Foreign reserves (lagged)	0.012	-0.069	-0.075**	-0.057*	0.014	0.016
	(0.045)	(0.051)	(0.025)	(0.033)	(0.017)	(0.016)
Democracy (Polity IV)	0.014	0.025	0.020	0.022*	0.026**	0.026**
	(0.020)	(0.024)	(0.014)	(0.013)	(0.010)	(0.010)
Democracy squared	0.001	0.006	0.004	-0.001	-0.002	-0.002
	(0.004)	(0.005)	(0.003)	(0.005)	(0.002)	(0.003)
Population size (lagged)	-0.117	0.269**	0.188**	0.250**	0.144***	0.149***
	(0.082)	(0.100)	(0.093)	(0.080)	(0.035)	(0.036)
Oil	0.929**	0.799**	0.518**	0.668**	0.425**	0.433**
	(0.400)	(0.370)	(0.245)	(0.217)	(0.145)	(0.148)
Previous civil war	6.888***	5.318***	-0.066			
	(1.506)	(0.333)	(0.162)			
Years since previous war				-0.017	-0.009	-0.009
				(0.012)	(0.006)	(0.006)
Mountainous terrain	0.001	0.003	0.004	0.006	0.001	0.002
	(0.004)	(0.006)	(0.003)	(0.004)	(0.003)	(0.003)
Years under SA	-0.027**	-0.026	0.008	0.006	-0.002	-0.004
	(0.013)	(0.016)	(0.008)	(0.010)	(0.008)	(0.009)
Constant	-5.793**	-10.293***	-2.488	-3.145*	-2.709**	-2.874**
	(2.267)	(2.043)	(1.904)	(1.759)	(0.832)	(1.080)
IMF participation equation						
GDP Per Capita (lagged)	-0.211***	-0.232***	-0.232***	-0.516***	-0.234***	-0.517***
	(0.039)	(0.034)	(0.033)	(0.033)	(0.034)	(0.033)
Economic growth (lagged)	-0.016**	-0.017**	-0.020***	-0.007	-0.017**	-0.007
	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)	(0.004)
Foreign reserves (lagged)	-0.081***	-0.064***	-0.063***	-0.034**	-0.064***	-0.033**
	(0.018)	(0.014)	(0.014)	(0.010)	(0.014)	(0.010)
Democracy (Polity IV)	0.003	0.004	0.003	0.006	0.005	0.007
	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Democracy squared	-0.004**	-0.003**	-0.002*	-0.007***	-0.003**	-0.007***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Population size (lagged)	-0.009	-0.039	-0.027	-0.085***	-0.043*	-0.085***
	(0.029)	(0.025)	(0.024)	(0.020)	(0.024)	(0.020)
Oil	0.046	0.058	0.045	0.149	0.060	0.142
	(0.109)	(0.098)	(0.098)	(0.091)	(0.098)	(0.091)
Previous civil war	-0.222**	-0.102	-0.146*			
	(0.096)	(0.082)	(0.084)			
Years since previous war				0.011***	0.002	0.012***
				(0.003)	(0.003)	(0.003)
Years under SA	0.025***	0.026***	0.025***	0.041***	0.026***	0.041***
	(0.005)	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)
Voting with the US in the UN	1.202**	1.820***	1.361***	3.192***	1.846***	3.172***
	(0.515)	(0.396)	(0.343)	(0.364)	(0.393)	(0.369)
Constant	0.699	0.889**	0.779*	3.925***	0.935**	3.935***
	(0.511)	(0.453)	(0.456)	(0.399)	(0.456)	(0.400)
Observations	1957	2867	2733	2867	2867	2867
Log pseudolikelihood	-946.4	-1186.9	-1142.2	-1460.4	-1464.2	-1727.8
rho	-.927	-.603	.969**	.736**	.206	-.006

Robust standard errors in parentheses. Time dummies not shown. * p <.10, ** p <.05, *** p <.01

[23] Independent variables in the civil war equation t-1, in the IMF equation, t-2

4. Chapter 3

A Race to the Bottom in Labour Standards?[24]

4.1 Introduction

While many concerns have been expressed over the impact of increasing globalization, many of them centre on the possibility of a race to the bottom in which governments seek to attract foreign direct investment (FDI) by removing policies that, although potentially socially desirable, are viewed as unattractive to firms. This worry has been expressed in the arenas of taxation, environmental regulation, and labour standards, among others. While there is a growing literature estimating the extent of the race to the bottom in international taxation and environmental policies, to our knowledge to date there is no evidence on the potential race to the bottom in labour standards. This is the gap the current paper fills. Using panel data on 148 developing countries from 1985 to 2002, we utilize spatial econometric methods to estimate whether the Mosley (2011) and Mosley and Uno (2007) measure of labour rights in one country depends on those elsewhere. For the full sample, we find a significant and positive spatial lag, which is consistent with strategic complements and a necessary condition for there to be a race to the bottom. In particular, this seems to be driven primarily by competition in labour practices rather than labour laws, suggesting that competition is driven less by a failure

[24] Coauthored with Ronald B. Davies, University College Dublin.

to institute regulations than by an unwillingness to enforce them. Since there is a noticeable downward trend in both of these measures over the sample period, we take this as evidence of a race to the bottom.

Although there has been less attention paid to the potential for a race to the bottom in labour standards as compared to one in taxes or environmental policies, the essence of the argument is the same. Labour standards such as the right of collective bargaining result in higher labour costs. All else equal, mobile investment would prefer a location with weaker standards and lower costs. Evidence of FDI being deterred by labour standards is provided by Görg (2002), Javorcik and Spatareanu (2005), and Dewit et al. (2009). It should be noted, however, that there is disagreement on this issue, with Kucera (2002) and Rodrik (1996) providing dissenting opinions.[25] The issue of how FDI depends on standards, however, is a very different question from the one we ask, which is whether labour standards in one location depend on those in another.[26] In particular, even if FDI does not flow in as a result of a country's reduction in labour standards, if politicians believe that it does then this alone could result in a race to the bottom.

Although to our knowledge no one has attempted to estimate the extent of the race to the bottom in labour standards before, spatial econometrics have been used to look for a race to the bottom in taxes and in environmental standards. The first group of work includes Devereux, Lockwood, and Redoano (2008), Davies and Voget (2008), Overesche and Rinke (2009) and others. Generally, this work has focused on tax competition between developed countries where there is some evidence of a positive spatial lag, meaning that as tax rates fall

[25] One possible reason they provide is that operating in a high standards location provides consumers a guarantee on how a firm treats its workers. As such, they may be willing to pay more for the firm's product on humanitarian grounds. See Greenhill et al. (2009) for a full discussion. In addition, there is evidence that increased FDI may improve labour standards (Mosley 2011, Davies and Voy 2009, Neumayer and de Soysa 2005).

[26] Greenhill et al. (2009) do test to see whether the "practice content of trade" is a predictor for a given nation's labour standards. However, although they do control for the potential endogeneity of trade volumes, they do not deal with potential endogeneity in standards that would result from competition.

in one nation, this lowers tax rates elsewhere. An exception to this is Klemm and van Parys (2009) who focus on Latin America and Africa, finding that they compete in tax holidays. In the environmental literature, the focus has been on two issues: the joint adoption of environmental agreements (including the work of Beron et al. (2003), Murdoch et al. (2003) and Davies and Naughton (2006)) and interaction in environmental policies (which includes Fredriksson and Millimet (2002), Levinson (2003) and Fredriksson et al. (2004)). These studies tend to find evidence consistent with the race to the bottom. However, due to data limitations, many of them either restrict their attention to developed countries or to competition across US states. Davies and Naughton (2006) are an exception to this, who find that developed countries affect the treaty participation of both developed and developing nations whereas the developing nations only tend to impact themselves.

For our full sample using GDP weights (which assume that a given nation pays more attention to the standards in larger economies), our estimates find that a standard deviation decline in labour standard elsewhere leads a given country to lower its own standards by 3.8%. Although this magnitude varies somewhat when weighting by per-capita GDP or trade openness, the qualitative result is the same. When we decompose our measure of labour standards into its components – the laws guaranteeing labour rights (laws) and the enforcement of those laws (practices) – we find evidence primarily when using labour practices. This holds for both significance and magnitude of the estimated impact. This suggests that while countries may well attempt to "put on a good face" by instituting labour-friendly laws for reasons similar to those discussed by Kucera (2002), they may then be competing for FDI by simply turning a blind eye towards violations of those laws (or are simply unable to adequately enforce them). This finding is also notable because both laws and practices have similar trends, indicating our finding for practices is causal rather than the result of an uncontrolled for time trend. We also estimate our model for subsamples of the

data. These estimates reveal that the competition is primarily driven by countries with weak standards, occurs both in relatively poor and relatively rich countries, and is strongest for Latin America and the Middle East. Again, these differences stand out against a similar downward trend in standards for each group and region, suggesting that we are capturing evidence of interdependence in standards instead of a mere trend.

The paper proceeds as follows. Section 4.2 describes both our data and our methodology. Section 4.4 discusses the results and Section 4.5 concludes.

4.2 Data and method

In this section, we describe both our data, which is a panel data set across 148 countries from 1985 to 2002, and our estimation specification.

4.2.1 Estimation Specification

Our baseline specification estimates the labour standards in country *i* in year *t* as a function of a set of exogenous variables $X_{i,t}$ (which includes a lagged dependent variable):

$$LR_{i,t} = \beta_i + \beta X_{i,t} + \varepsilon_{i,t} \quad (1)$$

where β_i is the country-specific constant and $\varepsilon_{i,t}$ is the error term. Our control variables are drawn from the existing literature and are described below. To this baseline, we then introduce the labour rights in other countries in year *t*, a variable known in the literature as the spatial lag. Specifically, we estimate:

$$LR_{i,t} = \beta_i + \rho \sum_{j \neq i} \omega_{j,i,t} LR_{i,t} + \beta X_{i,t} + \varepsilon_{i,t} \quad (2)$$

where $\sum_{j \neq i} \omega_{j,i,t} LR_{i,t}$ is the spatial lag, i.e. the weighted average of labour standards in the other countries. As our baseline weights, we utilize $\omega_{j,i,t} = \frac{GDP_{j,t}}{\sum_{k \neq i} GDP_{k,t}}$. In words, the share that country *i* gives to country *j* is equivalent to *j*'s share of the total GDP across countries not

including country *i*.[27] Our rationale for using GDP as the weight is two-fold. First, one might anticipate that country *i* pays more attention to what is taking place in larger countries rather than small ones. Second, if the goal of manipulating labour standards is to attract FDI, the choice of labour standards will depend on the elasticity of FDI to a given country's policies. With this in mind, if country *j* is already attractive to FDI relative to country *k*, then a change in *j*'s labour standards may have a larger impact on the allocation of FDI than a comparable change in *k*. This in turn would make *i* more responsive to *j*'s labour standards than to *k*'s, a difference that (2) reflects by giving a greater weight to *j*.[28] Since, as confirmed in many studies and reviewed by Blonigen (2005), FDI is attracted to larger countries, this would imply a greater sensitivity on the part of country *i* to the labour standards of a large country. GDP has been used as a weight in several papers estimating the race to the bottom in taxation (Devereux, Lockwood, and Redoano, 2008, for example). In addition, we check our results by using two additional weights, $\omega_{j,i,t} = \frac{\text{per-capita GDP}_{j,t}}{\sum_{k \neq i} \text{per-capita GDP}_{k,t}}$ and $\omega_{j,i,t} = \frac{Openness_{j,t}}{\sum_{k \neq i} Openness_{k,t}}$ where $Openness_{j,t}$ is the sum of exports plus imports relative to GDP (a common proxy for the inverse of trade costs in the empirical FDI literature). For both of these, our rationale is comparable to the choice of GDP since FDI is often attracted to wealthier and more open countries. Nevertheless, since high per-capita GDP can be correlated with wage costs thus deterring vertical FDI (in which MNE output is intended for export out of the host) and greater openness reduces the need for horizontal FDI (in which FDI is intended to replace exports to the host), the net impact of these factors is less clear-cut than GDP. Indeed, as discussed by Blonigen (2005), the literature finds mixed results for these variables. We

[27] As described by Anselin (1988), it is common to "row standardize" the weights so that the sum of the weights adds up to one.

[28] Baldwin and Krugman (2004) provide a model of precisely this issue for tax competition in which a large country, by virtue of its attractive domestic market, has a greater impact on FDI flows than a small country does.

therefore rely on the GDP weights for our primary results and use these alternatives as robustness checks.

The difficulty with the spatial lag is that if labour standards in *i* depend on those in *j* and vice versa, the spatial lag is endogenous. We deal with this and the lagged dependent variable by using the Blundell and Bond (1998) SYS-GMM estimator accounting for the Windmeijer (2005) correction.[29] In addition to using lagged values of the endogenous variables as instruments, we also follow standard spatial econometric procedure and use $\sum_{j \neq i} \omega_{j,i,t} X_{j,t}$,that is, the weighted average of the other nations' exogenous variables (but excluding their lagged dependent variables).[30] The intuition behind doing so is that for a given country *j*, its exogenous variables directly impact its own labour standards but are not directly dependent on those in *i*. Therefore they are correlated with the endogenous variable but are themselves exogenous, making them suitable instruments. Within the literature on the SYS-GMM estimator, there is concern regarding the potential inclusion of too many instruments (Roodman 2009a, 2009b). Therefore, in the reported results, we restrict the lag structure to *t-3* and *t-5*. The reason for using these years is that, when including *t-2* lags, our instruments failed to pass the exogeneity tests. Nevertheless, we experimented with a number of alternative sets of instruments (such as excluding some or all of the weighted average of the other nations' exogenous variables) and found qualitatively similar results in all cases.[31]

[29] In unreported results, we also utilized IV GMM estimation rather than one which deals with lags. The primary difference is that when doing so, we typically found a significantly positive spatial lag when using GDP weights. Thus, on the whole, our results are robust to alternative methods of controlling for endogeneity. These alternative results are available on request.

[30] In addition to the variants described below, we estimated a set of regressions in which all control variables, including the instruments for the spatial lag, were lagged one period. This was done in order to alleviate potential concerns that variables such as GDP (both in country *i* and elsewhere) might be affected by the labour standards *i* uses in year *t* (such as might be the case if it is able to attract FDI to the benefit of its GDP and the detriment of others). These alternative regressions yielded qualitatively identical results to those presented.

[31] These alternate results are available on request.

This baseline specification is modified to explore the robustness of our findings. The specifics of these modifications are described below.

4.2.2 Data

We use annual data for 148 countries from 1985 to 2002. The list of countries is reported in table 3.10 in the appendix. For our dependent variable, we use Mosley (2011) and Mosley and Uno's (2007) all-inclusive Labour Rights index constructed annually from 1985 to 2002 for 148 countries. This composite index, capturing "basic collective labour rights", follows the template of Kucera (2002), which covers 37 types of violations of labour rights under six different categories.[32] These six categories are (a) freedom of association and collective bargaining-related liberties, (b) the right to establish and join worker and union organizations, (c) other union activities, (d) the right to bargain collectively, (e) the right to strike, and (f) rights in export processing zones.[33] It is noteworthy however that the Mosley index does not capture aspects of labour standards such as minimum wages or individual labour rights like employment benefits and working conditions.

In each of these above mentioned six categories, violations of labour rights by the government or employers (be they local or foreign firms) are identified as an absence of legal rights, limitations on legal rights and/or a violation of those legal rights. The index then accounts for both the *de jure* (laws) labour standards and the *de facto* (practices) standards prevailing in a country. The law component of the index, which covers 21 of the 37 categories

[32] As such, it is an improvement over other measures of labour rights or standards which capture only a single factor, such the number of ILO conventions (Botero et al. 2000), rate of worker injuries (Bonnal 2008) or a single subjective index (Cingranelli and Richards 1999).

[33] These categories are line with those laid out by the *Declaration on Fundamental Principles and Rights at Work* adopted by ILO member states in June 1998. This declaration identified the core or fundamental labour rights as including the freedom of association (right to unionize), effective recognition of the right to collective bargaining (right to bargain and protest), elimination of all forms of forced or compulsory labour, effective abolition of child labour, elimination of discrimination with respect to employment and occupation and respect to minimum wages and hours of work.

in the index, captures whether or not the required laws to safeguard the collective rights of workers, for example whether an industry is allowed to impose limits on workers' right to strike or bargain collectively, are in place. The practices component, meanwhile, captures the actual number of violations observed in the labour rights prescribed in the laws. Thus, the practices component captures whether there are any registered acts of violations of the laws governing labour standards.

To construct the index, Mosley and Uno (2007) drew upon information from the US State Department's annual country reports on human rights practices, reports from both the Committee of Experts on the Application of Conventions and Recommendations (CEACR) and the Committee on Freedom of Association (CFA), and the annual surveys on violations of trade union rights which published by the International Confederation of Free Trade Unions (ICFTU).[34] If the information from all three sources displays violation of labour rights over the year, Mosley and Uno (2007) assigned a score of 1 for the relevant one of the 37 indicators for a country. If this is not the case a score of 0 is assigned.[35] Then, using the recommendation of two experts and following Kucera's (2002) methodology, weights were assigned to each of the indicators and the index was constructed. This resulted in a labour rights index which was coded on a scale of 0 – 28.5 and a labour practices rights index ranging from 0 – 27.5 wherein higher values represent upholding respect for labour laws/practices. The sum of these category scores is then the annual measure of labour rights

[34] The US report exclusively covers violations on labour rights in each country related to freedom of association, right to bargain collectively and strike, and export processing zones. The CEACR and CFA reports, both of which are associated with the ILO, are based on the information provided by the respective governments on complaints filed by unions, workers' organizations and other employee associations. The ILO mandates that these are submitted annually and that they include progress reports how grievances are being addressed. These reports are then reviewed by two independent experts to deal with potential misrepresentation. The ICFTU, rechristened the International Trade Union Confederation (ITUC) in 2006, surveys provide information on legal barriers to unions, violations of rights, murders, disappearances and detention of members associated with labour unions.

[35] If violation of labour rights in respective indicators is recorded more than once, in either one source or in multiple sources, the maximum value according to Mosley and Uno (2007) remains 1.

violations, which, in our sample of developing countries has a mean of 25.7 and a maximum of 37. Contrasting this with developed countries, where scores reach 76.5, illustrates the relatively weak protections developing country workers are provided. Overall, the Mosley and Uno (2007) comprehensive measure is a huge improvement on previous indices, such as those used by Cingranelli and Richards (2006) and Bohning (2005), because of the multiple sources of information, sophisticated weighting methodology and reliability of the information.

Having both the overall index and its two components provides us with two advantages. First, it permits us to examine whether there is any evidence of a race to the bottom in one component or the other, that is, whether governments appear to be competing by altering legal frameworks or simply by turning a blind eye towards violations. This latter is of particular concern since a nation may bow to international pressure and introduce legal labour rights but then simply fail to enforce them. Alternatively, strong laws may be undermined by weak enforcement, resulting in a low practices score. As shown in table 3.1, the correlation between the two measures is 0.20, suggesting that this is indeed a possibility. Second, although a positive spatial lag is suggestive of a race to the bottom, it could also signify a race to the top. In particular, one might expect that workers in one country might observe superior labour standards in other countries and demand similar treatment (and thus introducing the possibility of yardstick competition rather than competition for mobile firms).[36] In this case, one might expect an improvement in laws over time even as violations rise as more demanding workers file more registered complaints against their employers. This idea of diffusion through 'public awareness' and the spread of 'norms and ideas' is explored by Neumayer and de Soysa (2006), Baghwati (2004) and Finnemore and Sikkink (1998). As

[36] Within the taxation literature, Salmon (1987) was the first to develop a theory of "yardstick competition" in which the tax authority in one jurisdiction depends on that elsewhere not because officials use taxes to attract mobile factors, but because voters in their jurisdiction judge the performance of the authority by comparing the local tax rate to those elsewhere. Bordignon, Cerniglia, and Revelli (2003) and Allers and Elhorst (2005) utilize spatial econometrics to find positive spatial lags which they interpret as evidence of yardstick competition.

shown in figure 1.1, however, we find that both laws and practices have worsened over time, suggesting both an erosion of legal protections and increased violations of those weakened standards although it is indeed practices that have fallen fastest. In figure 2.2, where we report sample averages weighting by GDP (as is done in the spatial lag), these declines are even more pronounced.[37]

In choosing our vector of control variables ($X_{i,t}$), we follow the work of Caraway (2009), Greenhill et al. (2009), Mosley and Uno (2007), Neumayer and de Soysa (2005, 2006, 2007), Busse (2004), Arestoff and Granger (2004), Brown (2001) and others. Among the standard controls in the literature are measures of economic development. With this in mind, we include logged per capita GDP (measured in constant 2000 US dollars) and its growth rate (ERS, 2010).[38] We also include $\text{Openness}_{i,t}$ to control for a country's exposure to world markets. Following Neumayer and de Soysa (2006), we utilize the manufacturing value added share in GDP, which is included since labour rights in manufacturing are likely better reported than those in agriculture. We also follow their lead and include the total labour force participation rate which is intended to capture the idea that higher the participation would mean greater demand for protective labour rights. Following Boockman (2006) and others, we control for two political variables. The first is $Democracy_{i,t}$, which is the average score from Freedom House's civil and political liberties ranking and ranges 0 (full liberties) to 7 (severely limited liberties).[39] We also include a variable from Beck et al. (2001) that captures the ideology of the incumbent government. We recode this measure so that it ranges between

[37] These diffusion of norm effects are found to be much stronger in bilateral trade (see the 'California effect' in Greenhill et al. (2009)).
[38] We also use constant 2000 US dollars in constructing our weights.
[39] The Polity IV measure could not be considered because our sample includes many small countries such as Barbados, Antigua and Barbuda, for which the Polity IV index is absent. In order to avoid losing too many observations, we opt for the Freedom House score. Alternatively, when using the Polity IV index we could not find any significant changes in our main results.

-1 and 1, with higher numbers indicating a more leftist (and therefore potentially pro-labour) government.

Additionally, we account for the ratification of key ILO conventions to measure whether these agreements have had any measurable impact. Rodrik (1996), Busse (2002) and Neumayer and de Soysa (2006) fail to find any impact of these agreements on labour rights in developing countries. We follow Neumayer and de Soysa (2006) to include a dummy variable one equal to one when a nation has ratified ILO convention number 87, which deals with freedom of association, and if a country has ratified convention number 98 which secures the right to collective bargaining. The variable is constructed using the information from ILO's Database of International Labour Standards (www.ilo.org/ilolex/english/). In addition, we also include a dummy variable capturing whether a country has signed a Structural Adjustment Facility program with the IMF or otherwise, obtained from Dreher (2006) and Boockmann and Dreher (2003). For details on summary statistics, the measurement of our data, or their sources, see table 3.11 and table 3.12 respectively in the appendix.

4.3 Empirical Results

4.3.1 Baseline Results

Table 3.2 presents our baseline results. Column 1 shows results not including the spatial lag or the lagged dependent variable to ease the comparison between our results and those of others studying the determinants of labour rights. As expected, we find that countries with faster growing GDPs, less open economies, better democracies and that have ratified the ILO conventions tend to have better labour rights. Of additional note is the significant downward trend in labour rights over time. After controlling for country-specific fixed effects, however, our other controls are insignificant. Column 2 modifies this by including the one year lag of labour rights (and thus moving from fixed effects to SYS-GMM estimation).

In addition, as discussed by Beck and Katz (1995), it aids in controlling for potential dynamic effects of the exogenous variables on the dependent variable. As can be seen, the coefficient on the lag is significantly positive and its confidence interval ends well before one rejecting a unit root. Column 2 then forms our preferred specification.

Columns 3 through 5 add to Column 2 by including the spatial lag term using GDP, per-capita GDP, and Openness weights respectively. With regards to the controls, this results in more significance, with smaller, industrialized, left-leaning countries having significantly better rights. Turning to the coefficient of interest, in each case, we find a positive and significant spatial lag. A rough interpretation of the coefficient on the spatial lag for the GDP weights, is that if all other countries lower their labour rights by one point, the country in question would lower its labour rights by 0.41 points.[40] Alternatively, a standard deviation reduction in the spatial lag (a reduction of 2.4) would then reduce those in the country in question by 0.984, a 3.8% decline at the sample mean. This lies in between the estimated impact when per-capita GDP weights are used (a 6.1% decline) and when Openness weights are used (a drop of 2.5%).[41] Another way to interpret the coefficient on the spatial lag is to calculate the change in country *i*'s labour rights from a change in another country *j*'s labour rights, which is equal to $\omega_{j,i,t}\rho$. This is then the slope of the *i*'s best response and is a measure of the degree of labour standards competition between countries. Since the spatial lag is positive, this can be interpreted as evidence of strategic complementarity. While strategic complements can theoretically result in a race to the bottom or the top, since the trend in labour rights is downward, we interpret our results as evidence of an economically meaningful race to the bottom in labour rights.

[40] Note that in this and in the Openness weighting scheme, we find spatial lags that are statistically significantly less than 1. This is yet another reason to prefer the GDP weighting scheme over the per-capita GDP one since the game theoretic interpretation of a coefficient greater than one would be that of an unstable Nash equilibrium.

[41] Note that these difference result from not only different coefficients but also different standard deviations in the spatial lag (1.62 for the per-capita weights and 1.82 for the Openness weights).

This, however, is only a part of the total effect, however, since there is also an indirect effect arising from how a change in the spatial lag affects labour rights for country *i* which in turn affects those in *j*, further impacting *i*. This also applies to changes in the exogenous variables. Rewriting (1) in its matrix form,

$$Y = A + \rho WY + \beta X + \varepsilon \tag{3}$$

where A is a vector of country specific intercepts and W is the weighting matrix with $\omega_{j,i,t}$ in the *i,j*th element and zeros elsewhere (i.e. so that the country rights for country *i* in year *t* do not predict itself and that values for years other than *t* are given zero weights in predicting the labour rights in *t*), define $M = I - \rho W$. Then (3) can be rewritten as:

$$Y = M^{-1}A + M^{-1}\beta X + \varepsilon \tag{4}$$

implying that the effect of an exogenous variable is $\left(I - \rho W\right)^{-1}\beta$.[42] This too, however, is only a portion of the impact, since it only captures the static effect. In addition, there is a dynamic effect since the change in year *t* has both direct and indirect implications for future years through the lagged dependent variable. Since the weights vary by year, the total impact would depend on all of these issues as well as the time path of the weights. Since there is no obvious choice to make regarding the future path of the weights (since to calculate the long-run effects would require us to make out of sample forecasts on the weights), we are unable to calculate the total effects. Nevertheless, it is worth noting that our estimates suggest that the adoption of the ILO conventions does not significantly impact labour rights, something we return to below. Finally, with respect to our instruments, we use Hansen's J-test (Hansen, 1982) which shows that the null-hypothesis of exogeneity cannot be rejected at the conventional level of significance.[43]

[42] Note the importance of having $\rho<1$ for the calculation of this effect.

[43] As discussed by Roodman (2009a, 2009b), the Blundell-Bond estimator can fall prey to an overabundance of instruments, inflating the J-test results. As noted above, in alternate specifications, we explored alternative sets of instruments. In these unreported results, we found comparable results for the spatial lags. When including t-2

In table 3.3, we repeat the specification for table 3.2 columns 3 through 5 but use the two sub-indices of labour rights: labour practices (columns 1-3) and labour laws (columns 4-6). For the control variables, as with the combined index, smaller, democratic, and left-leaning states have higher labour practices and labour laws. In addition, wealthier, less open, and more industrialized countries have better labour practices although this has no impact on labour laws. Finally, unlike the combined index where the ILO treaties had no significant effect (a result mirroring that of Rodrik (1996), Busse (2002) and Neumayer and de Soysa (2006)), these treaties now raise labour laws but lower labour practices. This latter result would be consistent with these agreements leading countries to pass more laws protecting workers but turning an increasingly blind eye to violations of those laws.

Turning to the spatial lag, for labour practices, we find results that are comparable to those for the combined index results with a standard deviation decline in all other nations' labour practices leading to a decline in those of the country in question of 3% (using GDP weights), 5.6% (per-capita GDP weights), and 2.6% (Openness weights). For labour laws, however, we only find significance for the per-capita GDP and Openness weighting schemes. Thus the results are somewhat less robust for labour laws. This would be reasonable if nations find it more difficult to compete for FDI in laws (since doing so may draw international criticism) than in how they choose to apply the laws they have on the books. This also mirrors the differences across the two measures regarding the ILO labour rights conventions. For those two schemes resulting in a significant lag for laws, the predicted percentage changes in labour laws from a standard deviation decline in the spatial lag are 2.3% (per capita GDP weights) and 2.2% (Openness weights), again suggesting that the responsiveness of labour

instruments for the GMM-style variables, we were not always able to reject endogeneity. In addition, when using some of the subsamples, we were forced to drastically reduce the number of instruments in order to achieve J-test values less than 1. However, since we still found results comparable to those reported here, we opted to maintain a consistent set of instruments across the regressions in order to simplify the presentation and avoid confusion.

laws in a given country to those elsewhere is less than the responsiveness in the application of those laws.

In table 3.4, we restrict our attention to the non-OECD countries out of the concern that the results may be driven by the OECD members, i.e. relatively advanced countries with strong labour standards. Since, as argued by Mosley and Uno (2007), these countries are perhaps less likely to compete for FDI using labour standards as opposed to other means, if they are behind our significant spatial lag then this would call into question the interpretation of our results. Note that in this (as well as in all subsamples below), when we create a subsample we recalculate the spatial lag and the traditional IV-style instruments using only those nations in the subsample, i.e. assigning those outside of the subsample zero weight. This then assumes that the non-OECD subsample does not respond to OECD member labour rights. As can be seen, our results for this non-OECD only subsample are comparable to those for the main sample, indicating that our results are not being driven by the relatively advanced nations. Also, although from this point forward we only report the results for our preferred GDP weights for parsimony, when using the alternative weighting schemes, we found results comparable to the GDP weights with the exception that as above we often obtained significantly positive spatial lags when using the labour laws index as our dependent variable.[44]

4.3.2 Results for different country categories

The above results provide evidence consistent with a race to the bottom both in the overall labour rights index, labour practices, and, to a lesser extent, in labour laws. In table 3.5, we explore this further by separating our countries into two categories: those for which the mean labour rights index over the sample period was below the median and those for

[44] These results are available on request.

which their mean was above the median. We do this to investigate whether it is the case that the extent of competition differs between developing nations with relatively weak standards and those with relatively strong standards. Note that as in the non-OECD sample, we recalculate the spatial lags using only within group countries, implying that below the median countries do not respond to those above the median and vice versa. As can be seen, we find much stronger evidence of competition among countries with relatively low standards where the coefficients are somewhat greater than those in the full sample. This, combined with the lower average level of standards means that a one standard deviation decline in the spatial lag would lower the combined index by 5.1% and practices by 4.3% with no significant effect for laws. In contrast, we only find a significant coefficient on labour practices for the high standard group with its coefficient being much smaller than its counterpart for the low standard group, which when combined with the greater average level of practices, results in an estimated 0.8% decline in a given country's standards when the spatial lag falls by a standard deviation. It is also worth noting that if our results were simply capturing an overall trend in labour standards, one would expect similar results for the above and below median groups because their trends are comparable. The fact that we find distinct results suggests that we are capturing something other than a mere trend in the dependent variable.

Table 3.6 repeats the estimates of table 3.5 but also includes the other group's spatial lag, i.e. it allows for countries below the median to respond to those above and vice versa.[45] For those below the median, we find a picture similar to that before with respect to within group competition, but no evidence for competition across groups. For those with relatively high standards, we do find some evidence suggesting that the above median countries respond to those below the median. The estimated effect of a one standard deviation decline in the

[45] Ideally, we would choose to estimate the above and below median specifications simultaneously. However, to our knowledge, such an estimator does not exist.

spatial lag for below median countries is -1.7% for both above median labour rights and practices. Thus, to the extent that high standard countries do compete with low standard countries, the extent of this competition appears to be less severe than that between low standard nations. Again, however, there is little evidence of competition among the strong standards countries.

Tables 3.7 and 3.8 again split our sample into two groups but delineate countries according to whether their sample average of per-capita income was above or below the median.[46] Note that since per-capita GDP is generally insignificantly correlated with labour standards (something that holds true even in a univariate regression), that this is a different classification of countries from that above. Table 3.7 corresponds to table 3.5 in that it assumes no cross-group interactions. For the relatively poor countries, we only find a significant spatial lag for labour practices where a one standard deviation decline in the spatial lag results in a 2.5% decline in practices. We find more significance in the wealthier group of countries, where the estimated impacts of a standard deviation decline in the spatial lag results in a 3.9% decline in labour standards and a 3.2% drop in practices. [47] Table 3.8 modifies the estimation of 3.7 by introducing cross-group spatial lags. This addition does not affect the estimated pattern of within-group competition. Further, with one exception, we find no evidence of cross-group competition.

The above results suggest that competition is relatively fiercest between nations with already low standards (which may be those which have competed heavily along this dimension in the past) and those with above average incomes. Further, there is little evidence

[46] In unreported results, we classified countries into three categories corresponding to a country's 2002 World Bank classification into the lower income, lower middle income, and upper middle income categories. In these results, we found strong evidence for within group competition by the middle income countries, limited evidence of such competition for the lower income countries, and no significant competition in the upper middle income countries. Further, we found no consistent evidence of cross-category competition.

[47] In unreported results, resource rich countries were removed from the high income country category. Nevertheless, a positive coefficient on the within group was found, indicating that the result is not driven by high per-capita income, low labour protection resource rich economies.

of cross-group competition, suggesting that these nations may be competing for different types of investment (for example, unskilled labour intensive FDI may primarily consider low income countries whereas skilled labour intensive FDI may only consider high income countries when deciding where to locate). Furthermore, the greatest evidence is for competition in practices rather than laws. This would be consistent with nations "putting on a good face" by instituting labour laws, but allowing firms to bypass those laws in practice.

4.3.3 Results for different regions

In addition to splitting our sample along the above characteristics, we do so across regions. There are two primary reasons for doing so. First, one might expect that countries within a region are much more likely to be competing with one another for FDI. This is one reason Klemm and van Parys (2009) separate their sample when looking for evidence of tax competition in developing nations. Second, as discussed by Mosley and Uno (2007) and Neumayer and de Soysa (2006) there may be religious and cultural differences across countries which influence the decision of what level of labour standards to enforce.[48] With this in mind, table 3.9 presents the estimated coefficient for the spatial lag using each of the dependent variables across five regions: Asia, Sub-Saharan Africa, Europe, Latin America, and the Middle East and Northern Africa. Note that these regressions include the full set of controls but that these are not reported for ease of presentation.[49]

In comparison to the full sample results, we generally find less significant results. Given the large drop in the number of observations, this is perhaps unsurprising. Nevertheless, we do find significant differences across regions. Latin America exhibits

[48] Also, see Cho (2010) for these arguments with respect to womens' labour rights.
[49] The full sets of estimates are available on request. Note that we do not estimate cross-group interactions for these region subsamples since to do so required us to include five spatial lags which, given the sample sizes, resulted in little of interpretive value.

coefficients most in line with the full sample results, that is, a significantly positive spatial lag for the combined index and practices. The Middle East and African nations also exhibit a positive spatial lag, although only for practices. Neither Europe nor Sub-Saharan Africa results in significant spatial lags. Perhaps most remarkable are the results for Asia where we find a significantly *negative* spatial lag regardless of the measure of labour standards used. However, in unreported results dropping China from the set of Asian countries, we no longer found a significant spatial lag for labour rights or labour laws. When India (the second largest GDP in this subsample) is also excluded, we did not obtain significant spatial lags for any of the measures of labour standards. This suggests that the inclusion of these two large outliers is driving the unexpected negative coefficient.

Finally, it is important to comment on our use of a time trend rather year-specific constants. There are two reasons for doing this, both related to the fact that when including year-specific effects, the variation the estimation utilizes is that relative to the within-year average. First, from a game theoretic perspective, one would expect that when countries are very similar, their Nash labour standards may be similar. In the extreme, if all countries are identical, theory can easily obtain the result that equilibrium policies are identical. When estimating such a relationship with year dummies, however, this will drive down the significance on the spatial lag because it varies little across countries within a year. As a result, even if competition is driving the data generation process, the estimation can obscure that fact. Second, one must keep in mind the construction of the spatial lag, which is the weighted average of other countries' policies. Consider two countries with equal GDPs, *i* with a high labour rights index and *j* with a low index. By construction, the spatial lag for *i* will be less than that of *j* because the only difference in their lags is that *i*'s includes *j*'s index in the summation whereas *j*'s includes *i*'s (with the difference between the two being the difference in their index numbers multiplied by the common weight). As a result, countries with strong

policies will tend to have small spatial lags whereas countries with weak policies will tend to have large spatial lags simply by construction. When using year dummies where variation in the spatial lag is relative to the yearly average drives the coefficient, this creates a downward pressure on the estimated coefficient since high index countries will have below average spatial lags within a given year. In fact, when we estimate our results in tables 3.2 and 3.3 but use year dummies instead of the time trend, we find significantly negative coefficients on the spatial lag which were all significantly less than -1 (for the GDP weights these were -5.829, -8.231, and -4.944 for the combined index, practices, and laws respectively). Thus, because of the nature of the spatial lag variable, it is generally unwise to use year dummies (see Klemm and van Parys (2009) for more discussion on this issue).

4.4 Conclusion

The goal of this paper was to present the first set of empirical results exploring the possibility of a race to the bottom in labour standards. Using the Mosley (2011) measure of labour rights as well as its components of labour practices and labour laws, we utilize a spatial econometrics approach to estimate the extent of interdependence of labour standards across countries. We find a robustly positive and significant spatial lag which is consistent with strategic complements in both practices and the combined labour rights index. Notably, this pattern is less evident in labour laws, suggesting that competition is less in the institution of standards, but in their enforcement. Since all three measures declined over time, we interpret this as competition for FDI as opposed to labour rights diffusion which would result in an improvement of laws, possibly even as practices declined as more workers sought to assert their rights. This does not imply that such competition is universal, however. We find that it is concentrated in the countries with relatively weak standards and that it is focused in particular parts of the world, notably the Middle East and Latin America.

These results suggest several potential policy considerations. First, we often find that international labour agreements, particularly those championed by the ILO, tend to raise labour laws but not practices. This suggests that international coordination on these measures may need to follow up and ensure that laws which are adopted are then enforced. Second, the ability of a nation to attract FDI via this (or any other measure) is contingent on the other factors that attract investment such as domestic market size, institutional quality and the like. In particular, the evidence reviewed by Blonigen (2005) indicates that multinationals are often attracted by lower trade barriers. As such, if the developed world signs a free trade agreement with a low labour standard country, thereby increasing its trade openness, our estimates indicate that this would force others to respond by competing more fiercely in labour standards to avoid losing investment. This suggests that it may be important to be mindful of such implications, particularly in Latin America and the Middle East, when pursuing international agreements or other policies that might affect the distribution of FDI**.**

4.5 References

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Table 3.1: Bivariate Correlations across Measures of Labour Standards

	Labour Rights Index	**Labour Rights Laws**	**Labour Rights Practices**
Labour Rights Index	1.0000		
Labour Rights Laws	0.8277	1.0000	
Labour Rights Practices	0.7197	0.20600	1.0000

Table 3.2: Baseline Results

	(1)	(2)	(3)	(4)	
Weighting Scheme			***GDP***	***Per-Capita GDP***	***Openness***
Spatial Lag			0.410***	0.972***	0.359***
			(0.103)	(0.164)	(0.117)
Lagged Dependent Variable		0.723***	0.748***	0.754***	0.749***
		(0.023)	(0.043)	(0.037)	(0.042)
Per capita GDP (log)	-0.834	-0.205	-0.032	-0.009	-0.036
	(0.655)	(0.927)	(0.116)	(0.111)	(0.113)
GDP (log)	0.200	-0.079	-0.607***	-0.605***	-0.591***
	(0.805)	(0.918)	(0.096)	(0.100)	(0.102)
GDP growth rate	0.007***	0.005**	-0.002	-0.000	-0.002
	(0.002)	(0.002)	(0.003)	(0.002)	(0.004)
Openness	-0.010***	-0.007**	-0.005**	-0.005**	-0.004*
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Industry Share in GDP	-0.017	0.012	0.031***	0.030***	0.029***
	(0.020)	(0.020)	(0.011)	(0.010)	(0.011)
Labour Force Participation	-0.024	-0.004	0.001	0.002	0.002
	(0.050)	(0.046)	(0.009)	(0.009)	(0.009)
Democracy	1.128***	0.666***	0.480***	0.472***	0.485***
	(0.129)	(0.115)	(0.103)	(0.099)	(0.102)
Government Ideology	0.287	0.243	0.473***	0.511***	0.504***
	(0.186)	(0.185)	(0.181)	(0.171)	(0.182)
IMF SAF participation	0.254	0.323	0.295	0.163	0.186
	(0.286)	(0.267)	(0.347)	(0.341)	(0.352)
ILO 87 and 98 Treaties	0.807***	0.338	0.005	0.033	0.007
	(0.280)	(0.246)	(0.143)	(0.139)	(0.142)
Trend	-0.467***	-0.293***	0.096**	0.218***	0.040
	(0.031)	(0.031)	(0.046)	(0.052)	(0.042)
Constant	971.862***	209.160***	-186.272**	-447.759***	-77.199
	(57.707)	(49.148)	(94.200)	(108.339)	(86.463)
Observations	2458	2334	2334	2334	2334
R-squared	0.701				
Hansen J-stat. (p-value)		0.145	0.574	0.380	0.178

Notes: All specifications include country-specific fixed effects. Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.3: Practices and Laws

	(1)			(2)		
	Practices			**Laws**		
Weighting Scheme	***GDP***	***Per-Capita GDP***	***Openness***	***GDP***	***Per-Capita GDP***	***Openness***
Spatial Lag	0.374***	1.096***	0.473***	0.129	0.928***	0.688***
	(0.069)	(0.148)	(0.118)	(0.106)	(0.198)	(0.175)
Lagged Dependent Variable	0.590***	0.648***	0.576***	0.803***	0.751***	0.700***
	(0.051)	(0.043)	(0.047)	(0.040)	(0.048)	(0.051)
Per capita GDP (log)	0.194**	0.180**	0.194**	-0.119	-0.131	-0.135
	(0.089)	(0.078)	(0.090)	(0.081)	(0.100)	(0.115)
GDP (log)	-0.510***	-0.463***	-0.511***	-0.196***	-0.246***	-0.301***
	(0.074)	(0.066)	(0.073)	(0.053)	(0.073)	(0.078)
GDP growth rate	-0.003	-0.001	-0.003	0.002	0.001	0.001
	(0.003)	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)
Openness	-0.003*	-0.004**	-0.002	-0.000	-0.001	-0.002
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
Industry Share in GDP	0.020**	0.019**	0.018**	0.011	0.011	0.012
	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.008)
Labour Force Participation	0.005	0.005	0.006	0.000	0.000	0.000
	(0.006)	(0.005)	(0.006)	(0.007)	(0.008)	(0.009)
Democracy	0.153**	0.143**	0.167**	0.295***	0.355***	0.411***
	(0.069)	(0.062)	(0.068)	(0.069)	(0.084)	(0.091)
Government Ideology	0.316**	0.288**	0.343**	0.263***	0.315***	0.345***
	(0.139)	(0.128)	(0.137)	(0.093)	(0.096)	(0.103)
IMF SAF participation	0.050	-0.018	-0.004	0.216	0.186	0.204
	(0.247)	(0.253)	(0.244)	(0.227)	(0.215)	(0.215)
ILO 87 and 98 Treaties	-0.424***	-0.376***	-0.451***	0.223**	0.315***	0.350**
	(0.118)	(0.110)	(0.120)	(0.103)	(0.118)	(0.138)
Trend	0.045*	0.177***	0.032	-0.015	0.045**	0.038
	(0.024)	(0.033)	(0.028)	(0.015)	(0.022)	(0.024)
Constant	-84.490*	-365.456***	-61.847	35.264	-102.033**	-80.306
	(48.700)	(69.346)	(58.450)	(31.213)	(48.394)	(50.984)
Observations	2334	2334	2334	2334	2334	2334
Hansen J-stat. (p-value)	0.184	0.427	0.351	0.269	0.535	0.143

Notes: All specifications include country-specific fixed effects. Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.4: Non-OECD Countries only

	(1) LR	(2) Practices	(3) Laws
Spatial Lag	0.364***	0.348***	0.105
	(0.108)	(0.073)	(0.117)
Lagged Dependent Variable	0.761***	0.617***	0.823***
	(0.043)	(0.061)	(0.037)
Per capita GDP (log)	-0.035	0.167*	-0.115
	(0.115)	(0.087)	(0.078)
GDP (log)	-0.594***	-0.483***	-0.188***
	(0.100)	(0.086)	(0.055)
GDP growth rate	-0.002	-0.002	0.002*
	(0.003)	(0.003)	(0.001)
Openness	-0.005**	-0.004**	-0.000
	(0.002)	(0.002)	(0.001)
Industry Share in GDP	0.029***	0.019**	0.010
	(0.011)	(0.008)	(0.007)
Labour Force Participation	0.003	0.005	0.001
	(0.009)	(0.006)	(0.006)
Democracy	0.428***	0.123*	0.270***
	(0.103)	(0.069)	(0.067)
Government Ideology	0.555***	0.331**	0.300***
	(0.194)	(0.150)	(0.098)
IMF SAF participation	0.345	0.075	0.253
	(0.348)	(0.244)	(0.241)
ILO 87 and 98 Treaties	-0.037	-0.462***	0.190*
	(0.145)	(0.122)	(0.103)
Trend	0.086*	0.050*	-0.012
	(0.048)	(0.027)	(0.014)
Constant	-167.176*	-93.779*	28.060
	(97.703)	(54.125)	(30.919)
Observations	2201	2201	2201
Hansen J-stat. (p-value)	0.559	0.290	0.401

Notes: All specifications include country-specific fixed effects. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 3.5: Above and Below Median Labour Rights

	(1)	(2)	(3)	(4)	(5)	(6)
	Below the Median Countries			**Above the Median Countries**		
	LR	**Practices**	**Laws**	**LR**	**Practices**	**Laws**
Spatial Lag	0.453***	0.471***	0.201	0.064	0.098*	-0.014
	(0.138)	(0.105)	(0.150)	(0.053)	(0.057)	(0.039)
Lagged Dependent Variable	0.652***	0.543***	0.700***	0.519***	0.368***	0.532***
	(0.057)	(0.071)	(0.051)	(0.074)	(0.070)	(0.073)
Per capita GDP (log)	-0.390**	0.127	-0.431**	0.038	0.068	-0.013
	(0.163)	(0.157)	(0.172)	(0.145)	(0.104)	(0.085)
GDP (log)	-0.419***	-0.445***	-0.025	-0.408***	-0.242***	-0.212***
	(0.112)	(0.102)	(0.093)	(0.115)	(0.079)	(0.067)
GDP growth rate	-0.015	-0.023	0.012	-0.000	-0.001	0.001*
	(0.041)	(0.034)	(0.017)	(0.001)	(0.001)	(0.001)
Openness	-0.006***	-0.004***	0.000	-0.001	0.000	-0.001
	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)	(0.001)
Ind. Share in GDP	0.046***	0.028**	0.018	0.008	0.003	0.004
	(0.015)	(0.013)	(0.011)	(0.016)	(0.009)	(0.011)
Labour Force Participation	-0.020	-0.015	-0.005	-0.013	-0.003	-0.009
	(0.016)	(0.014)	(0.015)	(0.011)	(0.007)	(0.008)
Democracy	0.634***	0.066	0.485***	0.239**	0.126*	0.151**
	(0.156)	(0.123)	(0.108)	(0.102)	(0.065)	(0.071)
Government Ideology	0.738***	0.470**	0.344**	0.060	-0.031	0.094
	(0.270)	(0.224)	(0.142)	(0.185)	(0.135)	(0.120)
IMF SAF participation	0.540	0.371	0.089	0.125	-0.206	0.380*
	(0.545)	(0.370)	(0.355)	(0.308)	(0.265)	(0.219)
ILO 87 and 98 Treaties	-0.240	-0.837***	0.388**	-0.087	-0.235**	0.065
	(0.221)	(0.204)	(0.185)	(0.180)	(0.115)	(0.116)
Trend	0.074	0.066*	-0.028	-0.117***	-0.092***	-0.061***
	(0.062)	(0.040)	(0.019)	(0.036)	(0.020)	(0.019)
Constant	-138.804	-127.232	62.961	250.587***	198.557***	136.395***
	(127.972)	(81.578)	(38.472)	(73.416)	(40.433)	(39.056)
Observations	1187	1187	1187	1147	1147	1147
Hansen J-stat. (p-value)	1.000	1.000	1.000	1.000	1.000	1.000

Notes: All specifications include country-specific fixed effects. Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.6: Above and Below the Median with Cross-Group Lags

	(1)	(2)	(3)	(4)	(5)	(6)
	Below the Median Countries			**Above the Median Countries**		
	LR	**Practices**	**Laws**	**LR**	**Practices**	**Laws**
Spatial Lag Below Median	0.419***	0.370***	0.118	0.223***	0.223***	-0.023
	(0.141)	(0.133)	(0.159)	(0.086)	(0.073)	(0.066)
Spatial Lag Above Median	0.057	0.127	-0.089	0.067	0.041	-0.017
	(0.063)	(0.081)	(0.086)	(0.055)	(0.057)	(0.040)
Lagged Dependent Variable	0.700***	0.616***	0.732***	0.545***	0.400***	0.527***
	(0.051)	(0.058)	(0.051)	(0.063)	(0.068)	(0.069)
Per capita GDP (log)	-0.392***	0.078	-0.410**	0.028	0.062	-0.013
	(0.152)	(0.136)	(0.161)	(0.138)	(0.100)	(0.084)
GDP (log)	-0.389***	-0.397***	-0.025	-0.398***	-0.230***	-0.218***
	(0.102)	(0.086)	(0.086)	(0.105)	(0.075)	(0.065)
GDP growth	-0.019	-0.025	0.007	-0.001	-0.001	0.001*
	(0.040)	(0.035)	(0.017)	(0.001)	(0.001)	(0.001)
Openness	-0.006***	-0.004***	0.000	-0.001	0.000	-0.001
	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)	(0.001)
Industry Share in GDP	0.047***	0.029**	0.019*	0.011	0.004	0.005
	(0.014)	(0.012)	(0.011)	(0.015)	(0.009)	(0.011)
Labour Force Participation	-0.018	-0.013	-0.004	-0.013	-0.003	-0.009
	(0.014)	(0.012)	(0.014)	(0.010)	(0.007)	(0.008)
Democracy	0.616***	0.088	0.455***	0.241**	0.123**	0.156**
	(0.147)	(0.113)	(0.103)	(0.100)	(0.062)	(0.070)
Government Ideology	0.741***	0.404*	0.351**	0.068	-0.026	0.096
	(0.259)	(0.211)	(0.138)	(0.185)	(0.131)	(0.120)
IMF SAF participation	0.403	0.244	0.048	0.171	-0.207	0.413*
	(0.550)	(0.377)	(0.355)	(0.307)	(0.268)	(0.217)
ILO 87 and 98 Treaties	-0.256	-0.743***	0.343**	-0.106	-0.224**	0.067
	(0.206)	(0.186)	(0.173)	(0.175)	(0.110)	(0.114)
Trend	0.112	0.085**	-0.054	-0.015	-0.031	-0.063***
	(0.077)	(0.041)	(0.034)	(0.058)	(0.026)	(0.020)
Constant	-218.474	-167.251**	117.198*	41.414	72.838	141.775***
	(158.493)	(83.569)	(70.348)	(118.773)	(53.203)	(42.640)
Observations	1187	1187	1187	1147	1147	1147
Hansen J-stat. (p-value)	1.000	1.000	1.000	1.000	1.000	1.000

Notes: All specifications include country-specific fixed effects. Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.7: Above and Below Median Average Income Countries

	(1)	(2)	(3)	(4)	(5)	(6)
	Below the Median Countries			**Above the Median Countries**		
	LR	**Practices**	**Laws**	**LR**	**Practices**	**Laws**
Spatial Lag	0.075	0.227***	-0.065	0.393***	0.431***	0.106
	(0.096)	(0.087)	(0.070)	(0.111)	(0.085)	(0.181)
Lagged Dependent Variable	0.751***	0.682***	0.751***	0.667***	0.491***	0.780***
	(0.045)	(0.044)	(0.052)	(0.054)	(0.071)	(0.067)
Per capita GDP (log)	-0.423	-0.466**	-0.007	0.358	0.813***	-0.156
	(0.298)	(0.221)	(0.201)	(0.234)	(0.274)	(0.159)
GDP (log)	-0.625***	-0.411***	-0.262***	-0.690***	-0.620***	-0.180**
	(0.122)	(0.065)	(0.088)	(0.124)	(0.113)	(0.077)
GDP growth rate	0.000	-0.001	0.001	-0.043	-0.049	0.005
	(0.001)	(0.001)	(0.001)	(0.045)	(0.035)	(0.012)
Openness	-0.007***	-0.006***	-0.000	0.001	0.002	-0.000
	(0.002)	(0.001)	(0.001)	(0.006)	(0.003)	(0.004)
Industry Share in GDP	0.042***	0.017*	0.024**	0.034**	0.044***	0.003
	(0.014)	(0.010)	(0.011)	(0.015)	(0.011)	(0.011)
Labour Force Participation	0.008	-0.003	0.015	-0.023**	-0.020*	-0.006
	(0.018)	(0.010)	(0.015)	(0.012)	(0.012)	(0.008)
Democracy	0.462***	0.211**	0.260***	0.611***	0.161*	0.353***
	(0.131)	(0.093)	(0.096)	(0.145)	(0.097)	(0.102)
Government Ideology	0.911***	0.450***	0.468**	0.366	0.195	0.224**
	(0.278)	(0.169)	(0.184)	(0.225)	(0.178)	(0.112)
IMF SAF participation	0.726*	0.076	0.527**	-0.367	-0.206	-0.321
	(0.380)	(0.322)	(0.251)	(0.537)	(0.354)	(0.363)
ILO 87 and 98 Treaties	0.033	-0.296**	0.272	0.309	-0.194	0.284*
	(0.227)	(0.149)	(0.178)	(0.237)	(0.166)	(0.171)
Trend	-0.033	0.042	-0.021	0.051	0.030	-0.027
	(0.051)	(0.040)	(0.015)	(0.047)	(0.028)	(0.035)
Constant	78.983	-73.251	49.647	-96.509	-58.299	60.322
	(103.128)	(80.225)	(30.970)	(96.120)	(56.770)	(74.207)
Observations	1157	1157	1157	1177	1177	1177
Hansen J-stat. (p-value)	1.000	1.000	1.000	1.000	1.000	1.000

Notes: All specifications include country-specific fixed effects. Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.8: Above and Below Median Average Income Countries with Cross-Group Lags

	(1)	(2)	(3)	(4)	(5)	(6)
	Low Income Countries			**High Income Countries**		
	LR	Practices	Laws	LR	Practices	Laws
Spatial Lag Below Median	0.086	0.220**	-0.083	-0.088	0.013	0.020
	(0.102)	(0.094)	(0.072)	(0.096)	(0.083)	(0.092)
Spatial Lag Above Median	0.241**	0.074	0.148	0.378***	0.428***	0.081
	(0.112)	(0.073)	(0.178)	(0.113)	(0.087)	(0.182)
Lagged Dependent Variable	0.758***	0.666***	0.750***	0.687***	0.504***	0.788***
	(0.043)	(0.048)	(0.056)	(0.052)	(0.060)	(0.060)
Per capita GDP (log)	-0.416	-0.476**	-0.023	0.330	0.793***	-0.152
	(0.298)	(0.229)	(0.199)	(0.224)	(0.261)	(0.154)
GDP (log)	-0.619***	-0.426***	-0.267***	-0.655***	-0.610***	-0.175**
	(0.119)	(0.066)	(0.093)	(0.115)	(0.103)	(0.071)
GDP growth	-0.000	-0.001	0.001	-0.047	-0.048	0.003
	(0.001)	(0.001)	(0.001)	(0.043)	(0.035)	(0.012)
Openness	-0.007***	-0.006***	-0.001	0.001	0.002	-0.001
	(0.002)	(0.001)	(0.001)	(0.006)	(0.003)	(0.004)
Industry Share in GDP	0.042***	0.017	0.025**	0.034**	0.044***	0.004
	(0.014)	(0.010)	(0.011)	(0.015)	(0.010)	(0.011)
Labour Force Participation	0.007	-0.003	0.014	-0.022*	-0.019*	-0.005
	(0.018)	(0.011)	(0.015)	(0.011)	(0.012)	(0.008)
Democracy	0.456***	0.211**	0.271***	0.589***	0.162*	0.350***
	(0.130)	(0.094)	(0.102)	(0.142)	(0.094)	(0.096)
Government Ideology	0.912***	0.466***	0.487***	0.360*	0.199	0.230**
	(0.271)	(0.169)	(0.184)	(0.214)	(0.174)	(0.109)
IMF SAF participation	0.728*	0.102	0.507**	-0.433	-0.215	-0.315
	(0.378)	(0.321)	(0.246)	(0.538)	(0.360)	(0.366)
ILO 87 and 98 Treaties	0.037	-0.306**	0.269	0.282	-0.201	0.269
	(0.224)	(0.152)	(0.173)	(0.230)	(0.164)	(0.166)
Trend	0.084	0.055	0.008	0.013	0.039	-0.030
	(0.082)	(0.045)	(0.038)	(0.069)	(0.042)	(0.037)
Constant	-159.714	-101.301	-10.483	-19.712	-74.744	67.295
	(167.388)	(91.337)	(78.851)	(141.093)	(86.142)	(77.854)
Observations	1157	1157	1157	1177	1177	1177
Hansen J-stat. (p-value)	1.000	1.000	1.000	1.000	1.000	1.000

Notes: All specifications include country-specific fixed effects. Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

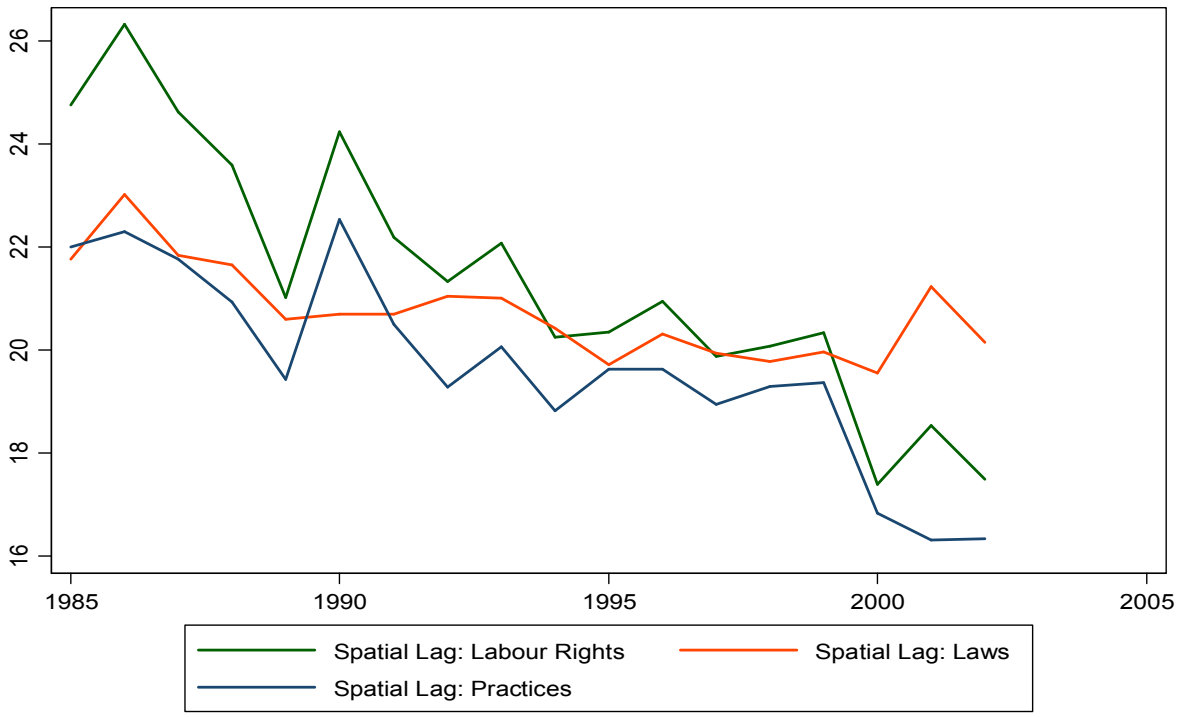
Table 3.9: Regional Results

	(1) ASIA	(2) SUBSAHARA	(3) EUROPE	(4) AMERICAS	(5) MIDEAST
Labour Rights					
Spatial Lag	-0.270*	0.006	0.031	0.247***	-0.022
	(0.160)	(0.094)	(0.074)	(0.069)	(0.173)
Labour Practices					
Spatial Lag	-0.220*	-0.010	0.070	0.200***	0.463***
	(0.123)	(0.080)	(0.058)	(0.063)	(0.141)
Labour Laws					
Spatial Lag	-0.208**	0.128	0.059	0.172	-0.002
	(0.094)	(0.152)	(0.050)	(0.187)	(0.132)
Observations	374	776	253	493	306

Notes: All specifications include all of the additional controls including country-specific fixed effects. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Figure 1.1: Labour Standards, Practices and Laws over time

Figure 1.2: Labour Standards, Practices and Laws over time (weighted by GDP)



4.6 Appendix

Table 3.10: List of countries under Study

Albania	Colombia	Haiti	Mexico	Slovenia
Algeria	Comoros	Honduras	Moldova	South Africa
Angola	Congo Dem. Rep.	Hungary	Mongolia	Sri Lanka
Antigua and Barbuda	Congo Republic	India	Morocco	St. Lucia
Argentina	Costa Rica	Indonesia	Mozambique	Sudan
Armenia	Cote d'Ivoire	Iran	Myanmar	Suriname
Azerbaijan	Croatia	Iraq	Namibia	Swaziland
Bahamas	Cuba	Israel	Nepal	Syrian Arab Republic
Bahrain	Cyprus	Jamaica	Nicaragua	Taiwan
Bangladesh	Czech Republic	Jordan	Niger	Tajikistan
Barbados	Djibouti	Kazakhstan	Nigeria	Tanzania
Belarus	Dominica	Kenya	Oman	Thailand
Belize	Dominican Republic	Korea Republic	Pakistan	Togo
Benin	Ecuador	Kuwait	Panama	Tonga
Bhutan	Egypt	Kyrgyz Republic	Papua New Guinea	Trinidad and Tobago
Bolivia	El Salvador	Lao PDR	Paraguay	Tunisia
Botswana	Equatorial Guinea	Latvia	Peru	Turkey
Brazil	Eritrea	Lebanon	Philippines	Turkmenistan
Brunei	Estonia	Lesotho	Poland	Uganda
Bulgaria	Ethiopia	Liberia	Qatar	Ukraine
Burkina Faso	Fiji	Libya	Romania	United Arab Emirates
Burundi	Gabon	Lithuania	Russian Federation	Uruguay
Cambodia	Gambia	Macedonia, FYR	Rwanda	Uzbekistan
Cameroon	Georgia	Madagascar	Saudi Arabia	Vanuatu
Cape Verde	Ghana	Malawi	Senegal	Venezuela
Central African Rep.	Guatemala	Malaysia	Seychelles	Vietnam
Chad	Guinea	Mali	Sierra Leone	Yemen Republic
Chile	Guinea-Bissau	Mauritania	Singapore	Zambia
China	Guyana	Mauritius	Slovak Republic	Zimbabwe

Table 3.11: Data sources and definitions

Variables	Data description	Data Sources
Labour Rights index	Measures 37 aspects of Labour rights (both Laws and Practices) on a scale of 0 – 74.5 (see section 3)	Mosley and Uno (2007)
Labour Rights Practices and Laws	Measures 16 aspects of Labour rights Practices on a scale of 0 – 27.5 and 21 aspects of Labour rights Laws on a scale of 0 – 28.5 (see section 3)	Mosley and Uno (2007)
Per capita GDP and growth rate	Per capita GDP (logged) in US$ 2000 constant prices and rate of growth of per capita GDP.	Economic Research Service (ERS), Washington DC
Openness	(Exports + Imports)/GDP	UNCTAD
Industry share in GDP	Share of industry value-added in total GDP	UNCTAD
Labour Force Participation Rate	Total Labour Force share in Population	UNCTAD
Democracy index	Average of Civil and Political Liberties index coded on a scale of 0 to -7 where highest value denotes better liberties.	Freedom House
Government's Ideology	Incumbent government's ideology coded on a scale of -1 to +1 where -1 is right wing, 0 is centrists, and +1 is right wing in power.	DPI (Database of Political Institutions dataset developed by Keefer 2001).
IMF SAP	Dummy capturing whether a country was under IMF's Structural Adjustment Program or not	Dreher (2006)
ILO 87 and 98 conventions ratified	Dummy capturing whether a country ratified ILO conventions on labour rights, 87 and 98 or not	ILO database on conventions

Table 3.12: Descriptive Statistics

Variables	Mean	Standard Deviation	Minimum	Maximum	Observations
Aggregated Labour Rights	25.873	7.750	0.000	37.000	2458
Labour Rights Practices	22.231	4.445	0.000	27.500	2610
Labour Rights Laws	22.642	5.499	0.000	28.500	2610
Per capita GDP (log)	7.122	1.366	2.856	10.995	2610
GDP (log)	8.854	1.880	4.813	14.069	2461
Growth Rate of GDP	1.848	27.811	-44.191	973.608	2610
Openness	62.86	53.012	4.96	986.64	2334
Industry Share in GDP	29.609	13.459	0.270	91.607	2468
Labour Force Participation Rate	66.644	11.766	6.755	93.200	2610
Democracy (Freedom House)	-4.228	1.795	-1.000	-7.000	2505
Government's Ideology	0.105	0.694	-1.000	1.000	2473
IMF SAP participation	0.115	0.319	0.000	1.000	2610
ILO 87 and 98 Treaties Ratified	1.287	0.851	0.000	2.000	2610
Spatial lag: Labour Rights	21.267	2.409	16.684	27.012	2461
Spatial lag: Labour Rights Practices	19.565	1.808	16.073	22.807	2461
Spatial lag: Labour Rights Laws	20.702	0.871	19.109	23.785	2461

5. Chapter 4

Fighting Corruption or Elections? The Anti-Corruption Policies in India: A Subnational Study

5.1 Introduction

Are anti-corruption institutions designed to check corruption subject to political manipulation? While there is a lot of anecdotal evidence that anti-corruption institutions, at least among developing countries, are subject to political manipulation, there is no empirical evidence so far to suggest that they are subject to political manipulation and how such manipulation works.[50] The key difference when it comes to anti-corruption activities between the developed and the developing countries is the role that anti-corruption agencies play. In developed countries these agencies operate independently and are well equipped with investigative powers and prosecuting those involved in corrupt activities. In developing countries, however, it has been widely reported that anti-corruption agencies are designed in a way that they are vulnerable to political manipulation (Meagher and Voland 2006). Gareth Newham, Head of the Crime and Justice Program of South Africa notes that, "…allegations (of corruption) point to reasons why the political elite might choose not to strengthen the

[50] See USAID detailed report on Anti-Corruption Agencies (2006) authored by Meagher and Voland covering issues specifically related to purpose, pitfalls and performance of anti-corruption agencies across countries.

independence and ability of the Directorate of Priority Crimes Investigations (DPCI) to investigate corruption committed by those at the highest levels of government."[51] Jennett and Repucci (2006) highlight how vulnerable anti-corruption agencies in countries like Vietnam, South Korea, Nigeria, Guatemala and Montenegro are to political manipulation. Despite documented anecdotal evidence, as noted above, there is little empirical analysis on this topic mainly due to lack of data on government's anti-corruption activities and difficulty in quantifying political manipulation of agencies. Focusing on states within India, this paper presents first such evidence that anti-corruption institutions and bureaus serve political interests of politicians as they are under the direct control of their respective state governments.[52]

Despite rapid economic growth over the last couple of decades, one issue which has attracted lot of attention in India is rampant corruption (Global Integrity Report 2009). Through examining 11 selected public services in India, Transparency International India (TII) and the Centre for Media Study (CMS) find that roughly 22,728 Below Poverty Line (BPL) households paid about Rs 9000 million (US$ 212 million) in bribes to get access to basic need-based public services (Transparency International Report 2008).[53] In addition, according to the Corruption Perception Index (CPI) published by Transparency International (TI), India was ranked 72nd among 100 listed countries in controlling corruption in 1999,

[51] See: http://www.iss.co.za/iss_today.php?ID=1475, accessed: October 2011.

[52] The prominent anti-corruption institutions in India are: (i) Central Bureau of Investigation (CBI): national investigative agency established in 1964, which has two branches, namely the Anti-Corruption Division and Special Crimes Division. The Anti-Corruption Division concentrates purely on investigating cases related to bribery and corruption, while the Special Crimes Division is focused more on conventional crime. (ii) Central Vigilance Commission (CVC): set up in 1964 to advise and guide police in the field of vigilance. Following the CVC, many states in India have adopted their own State Vigilance Commissions (SVCs), broadly along the lines of the CVC. (iii) *Lokayukta:* an anti-corruption Ombudsman (at state-level) established in many states for the redressal of citizens' grievances. The following states have adopted the *Lokayukta* Act: Orissa (1970), Maharashtra (1971), Bihar (1973), Rajasthan (1973), Uttar Pradesh (1975), Madhya Pradesh (1981), Andhra Pradesh (1983), Himachal Pradesh (1983), Karnataka (1985), Assam (1986), Gujarat (1986), Kerala (1988), Punjab (1995), Delhi (1996), Haryana (1996), Chhattisgarh (2002), Jharkhand (2002) and Uttarakhand (2006).

[53] http://www.transparency.org/publications/newsletter/2008/august_2008/anti_corruption_work/india_household_corruption_study_2007, accessed: September 2011.

slipping to 87th by 2010, tied alongside Liberia and ranked lower than Malawi and Lesotho (Transparency International 2011). Big ticket scams have been a consistent feature in India during the last two decades.[54] For instance, corruption charges related to the Bofors defense deal not only united the opposition parties, but also galvanized the entire country, forcing the Indian government to legislate the Prevention of Corruption Act in 1988 (Chawla 1990). Likewise, the recent US$ 14 billion Telecommunication 2G spectrum auction corruption scam, involving the Telecommunications Ministry in India was ranked 2nd in the list of "all-time top 10 abuses of power" by the Time magazine[55] (Time 2011). The exasperation of the public with corruption is reflected in the bloody confrontation which has been taking place from 2010 onwards between civil society groups and the Indian government. Civil society groups [such as India Against Corruption (IAC), the National Campaign for Peoples' Right to Information (NCPRI), and Loksatta] are advocating a strong new anti-corruption Ombudsman at both central (known as *Lokpal*) and state levels (referred to as *Lokayukta*), which are free from political interference, equipped with independent investigative and prosecution powers and increased scope of jurisdiction. One of the main arguments of civil society groups is that the existing anti-corruption institutions and laws have been used over the years by incumbent governments (irrespective of the party in power, both at central and state-level) to serve their political interests.[56] I test this argument empirically by integrating the theories on political

[54] See the list of various multi-million dollar corruption scandals unearthed in the recent past in India: http://www.bbc.co.uk/news/world-south-asia-12769214, accessed: November 2011.

[55] For full list see: http://www.time.com/time/specials/packages/completelist/0,29569,2071839,00.html, accessed: January 2012.

[56] According to the Global Integrity Report (2009), none of the existing anti-corruption institutions are sufficiently protected from undue and excess political interference in practice. Starting with appointments to defining powers, these institutions are directly controlled by the government. Appointments are often biased and based on bureaucrats' loyalty to the ruling party. Even the appointments of respective government departmental vigilance wings are made by the government and not the CVC. Secondly, most of these investigative agencies are merely advisory in nature, except the CBI which is directly controlled by the central government and does not have the power to investigate cases in the states without the permission of the respective state government. The allegation against the CBI is that the incumbent state government, aligned with the center, often uses the CBI to frame their political opponents in cases related to corruption (however, I could not verify this because the disaggregated data on cases registered by the CBI in states is not available). Thirdly, the scope and jurisdiction

budget cycles and theory associated with political capture of anti-corruption institutions in India. By doing this, I examine if existing institutions have indeed served the electoral and political interests of politicians.

Using panel data on 30 Indian states during the 1988–2009 period, I find evidence of the political manipulation of anti-corruption institutions during elections, wherein the scheduled elections (but not unscheduled elections) are associated with an increase in the number of corruption cases registered. Furthermore, I find these effects to be stronger in 'swing states', where the margin of victory of the incumbent in the previous election was 5% or less, as well as in those state scheduled election years which coincide with national elections. However, I do not find any effect of state scheduled elections on corruption cases being investigated by anti-corruption agencies. These findings shed light on the acknowledged, though understudied, role of political manipulation of anti-corruption institutions in India. It is noteworthy that though it is difficult to empirically test the causal link, the mere presence of electoral cycles serves as suggestive evidence of the political manipulation which I described above.

These findings have broad relevance for the study on anti-corruption institutions in the developing world, where we are most likely to observe political manipulation. There is a very small, but growing literature in this area (for instance see Meagher and Voland 2006 for a comprehensive review on anti-corruption institutions across countries). This paper adds to this literature in two ways. First, it contributes a new measure for capturing election cycles in the

of these institutions is often restricted to government servants and employees in the public sector. Even among government servants, not all top level bureaucrats and politicians are covered. Most importantly, prosecuting the medium and top-level bureaucrats is highly difficult because before investigating the suspect, the agencies must get permission from the same authorities against whom the case has to be investigated. Fourthly, none of the anti-corruption institutions have powers to prosecute and hence have to rely on the judiciary system. The inefficiencies of the Indian judiciary system are well documented by Chemin (2010, 2009). According to Chemin (2010), about 3.1 million cases are pending in all the 21 High Courts, and roughly 20 million cases in the lower courts in India. Fifthly, most of these institutions have problems related to coordination and are severely under staffed and inadequately funded. It is precisely because of these reasons the anti-corruption activities of the government have been politicized over the years.

anti-corruption activities undertaken by the incumbent government. Second, a relatively new body of empirical literature examines the importance of institutions in controlling corruption. Apart from the broad socio-economic factors, Treisman (2007, 2000) and De Haan and Seldadyo (2006) Clausen, Kraay and Nyiri (2011) point towards a host of institutional factors associated with the control of corruption. Aaken, Feld and Voigt (2008) demonstrate that *de facto* independence of prosecution agencies robustly reduces corruption of government officials. Gerring and Thacker (2004) exploit the role of different political institutional arrangements on political corruption and find that unitary and parliamentary forms of government help reduce levels of corruption. Two other studies also examine the impact of institutions on corruption. Bjørnskov (2011) and Dreher, Kotsogiannis and McCorriston (2007) find that an improvement in institutional quality reduces shadow economy and affects the corruption market, The exact relationship between corruption and institutional quality is found to be ambiguous and depends on the relative effectiveness of the institutional quality in the shadow and corruption markets. However, these studies remain silent about the potential political capture of these institutions that play a pivotal role in controlling corruption. I add a new dimension to this strand of literature by providing an empirical test of the theories of political manipulation (why and how manipulation of anti-corruption institutions occurs) in India.

This paper also contributes to the strand of literature on political budget cycles, which argues that opportunistic pre-electoral manipulation or cyclical manipulation by incumbent politicians occurs in order to increase their chances of re-election (starting with Nordhaus's 1975 formal model of the political business cycle and followed by the works of Tufte 1975, Fair 1978, Rogoff 1988, and 1990). There have been a number of studies in this area, not only related to industrialized countries, but also among a cross-section of developing countries including India, that have considered the applicability of political determinants to fiscal

policy, infrastructure, agricultural credit and law and order decisions. I, however, assume that policy manipulation to maximize the chances of an incumbent's re-election can also be associated with key governance related issues such as corruption through controlling anti-corruption institutions. I thus provide an empirical test of politically motivated cyclical manipulation of anti-corruption institutions in India. Compared to previous studies on political budget cycles, this study has a significantly larger sample covering all 30 states in India during the 22 year post-economic reforms period spanning 1988 to 2009, allowing for state-fixed effects.

The rest of the paper is structured as follows. In section 5.2, I derive some testable hypotheses illustrating why and how political manipulation of anti-corruption institutions work in India. Section 5.3 describes the data and methodology adopted, while section 5.4 discusses the results. Finally, section 5.5 concludes with some policy implications and identifies avenues for further research.

5.2 Hypotheses

Corruption, understood as using public office for private gains, can have severely adverse effects on the socioeconomic development of a country (Bardhan 1997, Murphy, Shleifer, and Vishny 1993, Shleifer and Vishny 1993). The literature shows that corruption has negative effects on economic growth (Mauro 1995), productivity (Dreher and Herzfeld 2010), investment (Shleifer and Vishny 1993), entrepreneurship (Dreher and Gassebner 2011), international and domestic trade (Bjørnskov 2011, Krueger 1974), the informal sector (Dreher and Schneider 2010), basic public services delivery (Transparency International 2007), and can entrap a country in poverty in the long run (Aidt 2009).

The economic model of corruption is something akin to Ehrlich's (1973) work on the economic analysis of crime and punishment, in which the probability of an individual

resorting to corrupt practice π_i depends on a host of factors (such as expected payoff and opportunity costs: X_i) and, importantly, on the probability of getting caught or arrested (a_i), followed by the probability of getting investigated (i_i), prosecuted and convicted thereafter (p_i).

$$\pi_i = f\{\ X_i\ (a_i\ \times i_i \times p_i)\ \} \qquad (1)$$

As punishing the convict is the prerogative of the judiciary in India, arresting the accused and investigating the case is purely under the control of the executive branch of the government (Ghosh 2006). In the Indian setting, it is the executive branch of the government which is susceptible to political manipulation because of two reasons. First, the incumbent often uses coercive control mechanisms to control bureaucrats because the bureaucratic output, other things being equal, often influences the longevity of the incumbent remaining in power (Iyer and Mani 2011, Mueller 2009). Second, some bureaucrats with long term career concerns tend to respond to the incentives provided by the incumbent (see Iyer and Mani 2011). Such bureaucrats play into the hands of the incumbent in return for financial and material gains, such as a posting to higher job assignments which are usually considered prestigious, which put them in the position of making influential policy decisions, or increases the probability to maximize their rent seeking preferences.[57] The incumbent may thus exert pressure on an anti-corruption agency or state police, which are directly under the control of government, to crack down on corruption during the elections period. Steps taken to control corruption during elections are associated with the performance voting theory which argues that voters are interested in re-electing competent leaders (Powell and Whitten 1993). Poor outcomes in terms of economic conditions and governance issues are evidence of the incumbent's incompetence. Extending the same line of argument to corruption, it can be

[57] Of course this does not mean that a meritocratic bureaucrat with high skills, as per the Webarian notion of bureaucracy, will always be vulnerable to such political pressures (Alesina and Tabellini 2007).

argued that voters reward lower levels of corruption, holding economic conditions constant, because they probably reflect the incumbent's competence. Consider for example the case where a citizen is dissatisfied with the rampant corruption which increased during the incumbent's tenure. The performance voting model predicts that the individual will vote against the incumbent in the elections under the assumption that replacing the incumbent with different leader in office would produce different policies to tackle corruption. Thus, voters can motivate the incumbent to focus on corruption by linking their vote to incumbent's efforts to control corruption.

Numerous studies, starting with Nordhaus's (1975) formal model of the political business cycle, show how opportunistic incumbent politicians engage in pre-electoral manipulation of government policies to increase their chances of re-election (Tufte 1975, Fair 1978, Rogoff 1988, 1990, Alesina and Sachs 1988, Alesina et al. 1993, Besley and Case 1995, Khemani 2004, Shi and Svensson 2006, Gosh 2006, Cole 2009). Policy manipulation, however, need not be restricted to socioeconomic issues alone and can also be extended to key governance related issues such as controlling corruption. However noble the intentions of the incumbent to control corruption during the elections period may be, voters often fail to observe the effects of any such efforts exerted by the incumbent on the ground. For instance, passing legislation to tackle corruption may not have any effect if the capacity to enforce is weak. Therefore, the incumbent may choose to signal her competence on controlling corruption by ordering the state anti-corruption agencies and bureaus to increase the frequency of corruption raids and register cases against those allegedly involved in corrupt practices. The incumbent state governments therefore have strong incentives to exert more pressure on anti-corruption institutions in the run-up towards elections, thereby generating electoral cycles in the controlling of corruption.

Note that this line of argument implicitly assumes that the incumbent wants corruption to be controlled during elections and more importantly, voters seem to be cognizant and concerned about anti-corruption activities.[58] The absence of pre-poll surveys covering elections in all 30 states in the 1990s makes it difficult to empirically test these claims. The issue of corruption has always been one of the key governance issues during the state legislative and national election campaigns in India (Sharma and Gupta 2006). The subsequent section documents these dynamics with some illustrative cases. That said, even if registering more corruption cases during the run up towards elections does not yield any political mileage, if incumbent politicians believe that it does, then this alone could result in political budget cycles in the registered corruption cases.

It is also noteworthy that during election time, the issue of governance including corruption is often brought to the forefront of electoral debates and discourses. In addition, high 'audience costs' in the form of intense media pressure and NGO activists' scrutiny on corruption deals and shady scandals involving bureaucrats and political representatives, threaten the incumbent's chances of re-election. The costs associated with 'naming and shaming' the incumbent government during the election period for its inaction in curbing corruption can prove to be very costly. Thus, keeping in view the chances of re-election, it is reasonable to suggest that the incumbent tries to project his or her competence to voters by exerting pressure on anti-corruption agencies to crack down on corruption in the election season.[59]

[58] Lack of pre-poll data, at least in the early and mid-1990s, for all 30 states makes it difficult to control for voters' perception on corruption just before the elections. Likewise, it is also difficult to control for each and every corruption case reported by local, national and English media, as well as newspapers in each state and in every year due to difficulty in accessing archives of hundreds of newspapers in circulation in each of the 30 Indian states. However, we do control for total newspaper (local, national and English language) circulation in each state, a proxy for media penetration and access to information on anti-corruption activities in the country.

[59] However, it is not very clear as to who becomes the target of anti-corruption agencies during the election period. Obviously, it is expected that the agencies might not target corrupt individuals and officials allegedly

An important point to note here is that it is unreasonable to expect that such effects hold across all elections. Scheduling of elections, especially in India, does not always follow a constitutionally established pattern. Most often, the timing of elections is determined by the incumbent, when conditions suit them the most, or due to the collapse of a government in some cases. Therefore, as highlighted by Khemani (2004) and others, it is important to distinguish between scheduled elections and unscheduled elections. While the former follow a constitutionally scheduled pattern and occur once every five years, the latter are elections that take place in the middle of an incumbent's constitutionally established five year term, either because of imposition of President's Rule by the central government (which requires dissolving state legislative assembly and calling fresh elections) or coalition compulsions, or due to the incumbent calling early elections.[60] If cyclical manipulation of anti-corruption institutions does actually exist, then this would effectively mean that unscheduled elections may not be associated with an increase in corruption cases registered by anti-corruption agencies because the nature and timing of such elections are often sudden and unanticipated. On the other hand, the timing of scheduled elections and the run-up to them is often predictable, thus they should be associated with an increase in corruption cases registered by anti-corruption agencies. Therefore, anti-corruption activities of the state governments should be higher during scheduled elections compared to the unscheduled elections. As scheduled elections are predictable, incumbent politicians and bureaucrats working under them have an ex ante schedule to guide their anti-corruption activities. When the state elections are unscheduled – due to collapse of coalition government or imposition of President's rule by the

close to the incumbent. Unfortunately, the data used on corruption cases registered and investigated here do not provide details of the individuals and officials being convicted and prosecuted.

[60] Although in the post-independence period, the state assembly elections in India were synchronized with national Parliamentary elections, due to several unscheduled elections in different states in later years, the state legislative election cycles no longer coincide with the national election cycle. Also, each state legislative election cycle does not coincide with other states' legislative election cycles.

centre – it might prove difficult for the incumbent to adjust the anti-corruption activities accordingly. Based on this discussion, I derive the following testable hypotheses:

Hypothesis 1: *Scheduled elections, and not unscheduled elections, are associated with an increase in the number of corruption cases registered.*

Hypothesis 2: The *number of corruption cases registered is responsive to the proximity to a scheduled election.*

Additionally, it is important to distinguish between the electoral effects on corruption cases registered and cases being investigated by anti-corruption agencies for two specific reasons. First, incumbent is only interested in using anti-corruption agencies to register cases in order to signal her competence in fighting corruption to the voters. Hence, there is no political will on the part of the incumbent to actually carry out follow up investigations on those cases unless there is intense media pressure on a particular case. The second, more practical reason, is that investigation of corruption cases is often time-consuming and not really under the control of the government *per se*. Here, a host of issues come into play such as the nature of the investigation, size and magnitude of the case, efficiency and effectiveness of investigative agencies, among others, which are not under the control of the government. I thus expect that:

Hypothesis 3: *Corruption cases investigated in a year are not responsive to either scheduled elections or the electoral cycle.*

According to the 'pork barrel politics' theory, the incumbent faces the dilemma of whether to channel the resources to his or her core support group or to the undecided voters. Competing theories of tactical redistribution suggest that such politically motivated redistribution of resources depend on the political objective of the incumbent, which is to maximize his or her votes (Cox and McCubbins 1986, Lindbeck and Weibull 1987, Dixit and Londregan 1996). While Cox and McCubbins' (1986) model suggests that the decision

whether to target resources towards the core support group or the undecided voters depends on the incumbent's risk taking attitude, Lindbeck and Weibull (1987) and Dixit and Londregan's (1996) models predict that if an incumbent's objective is to maximize votes, as in parliamentary democracies like India, then resources would be channelled to undecided voters whose voting decisions are determined by the amount of public goods they receive, rather than the affinity towards the incumbent or party ideology.[61] In other words, more resources are allocated to swing areas/regions, as swing voters are expected to switch their vote in favor of the incumbent, where the electoral race is expected to be tight. Similar results are discussed by Shariff (2011), Keefer and Khemani (2009), Case (2001), Levitt and Snyder (1995) and Snyder (1989). Extending the same logic to corruption in India, intense electoral competitiveness, measured by the margin of victory in the previous elections, between the incumbent party and opposition parties might actually generate incentives for the incumbent to act more swiftly on corruption during the election period. The history of electoral competition in India shows that elections are very tight. In fact, electoral competition has become very intense during the study period adopted in this paper, where a small swing in vote share can result in a change in government (Cole 2009). I therefore expect:

Hypothesis 4: An *increase in the number of corruption cases registered is more responsive to scheduled elections in a swing state than scheduled elections in a non-swing state*.

Finally, I make the a priori assumption that incumbents in states would want to extract more resources and other favors from the central government. To do so, they not only need to increase their chances of re-election, but also win more seats in national elections in order to increase their bargaining power with the center. As mandates at national level throughout the

[61] Lindbeck and Weibull (1987) also propose an alternative model in which if the objective of incumbent is to secure majority seats, as in presidential democracies, and if other parties are not as popular as the incumbent, then it is likely that more resources are diverted to core support groups.

1990s increasingly appeared like a sum total of state level verdicts, the incumbent at the state level has clear incentives to engage in influencing both state and national electoral outcomes.[62]

Hypothesis 5: An *increase in the number of corruption cases registered is also responsive to state scheduled elections which coincide with national parliament elections.*

5.2.1 Illustrative Cases

Before empirically evaluating the hypotheses, I describe some illustrative cases in a few states to document these dynamics in detail. The issue of corruption has always dominated national and state elections in India (Sharma and Gupta 2006). For instance, in the 1984 national elections, Rajiv Gandhi contested and won elections on the back of promising to control corruption and fight insurgency in Punjab. However, during his tenure (1984–1989), the Indian National Congress (INC hereafter) government was accused of accepting 60 billion Indian Rupees (US$ three billion) in bribes from Bofors, a Swedish arms producer (BBC 1999), as well as being involved in other scandals such as the HDW German shipyard submarine deal (Sharma and Gupta 2006). These corruption scandals not only united the opposition parties, but also illustrated the rampant corruption existent at the state level, particularly in those states ruled by the INC party. With several corruption cases being unearthed by the media in states like Uttar Pradesh, Madhya Pradesh and Andhra Pradesh, all ruled by the INC in the late 1980s, it was widely felt that corruption at the top created conditions of legitimacy for graft at the lower level (Chawla 1990). With extensive media coverage, corruption once again became a key issue in 1989 national and state elections (Chawla 1990). As a result, there was a spike in the number of corruption cases registered in

[62] This is a reasonable assumption to make, as leading Indian social scientist Yogendra Yadav succinctly puts, "ordinary citizens (in India) look at national politics through the prism of their own state" (Yadav and Palshikar 2011). Therefore, political contestation at the state level post-1990s started to play key role in determining the outcomes at the national level.

the states (Bihar, Gujarat, Himachal Pradesh, Maharashtra, Orissa, and Rajasthan) which witnessed scheduled elections in 1990.

Another example comes from the state of Andhra Pradesh. In the run up towards state scheduled elections (which also coincided with national parliamentary elections) in 2009, the then Chief Minister of the state publicly announced they would lend a free hand to the state Anti-Corruption Bureau (ACB) to crack down on corruption. "The Chief Minister complimented the ACB officials for the continuous traps and raids and booking of cases even during the elections and asked them to continue the same if needed more vigorously"(quoted in the Reachout New Bureau 2009).[63]Accordingly, the number of corruption cases registered during the election year increased from 244 to 330.

Likewise, in the run up towards elections, Orissa's State Vigilance Department dramatically increased the number of corruption cases registered with the full backing of the state Chief Minister. In comparison to 203 cases in 2008, the State Vigilance Department registered about 385 cases in the 2009 state scheduled election year.[64] Similar such practice can be observed under the tenure of Chandrababu Naidu in Andhra Pradesh (1995–2004) and Nitish Kumar in Bihar (2005–2011) both proclaimed to be strict against corruption and intent on maintaining law and order.[65]

5.3 Data and method

In this section, I describe the data, which is a panel dataset across 30 Indian states during the 1988–2009 period (22 years), and the estimation specifications. The objective is to

[63] "Free hand to ACB to deal with corruption," Reachout New Bureau 2009, http://www.reachouthyderabad.com/newsmaker/hw265.htm, accessed: November 2011.
[64] "Orissa on anti-corruption drive mode," The Economic Times, November 2009, http://articles.economictimes.indiatimes.com/2009-11-08/news/28493922_1_corruption-vigilance-awareness-week-special-courts, accessed: December 2011.
[65] "The Illusionist of Bihar," December 2011, Bihar Times, http://expressbuzz.com/magazine/The-illusionist-of-Bihar/338930.html, accessed: January 2012.

identify the impact of the timing of state elections on corruption cases registered and investigated in Indian states. The base specifications (2) and (3) are formulated below.

5.3.1 Estimation Specification

The baseline specification estimates the number of corruption cases registered and investigated by the respective state anti-corruption agencies separately (CC_{it}), in state *i* in year *t* as a function of a set of elections E_{it} and other exogenous variables Z_{it} :

$$CC_{it} = \gamma E_{it} + \beta Z_{it} + \nu_i + \lambda_t + \omega_{it} \qquad (2)$$

Where, ν_i denotes state fixed effects to control for unobserved state specific heterogeneity in the panel dataset, λ_t is time specific dummies and ω_{it} is the error term. For the dependent variable, I use two variables, namely the total number of corruption cases registered and investigated by the respective state anti-corruption agencies in state *i* in India in year *t,* reported by the National Crimes Records Bureau (NCRB hereafter) for the 30 states and five union territories from 1988 to 2009. It is noteworthy that for most of the states, the data are only available from 1989 onwards. The NCRB was constituted in 1986, with headquarters in New Delhi under the Ministry of Home Affairs. The major task of the NCRB, among others, is to function as a clearing house of information on crime and criminals operating at national and state levels. They coordinate with the respective States Crime Records Bureaus (SCRBs) in collecting and processing crime statistics at the state and national level. Along with all other crime data, the data on corruption cases (registered and investigated by the respective state anti-corruption agencies) are collected every year by each state's vigilance department and are made available to the NCRB, which then publishes these numbers in its annual reports. The most important reason for selecting this dataset is not only its reliability, but also the fact that it is the only credible dataset available which provides

information on various forms of crimes recorded at the state level and in major metropolitan areas. The coding of each section for each state-year combination is evaluated by a set of trained coders at the NCRB and is then cross checked by the Crime and Criminal Network Tracking System (CCNTS) project of the Ministry of Home Affairs.

Since 1988, with the inception of the Prevention of Corruption Act, state police have been allowed to register cases by arresting suspects and lodging criminal proceedings under the Prevention of Corruption Act 1988. Previous to this Act, it was left to the prerogative of the respective state police forces to act against officials and others involved in corruption according to the respective state laws on corruption. The data on corruption cases registered and investigated capture cases related to both public servants, including state and central government officials, and people from the private sector. Under the Prevention of Corruption Act, state police and state anti-corruption agencies have the mandate to register cases against non-public servants who are suspected of corrupt practices. Note that the data on cases registered and investigated also include the cases at the state level dealt with by the CBI. The NCRB, however, do not provide disaggregated data on the number of cases registered and investigated by the CBI and respective state police forces or state anti-corruption agencies. It is also worth noting that the data on corruption here do not include other forms of economic offenses, such as drug trafficking, trafficking of cultural property, money laundering, smuggling, financial fraud and tax evasion, but do include the criminal breach of trust. Figures 2.1 and 2.2 show how the number of corruption cases registered (investigated) has evolved in the various states during the 1989–2009 period. On average, a larger number of cases have been registered and investigated in Maharashtra, Orissa and Rajasthan, followed by Andhra Pradesh, Gujarat, Karnataka, Kerala, Punjab, Tamil Nadu and Uttar Pradesh. Interestingly, the number of cases registered and investigated is low in West Bengal and Assam, where the incumbent has been in power for more than three decades in the former,

and three consecutive terms in the latter, respectively. The average number of cases registered during the study period is about 104, and roughly 212 cases were investigated.

The vector E_{it} includes the main variables of interest, namely state legislative assembly elections, which includes both scheduled and unscheduled elections. Accordingly, the value 1 is coded if a state *i* in year *t* witnesses state legislative elections which are scheduled and 0 otherwise. Likewise, another dummy variable is created where the value 1 is coded if a state *i* in year *t* witnesses state legislative elections which are unscheduled and 0 otherwise. The elections data are from the Election Commission of India, which provides complete details on each state and national election. As per the Indian constitution, scheduled elections are those which are scheduled to take place every five years, and unscheduled elections are those which have occurred in the middle of a five-year cycle. As pointed out by Khemani (2004), unscheduled elections occur for various reasons, such as a shift in political alignments (e.g., members of the ruling party quit the ruling alliance), political instability due to the compulsion of a coalition (e.g., withdrawal of support by a coalition partner insofar that government might not possess the required numbers to maintain its majority in the house), or the imposition of President's Rule by the center, which has the authority to recommend the removal of a state government, with the Indian president then taking control. President's Rule is usually recommended when there is a failure in the constitutional machinery of the state (e.g., heightened political instability or a loss of law and order). It is noteworthy that any election occurring after the imposition of President's Rule is coded as unscheduled. It is also plausible that the incumbent might call for an early election. In fact, in the study period, close to 38% of total state legislative elections are unscheduled elections. Some of them owe to the calling of early elections by the incumbent, others because of President's Rule being imposed by the central government, as well as political instability. It is precisely for this reason that I

distinguish between scheduled elections and unscheduled elections – the timing of unscheduled elections is unanticipated due to the three reasons (shift in political alignments, political instability, and President's Rule) and may not be exogenous to government policy on tackling corruption if the incumbent calls for early elections. On the other hand, unlike unscheduled elections, scheduled elections are fully exogenous simply because the scheduled elections and the electoral cycle are fixed by constitutional arrangements, and therefore the incumbent can fully predict the exact timing of such elections. Thus, it is reasonable to expect that the incumbent will strategically plan policies to influence the outcome of scheduled elections. If this is indeed true, then I expect varying effects of scheduled and unscheduled elections on corruption variables. The distinction between these two types of elections allows me to test the first hypothesis, which is that scheduled elections have a significant positive impact on corruption cases, while unscheduled elections do not.

To the baseline equation (2), I introduce distance from elections in state *i* in year *t*, a variable known in the literature as the electoral cycle to examine how the temporal distance from a specific scheduled election year affects corruption cases. I estimate:

$$CC_{it} = \sum_{\delta=1}^{4} \psi_{\delta} T^{\delta}{}_{it} + \gamma SE_{it} + \beta Z_{it} + \nu_i + \lambda_t + \omega_{i_t} \qquad (3)$$

Where, $T^{\delta}{}_{it}$ is the vector of the electoral cycle comprising four dummy variables, capturing $T^{\delta}{}_{it}$ for δ = 1 is a dummy coded if state *i* in year *t* is one year away from a scheduled election year (SE_{it}); δ = 2 is another dummy variable if state *i* in year *t* is two years away from a scheduled election year; δ = 3 is other dummy variable if state *i* in year *t* is three years away from a scheduled election year and δ = 4 is the last dummy variable if state *i* in year *t* is four years away from a scheduled election year. It is noteworthy that if one of the years in the electoral cycle coincides with a unscheduled election then it is treated as one, two,

three or four years before a scheduled election, and the year after that unscheduled election year will be coded as four years away from a scheduled election year and so on.[66] This method is adopted by Khemani (2004) to nullify the effect of the timing of unscheduled elections, particularly if it is viewed as an outcome of an event whose effects do not last beyond the event. Meaning, a negative shock in a particular year leading to unscheduled election does not affect the probability of such a shock in the future.

Finally, in order to examine other political considerations listed in the third and fourth hypotheses, I examine the following specification:

$$CC_{it} = \gamma SE\_SW_{it} + \gamma SE\_(1-SW)_{it} + \beta_2 Z_{it} + \nu_i + \lambda_t + \omega_{it} \qquad (4)$$

Where, SE_SW_{it} is a dummy for scheduled elections taking place in a 'swing state'. The swing state is identified based on the margin of victory of the incumbent in the previous elections against their rival contesting party was 5% or less in terms of vote share. It is important to note that electoral competition in India is very high and a small swing of 5% in the vote share can dramatically increase (decrease) the seats. $SE\_(1-SW)_{it}$ represents scheduled elections dummy in a non-swing state, which helps distinguish the differential effects between swing states and non-swing states. Finally, to examine the effects of state scheduled elections coinciding with national elections, I utilize:

$$CC_{it} = \gamma SE\_NE_{it} + \gamma SE\_(1-NE)_{it} + \beta_3 Z_{it} + \nu_i + \lambda_t + \omega_{it} \qquad (5)$$

Where, SE_NE_{it} is a dummy variable when the scheduled state elections coincides with national parliament elections (*Lok Sabha*) and $SE\_(1-NE)_{it}$ is also a dummy capturing the state scheduled elections which do not coincide with national parliamentary elections. The

[66] Note that introducing these dummies into the model does not result in multicollinearty problem.

data for the dates of national elections, swing states (i.e., margins of victory) are from the Election Commission of India's statistical reports on each state legislative and parliament election.

The vector of control variables (Z_{it}) includes other potential determinants of corruption cases registered and investigated in state *i* during year *t*. Since this is the first such study on elections and corruption in India, I follow the pioneer cross-country studies on determinants of corruption of Treisman (2000, 2007), Aidt (2003), Dreher, Kotsogiannis and McCorriston (2007), Aaken, Feld and Voigt (2008) and other comprehensive evaluations of early studies on corruption (Mauro 1995). Accordingly, the models control for the effects of development by including respective states' GDP (logged) in Indian rupees using 1993-94 constant prices, obtained from the Reserve Bank of India's macroeconomic dataset (Mauro 1998, 1995). I also control for population (logged) as larger states tend to have more corruption cases registered. Following Brunetti and Weder (2003), Ferraz and Finan (2008) and Bjørnskov (2011), I also capture the effects of the media by including the log of total newspaper (English, Hindi and the respective states' local languages) circulation per-capita. As argued by De Haan and Seldadyo (2006), regulatory capacity is key in tackling corruption. I capture regulatory capacity with the effective police infrastructure by including the number of total criminal cases (Indian Penal Code – IPC) pending for investigation per policemen in state *i* in year *t*. I expect a higher ratio is detrimental to the number of corruption cases registered and investigated because of intense pressure on police to deal with other criminal cases. The data for police infrastructure are obtained from various annual reports on crime published by the NCRB. I also include a proxy for anti-corruption legislation in place to control for corruption in each state by using a dummy which codes the value 1 if the state has *Lokayukta* Act (state Ombudsman Act), and 0 otherwise. Finally, following Besley and Burgess (2000), I also capture the number of years various political parties are in power in state *i* in year *t* to control

for the longevity of the incumbent in power. I expect the longer the party is in power, the lower will be the focus on controlling corruption. These variables include dummies for each of the following parties in power, namely the Indian National Congress (INC hereafter), centre-left in ideology, the Bharatiya Janata Party (BJP henceforth), centre-right, the Left Front led by the Communist Party of India-Marxist (CPI-M), leftists, and other regional parties often considered soft left in ideology. Details on variable definitions and data sources are reported in table 4.5 in the appendix.

As the count data on corruption cases registered and investigated are strongly skewed to the right (with an accumulation of observations at zero) and display significant overdispersion, with the variance being greater than the mean (see descriptive statistics reported in table 4.6 in appendix), I estimate the regressions employing the Negative Binomial estimator to Poisson estimator – which restricts the variance equal to the mean (Lawless 1987, Cameron and Trivedi 1998). Note that the 'goodness-of-fit' test supports using the Negative binomial over the Poisson estimation method. I employ state specific and year specific fixed effects and heteroskedasticity consistent robust standard errors (Beck and Katz 1995). It is noteworthy that there exists sufficient variation among elections, with 38% of the all-state legislative elections being unscheduled elections (62% therefore being scheduled elections) to employ fixed effects without any likely problems occurring.

5.4 Empirical Results

5.4.1 Baseline Results

Table 4.1 presents the baseline results capturing the effect of elections and the electoral cycle on corruption cases registered. Table 4.2 focuses exclusively on corruption cases being investigated. In table 4.3, I examine how other political considerations (such as swing state effect and national elections) affect an incumbent's efforts to control corruption.

As discussed, all the models are estimated using negative binomial regression estimations and results in all the tables report coefficients. I compute marginal effects at the mean of the explanatory variables.[67] Beginning with column 1 in table 4.1, the results show that elections in general, though positive, have no statistically significant effect on the number of corruption cases registered. However, when disaggregating the elections into scheduled and unscheduled, I find a strong positive effect of scheduled elections in column 2, which is significantly different from zero at the 5% level. The marginal effects suggest that a scheduled election year, holding other controls at their mean, is associated with roughly eight additional corruption cases being registered than other years. On the other hand, in column 3 I find that unscheduled elections actually have a negative effect on corruption cases registered, which is significantly different from zero at the 5% level. These divergent results with respect to scheduled and unscheduled elections actually underline the importance of distinguishing the effects between the two. These results support hypothesis 1, i.e., unscheduled elections are unanticipated while scheduled elections are fully anticipated events, thereby providing incentives for politicians to engage in manipulative politics to influence election outcomes.

In column 4, I include both scheduled elections and unscheduled elections together. As can be seen, I still find a positive and significant effect of scheduled elections and a negative effect for unscheduled elections. Controlling for unscheduled elections, the marginal effects show that a scheduled election year is associated with roughly seven more corruption cases being registered. In column 5, I include the electoral cycle. Although two of the four variables in electoral cycle are statistically significant, they are found to be jointly significant at 10% level. As scheduled elections draw closer, there is an increase in the number of cases being registered. A graphical representation (in figure 2.3) show that the number of corruption cases registered is responsive to the proximity to a scheduled election year, i.e., the number of cases

[67] I use Stata 11.0's margins command to compute marginal effects.

tends to increase steadily as a scheduled election year draws closer. These results are in line with the second hypothesis that if there is some evidence of an electoral cycle, with respect to corruption cases registered, then the idea of strategic manipulation of policy to affect political outcomes is feasible.

In table 4.2, the dependent variable is the number of corruption cases investigated by anti-corruption agencies in the respective states. As one can see here, in column 1, I do not find any statistical significance for all state legislative elections grouped together. The effects remain statistically insignificant when disaggregating the elections into scheduled and unscheduled in separate models, reported in columns 2 and 3 (in table 4.2). Though the effect of scheduled elections is positive, it remains statistically insignificant with a p-value of about 0.14. On the other hand, unscheduled elections also remain statistically insignificant. In columns 4 when including scheduled and unscheduled elections together, I do not find any significant effect for either of the elections. I introduce electoral cycle in column 5 (of table 4.2). As can be seen here, there is no clear-cut relationship between the electoral cycle and the number of corruption cases being investigated. These results on the investigation of corruption cases lend support to hypothesis 3 which reflects two issues: First, unlike corruption cases registered, investigations are a time consuming process and a lot depends not on the investigative mechanism of the respective state police forces, but also on the availability of efficient police infrastructure to finalize the investigation and send the case to a judiciary trial. Second, as mentioned earlier, it is also plausible that the incumbent is only concerned with signaling his or her competence, i.e., by increasing arrests of people involved in corruption during the election period, and may not actually be interested in following up with investigation of these cases.[68]

[68] It also noteworthy that the results reported in table 4.1 and 4.2 do not change much, even after introducing respective lagged dependent variables. These results are not shown here due to brevity.

Before moving further, I discuss the findings on the control variables reported in both tables. Interestingly, in both models, the control variables remain largely consistent. There is a positive relationship between the level of state GDP and control of corruption (both cases registered and investigations). Holding other variables at their mean, a standard deviation increase in the (logged) state GDP would result in 19 more cases being registered and 18 more cases being investigated, which is about 11% and 7% of the standard deviation of cases registered and investigated respectively. This could be a reflection of two things. First, in a developing country like India, richer states tend to attract a lot of corruption because of the sheer size of economic activity. Second, it also plausible that rich states tend to spend more on police infrastructure which can effectively reduce corruption. The findings on newspaper circulation are interesting. Although newspaper circulation has a strong positive impact on corruption cases registered which is significantly different from zero at the 5% level, it remains statistically insignificant with respect to investigation of cases. One reason for this could be that the media can play a positive role in unearthing the scams and scandals associated with corruption, which in turn puts pressure on state administration to act (Bjørnskov and Freytag 2011). However, once these cases are reported, there is often no follow up by the media on the investigations and trials.[69]

The findings related to police efficiency measures, i.e., the total number of criminal cases per policemen, are even more interesting. It seems to have no effect on cases being registered, but there is a strong negative effect on cases being investigated which is significantly different from zero at the 5% level. Holding other variables at their mean, a standard deviation increase in total criminal cases per policemen would mean nine less

[69] Of course there are some exceptions, especially if the cases are associated with ministers or businessmen, and the nature and size of the corruption is large and attracts public attention. For instance, the corruption cases associated with the Commonwealth Games and 2G telecom licenses, in which not just businessmen were arrested, but also politicians - prominent among them a cabinet minister of telecom - were put behind bars, are prime examples of the media following up with the investigation of cases associated with big ticket corruption.

corruption cases being investigated, which is about 3% of the standard deviation of total corruption cases investigated. This negative finding highlights another pressing demand by civil society activists for police reforms, which is pending in the form of a draft note with successive governments. Likewise, I also find that states which have the *Lokayukta* Act (state Ombudsman) in place are associated with roughly 10 additional cases being investigated than states without it. However, it has no significant effect on corruption cases registered. Finally, with respect to political variables, in line with popular perception, no particular political party being in power is associated with either an increase in corruption cases registered or investigated. The exception is the Left Front, which is associated with an increase in cases being registered. This effect remains statistically insignificant when it comes to investigations, however. Prominent among other parties is the Indian National Congress and its allies, which has a negative effect on corruption cases registered at the 5% level.

5.4.2 Results on other political considerations

In table 4.3, I test if the evidence for elections on controlling corruption is robust to another political variable that has been identified in the public choice literature as an important predictor of manipulative politics — the proportion of votes. Accordingly, I introduce two dummy variables in columns 1 and 2, namely scheduled elections in a swing state, and scheduled elections in a non-swing state, in order to examine if incumbent governments target regions where the electoral race is tight or safe. As can be seen from column 1, I find a strong positive effect of scheduled elections in a swing state on the number of corruption cases registered, which is statistically significant at the 5% level. When estimating marginal effects, I find holding control variables at their mean, that scheduled election years in swing states result in roughly 15 additional cases being registered in comparison to non-swing states (see column 1). Although positive, I do not find a statistically

significant effect in either swing or non-swing states for corruption cases under investigations (see column 2). In columns 3 and 4, I replicate these results by replacing scheduled elections with unscheduled elections. I do not find any significant effect of unscheduled elections on both corruption cases registered and investigated.[70] These findings highlight that the effect of elections are statistically distinguishable between states which happen to be swing states, and those that are not. This also means that incumbent state governments target states where the electoral race is tight, instead of the safe states where the incumbent is expected to do better.[71]

Table 4.3 also captures the effects of state scheduled elections coinciding with national elections (see column 5 and 6). Accordingly, I introduce two variables, namely state scheduled elections coinciding with national elections, and state scheduled elections which do not coincide with national elections. The results in columns 5 and 6 show that state scheduled elections coinciding with national elections have significant positive effects on both corruption cases registered and investigated. Controlling for other variables, the marginal effects show that the scheduled state elections coinciding with national elections witness an increase in about 10 additional cases registered and 23 additional cases investigated compared to other years. Both remain significantly different from zero at the 10% and 5% levels, respectively. In columns 7 and 8, I replicate these results, but replace scheduled elections with unscheduled elections. As can be seen here, I do not find any effects of unscheduled elections that coincide with national elections on either cases registered or investigated.[72] An interesting finding which emerges from table 4.3 is that unlike in previous models, the marginal effects on cases investigated are higher compared to cases registered. Once again, the control variables reported in table 4.3 are consistent with the theoretical expectations.

[70] These results remain the same even after controlling for swing state dummy in the models.
[71] Note that interacting swing state dummy with scheduled elections dummy yields almost identical results, i.e. swing states conditional upon scheduled elections witness more corruption cases being registered. However, the statistical significance drops from 5% as reported above to 10% level. The interaction effects however remain statistically insignificant for corruption cases investigations.
[72] These results remain identical even after controlling for national elections dummy in the models.

5.4.3 Checks on Robustness

I examine the robustness of the main findings in the following ways. First, I re-estimate the baseline regressions, varying the set of main independent variables, i.e., the scheduled elections dummy. I generate a dummy if the scheduled elections in a year occur after March, thus ignoring all the scheduled elections which would have occurred during January – March. It is unreasonable to expect that a spike in the number of corruption cases registered would have occurred during the first two months in a calendar year after which scheduled elections take place.[73] Moreover, the elections code of conduct, which prohibits incumbent politicians from using policy instruments for electoral gains, comes into play weeks after the announcement of the election schedule by the Election Commission of India. I find that, in total, there are eight such scheduled election years across all states. Re-estimating the baseline results without these scheduled election years yields almost identical results as reported in table 4.1 and 4.2. In fact, the statistical significance of the scheduled elections dummy increased from 5% (as reported in table 4.1) to 1%.

Second, it is plausible that unscheduled elections might be endogenous to corruption cases registered and investigated, especially if the incumbent chooses to call early elections. The issue is not trivial, because those who argue that the timing of unscheduled elections are sudden and unanticipated also make causal claims that governance related issues might trigger unscheduled elections. Also, controlling corruption in a state might be correlated with other omitted variables such as civic associations, which I have not accounted for in the model. In order to deal with this problem, I use an instrumental variable approach wherein I instrument for the endogenous unscheduled elections. The factors which cause unscheduled elections, as

[73] Unfortunately, the NCRB does not provide monthly information on the number of corruption cases being registered and investigated by the respective state anti-corruption agencies.

argued by Khemani (2004), are numerous. These include imposing President's rule, political instability and an incumbent using the option of calling early elections. Keeping in mind these three factors, I use three different instrumental variables which proxy these three political factors. First, I use a dummy coding the value 1 for a state if the incumbent party or the leading party of a coalition government, from which the Chief Minister of the state *i* in year *t* comes from, belongs to the same party as that of the central government (or the leading party of a coalition government), and 0 otherwise. It has been pointed out that the imposition of President's rule, either due to political volatility or law and order problems in states, is more likely in states where the ruling party is not aligned with the center (Arulampalam et al. 2009, Khemani 2004). Second, political volatility in a state is also associated with fragile coalition governments or splits within the party. Several instances in Indian states viz., Arunachal Pradesh, Assam, Goa, Haryana, Jharkhand, Karnataka, Uttar Pradesh, among others, point towards this. I dummy code a value 1 for state *i* in year *t* if it is governed by a coalition government, and 0 otherwise, to capture political instability. Finally, incumbent governments can call early elections based on macroeconomic conditions (Pluemper et al. 2008). Using economic variables might violate the instrument exclusion restriction. I therefore use the seat share of the single largest party in the ruling government in the state legislative assembly. I expect incumbents whose seat share is below the threshold of a simple majority (i.e., 51% of seats) have more incentive to call early elections compared to 'survivors' lasting a full five-year term.

The normal procedure in the next step would be to utilize the two-stage least squares (2SLS – IV hereafter) method. However, employing instrumental variables in non-linear models such as negative binomial may be problematic. I therefore regress the endogenous variable – unscheduled elections – on the selected instruments using the conditional logit fixed effects (which is the first stage regression). I then predict the values of the endogenous

variable and regress the dependent variables – number of corruption cases registered and investigated – using negative binomial estimations (second stage regressions). The 2SLS–IV estimations with fixed effects were employed to check the validity of the instruments, which depends on two conditions. Firstly, the instruments must be correlated with the explanatory variable in question – otherwise they have no power. The variables discussed above are expected to be correlated with the endogenous variable. Secondly, the instruments should not vary systematically with the disturbance term in the second stage equation, i.e., $\left[\omega_{it} \mid IV_{it}\right] = 0$. In other words, the instruments cannot have independent effects on the dependent variable. The validity of the instruments is checked by the Hansen J-test, and the null-hypothesis of exogeneity cannot be rejected at a conventional level of significance.

After controlling for endogeneity associated with the unscheduled elections, using the predicted values generated from the first stage regression remains statistically insignificant. It is interesting to note that in the baseline models the unscheduled elections was negative and significant at 5% level in column 3 of table 4.1. After controlling for possible reverse feedback effects, it becomes statistically insignificant. Thus, after controlling for possible endogeneity concerns, the impact of unscheduled elections on both cases registered and investigated remains statistically insignificant.[74] The results with respect to the selected instruments in the first-stage analysis find that the coalition state governments dummy, and the seat share of the single largest party in the state assembly, explain unscheduled elections which are significantly different from zero at the 5% level, respectively.

Third, I split the sample by years to exclude the first three years after the Prevention of Corruption Act came into being in 1988. The idea behind excluding these first three years is to allow for state governments across the board to establish the required anti-corruption

[74] Note that these results remain the same when estimated using 2SLS-IV method. The joint F-statistics is about 13.3 (at 1% significance) and the Hansen J-statistic remains statistically insignificant at 5% level.

bureaus to tackle corruption in their respective states. The new results do not depict any significant change in the baseline results reported in tables 4.1 and 4.2. I further excluded another two years from the sample and the results are not drastically different.

Fourth, following Brandt et al. (2000) and King (1988), I replicate the baseline results reported in table 4.1 and 4.2 using an alternative estimate technique, namely a zero-inflated negative binomial method. Although it is true that there is over dispersion in the corruption cases data, it provides a good test for the robustness of my main findings as an alternative estimation technique. The results from the zero-inflated negative binomial estimations do support my earlier baseline findings. This apart, the results also support the previous findings on electoral cycles associated with corruption cases registered. Fifth, as an additional test for robustness, following Dreher and Gassebner (2008), I exclude the few observations with extreme values in both corruption cases registered and investigated. Despite this, the baseline results are qualitatively unchanged, suggesting that the results are not driven by extreme values.

Finally, I also estimate the baseline models on corruption cases registered and investigated using pooled OLS fixed effects method. Both the dependent variables are logged.[75] The main results remain robust to using OLS fixed effects models. However, the statistical significance of schedule elections comes down from 5% to 10% in OLS models. Unscheduled elections remain statistically insignificant. None of the election variables retain statistical significance in the corruption cases investigations. Note that estimating OLS fixed effects with per-capita cases (logged) also yields identical results. The robustness check results are not shown here due to brevity but are available upon request. In summary, taken together, the results seem to be robust to sample split, alternative specification, data and testing procedure.

[75] I add 0.1 for the 0 observations in both corruption cases registered and investigated before logging.

5.5 Conclusion

The idea that incumbent governments may manipulate fiscal and other economic policies in their bid to increase re-election chances, have been well documented, not only in the context of industrialized countries, but also among a cross-section of developing countries. However, almost all of these studies predominantly focus on macroeconomic policy outcomes. In this paper, I presume that policy manipulation to maximize the chances of re-election can also be associated with key governance related issues. Most relevant among these is the controlling of corruption, which is widely seen as India's pressing problem today. This paper attempts to fill this gap by identifying the effects of elections on controlling corruption, i.e., whether the timing of elections affects the responsiveness of incumbent governments in controlling corruption in India. Specifically, I examine the effects of state legislative assembly elections on the total number of corruption cases registered in that particular year, as well as the total number of corruption cases under investigation by the respective states' vigilance bureaus and anti-corruption agencies. Much of what we know about political influence on anti-corruption institutions to control corruption is based on anecdotal evidence. This paper represents a contribution to a new literature using innovative data to demonstrate empirically the effects of elections on anti-corruption activities of the incumbent government.

My findings are easily summarized. Using panel data on 30 Indian states during the 1988–2009 period, consistent with the idea that an incumbent might exert greater effort in an election year to control corruption, I find that scheduled elections are associated with an increase in the number of corruption cases registered, while unscheduled elections are not given that their timing is sudden and unanticipated. In addition, I find that the cases registered tend to increase as a scheduled election year draws closer. Furthermore, I find these effects to be stronger in 'swing states', where the margin of victory of the incumbent in the previous

election was 5% or less, as well as in those state scheduled election years which coincide with national elections. On the other hand, I do not find any impact of scheduled elections and the electoral cycle on corruption cases being investigated by anti-corruption agencies. Even after controlling for endogeneity associated with unscheduled elections, using instrumental variable estimations, it is scheduled elections, and not unscheduled elections, which seem to predict the increase in corruption cases registered.

In sum, three main policy implications follow from these results. First, the results highlight that political budget cycles are not restricted to economic and fiscal policies alone. Rather, such cycles are also associated with key governance related issues such as the control of corruption. If the effective provision of public goods shapes voters' perception on governments, then considering the control of corruption as a basic public good would provide incentives for incumbent politicians to manipulate anti-corruption activities in the hope of influencing election outcomes. Thus, greater attention should be paid to the role of political manipulation of anti-corruption institutions which is widely believed to be the case in developing world. Second, the interesting but contrasting results related to corruption cases registered vis-à-vis investigated; suggest that politicians engage in 'cheap talk' on controlling corruption, especially during the election period. It appears that they do not display political will thereafter to effectively combat the menace of corruption. This also means that politicians, willingly or unwillingly, are unable to ensure a consistent level of effective policy framework (such as strengthening anti-corruption institutions and laws) to try prevent corruption during their tenure in office. Manipulating anti-corruption agency during elections for electoral gains has serious implications for the anti-corruption policy not just in India but across the developing world. The potential long term effects could be politicization and weakening the very institution meant for tackling corruption. Finally, these findings highlight the need for strong anti-corruption institutions, both at national and state levels in India,

which do not come under the influence of politicians and are not merely advisory in nature, with no independent investigative and prosecution powers, as has been advocated by several civil society groups such as India Against Corruption (IAC), the National Campaign for People's Right to Information (NCPRI), and Loksatta. This could help effectively control corruption to a large extent.[76] Apart from independence, empowering these new institutions so that they cover all elected representatives, higher and lower levels bureaucrats, and other public servants under its ambit, is also vital. In addition, enacting new and effective laws to protect whistleblowers and incorporate a grievance redressal system, coupled with judiciary reforms, is also important because the corrupt in India, like in other developing countries, face little deterrent due to long delays in prosecuting perpetrators in court (Chemin 2008).

Future studies might look more carefully at separating top and low-level cases of corrupt government bureaucrats, and examine if incumbent politicians have any incentives to react differently towards these groups during the election period. In addition, if politicians think that the expected payoff from engaging in 'cheap talk' on controlling corruption during elections is higher, then it would be worthwhile to examine whether this 'cheap talk' helps the incumbent's chances of re-election in state legislative assembly elections.

[76] Refer to Government of India's *Lokpal* (Ombudsman) Bill version: http://www.box.net/shared/k9bz7pfzj6q6s0us9mil. India Against Corruption's (IAC) Jan Lokpal Bill version: http://www.lokpalbillconsultation.org/docs/lokpalbill2_2.pdf. Refer NCPRI's version of Bill: http://righttoinformation.info/ and refer Loksatta's Lokpal Bill versions: http://www.loksatta.org/cms/documents/lokpal/LokpalBill_LSandFDRposition.pdf

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Table 4.1: Elections, Electoral cycle and control of corruption (cases registered)

Variables	(1) Cases Registered	(2) Cases Registered	(3) Cases Registered	(4) Cases Registered	(5) Cases Registered
	Negative Binomial	Negative Binomial	Negative Binomial	Negative Binomial	Negative Binomial
All State Elections	0.0454				
	(0.0814)				
State Scheduled Elections		0.222**		0.202*	
		(0.111)		(0.111)	
State Unscheduled Elections			-0.248***	-0.220**	
			(0.0923)	(0.0927)	
One-year from State Scheduled Elections					0.0156
					(0.111)
Two-years from State Scheduled Elections					-0.117
					(0.0959)
Three-year from State Scheduled Elections					-0.187*
					(0.100)
Four-year from State Scheduled Elections					-0.185*
					(0.0961)
State GDP in Constant Prices (log)	0.341**	0.340**	0.327**	0.329**	0.333**
	(0.135)	(0.135)	(0.133)	(0.133)	(0.142)
Total Population (log)	0.519	0.463	0.512	0.451	0.594
	(0.693)	(0.681)	(0.685)	(0.671)	(0.711)
Newspapers circulation per head (log)	0.201**	0.198**	0.204**	0.200**	0.199**
	(0.0925)	(0.0922)	(0.0913)	(0.0913)	(0.0931)
IPC Cases per Police	-0.0259	-0.0211	-0.0406	-0.0317	-0.0211
	(0.0773)	(0.0763)	(0.0788)	(0.0768)	(0.0775)
Lokayukta Act (dummy)	0.0711	0.0686	0.0653	0.0649	0.101
	(0.108)	(0.109)	(0.107)	(0.108)	(0.108)
Indian National Congress ruling years (dummy)	-0.313***	-0.352***	-0.277***	-0.328***	-0.324***
	(0.103)	(0.104)	(0.0932)	(0.103)	(0.0980)
BJP ruling years (dummy)	-0.154*	-0.183**	-0.102	-0.146	-0.153*
	(0.0935)	(0.0915)	(0.0852)	(0.0932)	(0.0870)
Left Front ruling years (dummy)	0.207	0.182	0.260	0.220	0.216
	(0.170)	(0.172)	(0.162)	(0.170)	(0.172)
Regional Parties ruling years (dummy)	0.127	0.0986	0.156*	0.119	0.113
	(0.0901)	(0.0899)	(0.0878)	(0.0896)	(0.0874)
Wald chi2	4597***	4655***	4642***	4659***	4777***
Goodness-of-Fit test	15971***	16022***	15993***	15798***	15496***
Number of States	30	30	30	30	30
Number of Observations	595	595	595	595	595

Notes: (a) Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

(b) Reports coefficients of all explanatory variables.

Table 4.2: Elections, Electoral cycle and control of corruption (cases investigated)

Variables	(1) Cases Investigated	(2) Cases Investigated	(3) Cases Investigated	(4) Cases Investigated	(5) Cases Investigated
	Negative Binomial	Negative Binomial	Negative Binomial	Negative Binomial	Negative Binomial
All State Elections	0.0984				
	(0.0709)				
State Scheduled Elections		0.137		0.140	
		(0.0892)		(0.0902)	
State Unscheduled Elections			0.0219	0.0401	
			(0.101)	(0.102)	
One-year from State Scheduled Elections					-0.203**
					(0.0848)
Two-years from State Scheduled Elections					-0.195**
					(0.0843)
Three-year from State Scheduled Elections					-0.175**
					(0.0803)
Four-year from State Scheduled Elections					-0.102
					(0.0896)
State GDP in Constant Prices (log)	0.210**	0.208**	0.210**	0.209**	0.177*
	(0.105)	(0.103)	(0.103)	(0.104)	(0.107)
Total Population (log)	-0.667	-0.664	-0.673	-0.663	-0.509
	(0.805)	(0.806)	(0.803)	(0.805)	(0.803)
Newspapers circulation per head (log)	0.0387	0.0375	0.0399	0.0374	0.0290
	(0.0924)	(0.0926)	(0.0927)	(0.0925)	(0.0897)
IPC Cases per Police	-0.126**	-0.130**	-0.130**	-0.128**	-0.135**
	(0.0584)	(0.0582)	(0.0586)	(0.0588)	(0.0578)
Lokayukta Act (dummy)	0.185**	0.182**	0.183**	0.181*	0.210**
	(0.0927)	(0.0927)	(0.0923)	(0.0928)	(0.0929)
Indian National Congress ruling years (dummy)	-0.135*	-0.135*	-0.101	-0.140*	-0.122
	(0.0794)	(0.0796)	(0.0745)	(0.0804)	(0.0759)
BJP ruling years (dummy)	-0.113	-0.105	-0.0777	-0.112	-0.104
	(0.0793)	(0.0773)	(0.0748)	(0.0791)	(0.0760)
Left Front ruling years (dummy)	0.228	0.242	0.266*	0.232	0.218
	(0.156)	(0.156)	(0.157)	(0.157)	(0.151)
Regional Parties ruling years (dummy)	-0.0913	-0.0924	-0.0699	-0.0958	-0.0861
	(0.0739)	(0.0736)	(0.0712)	(0.0743)	(0.0712)
Wald chi2	5853***	5932***	5921***	5856***	5826***
Goodness-of-Fit test	16833***	16828***	16837***	16554***	16638***
Number of States	30	30	30	30	30
Number of Observations	595	595	595	595	595

Notes: (a) Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

(b) Reports coefficients of all explanatory variables.

Table 4.3: Elections and corruption cases in swing-states and national elections

Variables	(1) Cases Registered	(2) Cases Investigated	(3) Cases Registered	(4) Cases Investigated	(5) Cases Registered	(6) Cases Investigated	(7) Cases Registered	(8) Cases Investigated
	Negative Binomial	Negative Binomial	Negative Binomial	Negative Binomial	Negative Binomial	Negative Binomial	Negative Binomial	Negative Binomial
State Scheduled Elections in Swing States	0.440**	0.167						
	(0.204)	(0.143)						
State Scheduled Elections in Non-Swing States	0.0906	0.119						
	(0.112)	(0.105)						
State Unscheduled Elections in Swing States			-0.159	0.0803				
			(0.132)	(0.139)				
State Unscheduled Elections in Non-Swing States			-0.323***	-0.0274				
			(0.123)	(0.133)				
State Scheduled Elections with National Elections					0.313*	0.416**		
					(0.184)	(0.200)		
State Scheduled Elections without National Elections					0.190	0.0196		
					(0.134)	(0.0875)		
State Unscheduled Elections with National Elections							-0.174	-0.0841
							(0.113)	(0.134)
State Unscheduled Elections without National Elections							-0.317**	0.105
							(0.149)	(0.139)
State GDP in Constant Prices (log)	0.360***	0.210**	0.336**	0.215**	0.340**	0.212**	0.324**	0.215**
	(0.134)	(0.103)	(0.134)	(0.104)	(0.135)	(0.102)	(0.134)	(0.104)
Total Population (log)	0.491	-0.653	0.518	-0.673	0.481	-0.645	0.466	-0.634
	(0.676)	(0.808)	(0.684)	(0.802)	(0.679)	(0.798)	(0.688)	(0.809)
Newspapers circulation per head (log)	0.194**	0.0365	0.205**	0.0408	0.195**	0.0349	0.201**	0.0400
	(0.0924)	(0.0926)	(0.0910)	(0.0925)	(0.0923)	(0.0923)	(0.0916)	(0.0926)

IPC Cases per Police	-0.0190	-0.130**	-0.0378	-0.129**	-0.0241	-0.136**	-0.0439	-0.127**
	(0.0748)	(0.0582)	(0.0790)	(0.0586)	(0.0765)	(0.0582)	(0.0785)	(0.0584)
Lokayukta Act (dummy)	0.0827	0.184**	0.0642	0.181*	0.0848	0.214**	0.0620	0.186**
	(0.109)	(0.0932)	(0.107)	(0.0924)	(0.111)	(0.0934)	(0.106)	(0.0919)
Indian National Congress ruling years (dummy)	-0.358***	-0.137*	-0.284***	-0.104	-0.359***	-0.143*	-0.280***	-0.0941
	(0.104)	(0.0800)	(0.0936)	(0.0750)	(0.106)	(0.0803)	(0.0936)	(0.0740)
BJP ruling years (dummy)	-0.190**	-0.106	-0.105	-0.0785	-0.188**	-0.102	-0.103	-0.0733
	(0.0919)	(0.0778)	(0.0853)	(0.0749)	(0.0933)	(0.0776)	(0.0853)	(0.0745)
Left Front ruling years (dummy)	0.190	0.241	0.252	0.259	0.179	0.228	0.258	0.263*
	(0.170)	(0.155)	(0.163)	(0.158)	(0.171)	(0.156)	(0.162)	(0.156)
Regional Parties ruling years (dummy)	0.0983	-0.0926	0.152*	-0.0726	0.0967	-0.0956	0.153*	-0.0673
	(0.0897)	(0.0735)	(0.0871)	(0.0714)	(0.0899)	(0.0735)	(0.0881)	(0.0714)
Wald chi2	4623***	5934***	4688***	5919***	4609***	5936***	4631***	5939***
Goodness-of-Fit test	16012***	16827***	15890***	16717***	16004***	16817***	15973***	16819***
Number of States	30	30	30	30	30	30	30	30
Number of Observations	595	595	595	595	595	595	595	595

Notes: (a) Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

(b) Reports coefficients of all explanatory variables.

Figure 2.1: State-wise corruption cases registered

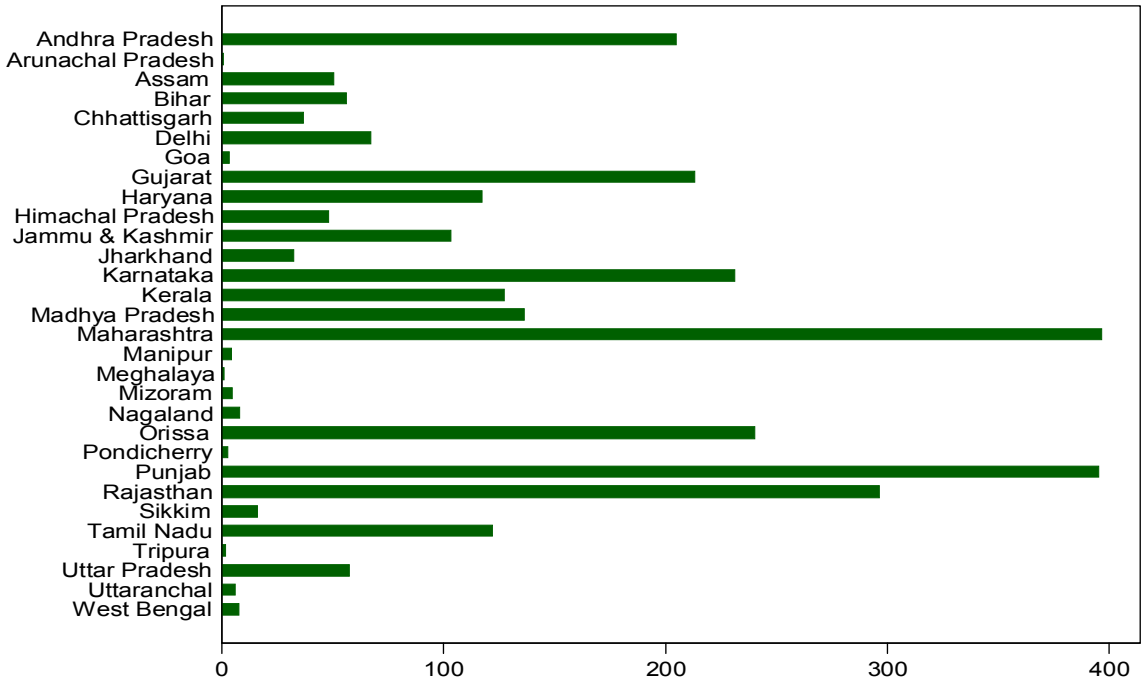


Figure 2.2: State-wise corruption cases investigated

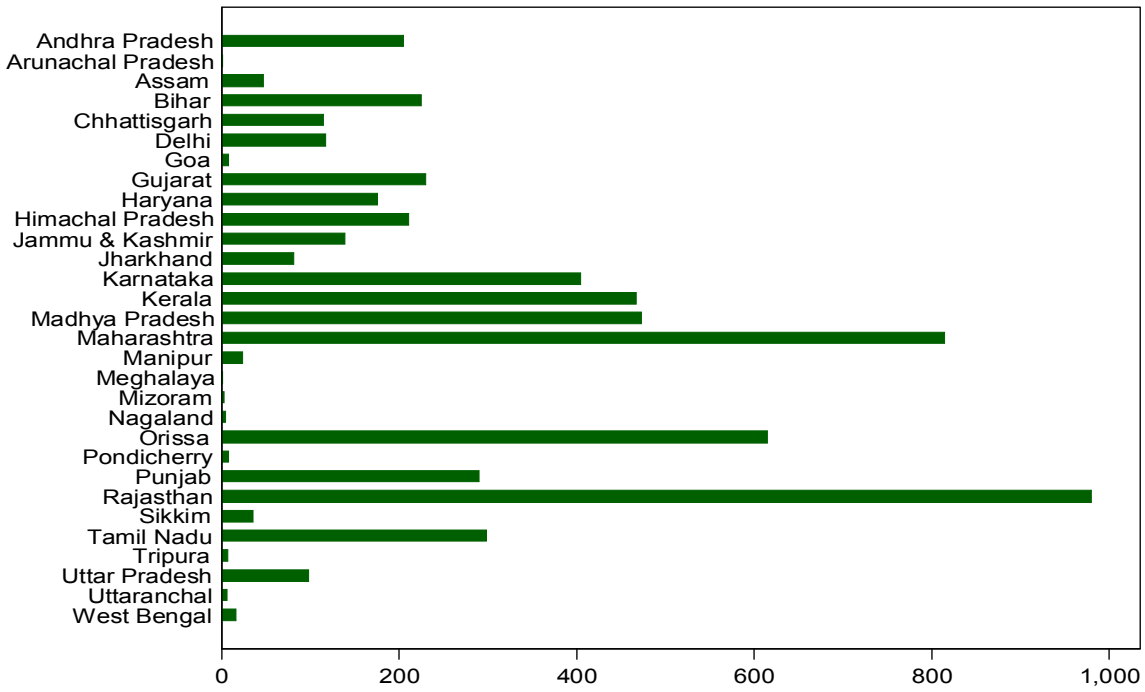
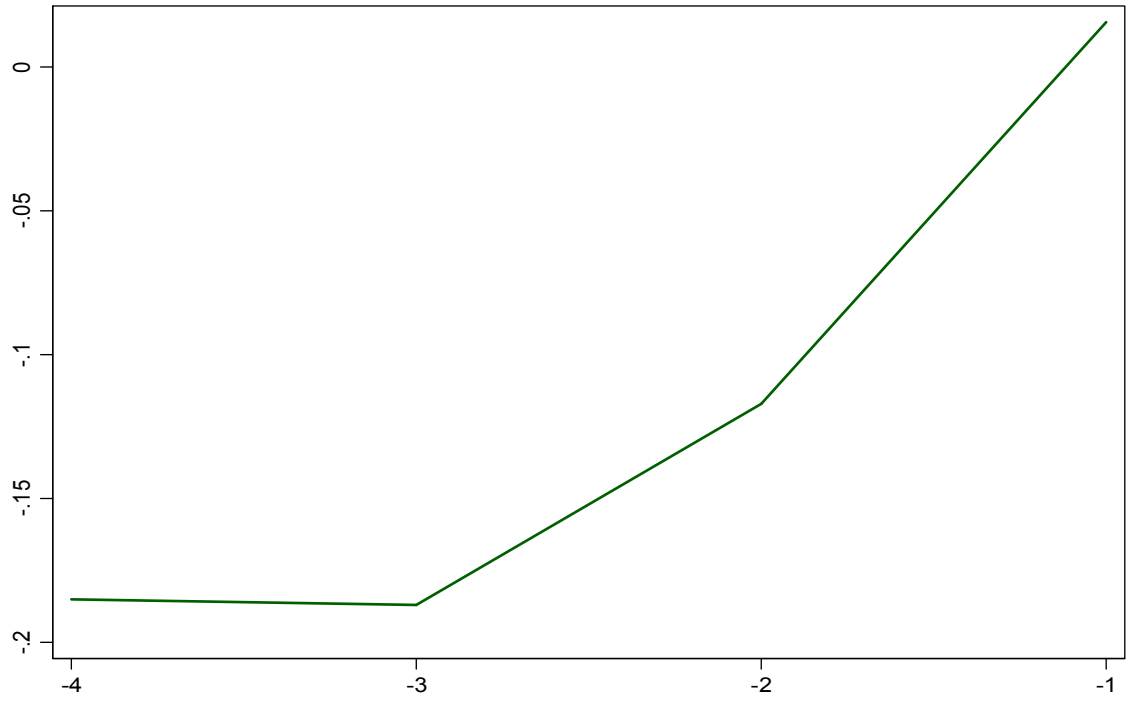


Figure 2.3: Electoral cycle on corruption cases registered



5.7 Appendix

Table 4.4: List of states under study

Andhra Pradesh	Haryana	Manipur	Sikkim
Arunachal Pradesh	Himachal Pradesh	Meghalaya	Tamil Nadu
Assam	Jammu & Kashmir	Mizoram	Tripura
Bihar	Jharkhand	Nagaland	Uttar Pradesh
Chhattisgarh	Karnataka	Orissa	Uttaranchal
Delhi	Kerala	Pondicherry	West Bengal
Goa	Madhya Pradesh	Punjab	
Gujarat	Maharashtra	Rajasthan	

Table 4.5: Data definitions and sources

Variables	Definitions and data sources
Corruption cases Registered	Total number of corruption cases registered by state police, CBI corruption branch under Prevention of Corruption Act, 1988 in state *i* in year *t*. The data was obtained from various Annual reports on economic offences from National Crimes Records Bureau, Government of India, New Delhi.
Corruption cases Investigated	Total number of corruption cases being investigated by state police and CBI corruption branch under Prevention of Corruption Act, 1988 in state *i* in year *t*. The data was obtained from various Annual reports on economic offences from National Crimes Records Bureau, Government of India, New Delhi.
Scheduled and Unscheduled elections	Dummy coding if the state *i* in year *t* witnessed a scheduled election or unscheduled election and 0 otherwise respectively. The data was obtained from various state assembly election reports published by the Election Commission of India.
Electoral Cycle	Separate dummies coding the value of 1 if a state *i* in year *t* is one-year, two-years, three-years and four-years away from an scheduled election and 0 otherwise. The data was own construction based on the information published by Election Commission of India on various state legislative assembly elections.
State GDP (log)	State GDP in 1993-94 constant prices (Indian Rupees) from Reserve Bank of India.
Newspapers' circulation	Total number of newspapers including English, Hindi and local languages circulation in thousands per head in state *i* in year *t* obtained from Press Registrar of India.
Lokayukta Act dummy	Dummy coding whether a state has Lokayukta Act (Ombudsmen) in place and 0 otherwise. The data was own construction based on the information provided by respective state governments.
Political Parties in power	Dummy coding whether a state is ruled by Indian National Congress, Bharatiya Janata Party, Left Front and Regional Party and 0 otherwise respectively. The data was own construction based on the information published by Election Commission of India.
Total cases under investigation per police	Total number of criminal cases (as per Indian Peneal Code – IPC) being investigated by police as a share of total civil police force in state *i* in year *t*. The data was obtained from various Annual reports on state-level police infrastructure from National Crimes Records Bureau, Government of India, New Delhi.

Table 4.6: Descriptive Statistics

Variables	Mean	Standard Deviation	Minimum	Maximum	Observations
Number of Corruption cases registered	103.983	166.499	0	2598	596
Number of Corruption cases investigated	211.239	284.143	0	1572	595
State Scheduled Elections	0.128	0.335	0	1	624
State Unscheduled Elections	0.085	0.279	0	1	624
One-year from State Scheduled Elections	0.171	0.377	0	1	624
Two-years from State Scheduled Elections	0.183	0.387	0	1	624
Three-year from State Scheduled Elections	0.192	0.394	0	1	624
Four-year from State Scheduled Elections	0.208	0.406	0	1	624
State GDP in Constant Prices (log)	10.139	1.667	6.31	13.15	639
Newspapers circulation per head (log)	-1.284	1.395	-4.43	1.26	624
IPC Cases per Police	1.899	1.053	0.2	4.7	623
Lokayukta Act (dummy)	0.513	0.500	0	1	624
Indian National Congress ruling years (dummy)	0.494	0.500	0	1	624
BJP ruling years (dummy)	0.199	0.399	0	1	624
Left Front ruling years (dummy)	0.093	0.291	0	1	624
Regional Parties ruling years (dummy)	0.375	0.485	0	1	624

6. Chapter 5

The Needy Donor: An Empirical Analysis of India's Aid Motives[77]

6.1 Introduction

India, widely seen as one of the success stories of globalization, has significantly accelerated its economic growth since the inception of economic reforms in 1991 (Basu and Maertens 2007, Basu 2008, Panagariya 2010). The country is one of the fastest growing economies in the world and host to some of the largest foreign investment inflows in recent years (UNCTAD 2010). Yet, for many, India's progress since its independence 65 years ago is disappointing. Despite rapid economic growth over the last decade, some areas in India continue to be severely underdeveloped (Banerjee 2010). India has a large domestic constituency of people suffering from underdevelopment, chronic poverty and mal-governance. According to the World Bank's (2011) estimates, 37% of the Indian population is below the poverty line of US$ 1.25 a day. Moreover, India ranks below its neighbors Bangladesh, Bhutan, Nepal, Pakistan and Sri Lanka in terms of life expectancy, access to sanitation, infant immunization, and underweight children. It

[77] Coauthored with Andreas Fuchs, University of Heidelberg.

also ranks below Bangladesh, Bhutan and Sri Lanka in controlling the infant mortality rate (Drèze and Sen 2011), below Sri Lanka in terms of the literacy rate and access to education (UNESCO 2011), below Nepal in the 2011 Global Hunger Index (IFPRI 2011), and below Bangladesh with respect to controlling literacy among female youths (Drèze and Sen 2011).

Therefore, it is not surprising to note that despite its rapid economic growth in recent years, India still receives development aid. In 2009, the total net official development assistance received by India from all donor countries was about US$ 2.502 billion, of which US$ 1.578 billion was in the form of net bilateral aid flows from countries organized in the Development Assistance Committee (DAC) (OECD 2012).[78] At US$ 630 million, India is still the single largest recipient of development aid from the United Kingdom (OECD 2012). [79] That being said, it is puzzling to note that India itself is an aid donor.[80] In fact, Indian engagement in delivering foreign aid goes back to the 1950s, with its primary target being to provide development assistance to neighboring countries. Traditionally, Indian foreign aid has focused on technical assistance. Ever since it began in 1964, the Indian Technical and Economic Cooperation (ITEC), India's flagship external assistance program, has provided training, education and technical expertise to about 40,000 NGO personnel, scholars and leaders from developing countries (Agrawal 2007).

Over the last few years, aid from India has diversified and gained prominence. During the economic reforms period spanning from 1992 to 2009, official foreign assistance provided under

[78] The DAC is a donor organization that consists of the European Union and 23 OECD countries. Specifically, Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Korea, Luxembourg, the Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, the United Kingdom and the United States are currently DAC members.

[79] Moreover, India also receives a substantial amount of aid from international NGOs. For example, in 2010, the Bill & Melinda Gates Foundation committed US$ 100 million to India (OECD 2012).

[80] Note that India avoids the term 'donor'. It rather perceives itself as a partner in South-South cooperation (see Chaturvedi 2008 for a discussion).

the umbrella of the Ministry of External Affairs (MEA) amounted to 18,950 crores Indian rupees (US$ 4.47 billion) according to its annual reports (MEA 1993-2010). The Ministry allocated 2,359 crores Indian rupees (US$ 444 million) to aid-related activities in the 2009 financial year alone (MEA 2010). According to Manning (2006: 375), India, together with China, is one of the two 'heavyweights' among the non-DAC donors. India's increased commitment to providing development aid is reflected in the government's decision to setup a separate agency by 2012 in order to oversee the aid allocation process (Patel 2011).

In contrast to the extensive empirical literature on the allocation of development aid from Western donor countries (e.g., Alesina and Dollar 2000), studies on development assistance provided by non-DAC donors lack rigorous empirical analysis. Notable exceptions are Neumayer (2003a, 2004) on Arab aid, Dreher and Fuchs (2011) on China's foreign assistance, and Dreher et al. (2011) on aid from donors outside the DAC in general (excluding India).[81] Concerning India's foreign aid in particular, to the best of our knowledge, no prior study provides an econometric analysis of the determinants of India's aid allocation decisions. This paper aims to fill this gap in the literature. A better understanding of the factors driving India's aid allocation decisions may offer important insights into why poor countries serve as donors of foreign aid to other developing countries.

India claims that its aid is more need-oriented than aid from richer donor countries as its economic and political structure is closer to that of other developing countries. If this is the case, India should provide more aid to countries that are closer to India in terms of economic development. We test this prediction empirically. At the same time, many suspect that India might be increasingly using foreign aid as an instrument to gain access to overseas markets for

[81] Given that India is poorer in terms of income per capita than any of the donors covered in Dreher et al. (2011), India serves as an excellent case to study the behavior of "needy" donors.

its goods and services, pave the way for Indian investment abroad, and secure access to natural resources (e.g., Agrawal 2007, Kragelund 2008). Another argument put forward is that Indian aid is extensively used as a foreign policy tool to expand the country's geopolitical and diplomatic influence (e.g., Agrawal 2007). The consensus in the literature is that political and commercial interests are important determinants of aid allocation for the DAC group of "rich" donors (e.g., Alesina and Dollar 2000, Neumayer 2005, Kuziemko and Werker 2006), as well as for multilateral organizations (e.g., Dreher et al. 2009, Kilby 2011). Not only do we also expect to find this for the "needy" donor India, we expect these relationships to be even more pronounced. We argue that India has more incentives to provide politically and commercially motivated aid since the country lags behind DAC donors in terms of economic development. We will elaborate this hypothesis below and test it empirically.

Our findings show that India's aid allocation is partially in line with our expectations of the behavior of a "needy" donor. Commercial and political self-interests dominate India's aid allocation. We find the importance of political interests, proxied by the voting alignment between donor and recipient in the United Nations, to be significantly larger for India than for all DAC donors. Moreover, we find that the "needy" donor favors countries which are closer geographically and that countries at a similar developmental stage are more likely to enter India's aid program.

The paper is structured as follows. Section 6.2 introduces India's foreign aid program and examines its evolution over time. Based on the previous aid literature, Section 6.3 develops our hypotheses on the aid allocation behavior of a "needy" donor. In Section 6.4, we empirically analyze the determinants of aid allocations by the MEA based on data for the years 2008-2010 from AidData, a project-level database (Tierney et al. 2011). To analyze whether Indian aid is

special, we further compare India's aid allocation decisions with those of other donors. In particular, we test whether Indian aid is motivated to a higher extent by political and commercial considerations and to a lesser extent by recipient needs compared to aid from "rich" donors. Finally, Section 6.5 summarizes our results, concludes, and provides policy implications.

6.2 An Overview of India's Aid Program

The origins of Indian development aid date back to the Colombo Plan of 1950, which a group of Commonwealth countries (including India) formulated in Sri Lanka with the objective of providing assistance to developing countries in order to raise their respective living standards. Along with the Colombo Plan, India started providing aid in the form of grants and loans. India's primary target in its early days after independence was to support neighboring countries, in particular Bhutan, Myanmar, and Nepal.[82] However, despite its active role, Indian development aid largely remained confined to the field of technical assistance, mainly due to resource scarcity and strong demand for developmental funds within the country.[83] As a founding member of both groups of states, India's aid program was anchored in the Non-Aligned Movement and the Group of 77 at the United Nations.

After the collapse of the USSR and a severe balance-of-payments crisis, India introduced pro-market economic reforms in 1991. Eventually, as the economy grew stronger, India deepened its engagement with developing countries and extended its aid program. The 2003-04 budget speech is considered as a sharp break in India's role as an actor in international development cooperation. India wanted to be perceived primarily as an aid donor and not as a

[82] For 1958, Chanana (2009) highlights Indian aid commitments of about Rs. 100 million (US$ 21 million) in multi-year grants to Nepal, Rs. 200 million (US$ 42 million) to Myanmar, and the financing of 60% of Bhutan's budget.
[83] According to Dutt (1980), a total of 1,442 people received technical training in India under the Colombo Plan up until 1960. According to the Colombo Plan Reports (as cited in Dutt 1980), this number increased to 3,550 between 1961 and 1971.

recipient of foreign assistance. Following the speech, India announced several key changes to its development cooperation (e.g., Price 2004). First, the country would only accept government-to-government aid that is untied and provided by five selected countries or the European Union. Second, India would repay its debt to most of its bilateral donors and multilateral institutions. Third, it would extend its own aid effort to other developing countries through debt cancellations for some Highly Indebted Poor Countries, and an increase in its grant and project assistance under the so-called India Development Initiative. Although the actual policy changes were softer in the beginning than the speech seemed to imply (see Price 2004 for a discussion), it became clear that India intended to play an important role in the world of international development cooperation. The provision of credit lines via India's Exim Bank is one of the most prominent outcomes of these reforms.

To provide a better understanding of how India's aid program evolved over time, we compiled data on India's aid budget since 1966 based on the annual reports of the Ministry of External Affairs (MEA 1967–2011).[84] This information needs to be interpreted with caution because of significant changes over time in the way the ministry categorizes its aid amounts.[85] Apart from that, note that the data exclude aid flows from institutions other than the MEA. Moreover, we lack detailed information on which fraction of the calculated aggregated aid values satisfy the OECD's definition of Official Development Assistance (ODA). Nevertheless, the figures should provide the reader with an intuition of the overall evolution of the size of India's

[84] Note that the DAC defines ODA as financial flows to developing countries provided by official agencies with the objective to promote economic development and welfare, and that contain a grant element of at least 25% (see http://www.oecd.org/dataoecd/26/14/26415658.PDF, accessed August 2011). Although we lack detailed information on the concessionality of each individual loan, it seems that aid provided by the MEA by and large qualifies as ODA. According to a study by ECOSOC (2008), 80% of India's total aid disbursed is grants. The remaining fraction is loans with an estimated grant element of 53-57%.

[85] Values for grant-in-aid to the Indian Council of Cultural Relations and support to the African National Congress are excluded from our analysis. See Agrawal (2007) for a discussion of limitations of the use of data from MEA annual reports as a proxy for India's aid budget.

aid program. As can be seen from figure 3.1, there is a spike in India's aid budget in 1972.[86] This is largely due to the additional external assistance provided by India to Bangladesh, which obtained independence from then West Pakistan (now Pakistan) in 1971 with the help of India. According to the MEA annual report in 1973, India allocated about 167.6 crores Indian rupees (about US$ 369.7 million in 2000 constant prices) of aid to Bangladesh in 1972 (mostly in the form of grants and concessional loans).

India's aid disbursements suffered a decline during the early 1990s, a period marred by balance-of-payments problems and political crises. However, from the mid-1990s onwards, there has been a surge in disbursements of development aid. Though there were ups and downs, which could be attributed to the change in government in 2004 and to the Global Financial Crisis starting in 2008, India's aid budget shows an increasing trend since the mid-1990s. Taken together, India's aid budget rose from 13.4 crores Indian rupees (about US$ 40.3 million in constant 2000 prices) in 1966, to 2,917.4 crores Indian rupees (US$ 362.8 million in constant 2000 prices) in 2010, which is roughly 0.04% of India's GDP. This amount, which only captures MEA aid, is comparable to Austria's total bilateral ODA (US$ 395.2 million in constant 2000 prices) and amounts to about two thirds of Italy's total bilateral ODA (US$ 547.0 million in constant 2000 prices).[87]

In addition to the MEA, India provides concessional finance via its Export-Import (Exim) Bank. The sum of all financial flows provided by the Exim Bank between 2005 and 2009 and registered on AidData (Tierney et al. 2011) amounts to US$ 2.45 billion (in constant 2000 prices). In contrast to MEA aid, the largest share of Exim Bank loans (73.2%) was allocated to

[86] Using data on India's GDP deflator and exchanges rates obtained from the World Development Indicators (available at http://databank.worldbank.org, accessed May 2012), we converted all aid values from Indian rupees in current prices to constant 2000 US$.

[87] A comparison with the figures on non-DAC donors provided in Dreher et al. (2011: 1952) underlines that India is one of the most important providers of development assistance outside the DAC.

Sub-Saharan African countries. Although Sinha and Hubbard (2011) find that most credits satisfy the criteria of a grant element of at least 25%, they conclude that Indian LOCs do not qualify as ODA as defined by the OECD. Since the credit lines are extended for the purpose of export promotion, these flows meet the criteria of officially supported export credits instead.[88] Therefore, we restrict our empirical analysis below to cover financial flows provided by the MEA only.

6.3 Hypotheses

The extensive literature on the allocation of development aid emphasizes that aid from Western donors and multilateral institutions is guided by strategic interests, in addition to economic needs in developing countries (Alesina and Dollar 2000, Kuziemko and Werker 2006, Dreher et al. 2009, Kilby 2009a). In contrast, research on non-DAC aid is still in its infancy. Manning (2006), ECOSOC (2008) and Kragelund (2008, 2010) provide good overviews of the aid activities of these so-called new donors. Among the few econometric studies on aid allocation by non-DAC donors are Neumayer (2003a, 2004) on Arab aid, Dreher and Fuchs (2011) on China's foreign assistance, and Dreher et al. (2011) on aid from donors outside the DAC in general. The literature usually groups the determinants of a donor's aid allocation into three categories. First, aid allocation follows recipient needs. Based on humanitarian motives, altruist countries provide more assistance to poorer countries. An important goal is poverty reduction. Second, aid is allocated based on good policies. Following the idea of merit, countries with good policies and good institutions are supported through increased aid flows. Third, donors' aid patterns are

[88] According to Sinha and Hubbard, the grant element varies between 41.25% for Heavily Indebted Poor Countries (HIPC) and 17.11% to 24.56% for middle income countries with medium to high levels of debt.

shaped by political and commercial self-interests. In the following, we discuss whether and how these motives are reflected in India's aid policy.

Referring to the role that Indian values might play in India's aid provision, Meier and Murphy (2011: 7) point out that, "Hinduism, Buddhism, Islam and Sikhism all espouse solidarity with the suffering and giving without expectations for return." In line with this, the Indian government claims that its aid program indeed responds to the economic needs of developing countries. For example, the MEA describes the ITEC program as "an earnest attempt by India to share the fruits of its [i.e., India's] socio-economic development and technological achievement with other developing countries" (ITEC 2011). According to Banerjee (1982: 27), India provides aid to neighboring countries "with the sole objective of restoring the local citizens to a place of primacy." If this is the case, India's aid should be targeted to needier countries.[89] We test the following hypothesis:

Hypothesis 1a: *India's aid allocation responds to the economic needs of developing countries.*

In this regard, Banerjee (1982: 55) claims that India's aid is particularly need-oriented since it provides the "appropriate technology and managerial experience" to other developing countries. He argues that India's aid is more need-oriented than aid provided by "rich" donors as its economic and political structure is closer to that of other developing countries. Similarly, the Indian MEA claims that it "possess[es] skills of manpower and technology more appropriate to the geographical and ecological conditions and the stage of technological development of several developing countries."[90] If we take this argument at face value, this implies that India should

[89] Dreher et al. (2011), in turn, find that non-DAC donors care less for recipient need than traditional DAC donors. Note, however, that their study excludes aid from India.

[90] Quoted on several websites of Indian embassies, e.g., the Indian embassy in Azerbaijan: http://indianembassybaku.org/en/8/ (accessed: February 8, 2012).

allocate more aid to countries that are at a similar stage of development. Consequently, aid from India should decrease with a recipient country's distance to India's own development level. We will test the following hypothesis:

Hypothesis 1b: *The "needy" donor India allocates more aid to countries at a similar stage of development.*

At the same time, India emphasizes that its aid serves "mutual benefit" (ITEC 2011), i.e., its aid allocation is also motivated by Indian interests that are not directly related to the developmental concerns of its partner countries in the developing world. In this regard, the MEA (2004: 133) openly admits that "[t]he Government has been using development aid, including grants and Lines of Credit (LOCs) on concessional terms as tools for promotion of India's political, economic and commercial interests." With respect to commercial interests, Indian aid is seen as an instrument not only to gain access to overseas markets for its goods and services, but also to pave the way for Indian investment abroad (Price 2004, Agrawal 2007, Kragelund 2008). The fact that India's aid is mainly 'tied aid' suggests that commercial interests play a dominant role. Moreover, India's aid is said to be targeted at developing countries possessing oil and other natural resources in order to meet the rising demand for energy resources back home (e.g., Chanana 2009). While the MEA (2009: xiii) admits that its aid was "helping Indian companies get project contracts and orders for supply of goods," it is emphasized that "the LOCs have helped in infrastructure development in these regions thereby creating considerable goodwill for

the country." With respect to the TEAM-9[91] program, Kragelund (2008) also identifies an overlap with the business activities of Indian oil companies.

In addition to commercial interests, the Indian foreign aid program is seen as a foreign policy tool to expand the country's geopolitical and diplomatic influence beyond the South Asian region, as well as an attempt to build military alliances elsewhere (e.g., Agrawal 2007). In this regard, Lafargue (2006) notes that Zambia, an Indian aid recipient, did not criticize India's nuclear tests in 1998 and recognized in 2003 that the Jammu and Kashmir regions are a part of India. Aid is considered a part of India's efforts to obtain support for the country's bid for a permanent seat in the United Nations Security Council (e.g., Kragelund 2008).[92] Moreover, India perceives its aid program as a tool to improve its image around the world. In this regard, the MEA states that the ITEC program "has generated immense goodwill and substantive cooperation among the developing countries," and that it "constitutes an integral part of India's South-South Cooperation effort which has been a traditional pillar of the country's foreign policy and diplomacy" (ITEC 2011). According to Agrawal (2007: 2), India aims to "develop a viable 'pro-India' constituency among key decision makers in recipient countries." Contrasting these views, Banerjee (1982: 54) argues that "India does not provide aid to its neighbours with the hope of extending its influence in the region." He criticizes allegations that India's aid was motivated by selfish motives.[93] Focusing on how India can actually use aid as a foreign policy tool, Dutt (1980) lists five elements: first, to improve bilateral relations, second, to improve India's image, third, to gain leverage and influence over recipient countries, fourth, to reward

[91] The Techno Economic Approach for Africa India Movement (TEAM-9) program offers LOCs to nine West African countries.

[92] Price (2004) hypothesizes that India, as an aid recipient, only accepts aid from three current permanent Council members and from three proposed Council members for the very same reason.

[93] Banerjee (1982) claims that India does not make recipient countries dependent on its assistance, instead strengthening their self-reliance. Moreover, he argues that India has not installed any military bases in a major recipient country.

recipients' policy position, and fifth, to maintain the stability and status quo in recipient countries. Taken together, we test the following hypothesis:

Hypothesis 2a: *India's aid allocation is guided by India's political and commercial self-interests.*

With India emerging on the world stage as a significant provider of development assistance, critics of its aid program question the diversion of resources away from internal development given the chronic socio-economic problems plaguing India. It is this paradox which raises suspicion that India's aid has mainly been allocated in accordance with the country's own interests. We expect a "needy" donor to behave differently than a developed donor country. More precisely, the importance of self-interest should be larger in India's case than for "rich" donor countries for several reasons. First, a "needy" donor is more exposed to public criticism of its aid allocation because of domestic deficiencies. In order to defend its aid allocation vis-à-vis its electorate, the country might be more inclined to follow political and commercial interests to a larger extent. In this regard, Price (2004) notes that the Indian government had to emphasize the benefits that accrue to India in order to gain domestic support for its foreign aid policy, especially the aid reforms after the 2003-04 Finance Minister's budget speech. Note that this need to defend aid expenditure is even larger in democracies like India, where the government faces elections, than in autocratic donor countries. A second explanation is evident if one assumes a declining marginal utility of wealth, i.e., a "needy" donor like India values an additional dollar of wealth more than richer countries. The "needy" donor, lagging behind the "rich" donor in terms of wealth, consequently has more incentives to provide strategic aid than the "rich" donor does. We formulate the following hypothesis:

Hypothesis 2b: *While the elasticity to recipient needs is lower for a "needy" donor like India compared to "rich" donors, the opposite is true for political and commercial factors in regards to their respective aid allocations.*

6.4 Empirical Analysis

6.4.1 Overview

In this section, we employ data on aid commitments by the MEA in constant 2000 US dollars, obtained from the project-level database AidData (Tierney et al. 2011).[94] Data are available for the 2008-2010 period.[95] In what follows, we only analyze aid projects traceable to countries, thus excluding aid provided to world regions if we lack information on the country breakdown.[96] To follow the OECD's definition of ODA, we further exclude projects related to military assistance, as well as aid provided to countries that are not on the DAC list of aid recipients.[97] Our aim is to estimate the motives behind India's aid allocation decisions. Beyond that, we compare India's aid allocation to that of other donor countries in order to investigate whether aid from the "needy" donor India is allocated based on different grounds.

The lion's share (89.7%) of India's aid administered by the MEA was allocated to South Asian countries (see figure 3.2). With the exception of Pakistan, all six South Asian countries were beneficiaries of Indian aid in this period of time. Southeast Asian countries received 5.5% of MEA aid during this period. This corresponds to a total of 18 countries which have obtained

[94] While the first entry in the aid database is "Welfare Activities for the Muktijoddhas (Freedom Fighters)" in Bangladesh in 2008, the database ends with an IT center in Osh in the Kyrgyz Republic in 2010.
[95] Note that our study period coincides with the Global Financial Crisis. This might have an effect on the aid allocation decisions made by India. Nevertheless, the drop in India's aid budget during this period is not substantial compared to previous years as we will see later in figure 3.1.
[96] About 5% of the total aid amount is not traceable to recipient countries.
[97] The DAC List of ODA Recipients is available at: http://www.oecd.org/dataoecd/23/34/37954893.pdf, as of January 1, 2006 (accessed February 14, 2011).

development assistance in this region. 2.2% of the Ministry's total aid amount has been received by 38 Sub-Saharan African countries, and 1.6% was directed to eight transition economies in Eastern Europe and Central Asia. In the Middle East and North Africa, only Palestine and Syria benefited from Indian aid (1.2% of India's total aid amount in the 2008-2010 period). Indian support in this region was significantly concentrated on providing various types of humanitarian assistance to Palestine. Finally, less than 0.1% of total aid allocations by the MEA were made available to 10 Latin American countries. Taken together, it is evident that India strongly favors countries in its neighborhood, as has been argued previously (e.g., Price 2005, Katti et al. 2009, Meier and Murphy 2011).

Figure 3.3 puts the spotlight on sectoral aid allocations. As can be seen, 23.1% of the aid committed was targeted to the energy sector (DAC code: 230), covering both the production and distribution of energy in recipient countries. The second most important sector was drinking water provision and sanitation facilities (DAC code: 140), making up 15.0% of the Ministry's total aid amount. 12.8% of MEA aid was allocated to transport and storage facilities in recipient countries (DAC code: 210), closely followed by 11.8% earmarked for commodity aid and general program assistance (DAC code: 500), which includes contributions for general development purposes in recipient countries. We also find that about 9.5% of total aid was allocated towards the development of activities associated with strengthening the administrative apparatus and government planning, activities promoting good governance, strengthening civil society, and other social infrastructure projects in the recipient countries, respectively (DAC codes: 150 and 160). 8.7% of the Ministry's aid was allocated to multi-sector activities (DAC code: 400), and 7.5% to the development of health-related activities such as building hospitals and health centers, and the provision of other health infrastructure (DAC code: 120). The MEA

also earmarked 5.6% for industrial development (DAC codes: 321-323). The remaining sectors are: education (3.2%, DAC code: 110), communications (1.4%, DAC code: 331-332), agriculture, forestry and fishing (0.5%, DAC code: 311-313), humanitarian purposes (0.9%, DAC code: 700) and unspecified (0.5%, DAC code: 998). Finally, less than 0.1% is targeted at banking and financial services (DAC code: 240).

These numbers serve as a first indication that India's foreign aid is motivated to a higher extent by commercial interests in comparison to need-based issues plaguing recipient countries. This is reflected in the fact that about 45% of the Ministry's aid has been directed at commercial sectors. Nevertheless, the development aid provided by the MEA also covers sectors concerned with the overall development of basic public goods (such as health, drinking water, education and agriculture), which made up about 24% of total aid allocations.

With respect to the role of developmental distance between India and recipient countries for aid allocation, figure 3.4 provides first descriptive evidence in favor of hypothesis 1b. The graph on the left shows the expected strong negative link between the (logged absolute) developmental distance and the probability of receiving aid from India. The graph on the right, however, shows only a weak negative correlation between developmental distance and (logged) aid commitments from India. We now turn to the econometric analysis.

6.4.2 Data and Method

We follow a common practice in the aid allocation literature and estimate India's aid allocation in two steps (e.g., Neumayer 2002). First, we estimate which countries enter India's aid program. Our dependent variable is a dummy that takes a value of 1 if India provided aid to a developing country on the DAC list of aid recipients. Second, given that a country receives aid from India,

we estimate the (logged) amount of aid in constant 2000 US dollars that has been committed to a particular recipient country. One way to estimate the first step (the so-called gate-keeping stage) is through a Probit (or Logit) model, which takes the binary nature of the data into account. In the second step, it may be preferable to include the inverse Mills ratio derived from the first step to avoid selection bias. Since we lack a suitable exclusion variable, we run a Heckman model without an exclusion variable, i.e., we identify the model based on the non-linearity inherent in the selection equation. The resulting Wald test does not reject the null hypothesis of independent equations (p-value: 0.650).[98] Therefore, we opt for an Ordinary Least Squares (OLS) estimation of the aid amount allocated to a recipient country.

For our econometric analysis, we sum bilateral aid allocation over the 2008-2010 period since it is difficult to explain aid allocation on a yearly basis due to its volatility (see also Gupta et al. 2006, Dreher et al. 2011). Concerning the selection of our explanatory variables, we follow the previous literature on aid allocation, in particular that on emerging donors (e.g., Dreher et al. 2011, Dreher and Fuchs 2011). To control for the effect of geographic proximity, we account for the (logged) distance between the recipient and donor country.[99] Distance can be seen as a proxy for costs associated with the provision of development aid. Aid costs are expected to be a particular concern for a "needy" donor with limited resources like India. Apart from this explanation, India might favor countries in its neighborhood (with the exception of Pakistan due to the bilateral conflict over Kashmir) as it aspires to become a regional power. Dreher et al. (2011) find that, in general, so-called new donors are more likely to provide aid to countries that are closer to them geographically. Given that India is even poorer in terms of income per capita

[98] Results available upon request.
[99] As defined in Mayer and Zignago (2006), bilateral distances are computed as the average of the distance between the major cities of the two countries, which are weighted by the share of the city in the overall population.

than any of the donors covered in Dreher et al. (2011), we expect to find a pronounced effect of distance on aid allocation for the "needy" donor under investigation.

We use several variables to examine whether India's aid responds to the needs of other developing countries (hypothesis 1a). To reflect humanitarian motives, the need orientation of donors is proxied by the recipient country's (logged) GDP per capita (measured in 2005 international dollars). A need-oriented donor should provide more aid to poorer countries. Thus, we expect a negative sign for this income measure. Next, we control for the (log) population of recipient countries. The intuition here is that larger countries need more resources to obtain visible effects of aid provision. In addition, we control for the (log) total number of people affected by natural disasters as an additional indicator of recipient need since disaster relief is part of the aid program of the MEA. Furthermore, we include developmental distance, which is measured as the (log) absolute difference between the income per capita of India and that of a particular recipient country. Hypothesis 1b implies that India's aid decreases with the developmental distance to a recipient country.

To proxy donors' political self-interests, we follow the literature and employ a recipient country's voting alignment with India in the United Nations General Assembly (UNGA). The UNGA voting alignment seems to be of large relevance for India since "marshalling support for Indian positions in forums such as the UN take up much of India's diplomatic effort" (Dutt 1980: 678). Relying on data from Voeten and Merdzanovic (2009), we calculate the number of times a country votes in line with India (either both voting yes, both voting no, both voting abstentions, or both being absent). We then divide the resulting value by the total number of votes in a particular year to derive a measure of voting coincidence between zero and one. We follow Dreher et al. (2011) and compute the voting alignment based on key votes as defined by the U.S.

State Department (Kilby 2009b).[100] Various empirical studies find that developing countries are favored in donors' aid allocation decisions when they have closer political ties (Thacker 1999, Alesina and Dollar 2000, Barro and Lee 2005, Dreher et al. 2009, Kilby 2009a). We also include a dummy variable that takes a value of 1 if a recipient country is a non-suspended member of the Commonwealth of Nations. It can be argued that India uses the Commonwealth as a forum to develop political and commercial ties. For example, over the years India has developed strong ties with Commonwealth countries in South and Southeast Asia, as well as Africa (Johnson and Kumar 2011). Beyond that, referring to colonization, Banerjee (1982: 54) views India's aid "as a part of the process to undo the injustice of ages."

To account for commercial interests, we include India's (log) total exports to a particular recipient country in constant US$. In addition, we follow Dreher et al. (2011) and use the recipient country's (log) depletion of mineral and energy resources as a proxy for a recipient's endowment of natural resources.

Finally, to account for merit as a motive for aid supply, institutional quality in the recipient countries is proxied by both the political rights measure from Freedom House (2009) and the corruption index from Kaufmann et al. (2009). The political rights variable is coded on a scale of 1-7, with higher values representing worse liberties, and lower values reflecting full liberties. As the world's largest democracy, India might reward democratic countries and provide less aid to autocratic countries in comparison. Note that India is the second largest donor in the UN Democracy Fund (US$ 25 million as of 5 January 2012), which underlines the importance that India attributes to the support of democratization.[101] Alternatively, India might follow the 'spirit of Bandung' (Lafargue 2006) and follow the principle of non-interference in internal

[100] Note that we also report the results with all votes as a robustness check.
[101] See UNDEF webpage: http://www.un.org/democracyfund/Donors/donors_index.html (accessed 11 February 2012).

affairs, i.e., its aid allocation might be independent of the institutional characteristics of the recipient country. If this is the case, we would expect India to be unresponsive to corruption in the recipient countries. The control-of-corruption index ranges from -2.5 to 2.5, with higher values corresponding to better governance.

For our time-varying explanatory variables, we take lagged values, i.e., the corresponding value in 2007, to mitigate endogeneity issues. The only exception is the disaster variable since it is reasonable to assume that the occurrence of natural catastrophes is exogenous. Since our export variable and UNGA voting alignment both show relatively high volatility over time, we follow Dreher et al. (2011) and take the average of the respective values in the three years preceding our period of investigation (2005-2007). All definitions and sources of variables are provided in the table 5.3 in appendix.

6.4.3 Main Results

Table 5.1 displays our results. While columns 1-3 show the results for the gate-keeping stage, columns 4-6 present the results of the allocation decision. Analyzing the coefficient on GDP per capita in column 1, Indian aid shows some need orientation. The probability that a developing country receives aid from India decreases with a country's stage of development. The coefficient is statistically significant, at the ten-percent level. In turn, both the number of people affected by natural disasters and country size have no significant impact on the probability that a developing country enters India's aid program, at conventional levels of significance.[102] To test whether India favors countries at a similar developmental stage (hypothesis 1b), we add the developmental distance to India to our regression in column 2. The corresponding coefficient

[102] Note that the coefficient on disasters becomes statistically significant in column 2, at the ten-percent level. The significant negative sign is strong evidence against the hypothesis that disaster-stricken countries are more likely to enter India's aid program.

shows the expected negative sign and is statistically significant, at the ten-percent level. Note that the coefficient on per-capita GDP loses its statistical significance. Considering that the developmental distance between India and developing countries is correlated with the recipient's income per capita, we drop this latter variable as a next step. As shown in column 3, developmental distance then reaches statistical significance at the five-percent level. This suggests that countries closer to India in terms of economic development are favored by the MEA, in line with hypothesis 1b. The corresponding marginal effect of a ten-percent decrease in developmental distances increases to 0.01 percentage points.

According to all three specifications (columns 1-3), countries which are closer to India geographically are favored. The probability that a country receives aid from India decreases with distance, at the one-percent level of significance. Holding all other explanatory variables constant at their mean and computing the marginal effects, a ten-percent decrease in bilateral distance leads to an increase in the probability to receive Indian aid by roughly 0.03 percentage points. The political and commercial variables do not have a significant effect on Indian aid in the gate-keeping stage. The coefficient on the UNGA voting alignment on key votes, the Commonwealth dummy, and the variable capturing the extraction of natural resources are all not statistically significant at conventional levels. Note that the Indian exports variable gains statistical significance in column 3, at the five-percent level, but the suggested negative effect is not robust (see columns 1 and 2). The indicators of recipient merit, political rights and control of corruption are not statistically significant at conventional levels in all three specifications. This finding would support the idea that India's aid of today still follows the 'spirit of Bandung', with the principle of non-interference in internal affairs.

Focusing on the sample to India's recipient countries, we analyze the subsequent allocation decision. As can be seen from column 4, we do not find a significant link between a recipient country's stage of development and the amount of aid received. This also holds true if we use the developmental distance between India and the recipient instead of the recipient country's GDP per capita (column 6), or if we include both variables at the same time (column 5). While this finding questions India's commitment towards recipient need at the allocation stage, we obtain a nuanced picture if we consider the effect of the number of people affected by disasters. While we did not find that disaster-affected countries are more likely to enter India's aid program, countries suffering from more severe natural disasters receive larger aid amounts if they are already among India's aid recipients. If the number of people affected increases by one percent, India's aid commitments increase by about 0.1 percent. Our results also show that larger countries are disfavored as the coefficient on population is negative and statistically significant, at the one-percent level. While this result seems surprising at first, it is in line with empirical evidence for China (Dreher and Fuchs 2011) and six other so-called new donors (Dreher et al. 2011). As was the case in the gate-keeping stage, geographic proximity is also an important determinant of aid amounts. A one-percent increase in the distance from India to a particular recipient country decreases India's aid commitments by about 1.6 percent, on average.

Political and commercial motives are also important for India's aid allocation decisions. Recipients with both a closer voting alignment with India in the UNGA and stronger commercial ties (proxied by Indian exports to recipient countries) do in fact receive larger aid flows from the "needy" donor, with both coefficients being significant at the one-percent level. If the voting alignment on key votes increases by ten percentage points, India increases its aid commitments by roughly 0.7%, on average. Accordingly, if Indian exports grow by one percent, aid increases

by 0.4%. These results support hypothesis 2a. In contrast to our expectations, however, India disfavors countries that are members of the Commonwealth. The coefficient on the Commonwealth dummy shows a surprising negative sign and is statistically significant, at the one-percent level. Our results indicate that India donates strategically in order to strengthen ties with developing countries with which it does not already share common ties with through being members of the Commonwealth. In these cases, the marginal benefit of aid giving may be higher compared to aid allocated to Commonwealth members. Moreover, recipient countries' extraction of natural resources does not have the expected positive impact on the size of India's aid flows. While we do not find a statistically significant effect of political rights on aid amounts provided by India, aid flows are significantly larger to countries with a relatively low level of corruption, at the one-percent level of significance, and in contrast to our findings at the gate-keeping stage.

Overall, the empirical results lend some support in favor of our "needy" donor hypotheses. In line with hypothesis 1b, countries at a similar developmental stage are more likely to enter India's aid program (but do not receive larger aid amounts). Moreover, political and commercial interests have an impact on the size of India's aid flows, which is empirical evidence in favor of hypothesis 2a. As a next step, we will compare the role that political and commercial motives play in India's aid allocation decisions with aid flows from richer donors. By doing this, we test whether aid allocation from the "needy" donor India is driven to a higher extent by political and commercial motives than is the case for richer donor countries (hypothesis 2b).

6.4.4 Comparison with DAC and Other Non-DAC Donors

Finally, we compare India's aid allocation with other donors to evaluate whether aid from the "needy" donor under investigation is special.[103] Dutt (1980, p. 676) expects India's aid allocation to be closer to that of the big powers than to Scandinavian aid since "Indian elites perceive India as having a role on the world stage," an assessment that became even more evident after the 2003 budget speech. The pattern of India's aid allocation is compared to the largest donors of the DAC, i.e., the United States, Japan and the three largest EU countries (Germany, France and the United Kingdom). We use the so-called 'like-minded donors' or 'good donors' (Canada, Denmark, Netherlands, Norway and Sweden) as a further benchmark. This latter group is said to provide development aid predominantly based on humanitarian motives.[104] Beyond that, we compare India's aid allocation with two emerging donors for which data are easily accessible. The first donor is South Korea, another large emerging Asian donor, which became a DAC member in 2010. The second one is the United Arab Emirates, which has provided sizable aid amounts since the oil crises of the 1970s. Data on ODA from these donors again cover the 2008-2010 period, and are obtained from the OECD (2012). Unfortunately, we cannot compare India with China, the largest non-DAC donor, since we lack sufficient data on China's foreign aid after 2005 (see Dreher and Fuchs 2011 for a discussion). We use a similar set of explanatory variables as in our baseline model in column 1 of table 5.1. Note that we replace the Commonwealth dummy, which is an India-specific variable, with a general dummy variable for common colonial history between donor and recipient. More precisely, the variable takes a value of one if donor and recipient had a common colonizer (e.g., the British Crown in the case of India) or if the recipient was a colony of the donor country after 1945 as defined in Mayer and Zignago (2006).

[103] We obtained data on aid allocation from the countries under comparison from the OECD (2012).

[104] Note that doubts have been raised as to whether the positive image of these donor countries is warranted (see, for example, Neumayer (2003b) with respect to human rights, or Strømmen et al. (2011) with respect to peace and human security). Similarly, in their ranking of aid agency practices, Easterly and Williamson (2011) find that Scandinavian donors perform surprisingly badly.

Moreover, we now employ the recipient's UNGA voting alignment on key votes with the respective donor (not necessarily India) and, analogously, we take the exports of the respective donor to a recipient economy.

In order to be able to compare the effects between donors, we run nested regressions rather than individual regressions for each donor (see also Berthélemy 2006, Dreher et al. 2011, Dreher and Fuchs 2011). This is done by interacting dummies for each donor country or donor group with each of our explanatory variables. In addition to the coefficients and the corresponding p-value of all explanatory variables for all donors (in parentheses), we compute the p-values of a Wald test for differences in the effect of a variable for a particular country and India (in italics).

Table 5.2 displays our results. Analyzing the role of recipient needs as measured by GDP per capita, we find that Indian aid shows the smallest need orientation than of all donors under investigation. The coefficient on GDP per capita for India is the smallest in absolute terms and significantly different from the EU-3 and the "good" donors, at least at the five-percent level of significance (see p-values of the Wald test in italics). Moreover, India is the only donor for which population size has a negative effect on aid commitments that is statistically significant at conventional levels, which questions India's actual concern for recipient needs. Only with respect to disaster response does India show some need orientation. Apart from Japan, India is the only donor with a statistically significant and positive coefficient on the number of people affected by disasters.

The effect of geographic distance between the donor and recipient is the largest for India compared to all other donors included in the analysis. This can be interpreted as evidence that aid costs matter more for a "needy" donor than for "rich" donors. The p-values of the Wald test in

italics show that the distance coefficient for India is significantly different, at least at the five-percent level, from the US, the EU-3 and the "good" donors. Analyzing the impact of the UNGA voting alignment on aid allocation, the coefficient for India is found to be the largest among the donors under investigation. While Indian aid is significantly more motivated by politics than aid from all traditional DAC donors, the difference between the coefficients is not statistically significant with respect to South Korea and the United Arab Emirates. While countries that share a common colonial legacy do not receive higher aid amounts from India and are even receiving less aid on average, the EU-3 and the "good" donors provide significantly more aid to countries which have had a colonial relationship with the respective donor country.

The effect of bilateral exports on aid amounts is larger for India than for any of the other donors under investigation. According to the p-values of the Wald test in italics, Indian aid has a significantly closer link to commercial relationships than aid from the "good" donors and Japan. With regard to its relationship with natural resource endowments, we find that neither of the donors rewards countries extracting natural resources through increased aid flows. Likewise, we do not find evidence that any of the donors under investigation reward countries with greater political rights. Finally, we find that, alongside India, Japan is the only other donor that provides significantly larger amounts of aid to recipients that score better on the control-of-corruption index, at conventional levels of significance.

6.4.5 Robustness checks

Next, we examine the robustness of our findings. To begin with, we analyze nine additional variables that might influence India's aid commitments in addition to those included in tables 5.1 and 5.2, respectively. First, Indian aid allocation decisions are said to be related to the prevalence

of Indian diaspora communities (e.g., Dutt 1980, Banerjee 1982, Lafargue 2006).[105] The (log) Indian migrant stock in recipient countries is obtained from two sources, namely the Global Migrant Origin Database (Parsons et al. 2007) and the MEA (2001). Second, in order to examine whether India targets traditional recipients of aid from China, we include a variable capturing the number of completed Chinese aid projects in recipient countries as a share of China's total aid over the 1996-2005 period (see definition in Dreher and Fuchs 2011). A positive sign could suggest aid competition between the two emerging Asian powers, as suggested by some scholars (see Cheru and Obi 2011, for instance). Third, we add a recipient country's (logged) infant mortality rate (children under the age of 5) as an alternative measure of India's need orientation. Fourth, we add a dummy for countries which share a border with India to test whether India favors its direct neighbors in addition to the role played by geographic distance. Fifth, we replace the UNGA voting alignment index covering key votes with an index that covers all votes. Sixth, to allow for an alternative definition of what constitutes a key vote from the Indian perspective, we consider only those votes which show opposite voting behavior to the United States on the one hand, and to the four BRIC countries on the other. More precisely, we construct a voting alignment index based on those votes where Brazil, Russia, India and China vote 'yes' and the United States votes 'no' (or vice versa).[106] This measure should reflect the one-dimensional voting pattern that continues to exist in the General Assembly after the end of the Cold War, with the United States and its Western allies on one pole and a "counterhegemonic voting bloc," most notably the rising powers, on the other (see Voeten 2000). Seventh, we replace the Commonwealth dummy with a dummy that takes a value of 1 if India and a recipient country

[105] Lafargue (2006) identifies Indian diaspora as intermediaries for Indian investments in their respective host country.

[106] We also considered the construction of a voting alignment index based on the instances in which India and Pakistan voted differently. There are, however, only very few instances in which India and Pakistan showed opposite voting behavior during our period of analysis.

share a common language (i.e., English). Eighth, the Commonwealth dummy is substituted by a dummy variable that takes a value of 1 if the recipient country and India had a common colonizer after 1945 (i.e., the British Crown). Ninth, we replace the political rights measure with a dummy capturing whether a recipient country qualifies as a democracy as defined in Cheibub et al. (2010).

Detailed tables containing the regression results are reported in tables 5.6 and 5.7 in appendix. In the gate-keeping stage (see table 5.6 in appendix), we do not find any statistical significance for the variables listed above, at conventional levels of significance. For example, neither Indian diaspora communities nor aid projects lead to a significant increase (or decrease) in the probability that a developing country enters India's aid program. The outlined changes in the definition of the various explanatory variables do not change our main conclusions. In the allocation stage, we confirm the large positive significant effect of a country's UNGA voting alignment when we use the two alternative definitions instead. Note that the common colony dummy takes a negative sign, at the one-percent level, in line with our results for the Commonwealth dummy. Apart from these variables, all other variables introduced do not reach statistical significance at conventional levels.

Finally, we run a sub-sample analysis by restricting our sample to those countries that receive aid from India. Aware that this approach has its limitations, we intend to control for differences between the sample of India's aid recipients and that of other donors.[107] As before, we run nested regressions by interacting dummies for each donor country with each of our explanatory variables. By construction, the results for India are exactly the same as those reported in table 5.2. These results are reported in table 5.7 in appendix. With respect to per-

[107] Our sample includes 51 countries that receive aid from India in the 2008-2010 period.

capita GDP, the respective coefficients for the United States, Japan and South Korea lose their statistical significance. When restricting the sample to Indian aid recipients only, Indian aid does not appear to be inferior with respect to need orientation compared to all other donors under investigation (see p-values of the Wald test in italics). Concerning the UNGA voting alignment, however, our results confirm the high importance of political interests in India's aid allocation. The respective coefficient for India is still larger than for any traditional DAC donor, the difference being statistically significant, at least at the five-percent level (except for the United States). Note that the coefficient on UNGA voting alignment is now larger for South Korea than for India, but the difference is not statistically significant at conventional levels (as indicated by the p-value in italics). Although the EU-3, South Korea and UAE retain the expected sign and level of significance on bilateral exports, "good" donors and Japan are now positive and significantly different from zero, at the one-percent level. Finally, we also find some changes with respect to the corruption variable. We now find that the coefficients for the EU-3 and the "good" donors (along with India and Japan) become positive and statistically significant, at least at the five-percent level of significance. With respect to population size, mineral and energy depletion, and political rights, our results largely mimic those in table 5.2. Taken together, while commercial interests do not seem to play a significantly larger role for India than for most "rich" donors, according to this robustness check, the sub-sample analysis largely confirms the outstanding importance of political interests compared to most traditional DAC donors.

6.5 Conclusion

Despite having a large amount of its population suffering from underdevelopment, chronic poverty and mal-governance, India has jumped on the bandwagon in the 'business' of

development aid. This is puzzling. According to a recent World Bank report on India, about 37% of the Indian population lives on less than US$ 1.25 a day (World Bank 2011). Although India has a large number of anti-poverty schemes and programs to tackle these problems, the progress made in poverty reduction is rather small. Against this background, it is ironic that India provides development aid to other developing countries. Many of India's aid recipients even have a larger income per capita than India.[108]

With the intension of understanding why poor countries such as India provide foreign aid, this paper has empirically analyzed India's aid allocation decisions. We utilized data on aid commitments by the Ministry of External Affairs to 127 developing countries in US dollars, obtained from the AidData database for the 2008-2010 period. To examine whether India is different, we also compared India's aid allocation decisions with those of other donors. Our empirical results show that India's aid allocation is partially in line with our expectations of the behavior of a "needy" donor. Commercial and political self-interests dominate India's aid allocation. We find the importance of political interests, proxied by UNGA voting alignment, to be significantly larger for India than for all traditional DAC donors under investigation. Moreover, India favors countries which are geographically closer, and countries at a similar developmental stage are more likely to enter India's aid program.

From our results, it appears that the "needy" donor India predominantly cares about its own needs rather than the needs of others. Given India's domestic problems, this is understandable. Although India's own interests dominate its aid allocation, it may nevertheless be the case that India's assistance is effective in terms of poverty reduction and other

[108] 23 recipients of Indian aid had a larger income per capita than India (based on 2007 values of GDP per capita in international dollars and purchasing power parity): Armenia, Belarus, Bhutan, Botswana, Cape Verde, Cuba, Ecuador, El Salvador, Fiji, Grenada, Indonesia, Jamaica, Maldives, Marshall Islands, Mauritius, Namibia, Samoa, Sao Tome and Principe, Seychelles, Sri Lanka, Tonga and Turkmenistan.

developmental goals with respect to recipient countries.[109] This merits further investigation. Concerning political self-interest, Agrawal (2007) raises doubts over the long-term political gains resulting from India's engagement. Future research may also evaluate whether Indian aid, officially aimed at the promotion of India's welfare in addition to that of aid recipients, actually supports India's own development.

While we find that India's allocation is partially in line with our expectations of a "needy" donor, India itself does not want to be perceived as such. This is made clear by the comments of India's Minister of Finance, Pranab Mukherjee, who characterized British aid to India as a "peanut" compared to India's own development expenditures.[110] Moreover, India made its ambitions clear by announcing to setup a foreign aid agency, which is said to manage the distribution of aid flows amounting to 11 billion US dollars over the next five to seven years.[111] If India aspires to be recognized as one of the big aid donors, it would be beneficial from India's point of view to, first, establish clearly outlined aid legislation, and second, increase its aid transparency. Clearly identified goals and the provision of detailed and transparent aid records will not only alleviate India's credibility as an emerging aid donor, but will also enhance the scope for coordination with other aid donors.

[109] If this is the case, India's aid would differ from DAC aid. Analyzing the effect of aid on growth, empirical evidence in Kilby and Dreher (2010) suggests that donor motives matter for aid effectiveness.
[110] "India tells Britain: We don't want your aid," *The Telegraph*, 4 February 2012, available at: http://www.telegraph.co.uk/news/worldnews/asia/india/9061844/India-tells-Britain-We-dont-want-your-aid.html (last accessed: May 28, 2012).
[111] "Aid 2.0," *The Economist*, 13 August 2011, available at: http://www.economist.com/node/21525899 (last accessed: May 28, 2012).

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Table 5.1: Allocation of India's aid commitments (2008-2010)

	SELECTION Probit			ALLOCATION OLS		
	(1)	(2)	(3)	(4)	(5)	(6)
(log) GDP per capita	-0.315*	-0.244		-0.241	-0.243	
	(0.060)	(0.182)		(0.226)	(0.209)	
(log) Developmental distance		-0.228*	-0.268**		0.012	-0.015
		(0.097)	(0.039)		(0.936)	(0.924)
(log) Affected from disasters	-0.060	-0.079*	-0.063	0.111**	0.112*	0.126**
	(0.137)	(0.059)	(0.126)	(0.037)	(0.055)	(0.015)
(log) Population	0.028	0.060	0.113	-0.526***	-0.530***	-0.451**
	(0.852)	(0.697)	(0.438)	(0.002)	(0.004)	(0.012)
(log) Distance	-0.847***	-0.798***	-0.802***	-1.668***	-1.670***	-1.695***
	(0.001)	(0.004)	(0.003)	(0.000)	(0.000)	(0.000)
UN voting (key votes)	0.364	0.526	0.689	6.918***	6.911***	6.631***
	(0.747)	(0.647)	(0.542)	(0.000)	(0.000)	(0.000)
Commonwealth	0.434	0.464	0.503	-1.203***	-1.209***	-1.182***
	(0.146)	(0.132)	(0.101)	(0.001)	(0.001)	(0.001)
(log) Indian exports	-0.152	-0.157	-0.197**	0.398***	0.400***	0.359***
	(0.123)	(0.117)	(0.036)	(0.001)	(0.002)	(0.003)
(log) Resource depletion	0.002	-0.002	-0.011	-0.019	-0.019	-0.024
	(0.924)	(0.895)	(0.488)	(0.339)	(0.355)	(0.217)
Political rights	-0.140	-0.144	-0.145	0.037	0.038	0.039
	(0.173)	(0.165)	(0.157)	(0.798)	(0.800)	(0.801)
Control of corruption	-0.228	-0.177	-0.289	1.474***	1.469***	1.459***
	(0.421)	(0.557)	(0.307)	(0.000)	(0.000)	(0.000)
Constant	12.592***	13.075***	11.043***	26.284***	26.253***	24.308***
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Number of observations	125	125	125	51	51	51
Prob>Chi2 / Prob>F	0.002	0.000	0.000	0.000	0.000	0.000
(Pseudo) R-Squared	0.17	0.19	0.18	0.83	0.82	0.82

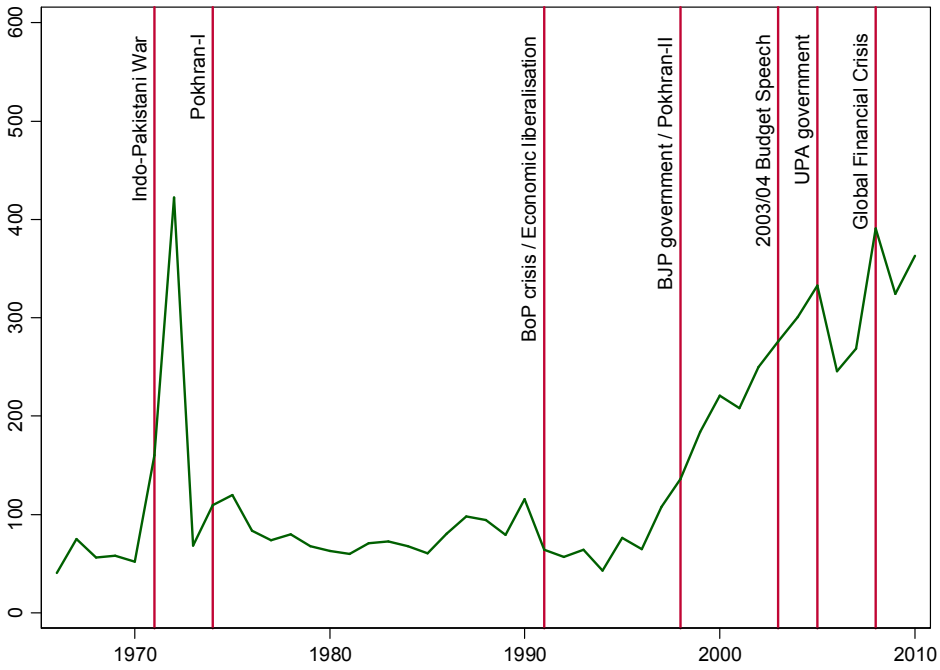
Notes: * (**, ***) indicates significance at the ten (five, one) percent level

Table 5.2: Comparison of India's aid allocation with other donors (2008-2010)

	India	USA	EU-3	Good donors	Japan	Korea	UAE
(log) GDP per capita	**-0.249**	-0.646**	-0.798***	-1.007***	-0.586***	-0.562*	-0.926***
	(0.165)	(0.021)	(0.000)	(0.000)	(0.000)	(0.067)	(0.007)
		0.211	*0.016*	*0.002*	*0.163*	*0.344*	*0.092*
(log) Affected from disasters	**0.097****	0.039	-0.044	0.054	0.103***	0.045	-0.076
	(0.039)	(0.500)	(0.358)	(0.231)	(0.007)	(0.524)	(0.392)
		0.384	*0.013*	*0.441*	*0.925*	*0.475*	*0.101*
(log) Population	**-0.483*****	0.699***	0.679***	0.462***	0.371***	0.524***	-0.012
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.004)	(0.960)
		0.000	*0.000*	*0.000*	*0.000*	*0.000*	*0.108*
(log) Distance	**-1.634*****	0.171	-0.386*	-0.722**	-1.483***	-0.779	-0.934
	(0.000)	(0.740)	(0.081)	(0.021)	(0.000)	(0.115)	(0.168)
		0.002	*0.000*	*0.033*	*0.681*	*0.123*	*0.321*
UN voting (key votes)	**6.826*****	2.009*	1.873*	0.165	0.926	1.923	2.453
	(0.000)	(0.077)	(0.085)	(0.906)	(0.506)	(0.627)	(0.424)
		0.006	*0.005*	*0.001*	*0.002*	*0.236*	*0.229*
Common colonial history	**-1.219*****	1.221	1.622***	4.803***			0.860
	(0.000)	(0.465)	(0.000)	(0.000)			(0.191)
		0.153	*0.000*	*0.000*			*0.008*
(log) Bilateral exports	**0.401*****	0.088	0.367***	0.121	0.068	0.285**	0.187**
	(0.000)	(0.620)	(0.004)	(0.173)	(0.220)	(0.025)	(0.019)
		0.118	*0.835*	*0.025*	*0.007*	*0.444*	*0.102*
(log) Resource depletion	**-0.027**	0.012	0.020	-0.011	-0.013	-0.020	-0.012
	(0.115)	(0.563)	(0.199)	(0.591)	(0.296)	(0.473)	(0.723)
		0.133	*0.017*	*0.514*	*0.498*	*0.816*	*0.691*
Political rights	**0.056**	-0.126	0.068	0.004	-0.012	0.058	0.058
	(0.676)	(0.306)	(0.340)	(0.967)	(0.888)	(0.744)	(0.752)
		0.333	*0.939*	*0.780*	*0.677*	*0.992*	*0.993*
Control of corruption	**1.481*****	-0.572	0.202	0.433	0.467**	-0.273	-0.032
	(0.000)	(0.188)	(0.438)	(0.227)	(0.045)	(0.552)	(0.964)
		0.000	*0.001*	*0.004*	*0.003*	*0.000*	*0.051*
Donor country dummies	**Yes**	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations				1371			
Number of recipients				125			
- per donor group	51	124	125	124	125	118	87
R-Squared				0.58			

Notes:
- Estimation technique: Nested OLS model with standard errors clustered by recipient country
- Dependent variable: (log) Aid commitments to recipient country, sum 2008-2010
- We report coefficients of the explanatory variables (corresponding p-values in parentheses)
- In italics: p-values of a Wald test of equal marginal effects of the respective donor (group) compared to India
- * (**, ***) indicates significance at the ten (five, one) percent level

Figure 3.1: Aid provided by the MEA in millions of constant 2000 US$ (1966-2010)



Notes: BJP: Bharatiya Janata Party; UPA: United Progressive Alliance led by Indian National Congress.

Figure 3.2: India's aid allocation by region (MEA, 2008-2010)

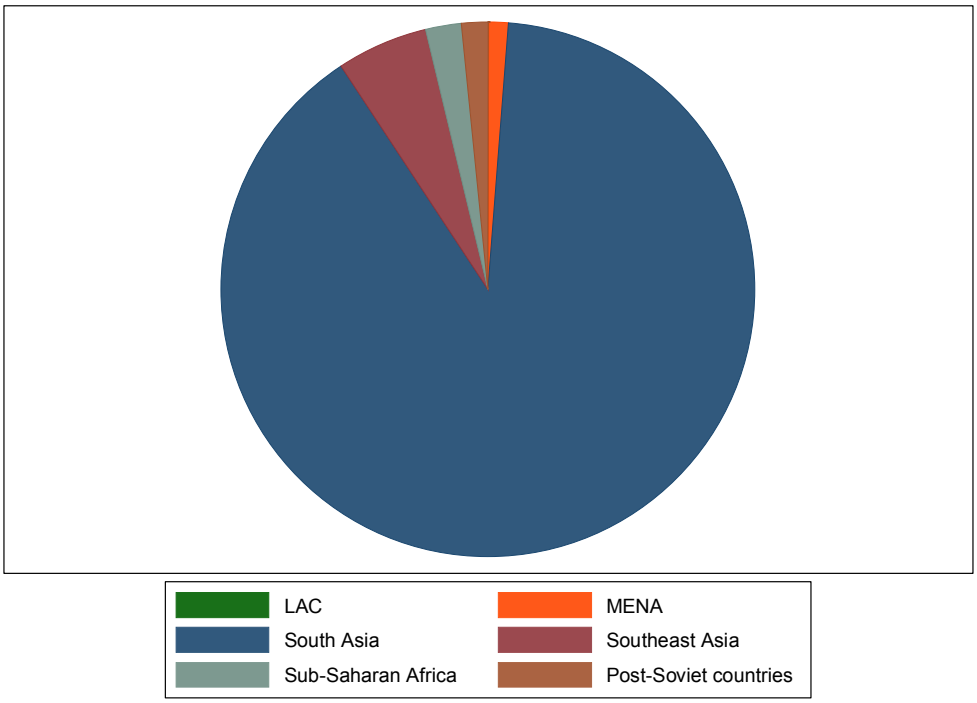


Figure 3.3: India's aid allocation by sector (MEA, 2008-2010)

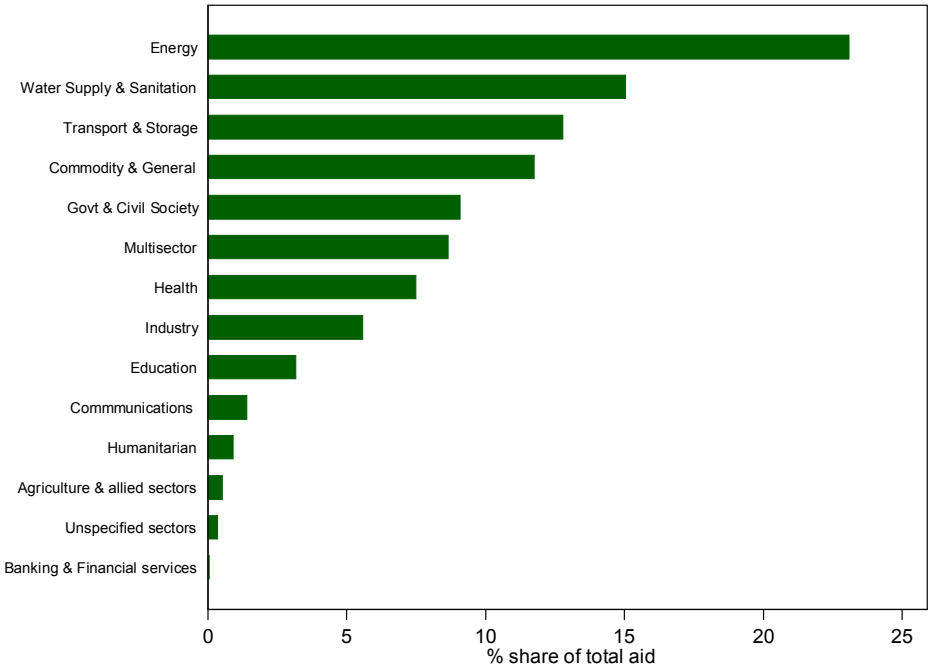
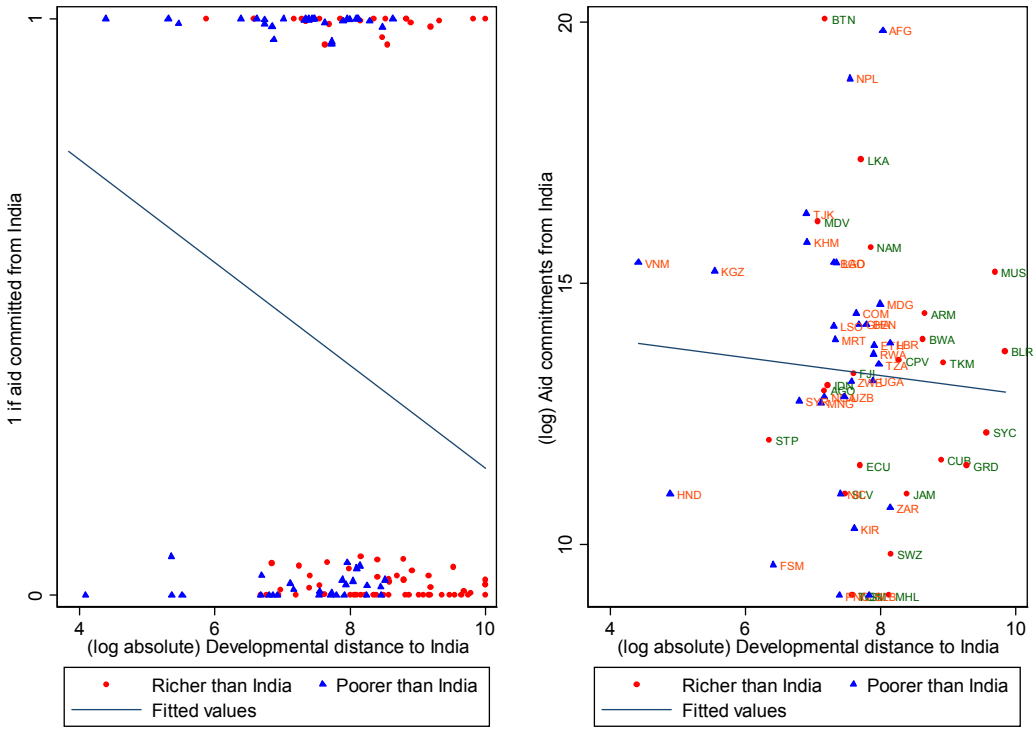


Figure 3.4: Aid allocation and developmental distance



6.7 Appendix

Table 5.3: Definitions and sources

Variable	Description	Source
Explained variables		
1 if aid commitment	1 if aid committed to recipient country, 2008-2010	AidData (Tierney et al. 2011)
(log) Aid commitment	(log) Aid commitments to recipient country (constant 2000 US$), sum, 2008-2010	AidData (Tierney et al. 2011)
Explanatory variables: Main results		
(log) GDP per capita	(log) GDP per capita (constant 2005 I$), lag	Penn World Tables (Heston et al. 2009)
(log) Developmental distance	(log) Absolute difference between the per-capita GDP of donor and recipient, lag	Own construction based on Penn World Tables
(log) Affected from disasters	(log) Number of people affected by disasters, average	EM-DAT (2010)
(log) Population	(log) Total population, lag	Penn World Tables (Heston et al. 2009)
(log) Distance	(log) Bilateral distance (weighted by populations of major cities)	CEPII (Mayer and Zignago 2006)
UN voting alignment (key votes)	UNGA voting alignment between donor and recipient (key votes), lag	Voeten and Merdzanovic (2009), Kilby (2009b)
Commonwealth	1 if recipient is a non-suspended member of the Commonwealth, lag	www.thecommonwealth.org, internet research
Common colonial history	1 if donor and recipient have had a colonial relationship or a common colonizer after 1945	CEPII (Mayer and Zignago 2006)
(log) Indian/Bilateral exports	(log) Total exports from donor to recipient country, lag	UN Comtrade via WITS (http://wits.worldbank.org)
(log) Resource depletion	(log) Product of unit resource rents and physical quantities of energy and minerals extracted, lag	World Bank (http://data.worldbank.org/indicator)
Political rights	Index of political rights rated on a seven-point scale (1: most free), lag	Freedom House (2009)
Control of corruption	Index ranging from -2.5 to 2.5 with higher values corresponding to better governance, lag	Kaufmann et al. (2009)
Explanatory variables: Robustness checks		
(log) Indian migrants (def. 1)	(log) Indian migrant stock in recipient country, 2000 round of population censuses	Global Migrant Origin Database (Parsons et al. 2007)
(log) Indian migrants (def. 2)	(log) Estimated size of Indian community in recipient country, 2001	MEA (2001b)
Chinese aid projects	Number of Chinese aid projects completed in recipient country (% of total), 1996-2005	Dreher and Fuchs (2011)
(log) Under-5 mortality Rate	(log) Mortality rate, under 5 years (per 1000), lag	World Bank

		(http://data.worldbank.org/indicator)
Neighbor	1 if donor and recipient share a border	CEPII (Mayer and Zignago 2006)
UN voting	UNGA voting alignment between donor and recipient, lag	Voeten and Merdzanovic (2009), Kilby (2009b)
UN voting (BRIC vs USA)	UNGA voting alignment between donor and recipient (disagreement between BRIC and USA), lag	Voeten and Merdzanovic (2009), Kilby (2009b)
Common language	1 if e if a language is spoken by at least 9% of the population in donor and recipient country	CEPII (Mayer and Zignago 2006)
Democracy	1 if the regime qualifies as democratic, lag	Cheibub et al. (2010)

Notes:
- Values in current US$ have been transformed to constant 2000 US$ using US Consumer Price Indices from the World Bank (http://data.worldbank.org/indicator)
- The value of 1 has been added to exports and natural resource variables as well as to the number of people affected by disasters before taking logarithms

Table 5.4: Descriptive statistics

	Observations	Mean	Stdandard Deviation	Minimum	Maximum
1 if aid commitment	125	0.41	0.49	0.00	1.00
(log) Aid commitment	51	13.28	2.45	9.02	20.07
(log) GDP per capita	125	8.37	0.97	5.95	10.16
Control of corruption	125	7.86	1.09	3.83	10.00
(log) Affected from disasters	125	9.21	4.34	0.00	18.71
(log) Population	125	15.62	2.02	10.59	21.00
(log) Distance	125	8.83	0.64	7.04	9.74
UN voting (key votes)	125	0.74	0.14	0.25	0.93
Commonwealth	125	0.30	0.46	0.00	1.00
Common colonial history	125	0.30	0.46	0.00	1.00
(log) Resource depletion	125	13.16	10.22	0.00	25.82
Political rights	125	3.94	1.95	1.00	7.00
Control of corruption	125	-0.47	0.59	-1.38	1.34
(log) Indian migrants (def. 1)	125	6.17	2.99	0.00	13.86
(log) Indian migrants (def. 2)	125	4.73	4.24	0.00	14.33
Chinese project aid	124	0.75	0.94	0.00	4.62
(log) Under-5 mortality Rate	125	3.84	0.90	1.76	5.57
Neighbor	125	0.04	0.20	0.00	1.00
UN Voting alignment	125	0.79	0.10	0.38	0.89
UN Voting alignment (BRIC vs USA)	125	0.92	0.12	0.37	1.00
Common language	125	0.31	0.47	0.00	1.00
Democracy	125	0.52	0.50	0.00	1.00

Notes: Descriptive statistics for sample as in table 5.1, column 1

Table 5.5: Further checks on Robustness – Probit estimations of Allocation of India's aid commitments (2008-2010)

	Baseline	(1a)	(1b)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(log) GDP per capita	-0.315*	-0.308*	-0.313*	-0.247	-0.469**	-0.315*	-0.316*	-0.321*	-0.331*	-0.320*	-0.319*
	(0.060)	(0.067)	(0.061)	(0.165)	(0.037)	(0.059)	(0.057)	(0.053)	(0.052)	(0.059)	(0.054)
(log) Affected from disasters	-0.060	-0.057	-0.060	-0.066	-0.058	-0.060	-0.062	-0.060	-0.058	-0.058	-0.053
	(0.137)	(0.170)	(0.139)	(0.102)	(0.145)	(0.134)	(0.128)	(0.139)	(0.151)	(0.152)	(0.177)
(log) Population	0.028	0.015	0.031	0.062	0.007	0.028	0.022	0.028	-0.027	-0.009	0.016
	(0.852)	(0.924)	(0.837)	(0.681)	(0.963)	(0.852)	(0.883)	(0.850)	(0.854)	(0.950)	(0.914)
(log) Distance	-0.847***	-0.806***	-0.847***	-0.783***	-0.825***	-0.841***	-0.873***	-0.850***	-0.852***	-0.852***	-0.797***
	(0.001)	(0.004)	(0.001)	(0.003)	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
UN voting (key votes)	0.364	0.375	0.337	0.157	0.610	0.365			0.602	0.530	0.380
	(0.747)	(0.738)	(0.767)	(0.889)	(0.590)	(0.746)			(0.590)	(0.641)	(0.727)
Commonwealth	0.434	0.408	0.424	0.432	0.494	0.432	0.436	0.448			0.549*
	(0.146)	(0.169)	(0.162)	(0.150)	(0.100)	(0.146)	(0.140)	(0.128)			(0.061)
(log) Indian exports	-0.152	-0.166	-0.157	-0.165*	-0.148	-0.152	-0.162	-0.144	-0.126	-0.136	-0.152
	(0.123)	(0.102)	(0.128)	(0.092)	(0.135)	(0.124)	(0.101)	(0.137)	(0.197)	(0.166)	(0.121)
(log) Resource depletion	0.002	0.002	0.001	0.001	0.003	0.002	0.002	0.002	0.003	0.003	0.003
	(0.924)	(0.908)	(0.948)	(0.940)	(0.885)	(0.923)	(0.896)	(0.920)	(0.877)	(0.864)	(0.852)
Political rights	-0.140	-0.136	-0.139	-0.133	-0.135	-0.140	-0.142	-0.131	-0.163	-0.162	
	(0.173)	(0.184)	(0.175)	(0.201)	(0.190)	(0.173)	(0.153)	(0.171)	(0.124)	(0.116)	
Control of corruption	-0.228	-0.237	-0.231	-0.211	-0.315	-0.230	-0.250	-0.212	-0.205	-0.219	-0.114
	(0.421)	(0.404)	(0.415)	(0.452)	(0.291)	(0.423)	(0.378)	(0.450)	(0.467)	(0.440)	(0.670)
(log) Indian migrants (def. 1)		0.026									
		(0.662)									
(log) Indian migrants (def. 2)			0.005								
			(0.883)								
Chinese project aid				0.190							
				(0.174)							
(log) Under-5 mortality Rate					-0.246						
					(0.291)						
Neighbor						0.035					
						(0.961)					
UN voting							0.978				
							(0.492)				
UN voting (BRIC vs USA)								0.073			
								(0.950)			
Common language									0.039		
									(0.894)		
Colonial relationship										0.148	
										(0.624)	
Democracy											0.442
											(0.133)
Constant	12.592***	12.416***	12.632***	11.210***	14.607***	12.546***	12.617***	12.704***	13.218***	13.031***	11.524***
	(0.000)	(0.000)	(0.000)	(0.002)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of observations	125	125	125	124	125	125	125	125	125	125	125
Prob>Chi2	0.002	0.003	0.003	0.003	0.001	0.003	0.002	0.002	0.002	0.002	0.002
Pseudo R-Squared	0.17	0.17	0.17	0.18	0.18	0.17	0.17	0.17	0.16	0.16	0.17

Notes: Dependent variable: Dummy that takes a value of one if aid was committed to a recipient country during the 2008-2010 period / * (**, ***) indicates significance at the ten (five, one) percent level

Appendix 5.6: Further checks on Robustness – OLS estimations of Allocation of India's aid commitments (2008-2010)

	baseline	(1a)	(1b)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(log) GDP per capita	-0.241	-0.235	-0.240	-0.214	-0.047	-0.230	-0.317	-0.286	-0.233	-0.249	-0.251
	(0.226)	(0.251)	(0.233)	(0.292)	(0.880)	(0.246)	(0.126)	(0.175)	(0.340)	(0.208)	(0.201)
(log) Affected from disasters	0.111**	0.112**	0.111**	0.109**	0.106**	0.108**	0.077	0.075	0.101*	0.097*	0.110**
	(0.037)	(0.041)	(0.040)	(0.042)	(0.034)	(0.043)	(0.162)	(0.173)	(0.060)	(0.063)	(0.036)
(log) Population	-0.526***	-0.533***	-0.526***	-0.531***	-0.532***	-0.522***	-0.502***	-0.465***	-0.363**	-0.483***	-0.542***
	(0.002)	(0.004)	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)	(0.008)	(0.037)	(0.004)	(0.003)
(log) Distance	-1.668***	-1.646***	-1.669***	-1.630***	-1.660***	-1.529***	-1.997***	-2.053***	-1.857***	-1.634***	-1.635***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
UN voting (key votes)	6.918***	6.968***	6.907***	6.933***	6.858***	7.215***			5.926***	6.826***	6.941***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)			(0.000)	(0.000)	(0.000)
Commonwealth	-1.203***	-1.214***	-1.210***	-1.210***	-1.236***	-1.199***	-1.050***	-1.000***			-1.292***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.003)	(0.006)			(0.000)
(log) Indian exports	0.398***	0.394***	0.395***	0.398***	0.413***	0.383***	0.365***	0.378***	0.292**	0.401***	0.412***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)	(0.015)	(0.001)	(0.002)
(log) Resource depletion	-0.019	-0.019	-0.019	-0.019	-0.019	-0.019	-0.010	-0.011	-0.014	-0.027	-0.020
	(0.339)	(0.325)	(0.316)	(0.350)	(0.349)	(0.347)	(0.625)	(0.622)	(0.534)	(0.153)	(0.303)
Political rights	0.037	0.038	0.037	0.039	0.030	0.035	0.072	0.096	0.052	0.056	
	(0.798)	(0.797)	(0.802)	(0.788)	(0.849)	(0.812)	(0.568)	(0.447)	(0.750)	(0.703)	
Control of corruption	1.474***	1.473***	1.470***	1.465***	1.426***	1.403***	1.508***	1.587***	1.505***	1.481***	1.461***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
(log) Indian migrants (def. 1)		0.011									
		(0.892)									
(log) Indian migrants (def. 2)			0.004								
			(0.920)								
Chinese project aid				0.080							
				(0.551)							
(log) Under-5 mortality Rate					0.341						
					(0.267)						
Neighbor						0.682					
						(0.268)					
UN voting							8.478***				
							(0.000)				
UN voting (BRIC vs USA)								5.854***			
								(0.000)			
Common language									-0.239		
									(0.486)		
Common colonial history										-1.219***	
										(0.001)	
Democracy											-0.259
											(0.498)
Constant	26.284***	26.111***	26.321***	25.727***	23.238***	24.922***	28.461***	29.206***	27.486***	25.511***	26.396***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of observations	51	51	51	51	51	51	51	51	51	51	51
Prob>F	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
R-Squared	0.83	0.82	0.82	0.82	0.83	0.83	0.84	0.84	0.77	0.83	0.83

Notes: Dependent variable: (log) Aid commitments to recipient country, sum 2008-2010 / * (**, ***) indicates significance at the ten (five, one) percent level

Table 5.7: Further checks on Robustness: Comparison of India's aid allocation with other donors

(Indian aid recipients only, 2008-2010)

	India	**USA**	**EU-3**	**Good donors**	**Japan**	**Korea**	**UAE**
(log) GDP per capita	**-0.249**	0.003	-0.628**	-0.647**	-0.314	-0.237	-1.174**
	(0.191)	(0.994)	(0.035)	(0.011)	(0.108)	(0.429)	(0.027)
		0.552	*0.203*	*0.154*	*0.823*	*0.970*	*0.119*
(log) Affected from disasters	**0.097***	0.131*	0.025	0.062	0.083*	0.099	-0.213
	(0.053)	(0.083)	(0.705)	(0.342)	(0.056)	(0.278)	(0.188)
		0.696	*0.244*	*0.595*	*0.844*	*0.979*	*0.073*
(log) Population	**-0.483*****	0.868***	0.543**	0.567***	0.312**	0.841***	-0.256
	(0.002)	(0.005)	(0.013)	(0.001)	(0.020)	(0.000)	(0.507)
		0.000	*0.000*	*0.000*	*0.000*	*0.000*	*0.601*
(log) Distance	**-1.634*****	0.184	-0.535	-1.139**	-1.467***	-1.336***	-2.001*
	(0.000)	(0.798)	(0.317)	(0.022)	(0.000)	(0.002)	(0.072)
		0.025	*0.041*	*0.394*	*0.618*	*0.544*	*0.739*
UN voting (key votes)	**6.826*****	4.023	-0.383	0.717	-0.076	10.602**	-1.883
	(0.000)	(0.165)	(0.880)	(0.766)	(0.964)	(0.016)	(0.678)
		0.389	*0.021*	*0.030*	*0.001*	*0.388*	*0.097*
Common colonial history	**-1.219*****	4.189***	1.039**	2.681***			0.606
	(0.000)	(0.007)	(0.050)	(0.000)			(0.519)
		0.001	*0.000*	*0.000*			*0.092*
(log) Bilateral exports	**0.401*****	0.043	0.506***	0.311***	0.220***	0.217*	0.231**
	(0.001)	(0.858)	(0.009)	(0.002)	(0.003)	(0.074)	(0.014)
		0.136	*0.616*	*0.468*	*0.179*	*0.184*	*0.226*
(log) Resource depletion	**-0.027**	0.037	0.012	-0.030	-0.019	-0.031	0.020
	(0.137)	(0.253)	(0.630)	(0.291)	(0.164)	(0.222)	(0.618)
		0.066	*0.084*	*0.926*	*0.716*	*0.914*	*0.301*
Political rights	**0.056**	-0.026	0.083	0.141	-0.052	0.186	-0.499
	(0.692)	(0.897)	(0.410)	(0.251)	(0.474)	(0.177)	(0.190)
		0.756	*0.881*	*0.697*	*0.491*	*0.468*	*0.200*
Control of corruption	**1.481*****	0.175	0.972**	1.321***	0.573**	-0.267	-1.157
	(0.000)	(0.805)	(0.021)	(0.005)	(0.048)	(0.607)	(0.398)
		0.094	*0.314*	*0.707*	*0.025*	*0.001*	*0.064*
Donor country dummies	**Yes**	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations				574			
Number of recipients				51			
- per donor group	51	51	51	50	51	49	33
R-Squared				0.65			

Notes:
- Estimation technique: Nested OLS with standard errors clustered by recipient country
- Dependent variable: (log) Aid commitments to recipient country, sum 2008-2010
- We report coefficients of the explanatory variables (corresponding p-values in parentheses)
- In italics: p-values of a Wald test of equal marginal effects of the respective donor (group) compared to India
- * (**, ***) indicates significance at the ten (five, one) percent level

7. Conclusion

This final section briefly summarizes the findings of this work. The thesis consists of three essays in the field of international political economy, and two essays in political economy of development in India. The international political economy essays focus on a wide array of topics. We examine the social effects of globalization and market economic policy reforms. In doing so, we have expanded the knowledge-base surrounding the political economy literature through several dimensions.

The question of whether free-market reforms cause decline in human rights is an issue that is extensively debated in the literature, particularly in debates surrounding globalization and the effects of IMF interventions. In the absence of a meaningful consensus, the question of whether economic reforms towards freer markets cause social dissent and disarray, measured by the level and degree of state violations of human rights, is one which needs to be dealt with empirically. We address this by examining the association between the level and rate of change towards more free-market policies, and the respect for the human rights of individual citizens in their own states. The results support those who argue that freer markets generate better economic conditions and higher levels of social harmony.

Advancing the debate on the intervention effects of global financial institutions on political stability, we critically examine the theoretical and empirical evidence of Hartzell,

Hoddie and Bauer (2010) find that signing on to an IMF structural adjustment program (SAP) increases the risk of civil war. We question their crucial assumptions about the impact of IMF programs on the economic environment, and more specifically, who actually wins and losses from liberalization, as well as who might be in a position to rebel. Clearly, separating the effects of a crisis itself from IMF interventions is crucial, since crises also generate losers in their own right. With minor adjustments to their dataset, we find the opposite of what they conclude. These findings highlight the need for a careful analysis; if one is to believe that IMF intervention leads to civil war, then it is important to find out the exact channels through which the IMF has this impact.

In the third chapter, we utilize spatial econometric methods to estimate whether labour rights in one country depend on those elsewhere. We find positive support for this argument, which is consistent with strategic complements and a necessary condition for there to be a race to the bottom. In particular, this seems to be driven primarily by competition in labour practices rather than labour laws, suggesting that competition is influenced more by a failure to enforce regulations than unwillingness to enforce them. Since there is a noticeable downward trend in both of these measures over the sample period, we interpret this as competition for FDI, as opposed to a diffusion of labour rights, which would result in an improvement in laws, possibly even as practices declined as more workers sought to assert their rights. We thus take this as an evidence of a race to the bottom.

In chapters four and five we examine two critical issues related to India. In chapter four, we investigate whether the timing of elections affects the responsiveness of the incumbent state governments to control corruption activities. The findings show that scheduled elections, but not unscheduled elections, are associated with an increase in the number of corruption cases

registered by anti-corruption agencies in Indian states. These effects are found to be stronger in swing states and in those states where state scheduled elections coincide with national elections. However, we do not find any effect of scheduled elections on corruption cases being investigated by anti-corruption agencies. The policy conclusion that can be drawn from the analysis is the need for independent anti-corruption institutions, which are free from political interference and are equipped with independent investigative and prosecution powers.

The final chapter examines the determinants of India's development aid program. With the intension of understanding why poor countries such as India provide foreign aid, this paper has empirically analyzed India's aid allocation decisions. We utilized data on aid commitments by the Ministry of External Affairs to 127 developing countries in US dollars, obtained from the AidData database for the 2008-2010 period. To examine whether India is different, we also compared India's aid allocation decisions with those of other donors. Our empirical results show that India's aid allocation is partially in line with our expectations of the behavior of a "needy" donor. Commercial and political self-interests dominate India's aid allocation. We find the importance of political interests, proxied by UNGA voting alignment, to be significantly larger for India than for all traditional DAC donors under investigation. Moreover, India favors countries which are geographically closer, and countries at a similar developmental stage are more likely to enter India's aid program.