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Summary

Institutions are a crucial part of our society. This cumulative dissertation includes four empirical essays examining institutions in different settings. It is divided into six separate parts: One general introduction at the beginning, a critical examination at the end, and four essays.

The second part (first essay) deals with the effectiveness of two different aid modalities. Budget support to the health sector and project-type interventions are compared based on their effectiveness. The discourse within the development community suggests that budget support would be more effective in countries with strong institutions. However, the empirical analysis does not present evidence in favor of this claim.

The third part (second essay) analyzes the relationship between economic development, institutions, and education. While many studies investigate the effect of institutions and education on growth, the interplay between these three variables is less researched. When estimating a vector error correction model, the results show a positive and significant effect of institutions and education on growth and vice versa. However, there is no evidence of a significant effect of education and institutions on each other.

The fourth part (third essay) is an extension of the second part. It examines the role of institutions in a neoclassical growth model over 138 years. Many studies do not explicitly control for institutions in growth equations or only investigate a relatively short period. The results suggest that institutions have a significant and positive effect on growth. Thus, the conclusion is that institutions are an important driver of economic development.

Finally, the fifth part (fourth essay) investigates the diffusion of institutions. The essay specifically looks at the spillover of human rights through trade, examining the California effect. The spatial autoregressive model indicates significant and positive spillovers of human rights through trade. Thus, the evidence presented is in favor of the California effect.

Contents

Part 1

General Introduction

Formal and informal institutions are a crucial part of our society. These laws and social norms affect our world and especially its development in many different ways. They "are the rules of the game in a society or, more formally, are the humanly devised constraints that shape human interaction" (North, 1990, p.3).[1]

This cumulative dissertation sheds some light on the importance of institutions in four different scenarios from an empirical point of view. It is a collection of different essays. Even though all parts of this dissertation contain an aspect of institutional economics, the parts are not connected in a strict sense and can be read in any order. Due to its cumulative nature, there is no general list of references at the end of the dissertation but a short list of references at the end of each part. Additionally, no list of tables or list of figures is presented in this dissertation. Again, this is due to the cumulative nature of this dissertation, a collection of single scientific essays. All figures and tables are discussed within the respective essays. In the following, I will briefly introduce the four different essays on institutional economics. This introduction is closely related to the abstracts of each of the essays. Further, footnotes at the beginning of each abstract indicate acknowledgments, a statement on the co-authors, and potentially the related publication.

The article presented in Part 2 examines the relationship of sector budget support to the health sector and the infant mortality rate for developing countries.[2] In the past decades, project-type interventions have been widely used in developing countries. These smaller-scale interventions often did not bring the results the donors intended, at least on

[1] North, Douglass C. (1990). *Institutions, Institutional Change and Economic Performance.* Cambridge: Cambridge University Press; 1990. DOI: 10.1017/CBO9780511808678.

[2] This part has been published in Review of Development Economics: Röthel, Tim (2023). "Budget support to the health sector—The right choice for strong institutions? Evidence from panel data". In: *Review of Development Economics* 27.2, 735–770. DOI: 10.1111/rode.12967.
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a macro level. At the beginning of the millennium, forums on aid effectiveness proposed new principles to increase the effectiveness of aid. Many scholars agreed that one of the answers would be budget support. This article tries to answer whether budget support is the efficient aid modality in countries with strong institutions. In the baseline scenario, a panel data analysis is applied, which includes 113 countries between 2010 and 2018. This dynamic linear panel model is estimated by using OLS and System GMM. Health sector aid, in general, has a significant and negative effect on the infant mortality rate in the average country. Sector budget support is insignificant in the baseline estimation and when interacting with a governance variable. In contrast, project-type interventions exhibit significant and negative effects on the outcome variable. The results indicate that sector budget support might not be the superior choice among the aid modalities in the health sector, even in countries with relatively strong institutions.

Part 3 analyzes the interplay of institutions, education, and economic development.[3] In the past, many articles have examined the effect of institutions and education on economic growth. However, evidence on the impact of growth on institutions and education and the interplay of education and institutions is scarce. Additionally, most of the literature focuses on post-war evidence only. We close a gap by examining the relationship between all three variables over 138 years. By estimating a vector error correction model for a sample of 20 OECD countries, we can differentiate between short- and long-run results. We find positive effects of institutions and education on growth and vice versa. However, institutions and education do not affect each other.

Part 4 is an extension of Part 3, focusing on the role of institutions in a neoclassical growth model.[4] Institutions are an important determinant of long-run economic growth.

[3] I would like to thank Martin Leschke, Felix Schlieszus, Jonathan Bothner, Tim Baule, and the participants of the Radein seminar 2023 for the helpful discussions and suggestions.
This article is a joint work with Martin Leschke. Martin Leschke and I contributed 25% and 75%, respectively. A revised version of this Part is currently under review at Kyklos.

[4] This Part has been published online in Applied Economics Letters: Röthel, Tim and Martin Leschke (2023). "Do institutions matter in the very long-run? - New evidence from OECD countries". In: *Applied Economics Letters.* DOI: 10.1080/13504851.2023.2211333.
I would like to thank Martin Leschke, Felix Schlieszus, Jonathan Bothner, Tim Baule, and the participants of the Radein seminar 2023 for the helpful discussions and suggestions.
This article is joint work with Martin Leschke. Martin Leschke and I contributed 25% and 75%, respectively.

However, most studies only examine this relationship over a relatively short period and do not regularly include institutional variables in growth models. This article applies the pooled mean group (PMG) estimator to a panel of 18 OECD countries over 138 years. We find evidence that institutions matter for economic growth in the very long-run and conclude that economists should include them in any empirical application of the neoclassical growth model. Our result is robust to various robustness checks.

Part 5 examines the diffusion of institutions, more precisely, the extent to which human rights are respected in a country through trade.[5] For decades, a narrative dominating public discourse in many Western countries suggested that trade with countries with a lower level of institutional quality could induce democratic change and good governance. One potential explanation for a change in the institutions in exporting countries is the so-called "California effect". However, developments in countries like Russia or China have led to disappointment regarding this hypothesis. Whether this disappointment is justified or only based on anecdotal evidence remains. In this article, we examine human rights spillovers through trade. We focus on the California effect of human rights in developed importing countries on the standards in developing exporting countries. Our sample includes 173 countries and covers the time period from 2000 to 2019. For four 5-year cross-sections, we estimate a spatial lag model by applying the generalized spatial two-stage least squares estimator using trade flows to construct the relevant weighting matrices. The results indicate positive human rights spillovers through trade and seem to be driven by spillovers from OECD to non-OECD countries, especially through trade in non-resource sectors. Our findings suggest the existence of the California effect and thus provide evidence contrasting the public opinion.

Part 6 provides a short critical examination of this cumulative dissertation and concludes.

[5]I would like to thank Martin Leschke, Felix Schlieszus, Tim Baule, and the participants of the graduate seminar at the University of Bayreuth for the helpful discussions and suggestions.
This article is a joint work with Jonathan Bothner. Jonathan Bothner and I contributed 50% each to this work.

Part 2

Budget Support to the Health Sector - the right choice for strong Institutions? Evidence from panel data

2.1 Introduction

Ever since the first dollar of development aid has been transferred to developing countries, there has been a debate on the effectiveness of aid and the ideal way how it should be delivered. In the early years, there was still a lack of quantitative analyses (Adelman and Chenery, 1966; Baldwin, 1969) and the focus laid on the relationship between aid, savings and investments, inspired by the Harrod-Domar Model (Arndt, Jones, and Tarp, 2010). This changed by the end of the 1990s. Starting with a political debate, dominated by Jeffrey Sachs and William Easterly, the question was if and how aid should be delivered.[1] Then, a large number of cross-country empirical evidence was published. These studies mainly examined the effect of aid on gross domestic product (GDP) growth. The ambiguous results did not solve the political debate either. On the one hand, aid was perceived to positively influence growth (e.g. Burnside and Dollar, 2000; Hansen and Tarp, 2001), and on the other hand, no significant relationship between these two variables could be found (e.g. Easterly, Levine, and Roodman, 2004; Rajan and Subramanian, 2008). This debate has not ended yet. While the meta-study by Mekasha and Tarp (2013) finds an overall significant and positive effect of aid on growth, more recent studies of aid on growth and aid on productivity find ambiguous or even negative effects (e.g. Bird and Choi, 2020; Groß and Nowak–Lehmann Danzinger, 2022).

[1] In "The End of Poverty", Sachs (2005) argues that poor countries often find themselves in a poverty trap, which they can only escape by using vast sums of foreign aid. In contrast, according to Easterly (2006) and Moyo (2010), aid might cause corruption, weak institutions and supports an increasing lobby of aid agencies.

A third strand of the literature focuses on aid on a more disaggregated level. This includes experimental approaches or impact evaluations of single development activities (e.g. Banerjee and Duflo, 2012) and studies researching the effect at the sub-national level. One common approach is to use geocoded aid data and the nightlight density in order to estimate the effect of aid on growth (e.g. Dreher et al., 2021; Khomba and Trew, 2022). Other studies only examine the effect of aid on specific sectors. The idea of these approaches is that the contradicting results regarding aid and GDP growth might be explained by the time it takes until aid flows are translated into economic growth and the complex link between these two variables. Therefore, some authors look at more disaggregated data where potential outcomes are more closely related to the aid flows, and a possible effect can be identified more easily. One example is aid to the health sector. Similarly, some studies use cross-sectional data, while others look at sub-national data only. The advantage of sub-national data and the focus on one country only is that a clear picture of the effect of aid on health outcomes can be drawn. De and Becker (2015), for example, find a positive effect of health-related aid on the decrease of disease severity in Malawi. Kotsadam et al. (2018) look at the effectiveness of aid on the infant mortality rate in Nigeria. They find that a closer proximity to aid projects leads to lower infant mortality in the area. However, the results from these studies might not necessarily be valid in other countries. Thus, it might be interesting to look at panel data studies as well to get a broader view of aid effectiveness in the health sector. Mishra and Newhouse (2009) find a significant effect of health sector aid on infant mortality. Similarly, Yogo and Mallaye (2015) show that a significant relationship between health sector aid and child mortality exists. Recent studies of health aid on the infant mortality rate support these positive effects on the decrease of the outcome variable (e.g. Woode, Mortimer, and Sweeney, 2021). These results regarding health outcomes[2] are in line with the development of several health indicators which show dramatic improvements over the last 30 years in low- and middle-income countries (World Bank, 2021). For example, life expectancy at birth has improved from around 63 years in 1990 to around 71 in 2018. Similarly,

[2]For the sake of completeness, there are certainly some studies which could not find a significant effect of aid to the health sector (e.g. C. R. Williamson, 2008; Wilson, 2011).

the infant mortality rate has fallen from around 71 in 1990 to around 31 in 2019 (World Bank, 2021). However, apart from the previously cited studies, evidence with respect to the effectiveness of health sector aid compared to official development assistance (ODA) is still relatively rare.

Additionally, most researchers look at general aid only. However, in order to improve policy recommendations, it is essential to further distinguish between different kinds of total aid or even different kinds of health sector aid. The Development Assistance Committee (DAC) of the Organisation for Economic Co-operation and Development (OECD) defines eight different modalities on how aid can be delivered (OECD, 2021b)[3], with the most well-known forms of aid being project-type interventions and budget support, which will be further examined in the following.

To the best of the author's knowledge, no cross-section analysis has yet either tested the effectiveness of budget support in the health sector or compared these two types of delivering aid to recipient countries. This distinction might be the answer to the debate of Easterly and Sachs since different institutional settings might simply require different aid modalities.

This is why, in this article, a panel study will be conducted in order to estimate the effect of health sector aid on infant mortality using 113 countries between 2010 and 2018 in the baseline scenario. Similar to the article by Mishra and Newhouse (2009), the starting point will be the aforementioned relationship, to then further disaggregate the aid data into project-type interventions and budget support. Eventually, an interaction term between a governance variable and budget support is used to test if the modality performs any better in the presence of strong institutions.

The main findings of this article are that aid to the health sector does significantly decrease the infant mortality rate. The same applies to project-type interventions where an even stronger effect can be found. Sector budget support has no significant effect on infant mortality for the standard specification. The effect remains the same when budget

[3] The aid types/co-operation modalities are budget support, core contributions and pooled programmes and funds, project-type interventions, experts and other technical assistance, scholarships and student costs in donor countries, debt relief, administrative costs not included elsewhere and other in-donor expenditures (OECD, 2021b).

support is interacted with a governance variable.

The course of this paper is the following: Section 2.2 provides an overview of the two aid modalities, project-type interventions and budget support, and discusses their advantages and disadvantages. In section 2.3, the empirical specification and identification strategies are introduced, and the data is briefly described. Section 2.4 presents the results with respect to the hypotheses formulated and section 2.5 tests their robustness. Section 2.6 summarises the results and concludes.

2.2 Aid Modalities

Project-type interventions, often simply known as project aid, are the most well-known and most widely used aid modality. They are defined as a "set of inputs, activities and outputs (...) to reach specific objectives/outcomes within a defined time frame, with a defined budget and a defined geographical area" (OECD, 2018). In 2019, more than 119 billion US Dollars (USD) in ODA had been transferred to developing countries. Project-type interventions accounted by far for the largest fraction with a volume of around USD 62 billion and a share of around 52 per cent (OECD, 2021a). Apart from many advantages[4] and positive evaluation results[5], there has been a debate about the Micro-Macro paradox (Mosley, 1986) and criticism towards the modality itself. The main problems are that projects are often supply-driven, usually involve high transaction costs, undermine the local administrative capacity and establish parallel systems for aid resources (Leiderer, 2012).

At the beginning of the new millennium, international summits took place in Rome and Paris to tackle these problems and increase aid effectiveness. There, the international community agreed on five fundamental principles: Ownership, Alignment, Harmonisa-

[4]For project-type interventions, the donor can largely influence the goal of the intervention as well as the actions taken. Since the projects are often conducted by western project implementation agencies, preferences between the donor and the agency are potentially largely aligned while the danger of defalcation or corruption is potentially low.

[5]To give only one example, the Deutsche Gesellschaft für Internationale Zusammenarbeit (GIZ) presents the evaluation results and milestones of their projects conducted in the name of the German government and other donors on their website. With respect to medical services, 3.889 health facilities have been improved, and 73.2 million people have gained access to better health services (GIZ, 2021).

tion, Managing for results and Mutual accountability. These principles were meant to mitigate the problems of past development cooperation. However, neither the Rome nor the Paris Declaration contains a clear recommendation which aid modality should be used in order to achieve the goals set, though both implicitly urge for an expansion of budget support (OECD, 2003; OECD, 2006).

Aid flows are labelled as budget support if ODA is directly transferred to the recipient's budget and the donor relinquishes a part of the exclusive control over its funds.[6] Two subcategories of budget support exist: general budget support and sector budget support. While the funds are unearmarked with general budget support, sector budget support is narrower. Here, donors and recipients intend to achieve specific targets in different sectors, e.g. health, education, etc. (OECD, 2018). Koeberle and Stavreski (2012) provide an overview of the characteristics of budget support. Funds are channelled through the recipient country's budgetary process and provided at regular intervals, aligned with the respective budget cycle. The aid flows are intended to support the recipient country's development program, for example, specific actions to improve the healthcare system. Additionally, performance assessments and an accountability framework are implemented by the recipient. Before budget support is provided, the donor and the recipient agree on general priorities of government spending.

Koeberle and Stavreski (2012) argue that eventually, budget support will strengthen country ownership and lead to more sustainability. Budget support lowers transaction costs for the recipient country and increases transparency and efficiency in contrast to traditional development aid approaches. Further advantages are the implementation of government-wide policies, increasing accountability to the home country and the recipient, and support of the recipient's budgetary process. However, even though the transaction costs of the implementation decrease with budget support, they could also rise, at least in the short term. This is due to the fact that aid flows are fungible, and the donor cannot observe if the contributions are used productively or if they end up in unintended channels due to rent-seeking or corruption. Other concerns are that the institutions in the recipient

[6] In this article, the definition of budget support by the OECD DAC is used since the data originates from the same resource. However, other definitions which are presented by Koeberle and Stavreski (2012) exist.

countries might not be ready to handle large sums of aid, leading to an inefficient use.
Budget support has a much smaller volume with around USD 4 billion in 2019 and a share of 3 per cent of overall aid. When distinguishing between general budget support and sector budget support, sector budget support accounts for approximately two-thirds of overall budget support (OECD, 2021a).
Since the formal introduction of budget support at the beginning of the 2000s (Koeberle and Stavreski, 2012), the literature provided mainly theoretical essays and evaluations with only a few cross-country analyses. In the theoretical literature, Leiderer (2012) highlights the importance of harmonisation and coordination between the donors, which could be achieved by budget support, while Cordella and Dell'Ariccia (2007) argue that budget support is preferable to project aid if the preferences of the donor and the recipient are aligned. Country case studies mainly discuss the aspect of conditionality and find, in contrast to the theory, that recipient countries often cannot freely decide how the money is spent. Some donors rather use the modality as leverage over the recipient government with respect to reforms and the direction of the development program (e.g. Faust, Leiderer, and Schmitt, 2012; Swedlund, 2013). This view is supported by Swedlund and Lierl (2020) claiming that budget support did not increase ownership of the recipient countries. Apart from these negative findings, Orth et al. (2017) conduct a systematic review regarding the effectiveness of budget support. They find that budget support positively affects the harmonisation between donors and recipients, reduces transaction costs on the recipient side and increases public spending. However, there is still a lack of evidence in other areas, for example, with respect to corruption. Dijkstra (2018) reviews several recent country evaluations and draws a rather positive picture of budget support, especially with respect to the reduction of poverty.
Fernandes Antunes et al. (2013) conduct a panel study on the effect of general budget support on health spending in lower- and middle-income countries. There, no significant effect for general budget support could be found. Other studies compare project aid and programme aid with respect to aid volatility, like the one by Fielding and Mavrotas (2005). T. Williamson and Dom (2010) focus on sector budget support, in particular. Based on several country case studies, they find that sector budget support leads to an expansion

of service delivery, quality and efficiency.

In general, the effectiveness of budget support should be higher if the recipient country has implemented a sound institutional framework.[7] The modality is seen as especially suitable for countries with high ownership, strong commitment and the capability to allocate the resources effectively (Koeberle and Stavreski, 2012). The literature on good governance suggests similar results, where aid is typically seen to be more effective in such environments (e.g. World Bank, 1992; Acemoglu, Johnson, and Robinson, 2004). One apparent reason is that transaction costs for donors are lower since the probability of misuse or allocation into the wrong channels decreases. Recent contributions confirm these results. Doucouliagos, Hennessy, and Mallick (2021) show that health aid significantly reduces infant mortality in the presence of good governance. Similarly, Langnel and Buracom (2020) argue that health expenditure seems to be more effective in countries with a higher administrative capacity and Tarverdi and Rammohan (2017) find that higher levels of governance lead to lower levels of under five mortality. However, some authors question the importance of democracy or good governance. Olson (2001) points out that an authoritarian leader may have the incentive to provide some public good in order to increase his personal revenues from taxes. This might be the case for the health sector. Similarly, Prahalad (2006) argues that bureaucrats might have the incentive to let business flourish in some sectors – even though they are corrupt or authoritarian – if they benefit from it.

2.3 Empirical Framework

As mentioned above, this paper examines the effectiveness of sector budget support to the health sector. Natural candidates for outcome variables would be the life expectancy, mortality rates or the incidence of a disease. While the life expectancy at birth is based on death probabilities, the incidence of a disease might change quickly but not sustainably in the presence of a shipment of drugs. Additionally, global, reliable data on the incidence of diseases is scarcely available. Compared to the previous examples, mortality rates

[7]The World Bank (1992) argues that sound economic and political institutions are the basis of good governance. Therefore, this article will use strong institutions and good governance as synonyms.

are often based on surveys and not probabilities and are widely available from relatively reliable sources. This is why infant mortality is often found as a dependent variable in the literature (e.g. Boone, 1996; Mishra and Newhouse, 2009). Further, infant mortality might also be favourable over child mortality as results can be achieved faster. Boone (1996) argues that infant mortality is a flash indicator of the living conditions of the poor since it quickly reacts to their economic status and access to health services.

In this article, the distinction between sector budget support and project-type interventions[8] is of particular interest. The OECD provides data regarding these aid types only from the beginning of the millennium. Therefore, the panel consists of data from 113 countries between 2010 and 2018. The first identification strategy employed is the pooled ordinary least squares (OLS) estimator. This can be seen as the usual starting point for panel data analysis. The regression equation to be estimated is the following:

$$logIM_{it} = \alpha logIM_{i,t-1} + \beta logAid_{it} + \gamma logX_{it} + v_t + \epsilon_{it} \tag{2.1}$$

IM_{it} denotes the infant mortality rate per 1000 live births, Aid_{it} is the volume of health sector aid per capita and X_{it} is a vector of control variables in country i at time t. The control variables consist of the size of the population, the fertility rate, the real GDP per capita, the regulatory quality, the HIV rate and the share of the urban population. The model also includes $IM_{i,t-1}$, which is the lagged infant mortality rate. Thereby, the model is a dynamic panel model since the lagged dependent variable is included. This is due to the fact that infant mortality is believed to be largely influenced by its past levels. Summary statistics of the variables and an overview of the countries included in the sample can be found in the Appendix in Tables A2.1 and A2.2, respectively. Finally, v_t denotes a vector of time dummies and ϵ_{it} the error term.

Following Nickell (1981), one would expect that the error term is composed of three parts: one country-specific, one time-specific and one time- and country-specific component. In equation (2.1), the time-specific effects are already drawn out of the error term, though the exogenous regressors are likely correlated with the country-specific effect. Therefore,

[8] In order to keep things simple, project-type interventions and project aid will be used as synonyms in the following. The same applies to sector budget support and budget aid.

typically, if country fixed effects are introduced, the fixed effects estimator is used in order to control for this bias. In dynamic environments, however, dynamic panel biases can arise since the lagged dependent variable may be correlated with the fixed effect, especially in a small T environment. The within transformation of the fixed effects estimator does not solve this problem. Roodman (2009b) discusses two solutions of this so-called "dynamic panel bias". The first one is to purge the fixed effects by first-differencing the data. Though, this does not completely solve the problem since predetermined variables might still be correlated with the differenced time- and country-specific error term. One possible solution is to instrument the differences with lagged levels of the regressors, as proposed by Anderson and Hsiao (1982) for two-stage least squares and by Arellano and Bond (1991) for the generalised method of moments (GMM) framework. To increase efficiency, especially for random walk-like variables, Blundell and Bond (1998) introduce the system GMM estimator. There, additionally, to the proposition by Arellano and Bond (1991), a second equation is estimated. Instead of instrumenting the differences with lagged levels, levels are instrumented with lagged differences.[9] This system of equation approach will be used here, in addition to the OLS estimator.

$$logIM_{it} = \alpha\ logIM_{i,t-1} + \beta\ logAid_{it} + \gamma\ logX_{it} + v_t + s_i + \epsilon_{it} \tag{2.2}$$

$$logIM_{it} = \alpha(\Delta logIM_{i,t-1}) + \beta(\Delta logAid_{it}) + \gamma(\Delta logX_{it}) + \Delta v_t + \Delta\epsilon_{it} \tag{2.3}$$

Equation (2.2) shows the equation for the first-differenced case, where s_i is the fixed effect and levels are instrumented by their respective lagged differences. Equation (2.3) is the differenced equation where levels are used as instruments. The lagged dependent variable

[9] In equation 2.2, country specific values (levels) are instrumented by lagged differences of these country specific values. In equation 2.3, country specific first differences are instrumented by country specific lagged values (levels).

is treated as predetermined, while the aid variables are treated as endogenous leading to the following orthogonality assumptions (Blundell and Bond, 1998):

$$E(s_i) = 0,\ E(\epsilon_{it}) = 0,\ E(s_i\epsilon_{it}) = 0 \tag{2.4}$$

$$E(IM_{i,t-r}\Delta\epsilon_{it}) = 0 \quad \text{for} \quad t = 2, ..., T \quad \text{and} \quad 3 \leq r \leq T-1 \tag{2.5}$$

$$E(Aid_{i,t-r}\Delta\epsilon_{it}) = 0 \quad \text{for} \quad t = 2, ..., T \quad \text{and} \quad 3 \leq r \leq T-1 \tag{2.6}$$

$$E(\Delta IM_{i,t-1}(s_i + \epsilon_{it})) = 0 \quad \text{for} \quad 2 < t \leq T \tag{2.7}$$

$$E(\Delta Aid_{i,t-1}(s_i + \epsilon_{it})) = 0 \quad \text{for} \quad 2 < t \leq T \tag{2.8}$$

The assumptions regarding the error term of the system GMM estimator are presented in equation (2.4). The other equations (2.5 to 2.8) are the initial conditions for the variables of interest, where Aid is treated as endogenous (Baltagi, 2010) and r denotes the number of lags. The estimation is conducted by using the xtabond2 command presented in Roodman (2009b). Following Roodman (2009a) and in order to avoid a finite sample bias, the amount of instruments is restricted to four lags. Two concerns remain. First, the problem of weak internal instruments, which is extensively discussed in Kraay (2015) for the case of the system GMM estimator. Second, similar to other estimation techniques including a fixed effect, the system GMM estimator cannot fully capture cross-country heterogeneity (Wooldridge, 2016).

The data for the dependent variable and for most independent variables originates from the World Bank's database (World Bank, 2021). Even though the data from the World Bank is used in most cross-country empirical studies with respect to population parameters, it is still prone to errors. Since many countries lack complete registration systems for the specific variables, the indicators are often based on sample surveys or are derived by indirect estimation techniques. Errors in the data collection process can therefore not be completely ruled out (World Bank, 2021). The aid data originates from the OECD DAC's creditor reporting system (CRS), which provides data on net ODA. The CRS includes deflated project level disbursements by recipient country, aid type and sector. Therefore, the aid flows can be disaggregated into sector budget support and project-type interventions and filtered by the health sector (OECD, 2021a). The CRS provides data on eight different sectors to which aid may be disbursed. The codes of interest for the health sector are 120 (Health), 121 (Health, General), 122 (Basic Health) and 123 (Non-communicable diseases). With respect to the aid modalities, the aid types A02 (Sector budget support) and C01 (Project-type interventions) are included (OECD, 2021b). Two limitations of the CRS data remain. First, only ODA data is available, ignoring other aid flows, for example, those from south-south cooperation. Second, even though the data is revised several times per year, the project-level data might still be subject to measurement error or underreporting (Mishra and Newhouse, 2009; OECD, 2021c). Eventually, the governance variable is taken from the worldwide governance indicators (WGI) which are reported in units of a standard normal distribution.[10] In the main specifications, regulatory quality is taken as the governance variable. According to Kaufmann and Kraay (2021), this variable, together with government effectiveness from the WGI, measures the government's ability to formulate and implement sound policies. As indicated in section 2.2, the government's administrative capability and capacities might be critical for the effectiveness of budget support. In a robustness test, government effectiveness and other governance variables are introduced as well.

The overall goal of this work is to identify the relationship between different modalities of health sector aid and infant mortality. Three hypotheses can be derived from the literature discussed above and tested using equations (2.1), (2.2) and (2.3).

Hypothesis 1: *Health aid has a beneficial effect on infant mortality.*

Hypothesis 2: *Sector budget support has a beneficial effect on infant mortality.*

Hypothesis 3: *Sector budget support, in presence of good governance, has a beneficial effect on infant mortality.*

The three hypotheses presented above are all alternative hypotheses. One would assume that the aid modalities negatively affect the infant mortality rate. However, since the word "negative" might be misinterpreted, the term beneficial is used. It is expected that

[10]The aid variables include many zeroes. Since the per capita aid values for the two variables are relatively low and often between 0 and 1, leading to negative values, the results may be biased. Therefore, the three variables have been linearly transformed by 1+x before taking the natural logarithm. This creates a zero lower bound for the aid terms. Similarly, the regulatory quality variable has been transformed.

aid is indeed effective, and a significant relationship can be found when looking at disaggregated data for the health sector. Therefore, hypothesis 1 is tested against the null hypothesis that health aid has no effect. Following section 2.2, if one does not control for the institutions implemented in a country, sector budget support is believed to have no significant effect on infant mortality due to rent-seeking, corruption and bureaucratic difficulties. Hypothesis 2 will be tested against the null hypothesis that sector budget support has no effect. However, if one controls for the institutional framework in a country, it is expected that sector budget support has a significant effect in countries with strong institutions. This is due to the fact that these countries might be able to handle the resource flows relatively better. Hypothesis 3 will be tested against the null hypothesis that sector budget support in the presence of good governance has no effect on infant mortality.

2.4 Results

Table 2.1 shows the baseline results of the OLS and the system GMM estimations with respect to hypothesis 1. The first three columns present the effect of health aid on infant mortality, excluding control variables, followed by three columns including regulatory quality and the last two columns including additional control variables. Table A2.8 in the Appendix shows the full results of the last two columns including control variables. For all eight specifications, we find a significant and negative effect of aid to the health sector on the infant mortality rate at least at the 5%-level. The coefficient estimated by System GMM is larger in absolute terms compared to the one estimated by OLS. In column 9, for example, the health aid variable is significant at the 5%-level with a coefficient of around -0.0366. This means that a one per cent increase in health aid reduces the infant mortality rate in the average country by around 0.0366%. The 95% confidence interval for the health aid coefficient is below zero for all estimations, ranging between -0.0658 and -0.0074 for the collapsed system GMM specification, including control variables. This indicates a beneficial effect of aid to the health sector. In contrast, the regulatory quality variable is insignificant for all specifications except for one where it is even positively

Table 2.1: Health aid and infant mortality

	(1) OLS	(2) SysGMM	(3) SysGMMCL	(4) OLS	(5) SysGMM	(6) SysGMMCL	(7) OLS	(8) SysGMMCL
	Dependent variable is *Log infant mortality*							
Lagged log infant mortality	1.0093***	1.0548***	1.0798***	1.0085***	1.0505***	1.0689***	1.0126***	1.0903***
	(0.0013)	(0.0118)	(0.0189)	(0.0015)	(0.0112)	(0.0245)	(0.0024)	(0.0346)
Log health aid	-0.0051***	-0.0337***	-0.0486***	-0.0049***	-0.0330***	-0.0469***	-0.0060***	-0.0366**
	(0.0014)	(0.0076)	(0.0169)	(0.0013)	(0.0079)	(0.0154)	(0.0015)	(0.0147)
Log regulatory quality				-0.0045	-0.0121	-0.0210	0.0069*	-0.0099
				(0.0057)	(0.0238)	(0.0585)	(0.0038)	(0.0398)
Controls	NO	NO	NO	NO	NO	NO	YES	YES
Time fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
R-squared	0.999			0.999			0.999	
Number of observations	897	897	897	897	897	897	865	865
Number of countries		113	113		113	113		110
Number of instruments		66	18		95	23		40
Hansen test: P-value		0.291	0.443		0.449	0.788		0.637
AR(1) test: P-value		0.004	0.007		0.004	0.005		0.008
AR(2) test: P-value		0.528	0.676		0.510	0.641		0.839

Notes: Standard errors are denoted in parentheses. The pooled OLS regressions use robust standard errors clustered by country. The system GMM regressions use the twostep Windmeijer (2005) finite sample correction for standard errors. SysGMMCL denotes the system GMM estimaor with collapsed instruments. All variables of interest are treated as predetermined. *, ** and *** denote significance at the 10%-, 5%- and 1%-level.

Table 2.2: Project aid, budget support and infant mortality

	(1) OLS	(2) SysGMM	(3) SysGMMCL	(4) OLS	(5) SysGMM	(6) SysGMMCL	(7) OLS	(8) SysGMMCL
	Dependent variable is *Log infant mortality*							
Lagged log infant mortality	1.0091***	1.0473***	1.0818***	1.0083***	1.0420***	1.0561***	1.0122***	1.0749***
	(0.0013)	(0.0103)	(0.0199)	(0.0015)	(0.0090)	(0.0341)	(0.0024)	(0.0330)
Log project aid	-0.0050***	-0.0165*	-0.0419**	-0.0048***	-0.0182**	-0.0394**	-0.0056***	-0.0276**
	(0.0015)	(0.0086)	(0.0162)	(0.0014)	(0.0085)	(0.0151)	(0.0015)	(0.0121)
Log budget support	-0.0015	0.0005	-0.0014	-0.0010	0.0026	0.0033	-0.0006	-0.0032
	(0.0019)	(0.0084)	(0.0129)	(0.0020)	(0.0074)	(0.0129)	(0.0018)	(0.0110)
Log regulatory quality				-0.0048	-0.0143	-0.0940	0.0061	-0.0297
				(0.0059)	(0.0250)	(0.1141)	(0.0039)	(0.0323)
Controls	NO	NO	NO	NO	NO	NO	YES	YES
Time fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
R-squared	0.999			0.999			0.999	
Number of observations	897	897	897	897	897	897	865	865
Number of countries		113	113		113	113		110
Number of instruments		95	23		124	28		45
Hansen test: P-value		0.488	0.877		0.767	0.990		0.882
AR(1) test: P-value		0.022	0.011		0.018	0.011		0.009
AR(2) test: P-value		0.482	0.672		0.470	0.598		0.873

Notes: Standard errors are denoted in parentheses. The pooled OLS regressions use robust standard errors clustered by country. The system GMM regressions use the twostep Windmeijer (2005) finite sample correction for standard errors. SysGMMCL denotes the system GMM estimaor with collapsed instruments. All variables of interest are treated as predetermined. *, ** and *** denote significance at the 10%-, 5%- and 1%-level.

significant at the 10%-level. The reason might be that the lagged dependent variable already captures the long-term influence of regulatory quality on infant mortality while no short term effect can be found.
In the GMM cases, Hansen's test for overidentifying restriction is conducted, which cannot be rejected at the 10%-level. In order to test for serial correlation, the Arellano and Bond test is included as well. While no serial correlation, as expected, can be rejected at lag one at the 1%-level, the AR(2) cannot be rejected.
The coefficient of the lagged dependent variable is slightly above one in all regressions, indicating that infant mortality exhibits an accelerating decrease. A value above one also means that infant mortality does not converge to its long-run equilibrium. Similar results have already been found by Mishra and Newhouse (2009).
In section 2.3, the first null hypothesis (H_0) was that health aid has no effect on infant mortality. This hypothesis can clearly be rejected for all specifications. Though, the effect of health sector aid on the infant mortality rate is relatively small. Between 2010 and 2018, the average country in the sample received around USD 83 million in health aid in total (USD 4.65 per capita) and had an infant mortality rate of around 33. As an example, if the volume of aid in this average country were doubled, the infant mortality rate would decrease by around 1.20 deaths per 1000 live births.
In the second hypothesis, two types of aid to the health sector are distinguished. Therefore, equation 2.1, 2.2 and 2.3 have to be adjusted. The term $\beta logAid_{it}$ will be split into $\beta_1 logProject_{it}$ and $\beta_2 logBudget_{it}$, with all else being equal. Table 2.2 presents the results with respect to the second hypothesis. The first three columns present the effect of project aid and budget support on infant mortality, excluding control variables, followed by three columns including regulatory quality and the last two columns including additional control variables. Table A2.9 in the Appendix shows the full results of the last two columns including control variables. For all eight specifications, we find a significant and negative effect of project aid to the health sector on the infant mortality rate. Again, the effect is stronger in the system GMM cases compared to the OLS cases. Contrary, no significant effect can be found when looking at budget support and regulatory quality. Therefore, the second null hypothesis (H_0) of section 2.3, that sector budget support has

Table 2.3: Interaction term of budget support and governance

	(1) OLS	(2) SysGMMCL	(3) OLS	(4) SysGMMCL
Dependent variable is *Log infant mortality*				
Lagged log infant mortality	1.0082***	1.0555***	1.0122***	1.0698***
	(0.0015)	(0.0323)	(0.0024)	(0.0275)
Log project aid	-0.0048***	-0.0438***	-0.0056***	-0.0303**
	(0.0014)	(0.0137)	(0.0015)	(0.0123)
Log budget support	0.0021	0.0105	0.0157	0.0327
	(0.0129)	(0.0343)	(0.0124)	(0.0347)
Log regulatory quality	-0.0047	-0.0530	0.0068*	-0.0230
	(0.0060)	(0.1002)	(0.0040)	(0.0218)
Log budget support * Log RQ	-0.0027	-0.0128	-0.0139	-0.0316
	(0.0113)	(0.0276)	(0.0108)	(0.0263)
Controls	NO	NO	YES	YES
Time fixed effects	YES	YES	YES	YES
R-squared	0.999		0.999	
Number of observations	897	897	865	865
Number of countries		113		110
Number of instruments		33		50
Hansen test: P-value		0.984		0.911
AR(1) test: P-value		0.006		0.008
AR(2) test: P-value		0.636		0.820

Notes: Standard errors are denoted in parentheses. The pooled OLS regressions use robust standard errors clustered by country. The system GMM regressions use the twostep Windmeijer (2005) finite sample correction for standard errors. SysGMMCL denotes the system GMM estimaor with collapsed instruments. All variables of interest are treated as predetermined. *, ** and *** denote significance at the 10%-, 5%- and 1%-level.

no effect on infant mortality, cannot be rejected for all specifications.

Eventually, the third hypothesis is tested using a proxy for the level of governance in the countries in the sample. As previously noted, this variable is taken from the WGI (Kaufmann and Kraay, 2021) and represents the level of regulatory quality in a country. According to Kaufmann, Kraay, and Mastruzzi (2011), the variable describes the ability of a government to formulate and implement sound policies and regulations. As in the previous two cases, the variable enters the model as a continuous variable. Table 2.3 presents the estimation results, with the OLS results in columns one and three and the system GMM results in columns two and four. Table A2.10 in the Appendix shows the full results of the last two columns including control variables. Again, all previously used control variables are included in the last two columns. The project aid variable again has a negative effect which is significant at least on the 5%-level for all specifications. The magnitude of the effect is similar to the one found in table 2.2. Turning to the budget support variable, it is insignificant for all specifications. Similarly, regulatory quality and the interaction term of budget support and regulatory quality are insignificant. However, the interaction term cannot be interpreted at first glance. For continuous to continuous interactions, it is possible to evaluate the effect of budget support on the infant mortality rate for different levels of regulatory quality. The grey line in figure 2.1 shows the average marginal effect and the grey-shaded area the respective 90% confidence interval. For all levels of regulatory quality, the marginal effect of budget support is insignificant. Thus, the third null hypothesis (H_0) of section 2.3, that sector budget support in the presence of good governance has no effect, cannot be rejected for all specifications.

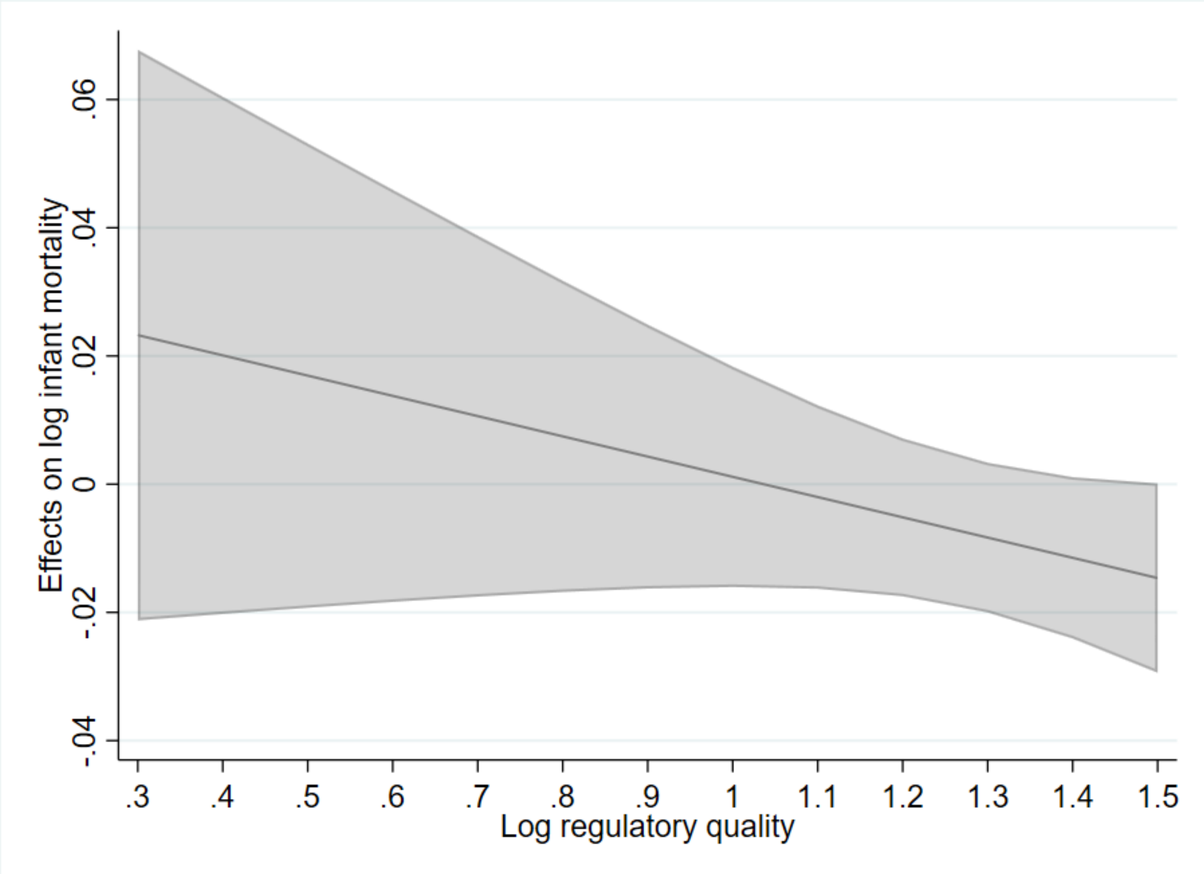

Figure 2.1: Marginal effects for the budget support interaction term

Overall, the results indicate that health aid has a robust and significant impact on the infant mortality rate. Aid directed to the health sector seems to be an effective way to improve health indicators. Similar results were found for project-type interventions. Since they account for a large fraction of the general health aid term, the results of health aid might be actually driven by project aid. Contrary, budget support seems to be ineffective. Several reasons might be the cause that no effect can be found. First, there is a debate in the literature on whether some countries are unable to handle such large amounts of money due to a low absorptive capacity, for example, leading to a dutch disease effect (e.g. Clemens and Radelet, 2003; Renzio, 2005). If budget support is mainly given to such countries, it may explain why no aggregate effect can be found. Second, since money is fungible, this kind of aid might be used for different purposes and not exclusively for improving health-related issues. As the literature on aid fungibility points out, aid might still be effective, even though it is fungible. For example, if aid were directed to any other non-health-related sector, it could still positively affect infant mortality through spillover effects, e.g. of education on health (Pettersson, 2007; Rana and D.-J. Koch, 2020). However, this is out of the scope of this article and might be subject to further research. Third, budget support might be effective but on a rather long time horizon which cannot be measured in this context due to the data availability. This may be due to the fact that budget support rather targets sector- or country-wide purposes, which take longer to translate into a measurable effect (S. Koch et al., 2017). In contrast, project-type interventions might provide drugs or vaccines which would have an immediate and positive effect on the infant mortality rate. Fourth, at the beginning of this article, the Rome and Paris Declaration on aid effectiveness were briefly mentioned. There, and on the following two high-level forums in Accra and Busan, the international community agreed on several principles to produce better impacts of aid (OECD, 2011). As mentioned above, these principles were often interpreted as a call for more budget support which would have many advantages over other aid modalities. However, Lundsgaarde and Engberg-Pedersen (2019), Keijzer, Klingebiel, and Scholtes (2020), and others find that donors did not always adhere to these principles, which led, instead of the expected decrease, to an increase in transaction cost. Additionally, they find that some donors started to suspend budget support in response to a rise in corruption. This non-compliance with the originally formulated principles might be another reason for the insignificance of budget support. Lastly, with respect to the effectiveness of budget support, there is still lack of evidence for many areas (Orth et al., 2017). Thus, budget support might just be ineffective in the case of the health sector and not affected by institutional factors. The following section will discuss some of these aspects.

2.5 Additional Tests and Robustness

The main results were all estimated with the assumption that there are no differences in the effectiveness of aid across different income groups. However, the aid effectiveness likely differs for countries with different starting points in income. Rahnama, Fawaz, and Gittings (2017) find that aid has a positive effect on economic growth in high-income developing countries and a negative effect in low-income developing countries. Similarly, Azam and Feng (2022) show that aid positively affects aggregate growth. However, when disaggregating the income groups, the effect seems to be driven by lower-middle-income countries. Thus, the sample has been split into three groups as classified by the World Bank (2022): low-income, lower-middle-income and upper-middle-income.[11] The results are presented in table A2.3 in the Appendix. Health aid remains significant and negative across all income groups. The results for project aid seem to be driven by lower-middle-income and upper-middle-income countries. For low-income countries, the coefficient is insignificant, while the results remain almost the same for the other two income groups. The split into different groups does not affect sector budget support and the interaction term. Here, the coefficient and joint coefficient remain insignificant. In general, the results seem to be robust to splitting the sample into different income groups and support the previous findings in the literature.

In section 2.4, a continuous variable has been used to test hypothesis 3. However, governance variables typically only change slowly over time. This might be the reason why no significant effect could be found for the continuous interaction term. In order to avoid putting too much weight on small changes in the indicator, the variable may also be split at the sample median. Thus, the same regression was repeated using a dummy variable

[11] Six countries were classified as high-income-countries for some periods and another five throughout all periods. Due to the small number, and since these countries are only slightly richer than upper-middle-income countries, they were also included in this group.

interaction approach. Regulatory quality will be converted and takes a value of one if it is above the sample median and zero otherwise. Table A2.4 in the Appendix shows the results. A t-test for the linear combinations of the interaction term and the budget support variable confirms the previous results. Again, no significant effect for the interaction term between budget support and the regulatory quality dummy can be found.

One could also argue that the problem is the institutional variable itself. Thus, as a robustness check, two other variables of the WGI are used to measure institutional quality: corruption and government effectiveness. As previously argued, these two might also be critical mediating factors. However, it is well known in the literature that the WGI, apart from many advantages, also suffer from disadvantages. Most criticism concerns biases in the data collection as well as the comparability of the indicators. Thomas (2010) provides an overview of the most common criticism regarding the WGI. Thus, the Polity5 variable from the Polity project was used as a third variable to test the robustness of the results of the interaction (Center for Systemic Peace, 2020). The results are displayed in table A2.5 in the Appendix and confirm those already found for the regulatory quality variable in most instances. For all three governance variables, the interaction term in the system GMM specification is insignificant for all levels of the respective governance variable. The joint marginal effects for the interaction term of sector budget support at different levels of the three governance variables are depicted in figure A2.6, A2.7 and A2.8 in the Appendix. Thus, the system GMM results confirm those of section 2.4.

Following the insignificant influence of the governance variables on budget support, one may argue that the institutional framework does not matter for the effectiveness of health aid in the recipient country. In order to test this argument, health aid and project aid were also interacted with the regulatory quality variable. Table A2.11 in the Appendix shows the results for the health aid variable, regulatory quality and a continuous to continuous interaction term. Again, due to the continuous nature of both variables, the result cannot be interpreted at first glance. Thus, the effect of health aid and project aid is evaluated at different levels of regulatory quality. The results for the system GMM estimation with collapsed instruments are depicted in figure A2.1 and A2.2 in the Appendix. Again, the grey line shows the average marginal effect and the grey-shaded area the respective 90%

confidence interval. Overall, a significant and negative effect at least at the 10%-level can be found for all levels of regulatory quality except for project aid for countries with low levels of regulatory quality. As depicted in figure A2.3 in the Appendix, only a small fraction of aid is directed to countries with these low levels of regulatory quality. Both interactions indicate that institutions matter for the effectiveness of aid, however, in a counterintuitive way. In countries with lower levels of governance, aid to the health sector seems to be slightly more effective. The reason might be that good governance leads to a higher provision of public goods (Weiss, 2000) and it directly influences the effectiveness of health spending (Farag et al., 2013). Thus, in countries with higher levels of governance, the quality of (health-related) public goods is relatively higher. Development cooperation might then have a smaller impact compared to countries with a lower provision of public goods.

Another conspicuousness is that both, health aid and project aid, are insignificant for lower and very high levels of governance. The reason might be that a large fraction of countries in the sample exhibit levels of governance between 0.9 and 1.3. Within this range, both aid terms are significant. One explanation might be a more precise estimation since more countries are included. Generally, this finding may explain why the interaction term is insignificant for some boundary points. Though it is likely not the reason for the superior performance of project aid compared to budget support in this sample.

Another criticism that may arise is that the allocation of budget support might be determined by other factors than that of project aid. One argument would be that budget support is only given to countries with a strong institutional framework, while project aid is given to those countries with a weak framework. This argument can be enfeebled when looking at figure A2.3 in the Appendix. There, the volume of log project aid (light grey) and log budget support (dark grey) is shown for different levels of log regulatory quality. It can be seen that budget support is directed to similar countries as project aid. Further, the volume is higher for those countries receiving relatively more project aid. The only difference is that project aid is also given to countries with very high and very low levels of regulatory quality. This is not the case for budget support. Additionally, one could argue that the use of budget support or project aid depends on the country's health situation or wealth. A similar pattern can be found when looking at the distribution of both modalities over the infant mortality rate and GDP per capita (Figure A2.4 and figure A2.5 in the Appendix). Again, the volume of project aid is much higher, but the distribution of the two modalities is rather similar. However, if budget support is actually directed just to countries with specific characteristics, this could lead to an endogeneity problem. Fortunately, most variables which are likely used to make a decision if a country receives a specific aid modality can be observed and, thus, be included in the equation. The German federal ministry for economic cooperation and development (BMZ), for example, bases its decision for budget support upon three criteria. First, budget support is predominantly given to countries with high levels of good governance. Second, the fiduciary risks are assessed, and third, the macroeconomic stability of the recipient country is analysed (BMZ, 2008). Since the regulatory quality variable and other governance variables are included in the regression, different levels of good governance are controlled for. Additionally, fiduciary risks are already included or likely correlated with the governance variable. The same is likely true for macroeconomic stability. In order to validate these assumptions, an additional regression will be run with respect to hypothesis two. There, the absolute value of the annual change in consumer prices will be included as a proxy for macroeconomic stability as well as corruption as a proxy for fiduciary risks. The former variable is taken from the World Bank (2021) and the latter from the WGI (Kaufmann and Kraay, 2021). Columns one and two in table A2.6 in the Appendix present the results for the OLS and the system GMM cases. When looking at the coefficients of project aid and budget support, one can see that the results did not change considerably. Table A2.6 also presents the results of additional robustness tests. The access to basic drinking water services and the health expenditure per capita might affect the infant mortality rate through different channels. Thus, both variables were included in columns three and four. Further, column five shows the OLS results for the inclusion of regional fixed effects. All extensions do not change the results. Project aid remains significant at least at the 5%-level.

Eventually, as in most previous studies (e.g. Mishra and Newhouse, 2009), the initial equations are estimated using averages over time. This is done in order to smooth any potential fluctuations of aid over time, which might affect the coefficients and especially the significance levels. The problem in this particular setting is that the panel is relatively short due to the previously described limited data availability, so the variables can only be averaged over relatively short periods. Table A2.7 in the Appendix presents two-year and three-year averages of the main results, including the previously described control variables. The OLS estimates for the two-year averages in the first two columns remain similar to the main results. The significance level drops for the three-year average estimation in columns 3 and 4. Health aid is significant and negative at the 10%-level, while project aid is insignificant. However, in this scenario, the number of observations drops significantly due to the averaging of the data. As a result, the standard errors become roughly twice as large in magnitude. At the same time, the coefficients remain relatively constant in magnitude.

2.6 Conclusion

To answer the question whether sector budget support is the right choice for countries with strong institutions, three hypotheses have been tested, focusing on health sector aid as the key explanatory and the infant mortality rate as the dependent variable. As health outcomes are believed to react much faster to changes in aid compared to broader measures like GDP growth, the health sector seems to be an ideal candidate. Within the health sector, the infant mortality rate is typically used as a proxy for general improvements with respect to health. Previous political and academic debates and contributions often generated controversial results regarding the effectiveness of aid. One potential reason is the undifferentiated view these analyses took. This article tried to look beneath the surface by using disaggregated aid and outcome variables to identify the effect of two different aid modalities on the health sector.

The first hypothesis considered the general relationship between aid to the health sector and infant mortality. There, a significant and negative effect of aid could be found. If health aid is increased by one per cent, the infant mortality rate in the average country decreases by around 0.037%, in the system GMM case. This supports the assumption that a positive effect of aid can be detected more easily on a disaggregated level. Since the focus of this paper is on the effect of sector budget support compared to other aid modalities, the health aid term was disaggregated. The effect of health sector budget support was then compared to project-type interventions in the health sector for the second hypothesis. Additionally, due to the belief that sector budget support is more efficient in countries with a sound institutional framework, it was interacted with a governance variable to test the third hypothesis. The overall findings were that project-type interventions exhibit a significant and negative effect on the infant mortality rate in the average country. Sector budget support is insignificant in the standard equation and when interacted with a governance variable. In contrast, both, health aid and project aid show a significant and positive (decreasing) effect when interacted with a governance variable. This effect is slightly stronger for countries with higher levels of governance.

Returning to the question raised at the beginning of this section, no clear answer can be given. Sector budget support has no significant effect on the infant mortality rate in countries with strong institutions. In contrast, project-type interventions show a significant and relatively strong negative effect on infant mortality. The policy recommendation which can be drawn from these estimations is, thus, that for most cases project-type interventions seem to be more effective than sector budget support.

In the beginning, the literature review indicated that the effectiveness of aid in general and health aid, in particular, is still contested. Further, in terms of budget support, many evaluations found positive outcomes. This article supports the positive findings of health aid to the health sector and adds an empirical analysis of budget support. In contrast to many evaluations, no positive effect of budget support could be found.

Besides the well-known criticisms towards budget support, e.g. corruption, rent-seeking, or an inefficient bureaucracy, other factors might have driven the results. One explanation might be that the volume of sector budget support is much smaller than the widely used project aid or that both modalities might be spent for different purposes. The effectiveness of sector budget support might also depend on additional factors that were not controlled for in this empirical framework. One example would be the willingness of those in power to base their actions on the institutions formally implemented. As indicated above, this can be seen with respect to the implementation of the results of the high-level meetings on aid effectiveness. Apart from these assumptions, sector budget support, in reality, might be simply less effective than project-type interventions. Though, to answer this question in its entirety, more research is needed.

The results presented above are only valid for the relationship between the two health aid modalities and the infant mortality rate. Even though infant mortality is a good proxy for other health-related outcomes, the effectiveness should also be tested with other dependent variables. Additionally, it might be interesting to research the effects of sector budget support in other sectors, for example, the education sector. Possibly, there, other additional modalities are frequently used by donors. It is yet unknown if some of the other existing modalities, for example, general budget support, might be even more effective.

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2.8 Appendix

Table A2.1: Summary statistics

	Observations	Mean	Standard dev.
Infant mortality rate (per 1000 live births)	1017	32.82	22.99
Aid per capita (Health sector; constant USD)	1019	4.65	4.87
Project aid per capita (Health sector; constant USD)	1019	3.80	4.25
Budget aid per capita (Health sector; constant USD)	1019	0.21	0.98
GDP per capita (constant 2010 USD)	985	4599.42	4875.38
Population (in Mill.)	1019	51.80	177.00
Fertility rate (births per woman)	1026	3.32	1.43
Urban population (% of total population)	1010	51.51	20.84
Incidence of HIV (per 1000, ages 15-49)	1026	1.40	3.41

Table A2.2: Countries in the sample

Country	Obs.	Country	Obs.	Country	Obs.
Afghanistan	8	Ethiopia	8	Nigeria	8
Albania	8	Gabon	8	Oman	8
Algeria	8	Gambia, The	8	Pakistan	8
Angola	8	Georgia	8	Panama	8
Argentina	8	Ghana	8	Paraguay	8
Armenia	8	Guatemala	8	Peru	8
Azerbaijan	8	Guinea	8	Philippines	8
Bahrain	8	Guinea-Bissau	8	Rwanda	8
Bangladesh	8	Guyana	8	Saudi Arabia	8
Belarus	8	Honduras	8	Senegal	8
Belize	8	India	8	Serbia	8
Benin	8	Indonesia	8	Sierra Leone	8
Bhutan	8	Iran, Islamic Rep.	8	Slovenia	8
Bolivia	8	Iraq	8	Somalia	8
Bosnia and Herzegovina	8	Jordan	8	South Africa	8
Botswana	8	Kazakhstan	8	South Sudan	8
Brazil	8	Kenya	8	Sri Lanka	8
Burkina Faso	8	Kyrgyz Republic	8	Sudan	8
Burundi	8	Lao PDR	8	Suriname	8
Cambodia	8	Lebanon	8	Syrian Arab Republic	8
Cameroon	8	Lesotho	8	Tajikistan	8
Central African Republic	8	Liberia	8	Tanzania	8
Chad	8	Libya	8	Thailand	8
Chile	8	Malawi	8	Timor-Leste	8
China	8	Malaysia	8	Togo	8
Colombia	8	Mali	8	Tunisia	8
Congo, Dem. Rep.	8	Mauritania	8	Turkey	8
Congo, Rep.	8	Mexico	8	Turkmenistan	8
Costa Rica	8	Moldova	8	Uganda	8
Cote d'Ivoire	8	Mongolia	8	Ukraine	8
Croatia	8	Montenegro	8	Uruguay	8
Djibouti	8	Morocco	8	Uzbekistan	8
Ecuador	8	Mozambique	8	Venezuela, RB	8
Egypt, Arab Rep.	8	Myanmar	8	Vietnam	8
El Salvador	8	Namibia	8	Yemen, Rep.	8
Equatorial Guinea	8	Nepal	8	Zambia	8
Eritrea	1	Nicaragua	8	Zimbabwe	8
Eswatini	8	Niger	8		

Table A2.3: Robustness: Split into different income groups

	Low income			Lower middle income			Upper middle income		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lagged log infant mortality	1.0117***	1.0121***	1.0134***	1.0061***	1.0059***	1.0060***	1.0139***	1.0131***	1.0138***
	(0.0036)	(0.0036)	(0.0037)	(0.0029)	(0.0029)	(0.0029)	(0.0052)	(0.0051)	(0.0053)
Log health aid	-0.0042*			-0.0065***			-0.0070***		
	(0.0024)			(0.0025)			(0.0021)		
Log project aid		-0.0031	-0.0043*		-0.0063**	-0.0063**		-0.0066***	-0.0063***
		(0.0024)	(0.0024)		(0.0025)	(0.0025)		(0.0023)	(0.0023)
Log budget support		-0.0001	0.0043		-0.0014	-0.0038		0.0026	-0.0020
		(0.0023)	(0.0033)		(0.0036)	(0.0051)		(0.0029)	(0.0017)
Dummy regulatory quality			0.0051*			0.0001			-0.0055
			(0.0027)			(0.0035)			(0.0054)
Log budget support * Dummy RQ			-0.0089*			0.0029			0.0067*
			(0.0046)			(0.0068)			(0.0040)
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Number of observations	213	213	213	307	307	307	345	345	345

Notes: Standard errors are denoted in parentheses. The pooled OLS regressions use robust standard errors clustered by country. The first three columns show the results for low-income countries, the following for lower middle-income countries and the last three for upper-middle and high-income countries. *, ** and *** denote significance at the 10%-, 5%- and 1%-level.

Table A2.4: Dummy interaction term of budget support and governance

	(1) OLS	(2) SysGMMCL	(3) OLS	(4) SysGMMCL
Dependent variable is *Log infant mortality*				
Lagged log infant mortality	1.0087***	1.0744***	1.0116***	1.0824***
	(0.0014)	(0.0196)	(0.0024)	(0.0310)
Log project aid	-0.0049***	-0.0360**	-0.0055***	-0.0300***
	(0.0015)	(0.0148)	(0.0016)	(0.0111)
Log budget support	-0.0036	0.0003	-0.0004	0.0069
	(0.0034)	(0.0093)	(0.0027)	(0.0108)
Dummy regulatory quality	-0.0014	0.0085	0.0012	-0.0119
	(0.0021)	(0.0144)	(0.0017)	(0.0144)
Log budget support * Dummy RQ	0.0034	0.0031	0.0002	-0.0044
	(0.0042)	(0.0169)	(0.0036)	(0.0168)
Controls	NO	NO	YES	YES
Time fixed effects	YES	YES	YES	YES
R-squared	0.999		0.999	
Number of observations	897	897	865	865
Number of countries		113		110
Number of instruments		33		50
Hansen test: P-value		0.881		0.929
AR(1) test: P-value		0.013		0.007
AR(2) test: P-value		0.629		0.827

Notes: Standard errors are denoted in parentheses. The pooled OLS regressions use robust standard errors clustered by country. The system GMM regressions use the twostep Windmeijer (2005) finite sample correction for standard errors. SysGMMCL denotes the system GMM estimaor with collapsed instruments. All variables of interest are treated as predetermined. *, ** and *** denote significance at the 10%-, 5%- and 1%-level.

Table A2.5: Robustness: Other governance variables

	(1) OLS	(2) SysGMMCL	(3) OLS	(4) SysGMMCL	(5) OLS	(6) SysGMMCL
	Dependent variable is *Log infant mortality*					
Lagged log infant mortality	1.0127***	1.1003***	1.0115***	1.0727***	1.0117***	1.1016***
	(0.0024)	(0.0368)	(0.0024)	(0.0316)	(0.0023)	(0.0369)
Log project aid	-0.0052***	-0.0177	-0.0054***	-0.0308***	-0.0063***	-0.0183
	(0.0015)	(0.0131)	(0.0015)	(0.0111)	(0.0016)	(0.0141)
Log budget support	0.0428***	0.0896	0.0332***	0.0403	0.0053	-0.0197
	(0.0152)	(0.0814)	(0.0111)	(0.0319)	(0.0119)	(0.0384)
Log corruption	0.0160***	-0.0328				
	(0.0047)	(0.0503)				
Log budget support * Log Corruption	-0.0398***	-0.0873				
	(0.0138)	(0.0746)				
Log government effectiveness			0.0040	-0.0260		
			(0.0051)	(0.0300)		
Log budget support * Log GE			-0.0304***	-0.0418		
			(0.0103)	(0.0296)		
Log polity					0.0052***	-0.0067
					(0.0015)	(0.0155)
Log budget support * Log Polity					-0.0021	0.0061
					(0.0045)	(0.0141)
Constant	-0.0968***	-0.1930	-0.0768***	0.0228	-0.0851***	-0.1466
	(0.0217)	(0.3259)	(0.0209)	(0.1902)	(0.0213)	(0.3802)
Controls	YES	YES	YES	YES	YES	YES
Time fixed effects	YES	YES	YES	YES	YES	YES
R-squared	0.999		0.999		0.999	
Number of observations	865	865	865	865	857	857
Number of countries		110		110		109
Number of instruments		50		50		50
Hansen test: P-value		0.916		0.953		0.903
AR1 test: P-value		0.012		0.006		0.025
AR2 test: P-value		0.865		0.803		0.899

Notes: Standard errors are denoted in parentheses. The pooled OLS regressions use robust standard errors clustered by country. The system GMM regressions use the twostep Windmeijer (2005) finite sample correction for standard errors. SysGMMCL denotes the system GMM estimaor with collapsed instruments. All variables of interest are treated as predetermined. *, ** and *** denote significance at the 10%-, 5%- and 1%-level.

Table A2.6: Robustness: Additional control variables

	(1) OLS	(2) SysGMMCL	(3) OLS	(4) SysGMMCL	(5) OLS
Dependent variable is *Log infant mortality*					
Lagged log infant mortality	1.0112***	1.0925***	1.0108***	1.0853***	1.0104***
	(0.0025)	(0.0311)	(0.0023)	(0.0357)	(0.0024)
Log project aid	-0.0054***	-0.0219*	-0.0065***	-0.0321**	-0.0053***
	(0.0017)	(0.0112)	(0.0015)	(0.0149)	(0.0015)
Log budget support	-0.0018	-0.0037	-0.0011	0.0010	-0.0014
	(0.0019)	(0.0065)	(0.0018)	(0.0124)	(0.0018)
Log regulatory quality	-0.0003	0.0598	0.0064	0.0022	0.0035
	(0.0053)	(0.0475)	(0.0045)	(0.0421)	(0.0049)
Controls	YES	YES	YES	YES	YES
Time fixed effects	YES	YES	YES	YES	YES
R-squared	0.999		0.999		0.999
Number of observations	818	818	832	832	832
Number of countries		105		108	
Number of instruments		55		55	
Hansen test: P-value		0.774		0.331	
AR(1) test: P-value		0.015		0.014	
AR(2) test: P-value		0.809		0.754	

Notes: Standard errors are denoted in parentheses. The pooled OLS regressions use robust standard errors clustered by country. The system GMM regressions use the twostep Windmeijer (2005) finite sample correction for standard errors. SysGMMCL denotes the system GMM estimaor with collapsed instruments. All variables of interest are treated as predetermined. *, ** and *** denote significance at the 10%-, 5%- and 1%-level.

Table A2.7: Robustness: Averages over time

	2 year average		3 year average	
	(1)	(2)	(3)	(4)
	OLS	OLS	OLS	OLS
Dependent variable is *Log infant mortality*				
Lagged log infant mortality	1.0248***	1.0238***	1.0326***	1.0315***
	(0.0073)	(0.0072)	(0.0130)	(0.0130)
Log health aid	-0.0143***		-0.0178*	
	(0.0049)		(0.0099)	
Log project aid		-0.0126**		-0.0154
		(0.0051)		(0.0101)
Log budget support		-0.0043		-0.0064
		(0.0069)		(0.0137)
Controls	YES	YES	YES	YES
Time fixed effects	YES	YES	YES	YES
R-squared	0.998	0.998	0.995	0.995
Number of observations	325	325	216	216

Notes: Standard errors are denoted in parentheses. The pooled OLS regressions use robust standard errors clustered by country. *, ** and *** denote significance at the 10%-, 5%- and 1%-level.

Table A2.8: Health aid and infant mortality incl. control variables

	(1) OLS	(2) SysGMMCL
Dependent variable is *Log infant mortality*		
Lagged log infant mortality	1.0126***	1.0903***
	(0.0024)	(0.0346)
Log health aid	-0.0060***	-0.0366**
	(0.0015)	(0.0147)
Log income	-0.0005	0.0097
	(0.0017)	(0.0326)
Log population	-0.0020***	-0.0166*
	(0.0006)	(0.0090)
Log fertility	-0.0015	-0.0674*
	(0.0031)	(0.0363)
Log HIV	0.0040**	-0.0045
	(0.0016)	(0.0111)
Log regulatory quality	0.0069*	-0.0099
	(0.0038)	(0.0398)
Log urban population	0.0083***	0.0079
	(0.0022)	(0.0434)
Constant	-0.0744***	-0.0318
	(0.0209)	(0.2748)
Time fixed effects	YES	YES
R-squared	0.999	
Number of observations	865	865
Number of countries		110
Number of instruments		40
Hansen test: P-value		0.637
AR1 test: P-value		0.008
AR2 test: P-value		0.839

Notes: Standard errors are denoted in parentheses. The pooled OLS regression uses robust standard errors clustered by country. The system GMM regression uses the twostep Windmeijer finite sample correction for the covariance variance. All variables are treated as predetermined. *, ** and *** denote significance at the 10%-, 5%- and 1%-level.

Table A2.9: Project aid, budget support and infant mortality incl. control variables

	(1) OLS	(2) SysGMMCL
Dependent variable is *Log infant mortality*		
Lagged log infant mortality	1.0122***	1.0749***
	(0.0024)	(0.0330)
Log project aid	-0.0056***	-0.0276**
	(0.0015)	(0.0121)
Log budget support	-0.0006	-0.0032
	(0.0018)	(0.0110)
Log income	-0.0002	0.0114
	(0.0017)	(0.0299)
Log population	-0.0019***	-0.0100
	(0.0006)	(0.0074)
Log fertility	-0.0011	-0.0490
	(0.0031)	(0.0367)
Log HIV	0.0040**	0.0000
	(0.0016)	(0.0104)
Log regulatory quality	0.0061	-0.0297
	(0.0039)	(0.0323)
Log urban population	0.0082***	0.0083
	(0.0022)	(0.0416)
Constant	-0.0789***	-0.1237
	(0.0210)	(0.2392)
Time fixed effects	YES	YES
R-squared	0.999	
Number of observations	865	865
Number of countries		110
Number of instruments		45
Hansen test: P-value		0.882
AR1 test: P-value		0.009
AR2 test: P-value		0.873

Notes: Standard errors are denoted in parentheses. The pooled OLS regression uses robust standard errors clustered by country. The system GMM regression uses the twostep Windmeijer finite sample correction for the covariance variance. All variables are treated as predetermined. *, ** and *** denote significance at the 10%-, 5%- and 1%-level.

Table A2.10: Interaction term of budget support and governance incl. control variables

	(1) OLS	(2) SysGMMCL
Dependent variable is *Log infant mortality*		
Lagged log infant mortality	1.0122***	1.0698***
	(0.0024)	(0.0275)
Log project aid	-0.0056***	-0.0303**
	(0.0015)	(0.0123)
Log budget support	0.0157	0.0327
	(0.0124)	(0.0347)
Log regulatory quality	0.0066*	-0.0230
	(0.0040)	(0.0218)
Log budget support * Log RQ	-0.0139	-0.0316
	(0.0108)	(0.0263)
Log income	-0.0002	0.0099
	(0.0017)	(0.0202)
Log population	-0.0019***	-0.0110
	(0.0006)	(0.0075)
Log fertility	-0.0012	-0.0397
	(0.0031)	(0.0359)
Log HIV	0.0040**	0.0018
	(0.0016)	(0.0104)
Log urban population	0.0083***	0.0088
	(0.0022)	(0.0306)
Constant	-0.0803***	-0.0947
	(0.0211)	(0.1793)
Time fixed effects	YES	YES
R-squared	0.999	
Number of observations	865	865
Number of countries		110
Number of instruments		50
Hansen test: P-value		0.911
AR1 test: P-value		0.008
AR2 test: P-value		0.820

Notes: Standard errors are denoted in parentheses. The pooled OLS regression uses robust standard errors clustered by country. The system GMM regression uses the twostep Windmeijer finite sample correction for the covariance variance. All variables are treated as predetermined. *, ** and *** denote significance at the 10%-, 5%- and 1%-level.

Table A2.11: Interaction term of health aid, project aid and governance

	(1) OLS	(2) SysGMMCL	(3) OLS	(4) SysGMMCL
Dependent variable is *Log infant mortality*				
Lagged log infant mortality	1.0126***	1.0919***	1.0122***	1.0795***
	(0.0024)	(0.0347)	(0.0024)	(0.0307)
Log health aid	-0.0048	-0.0629		
	(0.0049)	(0.0414)		
Log project aid			-0.0043	-0.0309
			(0.0052)	(0.0422)
Log budget support			-0.0006	-0.0030
			(0.0019)	(0.0089)
Log regulatory quality	0.0079	-0.0250	0.0071	-0.0114
	(0.0059)	(0.0496)	(0.0058)	(0.0378)
Log health aid * Log RQ	-0.0010	0.0233		
	(0.0042)	(0.0325)		
Log project aid * Log RQ			-0.0012	0.0019
			(0.0045)	(0.0347)
Controls	YES	YES	YES	YES
Time fixed effects	YES	YES	YES	YES
R-squared	0.999		0.999	
Number of observations	865	865	865	865
Number of countries		110		110
Number of instruments		45		50
Hansen test: P-value		0.659		0.781
AR(1) test: P-value		0.005		0.008
AR(2) test: P-value		0.792		0.871

Notes: Standard errors are denoted in parentheses. The pooled OLS regressions use robust standard errors clustered by country. The system GMM regressions use the twostep Windmeijer (2005) finite sample correction for standard errors. SysGMMCL denotes the system GMM estimaor with collapsed instruments. All variables of interest are treated as predetermined. *, ** and *** denote significance at the 10%-, 5%- and 1%-level.

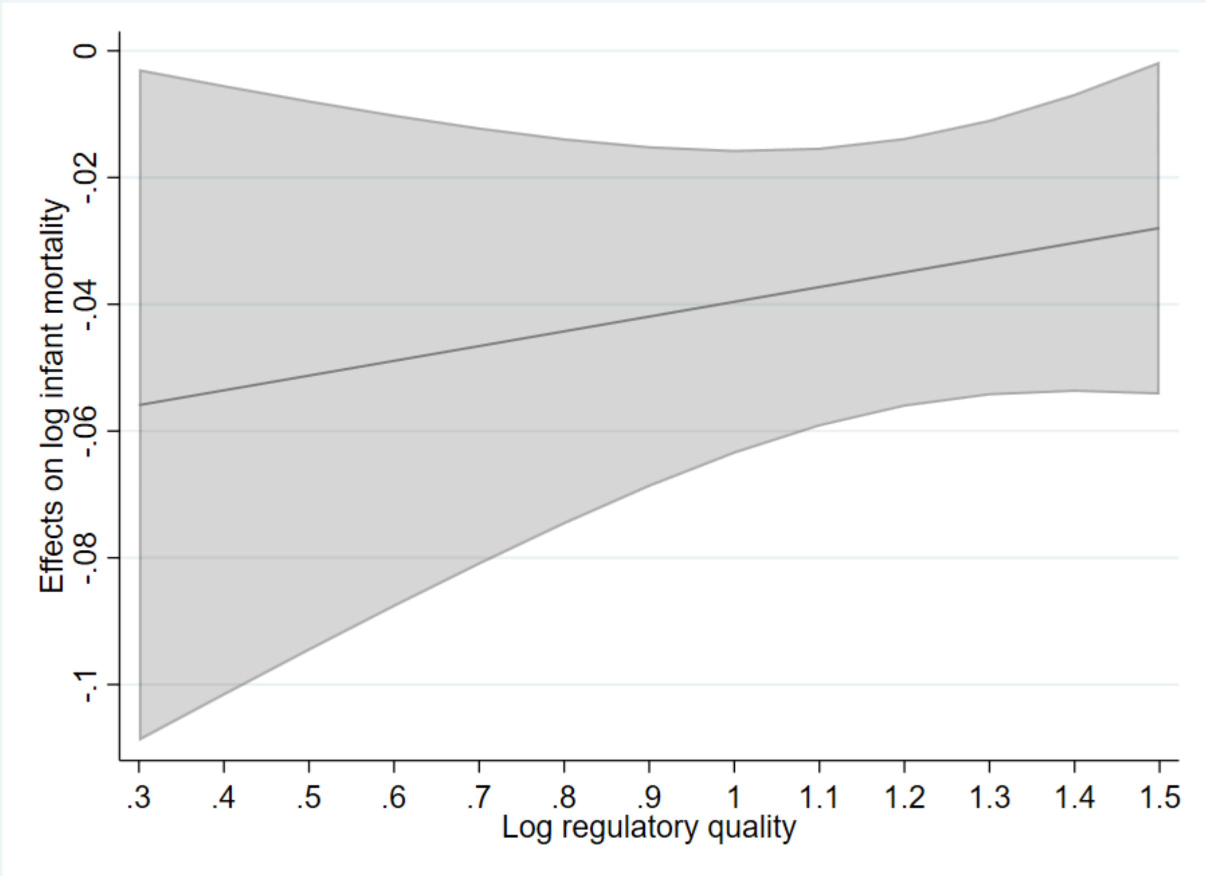

Figure A2.1: Marginal effects for the health aid interaction term

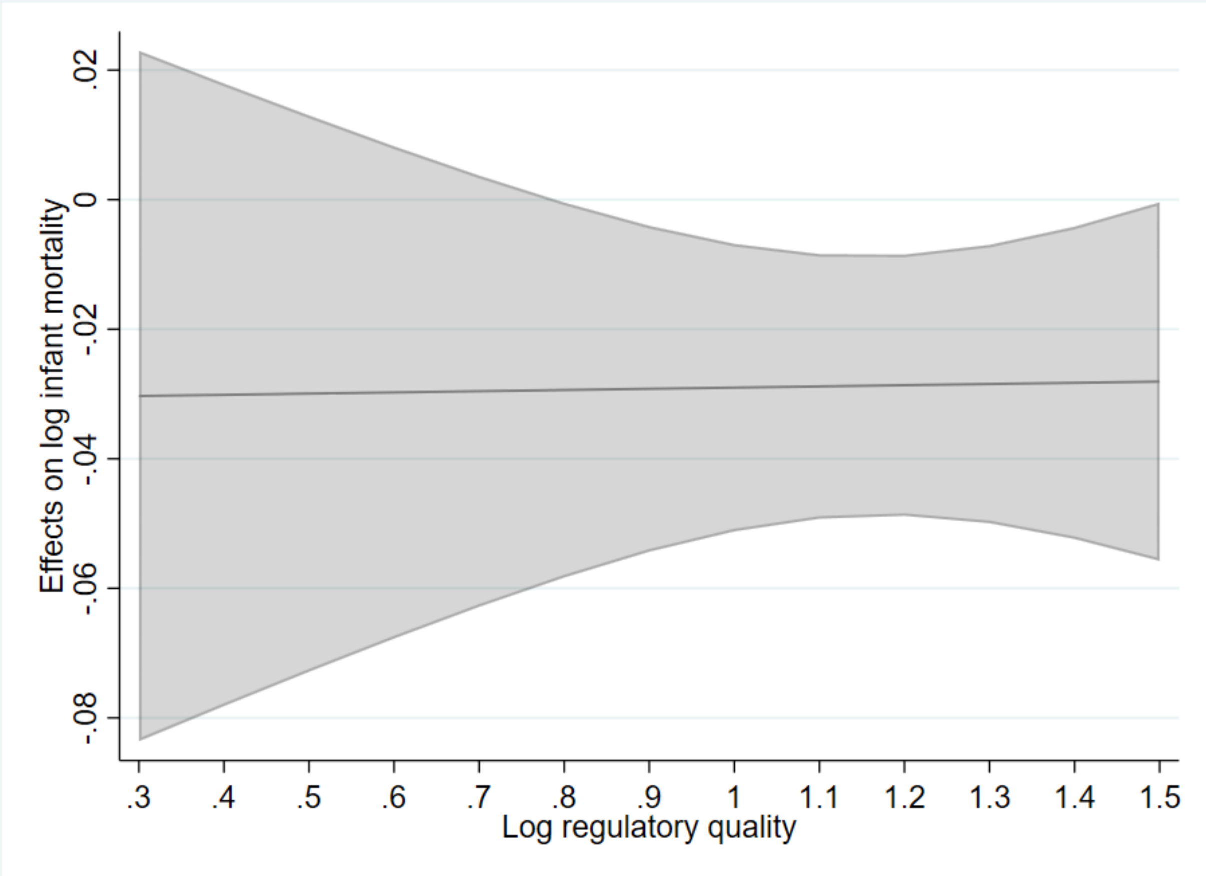

Figure A2.2: Marginal effects for the project aid interaction term

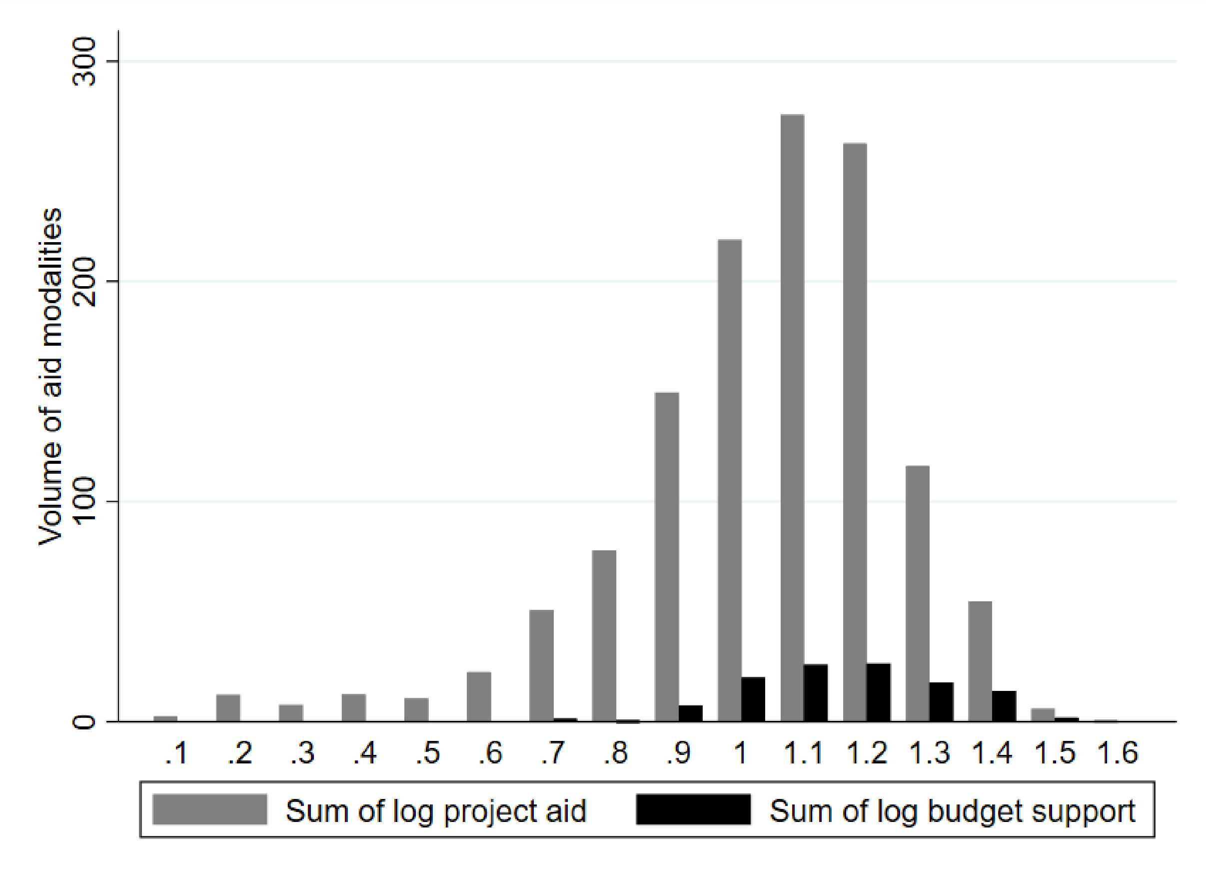

Figure A2.3: Log budget support and log project aid over log regulatory quality

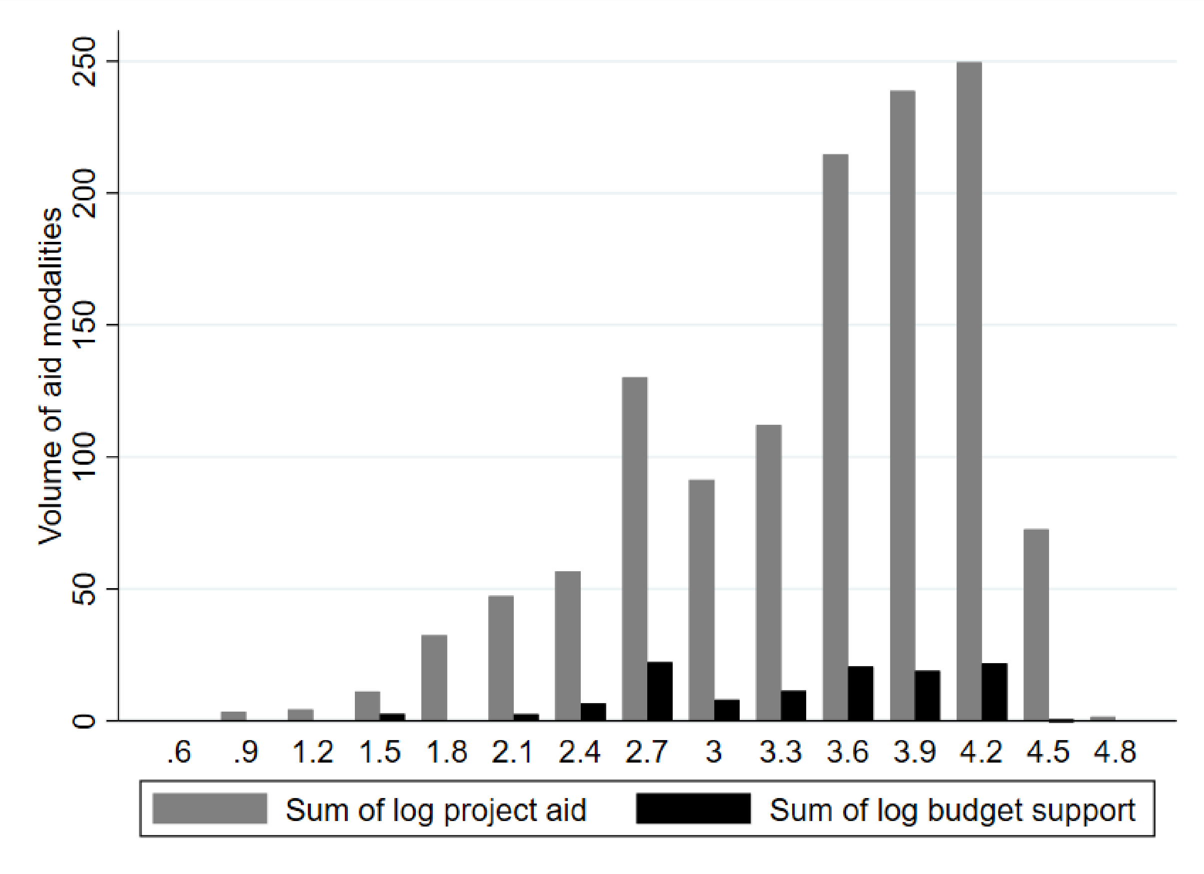

Figure A2.4: Log budget support and log project aid over log infant mortality

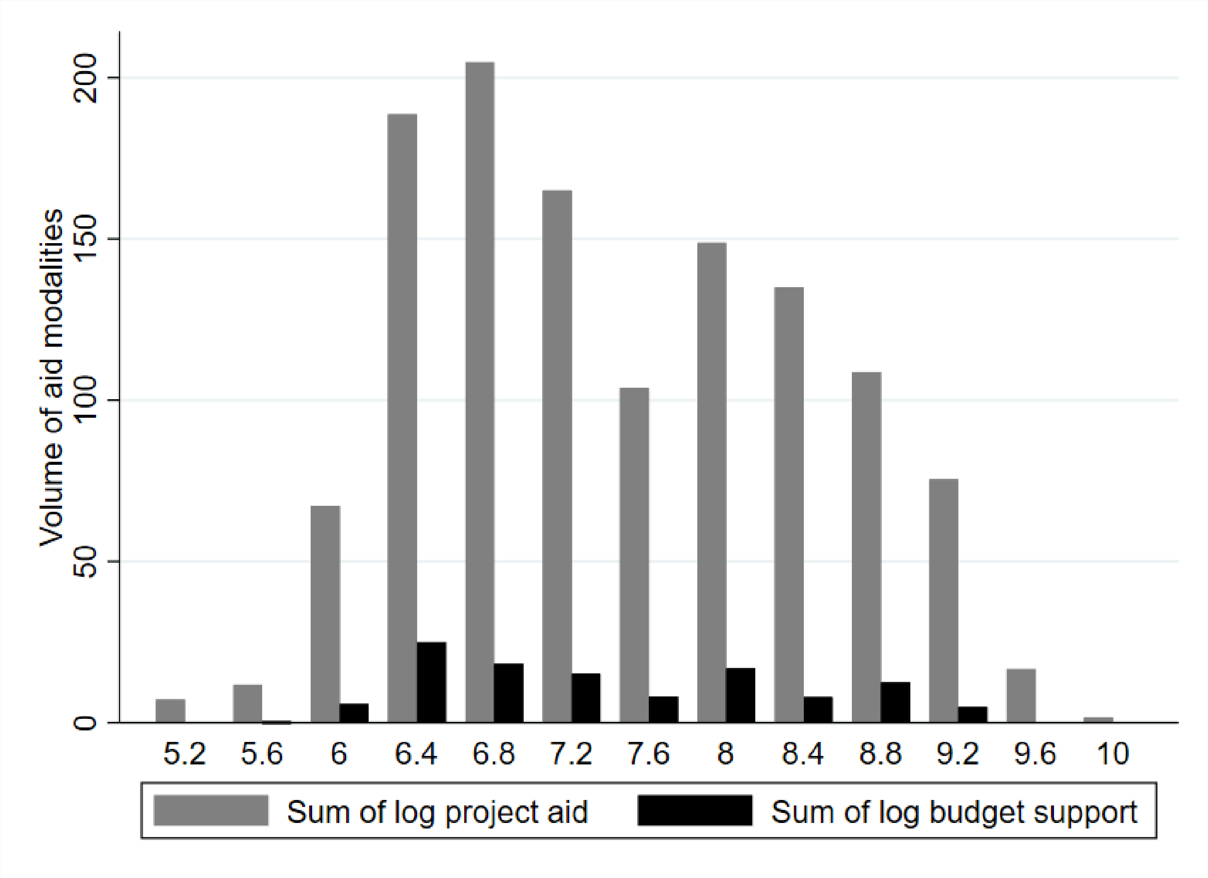

Figure A2.5: Log budget support and log project aid over log GDP per capita

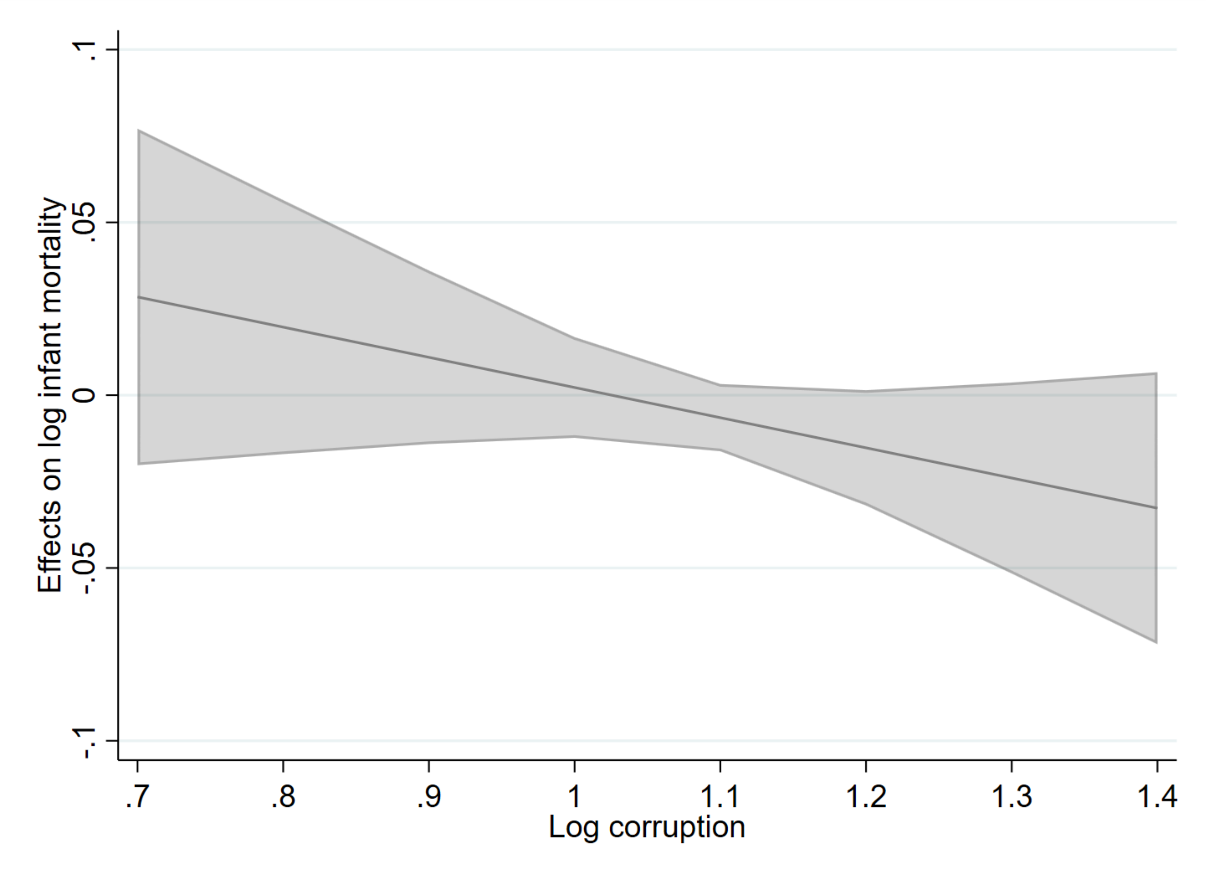

Figure A2.6: Marginal effect of interaction term of budget support and corruption

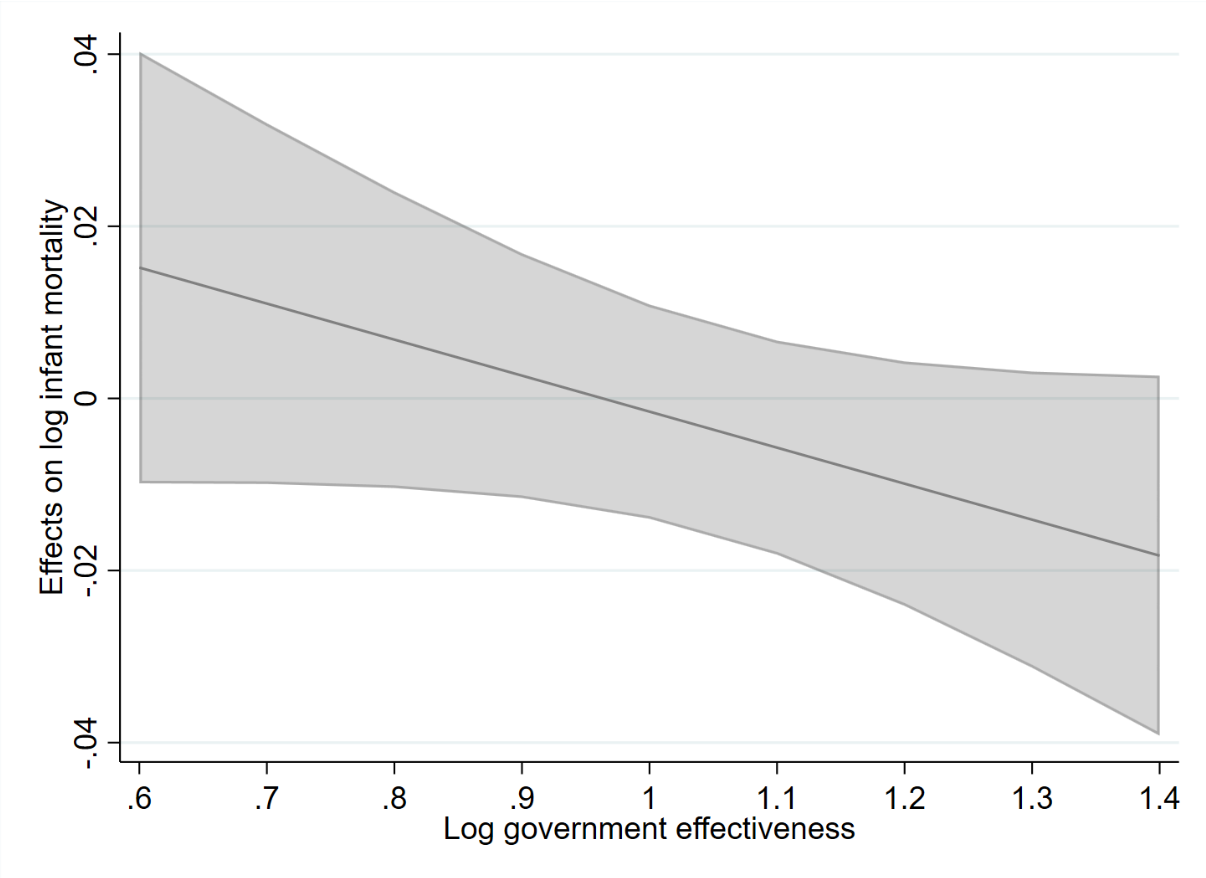

Figure A2.7: Marginal effect of interaction term of budget support and government effectiveness

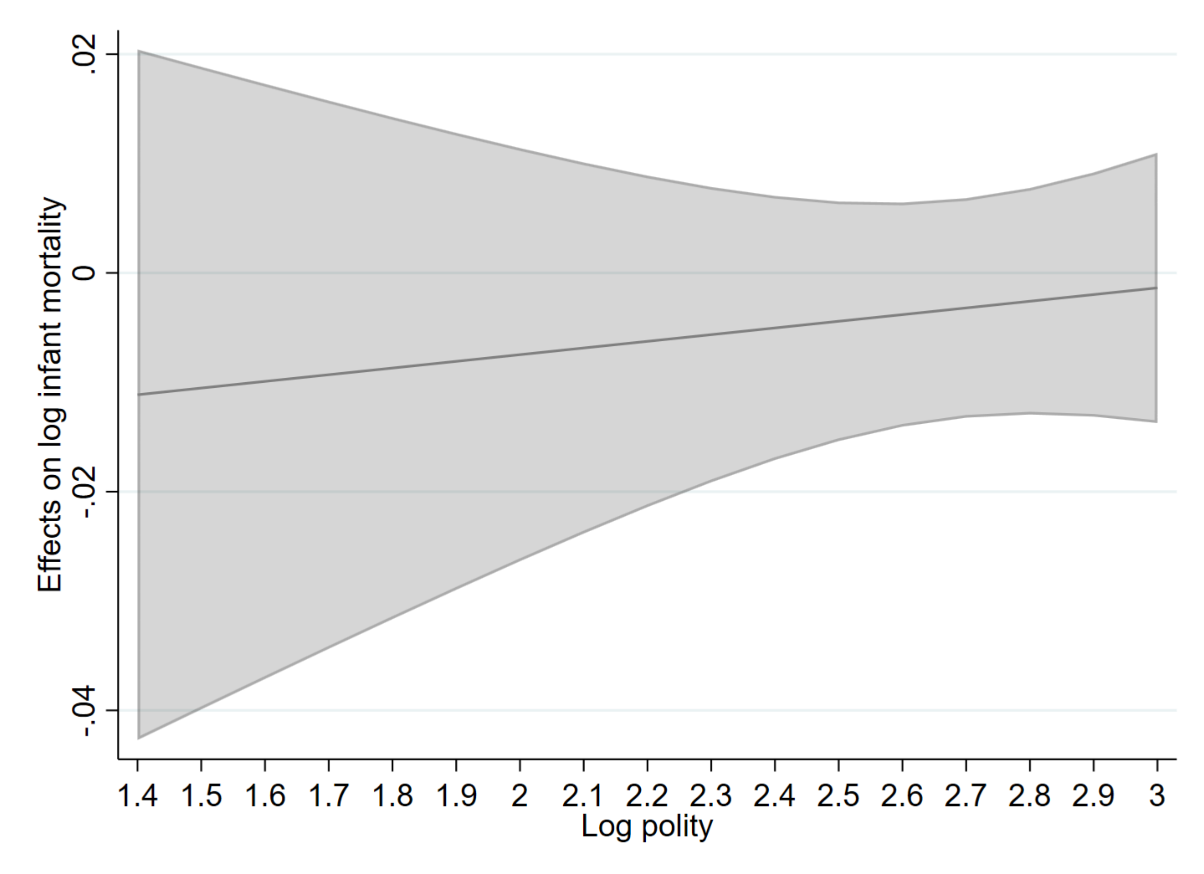

Figure A2.8: Marginal effect of interaction term of budget support and polity

Part 3

Unveiling the Mutual Dynamics - Institutions, Education, and Economic Growth Over 138 Years in OECD Countries

3.1 Introduction

According to institutional economists, such as Douglass North (e.g. 1990), James Buchanan (e.g. 1975), Daron Acemoglu (e.g. 2001), and others, institutions play a crucial role in economic development, shaping the incentives, behaviours, and interactions of individuals and organizations within a society. Institutions impact economic development in three main ways: First, property rights are fundamental for economic development. When individuals and businesses have confidence that their property will be protected and that contracts will be enforced, they are more likely to invest, innovate, and engage in productive economic activities. Second, effective market institutions, such as competition laws, regulatory frameworks, and consumer protections, create a level playing field for businesses. They prevent monopolies, encourage fair competition, and ensure markets operate efficiently, leading to innovation and economic growth. Third, stable political institutions guarantee checks and balances and, therefore, prevent exploitation by the state, create an environment of certainty, keep corruption down, and encourage long-term investment. While neoclassical economists often implicitly included institutions in their models, it took some time until they used such variables regularly and explicitly (North, 1990). Dawson (1998), for example, extends the neoclassical model by Mankiw, D. Romer, and Weil (1992) by incorporating an institutional variable.

Besides institutions, education (or human capital) is another critical determinant of economic development. One obvious explanation for this relationship is the higher productivity of workers resulting from more education. The higher level of productivity leads to more output and thus economic development or growth (e.g. Lucas, 1988). The empirical literature (e.g. Mankiw, D. Romer, and Weil, 1992; Acemoglu, Naidu, et al., 2018) supports the hypothesis that institutions as well as education are both critical factors influencing the total factor productivity. Thus, in this article, we will examine the relationships between these three variables in more detail.

However, development is not a one-way street. Higher economic development, and thus higher wealth, might lead to higher education spending and a higher demand for education (e.g. Glewwe and Jacoby, 2004). Additionally, with higher wealth, people might be better able to participate in the political process and push for better institutions (e.g. Tian et al., 2020). Eventually, what can we say about the relationship between institutions and education? People might only fully reflect on the importance of institutions and make the right voting decisions if they are sufficiently educated (e.g. Fortunato and Panizza, 2015). Moreover, suppose there is no room for development due to a lack of property rights or other institutional restrictions. In that case, there is no need to invest in one's own education. This could be seen in many Soviet countries or other autocracies where careers are not the result of personal effort but are tied to party memberships or loyalty to the regime. These hypotheses will be discussed in more detail in section 3.2.

Against this background, we would like to close two research gaps. First, as laid out above, we do not only analyse to which extent institutions and human capital foster GDP growth. We will also analyse the impact of human capital and GDP growth on institution-building and whether GDP growth and institutions will raise human capital. Secondly, we will analyse these interconnections over a long period (from 1870 until 2007) for 20 OECD countries using panel data. We will use an error correction model to capture long-run similarities between these countries and short-run differences across the countries. In the literature, the relationship between these three variables has yet to be systematically examined, especially over such a long period.

The paper's main findings are that institutions and human capital significantly foster growth. These results are robust to various extensions. Further, GDP growth positively affects human capital and institution building. However, the effects of education and institutions on one another are not robust.

The course of this paper is as follows: Section 3.2 provides an overview of the literature with respect to our hypotheses. Section 3.3 introduces the data and the empirical framework, whereas section 3.4 presents the main findings. Extensions are discussed in section 3.5. The paper finishes with a short conclusion.

3.2 Literature Review

Over time, increases in the quantity and quality of physical capital, labour, and technical and organisational progress should shift potential GDP p.c. (Islam, 1995; Mankiw, D. Romer, and Weil, 1992). This is the wisdom of the "old growth theory" (Solow, 1956). Investment in a competitive setting fosters GDP growth through improved productivity, technological progress, openness, and secure property rights. Moreover, the geographical location and culture may influence institution-building and investment and, therefore, should impact GDP p.c. (e.g. Acemoglu, Johnson, and Robinson, 2001; Easterly and Levine, 2003; Rodrik, Subramanian, and Trebbi, 2004).

Since the 1990s, economists such as Rodrik (2000) and many others argued that institutions are important determinants of growth, and that property rights represent a core category of economic institutions. The theoretical background of this hypothesis is much older starting with publications of early enlightenment philosophers like Hobbes (1651) and Locke (1690). The famous philosopher Hobbes (1651) argues that without law and order imposed by the state, life will be a war of "each against all" (Bellum omnium contra omnes), "...and the life of man solitary, poor, nasty, brutish, and short". Locke (1690) later emphasises that people should have fundamental rights, such as the rights to life, liberty, and property. Further, Montesquieu (1748) states that arbitrary governmental action against the will and the welfare of the people can only be avoided by implementing certain constraints on the government, e.g., the separation or division of the state's powers (into legislative, executive, and judiciary power). The economic philosophers Hayek (1960) and Buchanan (1975) have pushed this idea further.

More recently, Acemoglu, Johnson, and Robinson (2004) attributed great importance to the state's role in formalising and protecting property rights. The focus on institutions, such as property rights and political constraints, does not mean we should neglect other variables' impact on GDP p.c. It entails only the hypothesis that without property rights and political constraints, sustainable GDP p.c. growth cannot seriously be explained in the long-run. Unfortunately, institutional variables, such as rule of law, property rights, or constitutional law, cannot be directly measured (at least historically). This might be a problem, especially if one is interested in determining the quality of institutions in detail. Against this background, one solution is to use proxies.
Glaeser, La Porta, et al. (2004) use several different institutional measures to estimate the effect on economic growth. Some variables, for example, executive constraints or the expropriation risk, are significant and positive; others, for example, judicial independence, are not. Another more recent study by Góes (2016) indicates a positive relationship between economic freedom and GDP growth. Finally, in their meta-regression analyses, Efendic, Pugh, and Adnett (2011) and Colagrossi, Rossignoli, and Maggioni (2020) show that overall the evidence indicates a positive effect of institutions or democracy on economic performance. This leads us to our first hypothesis.

Hypothesis 1: Institutions have a positive effect on economic growth.

In the Keynesian growth model as well as in the neo-classical models of the Solow type, physical capital and exogenous technological progress played a dominant role. Since the mid of the 1980s, endogenous growth theories (e.g., P. M. Romer, 1986; P. M. Romer, 1990; Lucas, 1988) identified human capital and education as important factors contributing to long-run economic growth. A vast amount of empirical research supports the hypothesis that better education (human capital) will lead to higher growth rates (e.g., Mankiw, D. Romer, and Weil, 1992; Barro, 2000; Acemoglu and Angrist, 2000; Krueger and Lindahl, 2001). Ehrlich and Murphy (2007) describe human capital as an "intangible asset, best thought of as a stock of embodied and disembodied knowledge, comprising education, information, health, entrepreneurship, and productive and innovative skills". Increases in human capital arise through investment in schooling, job training, and health, as well as through research and development projects and informal knowledge transfers. Indeed, the idea that education and human capital are important factors for the development of an individual and a country can already be found in the works of "old" economists such as A. Smith (1776), Pigou (1920), and Machlup (1972). Even though the evidence of a positive effect of education seems clear at first, other studies do not find any significant effects of higher school enrolment and educational attainment (e.g., Knowles and Owen, 1995; Islam, 1995). This effect might even turn negative above a certain threshold of education. McGowan and Andrews (2017) argue that higher skill and qualification mismatch is associated with lower labour productivity and, thus, potentially with lower growth rates. Finally, Benos and Zotou (2014) summarise these results in their meta-regression analysis, pointing at the large problem of publication bias inherent in this body of literature. Nevertheless, we follow the endogenous growth theories and propose the following hypothesis.

Hypothesis 2: *Human capital has a positive effect on economic growth.*

Even if most researchers analyse the impact of institutions on growth, a reversed influence is conceivable, too. Diamond (1999) argues that agricultural progress and early economic growth enabled better institutions. Moreover, Lipset (1959) and Moore (1967) support the idea that higher income is a basis for creating better institutions. Also, some older analyses of democratic transition promote the hypothesis that higher economic development makes it easier to introduce democratic institutions (Kuznets, 1965; Kuznets, 1966). Following the argument by Acemoglu and Robinson (2000), a higher level of economic development might lead to the population demanding more redistribution from the political elites. In order to avoid unrest or revolution the elites extend democratic institutions ensuring future participation of the public in the redistribution process. Nevertheless, a critical junctures hypothesis formulated by North (1990) and Acemoglu, Johnson, and Robinson (2004) emphasises that the main direction of causality is from democracy or institutions to income.

In their theoretical work, Zak and Feng (2003) demonstrate that economic growth is an important factor in the speed of transition to democracy. The results of these theoretical works are supported empirically by Heid, Langer, and Larch (2012), who find a positive relationship between income and democracy.

In the long-run, Gundlach and Paldam (2009), and Tian et al. (2020) provide evidence for a strong and positive effect of income on democracy and institutions, respectively. They argue that income might be one of the most critical determinants of democratic transition. Acemoglu, Johnson, Robinson, and Yared (2008) find a positive correlation but no causal effect of income on democracy over a period of 500 years. All in all, we hypothesise the following relationship.

Hypothesis 3: *Economic development has a positive effect on institutions.*

According to Lipset (1959), wealth may lead to better education systems and social capital, inducing better democratic institutions (e.g., property rights). Djankov et al. (2003) add that more educated people are more likely to solve conflicts in a regulated manner and less likely to use violence. Glaeser, La Porta, et al. (2004) find that the level of schooling significantly affects the change in institutions in the following years. However, Acemoglu, Johnson, Robinson, and Yared (2005) show that time effects might drive this effect. When including these time effects in the regression of Glaeser, La Porta, et al. (2004), they cannot confirm the significant effect of schooling on democracy.

In contrast, Apergis and Payne (2017) find a positive relationship between education and democracy. The effect of education on democracy appears to be stronger in countries with lower income levels. The studies by Bobba and Coviello (2007) and Castelló-Climent (2008) support these results over a longer period between 1965 and 2000 and 1970 and 2000, respectively, for a large sample of countries. On the other hand, Fortunato and Panizza (2015) find a positive correlation between education and the quality of government in countries with higher levels of democracy.

Glaeser, Ponzetto, and Shleifer (2007) develop a theoretical model to analyse the channel between education and democracy. They argue that education leads to higher civic engagement, and well-educated people benefit relatively more from democracy. Thus, in an autocratic regime, once people demand more education due to a higher return on education, the autocrat needs to provide more education in order to prevent a revolution. Parente, Sáenz, and Seim (2022) argue that this leads to a modernisation process which eventually ends in a democracy. This theory is supported by an empirical study by Apergis (2018), providing new evidence from 161 countries that education supports democracy. Similarly, Milligan, Moretti, and Oreopoulos (2004) find that education increases attention to and interest in politics and thus leads to more informed and "better" voting behaviour. Thus, we propose the following hypothesis.

Hypothesis 4: Human capital has a positive effect on institutions.

Bjørnskov (2009) examines the effect of initial GDP p.c. and social capital on the growth rate of the average schooling length. Social capital is positively related to schooling length. However, initial GDP p.c. negatively affects the average schooling length. The author interprets this result as the high initial investment of wealthier countries to improve the education system. Oketch (2006) argues that richer countries invest more in education in order to achieve higher growth rates. He finds that per capita growth positively affects investment in education and, thus, increases human capital formation. Additionally, on a micro level, several studies investigate the effect of family income on child performance in school. Duncan, Magnuson, and Votruba-Drzal (2014), for example, argue that low-income families lack the resources to invest in education. A boost in parents' income leads to better school performance. This is in line with Glewwe and Jacoby (2004). They find that an increase in household wealth leads to a significant increase in school enrolment rates. They conclude that human capital does not only affect growth, but a reinforcing process between these two variables exists. Overall, we assume that higher GDP p.c. leads to a positive effect on human capital.

Hypothesis 5: Economic development has a positive effect on human capital.

Ehrlich and Lui (1999) provide a theoretical framework for the effect of institutions on human capital. They argue that weak institutions will get individuals to invest in rent-

seeking activities instead of human capital. Thus, in the absence of strong institutions, the level of human capital should be relatively lower. Glaeser, La Porta, et al. (2004) also estimate the effect of the initial level of institutions and of GDP p.c. on the growth of education. While GDP p.c. has a robust and significant positive effect, the different measures of institutions exhibit no such effect. In contrast, Rodrik, Subramanian, and Trebbi (2004) show that rule of law positively influences the human capital per worker. Brown and Hunter (2004) and Stasavage (2005), both examine the effect of democracy on education spending, especially for primary schools. They find a positive relation for Latin American and African countries, respectively. Samuels and Vargas (2023) confirms this result for a larger sample, in the absence of agrarian elites. Other studies examine the link between institutions and various educational outcomes. Tiongson (2000) and Mo (2001) find negative effects of corruption on educational outcome variables.
In contrast, Pellegrini and Gerlagh (2004) find no significant relationship between corruption and schooling. The result has been confirmed by Pellegrini (2011) using a newer dataset. Finally, this leads to our last hypothesis.

Hypothesis 6: Institutions have a positive effect on human capital.

In this article, the long-run evidence is of particular interest. Thus, we will turn to long-run studies, e.g., using growth or GDP p.c. as the dependent variable. Almost all long-run studies examine the period between 1960 and the beginning of the millennium. Gerring et al. (2005) and Acemoglu, Naidu, et al. (2018), for example, look at the effect of democracy on growth in a large sample of countries between 1950 and 2000, and 1960 and 2010, finding positive and significant effects. Gründler and Krieger (2016) confirm these results. Similarly, Hanushek and Woessmann (2007) and Hanushek and Woessmann (2012) examine the effect of schooling on economic growth and find positive and significant effects. Bassanini and Scarpetta (2002) estimate the human capital augmented growth model. There in the long-run, human capital significantly increases the growth rate. K. Lee and Kim (2009), Rivera-Batiz (2002), and Dias and Tebaldi (2012) show that both, education and policies, matter for economic growth. An older study by O'Rourke and

Williamson (1997) provides some historical evidence with respect to globalisation, schooling and growth in the European periphery between 1870 and 1913. They identify schooling as a driver of convergence for some countries within their sample. Finally, Röthel and Leschke (2023) examine the effect of institutions and education in a neoclassical growth model between 1870 and 2007. They find robust positive and significant effects of both variables.

In contrast to most of these studies, we analyse the mutual influence of the three variables, GDP p.c., human capital and institutions, essential for social development. This allows us to test if these variables follow a virtuous circle since the dynamics between the variables are sufficiently accounted for the first time. Since we estimate the relationship over a long time, we are able to differentiate between short- and long-run effects of the variables on one another.

3.3 Empirical Framework

In this study, the relationship between economic development, institutions and education, in the long-run, is of particular interest. The dataset comprises 20 countries between 1870 and 2007. As in many previous studies (e.g. Acemoglu, Johnson, Robinson, and Yared, 2008), we measure economic development using the GDP p.c. data from the Maddison Project Database (Bolt and Van Zanden, 2020). Concerning the institutional variable, we already discussed the argument by Buchanan (1975) in favour of property rights or binding rules for politicians above. This is in line with the results of Acemoglu and Johnson (2005) that property rights which protect the citizens' rights against the government are of first-order importance for economic outcomes. Thus, as a proxy for institutions, we use the liberal democracy index from the Varieties of Democracy Institute. This index takes a negative view of political power and emphasises individual rights against the tyranny of the state (Coppedge et al., 2022). Finally, for our purpose, educational attainment regarding secondary education seems to be suitable as a proxy for overall education. Primary education shows less differences across countries, and tertiary education is an inappropriate measure of education in the 19th century. The education variable is taken from the Barro-Lee dataset and measures the average years of secondary schooling within the population between 15-64 years (Barro and J.-W. Lee, 2015). Many previous studies (e.g. Acemoglu, Johnson, Robinson, and Yared, 2005) used this variable, too. The education variable is only available every five years. Since the other two variables contain yearly data, we linearly approximate the education variable to obtain yearly data, too.

We assume that institutions and education are not only determinants of total factor productivity, but (even more importantly) a precondition for private investment. Thus, in the following specifications, we will only focus on the three abovementioned variables and not include investment. However, this has been done by Röthel and Leschke (2023) who followed Dawson (1998) and investigated the factor productivity-effect of institutions and education on growth in a full growth model, including the investment variable, over the same period.

Table A3.1 in the Appendix presents the descriptive statistics. Since secondary education describes the average years of secondary schooling, it takes a value between 0 and 7.45 in the sample. Liberal democracy varies between 0.01 and 0.90, where higher levels of democracy are associated with higher index values. In the regression, we specify all variables in logarithmic form. This is in line with previous studies (e.g. Wooldridge, 2020; Röthel, 2023) and allows us to interpret the results as elasticities. Since taking the natural logarithm would create negative values for the democracy and education variables, we first apply the following linear transformation on these variables: $y = 1 + x$.

Figure 3.1 shows the average development of these three variables across the countries in the sample over time. Starting in 1870 at a rather low level, all three variables exhibit continuous and parallel growth until reaching their peak in 2007. The only exception are the values for liberal democracy in the years around the second world war exhibiting a decline before recovering again in the 1950s.

Turning to the regression equation and estimation strategy, we assume that lagged values of the variables affect their current values and the values of the other variables. Thus, we look at a dynamic linear relationship. There are several ways which are typically used to estimate dynamic panel specifications. Usually, when the panel is short with many cross-sectional units, estimators like the Two-Stage Least Squares or System GMM esti-

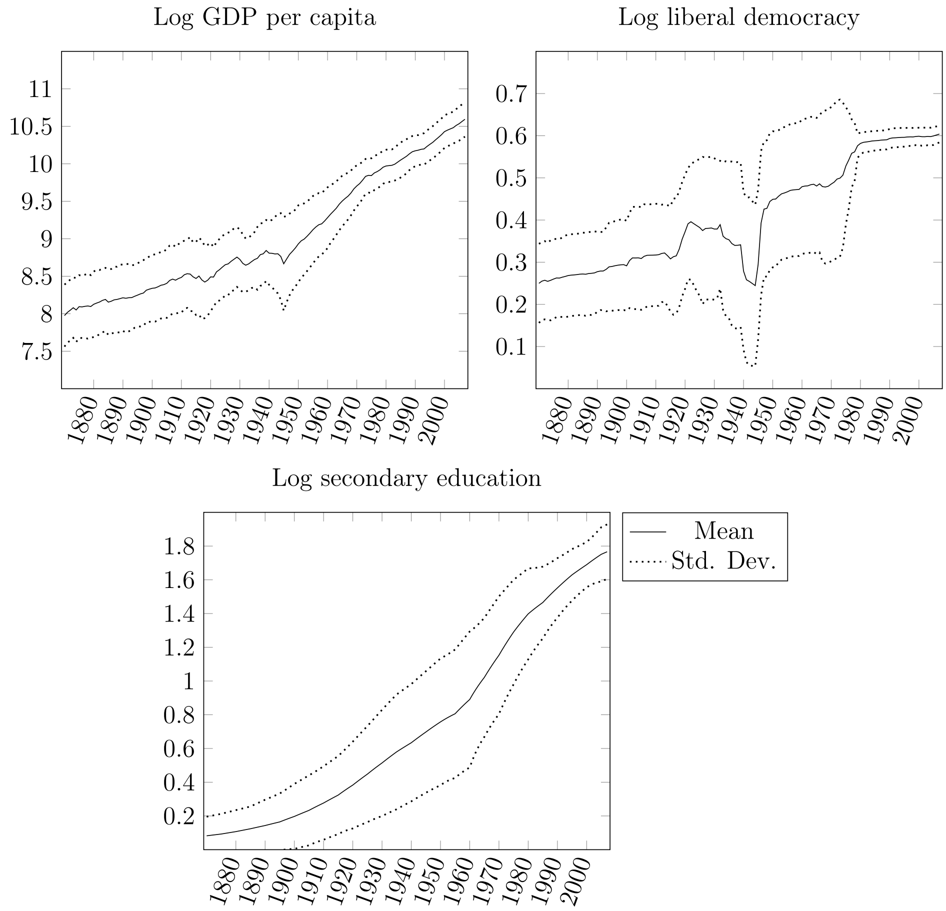

Figure 3.1: Mean and standard deviation of the variables (1870–2007)

mator are used. However, these estimators are not necessarily designed for other settings, for example, with both, large N and T (Roodman, 2009). Pesaran and R. P. Smith (1995) describe different possibilities for dealing with such data: One way would be to apply an estimator imposing a common slope, like the dynamic fixed effects (DFE) estimator. Other possibilities would be either averaging the data over the cross-sectional units and estimating all together as a time series or averaging the data over time and estimating one single cross-section. However, Pesaran and R. P. Smith (1995) show that these estimators mostly provide inconsistent results in a dynamic environment. Additionally, the DFE estimator suffers from the so-called Nickell bias (Nickell, 1981). With large T, one could run single regressions for each unit and average the coefficients across groups, yielding the overall effect. This strategy is referred to as mean group estimator (MG) which is consistent at least for large N and T. Further, when applying a pooled estimator (fixed or random effects), all parameters across units are constrained to be the same, except for the intercepts. In contrast, with the MG estimator, all parameters are estimated separately across units. Thus, potential similarities are neglected (Pesaran, Shin, and R. P. Smith, 1999). In order to account for dynamic heterogeneous models, Pesaran, Shin, and R. P. Smith (1999) introduce the pooled mean group estimator (PMG). This intermediate estimator accounts for both short-run heterogeneity across units and long-run similarities. Thus, it combines pooling and averaging.

The data described above has a moderately large N and large T. Thus, either the MG or the PMG estimator can be applied. Since all countries are OECD countries, it is plausible to assume that in the long-run, all are affected by similar technological and other effects. However, in the short-run, these countries presumably differ in terms of these factors and deviate from the common long-run equilibrium. Therefore, we will assume that in the short-run, heterogeneity across countries exists, while in the long-run, similarities can be observed. Following the previous explanations in this section, we will apply the PMG estimator and compare the results to the MG and the DFE estimator. Following Pesaran, Shin, and R. P. Smith (1999) and Blackburne and Frank (2007), a convenient starting point is the following simple autoregressive distributed lag (ARDL) (p,q,q) framework:

$$y_{it} = \sum_{j=1}^{p} \lambda_{ij} y_{i,t-j} + \sum_{j=0}^{q} \delta_{1ij} d_{i,t-j} + \sum_{j=0}^{q} \delta_{2ij} s_{i,t-j} + \mu_i + \epsilon_{it}, \tag{3.1}$$

where y_{it} denotes GDP p.c, d_{it} Liberal Democracy, and s_{it} Secondary Education in country i at time t. μ_i represents the country fixed effects and ϵ_{it} the error term. Reparameterizing, we get the following error correction equation:

$$\begin{aligned} \Delta y_{it} = \phi_i (y_{i,t-1} - \theta_{1i} d_{it} - \theta_{2i} s_{it}) + \sum_{j=1}^{p-1} \lambda^*_{ij} \Delta y_{i,t-j} \\ + \sum_{j=0}^{q-1} \delta^*_{1ij} \Delta d_{i,t-j} + \sum_{j=0}^{q-1} \delta^*_{2ij} \Delta s_{i,t-j} + \mu_i + \epsilon_{it}, \end{aligned} \tag{3.2}$$

where $\phi_i = -(1 - \sum_{j=1}^{p} \lambda_{it}), \theta_{1i} = \frac{\sum_{j=0}^{q} \delta_{ij}}{1-\sum_k \lambda_{ik}}, \theta_{2i} = \frac{\sum_{j=0}^{q} \delta_{ij}}{1-\sum_k \lambda_{ik}},$

$$\lambda_{ij}^{*} = -\sum_{m=j+1}^{p} \lambda_{im},\ j = 1, 2, ..., p-1,$$

and

$$\delta_{1ij}^{*} = -\sum_{m=j+1}^{q} \delta_{im},\ \delta_{2ij}^{*} = -\sum_{m=j+1}^{q} \delta_{im},\ j = 1, 2, ..., q-1. \tag{3.3}$$

ϕ denotes the error correction parameter and can be interpreted as the speed of adjustment to equilibrium. We assume that our variables are nonstationary (integrated of order one I(1)) and cointegrated. Thus, the first part of equation 3.2 in brackets represents the long-run relationship between our three variables of interest. Since we assume a cointegrating relationship, they are stationary. The latter part of equation 3.2 are the short-run dynamics that are first-differenced to make them stationary. Equations 3.1 and 3.2 are exemplary for the effects of institutions and education. However, we also estimate the following two ARDL (p,q,q) equations and reparameterize them accordingly, in order to estimate the effect of GDP and education on institutions and of GDP and institutions on education:

$$d_{it} = \sum_{j=1}^{p} \lambda_{ij} d_{i,t-j} + \sum_{j=0}^{q} \delta_{1ij} y_{i,t-j} + \sum_{j=0}^{q} \delta_{2ij} s_{i,t-j} + \mu_i + \epsilon_{it}, \tag{3.4}$$

$$s_{it} = \sum_{j=1}^{p} \lambda_{ij} s_{i,t-j} + \sum_{j=0}^{q} \delta_{1ij} d_{i,t-j} + \sum_{j=0}^{q} \delta_{2ij} y_{i,t-j} + \mu_i + \epsilon_{it}. \tag{3.5}$$

In order to test the assumptions laid out above, we conduct stationarity tests of the variables. Baltagi (2010) provides an overview of several common unit root tests. In

this paper, the Im, Pesaran and Shin (IPS) test (Im, Pesaran, and Shin, 2003) and the Fisher test (Choi, 2001) are applied since both seem to be most suitable in a large N and T setting. Table A3.2 in the Appendix shows the results of the unit root tests. The null hypothesis that all panels contain a unit root is tested against the alternative that some panels are stationary for the IPS test and against the alternative that at least one panel is stationary for the Fisher test. When the variables are specified in levels, the null hypothesis cannot be rejected for all variables. However, in the first difference specification, the null hypothesis can be rejected for all variables at the 1%-level. Additionally, we need to test the assumption that the three variables are cointegrated. We have conducted the panel cointegration tests by Pedroni (2004) and Westerlund (2005). Table A3.3 in the Appendix shows the results. Both tests reject the null hypothesis of no cointegration among the variables.
Finally, we test the lag order by using Akaike's information criterion (AIC) (Akaike, 1974). The results in table A3.4 in the Appendix show that the ARDL model (2,2,2) is the preferred one for GDP p.c. and liberal democracy as dependent variables. For the specification with secondary education as the dependent variable, the ARDL (2,1,2) is preferred. Thus, equations 3.2, 3.4 and 3.5 will be adjusted accordingly.

3.4 Results

We will start by examining the effect of institutions and education on GDP p.c. The null hypotheses that both variables have no significant effect on GDP p.c. will be tested against the alternatives presented above in hypothesis 1 and hypothesis 2. Table 3.1 presents the baseline results of the three estimators: PMG, MG and DFE. Starting with the convergence coefficients, we find negative and significant estimates, indicating convergence to the long-run equilibrium. We can observe similar results for the three estimators when looking at the long-run coefficients. Both liberal democracy and secondary education positively affect GDP p.c. When looking at the short-run coefficients, all estimates are insignificant, at least in the PMG and MG estimations. The effects are long-term, likely because of the nature of the two explanatory variables. It usually takes some time

Table 3.1: Baseline results GDP p.c.

		(1) PMG	(2) MG	Hausman test	(3) DFE
Convergence coefficient	$LogGDPp.c._{t-1}$	-0.0292***	-0.0436***		-0.0194***
		(0.0062)	(0.0081)		(0.00392)
Long-run coefficients	$LogLiberalDemocracy_t$	3.7662***	4.0086**		3.395***
		(0.4288)	(1.5945)		(0.685)
	$LogSecondaryEducation_t$	0.6709***	0.8416***		1.045***
		(0.0852)	(0.3061)		(0.139)
				0.57	
Short-run coefficients	$\Delta LogGDPp.c_{t-1}$	0.1090***	0.1012**		0.148***
		(0.0366)	(0.0396)		(0.0191)
	$\Delta LogLiberalDemocracy_t$	-0.0649	-0.0686		0.242***
		(0.1286)	(0.1269)		(0.0368)
	$\Delta LogLiberalDemocracy_{t-1}$	-0.0074	-0.0140		0.0227
		(0.1215)	(0.1194)		(0.0377)
	$\Delta LogSecondaryEducation_t$	0.0219	0.0350		0.0522
		(0.1423)	(0.1358)		(0.143)
	$\Delta LogSecondaryEducation_{t-1}$	0.0841	-0.0471		-0.00206
		(0.1733)	(0.1691)		(0.144)
	$Constant$	0.2194***	0.3312***		0.148***
		(0.0435)	(0.0615)		(0.0303)
	Number of observations	2681	2681		2681
	Number of countries	20	20		20
	Number of periods	136	136		136

Notes: The Hausman test is indicating that the PMG estimator is preferred over the MG estimator.
*, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively.

until a change in democracy and education translates into economic growth. Suppose a government introduces a new regulation concerning property rights within a country (partly measured by the liberal democracy variable). In that case, strengthening property rights, for example, might affect business decisions in the same year. However, it might take longer until these business decisions, such as private investment, are reflected in a country's GDP. Similarly, an increase in the human capital stock of a country (reflected in the secondary education variable) will likely not increase GDP in the same year. It will take some time until companies hire these employees with higher education. Furthermore, it will take even longer until these highly skilled employees affect the companies' output and, thus, a country's GDP.

Section 3.3 shows that the PMG estimator is consistent and efficient if the long-run restrictions are true. The MG estimator would be consistent as well. However, if the true model would be heterogeneous in the long-run, only the MG estimator would be consistent (Blackburne and Frank, 2007). Additionally, we know that the dynamic panel bias

Table 3.2: Baseline results liberal democracy

		(1) PMG	(2) MG	Hausman test	(3) DFE
Convergence coefficient	$LogLiberalDemocracy_{t-1}$	-0.0630***	-0.0816***		-0.0597***
		(0.0141)	(0.0140)		(0.00600)
Long-run coefficients	$LogGDPp.c._t$	0.0490*	-0.1231		0.106***
		(0.0263)	(0.1773)		(0.0309)
	$LogSecondaryEducation_t$	0.0886***	0.3948		0.0442
		(0.0330)	(0.2824)		(0.0431)
				1.49	
Short-run coefficients	$\Delta LogLiberalDemocracy_{t-1}$	0.2461***	0.2373***		0.275***
		(0.0339)	(0.0297)		(0.0188)
	$\Delta LogGDPp.c._t$	0.0654**	0.0617*		0.0762***
		(0.0324)	(0.0322)		(0.00996)
	$\Delta LogGDPp.c._{t-1}$	-0.0097	-0.0108		-0.0111
		(0.0295)	(0.0309)		(0.0100)
	$\Delta LogSecondaryEducation_t$	0.0039	0.0117		-0.00566
		(0.0794)	(0.0692)		(0.0740)
	$\Delta LogSecondaryEducation_{t-1}$	0.1107	0.0961		0.0967
		(0.0940)	(0.0832)		(0.0747)
	Constant	-0.0056***	-0.0195		-0.0354**
		(0.0017)	(0.0300)		(0.0158)
	Number of observations	2681	2681		2681
	Number of countries	20	20		20
	Number of periods	136	136		136

Notes: The Hausman test is indicating that the PMG estimator is preferred over the MG estimator.
*, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively.

weakens the DFE estimator. Thus, we need to choose between the PMG and the MG estimator. In such a case, in order to find the preferred estimator, we can conduct a Hausman test (Hausman, 1978). Table 3.1 shows the test statistic in the column "Hausman test". It indicates that the PMG model is preferred over the MG model. The results confirm the hypothesis that institutions and education positively affect GDP p.c. The respective alternative hypotheses 1 and 2 can both be rejected on the 1%-level.

As depicted in figure 3.1, our variables seem to be affected by historic events. Thus, we have also included time dummies controlling for such events. Table A3.5 shows the results for different time dummies in columns 1 and 2. In column 1, we include dummies for the two World Wars only. To control for the Great Depression, the Marshall Plan and World War II aftermath, the Oil Crisis and the Fall of the Iron Curtain, we add even more dummies in column 2. The results are similar to those of the baseline estimation. The coefficient of liberal democracy remains significant and positive with a slightly smaller magnitude. In contrast, the coefficient of secondary education positively and significantly affects economic development, gaining magnitude. As expected, both World War dummies strongly and negatively affect GDP p.c. growth. In contrast, the Marshall Plan dummy, capturing the World War II aftermath, is significant and positive, reflecting the economic revival after the war. The remaining dummies are insignificant.

Table 3.3: Baseline results secondary education

		(1) PMG	(2) MG	Hausman test	(3) DFE
Convergence coefficient	$LogSecondaryEducation_{t-1}$	-0.0071***	-0.0116***		-0.0045***
		(0.0017)	(0.0024)		(0.0007)
Long-run coefficients	$LogLiberalDemocracy_t$	0.0075	-2.7731		0.4360
		(0.1313)	(4.0061)		(0.3540)
	$LogGDPp.c._t$	0.7616***	0.7770		0.6810***
		(0.0294)	(0.6218)		(0.0640)
				0.80	
Short-run coefficients	$\Delta LogSecondaryEducation_{t-1}$	0.8736***	0.8474***		0.8820***
		(0.0103)	(0.0106)		(0.0094)
	$\Delta LogLiberalDemocracy_t$	0.0087	0.0048		-0.0023
		(0.0063)	(0.0072)		(0.0049)
	$\Delta LogGDPp.c._t$	-0.0031	-0.0054**		-0.0014
		(0.0025)	(0.0026)		(0.00260)
	$\Delta LogGDPp.c._{t-1}$	-0.0033	-0.0046		-0.0000
		(0.0030)	(0.0028)		(0.0024)
	$Constant$	-0.0420***	-0.0586***		-0.0235***
		(0.0102)	(0.0122)		(0.0041)
	Number of observations	2686	2686		2686
	Number of countries	20	20		20
	Number of periods	136	136		136

Notes: The Hausman test is indicating that the PMG estimator is preferred over the MG estimator.
*, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively.

Further, we examine the relationship between GDP p.c., secondary education and liberal democracy as the dependent variable. Here, we will test the null hypotheses that both variables have a significant effect on liberal democracy against the alternatives formulated in hypotheses 3 and 4. The results are presented in table 3.2. Again, the convergence effects are significant and negative, indicating convergence to the long-run equilibrium in all cases. Since the Hausman test suggests that the PMG estimator is preferred, we will focus on the first column in the following. In the long-run, GDP p.c. and secondary education positively and significantly affect liberal democracy. However, GDP p.c. is significant at the 10%-level only. In the short-run, all variables except the lagged dependent variable and the change in GDP p.c. are insignificant. A change in GDP p.c. positively influences a change in liberal democracy. As explained in section 3.2, one way to interpret the result would be that when people are better educated they might acknowledge the value of a higher level of institutions and thus demand this kind of change in institutions. In addition, the higher the level of GDP p.c., and thus the level of wealth of the people, the more time can be used to demand a higher level of institutions. Both arguments follow Lipset (1959). Returning to hypotheses 3 and 4, we can reject both null hypotheses.

Similar to the above, we test the results when adding the same time dummies. Table A3.7 in the Appendix presents the results. The effect of education on democracy is positive and significant in all specifications. However, the significance level decreases compared to the baseline results. In contrast, the significance level of GDP p.c. increases. The World War II dummy remains significant and negative, and the Marshall Plan dummy remains significant and positive.

Finally, we examine the relationship between GDP p.c., liberal democracy and secondary education as the dependent variable. Again, we test the null hypotheses that both variables have no effect on secondary education against the alternatives 5 and 6 formulated above. Table 3.3 presents the results. The convergence coefficient is significant and negative in all specifications, indicating convergence. Additionally, as in the previous cases, the Hausman test favours the PMG estimator. The long-run coefficient of GDP p.c. significantly and positively affects secondary education, while the effect of liberal democracy is insignificant. All short-run coefficients, except for the lagged dependent variable, are insignificant. The short-run results may not be surprising since it takes relatively long until a shock towards the education system translates into a change in school attainment. In the long-run, GDP p.c. is a country's potential wealth to invest in the education sector. Thus, with a higher level of GDP, a country can invest more in education, which may lead to higher attainment rates in the future. The causal link between institutions and education is not straightforward and remains insignificant. Returning to hypotheses 5 and 6, we can reject the null hypothesis of the former while we cannot reject the null hypothesis of the latter. Table A3.9 in the Appendix presents the results, including time dummies. The results remain almost exactly the same and confirm those of the baseline estimation. GDP p.c. remains significant and positive, while liberal democracy is insignificant. Nevertheless, we treat our results as preliminary and will further test them in the following extensions section.

3.5 Extensions

Several factors might influence our results that we have yet to control for. First, we will focus on the estimation with GDP p.c. as the dependent variable. The coefficients, for example, might be driven by single countries only. Thus, we have again estimated the PMG model of equation 3.2, testing for the sensitivity of a reduction of countries. Figure A3.1 in the Appendix displays the point estimate and the 95%-confidence interval for liberal democracy when each of the countries is removed from the sample. The point estimate to the left shows the main "baseline estimation" from table 3.1. The second point estimate displays the results when Austria (AUS) is removed from the sample. This procedure is repeated until every country has been removed from the equation once. Similarly, figure A3.2 in the Appendix shows the sensitivity of the education variable when single countries are removed. Again, on the left, "MAIN" denotes the baseline estimation followed by the other countries. All point estimates remain positive and significant, confirming the results above. However, the magnitude of the coefficients slightly differs when removing single countries.

Figures A3.3 and A3.4 in the Appendix show the baseline results for both variables from equation 3.4 when countries are excluded from the sample. The long-run coefficient of GDP p.c. remains significant and positive, confirming the previous results. In contrast, the coefficient of secondary education is insignificant for most of the specifications. Table 3.2 provides a significant coefficient on the 10%-level. However, even on the 10%-level our results are not robust to the exclusion of single countries.

Figures A3.5 and A3.6 in the Appendix show the results for the exclusion of countries with respect to equation 3.5, where secondary education is the dependent variable. Similar to the previous results, the long-run coefficient of GDP p.c. is significant, while the liberal democracy coefficient is insignificant. Overall, most results are robust to removing single countries from the sample. Only secondary education does not seem to affect liberal democracy in the long-run.

Further, one could argue that the lag structure affects the PMG results. Even though the lag structure has been chosen based on the AIC, we will also look at the results of the ARDL(1,1,1), ARDL(2,1,1), ARDL(1,2,1), ARDL(1,1,2), ARDL(1,2,2), ARDL(2,2,1) and ARDL(2,1,2). For GDP p.c. as the dependent variable, they are presented in table A3.6 in the Appendix. Generally, the coefficients all remain stable throughout the different specifications and confirm the previously found results. Both, liberal democracy and secondary education, remain positive, significant and of roughly the same magnitude as in previous specifications. Table A3.8 in the Appendix shows the same for liberal democracy as the dependent variable. Again, the results for the convergence coefficient and GDP p.c. remain the same and stable. However, the long-run secondary education coefficient is either insignificant or positive and significant. This confirms the results from the previous exclusion of countries. Lastly, we also test the sensitivity to the lag structure when secondary education is the dependent variable. There, the structure of table A3.10 in the Appendix differs from those presented before. Since the model with only one lag of the dependent variable is not solvable, we have tested the sensitivity to a different lag structure only for the ARDL(2,1,1), ARDL(2,2,1) and ARDL(2,2,2). Remember that the AIC indicated that the ARDL(2,1,2) is the best choice in the baseline estimation. Again, the results remain similar to those of the previous estimations. GDP p.c. positively and significantly affects secondary education, while liberal democracy is insignificant.

Eventually, one could argue that, instead of using yearly data, it would be more appropriate to use 5-year averages. This might be advantageous since institutions and education usually only change slowly over time. When using averages, we also avoid putting too much emphasis on small changes. Finally, as explained above, the education data is only available every five years. Table A3.11 in the Appendix presents the PMG baseline results, including time dummies for all three dependent variables. In column one, we find similar positive and significant coefficients of the long-run estimates of liberal democracy and secondary education. In column 2 and 3, we also see a significant and positive effect of GDP p.c. on institutions and education, respectively. Additionally, liberal democracy does not seem to affect education in the long-run. Again, these results confirm the previous findings. Similar to the above, the long-run education coefficient falls out of line, when liberal democracy is the dependent variable. In column 2, we find a significant and negative effect on institutions. This result confirms the non-robustness of the effect of this variable on institutions.

We will now return to the hypotheses proposed in section 3.2. The null hypotheses 1 and 2, that institutions and human capital have no effect on economic growth, can both be rejected. The null hypotheses 3 and 5, that economic development has no effect on institutions and human capital, respectively, can also be rejected. Moreover, our results indicate that we cannot reject the null hypothesis 4 that human capital does not affect institutions. The same holds for null hypothesis 6, that institutions have no effect on human capital.

3.6 Conclusion

A large share of the literature and empirical analyses support the idea that both institutions and human capital (education) have a significant impact on GDP p.c. (growth). Our empirical analysis is in line with these findings. Based on the long time horizon, from 1870 until 2007, we are able to show the high impact of institutions and education on GDP p.c. But we do not stop at this point. Some strands of literature also provide four additional hypotheses: (1) GDP p.c. should positively influence institutions. (2) Also, human capital might affect institution-building in a positive manner. (3) GDP p.c. is claimed to have a positive influence on human capital because the richer a country is the more money can be spent to improve the education system. (4) Further, better institutions should support the education system, too. In order to test these hypotheses, we estimate a modified ARDL model, the error correction model, using the PMG estimator. Human capital is measured by the average years of secondary schooling between 15-64 years; economic development by GDP p.c., and as a proxy for institutions, we use the liberal democracy index, which indicates institutional constraints against arbitrary policy action of the state.

The model and the data seem to be suitable for analysing long-run equilibrium relationships between the variables GDP p.c., institutions, and human capital.

As mentioned above, our results confirm the hypothesis that institutions and education positively affect the GDP p.c.; this effect is stronger for institutions compared to education. Furthermore, we find that in the long-run, GDP p.c. has a positive effect on institutions. However, the same is not true for the relationship between education and institutions. In addition, our results show that GDP p.c. significantly and positively affects secondary education. Again, this is not the case for the effect of institutions on education. These findings hold if we extend the basic model by including time dummies, if we exclude single countries from the sample, and if we use different lag structures or average the data. Based on our results further research is necessary (a) to not only focus on GDP p.c. and its influencing variables but to examine the mutual relationship of institutions, human capital, and GDP p.c. more closely and (b) to shed more light on the different transmission mechanisms.

3.7 References

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3.8 Appendix

Table A3.1: Descriptive Statistics

	Obs.	Mean	Std. Dev.	Min	Max
GDP per capita	2747	12162.71	10779.54	1352.00	74879.81
Liberal democracy	2744	0.53	0.25	0.01	0.90
Secondary education	2760	1.53	1.63	0.00	7.45

Table A3.2: Tests for unit root

	IPS test		Fisher test	
Variable	Level	First difference	Level	First difference
Log GDP per capita	12.6696	-24.2116***	10.0948	-23.6456***
Log liberal democracy	1.9136	-22.7993***	2.0539	-22.4411***
Log secondary education	6.9561	-5.9711***	6.6765	-6.2476***

Notes: For the IPS test, 2 lags are used in the ADF regressions and the W-t-bar statistic is displayed. For the Fisher test, 2 lags are used in the ADF regressions and the Inverse normal Z-statistic is displayed. *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively.

Table A3.3: Tests for cointegration

Test	Statistic
Westerlund test	
Variance ratio	-1.9698**
Pedroni test	
Modified Phillips-Perron	1.4950*
Phillips-Perron	1.5522*
Augmented Dickey-Fuller	3.5074***

Notes: For the Westerlund test, the allpanels option is being used. For the Pedroni test, 2 lags are used in the ADF regressions. *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively.

Table A3.4: Lag order selection

Model	(1) AIC	(2) AIC	(3) AIC
ARDL(1,1,1)	-9230.12	-13899.01	-
ARDL(2,1,1)	-9234.00	-14125.03	-20220.79
ARDL(1,2,1)	-9210.13	-13884.07	-
ARDL(1,1,2)	-9182.09	-13818.35	-
ARDL(1,2,2)	-9233.80	-13907.24	-
ARDL(2,2,1)	-9312.41	-14215.13	-20204.45
ARDL(2,1,2)	-9257.69	-14146.32	-20223.85*
ARDL(2,2,2)	-9334.78*	-14235.08*	-20205.85

Notes: The table shows the AIC for different lag orders for the different models. Column (1) presents the models with GDP p.c. as the dependent variable, column (2) the models with liberal democracy as the dependent variable and column (3) the models with secondary education as the dependent variable. The star indicates which of the models is preferred according to the AIC.

Table A3.5: Baseline results GDP p.c. with time dummies

		(1) PMG	(2) PMG
Convergence coefficient	$LogGDPp.c._{t-1}$	-0.0201***	-0.0128***
		(0.0039)	(0.0027)
Long-run coefficients	$LogLiberalDemocracy_t$	2.5326***	2.5779***
		(0.5031)	(0.7854)
	$LogSecondaryEducation_t$	1.0310***	1.1321***
		(0.1110)	(0.1735)
	World War I (1914-18)	-2.2270***	-3.5073***
		(0.4876)	(1.1258)
	World War II (1939-45)	-1.3921***	-2.6626***
		(0.3463)	(0.9088)
	Great Depression (1929-30)		-0.9228
			(0.5657)
	Marshall Plan (1946-52)		1.6417***
			(0.6336)
	Oil Crisis (1973-74)		0.2368
			(0.4888)
	Fall of the Iron Curtain (1989-1990)		-0.4486
			(0.4978)
Short-run coefficients	$\Delta LogGDPp.c._{t-1}$	0.0870**	0.0691*
		(0.0378)	(0.0381)
	$\Delta LogLiberalDemocracy_t$	-0.0484	-0.0601
		(0.1219)	(0.1180)
	$\Delta LogLiberalDemocracy_{t-1}$	0.0221	0.0211
		(0.1138)	(0.1123)
	$\Delta LogSecondaryEducation_t$	0.1229	0.0918
		(0.1580)	(0.1590)
	$\Delta LogSecondaryEducation_{t-1}$	0.0457	0.0723
		(0.1783)	(0.1791)
	Constant	0.1630***	0.1097***
		(0.0287)	(0.0201)
	Number of observations	2681	2681
	Number of countries	20	20
	Number of periods	136	136

Notes: *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively.

Table A3.6: Sensitivity to lag structure GDP p.c.

		(1) ARDL(1,1,1)	(2) ARDL(2,1,1)	(3) ARDL(1,2,1)	(4) ARDL(1,1,2)	(5) ARDL(1,2,2)	(6) ARDL(2,2,1)	(7) ARDL(2,1,2)
Convergence coefficient	$LogGDPp.c._{t-1}$	-0.0232***	-0.0243***	-0.0218***	-0.0241***	-0.0224***	-0.0194***	-0.0249***
		(0.0041)	(0.0044)	(0.0038)	(0.0043)	(0.0039)	(0.0037)	(0.0045)
Long-run coefficients	$LogLiberalDemocracy_t$	3.3757***	2.9176***	3.2492***	3.2674***	3.1963***	2.5970***	2.8652***
		(0.4927)	(0.4422)	(0.5268)	(0.4687)	(0.5083)	(0.5246)	(0.4294)
	$LogSecondaryEducation_t$	0.8067***	0.8663***	0.8451***	0.8237***	0.8613***	1.0166***	0.8837***
		(0.1003)	(0.0928)	(0.1065)	(0.0972)	(0.1040)	(0.1144)	(0.0909)
	Constant	0.1836***	0.1934***	0.1736***	0.1907***	0.1785***	0.1579***	0.1980***
		(0.0295)	(0.0325)	(0.0277)	(0.0310)	(0.0286)	(0.0274)	(0.0335)
	Time dummies	YES	YES	YES	YES	YES	YES	YES
	Number of observations	2705	2686	2682	2687	2682	2681	2686
	Number of countries	20	20	20	20	20	20	20
	Number of periods	137	136	136	136	136	136	136

Notes: All specifications were estimated using the PMG estimator. *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively.

Table A3.7: Baseline results liberal democracy with time dummies

		(1) PMG	(2) PMG
Convergence coefficient	$LogLiberalDemocracy_t$	-0.0858***	-0.0856***
		(0.0255)	(0.0246)
Long-run coefficients	$LogGDPp.c._t$	0.0785***	0.0840***
		(0.0192)	(0.0192)
	$LogSecondaryEducation_t$	0.0458*	0.0416*
		(0.0235)	(0.0239)
	World War I (1914-18)	-0.0086	-0.0002
		(0.0207)	(0.0218)
	World War II (1939-45)	-0.2508***	-0.2687***
		(0.0193)	(0.0210)
	Great Depression (1929-30)		0.0085
			(0.0315)
	Marshall Plan (1946-52)		0.1133***
			(0.0208)
	Oil Crisis (1973-74)		0.0069
			(0.0312)
	Fall of the Iron Curtain (1989-1990)		-0.0027
			(0.0314)
Short-run coefficients	$\Delta LogLiberalDemocracy_{t-1}$	0.2335***	0.2212***
		(0.0346)	(0.0361)
	$\Delta LogGDPp.c._t$	0.0465*	0.0391
		(0.0276)	(0.0256)
	$\Delta LogGDPp.c._{t-1}$	-0.0202	-0.0268
		(0.0289)	(0.0276)
	$\Delta LogSecondaryEducation_t$	0.0135	0.0370
		(0.0807)	(0.0796)
	$\Delta LogSecondaryEducation_{t-1}$	0.1372	0.1214
		(0.0963)	(0.0966)
	Constant	-0.0262***	-0.0303***
		(0.0076)	(0.0084)
	Number of observations	2681	2681
	Number of countries	20	20
	Number of periods	136	136

Notes: All specifications were estimated using the PMG estimator. *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively.

Table A3.8: Sensitivity to lag structure Liberal Democracy

		(1) ARDL(1,1,1)	(2) ARDL(2,1,1)	(3) ARDL(1,2,1)	(4) ARDL(1,1,2)	(5) ARDL(1,2,2)	(6) ARDL(2,2,1)	(7) ARDL(2,1,2)
Convergence coefficient	$LogLiberalDemocarcy_{t-1}$	-0.0745***	-0.0845***	-0.0758***	-0.0753***	-0.0765***	-0.0853***	-0.0848***
		(0.0251)	(0.0258)	(0.0253)	(0.0252)	(0.0254)	(0.0255)	(0.0258)
Long-run coefficients	$LogGDPp.c._t$	0.0767***	0.0865***	0.0604***	0.0780***	0.0652***	0.0764***	0.0873***
		(0.0204)	(0.0185)	(0.0207)	(0.0205)	(0.0206)	(0.0192)	(0.0186)
	$LogSecondaryEducation_t$	0.0383	0.0334	0.0589**	0.0374	0.0538**	0.0471**	0.0339
		(0.0249)	(0.0226)	(0.0251)	(0.0252)	(0.0251)	(0.0233)	(0.0229)
	Constant	-0.0202***	-0.0312***	-0.0105***	-0.0215***	-0.0138***	-0.0244***	-0.0321***
		(0.0069)	(0.0092)	(0.0040)	(0.0072)	(0.0049)	(0.0072)	(0.0094)
	Time dummies	YES	YES	YES	YES	YES	YES	YES
	Number of observations	2705	2682	2686	2687	2686	2681	2682
	Number of countries	20	20	20	20	20	20	20
	Number of periods	137	136	136	136	136	136	136

Notes: All specifications were estimated using the PMG estimator. *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively.

Table A3.9: Baseline results secondary education with time dummies

		(1) PMG	(2) PMG
Convergence coefficient	$LogSecondaryEducation_t$	-0.0076***	-0.0079***
		(0.0019)	(0.0019)
Long-run coefficients	$LogGDPp.c._t$	0.7450***	0.7499***
		(0.0256)	(0.0240)
	$LogLiberalDemocracy_t$	0.1595	0.0748
		(0.1245)	(0.1076)
	World War I (1914-18)	0.0220	0.0110
		(0.0580)	(0.0506)
	World War II (1939-45)	0.2483***	0.2094***
		(0.0622)	(0.0541)
	Great Depression (1929-30)		-0.0896
			(0.0770)
	Marshall Plan (1946-52)		0.1095**
			(0.0487)
	Oil Crisis (1973-74)		0.0124
			(0.0781)
	Fall of the Berlin Wall (1989-1990)		0.0910
			(0.0563)
Short-run coefficients	$\Delta LogSecondaryEducation_{t-1}$	0.8706***	0.8694***
		(0.0109)	(0.0112)
	$\Delta LogLiberalDemocracy_t$	0.0094	0.0089
		(0.0062)	(0.0063)
	$\Delta LogGDPp.c._t$	-0.0022	-0.0031
		(0.0025)	(0.0025)
	$\Delta LogGDPp.c._{t-1}$	-0.0023	-0.0030
		(0.0028)	(0.0029)
	Constant	-0.0450***	-0.0466***
		(0.0118)	(0.0118)
	Number of observations	2686	2686
	Number of countries	20	20
	Number of periods	136	136

Notes: All specifications were estimated using the PMG estimator. *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively.

Table A3.10: Sensitivity to lag structure Secondary Education

		(1) ARDL(2,1,1)	(2) ARDL(2,2,1)	(3) ARDL(2,2,2)
Convergence coefficient	$LogSecondaryEducation_{t-1}$	-0.0075***	-0.0076***	-0.0078***
		(0.0019)	(0.0019)	(0.0019)
Long-run coefficients	$LogLiberalDemocracy_t$	0.1652	0.1219	0.1014
		(0.1296)	(0.1298)	(0.1214)
	$LogGDPp.c._t$	0.7406***	0.7449***	0.7505***
		(0.0264)	(0.0266)	(0.0254)
	Constant	-0.0439***	-0.0447***	-0.0458***
		(0.0112)	(0.0115)	(0.0118)
	Time dummies	YES	YES	YES
	Number of observations	2687	2682	2681
	Number of countries	20	20	20
	Number of periods	136	136	136

Notes: All specifications were estimated using the PMG estimator. *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively.

Table A3.11: 5-year average estimation

		(1) GDP	(2) Institutions	(3) Education
Convergence coefficients		-0.1081***	-0.1966***	-0.1275***
		(0.0267)	(0.0499)	(0.0314)
Long-run coefficients	$LogLiberalDemocracy_t$	1.5763***		0.1006
		(0.2935)		(0.1097)
	$LogSecondaryEducation_t$	1.2454***	-0.2837***	
		(0.0759)	(0.0427)	
	$LogGDPp.c._t$		0.2950***	0.7330***
			(0.0286)	(0.0207)
	WWI	-1.1019***	0.1006***	0.0127
		(0.2024)	(0.0264)	(0.0423)
	WWII	-0.6326***	-0.4422***	0.1813***
		(0.1804)	(0.0460)	(0.0627)
Short-run coefficients	$\Delta LogGDPp.c._t$		-0.0454	-0.0122
			(0.0704)	(0.0235)
	$\Delta LogGDPp.c._{t-1}$	0.1355***	-0.0930**	-0.0417
		(0.0474)	(0.0373)	(0.0304)
	$\Delta LogLiberalDemocracy_t$	-0.5494**		0.0632
		(0.2712)		(0.0667)
	$\Delta LogLiberalDemocracy_{t-1}$	0.3140*	0.1889***	
		(0.1680)	(0.0712)	
	$\Delta LogSecondaryEducation_t$	0.2750**	0.0759	
		(0.1131)	(0.0649)	
	$\Delta LogSecondaryEducation_{t-1}$	-0.2092	0.2069***	0.6918***
		(0.1477)	(0.0779)	(0.0410)
	Constant	0.8753***	-0.3973***	-0.7449***
		(0.1978)	(0.1009)	(0.1855)
	Number of observations	514	514	515
	Number of countries	20	20	20
	Number of periods	26	26	26

Notes: All specifications were estimated using the PMG estimator. *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively.

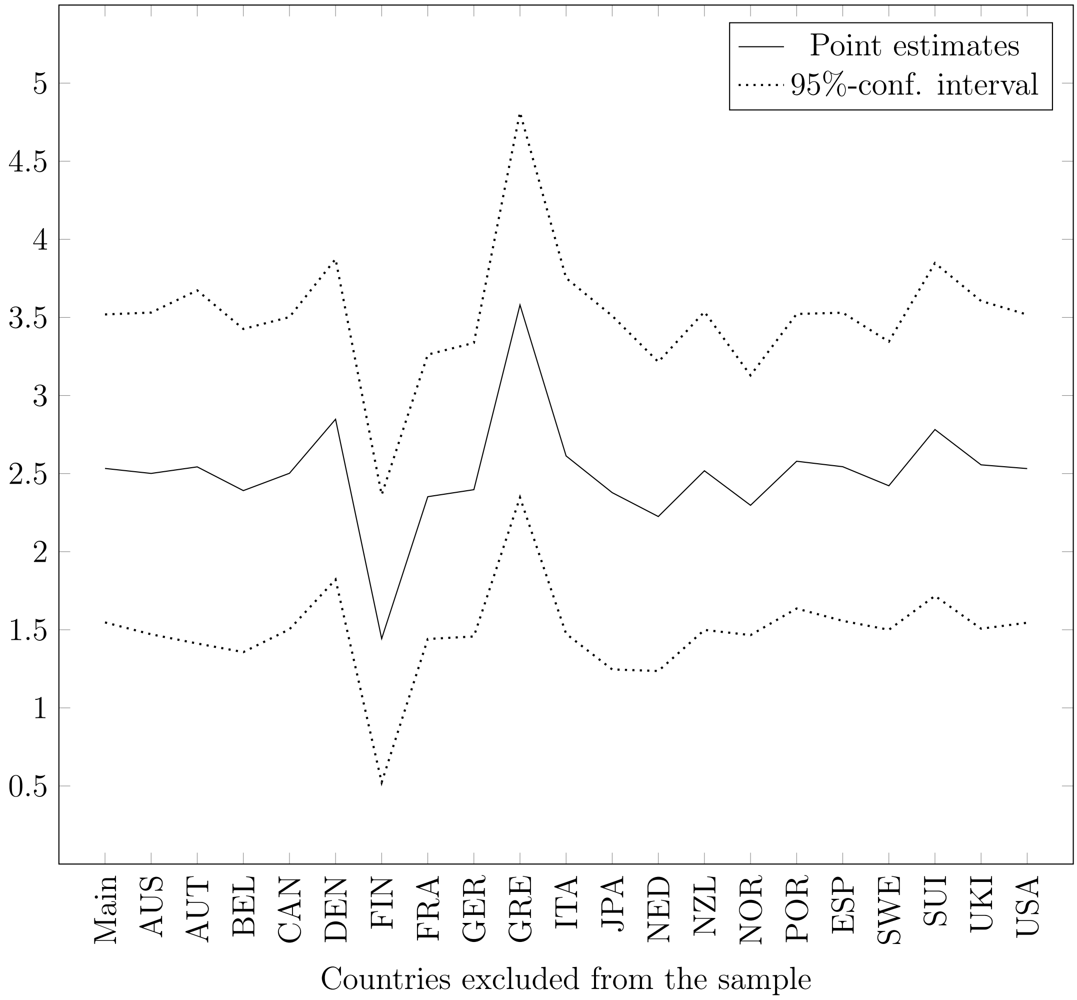

Figure A3.1: Baseline GDP estimation: Sensitivity of liberal democracy coefficient

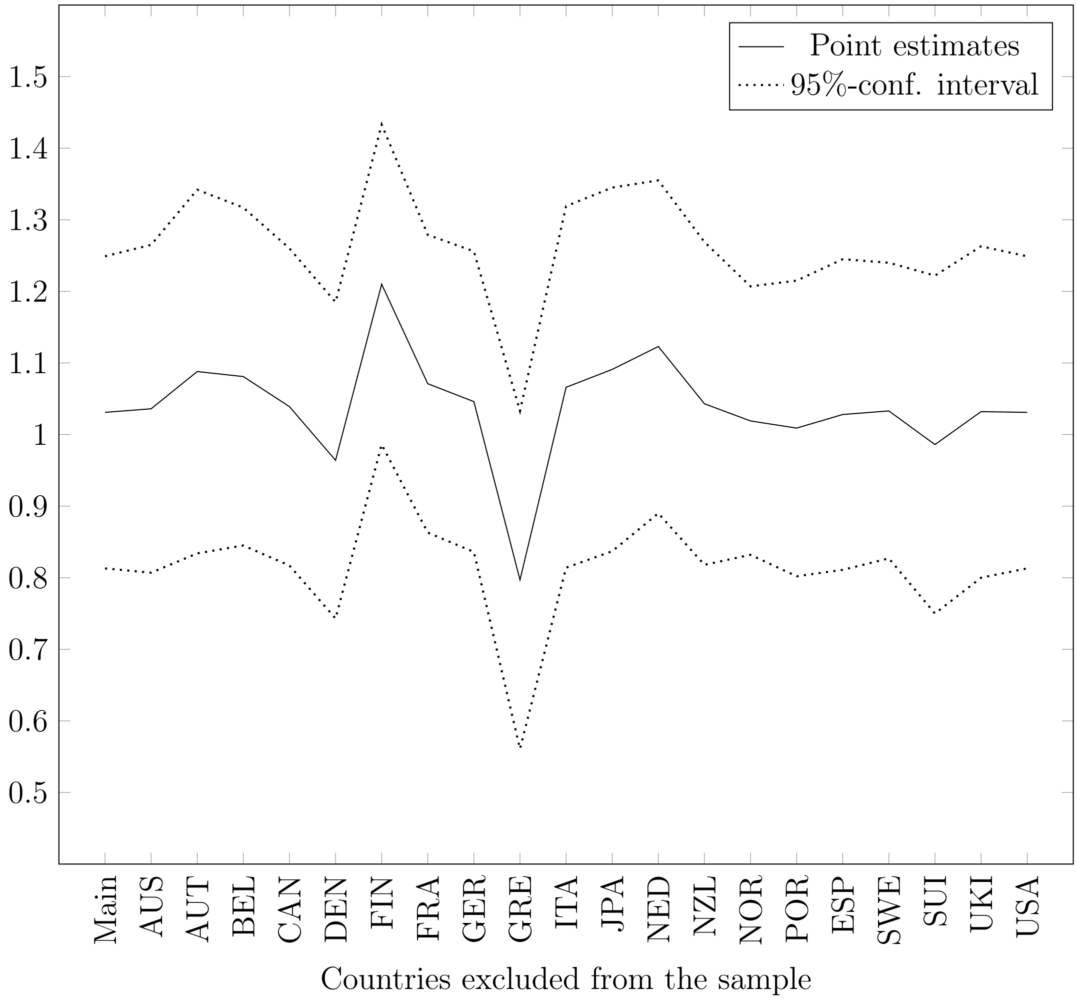

Figure A3.2: Baseline GDP estimation: Sensitivity of secondary education coefficient

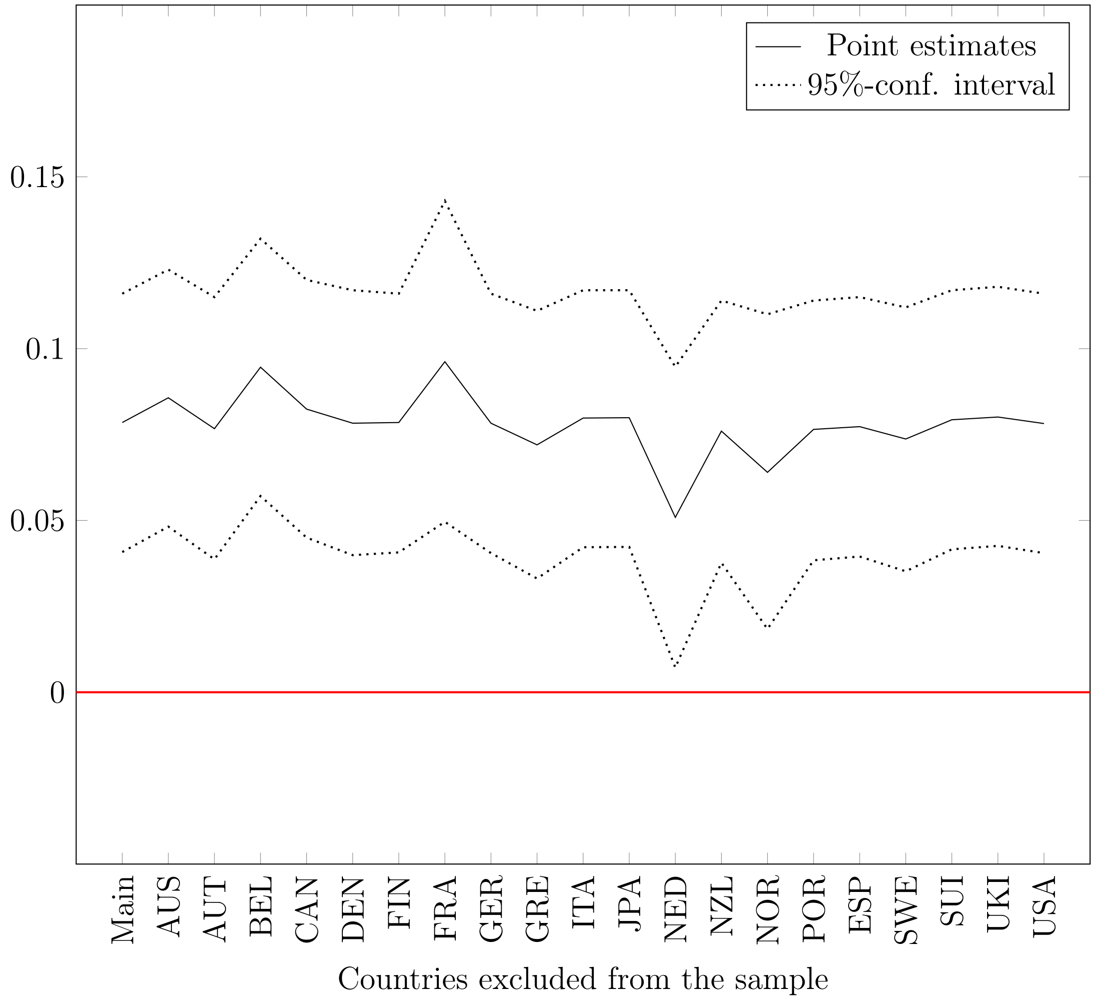

Figure A3.3: Baseline Liberal Democracy estimation: Sensitivity of GDP p.c. coefficient

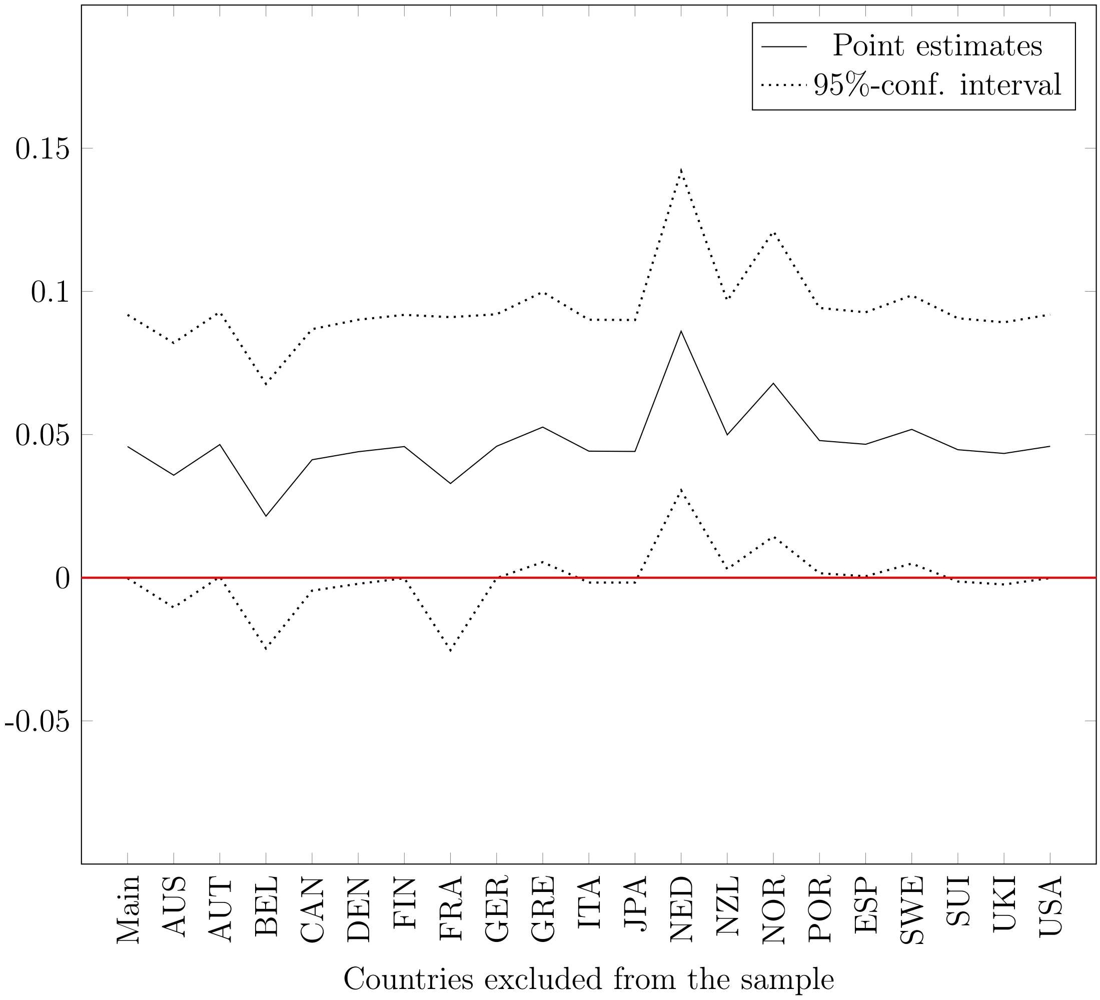

Figure A3.4: Baseline Liberal Democracy estimation: Sensitivity of Secondary Education coefficient

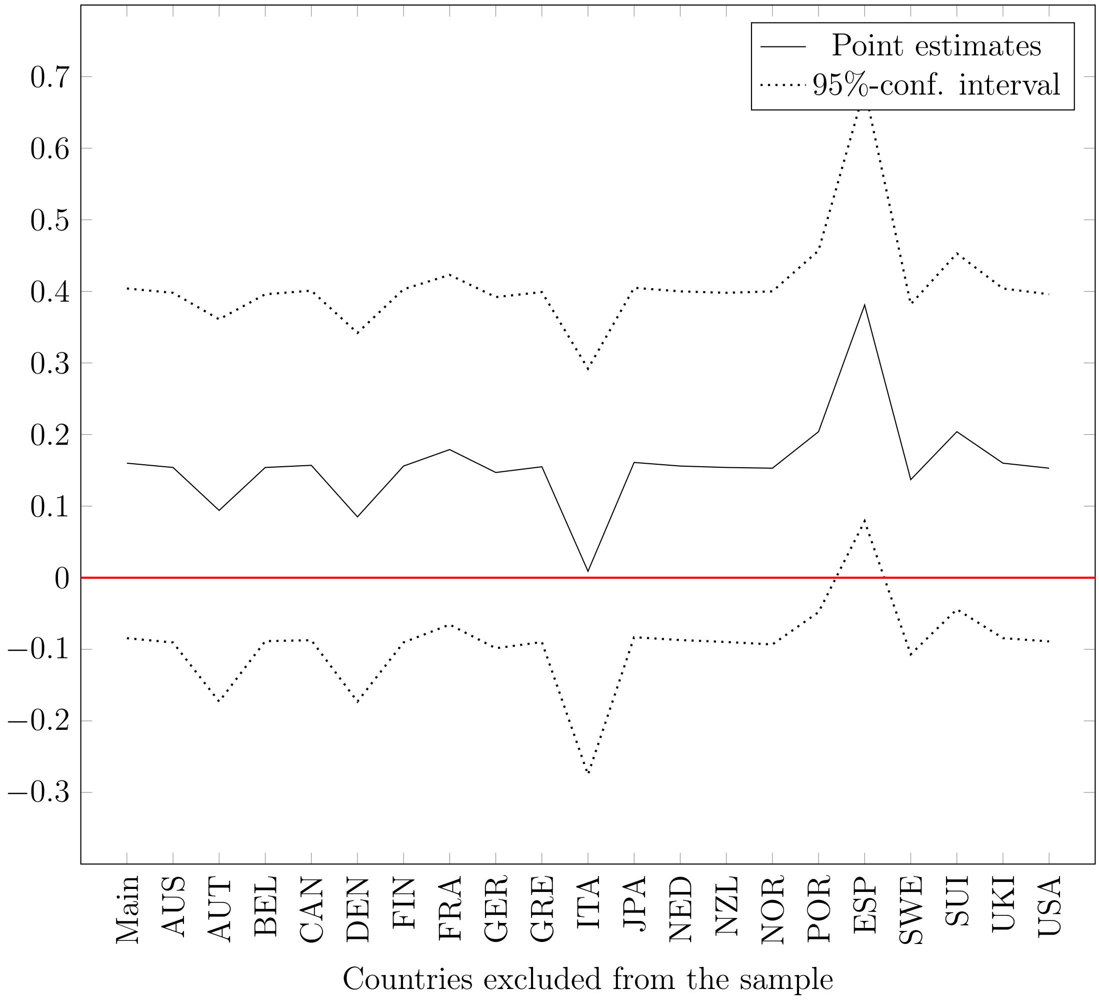

Figure A3.5: Baseline Secondary Education estimation: Sensitivity of Liberal Democracy coefficient

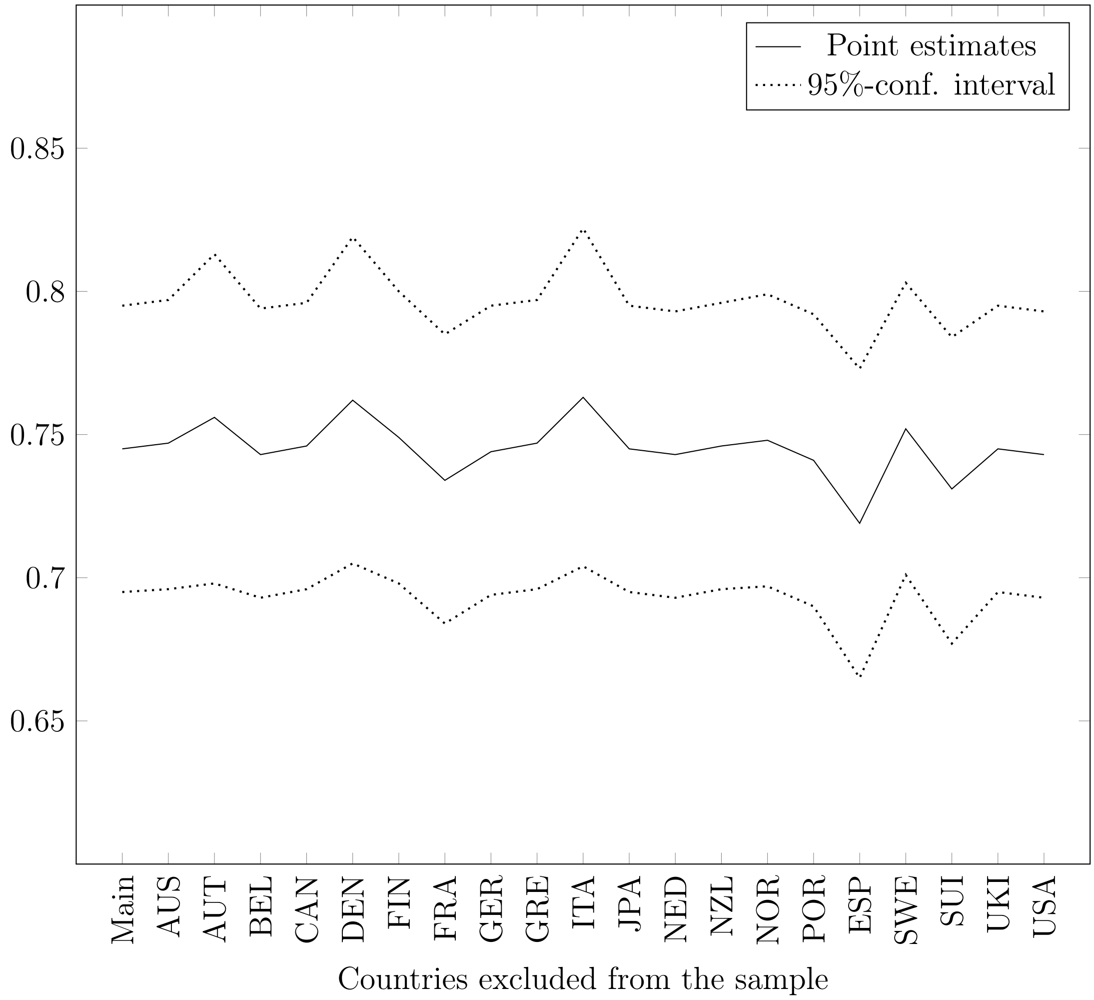

Figure A3.6: Baseline Secondary Education estimation: Sensitivity of GDP p.c. coefficient

Part 4

Do institutions matter in the very long-run? New evidence from OECD countries

4.1 Introduction

By the end of the last century, many economists, e.g. North (1990), emphasised the importance of institutions for economic growth. Buchanan (1975), for example, argued that institutions, such as property rights or binding rules for politicians, are important foundations for economic development since they prevent the government, amongst others, from arbitrary action. With such a constitutional framework, investment in physical and human capital will be higher and more efficient (Acemoglu, Johnson, and Robinson, 2001). In the past, researchers often treated institutions as constant in neoclassical growth models (e.g. Mankiw, Romer, and Weil, 1992). At the end of the 1990s, economists, e.g. Dawson (1998), extended this specification by including an institutional variable as a determinant of total factor productivity.

In the empirical literature, most studies only evaluate the effect of institutions on growth during the period between the Second World War and the beginning of the millennium. This period has been one of great progress in most western countries. Thus, the question arises if the results remain valid when examining longer periods.

In this study, we combine relatively new long-run datasets to estimate the effect of institutions on growth between 1870 and 2007. Our sample consists of 18 OECD countries. Since institutions mostly change slowly over time and thus most likely affect growth only over a more extended period, we estimate an error correction model that allows us to differentiate between short-run and long-run coefficients. This differentiation has two advantages. First, we can identify the long-run effect of institutions on economic growth.

Second, it is plausible to assume that the OECD countries differ in the short-run due to single shocks affecting only one country. However, in the long-run, countries may behave similarly since they are all affected by a common technology, institutions and spillover effects. Thus, the PMG estimator may better fit real-world developments than previous estimation approaches.

4.2 Methodology and data

Following Bassanini and Scarpetta (2002), we estimate a restricted version of the neoclassical growth equation, including institutions. The following error correction model is a reparameterised autoregressive distributed lag (ARDL)(2,2,2,1) model:

$$\begin{aligned}\Delta y_{it} = -\phi(y_{i,t-1} - \theta_{1i}d_{it} - \theta_{2i}h_{it} - \theta_{3i}s_{it}) + \lambda_i \Delta y_{i,t-1} + \delta_{1i}\Delta d_{it} + \delta_{2i}\Delta d_{i,t-1} \\ +\delta_{3i}\Delta h_{it} + \delta_{4i}\Delta h_{i,t-1} + \delta_{5i}\Delta s_{it} + \delta_{6i}\Delta s_{i,t-1} + \mu_i + \epsilon_{it},\end{aligned} \tag{4.1}$$

where y_{it} is GDP p.c., d_{it} is the level of institutions, h_{it} is the human capital stock and s_{it} are the capital flows in country i at time t. ϕ denotes the error correction parameter and can be interpreted as the speed of adjustment to equilibrium. Since we would like to estimate the effect of institutions on economic growth in the long-run, we need to combine several novel databases. Our dependent variable, GDP p.c. is taken from Bolt and Van Zanden (2020). The main variable of interest is the liberal democracy index from Coppedge et al. (2022) as a proxy for institutions. This variable takes a negative view on arbitrary policy action and emphasises the importance of the protection of individual rights by the constitutional framework of a country. As in previous studies, we use secondary educational attainment from Barro and Lee (2015) as a proxy for the human capital stock in a country. Finally, we include the investment-to-GDP ratio from Jordà, Schularick, and Taylor (2017). Since our dynamic heterogeneous panel has a relatively long time horizon and a medium number of cross-sections, we follow Pesaran, Shin, and Smith (1999) and make use of three possible estimators: First, we estimate our model using the dynamic fixed effects estimator (DFE). Since this is a pooled estimator, all countries are constrained to have the same slope parameter. Pesaran and Smith (1995)

Table 4.1: Baseline results

	(1) PMG	(2) MG	Hausman test	(3) DFE
Convergence coefficient				
$LogGDPp.c._{t-1}$	-0.0229***	-0.0383***		-0.0147***
	(0.0071)	(0.0092)		(0.0038)
Long-run coefficients				
$LogLiberalDemocracy_t$	3.8335***	8.5864**		2.6054***
	(0.5558)	(4.0005)		(0.8107)
$LogSecondaryEducation_t$	0.6209***	-0.4174		0.8737***
	(0.0992)	(1.3388)		(0.1771)
$LogInvestment_t$	2.8552***	17.7537		7.2078***
	(0.6742)	(14.1944)		(2.0498)
			2.76	
Short-run coefficients				
$\Delta LogGDPp.c._{t-1}$	0.1215***	0.1008***		0.1632***
	(0.0311)	(0.0343)		(0.0205)
$\Delta LogLiberalDemocracy_t$	-0.2267	-0.2946		0.0876**
	(0.1410)	(0.1795)		(0.0401)
$\Delta Logliberaldemocracy_{t-1}$	-0.0377	-0.1212		0.0217
	(0.1313)	(0.1578)		(0.0374)
$\Delta LogSecondaryEducation_t$	0.0321	-0.0057		-0.0684
	(0.1152)	(0.1149)		(0.1327)
$\Delta LogSecondaryEducation_{t-1}$	-0.0211	-0.0375		0.1406
	(0.1710)	(0.1251)		(0.1342)
$\Delta LogInvestment_t$	0.4559***	0.3964***		0.4536***
	(0.0691)	(0.0650)		(0.0523)
$Constant$	0.1687***	0.2245***		0.1059***
	(0.0452)	(0.0813)		(0.0285)
Number of observations	2138	2138		2138
Number of countries	18	18		18
Number of periods	136	136		136

Notes: The Hausman test indicates that the PMG estimator is preferred over the MG estimator. *, ** and *** denote significance at the 10%-, 5%- and 1%-level.

argue that the DFE estimator in such settings is inconsistent and biased. Thus, our second option is the mean group estimator (MG). Here, all countries are estimated separately, and the respective coefficients' average is taken afterwards. The third possibility is to combine both estimators to the PMG estimator. There, we assume differences across countries in the short-run and thus use the MG estimator, and similarities in the long-run and thus use the DFE estimator.

4.3 Results

Table 1 reports the baseline results for equation 4.1 for the PMG, MG and DFE estimator. We already know that the DFE estimator is inconsistent and biased. However, we still need to assess if the additional long-run homogeneity assumptions of the PMG estimator are efficient compared to the MG estimator. The Hausman test, in column 3, provides evidence in favour of the PMG estimator. Thus, in the following, we will focus on the PMG estimator only. Since the convergence coefficient is significant and negative, we find evidence for convergence to the long-run equilibrium. This implies that short-run deviations from the long-run equilibrium path are corrected. When looking at the long-run coefficients, we find overall positive and significant results. The results of the standard growth model hold in the very long-run. Investment and human capital are important determinants of economic development. Our variable of interest, liberal democracy, exhibits a strong and positive effect on GDP p.c. growth. We find that institutions significantly push total factor productivity. In contrast, most of the short-run coefficients are insignificant. Only investment has a positive short-run effect, providing evidence for the Keynesian demand effect. In such settings, the chosen lag structure may be a critical determinant of the coefficients. Thus, in table 4.2, columns 1-5 present different lag structures (incl. the main specification in column 3 as a reference). All estimates are PMG estimates. We also report the Akaike information criteria (AIC), which was used to determine the optimal lag length in the first place. The results are robust to different lags and only slightly change in magnitude. In addition, we assess robustness by controlling for time-specific effects in the long-run estimation. Thus, we have added 10-year dummies starting with the second period (1880-1889) in order to control for such effects. Column 6 shows the results when these dummies are included. Since these time fixed-effects absorb some of the effects, the magnitude of all coefficients decreases. Nevertheless, all variables remain significant and positive. Lastly, we add two world war dummies to control for

Table 4.2: Extensions

	(1) ARDL (1,1,1,1)	(2) ARDL (1,2,2,1)	(3) ARDL (2,2,2,1)	(4) ARDL (2,1,1,1)	(5) ARDL (1,2,2,2)	(6) 10 year Dummies	(7) War Dummies
Convergence coefficient							
$LogGDPp.c._{t-1}$	-0.0238***	-0.0227***	-0.0229***	-0.0236***	-0.0220***	-0.0308***	-0.0197***
	(0.0070)	(0.0069)	(0.0071)	(0.0072)	(0.0066)	(0.0086)	(0.0060)
Long-run coefficients							
$LogLiberalDemocracy_t$	3.8199***	3.8928***	3.8335***	3.8218***	3.9709***	1.1515***	4.3865***
	(0.5555)	(0.5786)	(0.5558)	(0.5604)	(0.6354)	(0.2841)	(0.6936)
$LogSecondaryEducation_t$	0.6124***	0.6033***	0.6209***	0.6250***	0.6195***	1.1702***	0.5310***
	(0.0953)	(0.1021)	(0.0992)	(0.0954)	(0.1080)	(0.0899)	(0.1141)
$LogInvestment_t$	3.4616***	3.4838***	2.8552***	2.7325***	3.5654***	2.4972***	3.0029***
	(0.6771)	(0.7054)	(0.6742)	(0.6593)	(0.7458)	(0.7035)	(0.7626)
Number of observations	2154	2139	2138	2140	2122	2138	2138
Number of countries	18	18	18	18	18	18	18
Number of periods	137	137	136	136	136	136	136
AIC	-8385.45	-8405.89	-8467.70	-8392.35	-8399.51	-	-

Notes: *, ** and *** denote significance at the 10%-, 5%- and 1%-level. The ARDL denotes the number of lags for the dependent variable and the independent variables in the order of the variables denoted in the output. In columns 6 and 7, 10-year and war dummies are included, respectively.

the war-specific effects on the sample. The World War I dummy captures the period between 1914 and 1918, and the World War II dummy captures the period between 1938 and 1945. Again, the results remain positive and significant. Interestingly, the effect of liberal democracy is the strongest in this specification. This points at the adverse effects of the war, which were not accounted for in the baseline estimation and thus extenuated the effect of liberal democracy. Eventually, we test the robustness of the baseline PMG results. Figure 4.1 shows the point estimates and 95%-confidence interval of the liberal democracy variable when different countries are excluded from the sample. "Main" denotes the point estimate from the baseline estimation. We find that the results are robust to the exclusion of different countries.

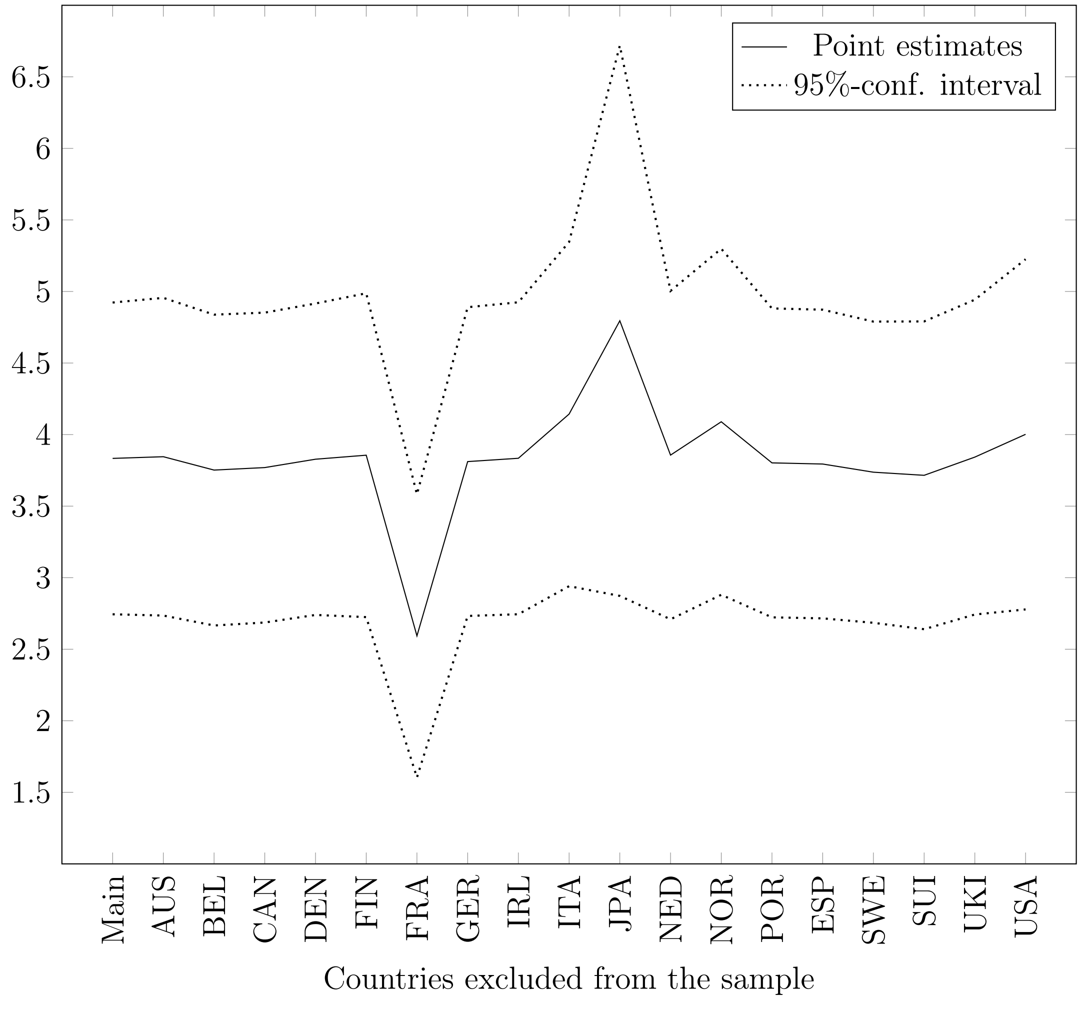

Figure 4.1: Sensitivity of liberal democracy

4.4 Conclusion

This article analysed the relationship between institutions and economic growth in an augmented neoclassical growth model in 18 OECD countries over 138 years. We find a significant and positive effect of the institutional variable on GDP p.c. growth. This effect is robust to different lag structures, the inclusion of dummy variables, war dummies and the exclusion of countries from the sample. As institutions critically impact total factor productivity and economic growth, they should be systematically included in standard growth models.

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Part 5

Dream of Californication - Trade, Human Rights and the California Effect

5.1 Introduction

Recent geopolitical events have led to a public condemnation of long-believed narratives. In Germany, one such narrative is called "Wandel durch Handel" or change through trade (Bahr, 1963; The Economist, 2022; Moens, 2022). This idea stems from the Cold War era, postulating that Western countries could induce democratic change in the Soviet Union through trade and cooperation instead of pure confrontation. After the fall of the iron curtain, the "Wandel durch Handel" idea lived on and until recently was often applied when discussing trade between Western countries and Russia. Initially, the idea was based on a speech held by German politician Egon Bahr (1963). One widespread interpretation of this idea was that if Europe imported Gas from Russia, this might have a positive effect on Russian institutions due to institutional spillover. Thus, when Russia invaded Ukraine in early 2022, many people in the West believed that this principle had failed (Dodman, 2022; Deutsche Welle, 2022; Strupczewski, 2022). One might conclude, somewhat pessimistically, that institutional spillovers via trade are at best small and at worst non-existent.

However, deriving this conclusion from a single experience, i.e., the Western European experience with Russia, could be considered eurocentric. What did not work with Russia in the past might still work in other constellations. To understand why a convergence of trading partners' institutions might occur or not, it is worthwhile to think about the mechanisms at play which may induce such a change. Countries or firms are often not put under pressure by other countries' governments but rather by non-governmental actors such as consumers, firms, or non-governmental organizations. Incidents like the Rana Plaza collapse in 2013 led to public outrage about workers' safety conditions in the textile industries, especially in lower-income countries (e.g. Chandran, 2016). Parts of the civil society often argue that Western countries should not buy products from lower-income countries in which human rights are not respected.[1] This may put pressure on these countries to improve their human rights standards in order for domestic firms to gain access to export opportunities. The outcome of this phenomenon is consistent with the "Wandel durch Handel" narrative. It posits that human rights standards of countries in which these standards are high spill over to countries with low human rights standards if these are allowed to trade with them. That is, we can think of countries with low human rights standards as exporting goods and services to countries with high human rights standards and importing their regulations, values, or institutions. This is often referred to as the "California effect" (Vogel, 1997). In recent years, Western countries tried to push for such changes by introducing further regulation on supply chains (e.g. Bundesministerium der Justiz, 2021).

The hypothesis of the "California effect" hence proposes that change through trade occurs through exporting firms and their governments being pressured to adjust their human rights standards to the preferences of consumers, non-governmental organizations, and governments of importing countries in order to gain market access. This may explain why change through trade does not always "work": A high bargaining power of exporting firms and countries may undermine the "California effect". Hence the market structure of the goods and services being traded may determine whether convergence in human rights standards occurs through trade or not. Products such as oil and gas which are strategic resources for importing countries for which trading partners cannot be easily replaced can thus be expected to be less effective as a transmission vehicle of the "California effect" than others.

In this article, we intend to examine human rights spillovers through trade empirically. We try to answer three questions: First, do human rights in importing countries affect human rights in exporting countries? Second, do human rights in powerful importing countries affect human rights in less powerful exporting countries (California effect)? Third, is there

[1] See for example https://www.business-humanrights.org/en/ or https://www.ethicalconsumer.org/ethicalcampaigns/boycotts.

a difference concerning the previous question when differentiating between resource and non-resource exports?

We start with a simple bilateral ordinary least squares (OLS) estimation, including an export-weighted average of human rights. Then, we apply the generalized spatial two-stage least squares (GS2SLS) estimator proposed by Badinger and Egger (2011) to estimate a spatial autoregressive (SAR) model with spatially dependent errors by applying the generalized spatial two-stage least squares estimator. We try to overcome the problem of an endogenous weighting matrix by applying the control function approach by Qu, Lee, and Yang (2021).

This article contributes to the literature in several ways. While the international diffusion of human rights via trade flows has been studied before (e.g. Cao, Greenhill, and Prakash, 2013), there are various characteristics which make our approach unique. To the best of our knowledge, this is the first study which examines spillovers which are expected under the hypothesis of the "California effect" in particular. We do so by splitting up the weighting matrix of interest into two matrices, one containing trade flows relevant for the "California effect" and one containing all other trade flows. In previous studies two additional problems arose. Often spatial studies only include a small fraction of countries and only one weighting matrix accounting for spatial dependence. In this article, we include up to 173 countries in our sample and we simultaneously control for several weighting matrices, thus limiting the endogeneity problem. Further, we apply the recent approach by Qu, Lee, and Yang (2021) to deal with endogenous weighting matrices.

Our results provide evidence for the existence of spillovers of human rights through trade. Moreover, when estimating the California effect, we find significant and positive effects. However, the effects are only significant from 2010 onwards. They remain significant when adding additional weighting matrices and when applying the control function approach. Our extensions concerning the differentiation of resource and non-resource exports provide similar results. The California effect for non-resource exports is significant and positive. Interestingly, we also find evidence for the California effect when looking at resource exports.

The remainder of this paper is structured as follows: Section 5.2 provides an overview of the effect of trade on human rights in general, an introduction to the California effect, and other spatial approaches in the field of human rights. In section 5.3, the empirical specification is introduced, the data is briefly described. Also, origins of and solutions to potential issues of endogeneity are discussed. Section 5.4 presents the results with respect to the hypotheses formulated and section 5.5 extends our approach to resource exports and a different groupings of countries. Section 5.6 summarizes the results and concludes.

5.2 Literature Review

We start our literature review rather broadly by looking at the relationship between institutions and trade in general. This literature is important since it provides insights about the general effect of trade relationships between countries. Several empirical studies find a positive link between trade openness and democracy (López-Córdova and Meissner, 2008; Liu and Ornelas, 2014). Proponents of such a positive link often do not argue for a direct effect of trade on democratization, but rather hypothesize an indirect link between the two (López-Córdova and Meissner, 2008). One such indirect link is proposed to be economic growth, with international trade leading to increased growth (Alcala and Ciccone, 2004; Frankel and Romer, 1999; Feyrer, 2019), which in turn fosters democratization and institutional development (Lipset, 1959; Barro, 1999; Heid, Langer, and Larch, 2012). A second variable which may provide a channel for an indirect effect of trade on institutional development is income inequality. According to Acemoglu and Angrist (2000) democratization can work as a commitment device for elites to give the prospect of future redistribution efforts, in order to prevent social unrest in the face of rising inequality. By affecting income inequality within countries due to Stolper-Samuelson effects, international trade can thus affect democratization (Acemoglu and Robinson, 2006). Liu and Ornelas (2014) further argue, that increased international trade through participation in free trade agreements reduces protectionist rents and thus reduces the incentive of autocratic groups to seek power in order to get a hold of them. Thus, the authors argue, free trade agreements can have a stabilizing effect on democracy.

Additionally, some studies look at the effect in the opposite direction, examining if trade is affected by democracy. Again, the literature provides evidence for a positive relationship between democracy and trade (Milner and Kubota, 2005; Aidt and Gassebner, 2010) and democracy and foreign direct investment (FDI) (Lacroix, Méon, and Sekkat, 2021).

While these studies examine the effect of trade on institutional development in general, they focus primarily on democratization and they do not examine heterogeneity in the effects depending on the trading partners. Such a heterogeneity would be expected if trade flows merely provide the vehicle for international spillovers of institutions to take place. In this study we examine trade-induced international spillovers of institutions other than democracy. Specifically, we look at human rights standards. Thus, we will now examine the literature on policy diffusion in more detail. Dobbin, Simmons, and Garrett (2007) provide an introduction to different theories of policy diffusion from a social science point of view. They compare constructivism, coercion theory, competition theory, and learning theory and provide detailed explanations of each of the theories. Our argument in this article will be most closely related to constructivism. Cao, Greenhill, and Prakash (2013) provide an intuitive explanation for the international diffusion of human rights standards through trade. They base their argument on insights by Vogel (1997) regarding the geographic spread of regulation standards.

Vogel (1997), who examines the diffusion of environmental standards within the United States (US), describes two opposing scenarios of how trade and regulation could affect each other. First, regulation could lead to costly and complicated processes, which makes products uncompetitive on the world market. Thus, a country would need to lower its standards in order to be competitive again. If other countries respond to this lowering of standards, we could end up in a downward spiral, or race to the bottom. This race to the bottom is also known as the “Delaware effect” since Delaware was well-known for having a very low standard of corporate chartering law. Second, countries could instead introduce higher standards for their domestic markets, making market access for foreign firms or products subject to adherence to these standards. Vogel (1997) adds that, in general, producers favor similar regulations across countries since this lowers their production cost. The European Union (EU), for example, adopted German car emission regulation in the 1980s. They did not only choose the German regulations since Germany is the largest car exporter in Europe but also since the German regulation was similar to the one in the US, which was one of the most important export destinations back then. This phenomenon, that exporting countries align their regulation to export destinations with stricter regulation, is called the "California effect". The name stems from the example of California which had the highest environmental standards among the US states. Instead of imposing lower standards, many other states followed California by setting standards of the same level as California. Vogel (1997) identifies three mechanisms which lead to the California effect: First, stricter regulation may be a competitive advantage for domestic firms in potential export destinations as it serves as a trade barrier for foreign competitors. Thus, domestic firms will be in favor of such regulation and foreign firms seeking to gain market access may lobby their respective governments to align the regulation. Second, richer countries with higher standards restrict foreign firms' market access to the adherence to these standards. If foreign firms follow these stricter standards, foreign standards might increase as well. Third, free trade negotiations might be a tool for richer nations to pressure their counterparts into adopting higher standards. The question remains if the empirical literature supports the California effect or the Delaware effect.

The literature on spillovers of institutions through trade or trade-based diffusion mainly focuses on labor market regulation, democratic governance, liberalization and women's empowerment. Bernauer and Caduff (2004) directly provide support for the argument made by Vogel (1997). They argue that environmental regulation has become stricter since the 70s and that this has been forced by large green jurisdictions as trading partners. A large fraction of studies focuses on labor market regulation. Drezner (2001) provides an overview of studies examining the effect of globalization on labor standards. Overall, he does not find conclusive evidence for a race to the bottom but rather positive effects of globalization on labor standards. This positive evidence is also supported with respect to FDI stocks and flows (Messerschmidt and Janz, 2023; Mosley and Uno, 2007). Additionally, trade seems to be positively related to a lower level of child labor (Davies and Voy, 2009; Neumayer and Soysa, 2005). However, Lim and Prakash (2017) show that worker safety in the global south is affected by economic conditions in their exports markets in the global north. Greenhill, Mosley, and Prakash (2009) find that strong legal protection of labor rights in the importing country is associated with relatively stronger labor rights in the partner country. This is in line with the hypothesis of Vogel (1997). Other strands of the literature look at the spillover of democracy (e.g. Gleditsch and Ward, 2006; Simmons and Elkins, 2004; Doces and Magee, 2015) or women's rights (e.g. Potrafke and Ursprung, 2012; Neumayer and Soysa, 2007). The effects of trade and globalization are largely positive. Another related debate discusses the effect of trade between developed and developing countries on women's wages and employment (Wood, 1991; Kucera and Milberg, 2000; Berik, van der Rodgers, and Zveglich, 2004; Oostendorp, 2009).

In order to examine spillover of human rights through trade, we will next examine the study by Cao, Greenhill, and Prakash (2013) who extend the argument of Vogel (1997) to the case of human rights. They argue that stakeholders, non-governmental organizations, trade unions and consumer groups in exporting countries might pressure firms and the government in exporting countries to push for improvements of human rights in importing countries. Additionally, exporting firms might be concerned to lose access to foreign markets if human rights standards in their countries diverge too far from human rights standards in their export destinations. Thus, these firms will also lobby the government in the exporting countries to increase the human rights standards. The findings of Peterson, Murdie, and Asal (2018) support this argument. If countries abuse human rights and human rights organizations shame this abuse, then this will lead to relatively lower exports. However, it will not affect exports if importers have a similar level of human rights abuse.

The empirical examination of trade induced human rights spillovers has been the aim of earlier research. Cao, Greenhill, and Prakash (2013) conduct an empirical analysis to find support for their hypothesis of human rights spilling over to trade partners in accordance with the California effect. They use the physical integrity rights index by Cingranelli, Richards, and Clay (2021) to measure the adherence to human rights standards within countries and examine how a trade weighted average of human rights in trading partners affects human rights at home. Our approach differs from that of Cao, Greenhill, and Prakash (2013): While the approach of Cao, Greenhill, and Prakash (2013) is appropriate to examine the existence of human rights spillovers via trade overall, it cannot answer the question of whether the spillovers actually occur between those groups of countries between which one would expect them in accordance with the California effect. That is, the approach does not allow for a separate examination of human rights spillovers from importing countries with high human rights to exporting countries with low human rights standards. Our study aims at addressing this shortcoming through the construction of differentiated weighting matrices which are simultaneously included in the spatial regression. This is one of our key contributions to the literature. Similarly, Neumayer and Soysa (2011) examine the effect of a spatially lagged women's rights indicator on women's rights to analyze women's rights spillovers through trade. They too, however, do not differentiate different country groups when it comes to the spillovers. Another difference between our study and that of Cao, Greenhill, and Prakash (2013) is the data on human rights used for the analysis. The physical integrity rights index by Cingranelli, Richards, and Clay (2021) which Cao, Greenhill, and Prakash (2013) use measures human rights adherence on an ordinal scale ranging from 0 to 3. In this study, we use a human rights measure from the v-dem dataset by Coppedge, Gerring, Knutsen, Lindberg, Teorell, Altman, Bernhard, Cornell, Fish, Gastaldi, Gjerløw, Glynn, God, et al. (2023) and Pemstein et al. (2023) which has the advantage of being more precise than the measure by Cingranelli, Richards, and Clay (2021).

Several studies use spatial econometric approaches in order to capture potential spillovers between countries or regions. Prominent examples can be found in the literature on the determinants of economic growth. Ho, Wang, and Yu (2013) and Amidi and Fagheh Majidi (2020), for example, use trade flows to examine spatial dependence of the gross domestic product (GDP). However, in this article, we will focus on spillovers of human rights. Bell, Clay, and Murdie (2012) analyze the spillover effects of international human rights organizations on neighboring countries. They find positive effects on the level of human rights of the neighboring country if countries share the same border and if members of the human rights organizations can move across the border. Similarly, Kopstein and Reilly (2000) analyze the transformation of former communist countries. They identify the distance to non-communist countries as one of the leading factors of transformation.

Chyzh (2016) examines spillovers of rule of law across countries. They find that both contiguity and shared membership in an international organization (IO) lead to positive spillover effects when estimated separately. However, when both weighting matrices are used in the same specification the effect of IO membership dominates the common border effect. Goderis and Versteeg (2014) analyze the diffusion of constitutional rights among countries. They find that spillovers occur through shared characteristics of countries, such as a common colonizer, a common legal origin, a common religion, or the same aid donor. Edwards et al. (2018) estimate the effect of human rights spillovers using distance as the transmission mechanism. They find positive spillovers among all countries in their sample. Faber and Gerritse (2017), on the other hand, do not find evidence of human rights spillovers using inverse distances as spatial weights in a panel setting. Regarding spatial spillovers of institutional quality, the empirical evidence is similarly inconclusive with Faber and Gerritse (2012) and Kelejian, Murrell, and Shepotylo (2013) finding positive geographic spillovers across countries whereas Claeys and Manca (2011) do not find such an effect. Naveed et al. (2023) find spatial dependence in women's rights standards for neighboring countries. Greenhill (2010) finds positive human rights spillovers between members of intergovernmental organizations.

Overall, our main contribution to the literature is the following: To the best of our knowledge, this is the first study examining spillovers expected under the "California effect" in a proper way. Previous studies have often not examined spillovers between country groups separately but overall. We do so by splitting up the relevant weighting matrix into two matrices. This allows us to examine exports from countries with relatively low human rights to countries with relatively high human rights as a potential channel of human rights spillovers.

5.3 Empirical Framework

We conduct four cross-section analyses for the time periods 2000–2004, 2005–2009, 2010–2014, and 2015–2019. Each cross-section includes up to 173 countries. Throughout the study, we estimate several variations of the following spatial autoregressive model:

$$y_i = \sum_{q=1}^{s}(\rho_q \sum W_{qij} y_j) + X_i \beta + \epsilon_i, \tag{5.1}$$

$$\epsilon_i = \sum_{r=1}^{t}(\theta_r \sum W_{rij} u_i) + \mu_i \tag{5.2}$$

The dependent variable y_i indicates a measure of human rights in country i. $\sum W_{qij} y_j$ is a spatial lag of the dependent variable. It is a linear combination of the values of the dependent variable in all countries in the sample. The elements W_{qij} assign a spatial weight to each country pair ij, describing the spatial dependence relation in the dependent variable between the respective countries (Lesage and Pace, 2009). We allow for higher order spatial dependence in the dependent variable. That is, depending on the model specification, we control for multiple spatial lags of the dependent variable simultaneously with s ranging from one to seven. X_i is a vector of control variables, which includes an intercept. ϵ_i indicates the error term for country i, which we allow to be spatially dependent. Again, depending on the model specification, we allow for the simultaneous inclusion of multiple error lags $W_{rij} u_i$. The parameters to be estimated are ρ_q, β, and θ_r with the estimates of ρ_q being of primary interest in this study. A positive ρ_q indicates an overall positive spatial dependence in the dependent variable between countries in accordance with the weights W_{qij}.

In matrix notation, the model to be estimated can be expressed in the following form:

$$y = \sum_{q=1}^{s}(\rho_q W_q y) + X\beta + \epsilon, \tag{5.3}$$

$$\epsilon = \sum_{r=1}^{t} (\theta_r W_r u) + \mu. \tag{5.4}$$

With N denoting the number of individuals and K denoting the number of controls, including the intercept, other than the spatial lags, y and ϵ are $N \times 1$ vectors, W_q and W_r are $N \times N$ matrices, and X is an $N \times K$ matrix. The model expressed by equations (3) and (4) is equivalent to the model in equations (1) and (2).

Data

As a proxy for human rights, we use the Civil Liberties Index from the Varieties of Democracy (V-Dem) dataset by Coppedge, Gerring, Knutsen, Lindberg, Teorell, Altman, Bernhard, Cornell, Fish, Gastaldi, Gjerløw, Glynn, God, et al. (2023) and Pemstein et al. (2023). This index measures the extent to which civil liberty is respected in a given country. It is constructed as the average of three sub-indices measuring freedom from physical violence committed by the state, private liberties, and political liberties, respectively (Coppedge, Gerring, Knutsen, Lindberg, Teorell, Altman, Bernhard, Cornell, Fish, Gastaldi, Gjerløw, Glynn, Grahn, et al., 2023).

The country-specific control variables include the natural logarithm of GDP per capita in constant US Dollars (USD), the natural logarithm of the population size, as well as the squares of the two variables to account for potential non-linear relationships between economic development or country size and human rights. Since there is evidence that a country's openness to trade and FDI may affect its institutions and the respect for civil and political rights (López-Córdova and Meissner, 2008; Neumayer and Soysa, 2011; Hafner-Burton, 2005), we also control for trade openness, measured as the sum of exports and imports as a percentage of GDP, and for FDI inflows as a percentage of GDP. The data on GDP per capita stem from the World Development Indicators (WDI) database of the World Bank (2023). The data on population size stem from the Dynamic Gravity Dataset (DGD) by Gurevich and Herman (2018). We construct the variable measuring trade openness using export and import data from the International Trade and Production Database for Estimation (ITPD-E) by Borchert et al. (2022) and Borchert et al.

(2021) and data on nominal GDP from the WDI by the World Bank (2023). We aggregate all exports from a country to any other country and in any sector in the ITPD-E to obtain a country's total exports. We obtain a country's total imports analogously. The data on FDI inflows stem from the United Nations Conference on Trade an Development Statistics (UNCTADstat) data centre of the UNCTAD (2023). We also control for the Liberal Democracy index from the V-Dem dataset (Coppedge, Gerring, Knutsen, Lindberg, Teorell, Altman, Bernhard, Cornell, Fish, Gastaldi, Gjerløw, Glynn, God, et al., 2023; Pemstein et al., 2023). This index measures the extent to which individual and minority rights are protected against the tyranny of the state (Coppedge, Gerring, Knutsen, Lindberg, Teorell, Altman, Bernhard, Cornell, Fish, Gastaldi, Gjerløw, Glynn, Grahn, et al., 2023).

The human rights in a country may be influenced by cultural traits such as the country's religious composition (Potrafke and Ursprung, 2012). To account for the possibility of a country's religious composition being a confounding factor, we control for the shares of the Christian and Muslim populations in the total population. The percentages of Christians and Muslims in the population are obtained from the Pew Research Center (2022). Due to limited data availability, the shares of Christians and Muslims are those from 2010 in all the cross sections.[2] Finally, we also control for a conflict variable, which indicates whether there is an ongoing armed conflict in a country in a given year. To construct this conflict variable, we use data from the Major Episodes of Political Violence (MEPV) dataset from the Armed Conflict and Intervention Datasets by the Center for Systemic Peace (2022). Our conflict variable takes a value of one for a given country and year if there has been an episode of civil violence, civil warfare, ethnic violence, or ethnic warfare of any magnitude in the respective country and year and zero otherwise.[3] All variables are averaged over the respective five-year interval in each of the four cross-sections.

Examining the spatial dependence of human rights in cross-sectional analyses has the advantage that it allows us to control for multiple spatial lags in the dependent variable

[2]There are no data on the religious composition of North Makedonia in the data from the Pew Research Center (2022). We use the shares of Christians and Muslims in North Makedonia in 2023, as reported by the World Factbook by the CIA (2023).

[3]For a more detailed description of the data used to create the conflict variable, see the MEPV codebook by Marshall (2019).

simultaneously and include multiple error lags. This allows us to address one potential source of endogeneity, as explained later in this section. This would not be possible in a panel setting as this would require too much computing power. Moreover, the informativeness of these cross-sectional analyses should not be underestimated. Lesage and Pace (2009) point out that there is a dynamic motivation for a cross-sectional spatial autoregressive model, which is the one we are interested in estimating. They show that the cross-sectional spatial autoregressive model can be interpreted as the outcome of a corresponding long-run equilibrium of a space-time lagged autoregressive process, with the dependent variable of individual i being influenced by the dependent variable of other individuals in the previous period.

As we are interested in examining spatial spillovers between countries, we are concerned about any incompleteness of our sample of countries. Not including a country in our sample is akin to constraining all spillovers from this country to the countries in our sample to be zero. If the spillovers are non-zero, this can lead to biased estimates of the spatial relation in the dependent variable, especially later when we examine spillovers from different isolated country groups. We thus try to obtain samples covering as many countries as possible in each cross-sectional analysis. We do so by imputing certain data, being careful only to fill out gaps in our data that can be easily filled without risking any inappropriate data manipulation. A detailed description of the imputation procedure can be found in the Appendix: Imputation.

Using these imputation techniques to fill out some gaps in our dataset, we can increase the number of countries in our samples to 168 in the cross-section with the smallest sample and 173 in the cross-section with the largest sample. The summary statistics for all variables are shown in table A5.1 in the Appendix.

Weighting matrices

The key to examining the nature of human rights spillovers is the specification of the weights in the spatial lags of the dependent variable, i.e. the elements W_{qij} in equation (1) or, equivalently, the matrices W_q in equation (3). Our first spatial weighting matrix of interest is constructed using the exports from country i to country j as a weight. This allows us to test our first hypothesis:

> $H1_1$*: Human rights in importing countries affect human rights in exporting countries.*

We apply row sum normalization so that each weight W_{ij} represents the share of exports from country i to country j in the total exports from country i. Thus, the spatial lag of the dependent variable takes the form $\sum \frac{exports_{ij}}{exports_i} y_j$ with $exports_{ij}$ indicating the exports from country i to country j and $exports_i$ indicating total exports from country i. In matrix notation, the spatial weighting matrix of interest W_{exp} thus takes the following form, with N being the number of countries in the respective cross-section:

$$W_{exp} = \begin{pmatrix} 0 & Ex_{1,2} & Ex_{1,3} & Ex_{1,4} & ... & Ex_{1,N} \\ Ex_{2,1} & 0 & Ex_{2,3} & Ex_{2,4} & ... & Ex_{2,N} \\ Ex_{3,1} & Ex_{3,2} & 0 & Ex_{3,4} & ... & Ex_{3,N} \\ Ex_{4,1} & Ex_{4,2} & Ex_{4,3} & 0 & ... & Ex_{4,N} \\ ... & ... & ... & ... & ... & ... \\ Ex_{N,1} & Ex_{N,2} & Ex_{N,3} & Ex_{N,4} & ... & 0 \end{pmatrix}$$

Here, $Ex_{i,j}$ indicates the yearly exports from country i to country j, averaged over the period of the respective cross-section. The data on bilateral exports used to construct this weighting matrix stem from the ITPD-E by Borchert et al. (2021) and Borchert et al. (2022). This is a highly comprehensive dataset that includes data on bilateral exports for 265 countries in 170 industries across the sectors of agriculture, mining and energy, manufacturing, and services over the time period from 2000 until 2019. We construct the total exports for a given country pair by summing up the exports in all sectors for the respective country pair.

The spatial weights described above are appropriate to examine whether there are spillovers from importing to exporting countries overall. That is, the weighting matrix does not make any differentiation in the weights based on other characteristics of the exporters and importers. In accordance with the California effect, however, we would not expect human rights spillovers to occur from importers to exporters across the board. We would specifically expect human rights to spill over from importing countries with large market sizes and high human rights standards to exporting countries with small market sizes and lower human rights standards. This leads us to our second hypothesis:

> *$H1_2$: Human rights in powerful importing countries affect human rights in less powerful exporting countries.*

To be able to examine this specific spillover, we use Organisation for Economic Co-operation and Development (OECD) membership as a proxy for being a 'powerful' country, i.e. a country with a large market size. In contrast, we proxy small market size with not being an OECD member. Next, we decompose the spatial lag described above into two separate spatial lags. Our spatial weighting matrix of interest, W_{Cali}, uses the exports from country i to country j as a share of country i's total exports as weights, as described above. However, unlike in the spatial lag described above, all weights not corresponding to exports from non-OECD countries to OECD countries are set to zero. We then construct another complementary spatial weighting matrix W_{Other}, in which the exports from non-OECD countries to OECD countries are set to zero, and all the other bilateral exports are included as weights. In matrix notation, these spatial weighting matrices thus can be thought of as follows, with i and j being OECD countries and k and l being non-OECD countries in this exemplary representation:

$$W_{Cali} = \begin{pmatrix} 0 & 0 & 0 & 0 & ... \\ 0 & 0 & 0 & 0 & ... \\ Ex_{k,i} & Ex_{k,j} & 0 & 0 & ... \\ Ex_{l,i} & Ex_{l,j} & 0 & 0 & ... \\ ... & ... & ... & ... & ... \end{pmatrix} W_{Other} = \begin{pmatrix} 0 & Ex_{i,j} & Ex_{i,k} & Ex_{j,l} & ... \\ Ex_{j,i} & 0 & Ex_{j,k} & Ex_{j,l} & ... \\ 0 & 0 & 0 & Ex_{k,l} & ... \\ 0 & 0 & Ex_{l,k} & 0 & ... \\ ... & ... & ... & ... & ... \end{pmatrix}$$

Since in each cross-section, every country is either an OECD country or a non-OECD country, the sum of the two matrices W_{Cali} and W_{Other} is equal to the matrix using all exports as weights, W_{exp}, in each cross section. By simultaneously controlling for both spatial lags of the dependent variable in each cross-sectional analysis, we can examine human rights spillovers from importing OECD countries to exporting non-OECD countries in isolation.

We also examine whether the manifestation of the California effect depends on the type of good being exported. Specifically, we differentiate between oil and gas exports and non-oil-and-gas exports. Natural resources like oil and gas can only be imported from countries that are endowed with these natural resources due to geographical conditions. Oil and gas are also strategic resources that importing countries need for their general supply and are costly to substitute. Hence, it is plausible that exports of oil and gas are associated with more bargaining power on the part of the exporter relative to exports of other goods, which might undermine the California effect. We thus formulate a third hypothesis to be tested:

> $H1_3$*: Human rights in powerful importing countries affect human rights in less powerful exporting countries through non-oil-and-gas exports.*

To do so, we further split up the matrices W_{Cali} and W_{Other} into two matrices each, one only using oil and gas exports between i and j as a share of i's total exports as weights and one only using the shares of non-oil-and-gas exports as weights. We use sectoral bilateral export data from the ITPD-E by Borchert et al. (2022) and Borchert et al. (2021) to make this differentiation. To construct the matrices weighting the spatial dependence in the dependent variable by oil and gas exports, we only use aggregate exports in the sectors "extraction of crude petroleum and natural gas", "gas production and distribution", and "refined petroleum products". Conversely, we use aggregate exports in all sectors except for these three to construct the non-oil-and-gas exports weighting matrices.

In each cross-section we allow for spatial dependence in the disturbances in accordance with one or more weighting matrices, as indicated by equations (2) and (4). To choose the spatial weighting matrices determining the spatial dependence in the disturbances, we conduct Moran's I test of spatial dependence in the residuals. That is, we first estimate the model expressed by equations (1) and (3) without controlling for a spatially lagged dependent variable, using OLS. We then run the Moran test, testing for spatial dependence in the residuals in accordance with each of the spatial weighting matrices, which are to be used to construct the spatial lags of the dependent variable in the respective specification. Whenever we reject the null hypothesis of the residuals being independent and identically distributed at the 10 percent level, we include an error lag using the weighting matrix for which the null is rejected in the respective model.

Identification

We face three potential issues of endogeneity. The first endogeneity problem results from the spatial autoregressive structure of our model. The spatial lags of the dependent variables in equations (1) and (3) are endogenous. Hence, the OLS estimator is inconsistent (Ord, 1975; Anselin, 1988). This endogeneity issue arises because equations (1) and (3) imply that the dependent variable in country i is not only determined by the values of the dependent variable in country j but also vice-versa. This leads to a simultaneous equation bias if estimated using OLS. We address this issue by applying the GS2SLS estimator derived by Badinger and Egger (2011) and Prucha, Drukker, and Egger (2019). This estimator is based on the GS2SLS estimator proposed by Kelejian and Prucha (1998), Kelejian and Prucha (1999), Kelejian and Prucha (2004), and Kelejian and Prucha (2010) but it allows for higher-order spatial lags of the dependent variable. Since we control for more than one spatial lag of the dependent variable in our spatial regression, this estimator is appropriate for us to use.

To illustrate the idea of this estimator, let us follow Badinger and Egger (2011) – who build upon the insights of Kelejian and Prucha (1998), Kelejian and Prucha (1999), and Kelejian and Prucha (2004) – and rewrite equation (3) in reduced form as

$$y = (I - \sum_{q=1}^{s} \rho_q W_q)^{-1}(X\beta + \epsilon), \tag{5.5}$$

with I being the $N \times N$ identity matrix. Equation (5) implies a mean of y of

$$E[y] = (I - \sum_{q=1}^{s} \rho_q W_q)^{-1}(X\beta) \tag{5.6}$$

and hence

$$E[\sum_{q=1}^{s} W_q y] = \sum_{q=1}^{s} W_q (I - \sum_{q=1}^{s} \rho_q W_q)^{-1} (X\beta). \tag{5.7}$$

Assuming $\sum_{q=1}^{s} |\rho_q| < 1$ we can use

$$(I - \sum_{q=1}^{s} \rho_q W_q)^{-1} = I + \sum_{i=1}^{\infty} (\sum_{q=1}^{s} \rho_q W_q)^i \tag{5.8}$$

to rewrite equation (7) as

$$E[\sum_{q=1}^{s} W_q y] = \sum_{q=1}^{s} W_q [I + \sum_{i=1}^{\infty} (\sum_{q=1}^{s} \rho_q W_q)^i](X\beta). \tag{5.9}$$

Equation (9) suggests X and any of the terms in $\sum_{i=1}^{G}(\sum_{q=1}^{s} \rho_q W_q)^i X$ as instruments for $\sum_{q=1}^{s} W_q y$ in the GS2SLS estimation, with G being any constant (Badinger and Egger, 2011). We choose $G = 2$ as suggested by the Monte Carlo simulation of Kelejian and Prucha (2004).

A second endogeneity problem arises from the possibility of other relevant spillovers of human rights through channels that are correlated with trade flows, leading to omitted variable bias. We address this issue by controlling for three potential confounding spatial lags in each cross-sectional analysis. The first one accounts for the possibility of human rights spillovers generally occurring across borders. We thus construct a spatial weighting matrix W_{Cont} in which the weight W_{ij} is assigned the value 1 if the countries i and j are contiguous and 0 otherwise.

We construct the second spatial lag using a spatial weighting matrix W_{Dist} in which the inverse geographical distances between countries serve as weights. Under row sum normalization, this spatial lag thus takes on the following form: $\sum(\frac{1}{distance_{ij}} / \sum \frac{1}{distance_i}) y_j$.

For the third spatial lag, accounting for spillovers related to the cultural proximity of two countries, we use a spatial weighting matrix W_{Lang}, which is constructed using weights W_{ij}, which are given the value 1 if countries i and j share a common official language and a value of 0 if they do not. Partner countries j are thus only given a positive weight in this matrix if they share an official language with country i. Under row sum normalization, this weight of j will be larger if there are only few other countries that also share an official language with i. On the other hand, country j is given a smaller positive weight if it is one country out of many sharing an official language with country i. Similarly, in W_{Cont}, country j is given a larger positive weight if i and j are neighbors and country i has few other neighbors, and a lower positive weight if i and j are neighbors and i has many neighbors.

Lastly, a third endogeneity concern that arises when using trade flows as weights is the potential endogeneity of trade flows themselves and, thus, the endogeneity of the weighting matrix. There is literature suggesting that a country's institutions or human rights affect its trade policies or economic openness (e.g. Aidt and Gassebner, 2010; Milner and Kubota, 2005; S. L. Blanton and R. G. Blanton, 2007). It is plausible that the similarity of human rights standards between two countries causes these two countries to trade more with each other, thus resulting in a reverse causality issue regarding our estimation procedure. One way this endogeneity of the weighting matrix has been dealt with in the literature is simply using lagged values of trade flows when constructing the spatial weight matrix (e.g. Amidi and Fagheh Majidi, 2020). We address the endogeneity of the export weights using a more novel control function approach proposed by Qu, Lee, and Yang (2021) instead. To implement this approach, for each cross-section, we first estimate the following equation:

$$LogExports_{ij} = \alpha LogDistance_{ij} + \gamma Contiguity_{ij} + \theta Language_{ij} + \delta_{1i} + \delta_{2j} + \xi_{ij}, \quad (5.10)$$

That is, we first regress the natural logarithm of bilateral exports from i to j, $LogExports_{ij}$, on variables which we can safely assume to be exogenous. These are $LogDistance_{ij}$, which is the geographical distance between i and j, $Contiguity_{ij}$, which is a dummy variable indicating whether i and j share a common border, and $Language_{ij}$, a dummy variable indicating whether i and j share a common official language. As Qu, Lee, and Yang (2021) suggest, we control for the fixed effects δ_{1i} and δ_{2j}. These fixed effects capture exporter- and importer-specific multilateral resistance and thus other determinants of exports from

i to j which may give rise to endogeneity in our estimations. We then estimate the model in equation (1) while controlling for the estimates on the multilateral resistance terms $\widehat{\eta_i}$:

$$y_i = \sum_{q=1}^{s} \rho_q \sum W_{qij} y_j + X_i \beta + \widehat{\eta_i} \lambda + \widehat{\epsilon_i}, \tag{5.11}$$

Here, $\widehat{\eta_i} = (\widehat{\delta_{1i}}, \widehat{\delta_{2i}})$, and $\widehat{\epsilon_i} = \epsilon_i + (\eta_i - \widehat{\eta_i})\lambda$. In other words, we pull potential unknown sources of endogeneity out of the error term by controlling for $\widehat{\eta_i}$.

5.4 Results

Before starting with the results, we need to test for a potential spatial correlation in the residuals. As described above, if we cannot reject the null hypothesis of spatial correlation at the 10 %-level using Moran's I test, we include an error lag for the respective weighting matrix. Table A5.2 in the Appendix presents the results.

We start with the simple OLS estimation in table 5.1. Columns 1 to 4 show the 5-year average coefficients for the four different cross-sections between 2000 and 2019. In contrast to the following specifications, the OLS specification is of a bilateral nature. Thus, the number of observations is the square of the number of countries. Our constructed export-weighted spatial lag is positive and significant at the 1%-level for all four periods. The effect gets slightly stronger over time. When looking at the control variables, we find significant and positive effects of liberal democracy. The effects of both real GDP and real GDP squared are negative and significant. The latter result might be surprising but can be explained by the inclusion of liberal democracy. This variable already catches a lot of the variation of the other explanatory variables. Population has a negative and significant effect in the first two periods. Additionally, trade openness positively affects the level of human rights in the last two periods. The other control variables are insignificant in this specification. Our first estimation results already indicate that exports might be an important determinant for the spillover of human rights between countries.

However, in order to overcome the endogeneity issues, we will move to proper spatial estimations using the GS2SLS estimator proposed by Badinger and Egger (2011). Table

Table 5.1: Baseline Estimation

	(1) 2000-2004	(2) 2005-2009	(3) 2010-2014	(4) 2015-2019
Export-weighted spatial Lag	0.0204***	0.0231***	0.0282***	0.0314***
	(0.0071)	(0.0067)	(0.0057)	(0.0057)
Liberal Democracy	0.8531***	0.8333***	0.8059***	0.8600***
	(0.0575)	(0.0554)	(0.0544)	(0.0560)
Log GDP real	-0.0217***	-0.0247***	-0.0247***	-0.0260***
	(0.0082)	(0.0071)	(0.0075)	(0.0070)
Log GDP real squared	-0.0082***	-0.0089***	-0.0097***	-0.0118***
	(0.0029)	(0.0032)	(0.0035)	(0.0040)
Log Population	-0.0129***	-0.0103***	-0.0053	-0.0073*
	(0.0040)	(0.0038)	(0.0044)	(0.0043)
Log Population squared	-0.0009	-0.0015	-0.0014	-0.0010
	(0.0018)	(0.0016)	(0.0019)	(0.0018)
FDI flows	0.0005	0.0001	0.0002	0.0002
	(0.0013)	(0.0002)	(0.0004)	(0.0012)
Trade Openness in % of GDP	-0.0000	0.0002	0.0004*	0.0004**
	(0.0002)	(0.0001)	(0.0002)	(0.0002)
Conflict	-0.0098	-0.0100	-0.0320	-0.0442
	(0.0269)	(0.0265)	(0.0270)	(0.0323)
Share of Muslim Population	-0.0003	-0.0002	-0.0002	0.0000
	(0.0005)	(0.0005)	(0.0004)	(0.0004)
Share of Christian Population	0.0003	0.0003	0.0003	0.0004
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Constant	0.3659***	0.3563***	0.3569***	0.3283***
	(0.0551)	(0.0500)	(0.0506)	(0.0526)
R-Squared	0.818	0.805	0.797	0.815
Number of Countries	168	171	173	172

Notes: *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively. Standard errors are in parentheses. Standard errors are clustered at the country level.

5.2 presents the results using a spatial lag of human rights weighted by exports, i.e. the lag is constructed using the weighting matrix W_{exp} presented above. The coefficients of the control variables are mostly similar to the ones in the OLS estimation concerning their magnitude and significance level. However, the coefficient we are most interested in is the one indicating the export-related spatial dependence in the dependent variable. Compared to table 5.1, the coefficient of the spatial lag is larger in magnitude but not significant in every period. In the first two periods, there is no significant effect at all. However, starting in 2010, we find a positive, strong and significant effect of the spatial lag. This indicates that over the last couple of years, we can find international spillovers of human rights via exports overall. If this result is robust, it can lead us to a conclusion regarding our first hypothesis: We can reject the null hypothesis that human rights in importing countries do not affect human rights in exporting countries. Finally, the attentive reader may have already observed that the sample size differs across periods. This is due to missing values in some cross sections.

Table 5.2: Overall Exports

	(1) 2000-2004	(2) 2005-2009	(3) 2010-2014	(4) 2015-2019
Liberal Democracy	0.8390***	0.8237***	0.7760***	0.8174***
	(0.0580)	(0.0554)	(0.0535)	(0.0517)
Log GDP real	-0.0214***	-0.0260***	-0.0298***	-0.0296***
	(0.0082)	(0.0071)	(0.0071)	(0.0072)
Log GDP real squared	-0.0085***	-0.0088***	-0.0084**	-0.0089**
	(0.0029)	(0.0032)	(0.0033)	(0.0039)
Log Population	-0.0130***	-0.0107***	-0.0061	-0.0104**
	(0.0040)	(0.0038)	(0.0042)	(0.0040)
Log Population squared	-0.0010	-0.0015	-0.0023	-0.0023
	(0.0018)	(0.0017)	(0.0018)	(0.0016)
FDI flows in % of GDP	0.0006	0.0001	0.0004	0.0002
	(0.0013)	(0.0002)	(0.0004)	(0.0011)
Trade Openness in % of GDP	-0.0001	0.0002	0.0003*	0.0003*
	(0.0002)	(0.0001)	(0.0002)	(0.0002)
Conflict	-0.0098	-0.0072	-0.0186	-0.0352
	(0.0266)	(0.0263)	(0.0251)	(0.0290)
Share of Muslim Population	-0.0003	-0.0003	-0.0003	0.0001
	(0.0005)	(0.0005)	(0.0004)	(0.0004)
Share of Christian Population	0.0002	0.0003	0.0001	0.0003
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Constant	0.2843***	0.2710***	0.1792**	0.1470*
	(0.1061)	(0.0890)	(0.0763)	(0.0842)
Spatial Lag weighted by				
Total Exports	0.1104	0.1150	0.2653***	0.2735***
	(0.1136)	(0.0883)	(0.0792)	(0.0793)
Pseudo R-Squared	0.818	0.805	0.808	0.828
Number of observations	168	171	173	172

Notes: *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively. Heteroskedasticity-robust standard errors are in parentheses. Error lags are included according to Moran test results in table A5.2. Columns 1, 2, and 3 do not include any error lags. Column 4 includes an error lag for Total Exports.

In order to investigate the existence of the California effect, we need to have a closer look at the export weighting matrix. As described in the previous section, we will divide the exports matrix into two different matrices, one for exports from non-OECD countries to

OECD countries and one for all other exports. Table 5.3 presents the results. Again, the estimates on the control variables are somewhat similar to the ones in table 5.2. Turning to the two spatial lags weighted by exports, the most important is considering spillovers from OECD to non-OECD countries. This spatial lag represents the California effect. Similar to the overall exports, the coefficient is positive and turns significant in 2010. This result supports the existence of the California effect with respect to human rights. Moreover, it indicates that the positive spatial dependence of human rights found for exports overall is mainly driven by human rights spillovers in accordance with the California effect, i.e. spillovers from importing OECD countries to exporting non-OECD countries. There is a weakly significant positive effect in the last period for the spillovers between all other export combinations of OECD and non-OECD countries. However, this coefficient is rather difficult to interpret since it captures all other combinations of spillover through exports. In this paper, our primary emphasis is on examining the California effect. Thus, we will not pay too much attention to this second spatial lag but rather use it as a control variable. Eventually, we are now able to discuss the second hypothesis. Since our coefficient for the California effect is positive and significant in the last two periods, we can reject the null hypothesis that human rights in powerful importing countries do not affect human rights in less powerful exporting countries.

Having established the main results of our article already, we will try to eliminate some problems of our approach in the following. One direct problem could be an omitted variable bias in the weighting matrices. Our spatial lag of exports might spuriously measure other geographic variables, for example, a common border, the distance, or a common language between countries. Thus, we include these additional spatial lags in our estimation. Table A5.3 in the Appendix presents the results for overall exports, and table A5.4 in the Appendix shows the split of total exports. The spatial lags weighted by a common border and a common language are insignificant in both specifications. However, the inverse distance seems to be an important determinant of spillover as well. Its coefficient is significant and positive, starting in 2010, as shown in table A5.3. The addition also leads to a reduction of the magnitude of the spatial lag weighted by exports. However, the coefficient is still positive and significant. Similarly, in table A5.4, inverse distance is significant and positive starting in the second period. Our spatial lag of interest, the California effect, loses its significance in the period 2010-2014. However, it remains significant in the last period. We can conclude that both results with respect to our hypothesis are robust to the inclusion of additional control spatial lags.

Table 5.3: Exports by Country Group

	(1) 2000-2004	(2) 2005-2009	(3) 2010-2014	(4) 2015-2019
Liberal Democracy	0.8717***	0.8589***	0.8086***	0.8429***
	(0.0597)	(0.0595)	(0.0558)	(0.0528)
Log GDP real	-0.0166*	-0.0212***	-0.0236***	-0.0197***
	(0.0086)	(0.0076)	(0.0081)	(0.0075)
Log GDP real squared	-0.0045	-0.0060*	-0.0044	-0.0036
	(0.0031)	(0.0035)	(0.0037)	(0.0040)
Log Population	-0.0112***	-0.0096**	-0.0052	-0.0085**
	(0.0042)	(0.0039)	(0.0044)	(0.0042)
Log Population squared	-0.0009	-0.0018	-0.0029*	-0.0028*
	(0.0018)	(0.0016)	(0.0016)	(0.0015)
FDI flows in % of GDP	0.0002	0.0000	0.0001	-0.0008
	(0.0013)	(0.0002)	(0.0004)	(0.0009)
Trade Openness in % of GDP	-0.0001	0.0002	0.0003*	0.0004**
	(0.0002)	(0.0001)	(0.0002)	(0.0002)
Conflict	-0.0161	-0.0044	-0.0106	-0.0329
	(0.0260)	(0.0263)	(0.0241)	(0.0271)
Share of Christian Population	0.0003	0.0003	0.0001	0.0003
	(0.0004)	(0.0004)	(0.0004)	(0.0003)
Share of Muslim Population	-0.0003	-0.0002	-0.0003	-0.0000
	(0.0005)	(0.0004)	(0.0004)	(0.0004)
Constant	0.3486***	0.3104***	0.2180**	0.1895**
	(0.1003)	(0.0828)	(0.0857)	(0.0837)
Spatial Lag weighted by				
Exports from non-OECD to OECD	0.0453	0.0829	0.2285***	0.2607***
	(0.1072)	(0.0857)	(0.0803)	(0.0776)
Other Exports	-0.0435	-0.0035	0.1538*	0.1485*
	(0.1145)	(0.0986)	(0.0913)	(0.0876)
Pseudo R-Squared	0.824	0.811	0.814	0.837
Number of observations	168	171	173	172

Notes: *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively. Heteroskedasticity-robust standard errors are in parentheses. Error lags are included according to Moran test results in table A5.2. Columns 1 and 2 do not include any error lags. Columns 3 and 4 include an error lag for Other Exports.

After tackling the endogeneity problem using a spatial estimator and several weighting matrices, we still need to deal with a possible problem of reversed causality. As described in the previous section, we address this issue by using the control function approach proposed by Qu, Lee, and Yang (2021). Countries might export to other countries due to some unobserved factors that affect the level of human rights in the country of destination in a similar way. Thus, to solve this problem, we include the predicted multilateral resistance terms of the first-stage equation in the second-stage equation in tables A5.5 and A5.6 in the Appendix. As explained above, if the coefficients of these terms are significant, our previous results may have been biased. As before, we include error lags if Moran's I test suggests rejection of the null hypothesis of no spatial dependence in the residuals at the 10 %-level. The results of Moran's I test for the control function approach are shown in table A5.8 in the Appendix. Table A5.5 presents the corresponding results for the spatial lag weighted by overall exports, including additional spatial lags as controls, as before. The exporter and importer effects are partly significant, indicating a potential endogeneity problem in the previous estimations. Once they are included, our results remain qualitatively the same. We still find significant and positive spatial spillovers through exports starting in 2010. Table A5.6 shows the same approach for the split exports weighting matrix. Again, the coefficients of our predicted variables from the first-stage equation are significant. Our coefficients of interest, the spatial lags weighted by exports from non-OECD to OECD countries, are significant and positive in the last two periods. Thus, our results obtained using the control function approach support the previous ones.

5.5 Extensions

The third hypothesis in section 5.3 refers to a difference between resource exports and non-resource exports. Since we assume OECD importers to have less bargaining power over non-OECD exporters when it comes to oil and gas exports, we expect the California effect to be particularly relevant for non-resource exports. Thus, in table 5.4 we include spatial lags weighted by common border, inverse distance, common language, and four more export matrices: resource exports from non-OECD to OECD countries, non-resource exports from non-OECD to OECD countries, resource exports between all other country pairs, and non-resource exports between all other country pairs. Thus, we can now look

Table 5.4: Exports by Country Group: Resources vs. Non-Resources

	(1) 2000-2004	(2) 2005-2009	(3) 2010-2014	(4) 2015-2019
Liberal Democracy	0.8137***	0.8090***	0.7323***	0.7546***
	(0.0626)	(0.0575)	(0.0538)	(0.0542)
Log GDP real	-0.0107	-0.0156**	-0.0260***	-0.0255***
	(0.0096)	(0.0079)	(0.0091)	(0.0089)
Log GDP real squared	-0.0034	-0.0036	-0.0047	-0.0050
	(0.0033)	(0.0034)	(0.0039)	(0.0042)
Log Population	-0.0107**	-0.0105**	-0.0027	-0.0067
	(0.0050)	(0.0045)	(0.0051)	(0.0052)
Log Population squared	-0.0012	-0.0026	-0.0023	-0.0024
	(0.0018)	(0.0017)	(0.0020)	(0.0020)
FDI flows in % of GDP	0.0002	-0.0001	0.0003	0.0000
	(0.0013)	(0.0002)	(0.0004)	(0.0010)
Trade Openness in % of GDP	-0.0001	0.0001	0.0003	0.0003
	(0.0002)	(0.0001)	(0.0002)	(0.0002)
Conflict	-0.0101	0.0010	-0.0210	-0.0417
	(0.0275)	(0.0262)	(0.0259)	(0.0306)
Share of Muslim Population	-0.0002	-0.0000	-0.0001	-0.0000
	(0.0005)	(0.0004)	(0.0004)	(0.0004)
Share of Christian Population	0.0003	0.0003	0.0001	0.0003
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Constant	0.3081**	0.1949	-0.0678	-0.0580
	(0.1428)	(0.1347)	(0.1431)	(0.1597)
Spatial Lag weighted by				
Common Border	0.0002	-0.0003	-0.0382	-0.0309
	(0.0284)	(0.0312)	(0.0293)	(0.0305)
Inverse Distance	0.1782	0.2708	0.4195*	0.3977*
	(0.2297)	(0.2284)	(0.2386)	(0.2247)
Common Language	0.0268	0.0052	0.0149	0.0179
	(0.0599)	(0.0541)	(0.0485)	(0.0594)
Resource Exports from non-OECD to OECD	-0.0886	-0.0237	0.1424	0.2091**
	(0.1355)	(0.1055)	(0.1129)	(0.1062)
Other Resource Exports	-0.3040*	-0.1742	0.3294**	0.2533**
	(0.1845)	(0.1557)	(0.1419)	(0.1271)
Non-Resource Exports from non-OECD to OECD	-0.0328	0.0271	0.2611***	0.2664***
	(0.1207)	(0.0998)	(0.0988)	(0.0834)
Other Non-Resource Exports	-0.1292	-0.0784	0.1995*	0.2338**
	(0.1351)	(0.1155)	(0.1124)	(0.0937)
Pseudo R-Squared	0.831	0.817	0.815	0.828
Number of observations	168	171	173	172

Notes: *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively. Heteroskedasticity-robust standard errors are in parentheses. Error lags are included according to Moran test results in table A5.2. Column 1 does not include any error lags. Column 2 includes an error lag for Other Resource Exports. Column 3 includes error lags for Common Border and Inverse Distance. Column 4 includes an error lag for Common Border, Inverse Distance, and Other Non-Resource Exports.

specifically at resource and non-resource exports from non-OECD to OECD countries. The coefficients for the California effect for non-resources are positive and significant in the last two periods. This effect is similar to the ones we already found in our previous specifications. However, the California effect for resources is also positive and significant in the last period. Since both coefficients are positive and significant, we cannot argue that the California effect solely stems from non-resource exports. Nevertheless, we can come to a conclusion regarding hypothesis three. Human rights in powerful importing countries affect human rights in less powerful exporting countries through non-oil-and-gas exports. However, the same seems to be true for oil and gas exports, albeit only in the period from 2015 to 2019.

Finally, we conduct one more extension concerning the country groups. One could argue that the OECD is not simply a group of high-income countries which coincidentally all have relatively high human rights but a community sharing the same values respecting democracy, human rights, and many more. Thus, the California effect could be simply a phenomenon of this community and not a general matter between different groups of countries. To test this, we split the sample into a high and a low group. The high group comprises countries with a real GDP and human rights above the respective 75th percentiles. So, the high group consists of rich countries with high human rights standards only. Table A5.7 in the Appendix presents the results. The spatial lag weighted by exports from low to high represents the California effect. Its coefficients are significant and positive in the last two periods. Thus, the results are similar to those in the previous examples. We can conclude that it is not only the OECD that fosters spillover through trade but also other groups consisting of such high countries. However, many OECD countries are part of this high group. So, the effect could still be driven by OECD countries.

5.6 Conclusion

For many years, a common narrative has posited that increased trade between Western countries and countries with poor human rights records could lead to spillovers of human rights and democracy and thus improve living conditions around the globe. Due to recent geopolitical events, this "Wandel durch Handel" narrative has been called into question, a potential implication being that rich countries should not trade with countries that do not respect human rights or democratic values. A conclusion on this matter should not be made carelessly as it could have detrimental consequences for the poorest of this earth. An empirical analysis can help in determining whether doubts about the existence of beneficial human rights spillovers are based only on anecdotal evidence of singular unfortunate events or whether they are systematically justified.

One potential mechanism through which international diffusion of human rights may occur is the so-called "California effect": firms in importing and exporting countries are often pressured by non-governmental actors such as consumers, downstream firms, or non-governmental organizations to respect human rights. This may lead to firms in exporting countries trying to improve their standards or to lobby their respective governments in this regard in order to be able to export to countries where such practices are valued. In this article, we test the existence of this effect.

Our sample comprises four cross-sections between 2000 and 2019 with up to 173 countries. We estimate a spatial autoregressive model with spatially dependent errors by applying the GS2SLS estimator. As weighting matrices, we use contiguity, inverse distance, common language, and exports. The latter is the one we are interested in to test the existence of the California effect. Additionally, we use a recent control function approach to tackle the problem of an endogenous weighting matrix.

Overall, we test three hypotheses. First, we test if there are human rights spillovers through trade. While our coefficients of interest are insignificant in the first periods, we find these positive spillovers in the cross-sections starting from 2010. Second, we examine the California effect, focusing on spillovers from OECD to non-OECD countries. Again, our coefficients are positive and significant, suggesting the existence of the California effect. Both results are robust to various robustness tests. Finally, we differentiate between resource and non-resource exports. The results suggest that non-resource exports are a major driver of the California effect. However, in the most recent cross-section, we also find the California effect in resource exports.

Our results are at odds with the anecdotal evidence that spillovers through trade do not exist. When systematically examining human rights spillovers through trade, we find evidence for positive and significant spillovers. Trade seems to be an important vehicle for the improvement of human rights around the globe. An important policy implication of this study is that Western trade relationships with lower human rights standards should not be abandoned for supposedly ethical reasons. Instead, trade should be considered a transmission mechanism for human rights and other kinds of institutions.

As indicated before, our study has some limitations. Our estimated coefficients concerning the California effect are positive and significant after 2010. We interpret these estimates to indicate positive spillover effects from OECD to non-OECD countries. However, theoretically, this could also suggest spillovers from non-OECD to OECD countries through exports. This could imply that human rights standards in OECD countries may decrease if human rights standards in non-OECD trading partners decrease. Yet, we consider it unlikely that such an adverse spillover effect on OECD human rights standards is an important driver of our results. Additionally, we do not conduct a panel data analysis and thus ignore any time dimension for technical reasons. With most current statistical tools, it is not possible to simultaneously control for multiple weighting matrices when estimating spatial autoregressive models in a panel setting. If we had done this manually by treating all country-year combinations as individual observations in a cross-section and controlling for the appropriate fixed effects, the sizes of the weighting matrices would have risen to dimensions that would have made computation with conventional technical equipment difficult. Lastly, the typical shortcomings of cross-sectional studies apply, e.g. we cannot generalize our result to every country-pair but need to view it as an average result across all countries. However, this would have also been the case if we had applied a panel study.

5.7 References

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5.8 Appendix: Imputation

The first variable which we subject to imputation is population size. The data on population from the DGD by Gurevich and Herman (2018) is quite comprehensive. However, in this dataset, we are missing population size data for Taiwan. In the WDI by the World Bank (2023), however, there are data on the population size of Taiwan in the time periods that interest us. We impute the population size of Taiwan by first regressing population size according to the DGD on population size according to the WDI, including all countries for which data on population size is available in both datasets. The results are shown in column 1 of table 5.5. We then obtain the estimates to obtain the predicted population size of Taiwan in the respective years.

We analogously impute missing data on GDP per capita in constant 2015 USD in the World Bank (2023) data. In the data from the International Monetary Fund (2023) there is data on nominal GDP per capita in USD for several countries for which real GDP is missing in the data of the World Bank (2023). We thus first use the nominal GDP per capita data from the International Monetary Fund (2023) and a USD GDP deflator obtained from the WDI to obtain a variable measuring GDP per capita in 2015 USD implied by the nominal GDP per capita data of the International Monetary Fund (2023). We then regress GDP per capita in 2015 USD from the World Bank (2023) on the GDP per capita in 2015 USD implied by the data from the International Monetary Fund (2023), to obtain the predicted values of real GDP per capita for those countries for which the variable is missing in our data. The results of this regression are shown in column 2 of table 5.5. We use the predicted values from this regression for imputation.

For FDI flows as a percentage of GDP, there are data from the World Bank (2023), which partially complement our data from the UNCTAD (2023). As before, we impute the missing data from the UNCTAD (2023) by first regressing the FDI data from the UNCTAD (2023) on the FDI data from the World Bank (2023), using the predicted values for imputation. The results are shown in column 3 of table 5.5.

Lastly, we use imputation to complement our data on trade openness. Our variable of trade openness, as described above, is constructed by dividing the sum of a country's exports and imports by its nominal GDP. This share is multiplied by 100 to obtain a percentage value. For some countries, this variable is missing due to missing data on nominal GDP from the World Bank (2023) data. We thus construct a variable measuring trade openness in the same way as described above, while using nominal GDP data from the DGD by Gurevich and Herman (2018), which partially complements the nominal GDP data from the World Bank (2023). We then regress this alternative measure of trade openness on our original measure to obtain the predicted values for trade openness which we use for imputation. The results are shown in column 4 of table 5.5.

Table 5.5: Imputation

	(1) Population_{DGD}	(2) GDP_{WDI}	(3) FDI_{UNCTAD}	(4) Openness_{WDI}	(5) Openness_{WDI}
Population_{WDI}	0.9973***				
	(0.0076)				
GDP_{IMF}		0.9100***			
		(0.0160)			
FDI_{WDI}			0.8821***		
			(0.0843)		
Openness_{DGD}				0.8348***	
				(0.1800)	
Implied Openness_{WDI}					0.9285***
					(0.0781)
Constant	212257.2	545.0131***	0.4091	39.0860***	6.6289
	(217238.5)	(136.74)	(0.3315)	(6.8953)	(5.1865)
R-Squared	1	0.960	0.797	0.623	0.841
Number of observations	2904	3367	3350	2900	3423

Notes: *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively. Standard errors are in parentheses. Standard errors are clustered at the country level.

Since we still have missing values on nominal GDP, we construct a third measure of trade openness by using another measure of nominal GDP. For some countries, there is no data on nominal GDP in the WDI, for which there is data on GDP per capita in constant USD and on population size. We thus obtain a value of nominal GDP implied by the World Bank (2023) data by multiplying the GDP per capita in constant 2015 USD by a USD GDP deflator for 2015 obtained from the World Bank (2023) and by the population size. As before, we then regress our original measure of trade openness on the newly constructed measure of trade openness and use the predicted values for imputation. The results are shown in column 5 of table 5.5.

5.9 Appendix: Tables

Table A5.1: Summary statistics of the four cross-sections

	Observations	Mean	Std. Dev.	Min	Max
Civil liberties	684	0.70	0.24	0.03	0.98
Liberal Democracy	684	0.42	0.27	0.01	0.90
Log GDP real	684	8.43	1.45	5.59	11.58
Log Population	684	16.02	1.68	11.32	21.06
FDI flows % in GDP	684	5.34	14.16	-4.69	264.27
Trade Openness in % of GDP	684	77.39	56.19	3.92	627.69
Conflict	684	0.24	0.37	0	1
Share of Muslim Population	684	27.52	37.90	0	100
Share of Christian Population	684	53.24	37.55	0	100
Exports	684	555.37	1485.41	0.07	15179.71

Table A5.2: Moran's I Test: Test for spatial correlation among the residuals

	Chi-squared statistic			
Weighting Matrix	2000-2004	2005-2009	2010-2014	2015-2019
Contiguity	1.21	2.60	3.43*	5.49**
Inverse Distance	0.17	1.04	3.73*	6.58**
Common Language	1.45	1.16	0.11	0.03
Total Exports	0.45	0.44	2.51	5.50**
Exports from non-OECD to OECD	0.57	0.18	0.00	0.67
Other Exports	0.02	0.26	3.83*	5.35**
Resource Exports from non-OECD to OECD	0.00	0.03	0.03	0.02
Non-Resource Exports from non-OECD to OECD	0.74	0.18	0.00	0.78
Other Resource Exports	2.46	3.47*	2.32	2.29
Other Non-Resource Exports	0.66	1.72	2.24	3.55*
Exports from Low to High Income and Human Rights	0.47	0.21	0.06	0.62
Other Exports Income and Human Rights	0.05	0.25	3.48*	5.59**

Notes: *, ** and *** denote significance at the 10%-, 5%- and 1%-level.

Table A5.3: Overall Exports incl. Control Weights

	(1) 2000-2004	(2) 2005-2009	(3) 2010-2014	(4) 2015-2019
Liberal Democracy	0.8317***	0.7995***	0.7434***	0.7651***
	(0.0576)	(0.0557)	(0.0503)	(0.0533)
Log GDP real	-0.0231***	-0.0275***	-0.0310***	-0.0281***
	(0.0088)	(0.0075)	(0.0080)	(0.0083)
Log GDP real squared	-0.0076**	-0.0075**	-0.0059*	-0.0056
	(0.0030)	(0.0032)	(0.0034)	(0.0039)
Log Population	-0.0123***	-0.0098**	-0.0039	-0.0072
	(0.0045)	(0.0044)	(0.0049)	(0.0049)
Log Population squared	-0.0010	-0.0015	-0.0023	-0.0021
	(0.0018)	(0.0017)	(0.0019)	(0.0019)
FDI flows in % of GDP	0.0005	0.0001	0.0003	0.0001
	(0.0013)	(0.0002)	(0.0004)	(0.0010)
Trade Openness in % of GDP	-0.0000	0.0002	0.0003	0.0003
	(0.0002)	(0.0001)	(0.0002)	(0.0002)
Conflict	-0.0113	-0.0063	-0.0219	-0.0443
	(0.0280)	(0.0274)	(0.0255)	(0.0301)
Share of Christian Population	0.0002	0.0002	0.0001	0.0003
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Share of Muslim Population	-0.0003	-0.0003	-0.0002	0.0000
	(0.0005)	(0.0005)	(0.0004)	(0.0004)
Constant	0.2168	0.1248	-0.0493	-0.1230
	(0.1403)	(0.1376)	(0.1390)	(0.1573)
Spatial Lag weighted by				
Common Border	-0.0193	-0.0199	-0.0434	-0.0257
	(0.0281)	(0.0305)	(0.0286)	(0.0319)
Inverse Distance	0.1321	0.2843	0.3716	0.3904*
	(0.2172)	(0.2172)	(0.2283)	(0.2296)
Common Language	0.0265	0.0090	0.0448	0.0347
	(0.0538)	(0.0536)	(0.0483)	(0.0609)
Total Exports	0.0741	0.0677	0.2280**	0.2517***
	(0.1241)	(0.1004)	(0.0915)	(0.0837)
Pseudo R-Squared	0.819	0.807	0.811	0.823
Number of observations	168	171	173	172

Notes: *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively. Heteroskedasticity-robust standard errors are in parentheses. Error lags are included according to Moran test results in table A5.2. Columns 1 and 2 do not include any error lags. Column 3 includes error lags for Common Border and Inverse Distance. Column 4 includes error lags for Common Border, Inverse Distance, and Total Exports.

Table A5.4: Exports by Country Group incl. Control Weights

	(1) 2000-2004	(2) 2005-2009	(3) 2010-2014	(4) 2015-2019
Liberal Democracy	0.8582***	0.8306***	0.7723***	0.8003***
	(0.0590)	(0.0578)	(0.0490)	(0.0520)
Log GDP real	-0.0181**	-0.0214***	-0.0250***	-0.0196**
	(0.0090)	(0.0080)	(0.0076)	(0.0078)
Log GDP real squared	-0.0024	-0.0032	-0.0022	-0.0014
	(0.0034)	(0.0037)	(0.0038)	(0.0041)
Log Population	-0.0112**	-0.0096**	-0.0047	-0.0074
	(0.0047)	(0.0045)	(0.0047)	(0.0048)
Log Population squared	-0.0006	-0.0014	-0.0028*	-0.0022
	(0.0018)	(0.0016)	(0.0015)	(0.0016)
FDI flows in % of GDP	0.0001	0.0000	0.0001	-0.0007
	(0.0013)	(0.0002)	(0.0004)	(0.0009)
Trade Openness in % of GDP	-0.0001	0.0002	0.0003*	0.0004*
	(0.0002)	(0.0001)	(0.0002)	(0.0002)
Conflict	-0.0137	0.0021	-0.0121	-0.0405
	(0.0275)	(0.0282)	(0.0247)	(0.0287)
Share of Muslim Population	-0.0003	-0.0003	-0.0003	-0.0001
	(0.0005)	(0.0005)	(0.0004)	(0.0004)
Share of Christian Population	0.0002	0.0001	-0.0000	0.0001
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Constant	0.2453*	0.1293	-0.1262	-0.0978
	(0.1363)	(0.1356)	(0.1282)	(0.1333)
Spatial Lag weighted by				
Common Border	-0.0023	0.0003	-0.0370	-0.0227
	(0.0292)	(0.0319)	(0.0297)	(0.0325)
Inverse Distance	0.2453	0.3932*	0.5923***	0.5263***
	(0.2243)	(0.2272)	(0.2150)	(0.1968)
Common Language	0.0253	0.0101	0.0686	0.0424
	(0.0604)	(0.0557)	(0.0492)	(0.0587)
Exports from non-OECD to OECD	-0.0396	-0.0146	0.1339	0.1739**
	(0.1272)	(0.1011)	(0.0948)	(0.0850)
Other Exports	-0.1452	-0.1243	0.0307	0.0548
	(0.1428)	(0.1239)	(0.1104)	(0.0949)
Pseudo R-Squared	0.826	0.815	0.819	0.838
Number of observations	168	171	173	172

Notes: *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively. Heteroskedasticity-robust standard errors are in parentheses. Error lags are included according to Moran test results in table A5.2. Columns 1 and 2 do not include any error lags. Columns 3 and 4 include error lags for Common Border, Inverse Distance, and Other Exports.

Table A5.5: Control function approach: Overall Exports

	(1) 2000-2004	(2) 2005-2009	(3) 2010-2014	(4) 2015-2019
Liberal Democracy	0.8034***	0.7811***	0.7276***	0.7471***
	(0.0566)	(0.0540)	(0.0481)	(0.0509)
Log GDP real	-0.0516***	-0.0516***	-0.0600***	-0.0489***
	(0.0119)	(0.0134)	(0.0126)	(0.0123)
Log GDP real squared	-0.0103***	-0.0081**	-0.0082**	-0.0079**
	(0.0032)	(0.0033)	(0.0034)	(0.0039)
Log Population	-0.0422***	-0.0380***	-0.0373***	-0.0295***
	(0.0111)	(0.0127)	(0.0110)	(0.0086)
Log Population squared	-0.0041*	-0.0043**	-0.0049**	-0.0039*
	(0.0022)	(0.0021)	(0.0022)	(0.0020)
FDI flows in % of GDP	0.0009	0.0002	0.0006	0.0005
	(0.0012)	(0.0002)	(0.0004)	(0.0010)
Trade Openness in % of GDP	-0.0002	0.0001	0.0002	0.0002
	(0.0002)	(0.0001)	(0.0002)	(0.0002)
Conflict	0.0022	0.0074	-0.0061	-0.0343
	(0.0275)	(0.0266)	(0.0252)	(0.0292)
Share of Muslim Population	0.0000	-0.0002	-0.0003	-0.0000
	(0.0005)	(0.0005)	(0.0004)	(0.0004)
Share of Christian Population	0.0004	0.0003	0.0001	0.0004
	(0.0004)	(0.0005)	(0.0004)	(0.0004)
Exporter Effects	0.0296	-0.0322	-0.0471**	-0.0639***
	(0.0234)	(0.0228)	(0.0221)	(0.0218)
Importer Effects	0.0305	0.0892***	0.1087***	0.1124***
	(0.0285)	(0.0296)	(0.0278)	(0.0276)
Constant	0.2557*	0.2616*	0.0312	-0.1044
	(0.1408)	(0.1423)	(0.1239)	(0.1739)
Spatial Lag weighted by				
Common Border	-0.0126	-0.0125	-0.0262	-0.0086
	(0.0266)	(0.0293)	(0.0274)	(0.0318)
Inverse Distance	0.0208	0.0608	0.2411	0.3574
	(0.2279)	(0.2417)	(0.2238)	(0.2573)
Common Language	0.0361	0.0315	0.0540	0.0265
	(0.0533)	(0.0500)	(0.0461)	(0.0561)
Total Exports	0.1188	0.0873	0.2545***	0.2834***
	(0.1223)	(0.1038)	(0.0859)	(0.0770)
Pseudo R-Squared	0.828	0.818	0.829	0.837
Number of observations	168	171	173	172

Notes: *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively. Heteroskedasticity-robust standard errors are in parentheses. Error lags are included according to Moran test results. Error lags are included according to Moran test results in table A5.8. Columns 1 and 3 do not include any error lags. Column 2 includes an error lag for Contiguity. Column 4 includes error lags for Common Border, Inverse Distance, and Total Exports.

Table A5.6: Control function approach: Exports by Country Group

	(1) 2000-2004	(2) 2005-2009	(3) 2010-2014	(4) 2015-2019
Liberal Democracy	0.8317***	0.8095***	0.7641***	0.7708***
	(0.0569)	(0.0547)	(0.0488)	(0.0517)
Log GDP real	-0.0509***	-0.0490***	-0.0531***	-0.0419***
	(0.0119)	(0.0131)	(0.0123)	(0.0122)
Log GDP real squared	-0.0041	-0.0040	-0.0030	-0.0036
	(0.0034)	(0.0036)	(0.0038)	(0.0041)
Log Population	-0.0469***	-0.0423***	-0.0397***	-0.0310***
	(0.0109)	(0.0123)	(0.0107)	(0.0084)
Log Population squared	-0.0040*	-0.0045**	-0.0056***	-0.0040**
	(0.0021)	(0.0020)	(0.0020)	(0.0019)
FDI flows in % of GDP	0.0004	0.0000	0.0003	-0.0002
	(0.0012)	(0.0002)	(0.0004)	(0.0009)
Trade Openness in % of GDP	-0.0002	0.0001	0.0002	0.0003
	(0.0002)	(0.0001)	(0.0002)	(0.0002)
Conflict	0.0018	0.0188	0.0056	-0.0290
	(0.0266)	(0.0270)	(0.0250)	(0.0287)
Share of Muslim Population	0.0000	-0.0002	-0.0003	-0.0001
	(0.0005)	(0.0005)	(0.0004)	(0.0004)
Share of Christian Population	0.0004	0.0002	-0.0000	0.0004
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Exporter Effects	0.0305	-0.0298	-0.0474**	-0.0588***
	(0.0223)	(0.0216)	(0.0217)	(0.0206)
Importer Effects	0.0424	0.0942***	0.1128***	0.1090***
	(0.0282)	(0.0281)	(0.0261)	(0.0263)
Constant	0.3044**	0.2586*	0.0558	-0.1427
	(0.1349)	(0.1379)	(0.1320)	(0.1774)
Spatial Lag weighted by				
Common Border	0.0113	0.0144	-0.0058	0.0160
	(0.0274)	(0.0308)	(0.0297)	(0.0338)
Inverse Distance	0.1378	0.1900	0.3100	0.4947*
	(0.2272)	(0.2415)	(0.2334)	(0.2573)
Common Language	0.0364	0.0276	0.0645	0.0175
	(0.0616)	(0.0518)	(0.0423)	(0.0514)
Exports from non-OECD to OECD	-0.0185	0.0066	0.1719**	0.2374***
	(0.1220)	(0.1017)	(0.0849)	(0.0730)
Other Exports	-0.1528	-0.1195	0.0692	0.1262
	(0.1368)	(0.1228)	(0.0966)	(0.0881)
Pseudo R-Squared	0.839	0.828	0.838	0.842
Number of observations	168	171	173	172

Notes: *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively. Heteroskedasticity-robust standard errors are in parentheses. Error lags are included according to Moran test results. Error lags are included according to Moran test results in table A5.8. Column 1 does not include any error lags. Column 2 includes an error lag for Contiguity. Column 3 includes an error lag for Other Exports. Column 4 includes error lags for Common Border, Inverse Distance, and Other Exports.

Table A5.7: Exports by Country Group: High Income and High Human Rights

	(1) 2000-2004	(2) 2005-2009	(3) 2010-2014	(4) 2015-2019
Liberal Democracy	0.8410***	0.8149***	0.7345***	0.7607***
	(0.0582)	(0.0569)	(0.0489)	(0.0514)
Log GDP real	-0.0216**	-0.0250***	-0.0308***	-0.0273***
	(0.0090)	(0.0077)	(0.0077)	(0.0080)
Log GDP real squared	-0.0059*	-0.0058	-0.0068*	-0.0063
	(0.0033)	(0.0036)	(0.0036)	(0.0041)
Log Population	-0.0117**	-0.0095**	-0.0025	-0.0065
	(0.0046)	(0.0045)	(0.0049)	(0.0049)
Log Population squared	-0.0010	-0.0016	-0.0026	-0.0024
	(0.0018)	(0.0017)	(0.0017)	(0.0018)
FDI flows in % of GDP	0.0003	0.0001	0.0003	0.0000
	(0.0013)	(0.0002)	(0.0004)	(0.0011)
Trade Openness in % of GDP	-0.0001	0.0002	0.0004*	0.0003
	(0.0002)	(0.0001)	(0.0002)	(0.0002)
Conflict	-0.0129	-0.0071	-0.0184	-0.0450
	(0.0277)	(0.0274)	(0.0264)	(0.0304)
Share of Muslim Population	-0.0003	-0.0003	-0.0002	-0.0000
	(0.0005)	(0.0005)	(0.0004)	(0.0004)
Share of Christian Population	0.0002	0.0001	0.0002	0.0003
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Constant	0.2474*	0.1514	-0.1368	-0.0812
	(0.1414)	(0.1354)	(0.1488)	(0.1596)
Spatial Lag weighted by				
Common Border	-0.0123	-0.0085	-0.0541*	-0.0485
	(0.0293)	(0.0319)	(0.0287)	(0.0303)
Inverse Distance	0.1645	0.3279	0.5258**	0.4257**
	(0.2210)	(0.2237)	(0.2302)	(0.2140)
Common Language	0.0217	0.0072	0.0688	0.0434
	(0.0561)	(0.0541)	(0.0535)	(0.0628)
Exports from Low to High	0.0214	0.0157	0.1644*	0.2235**
	(0.1335)	(0.1075)	(0.0979)	(0.0926)
Other Exports	-0.0199	-0.0427	0.1938*	0.2391**
	(0.1515)	(0.1312)	(0.1146)	(0.1056)
Pseudo R-Squared	0.821	0.809	0.808	0.826
Number of observations	168	171	173	172

Notes: *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively. Heteroskedasticity-robust standard errors are in parentheses. Error lags are included according to Moran test results in table A5.2. Columns 1 and 2 do not include any error lags. Columns 3 and 4 include error lags for Common Border, Inverse Distance, and Other Exports.

Table A5.8: Moran's I Test: Control function approach

	Chi-squared statistic			
Weighting Matrix	2000-2004	2005-2009	2010-2014	2015-2019
Contiguity	0.45	2.92*	2.30	3.31*
Inverse Distance	0.00	0.41	0.97	3.34*
Common Language	0.88	0.64	0.02	0.00
Total Exports	0.18	0.04	2.53	5.96**
Exports from non-OECD to OECD	0.03	0.85	0.88	0.23
Other Exports	0.72	1.24	7.51***	11.13***

Notes: *, ** and *** denote significance at the 10%-, 5%- and 1%-level.

Part 6

Critical Examination

This cumulative dissertation includes four different essays examining institutions in various settings. Part 2 indicates that the effectiveness of budget support to the health sector does not depend on the level of institutions in a country. Parts 3 and 4 provide evidence that institutions, as well as education, are a crucial determinant of economic growth and vice versa. However, a significant relationship between institutions and education cannot be identified. Finally, Part 5 presents positive spillovers of institutions through trade. By examining institutions in these different settings, each of the essays tried to close a gap in the respective literature. These literature gaps are discussed in more detail in the individual essays.

From a philosophical point of view, the respective results presented in this dissertation should not be treated as proof. This dissertation is written following Popper's critical rationalism and falsifiability (Popper, 1935).[4] Thus, in the essays, hypotheses are deducted from the literature and theory and formulated so that it is possible to falsify them. These hypotheses should only be rejected but never accepted. The results should be seen as an indication in a specific direction but not as ultimate.

Similarly, from an empirical point of view, the results should be treated with a certain degree of caution. In the field of empirical economics or econometrics, researchers are constantly dealing with the problem of being able to interpret the results causally or just measuring correlation. Over the last decades, empirical methods have become more advanced in order to deal with such endogeneity issues leading to an interpretation as correlation only. Different appropriate approaches have been used in the essays presented to overcome the most apparent endogeneity issues. These different estimators, System GMM, Pooled Mean Group, and Generalized Spatial 2SLS, were based on proposals by the literature. One could interpret the results as an approach to a causal interpretation;

[4]Popper, Karl (1935). *Logik der Forschung. Zur Erkenntnistheorie der modernen Naturwissenschaft.* Wien: Springer Verlag (Schriften zur wissenschaftlichen Weltauffassung, 9).

however, one should never be thoroughly convinced. In the four different settings these estimators seemed to be the most appropriate to tackle the endogeneity issues. Of course one could argue that the estimators are still far from an "ideal" experimental setting. However, the preconditions for such an experimental setting could not be achieved using the samples described in the respective essays.

Another general empirical matter is potential data issues. Most of the data stems from large international organizations, which can be seen as credible sources. Though, the process of how this data is aggregated changes from time to time. This could imply that different results could emerge if a researcher tries to validate the results presented in these essays using novel or updated datasets. The same could apply to using different variables or different estimators. However, several robustness checks and extensions have been implemented in all essays. These include dependent variables from different sources, additional control variables, estimation of sub-samples, and many more. Since most results are robust to these extensions, the results can be interpreted with a healthy dose of confidence.

Finally, all essays use data on a country level, aggregating within countries. Thus, the coefficients are mostly averages across all countries in the samples. They represent the coefficient for the average country in the sample. Hence, the results should not be used to predict any developments for single countries. They mainly provide a cross-sectional average only. If one conducts a similar study in a sub-sample, a different sample, or a country case study, the results could differ from the ones presented here.

The limitations presented above are general ones that apply to all essays. Thus, it should be seen as a scratch on the surface. However, this is due to the cumulative nature of this dissertation. This makes it quite challenging to provide more general criticism in one part at the end. Hence, the reader is referred to single sections (e.g., section 2.3, 3.3, 5.3) in the respective essays, providing more details about the potential shortcomings, extensions, and robustness tests to deal with them. Each essay includes a chapter on the empirical framework, where the estimation technique is introduced. There, the advantages in overcoming potential issues of endogeneity in comparison to conventional estimators, e.g., ordinary least squares, are discussed at length.

The four essays presented in this dissertation provide new evidence related to the role of institutions in economics. Along with other studies from the literature, these results might help understand the respective topics better in the future.

Declaration in lieu of oath

I hereby declare that I have independently and without the use of aids other than those specified completed my dissertation with the title

"Essays on Institutional Economics"

I have not made use of unauthorized assistance from third parties. Furthermore, I have used only the stated sources or aids and referenced all statements I have adopted from the sources I have used verbatim or in essence. The thesis has not been submitted to any other examination authority in the same or a similar form.
I hereby consent to investigations by university internal bodies for scientific self-regulation in case of suspected academic misconduct during my doctoral studies.

Tim Röthel

Bayreuth, June 17, 2024