The effect of Human Rights on Economic Growth

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Preface

This paper was written in the spring and summer of 2008, as a master thesis for the Environmental and Development Economics Program at the University of Oslo. The idea for this paper came after a student trip to Russia in the autumn of 2007, which focused on human rights. Later discussions led to a search for empirical investigations of whether protection of human rights had an effect on economic growth. The results of the search were meagre. It was not until the beginning of 2008 that it occurred to me that this had potential to be the topic of my master thesis.

I would like to thank my supervisor Aanund Hylland for his comments and suggestions in the shaping and writing of the thesis. Carl Henrik Knutsen also deserves considerable gratitude, for our many discussions on the theory and estimation of the topic.

Finally, I would like to thank my wife Tone for her patience these last months, allowing me to spend time on this thesis that would otherwise be spent with her.

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Summary

The “Lee thesis” claims that there is a tradeoff between a country’s respect for human rights and its economic growth. This argument has been used for justifying the lack of protection of human rights in developing countries. Without acknowledging the legitimacy of the argument, this paper investigates the claim. It does this by analyzing the effects of a subset of human rights (physical integrity rights) on economic growth. The paper presents a brief review of previous theoretical and empirical contributions to understanding how human rights practices affect various economic variables, including foreign direct investment, domestic investment, foreign aid, and trade.

The main focus of the paper is on the empirical level, by means of econometric analysis on cross-sectional time-series data. With country-years as the unit of analysis, the paper presents estimations on the effect of physical integrity rights on economic growth through diverse econometric techniques. Using OLS, a cross-country estimation is performed on values averaged over time, and this estimates significant positive effects of physical integrity rights. However, deeper analysis using the panel data and dummy variables for each value of the physical integrity index (range 0-8) allows a nonlinear relationship to be observed.

The results are striking. There appears to be a strong nonlinear and non-monotonic relationship between the physical integrity index (range 0-8) and GDP growth. The results from OLS with panel corrected standard errors, random effects, fixed effects and nonparametric matching all indicate a similar structure. While a score of 0 on the physical integrity index is associated with the lowest growth rates, a score of 2 is associated with the highest growth, in all estimations. Scores above 2 are generally associated with substantially lower growth rates, though a relatively large increase in growth is observed as the score goes from 7 to 8.

The non-monotonic result is consistent with Robert Barro’s inverse U-shaped relationship between democracy and economic growth. His argument is that an initial reduction of repression facilitates higher growth through reduced fear and higher security, allowing a country’s inhabitants to be more productive. A subsequent reduction of repression leads to

less growth through demands for redistribution or political uncertainty. However, in contrast to Barro, here a final increase in growth is observed as the repression reaches the lowest level.

Further analysis indicates that this relationship is robust to various specifications. Regressions on disaggregated indices indicate that there are heterogeneous effects of the variables indicating torture, disappearances, extrajudicial killings and political imprisonment, which together make up the physical integrity index. These are estimated to have various effects on growth, possibly leading to the nonlinear and non-monotonic relationship between the aggregated physical integrity index and growth.

While human rights practices affect growth, growth also affects human rights practices. An attempt to control for reverse causality using two stage least squares was performed, and this reports a negative, though insignificant, effect of physical integrity rights on growth when using a single linear estimator. Nonlinear estimation using the technique did not give meaningful results. However, there are indications of endogeneity bias, and the empirical results using other techniques must be interpreted with caution.

The Lee thesis cannot be rejected on the findings in this paper. However, the Lee thesis can be rejected by rejecting the premises of the argument.

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1. Introduction

> …, a great many people in different countries of the world are systematically denied political liberty and basic civil rights. It is sometimes claimed that the denial of these rights helps to stimulate economic growth and is "good" for rapid economic development. Some have even championed harsher political systems – with denial of basic civil and political rights – for their alleged advantage in promoting economic development. This thesis (often called "the Lee thesis," attributed in some form to the former prime minister of Singapore, Lee Kuan Yew) is sometimes backed by some fairly rudimentary empirical evidence.
>
> Sen 1999, p.15

> …freedoms… are among the constituent components of development. Their relevance for development does not have to be freshly established through their indirect contribution to the growth of GNP or to the promotion of industrialization. As it happens, these freedoms and rights are also very effective in contributing to economic progress.
>
> Sen 1999, p 5

The "Lee hypothesis" claims that there is a tradeoff between a state's economic growth and its provision of protection against human rights violations. This claim has been an important argument for justifying developing countries' lack of protection of basic human rights. Whether one chooses to accept the premises of the argument or not, the claim that there exists a tradeoff between human rights and growth is one that is testable. This thesis attempts to do this in a limited fashion. Focusing only on the most basic of human rights, protection of a person's physical integrity, I will estimate the effects of improving human rights practices on economic growth.

1.1 Human Rights

Human rights refer to the basic rights and freedoms to which all individuals are entitled through the property of being human. The Universal Declaration of Human Rights adopted by the United Nations General Assembly in 1948 is a non-binding declaration urging member nations to promote a number of human, civil, economic and social rights. Later, in 1966, two treaties were created, the International Covenant on Civil and Political Rights (ICCPR), and the International Covenant on Economic, Social, and Cultural Rights (ICESCR), both of which are binding for signatory countries. In the thesis, I will focus on the most basic human rights, physical integrity rights. These include freedom from torture, freedom from extrajudicial killings and disappearances, and freedom from political imprisonment. Protection against violations of these rights is covered by the ICCPR.

1.2 Why is it important to study the effects of human rights on economic growth?

While there does exist a sizeable literature on the effect of development and economic growth on human rights, there appears to be little empirical research on the specific effects of physical integrity rights on economic variables. The relationship has been indirectly studied through analysis of broad measures of democracy and growth (Knutsen 2008). However, a specific study of the relationship between physical integrity rights and economic growth may be interesting for a number of reasons.

It may be the case that protection of these rights has a negative effect on growth. If so, then there exists a tradeoff between two factors of human welfare, between material welfare and the welfare provided by the rights examined. This view has been presented by, among others, the former prime minister of Singapore, Lee Kuan Yew, who justified limiting freedoms for the sake of economic stability and prosperity. If a tradeoff is found between physical integrity rights and economic growth, acknowledgement of this may lead to policies better designed to counter this tradeoff. Recognizing their intrinsic value, governments, international organizations, and domestic pressure groups can make use of research on this relationship to shape incentives to promote these rights more efficiently.

It may be the case that protection of human rights has no significant influence on growth rates. If so, there is no tradeoff between material welfare and welfare from the protection of rights. The promotion of human rights will then not harm the growth of countries, and therefore may not be used as a valid argument for the non-protection of these rights.

It may be the case that protection of human rights has a positive effect on growth rates. Instead of a tradeoff, there is a situation of reinforcement. The promotion of human rights will indirectly promote growth, and a policymaker who seeks to maximize growth should therefore facilitate protection of human rights. Then, protection of human rights has both intrinsic and instrumental value, and governments, international organizations, and domestic pressure groups can make use of this in promotion of human rights.

1.3 Methodology

This thesis is primarily empirically oriented. I will briefly present findings from previous research on the relationship between human rights and economic variables. The main focus, however, will be on the methodology and estimation of the effects of basic human rights on economic growth, through quantitative econometric techniques. Finally, the thesis will conclude with a discussion of the results and their applicability to the research question: is there a tradeoff between basic human rights and economic growth?

2. Human rights and economic growth

The discipline of human rights studies has a vast literature in the practices, philosophy, political science, international law, as well as economics of human rights. Amartya Sen has been a long standing defender of human rights in ethics and economics, and has contributed much to the field of human rights studies (Vizard 2005). Sen has criticized "standard" economic frameworks for neglecting to take sufficient account to the intrinsic and instrumental value of human rights, claiming that dominant approaches in economic studies have concentrated on economic processes and outcomes narrowly interpreted in terms of utility without any specific acknowledgment of fundamental freedoms and human rights. Sen argues that "…political liberty and civil freedoms are directly important on their own, and do not have to be justified indirectly in terms of their effect on the economy" (Sen 1999, p.16). In addition, he also argues that protection of human rights is efficiency enhancing in that greater freedom enhances the ability of people to help themselves.

As Sen argues, the intrinsic value of protection of human rights is an integral part of human welfare, and cannot be overemphasized. For example, protection against torture, arbitrary arrest and the right to political participation can be viewed as goods of extremely high value. Indeed, discovering the intrinsic value of protection of the various types of human rights is a fascinating study in itself. The criticism that economists tend to ignore human rights' intrinsic value is a fully valid criticism of this study as well (Sen 1999; Branco 2007). However, in my opinion, the *instrumental* value of human rights has also been understudied by economists. This thesis is an attempt to contribute to the latter field. It will focus solely on the instrumental value of human rights protection, through traditional economic measures of welfare. How does protection of human rights affect growth? Which channels do they work through, if any? I will attempt to sketch some of the more plausible channels below. But first I will look at some of the reverse causality mechanisms.

2.1 How does economic growth affect human rights?

This paper intends to investigate how practices of some human rights affect economic growth. However, it is very likely that economic growth affects practices of human rights,

and that we have a two-way causality. This may lead to biased estimators in the empirical investigation. Before progressing into the main analysis on how human rights protection affects growth, I will therefore consider the reverse case: how does economic growth affect the protection of human rights? An early attempt to investigate the causes and correlations between human rights violations, measured through torture and political imprisonment, and political and economic factors is Mitchell & McCormick (1988). The authors question whether the poorest nations are the worst violators of human rights, if the worst violators have a certain colonial background, and whether newer nations are more prone to repress. The article attempts to test these hypotheses using bivariate statistical analysis for a limited number of countries in 1984. The authors find that the economic factors have more explanatory leverage than the political factors. They find limited support for the thesis that poor countries are more prone to repression, and that only countries with very high income do well on human rights.

A more recent empirical investigation of the causes of human rights violations was undertaken by Poe et. al. (1999). Using pooled cross-sectional time series data from 1976-1993, the authors regressed a number of explanatory variables on measures of human rights practices, among them level of GDP and growth of GDP. The authors expect level of GDP to have a positive effect on the protection against human rights violations, since countries with higher incomes are less likely to face domestic rebellion to which country leaders may respond with repression. However, they argue that the effect of economic growth on human rights practices may not be straightforward. On one hand, economic growth generates a larger economy, so countries face less resource constraints that may lead to domestic rebellion. On the other hand: "Economic growth would increase repression because it increases the number of *déclassé* individuals and groups that are most prone to promote instability" (Poe et al 1999: 294). The authors argue that these two effects pull in opposite directions. A previous study undertaken by the authors in 1994 showed no significant effects of growth on human rights practices. The estimated effect of level of GDP is highly significant and positive on human rights practices, so that a higher domestic income level is associated with better practices. The estimated effect of GDP growth is significant at the 1% level in one of their specifications, and positive, but the estimated coefficient is not very big. Even an extremely high growth rate per year would have negligible effects on human rights practices (Poe et. al. 1999 p. 307).

2.2 How does the protection of human rights affect growth?

What about the effect of human rights on growth? Are there any theoretical foundations for claiming that protection of human rights has an effect on economic growth? Human rights, and the protection of these, are part of such a basic level of societal structure that their protection may have an affect on a range of factors. The different types of human rights are highly correlated with each other (Park 1987) and I review here some studies which have focused on human rights in general as well as some which have focused on physical integrity rights specifically. Some of the main channels I specify may be through foreign direct investment (FDI), trade, foreign aid, and domestic investment.

2.2.1 Direct effect on growth

A direct effect of protection of human rights may be that citizens and workers have an increased optimism, less fear, and more trust in the economy, and therefore choose to work more. Lorenz Blume and Stefan Voigt (2007) argue that physical integrity rights are a precondition for other rights such as property and civil rights, and are therefore efficiency enhancing. Their paper "Economic Effects of Human Rights" is the first paper I have found to empirically estimate the impact of human rights on economic variables, instead of vice versa (and to the authors' knowledge, the first paper published on the topic). Using human rights data from 1990 to 1997, Blume and Voigt estimate the effects of human rights practices on growth, investment, and productivity. They find that while the estimated direct effect of physical integrity rights is positive, it is not statistically significant.

Robert Barro (1998) estimated the effect of democracy on growth for approximately 100 countries between 1960 and 1990, and found that the effect appears to be inverse U shaped – initially increasing but later decreasing. The author explains this relationship by arguing that in the worst dictatorships an initial strengthening of political freedom leads to growth through limiting the states' power, but a further increase in political freedom leads to a reduction in growth through a dominating effect of pressure for redistribution (p. 59). Is this argument transferrable to the case of physical integrity rights as well? As a country with high levels of repression of physical integrity rights moves toward better practices, it is possible to imagine that the reduction in practices of torture, disappearances, political imprisonment and extrajudicial killings reduce levels of fear in the economy. Blanton and

Blanton (2007b: 146) argue: “Citizens who do not fear violent government retribution are more willing to contribute their time, talents—and more importantly, their ideas—towards the economic good of their country”. However, if the repression is used in the first place to keep domestic pressures under control, reduced repression may allow these pressures to surface – perhaps leading to demands for economic redistribution or political instability. This could be an argument for an effect of physical integrity rights on growth similar to Barro’s findings.

2.2.2 Effect on Foreign Direct Investment

How do human rights practices affect foreign direct investment? Busse (2004) has explored this empirically, and found that a country’s respect for civil and political rights has a significantly positive effect on foreign direct investment. In addition, Busse reports changes in the effect of rights over time. While in the 1990s the coefficient of civil and political rights is significantly positive, the coefficient in the 1980s is not significant but still positive. However, in the 1970s the reported coefficient is negative, though not significant. This implies that the strong significant relationship does not hold for the 1970s and 1980s, and there is no evidence that the Transnational Corporations made decisions based on these rights in this period. Busse presents two hypotheses for the change in policies. The first is that the main sectors of FDI have changed over the time period. Busse claims that FDI in the 1970s was driven more driven by transnational companies’ (TNCs) search for raw materials (Busse, p. 57), which depended more on location of these resources, and therefore more on specific host countries. TNCs were therefore more dependent on good relations with governments of the host country. In the 1980s and 1990s other motives were driving investment by TNCs, such as cheap labor and access proximity to export markets. Since TNCs had a wider choice of countries for these activities, they could be more selective of their investment sites. In addition, they depended less on the local government than previously. The second hypothesis explaining change in policy presented by Busse is that the increased focus of nongovernmental organizations (NGOs) on exposing human rights violations of TNCs. As technology has eased the spread of information, increasing awareness of violations has made TNCs vulnerable to campaigns exposing unpalatable practices. TNCs must now weigh the benefits of cheap labor against the probability of negative publicity.

Blanton and Blanton (2007b) have analyzed the effects of human rights practices on FDI over the time period 1980-2004. They argue that good human rights practices can reduce risks for investors by increasing political stability in host countries and reducing the probability of "audience costs", costs imposed by public campaigns targeting companies involved in poor human rights (HR) practices. The study focuses on investment in non-OECD countries, with FDI inflows as the dependent variable. Controlling for market size, development, economic growth, trade openness, government consumption, resource wealth, and democracy, the authors find that HR practices affect FDI flows positively and significantly both directly and indirectly through human capital (life expectancy and education). They also calculate the magnitudes of the effects, estimating that the direct effect of a shift from the lowest level of respect of human rights to the highest level is associated with an increase of FDI flow equivalent to over 4% of the host country's GDP. The indirect effects are estimated to be much smaller, but still substantial.

2.2.3 Domestic investment

By investing resources instead of consuming them, such as investing in infrastructure like roads, power, and telecommunications, economies can facilitate future growth. Private investment production techniques and factories will allow these to produce goods and services more efficiently. Many of the arguments that human rights practices affect foreign direct investment are also valid for domestic investment. Blume and Voigt also estimate the effects of human rights on investment and productivity, both of which are assumed to positively affect growth. They do find significant positive effects of physical integrity on investment and productivity.

Farber (1999) explores theoretically how investors may infer information on the investment climate, and specifically risk of expropriation, from a country's human rights practices. The author assumes that providing protection of human rights has a cost, since they act as trumps over normal government decisions. Since they are costly to provide, provision of them can be interpreted as a credible signal that the countries are willing to pay short term costs for long term gains. Investors infer from this that countries have a low discount rate, and that it is less likely that the country will sacrifice long-term interests to benefit short-term ones.

2.2.4 Effect on Trade

Blanton and Blanton (2007a) estimate the effect of human rights on international dyadic trade patterns. Using imports in pairs of countries as the dependent variable, they use cross-sectional time-series analysis to estimate the effects of human rights for the years 1989-2000 for 154 countries. They explain some different rationales for trade to be positively associated with human rights practices (p.102-103).

- Repressive states are weak states.
- Economic actors need confidence that a government will not disrupt economic actions through military action, embargos or state corruption. Repressive states are a priori interventionist.
- Respect for human rights generates positive spillovers that make them better trading partners.
- Citizens who are free from fear of government repression are more motivated to contribute their time and talents to the economy.

Controlling for GDP, population size, geographic distance, similar language, regime type, military alliance, conflict, and whether both countries are members of the WTO, Blanton and Blanton find empirical support for the overall hypothesis that respect for human rights advances trade. Their first estimation finds that trade is held back by the practices of the trading partner with weakest HR practices. Their second estimation finds that the better the partner with the highest HR practices is, the more this advances trade. A third estimation confirms what is implied by the two previous ones: the average level of the two trading partners has a positive effect, such that countries with higher HR practice averages trade more.

While significant, the estimated magnitudes of the effects of human rights are much smaller than traditional economic explanations of trade. While a change from the lowest to highest value of market size, for example, gives an estimated increase of 28.5% in trade volume, a change from the lowest to highest value of human rights practices gives an estimated increase of 2.9%. The estimate is close to the estimate from the lowest to highest change in value of the distance variable, which is 3.7%.

2.2.5 Effect on Foreign Aid

Neumayer (2003) investigates whether there are significant patterns of foreign aid based on human rights practices. Specifically, he estimates the effect of physical integrity rights and political rights on OECD countries choice of countries to assist with bilateral aid, and thereafter the choice of the size of the bilateral aid. In his analysis, Neumayer controls for need through the log of GDP per capita, and interests through former colonial status, geographical distance and military alliance. His findings are mixed: while respect for political rights does seem to be a significant determinant of whether a country receives aid for most donors, physical integrity rights do not appear to be significant. On the other hand, the size of aid the receiving countries get is related positively to physical integrity rights for France, Japan, the United Kingdom, Denmark, Canada and Australia, while the opposite is true for Italy, the United States, the Netherlands, Norway, Luxembourg and Switzerland.

3. Methodology

In the thesis, I will follow Blume and Voigt's main approach to estimation of effect of human rights on growth. There will be, however some major differences. While Blume and Voigt focus on four groups of human rights, these being physical integrity rights, property rights, civil rights and emancipatory rights, I will focus solely on physical integrity rights: freedom from torture, extrajudicial killing, political imprisonment, and disappearances. On the other hand, I will make use of more data. While Blume and Voigt only use data from 1990 to 1997, I will use data from a longer time period, from 1981 to 2006. Additionally, I will use more sophisticated econometric techniques, utilizing the panel structure of the data.

In estimation, I will use only reduced form analysis. At present, I believe the effects of human rights on growth are too understudied and complex for there to be any advantage to structural form estimation. Further studies may allow the construction of a coherent theoretical model, which can then be tested using structural form.

3.1 Choice of welfare variable

The analysis focuses entirely on the instrumental value of human rights, through traditional economic measures of welfare.

> Growth is not an end in itself. But it makes it possible to achieve other important objectives of individuals and societies. It can spare people en masse from poverty and drudgery. Nothing else ever has….. In short, we take the view that growth is a necessary, if not sufficient, condition for broader development, enlarging the scope for individuals to be productive and creative.
>
> (World Bank 2008a: 1)

The main question of this thesis is whether respect for human rights has an effect on economic welfare. Economic welfare, through GDP (Gross Domestic Product) is a measure of a nation's income as a whole. It is indeed a very crude measure of a country's inhabitant's economic welfare, not to mention their general welfare. A somewhat better measure of welfare is established by adjusting for price levels through PPP (purchasing power parity), and dividing this by the population, giving us GDP PPP per capita.

This measure is not perfect in any way, since it will only measure an average income per head, telling us nothing about the distribution of income. Other measurements of human welfare may certainly be more precise in measuring actual welfare: measurements that

include the distribution of income, health, access to public services, education, as well as income itself would be more suitable if the goal was to evaluate whether human rights practices affected a broad measure of human welfare. The Human development index (HDI) is a well known attempt to measure human welfare, taking into account life expectancy, literacy, education, living standards, as well as GDP per capita.

While GDP PPP is a narrow and perhaps crude measure of human welfare, it is an important part of development and welfare. It is also a widely used measure of economic outcomes and performance, with much data available. The data on GDP used in this analysis has been acquired from the World Bank's World Development Indicators (WDI).

3.2 Measuring human rights

How does one then measure and quantify the broad concepts and practices of human rights? By their nature, violations of human rights may be difficult to report, measure, classify and aggregate (Landman 2004). Meaningful econometric analysis of the research questions is dependent on an adequate amount of data on the countries of interest, preferably over a number of years making it possible to analyze intra-country variation as well as inter-country variation.

3.2.1 Signatories of human rights treaties

One method of measurement could be to observe which countries have signed and ratified international treaties protecting human rights. For example, the ICCPR has 160 signatories and the ICESCR has 157 signatories. By using dummy variables for each treaty, one could create an index of human rights treaties. However, this approach is flawed, in many ways. From a statistical point of view, the high percentage of signatories leaves us with little information, giving us with too little variation to analyze in any sophisticated way. In addition, since countries do not tend to withdraw from the treaty, variation over time will be attributed to new signatories. Yet the main problem of this approach is that even signatories of these treaties in fact violate human rights, and one will not have any information about these violations. If it is *de facto* violations I am interested in, and not *de jure*, other approaches may be more suited. In fact, some studies (Neumayer 2003, Neumayer 2005)

have found that the signing of human rights treaties have no or even *negative* correlation with practices of human rights.

3.2.2 Human rights practices

While the two covenants on human rights are legally binding for signatories, the actual protection of the rights may vary to a great extent. As I am interested in de facto protection of human rights, a necessity for empirical estimation is the accurate measurement of human rights practices. This, however, presents plenty of problems in itself. A number of deep-rooted challenges to recording violations of human rights are prevalent. In many countries, governments play a major role in the collection of data and the publishing of statistics. By their character, violations of human rights are symptoms of some type of failure of the state to protect these rights. For data collection to be reliable, some kind of external monitor must be used, and even then there are a number of pitfalls. Subjective judgments may lead to systematic and unsystematic bias, and different methods of evaluation may lead to different results.

> The primary problem, as in all human rights analyses, is simply the inadequacy of information on such violations, since governments are understandably reluctant to publicize their use of arbitrary imprisonment, torture, or killing.
>
> Mitchell and McCormick (1988, p. 483)

Even when assuming one has reliable information, another challenge emerges: how does one quantify information in a meaningful way so that it is possible to compare data over time and other countries? Compressing complex data based on events into an indicator to be used by statistics will invariably lead to some data loss and loss of complexity. It is not straightforward to compare a country that uses widespread, but mild torture with a country with rare, but severe torture.

3.2.3 CIRI Human Rights data

The Cingranelli and Richards (CIRI) dataset is the most comprehensive dataset on human rights that I have found, in length of time, number of countries, and violation types measured. The information for this database is based on the United States Department of State's Country Reports on Human Rights Practices, which is published annually. In

addition, Amnesty International's Annual Report is used[1] to verify the physical integrity rights (extrajudicial killings, disappearances, torture, and political imprisonment). The information is coded to data by at least two independent coders, using a coding manual to ensure consistency of coding. The dataset measures as many as 195 countries over 26 years, from 1981 to 2006.

The dataset is freely available for research purposes. The dataset has 13 ordinally categorized indicators at the year-country level. I will primarily use the aggregated physical integrity index, with 0 being the lowest level of respect and 8 the highest. The data in the physical integrity index is aggregated by adding together the indicators for Extrajudicial killings (0-2), Disappearances (0-2), Torture (0-2), Political imprisonment (0-2). A score of 0 indicates that a practice happened frequently in a country-year, a score of 1 indicates that a practice happened occasionally, and a score of 2 indicates that a practice did not occur in a country-year. A score of 0 on all four variables leads to a score of 0 in the physical integrity index, a score of 2 on all leads to an aggregated score of 8.

> Extrajudicial killings are killings by government officials without due process of law. They include murders by private groups *if* instigated by government. These killings may result from the deliberate, illegal, and excessive use of lethal force by the police, security forces, or other agents of the state whether against criminal suspects, detainees, prisoners, or others.
>
> Disappearances are cases in which people have disappeared, political motivation appears likely, and the victims have not been found. Knowledge of the whereabouts of the disappeared is, by definition, not public knowledge. However, while there is typically no way of knowing where victims are, it is typically known by whom they were taken and under what circumstances.
>
> Torture refers to the purposeful inflicting of extreme pain, whether mental or physical, by government officials or by private individuals at the instigation of government officials. Torture includes the use of physical and other force by police and prison guards that is cruel, inhuman, or degrading. This also includes deaths in custody due to negligence by government officials.
>
> Political imprisonment refers to the incarceration of people by government officials because of: their speech; their non-violent opposition to government policies or leaders; their religious beliefs; their non-violent religious practices including proselytizing; or their membership in a group, including an ethnic or racial group.
>
> Cingranelli & Richards (2008: variable descriptions)

[1] Due to possible bias of US State Department Country Reports (Landman 2005:558)

3.3 The basic model

I am interested in the relationship between human rights practices in a country and the country's economic growth. I have data from each country from 1981 to 2006. The basic econometric model is as follows:

$$\%\Delta Y_{it} = \alpha_i + \beta \mathbf{X}_{it} + \gamma \mathbf{HR}_{it} + \lambda \mathbf{I}_{it} + \chi \mathbf{O}_{it} + \mu_{it}$$

The economic growth (change in income Y), in country i at time t, is a result of an intercept (which may be country specific), and a vector of standard explanatory variables **X** and their coefficients. Our area of interest is the variables of basic human rights **HR**, and their corresponding coefficients. Through including transmission channels **I**, I can estimate the direct effects of HR, and by omitting them I find the direct plus the indirect effects. I will also extend the initial controls with additional controls **O**.

The standard explanatory variables **X** are: log of PPP-adjusted GDP per capita, population growth rate, and average total years of education. GDP per capita is included as a control since I assume that a country's growth is in part dependent on the level of its GDP per capita, and that this effect decreases as GDP increases. The effect of population growth on economic growth is controlled for by including it as a variable. The education data is perhaps the weakest link in the analysis, being compiled by interpolating between 5 year intervals, and only with data from 100 countries. The GDP and population variables are from the World Bank's WDI, while the education data is from the Barro-Lee data set.

The **HR** variables will be based on the physical integrity (PI) index, and the variables for torture, disappearances, political imprisonment and extrajudicial killings. The analysis will use various specifications of the PI index, using PI itself, a single dummy indicator or multiple dummies. The PI index is coded on a scale of 0-8, where 8 is the measure of best practices. In the dataset, it has a distribution of:

Table 3.1 Frequency and distribution of the Physical Integrity Index

PI	Frequency	Percent	Cumulative Percent
0	207	5.24	5.24
1	224	5.67	10.91
2	294	7.44	18.36
3	347	8.79	27.15
4	515	13.04	40.19
5	556	14.08	54.27
6	547	13.85	68.12
7	644	16.31	84.43
8	615	15.57	100.00
Total	3949	100.00	

The variables included in **I** will be real share of investment, foreign direct investment (FDI), official development assistance (ODA), and degree of openness in the economy. A major driver of economic growth is the level of investment in the economy. I earlier argued that the level of investment may be affected by human rights practices, and that this may be an important transmission channel for human rights' effect on economic growth. FDI, while a part of investment, may be interesting in its own right, due to dispersion of technologies which may themselves have an effect on growth. Official development assistance is included in the transmission variables, and for some countries, ODA can contribute to a significant share of GDP. Finally, I have included openness of the economy as a transmission variable, which is the ratio of imports plus exports to GDP. For data on investment and openness, I have used Penn World Tables, and for data on FDI and ODA I used WDI.

Finally, the additional control variables **O** will be regional dummies, religion dummies and dummies for colonial influence[2]. This structure follows the main approach of Blume and Voigt, while some of their variables under **X** are moved to **I**, since I consider them to be transmission variables. Additionally, the controls under **O** are different.

[2] The data on these and other controls come from Knutsen (2007).

3.4 Challenges to estimation

The measurement of the effects of basic human rights is challenging on many levels. I am dealing with a concept that is difficult to measure and quantify, which has a broad influence on the economy, with possible diverse effects. In addition, there is likely a two-way causality. Some of the major challenges are:

Measurement error/biased human rights indicators

By nature, information about human rights violations is not easy to obtain, due to many factors, such as victims being unwilling to report violations and oppressors having the power to control access to victims. Even in a situation of full information about the violations, I would still face problems of quantifying violations that may differ in severity and extent into a single measure. I keep this in mind with any data on human rights, as measures of human rights violations may be biased and measured with error.

The data on physical integrity is an additive index of four different types of violations. Each of these violation types is weighted equally, and each violation type has three levels which also are weighted equally. This somewhat arbitrary additive index may lead to problems, in that two units with the same aggregate score may have very different practices. The aggregate index may be compressing multiple dimensions into a unidimensional indicator. I will therefore investigate the relationships with use of the disaggregated indices as well.

Endogeneity bias

Another significant challenge in the estimation is that it is likely that I have an endogeneity bias, in that the causality goes both ways; while human rights affects growth, growth also affects human rights. If action is not taken to correct this, the estimators will be biased. Estimation by help of instrumental variables may be used to correct this; the challenge is then to find a suitable instrument. Blume and Voigt use a different approach: to ensure that each measurement of human rights is not affected by the dependent variable, they use a lag of three years, such that for GDP growth in year 1993, HR data from 1990 is used. I will follow this procedure in the analysis, and will primarily use a lag of three years. This has very real risks however, as while the lag of three years will ensure that there is not a direct effect of growth in year t on the human rights practices in year t-3, an autocorrelation of

order 3 or more will leave the variable correlated with the disturbance, giving inconsistent estimators. An attempt to correct for this will be done by running instrumental variable regression using a longer lag as an instrument. A Hausman specification test can then test the endogeneity of the three year lag.

Choice of control variables

The choice of control variables is also a challenge to estimation, as the effect of human rights is likely to have a strong indirect effect on GDP growth, as well as a direct effect. The inclusion of control variables in a regression will control away any indirect effects, so the estimation may be sensitive to which control variables are included. My solution to this is to estimate each step twice: once with the transmission variables included, and once without them. The former will estimate only the direct effects of the human rights variable, while the latter will include the indirect effects of the omitted transmission variables as well. This is not without problems, since the transmission variables may also have reverse causality, and affect the HR variables. I will therefore use caution when interpreting indirect effects.

Misspecification of the model

A potentially major weakness of this study may be that the model is misspecified in some way. The relationships I study may be nonlinear, there may be significant interaction effects, or omitted variables. To achieve the highest robustness of results, I will use different techniques to estimate these effects, among them OLS with panel corrected standard errors, fixed and random effects regressions, 2SLS regression, and nonparametric matching methods.

Yet econometric techniques may only partially mitigate the risk of misspecification. As previously mentioned, violations of human rights vary to a great extent, in both intensity and character. The indicators that I use for measurement and estimation may be (and probably are) condensed into too few dimensions, losing information in the process. Perhaps the nature, the causes and the effects of human rights violations in China in 2006 are fundamentally different than violations in Yugoslavia in 1991, even though they have the same score on the physical integrity index. It is possible I am dealing with context dependent effects, and that generalization over time and geography is difficult.

This potential problem does not disappear. It will be an Achilles heel of this quantitative analysis. The best econometric techniques will only partially mitigate this. It should be kept in mind when interpreting this and similar studies. I do what I can with the data I have.

3.5 Which results do I expect?

What kind of effects do I expect to see from estimation? The research previously presented gives some indication of which ways the direct effects and indirect effects will work. While Blume and Voigt did not find any significant relationship between physical integrity rights and economic growth, the estimate they did get was positive. They did find a significantly positive relationship between physical integrity rights and both investment and productivity, and that this is likely to have a positive indirect effect on growth. The other studies indicate that physical integrity rights have a positive effect on FDI and investment in general, as well as trade. These are all assumed to have a positive effect on growth.

4. Empirical Results

I will now use econometric analysis to investigate if there is any support for the hypothesis. By analyzing the data available, I will attempt to specify the relationships and filter out the noise, through diverse econometric techniques. While ordinary least squares (OLS) simply treats each country-year as an independent unit, panel data regression allows us to correct for autocorrelation, as well as country and time specific effects, and panel specific standard errors.

The structure of this part will be as follows: First, I will present a simple OLS regression on cross-sectional values averaged over time, before I move on to OLS on pooled cross-sectional time-series data with panel corrected standard errors. I then run random and fixed effects estimation before using two-stage least squares (2SLS). Finally, I use nonparametric propensity score matching and compare the results. In each regression, I start with the main control variables **X**, the human rights variables **HR**, and the indirect transmission channels **I**. As I am interested in indirect effects as well as direct effects of human rights practices, I thereafter drop **I** and repeat the regression without the transmission channels, enabling the coefficient of physical integrity to capture the indirect effects, as well as the direct effect. Finally, other control variables are presented, dummies for region, religion and colonial influence, and the process is repeated.

4.1 Cross-sectional data: OLS regression

As a starting point, I follow Blume and Voigt's approach to estimation of the effect of the physical integrity index on growth. The authors use OLS on cross sectional data created primarily by averaging values over the time period 1993-2000 for GDP growth and investment, as well as using data for GDP per capita in 1990 and average years of schooling from 1985. To reduce the risk of reverse causality, they lag the data for physical integrity and empowerment index so that it is averaged from the period 1990-1997 (Blume and Voigt 2007: 528). Ending up with a sample size of 110, the estimated effect of the physical integrity index is positive but insignificant. As the dataset I have available is much longer in time than that which is used by Blume and Voigt (1981- 2006), I repeat the estimation to analyze if any effects of the physical integrity index are significant. The dataset I have is

initially in panel form, with each country having a number of years of data. For this first analysis, I have averaged the same variables as Blume and Voigt, and end up with pure cross-sectional data. An advantage to this technique is that it reduces the impact of any random measurement error.

A major disadvantage is that any variations in time are not used in estimation, thus much information is lost. Additionally, this technique does not allow for correction of autocorrelation, country specific effects, or any omitted variable bias, which is likely prevalent in the data. I will, however, be able to correct for this in the subsequent estimation techniques.

Table 4.1 Coefficients from OLS regression on cross-sectional data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable	Average growth GDP PPP per capita 1984-2006						
Log of GDP PPP per capita 1984	-0.5969	0.0910	0.1066	-0.0260	0.4125	-0.2826	-0.1525
	(-1.55)	(0.25)	(0.31)	(-0.07)	(0.885)	(-1.36)	(-0.73)
Average population growth 1984-2006	0.5095	0.2954	0.04935	0.0348	-0.0749	0.2544	0.1112
	(2.19)**	(0.95)	(0.18)	(0.10)	(-0.24)	(1.06)	(0.47)
Log of total years of education 1985	0.4690	-0.0673	0.1698	-0.2808	-0.1006		
	(1.17)	(-0.16)	(0.39)	()-0.52	(-0.18)		
Average investment share 1984-2006	0.1244	0.1078	0.04293	0.06199		0.0664	
	(3.60)***	(3.28)***	(1.10)	(1.24)		(2.13)	
Average FDI 1984-2006		8.44e-11	1.20e-10	1.20e-10		1.25e-10	
		(1.98)**	(3.21)***	(2.20)**		(2.54)**	
Average ODA 1984-2006		1.52e-09	6.71e-10	4.05e-10		1.12e-10	
		(3.28)***	(1.30)	(0.53)		(0.18)	
Average degree of openness 1984-2006		0.0018	-0.0023	-0.0041		-0.0046	
		(0.43)	(-0.68)	(-0.78)		(-1.08)	
Average PI index 1981-2003	-0.0349	0.2225	0.3646	0.3471	0.2592	0.2675	0.1570
	(-0.35)	(1.64)	(2.39)**	(2.46)**	(2.12)**	(2.05)**	(1.32)
Constant	4.9962	-0.6899	-0.06465	-0.0298	2.2595	3.1912	4.8919
	(2.06)**	(-0.27)	(-0.02)	(-0.01)	(0.59)	(1.04)	(1.89)*
N	96	74	74	74	74	107	107
Region controls			X	X	X	X	X
Religion controls				X	X	X	X
Colonial influence controls				X	X	X	X

Note: z-values in parentheses. * Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level

Using data from the longer time period, I repeat the Blume and Voigt's estimation with the same dependent and control variables. The results are reported in (1) in table 4.1. The results from regression (1) are similar to the results from Blume and Voigt: the standard explanatory variables have the same sign and somewhat similar z- values. The major difference from their estimation is that the estimated effect of the physical integrity index is now negative, while still insignificant. Regression (2) includes the rest of the transmission variables, FDI, Official Development Assistance, and openness to trade. The sample size is reduced, due to

less information on these variables. However, the regression left the estimate of the PI coefficient positive and close to significant at the 10% level. The effect of going one step up on the PI index is estimated to be 0.2 percentage points. The inclusion of controls for regions (3) and for religion and colonial influence (4), make the PI index significant at the 5% level, and the estimated influence is higher, at 0.35 percentage points per step on the PI index. Omitting the transmission variables, as in regressions (5) and (7) reduces the coefficient estimate, which could be an indication of negative indirect effects via these channels; however, it is premature to conclude.

There are some arguments for dropping the control for years of education in the subsequent regressions. There appears to be a multicollinearity problem between log of GDP per capita, and years of education. In addition, I only have data on years of education for 100 countries, which have a relatively narrow intersection with the other variables, limiting the breadth of the dataset. By dropping the variable, the analysis can be extended to more countries, from 74 to 107 in regressions (6) and (7) above, without much loss of explanatory ability.

In this first simple analysis, I find significant positive effects of human rights practices on growth, when controlling for various country-specific properties. I will move on to more advanced econometric techniques to further investigate whether this is a persistent result.

4.2 Pooled cross-sectional time-series: OLS with panel corrected standard errors

I now make use of the panel structure of the data to correct for different types of "noise". Pooled cross-sectional time series with panel corrected standard errors is a good starting point for this. This technique uses OLS as above, but uses the panel structure to calculate panel specific standard errors and autocorrelation. The advantage of this technique is that it utilizes cross-country variation as well as variation over time for inferences of the coefficients, and calculates panel specific standard errors and corrects for autocorrelation of the first order. A disadvantage of this method compared to random or fixed effects estimation is that it is more sensitive to omitted variable bias. Instead, it is assumed that all countries have the same intercept.

Table 4.2 Coefficients from OLS with panel-corrected standard errors

	(8)	(9)	(10)	(11)	(12)	(13)
Dependent variable:			GDP PPP growth in %			
Log of GDP PPP per capita	0.3770 (1.61)	0.3962 (1.55)	0.6711 (2.61)***	0.8616 (3.33)***	0.6697 (2.58)***	0.8657 (3.32)***
Growth of population	0.7636 (3.62)***	0.8202 (3.43)***	0.8600 (3.48)***	0.8155 (3.29)***	0.8659 (3.51)***	0.8219 (3.33)***
PI index lagged 3 year	-0.0266 (-0.39)	-0.0309 (-0.45)	-0.0419 (-0.61)	-0.0408 (-0.60)		
PI≥5 dummy lagged 3 years					-0.4493 (1.67)*	-0.3867 (1.43)
Investment share	0.1795 (5.97)***	0.1726 (5.84)***	0.1730 (5.47)***		0.1748 (5.52)***	
FDI	-3.04e-11 (0.69)	-2.36e-11 (0.54)	-3.44e-11 (-0.77)		-3.88e-11 (0.85)	
ODA	1.06e-09 (3.48)***	6.94e-10 (2.47)**	8.15e-10 (2.32)**		8.15e-10 (2.34)**	
Openness	0.0004 (0.12)	-0.00324 (0.93)	-0.0040 (-0.99)		-0.0036 (0.92)	
Region control		X	X	X	X	X
Religion control			X	X	X	X
Colonial influence control			X	X	X	X
Constant	-3.5349 (1.80)*	-2.4980 (-1.01)	-2.498 (-1.01)	-2.2610 (-0.72)	-5.6393 (1.84)*	-2.2802 (0.73)
Rho	-0.1781	-0.0917	-0.0917	0.2235	-0.0882	0.2279
Countries	122	122	122	122	122	122
N	2024	2024	2024	2024	2024	2024

Note: z-values in parentheses. * Significant at the 10% level. ** Significant at the 5% level.

*** Significant at the 1% level. Estimated coefficients for region, religion and colonial influence omitted for space reasons.

In regressions (8)-(11) I use the 3 year lag of the physical integrity index as the HR variable. The coefficient estimates for each of these regressions are negative, but insignificant. The results from the OLS on averaged values do not persist into in this specification. Perhaps this result is due to the specification itself? In regressions (12) and (13) I code a dummy variable indicating whether the physical integrity index takes value 5 or above, splitting the data approximately 40/60. The dummy will take value 0 for values on the index from 0 to 4, and 1 for values 5 to 8. The variable is lagged for 3 years as before. This dummy coding does appear to strengthen the significance of the estimates, with the coefficient still being negative, but significant at the 10% level in (12). The estimated effect of dropping the transmission variables in (13) is positive, reducing the coefficient estimate and the significance level.

I now investigate whether other specifications of the data are fruitful. The physical integrity scale is ordinally measured, but not cardinally, therefore it may not be expected to measure the effect of human rights practices linearly. Even if it was cardinally measured, it is likely that the effects of physical integrity practices in general do not have a linear effect on economic growth. In addition, the specification of a single dummy indicating “high” or

"low" on the physical indicator scale may be subject to a degree of arbitrariness based on the chosen divider, and equally inadequate to measure the effects on growth. To further investigate the structure of the effects, I therefore use dummy indicators for all values of the scale (omitting the value 0 on the physical integrity index), and repeat the estimation.

Table 4.3 Coefficients from OLS using panel corrected standard errors

	(14)	(15)	(16)	(17)	(18)	(19)
Dependent variable:			GDP PPP growth in %			
Log of GDP PPP per	0.3683	0.8068	0.3700	0.5987	0.6356	0.83015
capita	(1.60)	(3.40)***	(1.46)	(2.51)**	(2.52)**	(3.27)***
Population growth	0.7724	0.6575	0.8337	0.7921	0.8783	0.8342
	(3.67)***	(3.05)***	(3.53)***	(3.33)***	(3.61)***	(3.41)***
PI=1	1.4166	1.5816	1.3999	1.5385	1.4542	1.5622
Lagged 3 years	(2.30)**	(2.56)**	(2.29)**	(2.52)**	(2.37)**	(2.55)**
PI=2	2.1413	2.1119	2.1332	2.2053	2.2209	2.1857
Lagged 3 years	(3.51)***	(3.53)***	(3.53)***	(3.72)**	(3.67)***	(3.68)***
PI=3	1.2342	1.328	1.2535	1.4341	1.2912	1.4059
Lagged 3 years	(2.16)**	(2.30)**	(2.23)**	(2.56)**	(2.33)**	(2.51)**
PI=4	0.9273	1.1046	0.9593	1.2172	0.9624	1.1348
Lagged 3 years	(1.56)	(1.83)*	(1.64)	(2.09)**	(1.67)*	(1.94)*
PI=5	0.9813	1.1949	0.9862	1.2244	1.0094	1.1823
Lagged 3 years	(1.67)*	(2.02)**	(1.70)*	(2.14)**	(1.74)*	(2.04)**
PI=6	0.9108	1.1171	0.9304	1.2180	0.9021	1.0963
Lagged 3 years	(1.49)	(1.83)*	(1.53)	(2.05)**	(1.51)	(1.84)*
PI=7	0.6881	0.6657	0.651	0.7839	0.5852	0.6229
Lagged 3 years	(1.07)	(1.05)	(1.01)	(1.27)	(0.92)	(1.00)
PI=8	1.3975	1.2946	1.3246	1.4611	1.2793	1.3101
Lagged 3 years	(1.97)**	(1.86)*	(1.86)*	(2.12)**	(1.81)*	(1.88)*
Investment share	0.1797		0.1698		0.1675	
	(6.07)***		(5.89)***		(5.39)***	
FDI	-3.81e-11		-3.38e-11		-4.42e-11	
	(-0.92)		(-0.81)		(1.06)	
ODA	1.10e-09		7.58e-10		9.06e-10	
	(3.59)***		(2.62)		(2.60)	
Openness	0.0008		-.0025		-0.0030	
	(0.20)		(-0.71)		(0.75)	
Region control			X	X	X	X
Religion control					X	X
Colonial influence control					X	X
Constant	-4.676	-5.362	-3.4361	-3.2079	-6.7918	-3.4762
	(-2.40)**	(-2.56)**	(-1.38)	(-1.37)	(2.17)**	(-1.12)
N	2024	2024	2024	2024	2024	2024
Number of groups	122	122	122	122	122	122

Note: z-values in parentheses. * Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level. Estimated coefficients for region, religion and colonial influence omitted for space reasons.

The regression (14) indicates an initial surprise: while the estimates of the physical integrity dummies on growth are all positive, they do not appear to be linear, or even monotonic as the physical integrity index increases. The coefficient estimates in (14) predict that while there is a major jump from going from 0 to 1 on the physical integrity index, and a equally large jump from 1 to 2, the effect of going from 2 to 3 is negative, and all coefficient

estimates of index values larger than 2 are much smaller. In other words, for everything else equal, the highest growth rate is associated with a score of 2 on the physical integrity index.

In all the regressions, this pattern is persistent; there is a strong increase in estimated growth as PI goes from 0 to 1, and from 1 to 2. After that the estimated growth is lower, and decreases further as the index goes up. The exception to this seems to be at the top of the index, where the estimated coefficient of an index value of 8 is generally much larger than that of the index value of 7 for all regressions. The results seem robust against the inclusion of control variables. The same pattern is also seen in (16) and (17), which includes regional dummies, and appears to be even stronger than in (14) and (15). The addition of dummies for religion and colonial influence in (18) and (19) reinforce the impression.

What can be inferred about the indirect effects? For any comparable estimation, it seems to be more likely that the estimated coefficients of the index dummies are higher when the transmission channels are omitted. This could be an indication that the indirect effects of going up the physical integrity index are positive.

The results above indicate that the effects of physical integrity rights are nonlinear, and that a single linear estimator may be inadequate to capture the effects in the regressions. Instead, dummies for each value of the indicator will be primarily used for the further analysis. Before I draw any conclusions on the effects of physical integrity rights however, I will move on to more suitable estimation techniques. In the above method of estimation, there is a risk that I have not controlled for all relevant variables, and that the results suffer from an omitted variable bias. To examine this, I go on to estimation using the random effects model.

4.3 Panel data regression: Random effects

Random effects regression has the advantage that it controls for a degree of omitted variable bias, through the assumption that there are country-specific effects which are controlled for through a country-specific intercept. These intercepts are assumed to be normally distributed among countries at random, and are treated as if they are part of the error term. Random effects estimation gives more efficient estimators than fixed effects estimation since it saves on degrees of freedom. Also, it allows the inclusion of time-invariant controls, the region,

religion, and colonial influence dummies. Another advantage over fixed effects estimation is that it uses a weighted average of inter-country information as well as intra-country information (Kennedy 2003, p. 305).

Table 4.4 Coefficients from random effects estimation

	(20)	(21)	(22)	(23)
Dependent variable:	GDP PPP growth in %			
	Robust standard errors		AR(1)	
Log of GDP PPP per capita	0.4420992	0.7723118	0.5114903	0.8636076
	(1.59)	(3.58)***	(2.34)**	(4.50)***
Population growth	0.7617556	0.7220851	0.6863333	0.6425878
	(2.28)**	(2.20)**	(5.17)***	(4.79)***
PI=1 Lagged 3 years	1.377167	1.408513	1.473274	1.517184
	(1.70)*	(1.75)*	(2.42)**	(2.47)**
PI=2 Lagged 3 years	2.328779	2.319733	2.34751	2.31108
	(3.10)***	(3.10)***	(3.97)***	(3.87)***
PI=3 Lagged 3 years	1.516153	1.657703	1.575856	1.6855
	(2.14)**	(2.33)**	(2.73)***	(2.89)***
PI=4 Lagged 3 years	1.278185	1.520373	1.381929	1.555371
	(1.83)*	(2.18)**	(2.46)**	(2.75)***
PI=5 Lagged 3 years	1.240138	1.413483	1.327544	1.441829
	(1.77)*	(2.05)**	(2.33)**	(2.52)**
PI=6 Lagged 3 years	1.27696	1.535812	1.325695	1.508774
	(1.76)*	(2.17)**	(2.25)**	(2.56)**
PI=7 Lagged 3 years	0.8991225	1.108945	1.005041	1.100538
	(1.20)	(1.52)	(1.64)	(1.80)*
PI=8 Lagged 3 years	1.447179	1.701304	1.577975	1.699661
	(1.86)*	(2.25)**	(2.30)**	(2.48)**
Investment share	0.1347145		0.1412621	
	(6.14)***		(6.23)***	
FDI	-1.27e-11		-9.81e-12	
	(-0.60)		(-0.26)	
ODA	8.87e-10		8.47e-10	
	(3.09)***		(2.61)***	
Openness	0.0026201		0.0005049	
	(0.55)		(0.13)	
Region	X	X	X	X
Religion				
Colonial influence				
Constant	-4.594043	-5.444669	-5.082645	-6.191161
	(-1.54)	(-2.03)**	(-2.09)	(-2.59)
Rho			0.20464324	0.20245203
N	2023	2024	2023	2024
Number of groups	121	122	121	122

Note: z-values in parentheses. * Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level. Estimated coefficients for region, religion and colonial influence omitted for space reasons.

A disadvantage with this approach compared to OLS with panel-corrected standard errors is that it is not possible to simultaneously correct for heteroskedastic standard errors (through the robust option) and autocorrelation in the statistics program used, STATA. In (20) and (21) heteroskedasticity is corrected for, while (22) and (23) correct for autocorrelation.

Again, the same structure appears: an increase in the physical integrity index increases growth up to an index value of 2, after which growth decreases until an index value of 8, where it increases steeply. The heteroskedasticity robust estimation delivers approximately the same coefficient estimates for the variables of interest as the autocorrelation-corrected estimation, while the latter has generally higher z- values. The estimated extra growth associated with having a score of 2 versus 0 on the physical integrity index is a sizable 2.3 percentage points in direct effects, with a estimated negative (though much too small to be significant) indirect effect. For an index score above 0, the estimated coefficient of the index score equal 7 is the smallest, estimated between 0.89 and 1.00 percentage points, with a positive indirect effect estimated between 0.1 to 0.2 percentage points.

However, it is possible that random effects model is unsuited for this type of estimation. I argued before that the practices of human rights affects society at such a basic level that it may affect growth through a number of channels, and there is a very real risk that some of these are omitted from our estimation. This omitted variable bias may create correlation between the composite error used by random effects and the dependent variable, causing the random effects estimator to be biased (Kennedy 2003, p. 306). I move on to fixed effects estimation.

4.4 Panel data regression: Fixed effects

Fixed effects estimation takes a step further than random effects estimation, by creating intercepts specific for each country. This mitigates many problems with omitted variable bias, since any unobserved characteristics which are persistent for the country through the time series will be controlled for. The inclusion of the dummies of **O** is therefore not necessary (or possible), because they are controlled for by the estimation method. A**s** under random effects estimation, there is the disadvantage STATA does not allow correction for heteroskedastic standard errors simultaneously as for autocorrelation, so I run them sequentially.

Table 4.5 Coefficients from fixed effects estimation

	(24)	(25)	(26)	(27)
Dependent variable:	GDP PPP growth in %			
	Robust standard errors		AR(1)	
Log of GDP PPP per capita	2.5362 (3.02)***	2.5994 (3.99)***	2.9393 (5.07)***	3.1310 (6.00)***
Population growth	1.0026 (3.03)***	1.079 (3.44)***	0.93719 (5.93)***	1.0338 (6.52)***
PI=1 Lagged 3 years	1.506 (1.78)*	1.5355 (1.82)*	1.4285 (2.30)**	1.4173 (2.25)**
PI=2 Lagged 3 years	1.9899 (3.11)***	1.9482 (3.11)***	1.953227 (3.16)***	1.8653 (2.98)***
PI=3 Lagged 3 years	1.3649 (1.98)**	1.4215 (2.06)**	1.2382 (2.00)**	1.2622 (2.02)**
PI=4 Lagged 3 years	1.0707 (1.64)*	1.2244 (1.90)*	1.2319 (1.98)**	1.2939 (2.06)**
PI=5 Lagged 3 years	1.0036 (1.36)	1.2018 (1.63)	1.0522 (1.66)*	1.1373 (1.78)*
PI=6 Lagged 3 years	1.1605 (1.65)*	1.3995 (2.01)**	1.1478 (1.72)*	1.3016 (1.93)*
PI=7 Lagged 3 years	0.3606 (0.50)	0.5646 (0.79)	0.3809 (0.54)	0.4572 (0.64)
PI=8 Lagged 3 years	1.352 (1.66)*	1.5164 (1.87)*	1.4112 (1.74)*	1.5381 (1.88)*
Investment share	0.1902 (4.78)***		0.2739 (6.81)***	
FDI	-8.10e-11 (-3.58)***		-7.33e-11 (-1.45)	
ODA	8.35e-10 (2.20)**		6.93e-10 (1.46)	
Openness	0.0005 (0.05)		-0.00379 (-0.50)	
constant	-21.6903 (-3.45)***	-19.8327 (-3.74)***	-25.3361 (-7.26)***	-23.7703 (-7.19)***
Rho			0.2093	0.2025
N	2024	2024	1902	1902
Number of groups	122	122	118	118

Note: z-values in parentheses. * Significant at the 10% level. ** Significant at the 5% level.

*** Significant at the 1% level.

One disadvantage of using the fixed effects estimation method is that it uses a dummy for every country, which means it uses up an additional degree of freedom for each group, leading to a less efficient estimator. The technique analyses only the intra-country variation, variation over time, and does not utilize variation over countries. Another disadvantage of using fixed effects estimation is that since all country specific effects are controlled for, any persistent indirect effects of the variables of interest will also be controlled away. This is also a type of omitted variable bias, but one which may be interesting nonetheless, and fixed effects estimation removes this. The estimators above are sizably smaller than before, and this is likely due to this bias. Even so, the structure of the effect of going up on the physical integrity index is similar as before. Again all else equal, a score of 2 gives the highest

predicted growth, a score of 7 gives the second lowest predicted growth, before a jump up at a score of 8. The estimated coefficients of the physical integrity index are quite close between the heteroskedasticity robust estimation and the autocorrelation-robust estimation, with slightly higher z-values estimated from the latter.

4.5 Choice of estimation method

Which method of estimation is preferable? Pooled or panel? This depends on whether there are country specific effects, as estimated through country-specific intercepts. By performing an F-test on whether the country-specific intercepts are jointly significantly different from zero, I can find if the panel structure is necessary, and if not, I can use the more efficient pooled method. The null hypothesis, that all the intercepts are zero, is rejected at any level. This implies that I should use the panel structure. The next question to evaluate is whether I should use random effects or fixed effects. Random effects estimation is preferable if its conditions are satisfied. I performed a Hausman specification test, comparing estimates from fixed effects estimation with random effects estimation. The null hypothesis is that both fixed effects and random effects estimators are consistent, and the alternative hypothesis that random effects estimators are inconsistent. Again, the null hypothesis is rejected at any level. I conclude that for this estimation, the fixed effects model is the preferred estimation technique.

4.6 Panel data: Two stage least squares

In all of the previous regressions, I have ignored a major potential problem to estimation. While human rights practices affect growth, growth also affects human rights practices. I may have a problem of endogeneity. Blume and Voigt acknowledged this problem, and attempted to mitigate this by lagging the human rights variables with three years. I have followed this approach until now, while ignoring an obvious problem. Even with the 3 year lag in the physical integrity variable, if it has effects which are persistent over time, it may be correlated with the error term and therefore lead to inconsistent estimators. Two stage least squares regression is a common econometric technique to mitigate this problem. By finding an instrument which is correlated with the endogenous variable, and uncorrelated

with the error term, instrumental variable regression will allow us to estimate consistent coefficients.

For lack of a better instrument, I use the 6 year lag of physical integrity index as an instrument for current physical integrity values in 2SLS estimation with fixed effects. The results are reported below.

Table 4.6 Coefficients from 2SLS using fixed effects, first and second stage

	(28) Fixed effects estimation Stage 1	(29) Fixed effects estimation Stage 2
Dependent variable:	PI index	GDP growth
PI Instrumented		-1.5557
		(-1.49)
Log of GDP PPP per capita	0.6109	3.4355
	(3.43)***	(3.76)***
Population growth	0.1813	1.1992
	(4.22)***	(5.02)***
Investment share	0.0123	0.2859
	(1.13)	(6.54)***
FDI	-2.43e-11	-9.66e-11
	(-1.86)*	(-1.71)*
ODA	2.08e-10	1.07e-09
	(1.57)	(1.88)*
Openness	-0.0009	-0.0058
	(-0.46)	(-0.76)
PI index, lagged 6 years	0.08312	
	(3.65)***	
Constant	-1.2760	-21.9523
	(-0.93)	(-4.18)***
N	1691	1691
Number of countries	120	120

Note: z-values in parentheses. * Significant at the 10% level. ** Significant at the 5% level.

*** Significant at the 1% level.

In stage 2, the estimated coefficient for the physical integrity index is negative, though not significant. The size of the estimate is large and likely too large: -1.55 percentage points[3] less growth per step up on the physical integrity index. The estimated standard error is very large. By performing a Hausman specification test, I compared the results from 2SLS with the results from an equivalent fixed effects estimation using the 3 year lag of the physical integrity index. The Hausman test rejected the null hypothesis at the 5% level, (with a p-value of 0.042) that both of the estimates were consistent. These results are of course dependent on the assumption that the 2SLS estimators are consistent. Using the 5%

[3] The reported standard error for the coefficient was 1.046.

significance level, I infer that the fixed effects estimation is inconsistent. The preferred technique should be two stage least squares, most likely using fixed effects estimation.

However, I argued above that the effects of the physical integrity index on economic are likely not linear as well as not monotonic. A single variable will not pick up the effects I am interested in. I solved this in estimations above by introducing dummy variables for each of the values of the index. The standard procedure in 2SLS does not allow this, both due to identification problems, and due to that OLS is not a suitable method of regressing with dichotomous variables as the dependent variable. If there are ways to solve this problem, they elude me[4]. Instead, I keep this weakness in mind when I interpret the results.

4.7 Nonparametric matching

Matching estimation is a relatively novel approach in economics of investigating the effects of a dichotomous variable. The approach is similar to that of a controlled experiment, even if the data available is only observational. A controlled experiment approach can infer the effect of a treatment by dividing units by random into two groups, treating one and letting the other be the control group. The effects can then be found by comparing the two groups with each other. The matching technique performs a type of pseudo-experiment, without actually experimenting, based on the assumption of conditional independence. By dividing the observations into groups by the variable of interest, called "treated" and "controls", in this case low, and high, scores on the physical integrity index, it is possible to compare cases which are alike in all respects except for the treatment variable. The conditional independence assumption, as long as it is valid, assures us that the variable of interest is distributed randomly and uncorrelated with the error term as long as **X**, the control variables[5], are identical (Persson & Tabellini 2003, p. 138).

> In other words, if we are willing to assume conditional independence and consider countries with similar conditioning variables **X**, the counterfactual distribution of performance is the same as the observed distribution of performance.
>
> (Persson & Tabellini 2003, p. 138)

[4] Various specification types were tried in STATA, including a polynomial fit. However, the reported results were not meaningful.

[5] **X** here refers to all control variables, including **X**, **I**, and **O** as specified above.

The basic idea is that for countries which are similar in **X**, the values of the physical integrity index are randomly distributed, and comparing countries by the variable of interest is valid as in an experiment. The assumption is a very strong one, and likely violated in this case. This is important to keep in mind when interpreting the results. For any country with a "low" score on the index, the technique compares it with a "twin" country which has a "high" score. The difference in outcome, GDP growth, is then attributed to the treatment variable, the physical integrity index.

Matching is a technique which may be useful in an estimation of the kind performed in this thesis. When using least squares estimation, one must specify the relationship between the variables, which tends to be linear. In this case, as well as many others in social sciences, I do not know how the independent variables affect the dependent variable, and therefore face the risk of misspecification. The estimation results are sensitive to the model estimated, misspecification of the model could lead to biased estimators. Matching has the advantage that it is nonparametric, i.e. it does not make any assumptions about how the independent variables affect the dependent variable. Instead, it calculates the effects of the variable by comparing two (or more) similar cases, where observations which are as similar as possible and the difference between them is attributed to the "treatment". In contrast to linear regression, only local comparisons are used, and the effect is inferred by averaging these. Compared to OLS this does, however, come with a price, reducing efficiency of the estimators (Persson & Tabellini 2003, p. 139).

How does one determine which observations are "similar"? Countries differ on a number of dimensions, and how does one determine which ones should be weighted? There are different methods of determining which observations are to be matched. I here use the nearest neighbor estimation technique, which compares countries which are most similar in their likelihood to have the same value of the treatment variable. The technique allows for different types of specification, including robust standard errors, specifying the number of times each unit is matched, and the weighting of each variable. It should be kept in mind, though, that this method treats each observation independently from all others, and that any autocorrelation will not be corrected for. This technique, like linear regression, is also vulnerable to endogeneity bias. It is therefore important to treat the results with caution.

I dichotomize the lagged physical integrity index into a dummy indicator[6] which takes the value 1 at index values between 5 and 8 and 0 else. As before, I first include the transmission variables, and then exclude them to estimate the total effects. I vary the number of matches as well, between 1, 5, and 10 matches for each unit. The estimates are generally similar when varying the number of matches, while the reported standard errors are generally smaller as number of matches goes up. I report the results using 10 matches below.

Table 4.7 Average treatment effect from matching using single dummy

	(30)	(31)	(32)	(33)	(34)	(35)
Dependent variable:	GDP PPP growth in %					
Average treatment effect PI ≥5 dummy	-0.2838 (-1.18)	-0.0152 (0.06)	-0.2528 (-1.10)	-0.1784 (-0.84)	-0.2617 (-1.15)	-0.1944 (-0.85)
Log of GDP PPP per capita	X	X	X	X	X	X
Population growth	X	X	X	X	X	X
Investment share	X		X		X	
FDI	X		X		X	
ODA	X		X		X	
Openness	X		X		X	
Regional controls	X	X	X	X	X	X
Religion controls			X	X	X	X
Colonial Influence controls						X
Number of matches	10	10	10	10	10	10
Robust	X	X	X	X	X	X
N	2024	2024	2024	2024	2024	2024

Note: z-values in parentheses. * Significant at the 10% level. ** Significant at the 5% level.

*** Significant at the 1% level.

As seen above, the estimated average treatment effect of a dummy at 5 is always negative, although the estimated effect is not significant at any level in any of the estimations.

I have claimed earlier that there are strong reasons to believe that there is a nonlinear and non-monotonic relationship between physical integrity rights and growth. Therefore, the use of a dichotomous variable of interest may be a great disadvantage in this estimation, since the average treatment effect is exactly that, the *average* treatment effect. I mitigated this effect in linear regression by including a dummy for each value of the physical integrity index, and could do this without any loss of sample size. It is possible to do something similar in this case, but at a cost. I can create extract groups sorted by score of the index,

[6] To ensure robustness of results, I ran matching sequences with indicator cutoffs of 2,3 and 8. The results from these sequences were in line with the results from linear estimation.

such that only the score of 0 is compared with the score of 3, for example. However, for each comparison, this leads to a loss of sample size, as seen below.

Table 4.8 Average treatment effects from matching using multiple dummies

	(36)	(37)	
Dependent variable:		GDP PPP growth in %	
			N
PI=1 Lagged 3 years	1.4820	1.4049	246
	(2.47)**	(2.30)**	
PI=2 Lagged 3 years	2.4600	2.3900	294
	(3.90)***	(3.91)***	
PI=3 Lagged 3 years	1.7278	1.7290	327
	(2.74)***	(2.80)***	
PI=4 Lagged 3 years	1.8462	1.8874	448
	(2.85)***	(3.10)***	
PI=5 Lagged 3 years	1.8662	1.7909	450
	(3.01)***	(3.20)***	
PI=6 Lagged 3 years	1.9796	2.1248	404
	(2.89)***	(3.46)***	
PI=7 Lagged 3 years	1.5324	1.4254	385
	(2.17)**	(2.41)**	
PI=8 Lagged 3 years	2.2102	2.2752	268
	(3.30)***	(3.49)***	
Log of GDP PPP per capita	X	X	
Population growth	X	X	
Investment share	X		
FDI	X		
ODA	X		
Openness	X		
Regional dummies	X	X	
Robust	X	X	
Number of matches	10[7]	10	

Note: z-values in parentheses. * Significant at the 10% level. ** Significant at the 5% level.

*** Significant at the 1% level.

The results of this estimation are reported above. Each reported coefficient is the estimated extra percentage point growth that a country has by going from 0 to x on the index. From (36) it is clear that the estimated extra growth from going from 0 to 7 on the index, for example, is 1.5 percentage points. The presentation of the estimates is therefore similar to the presentation under least squares estimation, while the estimation technique is very different. All the estimated treatment effects are significant and positive, implying that they are all significantly higher than an index value of 0. In addition, they follow the structure of our previous results: the highest estimated growth is associated with a value of 2 on the physical integrity index, and at a higher level of protection growth is estimated to be lower.

[7] The procedure was also performed using 1 match per unit, with very similar estimates. Increasing the number of matches generally seems to decrease the standard errors, giving higher z-levels.

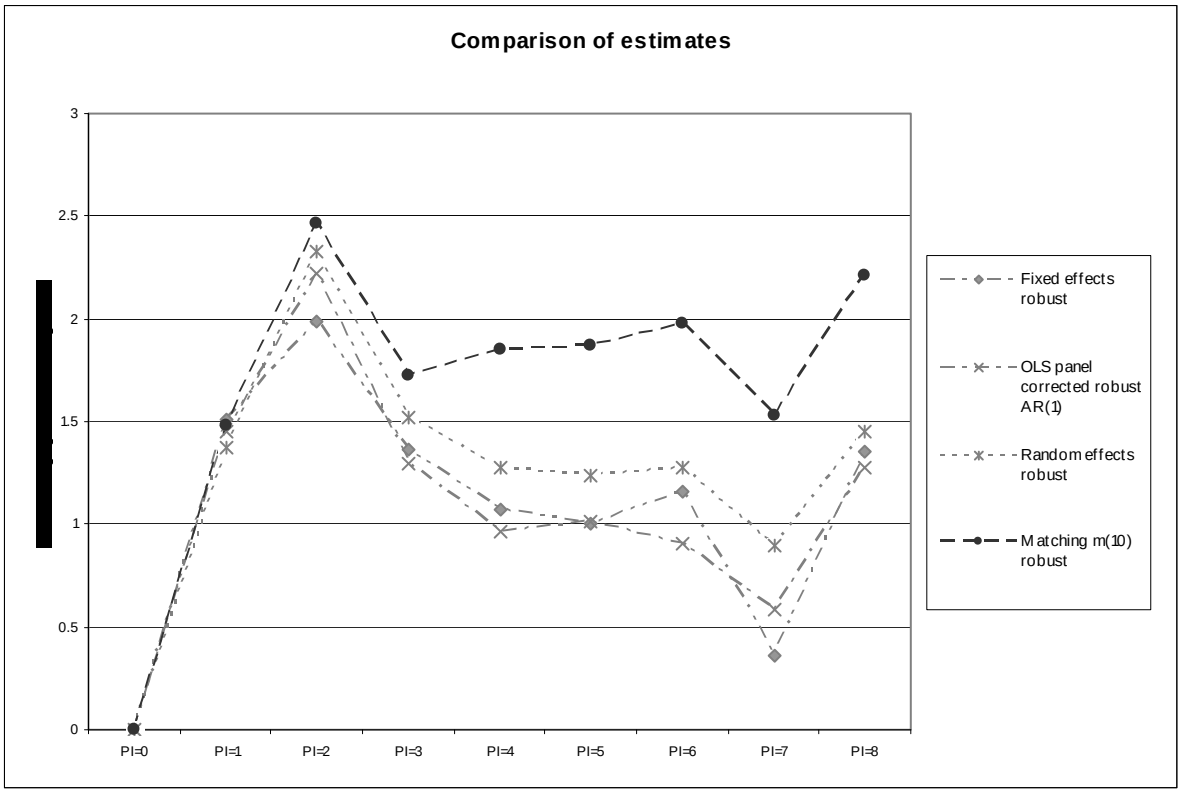
The final increase in estimated growth associated with the step up from 7 to 8 on the index is also observed.

4.8 Discussion of the results

The OLS estimations on a time-averaged cross-section gave indications that protection of physical integrity rights had a positive and significant effect on economic growth, when controlling for country specific properties. The estimation used the same approach as Blume and Voigt, but used a longer time-range to estimate the averages. The pooled data analysis using OLS with panel corrected standard errors estimated a negative effect while using a single dummy, indicating “low” and “high” values on the physical integrity index.

However, more sophisticated analysis has uncovered more information on the effect of the physical integrity index on economic growth. By using multiple dummy variables indicating the values of the index, a structure of the effect on growth has been revealed, which has been remarkably robust to estimation technique used. From OLS on pooled data to fixed effects panel data estimation, the general pattern of the results remain: for everything else equal, growth increases as a country goes from 0 to 2 on the physical integrity scale, and after that, growth is much lower. Each of the techniques associates an index value of 2 with the highest level of growth and an index value of 0 with the lowest level of growth. Another persistent result is that as a country goes from 7 to 8 on the index, estimated growth increases by a sizeable amount. This holds for both the direct effects, as well as the indirect effects through the channels specified, namely total investment, foreign direct investment, official development assistance, and openness to trade.

Figure 4.1 Comparison of PI dummy estimates using different techniques

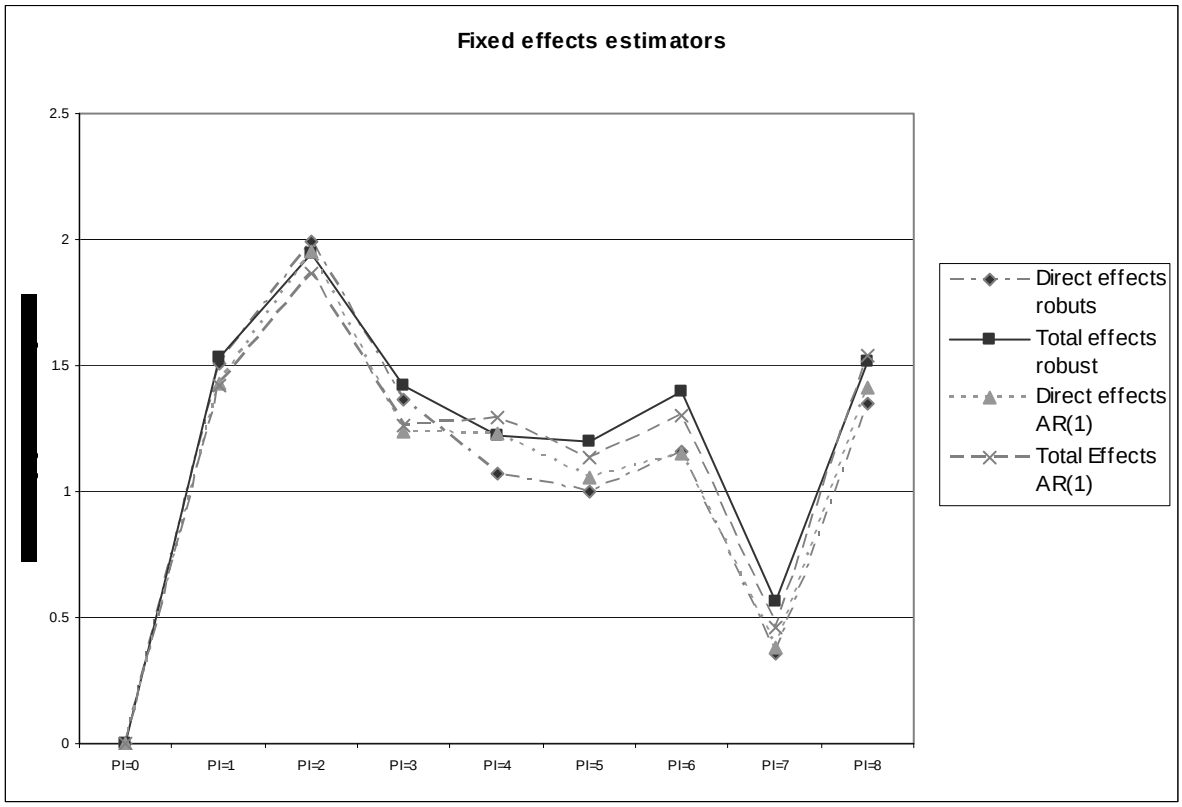


The table above presents the estimated coefficients from OLS with panel corrected standard errors, random effects, fixed effects, and matching. While the estimates from propensity score matching differ slightly, the results are remarkably similar across the techniques.

The structure of the results may partially explain why Blume and Voigt did not find any significant effects of the physical integrity index on growth: the effect is nonlinear, not monotonic, and hence not optimal for linear estimation. While there is a strong additional growth which seems to be attributed to an initial increase in the physical integrity index, this extra growth is much smaller for values between 3 and 7, though ending with a final increase at 8. The use of dummy variables for estimation of the relationship has allowed this relationship to be revealed.

Even though the main results appear to be robust to estimation technique, the tests reported in section 4.5 indicate that fixed effects estimation is the most preferable method with consistent estimators, at least when the possible endogeneity bias is ignored. The results from fixed effects estimation in section 4.4 are seen in figure 4.2.

Figure 4.2 Comparison of estimates from fixed effects estimation



The fixed effects model estimates that the direct effect of going up from 0 to 1 on the physical integrity index is very large: about 1.5 percentage points extra growth per year. The step from 1 to 2 has an estimated effect of an additional 0.5 percentage point. The next step up on the index has a negative effect, estimated to be -0.7 percentage points, with subsequent steps estimated to –0.15 percentage points and -0.12, successively. Going from 5 to 6 has an estimated positive effect of an additional 0.13 points of growth, while going from 6 to 7 has the largest estimated decrease, by -0.8 percentage points. Finally, the step from 7 to 8 gives an estimated 1 percentage point extra growth.

The indirect effects through investment, foreign direct investment, official development assistance and openness to trade are estimated to be much smaller. The difference between the points of direct effects and the points of total effects are attributed here to the indirect effect, and the difference can be seen on the graph above. For the AR(1) corrected estimates, the indirect effects are estimated to be negative (though very small) for values of 1 and 2 on the index, while the higher values are associated with positive indirect effects on growth.

Table 4.9 Difference between direct and total effects estimates

PI index	1	2	3	4	5	6	7	8
Estimated indirect effect, fixed effects AR(1)	-0.011	-0.088	0.024	0.062	0.085	0.156	0.076	0.127

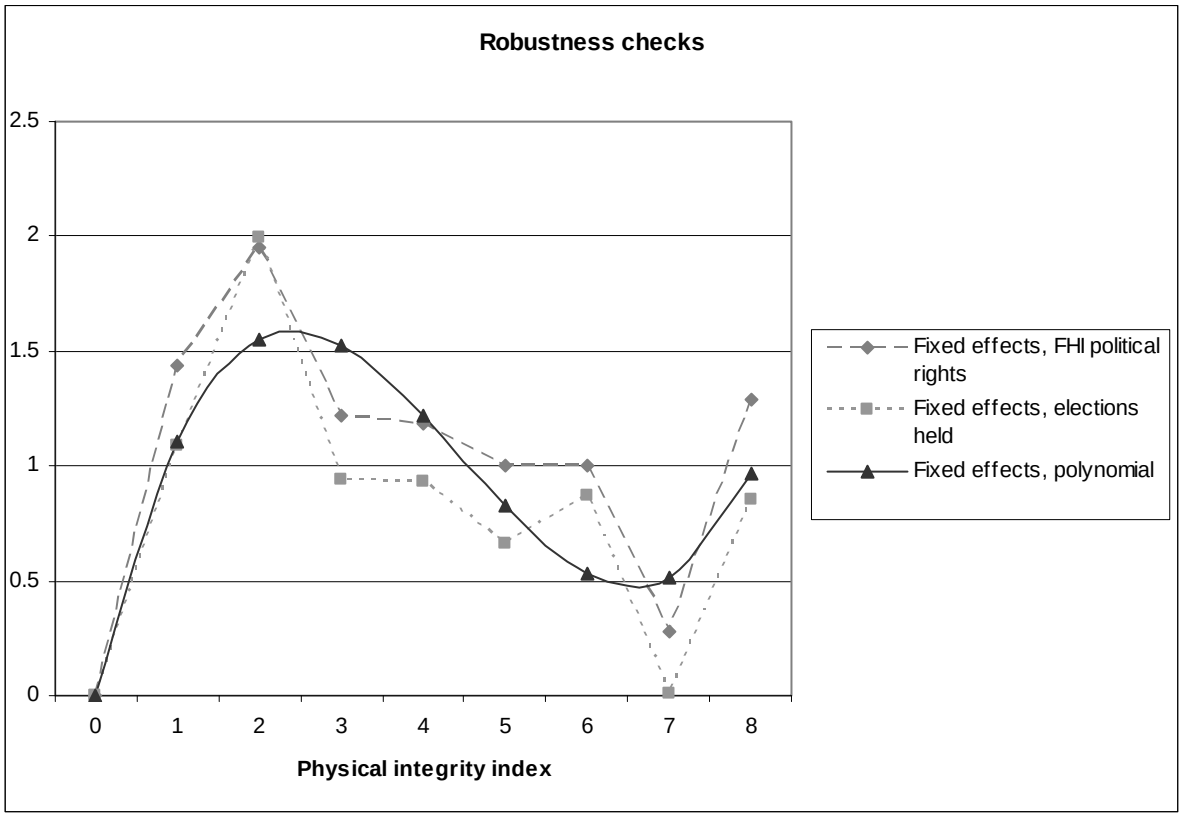
4.8.1 Robustness checks

To ensure that the results are not driven by some arbitrary choice of model or variable, I have run a number of other specification types, including different dependent variable (GDP PPP growth per capita) different control variables, additional control variables, year dummies, longer lag and no lag of the physical integrity index, interaction effects between the index and GDP, as well as divided the dataset in two time periods and compared the estimation. In all cases, the general structure of the effect of the physical integrity index on growth remains.

While dummies for each value of the physical integrity index are used in the main analysis, I have also done a number of analyses using the variables for the linear, squared, and cubed transformation of the variable. I have transformed the coefficient estimates into their value equivalents, and the results are very similar to the dummy estimates. The main results are unchanged from before, the highest growth is associated with the value 2 on the index, the lowest with value 0, and the shape of the polynomial is approximately the same, though smoothed out, in all specifications. Estimates using fixed effects can be seen in figure 4.3.

It may be argued that the result is driven by political factors such as democracy. Using both a narrow measure of democracy, (elections held), and a broader measure (Freedom House political rights index), I controlled for this effect. While reducing the coefficient estimates slightly, the general results remained unchanged in all specifications. Estimates using fixed effects can be seen in figure 4.3.

Figure 4.3 Estimates using polynomial and estimates controlling for democracy



In any regression, the size of the sample used for estimation is limited to the observations which contain information on each variable used for analysis. This means that the inclusion of an extra variable will limit the sample size, if information on that variable is a subset of the previous sample. The regressions above have been limited to only 122 countries, since this has been the intersection of all the variables analyzed. However, much of this has been limited by the inclusion of the interaction variables investment, FDI, ODA and openness to trade[8]. The omission of these variables expands the intersection of the variables left to 174 countries. Perhaps the exclusion of these 52 countries has lead to a bias, due to systematic selection of the countries? I compared the results from fixed effects estimation of total effects with restricted and unrestricted samples: on average the estimated coefficients for the physical integrity index dummies from the unrestricted sample are 0.08 (for AR(1)) and 0.13 (for robust) percentage points higher than the restricted sample. This is an indication that

[8] The total effects estimation was done by omitting these variables, but the sample size was restricted so that the sample size was held constant.

there is a sample bias due to the smaller sample size, though the bias does not change the main structure of the results.

4.8.2 Disaggregated index

Is the result perhaps some property of the physical integrity index itself? This is, as stated earlier, an additive index constructed simply by adding up the indices for political imprisonment, torture, disappearances, and extrajudicial killings. Each of these disaggregated indices has a value between 0 and 2, with the value 0 indicating that violations are frequent in a given year, 1 indicating violations occur occasionally, and 2 indicating violations did not occur at all. Do the various violation types have various effects? Running estimation with the disaggregated indices will examine this.

Table 4.10 Coefficients of disaggregated index

Dependent variable:	(38)	(39)	(40)	(41)
	GDP PPP growth in %			
	OLS panel corrected	Fixed effects AR(1)	OLS panel corrected	Fixed effects AR(1)
Log GDP PPP per capita	0.64048 (2.49)**	2.8622 (4.89)***	0.6554 (2.56)**	2.8589 (4.88)***
Population growth	0.8616 (3.49)***	0.9097 (5.74)***	0.8633 (3.50)***	0.9080 (5.73)***
Disappearances, lagged 3 years	0.1594 (0.88)	0.2439 (1.04)		
Extrajudicial killings, lagged 3 years	-0.3366 (-1.85)*	-0.4726 (-2.20)**		
Political Imprisonment, lagged 3 years	0.1834 (0.92)	0.1803 (0.86)		
Torture lagged 3 years	-0.0944 (-0.54)	0.0849 (0.38)		
Disappearances=1 dummy			0.5553 (1.30)	0.6343 (1.34)
Disappearances=2 dummy			0.4928 (1.20)	0.6426 (1.27)
Extrajudicial killings=1 dummy lagged 3 years			-0.3116 (-0.95)	-0.4920 (-1.32)
Extrajudicial killings=2 dummy			-0.6440 (-1.68)*	-0.9366 (-2.12)**
Political imprisonment=1 dummy lagged 3 years			0.1884 (0.70)	0.4275 (1.35)
Political imprisonment=2 dummy lagged 3 years			0.3581 (0.89)	0.3111 (0.74)
Torture=1 dummy lagged 3 years			-0.1804 (-0.85)	0.0486 (0.18)
Torture=2 dummy lagged 3 years			-0.0982 (-0.26)	0.1759 (0.37)
Investment share	0.1738 (5.49)***	0.2769 (6.88)***	0.1716 (5.41)***	0.2738 (6.79)***
FDI	-3.39e-11 (-0.78)	-7.14e-11 (-1.42)	-3.58e-11 (-0.81)	-7.16e-11 (-1.42)
ODA	7.68e-10 (2.22)	5.74e-10 (1.21)	7.79e-10 (2.25)**	5.85e-10 (1.23)
Openness	-0.0041 (-1.04)	-0.0043 (-0.58)	-0.0041 (-1.03)	-0.0039 (-0.52)
Controls for region, religion, and colonial influence	X		X	
Countries	122	122	122	122
N	2024	2024	2024	2024

Note: z-values in parentheses. * Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level. Estimated coefficients for region, religion and colonial influence omitted for space reasons.

What can be inferred from estimation with the disaggregated index? Above are two sets of regressions, each with the four variables, lagged for three years. The estimation is using OLS with panel corrected standard errors, and with fixed effects, both corrected for autocorrelation. The first regression set has included the four variables as themselves; the second is on the values dummies for values 1 and 2. From the first regression, only the coefficient of extrajudicial killings is significant, while negative, meaning that less extrajudicial killings actually is associated with less growth. Better practices on torture are

estimated by OLS to have a negative effect on growth, while fixed effects estimates them to be positive; however, both are insignificant. Better practices of disappearances and political imprisonment have estimated positive effects on growth, while also insignificant. The second regression set reveals more of the structure of the effect of the indices. The signs of the estimates are all the same, and only the dummy for the value 2 of extrajudicial killings is significant at any level. From OLS, the effects of extrajudicial killings and political imprisonment are estimated to be somewhat linear; the effects of disappearances and torture are estimated to be nonlinear. The fixed effects estimates that the effect of extrajudicial killings is somewhat linear, while the rest are nonlinear.

The insignificance of the estimators is likely due to multicollinearity of the indices, and this is the reason I have used the aggregated index in the primary estimation. But the striking observation here is that while the estimates above are generally insignificant, the estimated coefficients are negative for extrajudicial killings, while positive for disappearances and political imprisonment. If these in fact are real and persistent effects, they could in explain why there appears to be a nonlinear relationship between the physical integrity index and growth: the effects are pulling in different directions!

4.9 What does this mean?

There appears to be a significant nonlinear relationship between physical integrity human rights practices and economic growth in a country, where an initial increase in the physical integrity index is associated with an increase in growth, thereafter being associated with a decrease in growth, before a final step up again. When using the dataset I have available, the estimated effects of the physical integrity index on economic growth per capita are remarkably robust to estimation technique used.

In 2006, 11 countries had scores of 2 on the physical integrity index: Azerbaijan, Brazil, Chad, Côte d'Ivorie, Eritrea, Israel, Nigeria, Russia, Syria, and Venezuela. What does a score of 2 mean? In 36% of the cases it indicates that two types of physical integrity violations happen occasionally and two are violated frequently. In the rest it indicates that one type of physical integrity right is not violated, but the other three are violated frequently. In all cases where the physical integrity index has a score of 2, torture occurs. To put this

into perspective, in Western Europe and Northern America, only 4 country-years have been coded to a value of 4 between 1981 and 2006. Greece in 1983, the United Kingdom in 1991, and the United States in 2004 and 2005. None have been coded to below 4. The average score for Western Europe and Northern America is 7.37. Asia, on the other hand, has been coded to 2 or less in 35% of the observations, and over half of the observations (59%) are coded to be 4 or less. It has an average over the period of 3.75. The Middle East and North Africa have an average of 3.91, with 25% coded to a score of 2 or less. Sub-Saharan Africa has an average of 4.33 and 22% coded to a score of 2 or less. Globally since 1981, getting a score of 2 or less puts a country in the bottom 18% of measurements. This is not an accomplishment a country should be proud of.

5. Conclusion

After a number of different methods of estimating the effects of physical integrity rights on economic growth, it is clear that these are not straightforward. There are strong indications that the effect is not linear or monotonic, and that the effect of an increase in protection depends on the initial level of protection. The results are somewhat comparable to Barro's inverse U-effect of democracy on growth. The initial reduction of repression is associated with increased growth, while a subsequent reduction of repression is associated with decreased growth. In this case though, it is interesting to notice the increase in growth associated with the final reduction of repression. If this structure is a real and persistent effect, an explanation could possibly be similar to the one presented by Barro. Starting from the lowest level of protection, an initial increase in protection of physical integrity rights reduces fear, increases security, and allows a country's inhabitants to be more productive. Subsequent increases in protection levels let the oppressed react to the oppression. This may lead to growth-inhibiting consequences, such as political instability or demands for redistribution. The final increase in physical integrity rights may be caused by a resolution of the growth-inhibiting consequences.

Whether this is in fact driving the results is still an open question. I have argued that the effects of the disaggregated indices are estimated to be working in different directions, and if this is in fact the case, the results may be partially driven by this. It is also possible that the results are driven by some spurious relationship, and that some unseen factor is behind them. Future studies will hopefully give more insight into the effects of physical integrity rights on economic growth. In particular, I believe a few approaches may be rewarding: An investigation into whether the results are a consequence of the CIRI physical integrity index itself, through its construction or measurement. Further analysis into whether the results are driven by different effects of the disaggregated indices would be a part of this; as well as repeating the estimation with another dataset. Broadening the scope of analysis could also be advantageous, combining the various human rights in estimation. Panel data analysis has potential to expand on Blume and Voigt's approach here. An estimation more robust to endogeneity bias would be very welcome as well. Finally, a study measuring the effects of physical integrity rights on a broader measure of human welfare would be justified.

What can I say in regards to the Lee-thesis and physical integrity rights? I cannot reject the Lee-thesis on basis of the results I have found. The effects of physical integrity rights on economic growth are estimated to be non-monotonic. The lowest level of growth was in all estimates associated with the lowest level of protection. The highest level of growth was in all estimates associated with a level of protection which still, while an improvement, yields an objectionable society. While the Lee-thesis cannot be rejected as a result of the findings in this paper, it can be rejected by rejecting the premises of the argument. Human rights are rights which a person is entitled to by the property of being human. No economic analysis will change that.

6. References

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