Essays on Institutions and Economic Outcomes

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Summary

Institutions shape individuals' incentive structures and can thus have a substantial impact on economic outcomes. This dissertation examines institutions and their effect on aggregate economic outcomes in four papers.

The first paper examines the impact of corruption on foreign direct investment in developing countries. The study finds that countries with less corruption attract relatively more foreign direct investment. This effect is found to be particularly important for countries with high natural resource endowment.

The second paper analyzes the international diffusion of institutions through trade linkages. The results provide evidence for spillovers of human rights standards between countries that trade with one another. In particular, the study suggests that the standards in importing OECD countries positively affect the standards in exporting non-OECD countries.

The third paper explores how individual incentives can shape collective action in the context of protest behavior. Protest participation is modeled as a network good and the model's implications are examined empirically. The study highlights the relevance of network effects in protest dynamics.

The fourth paper studies the role of institutions in investment in high-tech sectors in European Union countries. The analysis shows that higher institutional and regulatory quality is associated with higher shares of investment in innovative sectors, high-tech industries and artificial-intelligence-intensive sectors.

Chapter 1

Introduction

Human behavior is generally shaped by social interaction. People tend to organize in groups to pursue certain goals collectively and to enjoy the efficiency gains from the division of labor. Within these groups, the actions of an individual are not only constrained by the laws of physics. They are also subject to rules made by humans and imposed by the group itself or by certain individuals in it. Following North (1990, 1991), we refer to such humanly devised rules as *institutions*. The rules comprised by this term can be formal or informal in nature. That is, the institutional environment in which an individual makes a decision is determined by formal rules and laws as well as by informal customs and cultural aspects such as language and religion.

Institutions shape the incentive structure of an individual. Depending on the institutional framework, different types of behavior that an individual may express can be rewarded or sanctioned. Often, these rewards or punishments will be imposed on an individual externally by other individuals in the group. For informal institutions, such enforcement through others may in some cases not be necessary to ensure an individual's compliance with an institution, due to habit formation and self-binding.

In this thesis, we examine how institutions affect aggregate economic outcomes. To understand how institutions affect economic outcomes, it is worthwhile to first reflect on what economic outcomes observed in the aggregate actually are. A group of people, such as a community or the population of a country, is not an entity that can make decisions in itself. Decisions are made by individuals within these groups. An economic outcome observed for a group is thus the cumulative result of the many decisions of the individuals that make it up. Institutions can thus affect an aggregate economic outcome by shaping incentive structures and thus channeling the behavior of individuals.

Depending on the institutional environment individuals find themselves in, cooperation may be incentivized or discouraged. The ability of individuals to engage in cooperative interactions is a key determinant of economic conditions observed in the aggregate. An individual will not be willing to engage in division of labor if they cannot be sufficiently certain that trading partners will uphold their end of a bargain. An entrepreneur will not make an investment if they cannot be sure that the returns will not be expropriated. Institutions define the "rules of the game" (North, 1991, p. 98) under which individuals interact. Knowing the rules governing the decisions of other individuals makes their behavior more predictable. This reduces uncertainty in interactions and thus allows for cooperation.

Institutions can stabilize mutually beneficial situations which require cooperative behavior or coordination of multiple individuals. If two individuals interact with one another under the same set of formal and informal rules, the behavior of the respective other party becomes more predictable. Uncertainty regarding the other individual's actions is reduced and behavior becomes coordinated, thus facilitating cooperation. That is, institutions lower transaction costs by making others' behavior more predictable under uncertainty.

In this cumulative dissertation, we examine the interplay between institutions and economic outcomes in four papers. In two applications, we examine the effect of institutions on investment. Private investment decisions are naturally responsive to institutions as they are always characterized by uncertainty. With institutions affecting uncertainty, there is a direct link between the institutional environment in which a potential investor is located and the expected profitability of their investment. Since uncertainty is differently relevant to an investment's expected profitability in different economic activities, the two investment-focused chapters of this thesis examine the role of institutions with a sectoral differentiation. One focus is on natural resources while the other application pays special regard to differential effects on investment in high tech industries.

Another paper examines the international diffusion of institutions through trade linkages. The focus here is on the transmission of human rights standards. That is, this application investigates how institutions themselves are determined, focusing on international spillovers as a potential factor.

In another study, this dissertation investigates at the micro-level how individual incentive structures can determine collective action in protest movements. While this paper does not examine institutions directly, it complements the macro-level analyses of the other three papers by showing how observed aggregate outcomes may differ depending on the incentive structures of individuals. As discussed above, it is the determination of these incentive structures at the individual level through which institutions matter.

The second chapter, *Institutions as a Determinant of FDI and the Role of Natural Resources*, examines institutions as a determinant of foreign direct investment (FDI) in developing countries. Here, particular attention is paid to the role of natural resource endowment in shaping this relation. Investments are usually not fully reversible and involve a time lag between commitment of resources and payoff. This setting makes the expected profitability of an investment, which determines an individual's decision to invest, highly responsive to uncertainty due to the rules of the game and potential changes thereof. An investor's access to natural resources is often controlled by governments. One might thus hypothesize that weak institutional frameworks, which allow investors to benefit from personal ties to decision makers, could make a country a more attractive destination for FDI, if FDI is largely concentrated in natural resources. This would suggest that a high natural resource endowment dampens the effect of institutional quality on FDI. On the other hand, investments in the natural resources sector are characterized by high sunk costs and long investment horizons. This, along with the tight control government officials usually have over the sector, makes the profitability of investment therein particularly vulnerable to hold-up problems. An institutional environment which allows the host country government to credibly commit to not exploiting irreversible investment, may thus be particularly relevant in this setting. This chapter assesses the effect of natural resource endowment on the relation between institutional quality, proxied by a corruption index, and FDI by examining a panel of 117 developing and emerging countries over the time period from 1996 to 2019. Institutional quality is found to be positively linked to FDI in developing countries with the effect being particularly relevant for countries with high natural resource endowment.

This finding is relevant for policy makers in developing countries who wish to attract FDI in order to further economic development. It highlights the role of institutions as a determinant of FDI. A strong institutional framework is shown to be no less relevant in settings of high natural resource endowment.

Certain limitations of this study should be acknowledged. First, due to limited data availability on industry-specific FDI at the time of writing, the effect of institutions on FDI is not examined in the natural resources sector specifically. Instead, the study examines the relationship between aggregate FDI and institutions and how it is influenced by a host country's natural resource endowment. FDI data broken down by industry arguably would have allowed for a more precise analysis. Second, while the finding is consistent with the idea that better institutions help solve the hold-up problems in natural-resource-seeking FDI, and this explanation is proposed for the results, the empirical approach does not allow an identification of this specific mechanism. Rather, the study finds empirical evidence for a result that would be expected if this mechanism were at play.

The third chapter, *Dream of Californication – Trade, Human Rights and the California Effect*, examines the interdependence of the institutional frameworks of countries trading with one another. The hypothesis of the so-called California effect provides an explanation for why the institutions of an importing country may affect the institutions of an exporting country. We examine the existence of a California effect when it comes to the adherence to human rights standards. Countries with low human rights standards – or firms in these countries – may respond to pressure by consumers or non-governmental organizations in other countries concerned about human rights standards in global value chains by improving human rights standards in order to access export markets in these countries. This would suggest institutional spillovers from importing countries with high human rights standards to exporting countries with lower standards. To examine the existence of such spillovers, we examine panel data on 173 countries observed over the time period from 2000 to 2019, as well as four cross-sections, using a spatial econometric approach. We find evidence for positive human rights spillovers from importing OECD countries to exporting non-OECD countries, suggesting a California effect.

Our results have potentially important foreign policy implications. In the public discourse, how to engage with countries that have different institutional environments and whether to trade with them, is frequently a topic of debate. Our results suggest that trade may be a vehicle for improvements in institutional quality among trading partners.

This analysis is also not without its limitations. While our results suggest spatial dependence of human rights standards via trade linkages, our econometric approach, strictly speaking, does not allow us to make a conclusion regarding the direction in which the spillover occurs. We mainly interpret our findings as an indication that non-OECD countries which export to OECD countries improve their institutions toward the level of OECD countries. However, the same results could be obtained if the situation were reversed and OECD countries' institutions deteriorated toward the level of the non-OECD countries that they trade with. Another limitation is that we only examine direct trade linkages. We cannot make statements about effects which may occur or not occur if countries trade indirectly with each other via third countries. A third limitation involves again our framing regarding the mechanism. As was the case in the previous chapter, our results are consistent with consumers, non-governmental organizations or political actors pressuring exporting countries to improve their institutions. Our empirical approach, however, does not examine this mechanism directly. Rather, we examine developments in macroeconomic outcome variables and whether they are consistent with the mechanism.

The fourth chapter, *Rationalizing Protests – From Individuals to Networks*, delves deeper into the microeconomic mechanisms through which individual incentives can shape collective action. We set up a theoretical model of a network good and apply it to the specific case of explaining protest participation. In this model, the utility an individual receives from engaging in protesting activity is comprised of a standalone benefit, which does not depend on the expected number of others participating in the protest, and of a network benefit, which increases with the expected number of participants. If the standalone benefit is larger than the network benefit, we speak of weak network effects. If the network benefit is larger than the standalone benefit, network effects are strong. Under weak network effects, there is a stable equilibrium number of protest participants for any level of participation costs. If, however, network effects are strong, then exactly two stable equilibria emerge: one in which there is no participation and one with full participation. We examine implications of the model empirically using US survey data from the American National Election Studies. We use a spatial econometric approach to examine the direct impact of individuals' characteristics on protest behavior and relate it to the indirect, that is, the network-driven impact of these characteristics on their protesting behavior. We find evidence for both standalone benefits and network benefits playing a role in the individual protest decision. Across various sub-groups of the sample, we find evidence for strong network effects in the protesting decision.

Our study provides insights into how protests can develop and into the determinants of their success. In particular, the insights can help explain heterogeneity observed in the dynamics and the success of protest movements.

The empirical analysis of this chapter is limited by data availability. Our theoretical model is dynamic. The data we use to examine implications of the model are cross-sectional. Hence, we cannot observe changes in protesting behavior of any given individual. We can only examine whether the patterns we see in the cross-sectional data are consistent with what we would expect, given the model. Furthermore, the individual-level data on protesting behavior is not very granular. We do not observe what kinds of protests an individual attended or how often an individual attended a protest. Hence, when it comes to the examination of network effects in sub-groups of individuals, we cannot observe whether individuals within one sub-group actually went to the same type of protest. The closest we get to this kind of analysis is clustering individuals by their political leanings and hence by groups which should potentially be interested in protesting for similar causes, and then examining network effects in protesting activity within these clusters.

The fifth chapter, titled *Why is Europe Lagging Behind in High Tech Sectors? The Role of Institutional and Regulatory Quality*, revisits the role of institutional frameworks as a determinant of investment. Specifically, we study how institutions shape the allocation of investment across industries differing in the intensity of innovative activity. Disruptively innovative activities are characterized by high uncertainty. This kind of business environment requires the ability to engage in trial-and-error processes and to rapidly respond to changing circumstances or new technological insights by scaling activities up or down. Institutional frameworks can determine how free economic agents are in these choices. Thus, institutions that reduce uncertainty and allow firms flexibility in their business endeavors may encourage investment in these risky high tech sectors. In institutional environments which do not share these characteristics, investment in mature industries in which technologies are well established and innovation occurs only incrementally may be relatively more profitable. We use data on 25 European Union countries observed over the time period from 2004 until 2019 to examine how institutional framework conditions affect investment shares in innovative sectors. Our results indicate that countries with higher institutional quality, more flexible labor market regulation and lower regulatory barriers to starting a business experience higher shares of investment in high tech sectors, in sectors with higher patenting activity and in artificial-intelligence-intensive sectors.

Our findings contribute to an ongoing policy debate about European competitiveness and the widening productivity growth gap between the EU and the United States. This topic has gained even more relevance with geopolitical shifts and technological breakthroughs observed in recent years. It has been pointed out that Europe's lagging behind in productivity growth is accompanied by an innovation gap. Our study highlights the role of institutional and regulatory frameworks in shaping these patterns.

Of course, this study also has its limitations. While we examine the role of institutional and regulatory quality for investment in high-tech industries, we cannot say much about other structural conditions which may impact sectoral investment patterns, such as access to finance, infrastructure or education. In fact, our analysis even suggests that institutional factors alone cannot explain the entire gap in high tech sector investment between the EU and the US, suggesting that other factors also play a role. Also, it should be noted that our empirical approach examines the determinants of sectoral investment shares within a country. That is, we can only make claims about the effect of institutions on the allocation of investment across sectors, not about investment levels.

The sixth chapter concludes the dissertation. Overall, the studies presented here provide insights into the role of institutions and their impact on observed economic outcomes.

Chapter 2

Institutions as a Determinant of FDI and the Role of Natural Resources[1]

Abstract

This study examines the link between institutional quality and foreign direct investment (FDI) flows to developing countries. The link is investigated at different levels of host countries' natural resource endowment. Weak institutions can be expected to attract FDI in natural resource abundant countries since they facilitate rent seeking behavior which is commonly thought to be prevalent in the natural resources sector. However, weak institutions also increase uncertainty, thus discouraging investments involving initial sunk costs as large as they commonly are in the natural resources sector. The aim of this study is to empirically assess how natural resource endowment moderates the effect of institutions on FDI. Using data on 117 developing and emerging countries over the time period 1996–2019, I estimate a dynamic panel model using the system generalized method of moments (GMM) estimator. I find a positive effect of institutional quality on FDI inflows only for countries with relatively high levels of natural resource endowment. The results are significant as they provide evidence for a narrative which is inconsistent with the results of earlier empirical research. They indicate that a higher natural resource endowment increases the importance of institutional quality as a determinant of FDI.

Keywords: *institutions, corruption, foreign direct investment, natural resources*

[1] A version of this chapter has been published in *Resources Policy*, Volume 99, December 2024 (Bothner, 2024).

2.1 Introduction

Over recent decades, worldwide foreign direct investment (FDI) has increased immensely. As Table 2.1 shows, average yearly global FDI inflows rose from about 992.8 billion USD over the time period 2000–2004 to about 2043.9 billion USD over the time period 2015–2019, an increase of approximately 106 per cent. High-income countries still receive the majority of global FDI. Yet, FDI flows to developing and emerging countries have also increased substantially. The average share of global FDI received by those countries which the World Bank classifies as low and middle income countries increased from 17.6 per cent over the time period 2000–2004 to 33.4 per cent over the time period 2015–2019. The majority of these FDI flows to non-high-income countries goes to countries classified as upper middle income countries which received on average about 24.7 per cent of global FDI over the time period 2015–2019. However, low income countries and lower middle income countries have also seen their FDI inflows increase substantially, relatively speaking, with the average share going to low income countries increasing from 0.4 to 0.8 per cent and the average share going to lower middle income countries increasing from 2.6 per cent to 7.9 percent. For poorer countries, FDI inflows can bring a lot of benefits. They are a source of capital and can thus contribute to economic growth. Also, FDI can be linked to technological spillovers (Blomström and Kokko, 1998; Wang, 1990; de Mello Jr., 1999; Barrell and Pain, 1999) and it can have positive employment effects (Nunnenkamp and Bremont, 2007). Hence, the question of what determines FDI inflows is especially relevant for less developed countries.

Table 2.1: Stylized Facts.

	2000–2004	2005–2009	2010–2014	2015–2019
Global net FDI inflows (in billion USD)	992.8	2,164.7	2,094.5	2,043.9
FDI share: low & middle income (in %)	17.6	21.3	32.5	33.4
FDI share: low income (in %)	0.4	0.4	0.9	0.8
FDI share: lower middle income (in %)	2.6	4.5	5.5	7.9
FDI share: upper middle income (in %)	14.6	16.4	26.1	24.7

Note: Author's own calculations using data from the World Development Indicators (WDI) by the World Bank (2022). All values are yearly averages over the respective time period. All values are rounded to the first decimal.

One potential determinant of FDI flows that has received much attention in the literature is the host country's institutional environment. North (1991) outlines the role of institutional frameworks in reducing uncertainty for investors. This is especially relevant to foreign investment since any activity on foreign markets already implies higher risks. Hence, it is plausible that institutions that reduce uncertainty, e.g. by constraining arbitrary decision-making and opportunistic behavior by government agents, lead to more FDI.

Extractive industries are often thought to have a special relationship with a country's institutional framework. As a strategically important sector, the natural resource sector is typically strictly controlled and regulated by host country governments. This can raise the incentive for industry agents in this sector to influence host country government officials in their favor. Thus, it seems plausible that institutions which enable corruption and rent-seeking by politicians or bureaucrats make a country a more attractive destination for FDI in the natural resource sector. Asiedu and Lien (2011) find empirical evidence that democracy only affects FDI positively in developing countries with low natural resource endowments, which might be a finding in support of this rent-seeking hypothesis.

At the same time, however, investments in extractive industries are typically characterized by high sunk costs and uncertainty regarding the profitability of the investment (Barham et al., 1998). When a multinational enterprise's (MNE) access to natural resources is strongly dependent on decisions by a host country's government and its agents, this creates a potential hold-up problem. The investor may fear that the host country government will exploit the initial specific investment, for example by renegotiating contracts after the sunk costs have been incurred or even by expropriation of the foreign investor. Governments that are not constrained in their capability to interfere in economic activity or to exploit investments of MNEs can thus present an element of uncertainty in the foreign investor's decision to engage in a host country. Investments in activities with high asset specificity, as in the natural resource sector, are especially at risk to be negatively impacted by such unconstrained behavior of government agents. High-quality institutions may reduce the likelihood of such exploitation occurring in the eyes of the investor. Thus, higher institutional quality may be especially relevant as a determinant of FDI in the natural resources sector.

To get an idea of the overall relationships between FDI, institutional quality and natural resource endowment in developing countries, let us split a sample of 117 non-high-income countries into two groups based on their resource endowment. Figure 2.1 shows the relationship between FDI inflows and the control of corruption index of the Worldwide

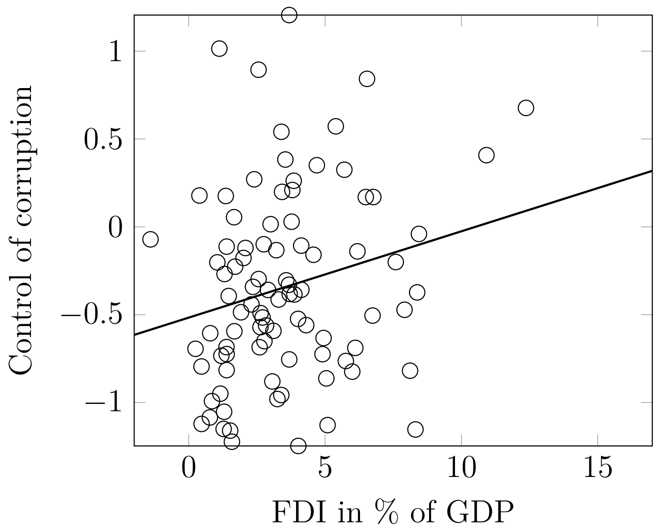

Figure 2.1: Countries with less than 50 % natural resource exports
Note: For the 117 countries listed in Table 2.5 the variables control of corruption, FDI as a percentage of GDP, and natural resource exports as a percentage of total merchandise exports are averaged over the time period 1996-2019. Depicted are only those 90 countries with a natural resource export share of less than 50 per cent.

Governance Indicators (WGI) by the World Bank (2022) for those developing countries with a share of natural resources in their total merchandise exports of less than 50 per cent. Figure 2.2 provides the analogous scatter plot for those countries with a share of natural resources in their total merchandise exports of more than 50 per cent.

Comparing the two figures, we can see that countries with high natural resource endowments are, on average, characterized by lower institutional quality, as indicated by the control of corruption index. Regarding the relation between FDI and the measure of institutional quality in the two groups, it is interesting to note that in both cases the slope of the line of best fit is positive. Looking only at the raw data, it does not seem implausible that institutional quality positively affects FDI in countries which are not resource endowed as well as in those with high natural resource endowments.

As outlined above, it is theoretically unclear whether institutional quality affects natural-resource-seeking FDI positively or negatively. Perhaps this is the reason why the empirical evidence on this question does not consistently point in one direction. Carril-Caccia et al. (2019) find that institutions have a stronger positive effect on FDI in oil-producing countries, a result that seems to contradict that of Asiedu and Lien (2011). The lack of conclusive empirical evidence shows that further examination of this issue is warranted. The study at hand aims to contribute to the closure of this gap in the literature by examining the effect of institutional quality on FDI inflows at different levels of host

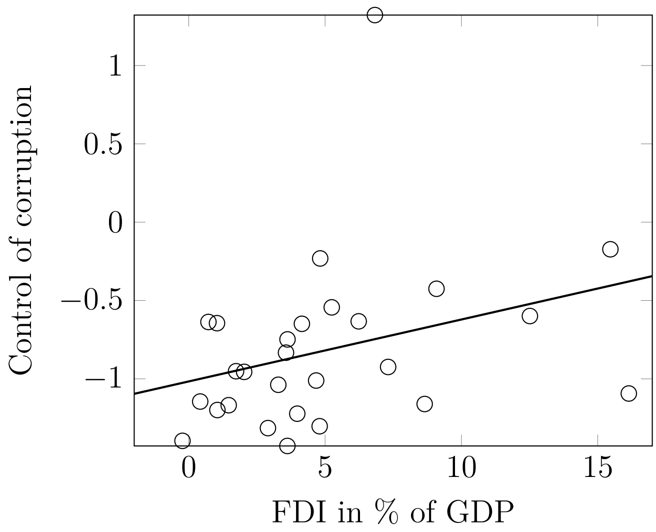

Figure 2.2: Countries with more than 50 % natural resource exports
Note: For the 117 countries listed in Table 2.5 the variables control of corruption, FDI as a percentage of GDP, and natural resource exports as a percentage of total merchandise exports are averaged over the time period 1996-2019. Depicted are only those 27 countries with a natural resource export share of more than 50 per cent.

countries' natural resource endowment.

The remainder of this article is structured as follows: Section 2.2 provides a review of related literature. In Section 2.3, the data and the empirical framework are presented. Section 2.4 shows and discusses the results. In Section 2.5, some robustness checks are conducted. Section 2.6 concludes.

2.2 Literature review

The relationship between institutional quality and economic development has been the subject of a vast body of literature. Empirical evidence indicates an overall positive effect of institutions on economic growth (Dawson, 1998; Dollar and Kraay, 2003; Rodrik et al., 2004; Acemoglu et al., 2005; Vianna and Mollick, 2018; Gründler and Potrafke, 2019; Röthel and Leschke, 2023). When it comes to the particular effect of institutions on FDI, however, the nature of the relationship is not as clear. In fact, there are theoretical arguments for a positive effect of institutional quality on FDI, while others predict a negative relationship between the two. In this section, I first review the literature on the different channels through which institutions may affect FDI in general. I then present existing literature examining the heterogeneity of these effects with respect to the natural resource endowment of the host country.

Let us first turn to channels through which institutions may positively affect FDI. A common argument for a positive effect of institutional quality on FDI emphasizes uncertainty as a deterrent of investment. A state seeking to attract FDI faces a time-inconsistency problem: FDI is ex post immobile and thus, once the investment has been made, disinvestment is costly (Vernon, 1971; Jensen, 2003). The state thus needs to credibly commit to not infringing on the foreign investor's property rights after the investment has been made and sunk costs have been incurred. Without institutions which constrain the executive, the foreign investor lacks assurance that their property rights will be respected in the long term. Property rights infringements by the host country government need not necessarily take the form of outright expropriations of firms or industries. They may also occur through 'creeping expropriation' (Graham et al., 2018) with the state changing taxes or renegotiating contracts or through corruption and rent-seeking by government officials placing additional costs on MNEs. The increase in MNEs' costs through government corruption and rent-seeking is unpredictable in magnitude and thus increases uncertainty for the foreign investor also with regard to their budgeting (Li and Resnick, 2003).

On the other hand, an institutional framework that protects private property rights and limits the state's ability to opportunistically engage in rent-seeking can serve as a commitment device, making the intention of the host state to not exploit the private investment more credible in the eyes of the foreign investor (Henisz and Williamson, 1999; Henisz, 2000). Strong institutions can thus decrease uncertainty and encourage private investment (North, 1991). While this argument holds for any kind of private investment – domestic or foreign – Henisz and Williamson (1999) show that it is especially relevant for FDI because additionally to the general insecurity of private assets associated with weak institutions, MNEs face the 'liability of foreignness'. According to Henisz and Williamson (1999), the political risk a foreign investor faces in a host country with weak institutions consists of a 'direct political hazard' and an 'indirect political hazard'. The direct political hazard results from the host state's politicians' and bureaucrats' ability to opportunistically seek rents for their own benefit. While this may imply raised costs for local and foreign firms, foreign firms can be expected to be affected worse since they have worse access to the political process. This is in line with Aizenman and Spiegel (2006) modeling weak institutions as raising all firms' costs like a tax, with the cost increase being larger for foreign firms. They argue that domestic firms are more familiar with the institutional environment of the host country and thus find it easier to circumvent the obstacles put up by weak institutions and corruption. The indirect political hazard, according to Henisz and Williamson (1999), is associated with weak institutions creating an uneven playing field to the advantage of local competitors and to the disadvantage of the MNE. Weak institutions might enable host country firms to opportunistically influence political decision makers to grant them favors at the expense of foreign firms. Host country politicians may be willing to follow suit because favoring a domestic firm at the expense of a foreign firm might be associated with relatively low political cost or could even be beneficial in terms of public opinion.

The idea that weak institutions can create an uneven playing field to the favor of domestic firms is also expressed in the international business literature concerned with regulatory capture. Here it is suggested that weak institutional environments can lead to national competition agencies being influenced by domestic industry in order to receive protection from foreign competition (Mariotti, 2023; Carpenter and Moss, 2013).

The theoretical arguments outlined above posit a positive effect of institutions on FDI. In the terms of the eclectic paradigm of international production by Dunning (1977, 1988), one could say these arguments show that a host country's institutions can constitute a location-specific advantage attracting FDI. Many empirical studies have found evidence for such a positive effect of institutional quality on FDI (Bayraktar, 2013; Biglaiser and Staats, 2010; Jensen, 2008; Huynh et al., 2020; Tunyi and Ntim, 2016; Wei, 2000; Bénassy-Quéré et al., 2007; Asiedu, 2006).

There are, however, also hypotheses positing a negative effect of institutions on FDI. One argument is based on the idea of 'greasing-the-wheel'-corruption. Proponents of this idea suggest that corruption can facilitate investment if it helps the investor to circumvent regulatory obstacles by paying 'speed money', i.e. bribing officials (Egger and Winner, 2005; Zhu and Shi, 2019). Aidt (2009) argues, however, that even if corrupt practices can lead to efficiency gains in restrictive institutional environments, that does not mean corruption or institutions enabling corruption are efficient. The argument is that the same obstacles that are circumvented with 'speed money' are often put in place by politicians or bureaucrats with the intention of seeking rents in the first place. That is, the obstacles themselves are a result of corruption and rent-seeking and the incentive to invest would thus be larger if there was no corruption at all.

Another argument for weak institutions enabling FDI is based on the idea that, unlike suggested by Henisz and Williamson (1999), institutions enabling rent-seeking behavior by decision makers can give foreign firms a competitive advantage over local firms in the host country. According to the eclectic paradigm by Dunning (1977, 1988) firms need an ownership specific advantage in order to engage in multinational activity. This ownership specific advantage may grant the MNE relatively large market power compared to local firms in the host country. Thus, the MNE may have a better bargaining position in interactions with the host country government which it can use to secure a monopoly or oligopoly position in the host country. In countries with institutions that facilitate rent-seeking by government officials, it is argued, MNEs will find it easier to use their market power to secure monopoly rents or otherwise gain favors at the expense of local competitors or the population (Li and Resnick, 2003). Several authors have argued that this facilitation of sharing higher rents between governments and MNEs makes autocracies more attractive as FDI destinations than democracies (O'Donnell, 1978; Escribà-Folch, 2017; Resnick, 2001). Indeed, it has been argued that a large market power and a strong bargaining position of the MNE can help solve the commitment problem of corrupt host country governments regarding the protection of MNEs' investments after sunk costs have been incurred (Fagre and Wells, 1982; Lecraw, 1984; Gomes-Casseres, 1990; Murtha, 1993). Given that FDI is sunk after investment, however, this should only be the case if the ex ante bargaining power of the MNE over the host country government persists after the sunk costs of investment have been incurred (Henisz and Williamson, 1999; Teece, 1986). That is, if, due to the ex post immobility of FDI (Vernon, 1971), investment in the host country implies a 'fundamental transformation' (O. E. Williamson, 1987) of the market structure with the host government gaining bargaining power over the MNE, then ex ante bargaining power of the MNE may not solve the commitment problem.

Thus far, we have identified three general channels through which the quality of a host country's institutions may affect FDI which are discussed in the literature: first, the positive effect of reduced uncertainty creating a friendlier investment climate; second, the positive effect of better institutions discouraging local firms to opportunistically influence host country decision makers at the expense of foreign firms; and third, the negative effect of better institutions limiting the ability of the foreign investor to influence host country government officials to their own benefit. We now turn to the question of how natural resource endowment may influence the relevance of these channels.

There are two characteristics of the natural resources sector which may alter the relationship between institutions and FDI: first, as a strategic sector, the natural resources sector is usually tightly regulated by host country governments. Thus, government officials can usually exercise control over private access to the natural resources sector. Second, natural resource investments have characteristics which, in theory, make them vulnerable to the hold-up problem described by O. E. Williamson (1987). That is, they are highly specific and characterized by high asset immobility, asset specificity, high capital intensity, high initial sunk costs, high risk regarding the profitability of the investment, long periods of costly exploration without revenue, and a long-term orientation (Barham et al., 1998; Otto, 2006; Andrews-Speed, 1998; Saidu, 2007; Vivoda, 2011). Barham et al. (1998) identify several factors leading to relatively high sunk costs and specificity in the natural resource sector: the facilities that need to be installed to extract natural resources are large and costly to relocate, leading to high site-specificity. In the natural resources sector, installations also tend to be specifically tailored to extracting certain raw materials. These investments are often accompanied by investments in supporting infrastructure, like transport or storage systems, which cannot be put to a different use. The necessity of investing in complementary assets such as transport infrastructure is intensified by the fact that the extraction of natural resources tends to occur in remote areas. Moreover, investments in the natural resources sector are characterized by high transaction costs. For example, in the exploration stage, searches for natural resources are conducted, the outcomes of which are uncertain. The search costs incurred are sunk. Another relevant factor is the high price volatility of natural resources which can impact the profitability of specific investments and raises uncertainty. These characteristics identified by Barham et al. (1998) lead to high sunk costs, asset specificity, and uncertainty of revenues of investments in the natural resource sector.

Given the tight control host country governments tend to have over natural resources, it has been suggested, that natural resources seeking FDI may be attracted to weak institutions. A common argument for why this might be the case is based on the idea that the government's control over natural resources invites rent-seeking behavior and corruption. The relation between a country's natural resource endowment and incentives for rent-seeking behavior has received a lot of attention in studies examining the so-called ‘resource curse’ (Ramsay, 2011; Busse and Gröning, 2013). This literature typically treats institutional quality and rent-seeking behavior or corruption as endogenous and examines how they are affected by the presence of natural resources. One argument why this might be the case is that governments that experience high revenues from natural resources do not develop an effective tax system, thus weakening state capacity and establishing an environment promoting rent-seeking activities (Karl, 1997). Also, the presence of large natural resource rents themselves may give government agents the incentive not to establish institutions which make it harder for them to inappropriately extract these rents (Ross, 2009). Several empirical studies have found evidence for a positive link between natural resources and corruption (Arezki and Brückner, 2011; Sala-i Martin and Subramanian, 2013).[2]

The vulnerability of the natural resources sector to rent-seeking and corruption shown by the resource curse literature may alter the relationship between a host country’s institutions and FDI. Wright and Zhu (2018) point out that the large capital requirements and the high sunk costs associated with natural resource investment constitute market entry barriers. Thus, the natural resources sector tends to be highly concentrated and there is the possibility for firms to retain monopoly or oligopoly positions. Weak institutions can then be attractive to natural resource seeking FDI, the argument goes, because in such an institutional environment the MNE may find it easier to opportunistically influence rent-seeking government officials in order to gain access to the natural resources sector, secure monopoly or oligopoly positions, and extract the corresponding monopoly or oligopoly rents. In other words, weak institutions may raise the potential benefit from opportunistic behavior by government officials or MNEs. This moderation effect of natural resources is essentially one of the explanations provided by Asiedu and Lien (2011) when they only find a positive effect of democratization on FDI for countries with low natural resource endowments but not for countries with high natural resource endowments. They argue that in autocracies multinational firms may find it easier to benefit from personal ties to decision-makers when engaging in FDI in the natural resource sector.[3] Kucera and Principi (2014) also examine the link between democracy and FDI while differentiating

[2]For an overview of the literature on the natural resource curse, see Ross (2015).

[3]In a similar study, Asiedu (2013) examines whether there is a resource curse in FDI in developing countries and whether a higher institutional quality mitigates such a resource curse. She finds that natural resource endowment negatively affects FDI and that this resource curse is mitigated by higher institutional quality.

between different industries. While they find the aggregate effect of democracy on FDI to be positive, they find a negative effect of democracy on FDI for mining and oil and gas extraction.

There is, however, also an argument in favor of the moderation of the institutions–FDI relationship by natural resource endowment to work in the opposite direction. The high sunk costs and the high asset specificity of investments in the natural resources sector make FDI in natural resources highly vulnerable to hold-up (O. E. Williamson, 1987). The credible commitment problem of host country governments described by Henisz and Williamson (1999) and Henisz (2000) in attracting FDI is more relevant for more specific investments. Thus, it can be argued, that FDI in natural resources is exposed to risks associated with opportunistic behavior by host country governments or local firms (Hefeker and Kessing, 2017) and should be more sensitive to uncertainty inducing institutions than other types of investment. This is in line with Carril-Caccia et al. (2019) finding the positive effect of institutional quality on greenfield FDI to be larger for oil-abundant countries. The high initial sunk costs and the high asset specificity in the natural resources sector suggest that investors in this sector need a lot of assurance regarding the long term security of their assets (Vivoda, 2011). This is consistent with studies identifying strong institutions, secure property rights, low corruption, and policy stability as determinants of the location decision of firms in extractive industries (Morgan, 2002; Tole and Koop, 2011).

The elaborations above show that, theoretically, the direction of the effect of natural resource endowment on the link between institutions and FDI is unclear. The empirical evidence appears to be inconclusive, with the findings of Asiedu and Lien (2011) and Carril-Caccia et al. (2019) being seemingly at odds with each other.

In this paper, I aim to contribute to the literature by empirically examining the relationship between institutional quality and FDI and the moderation of this relationship by natural resource endowment. The methodology is based on the one applied by Asiedu and Lien (2011) to examine the relationship between democracy, FDI, and natural resources. However, the common hypotheses which posit positive effects of democracy (Jensen, 2003) or autocracy (Li and Resnick, 2003; O'Donnell, 1978; Resnick, 2001) on FDI overall and on FDI in natural resource abundant countries in particular (Asiedu and Lien, 2011) tend to rely on hypotheses about effects of institutional characteristics often associated with democracy or autocracy on FDI and not on effects of democratization itself. I thus consider it worthwhile to examine the effect of institutions on FDI directly. This is consistent with Biglaiser and Staats (2010) finding that institutional environments and political risk commonly associated with democracies are more relevant as determinants of FDI than democratization itself. Hence, whereas Asiedu and Lien (2011) examine the relationship between democracy and FDI, I examine whether natural resource endowment moderates the relationship between institutional quality, measured by a corruption indicator, and FDI.

2.3 Data and empirical approach

The sample covers 117 developing and emerging countries over the time period 1996–2019. Table 2.5 in the Appendix lists the countries included in the sample. All the data stem from the World Development Indicators (WDI) of the World Bank (2022). In the estimation approach and in the choice of control variables, I closely follow Asiedu and Lien (2011). The dynamic panel model to be estimated is the following:

$$\mathbf{FDI}_{it} = \gamma\mathbf{FDI}_{it-1} + \beta_1\mathbf{inst}_{it} + \beta_2\mathbf{resource}_{it} + \beta_3\mathbf{inst}_{it} \times \mathbf{resource}_{it} + \beta_4\mathbf{X}_{it} + \delta_t + \epsilon_{it} \quad (2.1)$$

The dependent variable FDI_{it} is defined as inward FDI flows as a percentage of GDP in country i and year t to account for differences in market size. To account for cyclical fluctuations in FDI flows, all variables are averaged over 4-year periods, resulting in the sample covering six time periods: 1996–1999, 2000–2003, 2004–2007, 2008–2011, 2012–2015, and 2016–2019.

The variable $inst_{it}$ indicates the quality of institutions. The literature review in Section 2.2 has shown that the channel through which institutions may affect FDI negatively is by restricting the feasibility of rent-seeking by government officials and by limiting the influence MNEs have over politicians or bureaucrats. This is why I measure institutional quality using the control of corruption index of the Worldwide Governance Indicators (WGI) by Kaufmann et al. (2010). This index captures “perceptions of the extent to which public power is exercised for private gain” (Kaufmann et al., 2010, p. 4). It ranges from about -2.5 to 2.5 with higher values indicating less perceived corruption, i.e. a higher quality of institutions. The choice of this institutional variable thus speaks to the rent-seeking argument: If it is indeed the possibility of influencing government officials which makes a country with weak institutions an attractive destination for FDI, and if it is this channel which is more relevant for FDI in natural resource abundant countries, as suggested by authors such as Asiedu and Lien (2011), then I expect the control of corruption index to adequately capture this mechanism. The control of corruption index and all other institutional variables used in this study are normalized to range from zero to one.

The variable $resource_{it}$ measures a country's natural resource endowment. Following Asiedu and Lien (2011), natural resource endowment is measured as natural resource exports as a percentage of total merchandise exports. The World Bank (2022) provides data on fuel exports as a percentage of merchandise exports as well as on ores and metals exports as a percentage of merchandise exports. These two percentages are summed up to construct the natural resource variable. Due to the way these variables are defined by the WDI the natural resource endowment variable thus includes commodities in the SITC sections 3 (mineral fuels, lubricants, and related materials), 27 (crude fertilizer and crude minerals), 28 (metalliferous ores and scrap), and 68 (non-ferrous metals). $inst_{it} \times resource_{it}$ is the interaction of the institutional variable and the natural resource variable. Including the interaction term allows us to evaluate the relationship between institutions and FDI at different levels of the natural resource variable. If higher institutional quality discourages FDI in host countries with high natural resource endowments, we would expect negative estimates on control of corruption at high levels of the natural resource variable. If, on the other hand, higher institutional quality is especially relevant for FDI in countries with high natural resource endowments, we would expect positive estimates on control of corruption at high levels of the natural resource variable.

X_{it} is a vector of control variables, δ_t is a time-period fixed effect and ϵ_{it} is the country- and year-specific error term, which is assumed to include a country-specific component.

The following control variables are included in the regression: the trade volume (exports plus imports) as a percentage of GDP as a measure of a country's trade openness, GDP per capita in constant 2015 USD, a measure of infrastructure development, and inflation volatility.

The effect of trade openness on FDI is theoretically unresolved. In accordance with the tariff jumping hypothesis, trade openness is expected to be positively related to vertical

FDI and negatively associated with horizontal FDI (Helpman et al., 2004). Since all the countries in the sample are developing and emerging countries which tend to be more attractive destinations for vertical FDI, one might expect the estimated coefficient on trade openness to be positive.

GDP per capita and economic growth can be considered indicators of the attractiveness of a country's market and are thus expected to be positively linked to FDI. As there is evidence that the relationship between FDI and economic development might not be linear (Asiedu and Lien, 2003; Davies, 2008) I follow the approach of Asiedu and Lien (2011) and also control for the square of GDP per capita.

Inflation volatility is controlled for as a proxy for overall macroeconomic stability. It is measured as the absolute year-to-year difference in the inflation rate. In a robustness check, the absolute value of the inflation rate itself is used instead. The absolute values of the inflation rate are used because otherwise positive and negative inflation rates would cancel each other out when taking the 4-year average of the variable, thus creating an inadequate proxy for macroeconomic stability.

Gross fixed capital accumulation as a percentage of GDP is used to measure infrastructure development. According to the World Bank (2022), this variable includes "land improvements (fences, ditches, drains, and so on); plant, machinery, and equipment purchases; and the construction of roads, railways, and the like, including schools, offices, hospitals, private residential dwellings, and commercial and industrial buildings." This is a very broad measure of infrastructure development, and it raises obvious concerns about endogeneity, since the FDI inflows, by definition, lead to capital accumulation. To account for these issues, two other measures of infrastructure development are employed in robustness checks: the number of fixed telephone subscriptions per 100 people and the number of broadband subscriptions per 100 people.

In further robustness checks, I control for a different measure of corruption from the V-Dem dataset by Coppedge et al. (2022a), three different democracy indices from Freedom House (2023), the Polity project of the Center for Systemic Peace (2022), and the V-Dem dataset by Coppedge et al. (2022a), and the political constraint index by Henisz (2002), which was also obtained from the V-Dem dataset by Coppedge et al. (2022a).

With the exception of the institutional variables, all variables enter the equation in logarithmic form. Since some of the variables include negative and zero values, the natural logarithm of each variable is constructed using the following inverse hyperbolic sine transformation [4]:

$$y = ln(x + \sqrt{(x^2 + 1)}) \tag{2.2}$$

Table 2.2 shows the summary statistics of the used variables.

Estimating the dynamic model indicated by Equation (2.1) using the pooled OLS or the

Table 2.2: Summary statistics

	Mean	SD	Min	Max	N
ln(FDI/GDP)	1.61	0.91	−2.50	4.13	697
Control of corruption	0.38	0.19	0.00	1.00	697
Political corruption	0.61	0.26	0.00	1.00	683
ln(Natural resources)	3.06	1.49	0.00	5.30	631
ln(fuel)	2.06	1.67	0.00	5.30	632
ln(ores and metals)	1.97	1.43	0.00	5.06	642
ln(GDP p.c.)	8.54	1.00	6.24	10.46	701
ln(Trade openness)	4.93	0.50	1.11	6.05	677
ln(|Δ Inflation|)	1.89	0.92	0.21	7.38	699
ln(|Inflation|)	2.39	0.87	0.49	6.45	699
ln(Broadband subscriptions)	1.23	1.29	0.00	4.20	520
ln(Telephone subscriptions)	2.34	1.31	0.00	4.55	699
ln(Fixed capital)	3.75	0.38	1.03	4.78	663
Political rights	0.47	0.32	0.00	1.00	694
Polity	0.67	0.28	0.00	1.00	648
Electoral democracy	0.51	0.24	0.06	0.99	684
Political Constraints	0.31	0.30	0.00	0.96	682

FDI/GDP indicates FDI inflows in % of GDP. Control of corruption is the control of corruption index of the Worldwide Governance Indicators by Kaufmann et al. (2010). Natural resources is the percentage share of fuels, ores, and metals in total exports. Trade openness is the trade volume in % of GDP. The variables Broadband subscriptions and Telephone subscriptions indicate the number of broadband subscriptions per 100 people and the number of fixed telephone subscriptions per 100 people, respectively. Fixed capital indicates fixed capital accumulation as a percentage of GDP. |Δ Inflation| is the absolute year-to-year change in the inflation rate. |Inflation| is the absolute value of the inflation rate. Political rights indicates the political rights index by Freedom House. Polity is the polity index. Electoral democracy is the electoral democracy index from the V-Dem dataset. Political constraints indicates the political constraint index by Henisz (2002). The institutional variables were normalized so that they range from 0 to 1. All variables appearing in logarithmic form were transformed using the transformation indicated by Equation (2.2).

within estimator results in dynamic panel bias due to the individual-specific component of the error term (Nickel, 1981). The difference generalized method of moments (GMM)

[4] This transformation approximates the logarithmic function without encountering its problems when dealing with negative or zero values (Bellemare and Wichman, 2020).

estimator proposed by Arellano and Bond (1991) circumvents this problem by first-differencing the data and using lagged levels of the regressors as instruments for the first differences. The system GMM estimator proposed by Blundell and Bond (1998) takes this approach further by building a system of two equations: The equation in first differences and the original equation in levels. Past differences are used to instrument levels to estimate the equation in levels. The introduction of more instruments leads to efficiency gains compared to the difference GMM estimator, especially in situations in which the path of the dependent variable is close to a random walk (Roodman, 2009). In this study, I use the system GMM estimator to estimate the model given by Equation (2.1).

Using the system GMM estimator has the additional advantage, that it can address some further endogeneity issues arising in this study. The two variables of interest, natural resource endowment and control of corruption, can be expected to be endogenous. If FDI in resource-rich countries occurs primarily in the natural resources sector, then more FDI can be expected to positively affect the share of natural resources in total exports, since more FDI will then lead to more natural resource extraction. Regarding the endogeneity of corruption, there is literature suggesting that FDI may impact the quality of institutions in a host country (Donaubauer et al., 2018).

For the variables suspected to be endogenous – i.e. natural resources, institutions, and the interaction term of natural resources and institutions – the first lag is not included in the instrument sets. All other explanatory variables (with the exception of the time dummies) are treated as predetermined and all available lags are used as instruments. That is, in the first differences equation, the first lag and all further lags (up until the fifth lag) of the predetermined variables are used as instruments and the second lag and all further lags of the endogenous variables (and the lagged dependent variable) are used as instruments. In the levels equation, first differences of the predetermined variables and lagged differences of the endogenous variables are used as instruments. The time period dummies enter the instrument set in the levels equation but not in the first differences equation. In robustness checks, the sensitivity of the main results to adjustments in the number of lags used is examined.

I use the two-step estimator, which is asymptotically efficient, with Windmeijer (2005) finite sample correction of the standard errors.

An assumption for system GMM to be valid is the joint validity of the moment conditions

arising from the choice of the instrument set (Roodman, 2009). For each system GMM estimation in this study, I report the P-value of the Hansen (1982) J Test statistic for the joint validity of the moment conditions. A potential problem in the application of system GMM is instrument proliferation. While one cannot generally say how many instruments are too many, an arbitrary rule of thumb states that the number of instruments should not exceed the number of individuals in the sample. In each estimation I conduct, I bring down the number of instruments by collapsing the instrument sets (Roodman, 2009). Moreover, for each system GMM estimation I report the number of instruments and the number of individuals, i.e. the number of countries. I also check the robustness of the main results to further reductions in the number of instruments by limiting the number of lags used in the construction of the instrument sets.
The use of some lags as instruments for first differences may be invalid if there is first-order autocorrelation in the idiosyncratic component of the error term (Roodman, 2009). To address this potential concern I also report the P-values of the Arellano–Bond tests for first-order and second-order serial correlation in differences for each system GMM estimation.

2.4 Results

In a first step, I estimate the model indicated by Equation (2.1) without including any control variables beyond the lagged dependent variable, control of corruption, natural resources, and the time period dummies, using the pooled OLS estimator, the within estimator, and the system GMM estimator. The results are shown in columns 1, 2, and 3 of Table 2.3, respectively. In column 1, the OLS estimate on control of corruption is positive and statistically significant at the 10 per cent level. The statistical significance of control of corruption disappears in the fixed effects estimation, the results of which are shown in column 2. In the system GMM estimation in column 3, which accounts for the dynamic panel bias, the estimate on control of corruption is positive and statistically significant at the 5 per cent level. The OLS estimate and the within estimate on the lagged dependent variable indicate the upper bound and the lower bound of the unbiased estimate. It is thus not surprising that the system GMM estimate on the lagged dependent variable lies between the OLS estimate and the within estimate. The result of the system

GMM estimation reported in column 3 of Table 2.3 indicates an overall positive effect of institutional quality on FDI.

Next, I estimate the model indicated by Equation (2.1), including the interaction of

Table 2.3: Baseline results

Dependent variable: ln(FDI/GDP)	(1) OLS	(2) FE	(3) GMM	(4) OLS	(5) FE	(6) GMM	(7) GMM
ln(FDI/GDP)$_{-1}$	0.686***	0.168***	0.567***	0.687***	0.171***	0.594***	0.378***
	(0.039)	(0.059)	(0.072)	(0.039)	(0.061)	(0.085)	(0.095)
Control of corruption	0.308*	0.310	1.432**	0.308*	0.158	1.820***	1.071
	(0.162)	(0.689)	(0.617)	(0.162)	(0.694)	(0.678)	(1.005)
ln(Resources)	−0.007	0.101*	0.073	−0.007	0.080	−0.038	0.151
	(0.021)	(0.051)	(0.165)	(0.021)	(0.055)	(0.132)	(0.129)
Control of corruption×ln(Resources)				0.112	0.609*	0.284	0.875*
				(0.119)	(0.331)	(0.544)	(0.498)
ln(GDP p.c.)							−0.253
							(0.175)
ln(GDP p.c.)×ln(GDP p.c.)							0.097
							(0.134)
ln(Trade openness)							0.832**
							(0.413)
ln(|ΔInflation|)							−0.127
							(0.086)
ln(Fixed capital)							0.600**
							(0.277)
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.513	0.138		0.515	0.151		
Number of observations	539	539	539	539	539	539	520
Number of instruments			20			25	55
Number of countries			117			117	117
P-value of Hansen J statistic			0.875			0.770	0.249
P-value of AR(1) statistic			0.000			0.000	0.000
P-value of AR(2) statistic			0.337			0.306	0.140

The dependent variable is FDI as a percentage of GDP. In the Pooled OLS estimations, standard errors are clustered at the country level. In the system GMM estimations, the standard errors are two-step Windmeijer corrected. In the system GMM estimation, control of corruption and natural resource endowment are treated as endogenous, while all other regressors are treated as predetermined. All variables other than Control of corruption are constructed in logarithmic form in accordance with the transformation given in Equation (2.2). All variables which appear in interaction terms are centered around their respective means so that the estimates on the non-interacted variables are the estimates at the respective means. *, **, and *** denote significance at 10-, 5-, and 1 per cent level, respectively.

control of corruption and natural resources among the control variables. This allows for the effect of control of corruption on FDI to be moderated by a country's natural resource endowment. Columns 4, 5, and 6 of Table 2.3 show the OLS estimates, the within estimates, and the system GMM estimates, respectively. Only the within estimate on the interaction term is statistically significant at the 10 per cent level. However, including the interaction term allows us to estimate the coefficient on control of corruption at different levels of the natural resource variable, which provides us with estimates on control of corruption at different levels of the natural resource variable even if the interaction term itself is insignificant.

Finally, the system GMM estimation is conducted while additionally controlling for GDP per capita, the square of GDP per capita, trade openness, inflation volatility, and fixed capital accumulation. This should be considered the baseline specification, and its results are presented in column 7 of Table 2.3. The estimate on the interaction of control of corruption and natural resources is positive and statistically significant at the 10 per cent level, indicating a positive moderation effect. The estimates on the other control variables have the expected signs.

For the specifications in which the interaction of control of corruption and natural resources is controlled for, I estimate the coefficient on control of corruption at different levels of the natural resource variable. Table 2.4 shows the estimates on control of corruption at the 10th percentile of the natural resource variable, at the 25th percentile, at the mean, at the 50th percentile, at the 75th percentile, and at the 90th percentile. Column 1 of Table 2.4 shows the estimated marginal effects corresponding to the pooled OLS estimation in column 4 of Table 2.3. Since the pooled OLS estimate on the interaction term is positive, the estimated marginal effect of control of corruption increases in magnitude with higher levels of the natural resource variable. However, for relatively low levels of the natural resource variable, i.e., at the 10th percentile and at the 25th percentile, the estimated coefficient on control of corruption is not statistically different from zero. At the mean and at higher percentiles of the natural resource variable, control of corruption is statistically significant at least at the 10 per cent level; at the 75th percentile, control of corruption is significant at the 5 per cent level.

Column 2 of Table 2.4 shows the marginal effects of control of corruption estimated by system GMM. These estimates correspond to the specification of the system GMM estimation without any additional control variables, the results of which are shown in column 6 of Table 2.3. The results in column 2 of Table 2.4 seem to qualitatively confirm the results in column 1. At the 10th percentile and at the 25th percentile of the natural resource variable, the estimated marginal effect of control of corruption is not statistically significant at the 10 per cent level. At the mean and at the 50th percentile, the 75th percentile, and at the 90th percentile of the natural resource variable, the estimate on control of corruption is positive and statistically significant at the 1 per cent level. Lastly, column 3 of Table 2.4 shows the marginal effects of control of corruption estimated by system GMM, when controlling for other potentially confounding variables. These

Table 2.4: Baseline results – marginal effects

Percentile	(1) OLS	(2) System GMM	(3) System GMM
10th	0.061	1.192	−0.866
	(0.306)	(1.724)	(1.599)
25th	0.195	1.533	0.186
	(0.200)	(1.117)	(1.190)
Mean	0.308*	1.820***	1.071
	(0.162)	(0.678)	(1.005)
50th	0.320*	1.852***	1.169
	(0.163)	(0.641)	(0.999)
75th	0.447**	2.173***	2.161**
	(0.221)	(0.567)	(1.099)
90th	0.519*	2.356***	2.724**
	(0.279)	(0.781)	(1.270)
Controls	No	No	Yes

The dependent variable is FDI as a percentage of GDP. Reported are the estimates on control of corruption at different values of the natural resource variable. The estimates of the marginal effect in column 1 correspond to the regression specified in column 4 of Table 2.3. The estimates of the marginal effect in column 2 correspond to the regression specified in column 6 of Table 2.3. The estimates of the marginal effect in column 3 correspond to the regression specified in column 7 of Table 2.3. In the Pooled OLS estimations, standard errors are clustered at the country level. In the system GMM estimations, the standard errors are two-step Windmeijer corrected. *, **, and *** denote significance at 10-, 5-, and 1 per cent level, respectively.

estimates correspond to the baseline specification in column 7 of Table 2.3. Figure 2.3 provides a visual representation of the point estimates on control of corruption and the 95 per cent confidence intervals at different percentiles of the natural resource variable. Again, we see that the marginal effect of control of corruption is positive and statistically significant only at relatively higher levels of the natural resource variable. At the 75th percentile and at the 90th percentile of the natural resource variable, the estimate on control of corruption is statistically significant at the 5 per cent level. At lower values of the natural resource variable, the estimate on control of corruption is not statistically different from zero.

The results indicate a positive effect of control of corruption on FDI only for countries

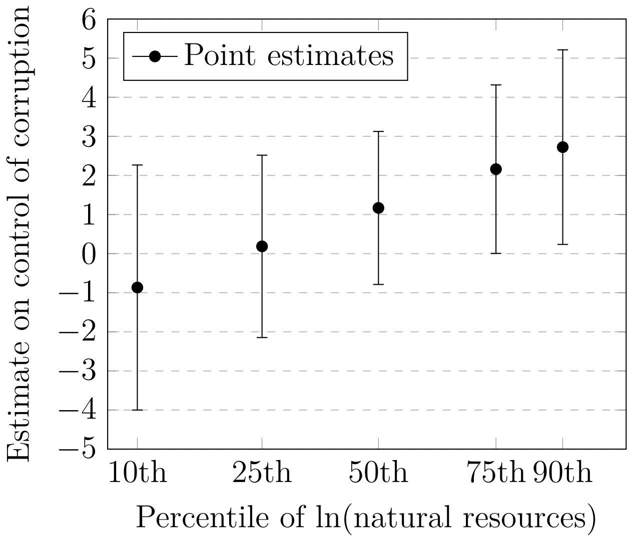

Figure 2.3: Marginal effect of control of corruption

with a sufficiently high level of natural resource endowment. This result appears to be somewhat inconsistent with the results of Asiedu and Lien (2011), who find the effect of democracy on FDI to be negative for countries with high natural resource endowments. This apparent discrepancy in the results is interesting, considering that the approach of this study is very similar to the approach of Asiedu and Lien (2011). This study, however, examines the effect of control of corruption on FDI and not that of democracy on FDI, which may explain this apparently contradictory finding. The positive estimates on control of corruption only for countries with relatively high levels of natural resource endowment indicate that institutional quality is especially important as a determinant of FDI in resource abundant countries. This can be explained by foreign investors facing a potential hold-up problem in the natural resource sector, which leads them to value institutions which decrease uncertainty in destinations for FDI. Moreover, the results seem to be in line with the finding by Carril-Caccia et al. (2019) that institutional quality positively affects greenfield FDI in oil-abundant countries.

2.5 Robustness checks

Estimating a model using system GMM involves several decisions that may seem somewhat arbitrary but can potentially influence the results. Hence, in a first set of robustness checks, I check the robustness of the baseline results to changes in the specification of the system GMM estimation.

In the baseline system GMM estimations, the instrument count was reduced by collapsing the instrument sets, and up to 5 lags were used in their construction. The instrument count can be further reduced by limiting the number of lags used to construct the instrument sets. I check the robustness of the main results to a reduction in the number of lags used in the system GMM estimation. The results are reported in Table 2.6 in the appendix.

In two separate estimations, I limit the number of lags of all control variables used to construct the instrument sets in the first difference equations to 4 and 3. The estimation results are reported in columns 1 and 2 of Table 2.6, respectively. The number of lags can also be reduced by not using the first lag of the control variables that were treated as predetermined in the main specification as instruments in the first difference equation. In a further robustness check, I repeat the estimation, using only lags 2 and 3 of all control variables to construct the instrument set in the first difference equation. Thus, in this estimation, all control variables – including those previously treated as predetermined – are treated as endogenous.[5] The results of this estimation are shown in column 3 of Table 2.6. Table 2.7 reports the marginal effects corresponding to the three estimations with reduced instrument counts. The baseline system GMM estimates on control of corruption at different levels of natural resource endowment appear to be robust to the reduction in instruments. Table 2.7 shows that only at the 75th and at the 90th percentile of the natural resource variable the coefficient on control of corruption is positive and statistically significant at the 5 per cent level. It is not statistically different from zero for countries with relatively low levels of natural resource abundance. This confirms the implication of

[5] To properly treat them as endogenous, I also use the lagged differences of all variables as instruments in the levels equation in this estimation.

the main results that only for countries with relatively high natural resource endowments, control of corruption is found to positively affect FDI.

Since this study is mainly interested in examining a moderation effect, it is appropriate to consider other potential moderation effects among the control variables. I thus repeat the system GMM estimation while additionally controlling for interactions of the natural resource variable with all of the other control variables. The estimation results are shown in column 1 of Table 2.8 and the corresponding marginal effects of control of corruption estimated at different levels of natural resources are shown in column 1 of Table 2.9. The estimated marginal effects are similar to those derived in the baseline approach. Once again, the results indicate that control of corruption positively affects FDI only at relatively large levels of the natural resource variable.

The variable used to measure natural resource endowment is constructed as the sum of the share of fuels in total merchandise exports and the share of ores and metals in total merchandise exports. To see whether any one of these components is the main driver behind the results, I repeat the estimation while replacing the previously constructed natural resource variable once by the share of fuels in total merchandise exports and once by the share of ores and metals in total merchandise exports. The estimation results are shown in columns 2 and 3 of Table 2.8 with the corresponding marginal effects of control of corruption reported in columns 2 and 3 of Table 2.9. When decomposing the natural resource variable into its two components and repeating the estimation separately for each, I do not find the same pattern as before. The effect of control of corruption on FDI is insignificant at each level of the two individual natural resource variables. Hence, it does not seem to be the case that it is predominantly oil or predominantly minerals driving the main results, but that the main results are only observable when both aspects, fuels and minerals, are considered.

I also check the robustness of the results to the usage of a different indicator of corruption. Column 4 of Table 2.8 shows the estimation results obtained when measuring corruption using the political corruption index from the varieties of democracy (V-Dem) dataset by Coppedge et al. (2022b) and Pemstein et al. (2022). This index ranges from zero to one and measures the degree to which political corruption is pervasive in a given country, with lower values indicating less corruption and higher values indicating more corruption (Coppedge et al., 2022a). Column 4 of Table 2.9 shows the marginal effects of this measure of corruption on FDI at different levels of the natural resource variable. For relatively high values of the natural resource variable, the estimate on corruption is negative and statistically significant at the 10 per cent level, which is consistent with the main results.[6]

Table 2.10 reports the estimation results obtained when controlling for different proxies for infrastructure development. Table 2.11 reports the corresponding marginal effects of control of corruption. The results are consistent with the baseline results.

In the baseline estimation, I controlled for the absolute year-to-year change in the inflation rate to account for general macroeconomic stability as a confounding factor. As a further robustness check, I control for the absolute value of the inflation rate instead. The estimation results are reported in column 1 of Table 2.12. Moreover, since there is research suggesting that there might be a nonlinear effect of inflation on economic growth (Sarel, 1996; Eggoh and Khan, 2014) and on FDI in particular (Agudze and Ibhagui, 2021), I estimate two further specifications of the model, additionally controlling for the squares of the respective inflation variables. The estimation results of these two specifications are reported in columns 2 and 3 of Table 2.12. The corresponding marginal effects of control of corruption on FDI are reported in Table 2.13. In all three specifications, the results support the main results.

Since Asiedu and Lien (2011) find that the effect of democracy on FDI is moderated by natural resource endowment, democracy and the interaction of democracy and natural resources may be confounding variables here. To address this issue, I separately control for three indicators of democracy: the political rights index by Freedom House, the Polity index by the Polity V project, and the electoral democracy index from the V-Dem dataset. Columns 1 to 3 in Table 2.14 show the respective estimation results. Columns 1 to 3 in Table 2.15 report the corresponding marginal effects of control of corruption when democracy is controlled for. The marginal effects are qualitatively similar to those obtained in the baseline specification. However, since Asiedu and Lien (2011) find the effect of democracy to differ for different levels of natural resource endowment, it seems appropriate to also control for the interaction of democracy and natural resources in this analysis. Columns 4 to 6 in Table 2.14 show the estimation results when the interaction of democracy and natural resources is included as a control variable, and columns 4 to 6 in

[6] The difference in the sign of the marginal effects between the main results and the robustness check using the political corruption index from the V-Dem dataset stems from the fact that higher values of the control of corruption index used to derive the main results indicate less corruption while higher values of the political corruption index indicate more corruption.

Table 2.15 show the corresponding marginal effects of control of corruption. The estimate on the interaction of control of corruption and natural resources is negative in two of the three estimations – i.e., those using the democracy indices from Freedom House and from the Polity project – so that the estimates on control of corruption reported in Table 2.15 decrease with higher values of the natural resource variable. However, consistent with the main results, in all three estimations, the estimate on control of corruption is only positive and statistically significant at the ten per cent level for relatively high values of natural resource endowment.

To account for political risk being a potential confounding variable, I control for the Political constraint index by Henisz (2002). Again, I control for the variable individually and interacted with the natural resource variable. The estimation results are reported in Table 2.16. The corresponding marginal effects of control of corruption are reported in Table 2.17. The results support the results of the baseline estimation. I next check the robustness of the results to dropping certain country groups from the sample. The original sample includes several countries which have been transitioning from centrally planned economies to more market-oriented economies. Many of these countries have experienced substantial economic growth and increased investment in recent decades. To address the potential concern that FDI determinants in these countries might generally differ from those in other countries in the sample, I repeat the baseline estimation while excluding such transition economies from the sample. The original sample also includes countries that are commonly thought of as tax havens. Thus, in a second check, I repeat the estimation while excluding countries that appear on the European Union list of non-cooperative jurisdictions for tax purposes from 2017 as well as countries that avoided being listed in 2017 by making certain commitments. Table 2.5 indicates the countries included in each group. Table 2.18 shows the estimation results, and Table 2.19 reports the corresponding marginal effects for each of the two estimations. Consistent with the main results, control of corruption appears to only positively affect FDI at relatively high levels of the natural resource variable.

Lastly, as a final robustness check, I split the sample at the median of the natural resource variable, creating one group of countries with relatively low natural resource endowments and one group of countries with relatively high natural resource endowments.[7]

[7]To identify countries which can be considered natural resource endowed over the entire time period of interest, I first calculate the average natural resource endowment over the six time periods for each

In these estimations the interaction of natural resources and institutions is not controlled for. Instead, we are interested in the estimate on control of corruption itself for each sub-sample. Table 2.20 shows the results of the system GMM estimations. The results are consistent with the main results. The estimate on control of corruption is positive and significant only for countries with high natural resource endowment. For countries with low natural resource endowment, the effect is insignificant.

2.6 Conclusion

This study has produced important insights regarding the link between institutional quality and FDI in developing and emerging countries. The results indicate an overall positive link between institutional quality and FDI inflows in developing countries. When allowing for the moderation of the effect of institutions by natural resource endowment, I find the positive effect of institutions on FDI only for countries with relatively high natural resource endowments. No significant effect can be found for countries with relatively low natural resource endowments. The results thus indicate a positive effect of institutional quality on FDI only if the natural resource endowment is above a certain level. They thus support the findings by Carril-Caccia et al. (2019).

The results are consistent with the idea that institutional quality is an especially relevant determinant of FDI in natural resources due to natural resource investments being characterized by high sunk costs and uncertainty.

On the other hand, the results are inconsistent with the hypothesis that institutions which encourage rent-seeking behavior make a country an attractive destination for natural resource investments. This is essentially the argument proposed by Asiedu and Lien (2011), who find the effect of democracy on FDI to be positive only for countries with low natural resource endowments. By using a corruption indicator as the variable of interest instead of a democracy indicator, I examine the effect of the propensity of rent-seeking on FDI more directly and do not find support for this hypothesis. Interestingly, the results are robust to controlling for democracy indicators.

The results of this study have important policy implications. There is a common idea that institutional quality is not an important determinant of FDI for countries with high

country in the sample. I then split the sample at the median of this average natural resource endowment, which is at a natural resource share in total merchandise exports of about 15.36%.

natural resource endowments or that it can even be a deterrent of FDI in these countries. This can lead to the conclusion that if these countries want to attract more FDI, they do not need to make efforts to improve their institutions or even that they should refrain from doing so. The results of this study tell a different story. For countries with high natural resource endowments, I find institutional quality to have a significant positive effect on FDI. This implies that countries with high natural resource endowment can reap the benefits of more FDI if they improve their institutions.

2.A Appendix

Table 2.5: Countries in the sample

America	East/South Asia & Pacific	Europe & Central Asia	Middle East & North Africa	Sub-Saharan Africa
Argentina	Bangladesh	Albania[a,b]	Algeria	Angola
Belize	Bhutan	Bulgaria[a]	Egypt	Burundi
Bolivia	China[a]	Bosnia and Herzegovina[a,b]	Iran	Benin
Brazil	Fiji[b]	Belarus[a]	Iraq	Burkina Faso
Chile	Indonesia	Armenia[a,b]	Jordan[b]	Botswana[a,b]
Colombia	India	Azerbaijan[a]	Lebanon	Central African Republic
Costa Rica[b]	Cambodia[a]	Georgia[a]	Libya	Cote d'Ivoire
Dominican Republic	Kiribati	Croatia[a]	Morocco[b]	Cameroon
Ecuador	Laos[a]	Hungary[a]	Palestine	Democratic Republic of Congo
Guatemala	Sri Lanka	Kazakhstan[a]	Syria	Congo
Guyana	Maldives[b]	Kyrgyz Republic[a]	Tunisia[b]	Comoros
Honduras	Myanmar	Lithuania[a]		Cabo Verde[b]
Jamaica[b]	Mongolia[b]	Latvia[a]		Ethiopia
Mexico	Malaysia[b]	Moldova[a]		Gabon
Nicaragua	Nepal	North Macedonia[a,b]		Ghana
Panama[b]	Pakistan	Montenegro[a,b]		Guinea
Peru[b]	Philippines	Poland[a]		Gambia
Paraguay	Papua New Guinea	Romania[a]		Kenya
El Salvador	Solomon Islands	Russia[a]		Lesotho
Suriname	Thailand[b]	Slovak Republic[a]		Madagascar
Uruguay[b]	Timor	Tajikistan[a]		Mali
	Tonga	Turkey[b]		Mozambique
	Vanuatu[b]	Ukraine[a]		Mauritania
				Mauritius[b]
				Namibia[b]
				Niger
				Nigeria
				Rwanda
				Sudan
				Senegal
				Sierra Leone
				Eswatini[b]
				Seychelles[b]
				Togo
				Tanzania
				Uganda
				South Africa
				Zambia
				Zimbabwe

[a] Transition economies
[b] Tax havens

Table 2.6: Robustness checks: system GMM

Dependent variable: ln(FDI/GDP)	(1) 4 lags	(2) 3 lags	(3) Endogenous
ln(FDI/GDP)$_{-1}$	0.378***	0.395***	0.509***
	(0.099)	(0.098)	(0.088)
Control of corruption	0.801	0.902	0.616
	(0.944)	(1.343)	(1.036)
ln(Resources)	0.159	0.083	0.023
	(0.131)	(0.143)	(0.153)
Control of corruption×ln(Resources)	1.030**	1.204*	1.439**
	(0.481)	(0.666)	(0.554)
ln(GDP p.c.)	−0.174	−0.157	−0.067
	(0.161)	(0.156)	(0.149)
ln(GDP p.c.)×ln(GDP p.c.)	0.094	0.076	0.030
	(0.135)	(0.139)	(0.096)
ln(Trade openness)	0.785*	0.547	0.053
	(0.431)	(0.569)	(0.475)
ln(|ΔInflation|)	−0.115	−0.120	−0.086
	(0.086)	(0.093)	(0.214)
ln(Fixed capital)	0.663**	0.813***	0.453
	(0.298)	(0.303)	(0.382)
Constant	Yes	Yes	Yes
Time period FE	Yes	Yes	Yes
Number of observations	520	520	520
Number of instruments	46	37	32
Number of countries	117	117	117
P-value of Hansen J statistic	0.167	0.097	0.386
P-value of AR(1) statistic	0.000	0.000	0.000
P-value of AR(2) statistic	0.141	0.161	0.124

The dependent variable is FDI as a percentage of GDP. Standard errors are two-step Windmeijer corrected. In columns 1 and 2, control of corruption and natural resources are treated as endogenous while all other regressors are treated as predetermined. In column 3, all regressors are treated as endogenous. In column 1, lags 2 to 4 of the dependent variable and the endogenous variables and lags 1 to 4 of the predetermined variables are used to construct the instrument sets in the first difference equation. In column 2, lags 2 and 3 of the dependent variable and the endogenous variables and lags 1 to 3 of the predetermined variables are used to construct the instrument set in the first difference equation. In column 3, lags 2 and 3 of all variables are used to construct the instrument set in the differences equation, and lagged first differences of all variables are used to construct the instrument set in the levels equation. All variables other than Control of corruption are constructed in logarithmic form in accordance with the transformation given in Equation (2.2). All variables which appear in interaction terms are centered around their respective means so that the estimates on the non-interacted variables are the estimates at the respective means. *, **, and *** denote significance at 10-, 5-, and 1 per cent level, respectively.

Table 2.7: Robustness checks: system GMM (Marginal effects)

Percentile	(1) 4 lags	(2) 3 lags	(3) Endogenous
10th	−1.480	−1.763	−2.571
	(1.549)	(2.450)	(1.942)
25th	−0.241	−0.315	−0.840
	(1.140)	(1.784)	(1.390)
Mean	0.801	0.902	0.616
	(0.944)	(1.343)	(1.036)
50th	0.916	1.036	0.776
	(0.935)	(1.307)	(1.008)
75th	2.083**	2.401**	2.401**
	(1.019)	(1.164)	(0.936)
90th	2.746**	3.176**	3.334***
	(1.182)	(1.289)	(1.072)

The dependent variable is FDI as a percentage of GDP. Reported are the estimates on control of corruption at different values of the natural resource variable. The estimates of the marginal effect in column 1 correspond to the regression specified in column 1 of Table 2.6. The estimates of the marginal effect in column 2 correspond to the regression specified in column 2 of Table 2.6. The estimates of the marginal effect in column 3 correspond to the regression specified in column 3 of Table 2.6. Standard errors are two-step Windmeijer corrected. *, **, and *** denote significance at 10-, 5-, and 1 per cent level, respectively.

Table 2.8: Robustness checks: controls

Dependent variable: ln(FDI/GDP)	(1) Interactions	(2) Fuel	(3) Ores	(4) V-dem
$\ln(\text{FDI/GDP})_{-1}$	0.443***	0.423***	0.251***	0.355***
	(0.095)	(0.093)	(0.095)	(0.099)
Control of corruption	0.655	0.708	0.366	
	(0.623)	(0.792)	(1.444)	
Political corruption				−0.011
				(0.491)
ln(Resources)	−1.537	−0.071	0.524***	0.096
	(1.175)	(0.108)	(0.161)	(0.164)
Control of corruption × ln(Resources)	0.587	0.651	0.985	
	(0.459)	(0.559)	(0.598)	
Political corruption×ln(Resources)				−0.830
				(0.589)
ln(GDP p.c.)	−0.154	−0.141	0.039	−0.122
	(0.137)	(0.176)	(0.202)	(0.128)
ln(GDP p.c.)×ln(GDP p.c.)	0.027	0.066	−0.010	0.125
	(0.114)	(0.157)	(0.169)	(0.148)
ln(GDP p.c.)×ln(Resources)	−0.124			
	(0.114)			
ln(Trade openness)	0.374	0.722*	0.497	0.775*
	(0.329)	(0.382)	(0.468)	(0.409)
ln(Trade openness)×ln(Resources)	0.190			
	(0.154)			
ln(|ΔInflation|)	−0.173*	−0.218**	-0.030	-0.120
	(0.088)	(0.087)	(0.094)	(0.103)
ln(|Δ Inflation|)×ln(Resources)	0.127*			
	(0.076)			
ln(Fixed capital)	0.510	0.728***	0.727**	0.735***
	(0.311)	(0.255)	(0.340)	(0.262)
ln(Fixed capital)×ln(Resources)	0.114			
	(0.233)			
Constant	Yes	Yes	Yes	Yes
Time period FE	Yes	Yes	Yes	Yes
Number of observations	520	521	526	509
Number of instruments	75	55	55	55
Number of countries	117	117	117	114
P-value of Hansen J statistic	0.680	0.099	0.245	0.292
P-value of AR(1) statistic	0.000	0.000	0.000	0.001
P-value of AR(2) statistic	0.140	0.173	0.116	0.101

The dependent variable is FDI as a percentage of GDP. Standard errors are two-step Windmeijer corrected. In column 2, fuel exports as a percentage of exports are used as the measure of natural resource endowment. In column 3, ores and metals exports as a percentage of total exports are used as the measure of natural resource endowment. In column 4, corruption is measured by the Political corruption index from the V-Dem dataset. The corruption and natural resources measures are treated as endogenous while all other regressors are treated as predetermined. All variables other than the corruption measures are constructed in logarithmic form in accordance with the transformation given in Equation (2.2). All variables which appear in interaction terms are centered around their respective means so that the estimates on the non-interacted variables are the estimates at the respective means. *, **, and *** denote significance at 10-, 5-, and 1 per cent level, respectively.

Table 2.9: Robustness checks: controls (Marginal effects)

Percentile	(1) Interactions	(2) Fuel	(3) Ores	(4) V-dem
10th	−0.644	−0.622	−1.366	1.827
	(1.427)	(1.483)	(1.743)	(1.615)
25th	0.061	−0.331	−0.871	0.829
	(0.941)	(1.277)	(1.594)	(0.950)
Mean	0.655	0.598	0.189	−0.011
	(0.623)	(0.811)	(1.443)	(0.491)
50th	0.720	0.708	0.366	−0.103
	(0.600)	(0.792)	(1.444)	(0.461)
75th	1.385**	1.545	1.380	−1.044*
	(0.607)	(0.989)	(1.598)	(0.634)
90th	1.763**	2.397	2.579	−1.579*
	(0.784)	(1.549)	(2.020)	(0.953)

The dependent variable is FDI as a percentage of GDP. Reported are the estimates on control of corruption at different values of the natural resource variable. The estimates of the marginal effect in column 1 correspond to the regression specified in column 1 of Table 2.8. The estimates of the marginal effect in column 2 correspond to the regression specified in column 2 of Table 2.8. The estimates of the marginal effect in column 3 correspond to the regression specified in column 3 of Table 2.8. Standard errors are two-step Windmeijer corrected. *, **, and *** denote significance at 10-, 5-, and 1 per cent level, respectively.

Table 2.10: Different infrastructure variables

Dependent variable: ln(FDI/GDP)	(1) Phones	(2) Broadband	(3) Phones & Broadband	(4) Fixed capital & Phones	(5) Fixed capital & Broadband	(6) Fixed capital, Phones & Broadband
$\ln(\text{FDI/GDP})_{-1}$	0.464***	0.452***	0.433***	0.379***	0.350***	0.332***
	(0.090)	(0.092)	(0.082)	(0.092)	(0.068)	(0.074)
Control of corruption	2.010*	0.803	1.707*	1.241	0.050	0.830
	(1.107)	(0.840)	(0.909)	(1.090)	(0.737)	(0.798)
ln(Resources)	0.186	0.059	0.059	0.213	0.041	0.096
	(0.140)	(0.144)	(0.136)	(0.130)	(0.116)	(0.134)
Control of corruption×ln(Resources)	0.364	1.354**	0.473	0.482	1.246*	0.537
	(0.520)	(0.654)	(0.521)	(0.504)	(0.692)	(0.568)
ln(GDP p.c.)	−0.765***	0.021	−0.614***	−0.562**	0.107	−0.346
	(0.258)	(0.216)	(0.211)	(0.262)	(0.170)	(0.244)
ln(GDP p.c.)×ln(GDP p.c.)	0.128	0.031	0.117	0.133	−0.002	0.051
	(0.137)	(0.111)	(0.126)	(0.135)	(0.109)	(0.105)
ln(Trade openness)	0.931**	0.644	0.916**	0.906**	0.761**	0.845**
	(0.458)	(0.444)	(0.409)	(0.413)	(0.366)	(0.391)
ln(|Δ Inflation|)	−0.111	−0.186**	−0.114	−0.085	−0.188**	−0.142*
	(0.088)	(0.079)	(0.086)	(0.086)	(0.079)	(0.081)
ln(Telephone subscriptions)	0.320**		0.392***	0.230*		0.222
	(0.139)		(0.134)	(0.127)		(0.155)
ln(Broadband subscriptions)		−0.106	−0.137*		−0.135	−0.109
		(0.111)	(0.080)		(0.093)	(0.073)
ln(Fixed capital)				0.415	0.832***	0.785***
				(0.293)	(0.246)	(0.267)
Constant	Yes	Yes	Yes	Yes	Yes	Yes
Time period FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	526	473	473	520	468	468
Number of instruments	55	55	61	61	61	67
Number of countries	117	115	115	117	115	115
P-value of Hansen J statistic	0.405	0.512	0.676	0.446	0.674	0.573
P-value of AR(1) statistic	0.000	0.000	0.000	0.000	0.000	0.000
P-value of AR(2) statistic	0.119	0.289	0.401	0.157	0.501	0.807

The dependent variable is FDI as a percentage of GDP. Standard errors are two-step Windmeijer corrected. In column 1, fixed telephone subscriptions per 100 people are used as a measure of infrastructure development. In column 2, fixed broadband subscriptions per 100 people are used as a measure of infrastructure development. In column 3, both, fixed telephone subscriptions per 100 people and fixed broadband subscriptions per 100 people are controlled for. In columns 4 to 6, fixed capital accumulation as a percentage of GDP is additionally controlled for. Control of corruption and natural resources are treated as endogenous while all other regressors are treated as predetermined. All variables other than control of corruption are constructed in logarithmic form in accordance with the transformation given in Equation (2.2). All variables which appear in interaction terms are centered around their respective means so that the estimates on the non-interacted variables are the estimates at the respective means. *, **, and *** denote significance at 10-, 5-, and 1 per cent level, respectively.

Table 2.11: Robustness checks: Different infrastructure variables (Marginal effects)

Percentile	(1) Phones	(2) Broadband	(3) Phones & Broadband	(4) Fixed capital & Phones	(5) Fixed capital & Broadband	(6) Fixed capital, Phones & Broadband
10th	1.203	−2.193	0.660	0.173	−2.708	−0.359
	(1.822)	(1.920)	(1.740)	(1.727)	(2.072)	(1.889)
25th	1.641	−0.566	1.228	0.753	−1.210	0.287
	(1.361)	(1.244)	(1.226)	(1.304)	(1.294)	(1.258)
Mean	2.010*	0.803	1.707*	1.241	0.050	0.830
	(1.107)	(0.840)	(0.909)	(1.090)	(0.737)	(0.798)
50th	2.050*	0.954	1.759**	1.295	0.188	0.890
	(1.091)	(0.817)	(0.886)	(1.079)	(0.692)	(0.757)
75th	2.463**	2.488***	2.295***	1.842	1.600**	1.499**
	(1.101)	(0.933)	(0.869)	(1.129)	(0.706)	(0.616)
90th	2.698**	3.360***	2.599**	2.152*	2.402**	1.844**
	(1.239)	(1.214)	(1.024)	(1.276)	(1.029)	(0.800)

The dependent variable is FDI as a percentage of GDP. Reported are the estimates on control of corruption at different values of the natural resource variable. The estimates of the marginal effect in columns 1 through 6 correspond to the regressions specified in the respective columns of Table 2.10. Standard errors are two-step Windmeijer corrected. *, **, and *** denote significance at 10-, 5-, and 1 per cent level, respectively.

Table 2.12: Different inflation variables

Dependent variable: ln(FDI/GDP)	(1) Absolute inflation	(2) Absolute squared	(3) Difference squared
$\ln(\text{FDI/GDP})_{-1}$	0.320***	0.383***	0.375***
	(0.096)	(0.090)	(0.081)
Control of corruption	1.675	2.149*	1.191
	(1.082)	(1.147)	(0.925)
ln(Resources)	0.247*	0.039	0.096
	(0.132)	(0.094)	(0.105)
Control of corruption×ln(Resources)	0.605	0.691	0.748
	(0.418)	(0.543)	(0.470)
ln(GDP p.c.)	−0.207	−0.221	−0.280
	(0.166)	(0.182)	(0.173)
ln(GDP p.c.)×ln(GDP p.c.)	0.074	0.081	0.076
	(0.116)	(0.122)	(0.140)
ln(Trade openness)	0.535*	0.354	0.765*
	(0.316)	(0.336)	(0.397)
ln(Inflation)	0.181	0.012	−0.500**
	(0.116)	(0.342)	(0.224)
ln(Inflation)×ln(Inflation)		0.005	0.088
		(0.066)	(0.053)
ln(Fixed capital)	0.809***	0.907***	0.719**
	(0.280)	(0.339)	(0.278)
Constant	Yes	Yes	Yes
Time Period FE	Yes	Yes	Yes
Number of observations	520	520	520
Number of instruments	55	61	61
Number of countries	117	117	117
P-value of Hansen J statistic	0.438	0.221	0.313
P-value of AR(1) statistic	0.000	0.000	0.000
P-value of AR(2) statistic	0.306	0.267	0.152

The dependent variable is FDI as a percentage of GDP. Standard errors are two-step Windmeijer corrected. Control of corruption and natural resources are treated as endogenous while all other regressors are treated as predetermined. All variables other than control of corruption are constructed in logarithmic form in accordance with the transformation given in Equation (2.2). All variables which appear in interaction terms are centered around their respective means so that the estimates on the non-interacted variables are the estimates at the respective means. *, **, and *** denote significance at 10-, 5-, and 1 per cent level, respectively.

Table 2.13: Marginal effects – Different inflation variables

Percentile	(1) Absolute inflation	(2) Absolute squared	(3) Difference squared
10th	0.336	0.619	−0.466
	(1.479)	(1.723)	(1.485)
25th	1.063	1.450	0.434
	(1.192)	(1.309)	(1.097)
Mean	1.675	2.149*	1.191
	(1.082)	(1.147)	(0.925)
50th	1.742	2.226*	1.274
	(1.079)	(1.144)	(0.920)
75th	2.427**	3.010**	2.122**
	(1.162)	(1.287)	(1.024)
90th	2.817**	3.455**	2.604**
	(1.288)	(1.480)	(1.190)

The dependent variable is FDI as a percentage of GDP. Reported are the estimates on control of corruption at different values of the natural resource variable. The estimates of the marginal effect in columns 1 to 3 correspond to the regressions specified in the respective columns of Table 2.12. Standard errors are two-step Windmeijer corrected. *, **, and *** denote significance at 10-, 5-, and 1 per cent level, respectively.

Table 2.14: Controlling for Democracy

Dependent variable: ln(FDI/GDP)	(1) Freedom	(2) Polity	(3) V-Dem	(4) Freedom	(5) Polity	(6) V-Dem
ln(FDI/GDP)$_{-1}$	0.418***	0.350***	0.351***	0.383***	0.339***	0.339***
	(0.083)	(0.104)	(0.085)	(0.087)	(0.100)	(0.080)
Control of corruption	1.529	1.985*	1.706*	2.014**	2.450*	1.849*
	(1.090)	(1.148)	(1.026)	(0.985)	(1.277)	(1.083)
ln(Resources)	0.085	0.088	0.175	0.099	0.102	0.211
	(0.108)	(0.159)	(0.143)	(0.110)	(0.167)	(0.138)
Control of corruption×ln(Resources)	0.816	0.229	0.729	−0.008	−0.082	0.242
	(0.514)	(0.378)	(0.493)	(0.634)	(0.461)	(0.684)
Democracy	0.097	−0.290	-1.004	0.165**	−0.641	−1.186*
	(0.091)	(0.577)	(0.742)	(0.073)	(0.567)	(0.637)
Democracy × ln(Resources)				−0.094**	0.313	0.514
				(0.040)	(0.467)	(0.359)
ln(GDP p.c.)	−0.243	−0.317*	−0.204	−0.179	−0.343*	−0.235
	(0.173)	(0.178)	(0.170)	(0.174)	(0.188)	(0.160)
ln(GDP p.c.)×ln(GDP p.c.)	0.089	0.056	0.093	0.093	0.030	0.099
	(0.131)	(0.139)	(0.141)	(0.131)	(0.137)	(0.113)
ln(Trade openness)	0.922**	0.667*	0.805**	0.817**	0.767*	0.895**
	(0.377)	(0.371)	(0.367)	(0.412)	(0.392)	(0.355)
ln(|ΔInflation|)	−0.118	−0.118	−0.113	−0.138*	−0.092	−0.119
	(0.085)	(0.092)	(0.088)	(0.081)	(0.095)	(0.086)
ln(Fixed capital)	0.553**	0.687***	0.530*	0.623**	0.706***	0.554**
	(0.246)	(0.249)	(0.273)	(0.249)	(0.267)	(0.276)
Constant	Yes	Yes	Yes	Yes	Yes	Yes
Time Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	515	488	509	515	488	509
Number of instruments	60	60	60	65	65	65
Number of countries	116	109	114	116	109	114
P-value of Hansen J statistic	0.397	0.353	0.338	0.612	0.396	0.462
P-value of AR(1) statistic	0.000	0.001	0.000	0.000	0.001	0.000
P-value of AR(2) statistic	0.178	0.098	0.086	0.205	0.095	0.081

The dependent variable is FDI as a percentage of GDP. The standard errors are two-step Windmeijer corrected. Democracy is measured by the Political rights index by Freedom House in columns 1 and 4, by the Polity index in columns 2 and 5, and by the electoral democracy index from the V-Dem dataset in columns 3 and 6. Control of corruption, the respective democracy indices, and resources are treated as endogenous while all other regressors are treated as predetermined. All variables other than Control of corruption and the democracy indices are constructed in logarithmic form in accordance with the transformation given in Equation (2.2). All variables which appear in interaction terms are centered around their respective means so that the estimates on the non-interacted variables are the estimates at the respective means. *, **, and *** denote significance at 10-, 5-, and 1 per cent level, respectively.

Table 2.15: Controlling for democracy (Marginal effects)

Percentile	(1) Freedom	(2) Polity	(3) V-Dem	(4) Freedom	(5) Polity	(6) V-Dem
10th	−0.277	1.477	0.092	2.032	2.631	1.314
	(1.859)	(1.564)	(1.720)	(2.056)	(1.928)	(2.212)
25th	0.704	1.753	0.969	2.022	2.533*	1.604
	(1.380)	(1.288)	(1.276)	(1.404)	(1.525)	(1.517)
Mean	1.529	1.985*	1.706*	2.014**	2.450*	1.849*
	(1.090)	(1.148)	(1.026)	(0.985)	(1.277)	(1.083)
50th	1.620	2.011*	1.787*	2.013**	2.441*	1.876*
	(1.069)	(1.139)	(1.010)	(0.954)	(1.257)	(1.053)
75th	2.545**	2.271**	2.613***	2.003**	2.348**	2.150**
	(1.024)	(1.139)	(1.014)	(0.930)	(1.172)	(1.049)
90th	3.071***	2.419**	3.083***	1.998*	2.295*	2.306*
	(1.139)	(1.209)	(1.146)	(1.140)	(1.224)	(1.277)

The dependent variable is FDI as a percentage of GDP. Reported are the estimates on control of corruption at different values of the natural resource variable. The estimates of the marginal effect in columns 1 to 6 correspond to the regressions specified in the respective columns of Table 2.14. Standard errors are two-step Windmeijer corrected. *, **, and *** denote significance at 10-, 5-, and 1 per cent level, respectively.

Table 2.16: Controlling for polcon

Dependent variable: ln(FDI/GDP)	(1) Polcon	(2) Polcon
$\ln(\text{FDI/GDP})_{-1}$	0.362***	0.368***
	(0.102)	(0.102)
Control of corruption	1.483*	1.378*
	(0.810)	(0.772)
ln(Resources)	0.099	0.097
	(0.148)	(0.146)
Control of corruption×ln(Resources)	0.781*	0.826*
	(0.414)	(0.436)
Polcon	0.223	0.167
	(0.503)	(0.450)
Polcon×ln(Resources)		0.027
		(0.325)
ln(GDP p.c.)	−0.308*	−0.302**
	(0.160)	(0.152)
ln(GDP p.c.)×ln(GDP p.c.)	0.080	0.097
	(0.134)	(0.131)
ln(Trade openness)	0.844**	0.925**
	(0.405)	(0.391)
$\ln(\lvert\Delta\text{Inflation}\rvert)$	−0.137	−0.137*
	(0.086)	(0.079)
ln(Fixed capital)	0.535*	0.458
	(0.309)	(0.296)
Constant	Yes	Yes
Time Period FE	Yes	Yes
Number of observations	510	510
Number of instruments	59	64
Number of countries	114	114
P-value of Hansen J statistic	0.426	0.530
P-value of AR(1) statistic	0.000	0.000
P-value of AR(2) statistic	0.125	0.113

The dependent variable is FDI as a percentage of GDP. The standard errors are two-step Windmeijer corrected. The institutional variables (Control of corruption and the political constraints index) and resources are treated as endogenous while all other regressors are treated as predetermined. All variables other than the institutional variables are constructed in logarithmic form in accordance with the transformation given in Equation (2.2). All variables which appear in interaction terms are centered around their respective means so that the estimates on the non-interacted variables are the estimates at the respective means. *, **, and *** denote significance at 10-, 5-, and 1 per cent level, respectively.

Table 2.17: Marginal effects – Controlling for polcon

Percentile	(1) Polcon	(2) Polcon
10th	−0.247	−0.450
	(1.190)	(1.267)
25th	0.693	0.543
	(0.891)	(0.909)
Mean	1.483*	1.378*
	(0.810)	(0.772)
50th	1.570*	1.470*
	(0.814)	(0.771)
75th	2.456**	2.405***
	(0.983)	(0.919)
90th	2.959**	2.937***
	(1.156)	(1.097)

The dependent variable is FDI as a percentage of GDP. Reported are the estimates on control of corruption at different values of the natural resource variable. The estimates of the marginal effect in columns 1 and 2 correspond to the regressions specified in the respective columns of Table 2.16. Standard errors are two-step Windmeijer corrected. *, **, and *** denote significance at 10-, 5-, and 1 per cent level, respectively.

Table 2.18: Reduced sample

Dependent variable: ln(FDI/GDP)	(1) No Transition	(2) No Tax havens
$\ln(\mathrm{FDI/GDP})_{-1}$	0.284***	0.356***
	(0.099)	(0.098)
Control of corruption	1.527	1.655
	(1.140)	(1.091)
ln(Resources)	0.237	0.159
	(0.183)	(0.130)
Control of corruption×ln(Resources)	1.346*	0.294
	(0.695)	(0.540)
ln(GDP p.c.)	−0.237	−0.359*
	(0.209)	(0.204)
ln(GDP p.c.)×ln(GDP p.c.)	0.240	0.055
	(0.171)	(0.129)
ln(Trade openness)	0.977*	0.743*
	(0.499)	(0.381)
ln(|ΔInflation|)	−0.198*	−0.149
	(0.113)	(0.109)
ln(Fixed capital)	0.265	0.541
	(0.399)	(0.370)
Constant	Yes	Yes
Time period FE	Yes	Yes
Number of observations	397	401
Number of instruments	55	55
Number of countries	91	91
P-value of Hansen J statistic	0.423	0.357
P-value of AR(1) statistic	0.001	0.000
P-value of AR(2) statistic	0.155	0.144

The dependent variable is FDI as a percentage of GDP. In column 1, all transition economies are excluded from the sample. In column 2, all tax havens are excluded from the sample. The standard errors are two-step Windmeijer corrected. Control of corruption and resources are treated as endogenous, while all other regressors are treated as predetermined. All variables other than Control of corruption are constructed in logarithmic form in accordance with the transformation given in Equation (2.2). All variables which appear in interaction terms are centered around their respective means so that the estimates on the non-interacted variables are the estimates at the respective means. *, **, and *** denote significance at 10-, 5-, and 1 per cent level, respectively.

Table 2.19: Marginal effects – Reduced sample

Percentile	(1) No Transition	(2) No Tax havens
10th	−1.628	0.994
	(2.298)	(1.960)
25th	−0.183	1.352
	(1.673)	(1.429)
Mean	1.527	1.655
	(1.140)	(1.091)
50th	1.691	1.688
	(1.113)	(1.066)
75th	3.381***	2.016**
	(1.198)	(0.991)
90th	4.224***	2.190**
	(1.450)	(1.092)

The dependent variable is FDI as a percentage of GDP. Reported are the estimates on control of corruption at different values of the natural resource variable. The estimates of the marginal effect in columns 1 and 2 correspond to the regressions specified in the respective columns of Table 2.18. Standard errors are two-step Windmeijer corrected. *, **, and *** denote significance at 10-, 5-, and 1 per cent level, respectively.

Table 2.20: Robustness check – Sample split

Dependent variable: ln(FDI/GDP)	(1) Low resource endowment	(2) High resource endowment
$\ln(\text{FDI/GDP})_{-1}$	0.339***	0.517***
	(0.089)	(0.140)
Control of corruption	−1.478	2.114**
	(1.273)	(1.004)
ln(Resources)	0.018	0.218
	(0.144)	(0.334)
ln(GDP p.c.)	0.240	−0.335
	(0.204)	(0.229)
ln(GDP p.c.)×ln(GDP p.c.)	−0.108	0.425**
	(0.142)	(0.206)
Trade openness	0.397	0.537
	(0.404)	(0.566)
$\lvert\Delta\text{Inflation}\rvert$	−0.222**	−0.165
	(0.098)	(0.132)
Fixed capital	0.477	0.538
	(0.365)	(0.465)
Constant	Yes	Yes
Time period FE	Yes	Yes
Number of observations	264	256
Number of instruments	50	50
Number of countries	58	59
P-value of Hansen J statistic	0.458	0.236
P-value of AR(1) statistic	0.002	0.003
P-value of AR(2) statistic	0.327	0.381

The dependent variable is FDI as a percentage of GDP. In column 1, only countries with natural resource endowment below the sample median are included in the sample. In column 2, only countries with natural resource endowment above the sample median are included in the sample. The standard errors are two-step Windmeijer corrected. Control of corruption and resources are treated as endogenous, while all other regressors are treated as predetermined. All variables other than Control of corruption are constructed in logarithmic form in accordance with the transformation given in Equation (2.2). All variables which appear in interaction terms are centered around their respective means so that the estimates on the non-interacted variables are the estimates at the respective means. *, **, and *** denote significance at 10-, 5-, and 1 per cent level, respectively.

Chapter 3

Dream of Californication – Trade, Human Rights and the California Effect[8]

Abstract

For decades, a narrative dominated the public discourse in many Western countries. This narrative implied that trade with countries with a lower level of institutional quality could induce democratic change and good governance. However, recent developments in countries such as Russia or China have raised doubts about this hypothesis. One potential explanation for a change in the institutions of the exporting countries is the so-called "California effect". It suggests institutional spillovers through trade from importing to exporting countries. In this article, we intend to examine this hypothesis empirically. In line with the California effect, we focus on the 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 a panel and four 5-year cross-sections, we estimate a spatial autoregressive model by applying the generalized spatial two-stage least squares estimator. We use trade flows to construct the relevant spatial 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.

Keywords: *Spatial analysis, spillover, diffusion, globalization, California effect*

[8]This chapter is joint work with Tim Hailer-Röthel with each author contributing equally to the paper. A version of this chapter exists as a working paper (Bothner and Röthel, 2024).

3.1 Introduction

Recent geopolitical events have led to a public questioning of long-held narratives. A famous narrative, originally from Germany, is often referred to as *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, it 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 that institutional spillovers via trade do not exist.

Deriving this conclusion from a single experience, namely the Western European experience with Russia, could be considered too simplistic and also 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 that may induce such a change. Countries or firms are often not pressured by other countries' governments to behave in a certain way but rather by non-governmental actors such as consumers, firms, or non-governmental organizations. For example, 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.[9] This may put pressure on these countries to improve their human rights standards to keep their access to foreign markets. Vogel (1997) referred to a similar idea as the "California effect". It explains why exporting countries align their institutions with those of importing countries to gain or keep market access.

Several obstacles could distort the California effect. A high bargaining power of exporting

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

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.

In this article, we empirically examine human rights spillovers through trade. We answer the following 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 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, regressing a country's human rights on an export-weighted average of its trading partners' 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. We tackle the issue of an endogenous weighting matrix by applying the control function approach by Qu et al. (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 et al., 2013), there are various characteristics that make our approach unique. To the best of our knowledge, this is the first study that specifically examines spillovers, which are expected under the hypothesis of the California effect. In previous studies, two additional problems arose. Often, spatial studies only include a small fraction of countries and account for only one source of spatial dependence. In this article, we include up to 173 countries, and we simultaneously control for several dimensions of spatial dependence, thus limiting the endogeneity problem.

Our results provide evidence for the existence of human rights spillovers through trade. Moreover, we find significant and positive estimates in line with the California effect. 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.

The remainder of this paper is structured as follows: Section 3.2 provides an overview of the literature on 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 3.3, we introduce the empirical specification and briefly describe the data. We also discuss the origins of and solutions to potential issues of endogeneity. Section 3.4 presents the results with respect to the hypotheses formulated, and section 3.5 extends our approach to resource exports and different groupings of countries. Section 3.6 summarizes the results and concludes.

3.2 Literature Review

We start our literature review by looking at the relationship between institutions and trade in general. Several 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 et al., 2012). A second variable that may provide a channel for an indirect effect of trade on institutional development is income inequality. According to Acemoglu and Robinson (2000), democratization can be a commitment device for elites to provide 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, 2005). Liu and Ornelas (2014) further argue that increased international trade through participation in free trade agreements reduces protectionist rents. It thus reduces the incentive of autocratic groups seeking power to get hold of these rents. Hence, they argue that free trade agreements can have a stabilizing effect on democracy.

Another part of the literature focuses on the effect in the opposite direction, examining if trade is affected by democracy. Again, there is 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 et al., 2021).

The studies mentioned above examine the effect of trade on institutional development with a focus on democratization. They do not examine spillovers of other institutions nor any heterogeneity in these spillovers arising from different trading partners. In this study, we examine trade-induced international spillovers of institutions other than democracy. Specifically, we look at human rights standards.

Cao et al. (2013) provide an intuitive explanation for the international diffusion of human rights standards through trade. They base their argument on insights by Vogel (1997) who analysed the geographic spread of regulation standards. Vogel (1997) examines the diffusion of environmental standards within the United States (US). He describes two opposing scenarios of how trade and regulation could affect each other. First, regulation could lead to costly and complicated processes, which make 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, they 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 (Vogel, 1997). Second, countries could 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 producers favor similar regulations across countries since this lowers their production cost. The European Union (EU), for example, adopted German car emission regulations in the 1980s. They did not only choose the German regulations because Germany is the largest car exporter in Europe, but also since the German regulations were similar to those 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 regulations, is consistent with 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 the same standards as in California. Vogel (1997) identifies three mechanisms which lead to the California effect: First, stricter regulation may be a competitive advantage for domestic firms in 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 their 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 provides evidence for the California 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. Drezner (2001) provides an overview of studies examining the effect of globalization on labor standards. Overall, he does not find evidence for a race to the bottom but rather positive effects of globalization on labor standards. There is also evidence for positive effects of FDI (Messerschmidt and Janz, 2023; Mosley and Uno, 2007). Additionally, trade seems to be related to a lower level of child labor (Davies and Voy, 2009; Neumayer and de Soysa, 2005). 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. Similarly, Greenhill et al. (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), women's rights (e.g. Potrafke and Ursprung, 2012; Neumayer and de Soysa, 2007), and women's wages and employment (Wood, 1991; Kucera and Milberg, 2000; Berik et al., 2004; Oostendorp, 2009).

Cao et al. (2013) 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 importing countries might pressure firms and the government in exporting countries to push for improvements in human rights. Additionally, exporting firms might be concerned about losing 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 may lobby the government in the exporting countries to increase the human rights standards. The findings of Peterson et al. (2018) support this argument. If countries abuse human rights and human rights organizations shame the abuse, 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 et al. (2013) find evidence in favor of human rights spilling over to trading partners, in line with the California effect. Similarly, Neumayer and de Soysa (2011) examine women's rights spillovers through trade. Our approach differs significantly from these two studies: While the approach of Cao et al. (2013) is appropriate to examine the existence of human rights spillovers via trade overall, it cannot answer the question of whether the spillovers 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 standards to exporting countries with low human rights standards. Our study addresses this shortcoming as will be explained in the following section. This is one of our key contributions.

Several studies use spatial econometric approaches in order to capture potential institutional spillovers between countries or regions. Bell et al. (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 the 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. Goderis and Versteeg (2014) analyze the diffusion of constitutional rights among countries. Their results indicate 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. T. H. 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. Faber and Gerritse (2012) and Kelejian et al. (2013) find positive geographic spillovers across countries whereas Claeys and Manca (2011) do not report such an effect. Naveed et al. (2023) show spatial dependence in women's rights standards for neighboring countries. Greenhill (2010) finds positive human rights spillovers between members of intergovernmental organizations.

3.3 Empirical Framework

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

$$y_i = \sum_{q=1}^{s}(\rho_q \sum W_{qij} y_j) + X_i\beta + \epsilon_i, \tag{3.1}$$

$$\epsilon_i = \sum_{r=1}^{t}(\theta_r \sum W_{rij} u_i) + \mu_i \tag{3.2}$$

The dependent variable y_i is 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:

$$y = \sum_{q=1}^{s}(\rho_q W_q y) + X\beta + \epsilon, \tag{3.3}$$

$$\epsilon = \sum_{r=1}^{t} (\theta_r W_r u) + \mu. \tag{3.4}$$

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 (3.3) and (3.4) is equivalent to the model in Equations (3.1) and (3.2).

Data

As a proxy for human rights, we use the Civil Liberties Index from the Varieties of Democracy (V-Dem) dataset (Coppedge et al., 2023b; 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 et al., 2023a). This index measures the level of human rights more precisely and addresses one of the shortcomings of previous studies that relied on ordinal measures of human rights (e.g. Cao et al., 2013).

The country-specific control variables include the natural logarithm of GDP per capita in constant US Dollars (USD) (World Bank, 2023), the natural logarithm of the population size (Gurevich and Herman, 2018), as well as the squares of these two variables to account for potential non-linear relationships between economic development or country size and human rights. Other studies suggest 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 de 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.

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, 2021) and data on nominal GDP from the WDI by the World Bank (2023). To obtain a country's total exports and imports, we aggregate all exports from a country to any other country and in any sector in the ITPD-E. The data on FDI inflows stem from the United Nations Conference on Trade and Development Statistics (UNCTADstat) data centre of the UNCTAD (2023). We also control for the Liberal Democracy index from the V-Dem dataset (Coppedge et al., 2023b; Pemstein et al., 2023) to account for differences in the countries' political systems.

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 obtained from the Pew Research Center (2022) in the cross-sectional analyses. [10] Finally, we also control for a conflict variable based on the Major Episodes of Political Violence (MEPV) dataset from the Armed Conflict and Intervention Datasets by the Center for Systemic Peace (2022). It 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.[11]

Our analysis mainly relies on cross-sectional estimations, where all variables are averaged over five-year intervals. 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 simultaneously and include multiple error lags. This addresses one potential source of endogeneity, as explained later in this section. 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

[10] Due to limited data availability, the shares of Christians and Muslims are those from 2010 in all the cross sections. Additionally, there are no data on the religious composition of North Macedonia in the data from the Pew Research Center (2022). We use the shares of Christians and Muslims in North Macedonia in 2023, as reported by the World Factbook by the CIA (2023).

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

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 Section 3.A.2 in the Appendix.

Using these imputation techniques, 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 3.5 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 (3.1) or, equivalently, the matrices W_q in equation (3.3). 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, as a share of country i's total exports, 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, 2022), including data on bilateral exports for 265 countries in 170 industries across different sectors. 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. Following 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 "powerful" 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 a 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 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:

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

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 all other 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, 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 (3.2) and (3.4). To choose the spatial weighting matrices determining the spatial dependence in the disturbances, we conduct the Moran's I test of spatial dependence in the residuals (Moran, 1950). That is, we first estimate the model expressed by Equations (3.1) and (3.3) 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. Table 3.6 in the Appendix presents the results.

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 (3.1) and (3.3) are endogenous. Hence, the OLS estimator is inconsistent (Ord, 1975; Anselin, 1988). This endogeneity issue arises because Equations (3.1) and (3.3) 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 et al. (2019). This estimator is based on the GS2SLS estimator proposed by Kelejian and Prucha (1998, 1999, 2004, 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) and rewrite Equation (3.3) in reduced form as

$$y = (I - \sum_{q=1}^{s} \rho_q W_q)^{-1}(X\beta + \epsilon), \tag{3.5}$$

with I being the $N \times N$ identity matrix. Equation (3.5) implies a mean of y of

$$E[y] = (I - \sum_{q=1}^{s} \rho_q W_q)^{-1}(X\beta) \tag{3.6}$$

and hence

$$E[\sum_{q=1}^{s} W_q y] = \sum_{q=1}^{s} W_q (I - \sum_{q=1}^{s} \rho_q W_q)^{-1}(X\beta). \tag{3.7}$$

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

$$(I - \sum_{q=1}^{s} \rho_q W_q)^{-1} = I + \sum_{i=1}^{\infty}(\sum_{q=1}^{s} \rho_q W_q)^i \tag{3.8}$$

to rewrite Equation (3.7) as

$$E[\sum_{q=1}^{s} W_q y] = \sum_{q=1}^{s} W_q [I + \sum_{i=1}^{\infty}(\sum_{q=1}^{s} \rho_q W_q)^i](X\beta). \tag{3.9}$$

Equation (3.9) 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 in which the weight W_{ij} is assigned the value 1 if the countries i and j are contiguous and 0 otherwise.

The second spatial lag we control for is constructed using a spatial weighting matrix 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, 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 a 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 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; Blanton and 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 et al. (2021) instead. To implement this approach, for each cross-section, we first estimate the following equation:

$$LogExports_{ij} = \alpha LogDistance_{ij} + \gamma Contiguity_{ij} + \theta Language_{ij} + \delta_{1i} + \delta_{2j} + \xi_{ij}, \quad (3.10)$$

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 et al. (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 (3.1) while controlling for the estimates on the multilateral resistance terms $\widehat{\eta_i}$:

$$y_i = \sum_{q=1}^{s} \rho_q \sum W_{qij} y_j + X_i \beta + \widehat{\eta_i} \lambda + \widehat{\epsilon_i}, \tag{3.11}$$

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 $\widehat{\eta_i}$.

3.4 Results

We start with the panel estimation in Table 3.1. Columns 1 to 3 show the OLS, fixed effects (FE), and GS2SLS results. Since we include country- and time-fixed effects in the latter two specifications, we cannot control for the time-invariant shares of the Muslim and Christian populations. Similar to the following specifications, the panel covers four 5-year periods over which all variables are averaged. The export-weighted spatial lag is positive and significant, at least at the 5%-level in the three specifications. The magnitude of the effect decreases from OLS to FE and GS2SLS. 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 in column 1 and insignificant in columns 2 and 3. The negative effect of GDP 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. The other control variables are mostly insignificant throughout the specifications. Overall, our first estimation results already indicate that exports may be an important determinant for the spillover of human rights between countries.

Table 3.2 presents the cross-section results using a spatial lag of human rights weighted by exports. The coefficients of the control variables are mostly similar to the ones presented above 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 3.1, we are now able to examine the heterogeneity of the spatial lag over time. 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.

Table 3.1: Baseline Estimation

	(1) OLS	(2) FE	(3) GS2SLS
Liberal Democracy	0.6430***	0.8038***	0.8921***
	(0.0616)	(0.0709)	(0.0433)
Log GDP real	-0.0272***	0.0219	0.0050
	(0.0064)	(0.0227)	(0.0161)
Log GDP real squared	-0.0072***	-0.0008	-0.0042
	(0.0023)	(0.0071)	(0.0045)
Log population	-0.0066**	0.0067	-0.0559*
	(0.0031)	(0.0364)	(0.0326)
Log population squared	-0.0008	0.0040	0.0115*
	(0.0013)	(0.0076)	(0.0060)
FDI flows in % of GDP	0.0003	0.0001	0.0001
	(0.0002)	(0.0001)	(0.0001)
Trade Openness in % of GDP	0.0001	-0.0001**	-0.0001
	(0.0001)	(0.0001)	(0.0001)
Conflict	-0.0032	-0.0237*	-0.0157
	(0.0190)	(0.0133)	(0.0104)
Share of Muslim Population	-0.0002		
	(0.0003)		
Share of Christian Population	0.0001		
	(0.0003)		
Constant	0.0668	0.2820***	0.0000
	(0.0604)	(0.0521)	(0.0000)
Spatial Lag weighted by			
Total Exports	0.4987***	0.1228**	0.0755**
	(0.0804)	(0.0476)	(0.0323)
Country Fixed Effects	NO	YES	YES
Time Fixed Effects	NO	YES	YES
R-Squared	0.858	0.652	
Pseudo R-Squared			0.982
Number of observations	668	668	668

Notes: *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively. Heteroskedasticity-robust standard errors are in parentheses. Standard errors in columns 1 and 2 are clustered at the country level. Column 3 includes an error lag for Total Exports.

In order to investigate the existence of the California effect, we need to have a closer

Table 3.2: 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 3.6. Columns 1, 2, and 3 do not include any error lags. Column 4 includes an error lag for Total Exports.

look at the export weighting matrix. As described in the previous section, we will divide the export matrix into two different matrices, one for exports from non-OECD countries to OECD countries and one for all other exports. Table 3.3 presents the results. Again, the estimates on the control variables are somewhat similar to the ones in Table 3.2. 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.

Table 3.3: 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 3.6. Columns 1 and 2 do not include any error lags. Columns 3 and 4 include an error lag for Other Exports.

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. Our results suggest heterogeneity in the intensity of human rights spillovers through trade over time. Specifically, we find evidence for the California effect only in the two periods after 2010 and not before. A possible explanation for these spillovers gaining in importance over time is an increase in awareness of human rights standards across value chains among consumers in OECD countries. This is in line with the results of Busse (2003), who finds a change in the relationship between democracy and FDI over time. This might be explained by the increased attention multinational activity receives from the public. He attributes this to the rise of the internet and improvements in communication technology, enabling NGOs to organize better and thus increase their influence on public opinion.

Having established the main results of our article already, we will tackle the potential endogeneity issues discussed above. 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 3.7 in the Appendix presents the results for overall exports, and Table 3.8 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 3.7. The addition also leads to a reduction of the magnitude of the estimate on the spatial lag weighted by exports. However, the estimated coefficient is still positive and significant. Similarly, in Table 3.8, the estimate on the spatial lag weighted by 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.

After addressing two potential sources of endogeneity by using the GS2SLS estimator and controlling for alternative weighting matrices, we still need to deal with the potential problem of endogenous export weights. As described in the previous section, we address this issue by using the control function approach proposed by Qu et al. (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 3.9 and 3.10 in the Appendix. If the coefficients of these terms are significant, our previous results may have been biased. 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 3.12 in the Appendix. Table 3.9 presents the corresponding results for the spatial lag weighted by overall exports, including additional spatial lags as controls. 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 3.10 in the Appendix 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 main results are supported by our findings obtained using the control function approach.

However, one limitation remains. We interpret the positive estimate of the California effect to indicate positive spillover effects from OECD to non-OECD countries. However, in principle, 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.

3.5 Extensions

The third hypothesis in Section 3.3 refers to a difference between resource exports and non-resource exports. Since we assume that OECD importers 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 3.4 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.

Table 3.4: 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 3.6. 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.

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 that 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. Consequently, the high group consists of rich countries with high human rights standards only. Table 3.11 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 group countries".

3.6 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 countries. An empirical analysis can help in determining whether doubts about the existence of beneficial human rights spillovers are based only on anecdotal evidence 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 improving their standards or lobbying their governments in this regard 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 a panel and 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 weights, 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 two 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 in later periods, suggesting the existence of the California effect. Both results are robust to various robustness tests. The fact that we find human rights spillovers through trade only in later periods and not before 2010 may be explained by increased awareness of consumers regarding human rights conditions along the value chains of imported goods and services. 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 that can be drawn from this study is that Western trade relationships with countries that have lower human rights standards should not necessarily be abandoned for ethical reasons.

Instead, trade should be considered a transmission mechanism for human rights and other kinds of institutions.

3.A Appendix

3.A.1 Tables

Table 3.5: 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 % of 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 3.6: 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 3.7: 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 3.6. 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 3.8: 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 3.6. 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 3.9: 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 in Table 3.12. 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 3.10: 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 in Table 3.12. 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 3.11: 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 3.6. 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 3.12: 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.

3.A.2 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 3.13. We then obtain the estimates to obtain the predicted population size of Taiwan in the respective years.

Table 3.13: 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 (217238.5)	545.0131*** (136.74)	0.4091 (0.3315)	39.0860*** (6.8953)	6.6289 (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.

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 3.13. 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) and then using the predicted values for imputation. The results are shown in column 3 of Table 3.13.

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 3.13.

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 3.13.

Chapter 4

Rationalizing Protests: From Individuals to Networks[12]

Abstract

We consider a theoretical model of network goods to explain protest participation. The model distinguishes between two equilibria: (i) when network effects are weak, individual participation is primarily driven by standalone benefits; (ii) when network effects are strong, expectations about others' participation become crucial. Using survey data from the American National Election Studies, we estimate a spatial autoregressive model and identify both standalone and network benefits through direct and indirect effects. Our findings reveal significant spillover effects in protest participation across individuals. Specifically, we identify both types of benefits for the full United States sample as well as within more homogeneous subgroups. Across subgroups, protest dynamics are predominantly shaped by strong network effects. These estimation results, in combination with the model's implications, offer new insights into the scale of contemporary protest movements in the United States.

Keywords: *Collective Action, Public Protests, Network Effects, Social Networks*

4.1 Introduction

Public Protests have been a vital force for citizens to express their discontent in the public sphere worldwide. The act of demonstrating – whether peaceful or violent – calls for policies, reforms, or even a change of government, and drives social and political change

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not only in democracies but also in authoritarian regimes. For instance, rallies that later became known as the Yellow Vest Protests arose in France from late 2018 until early 2019, when diesel prices increased by approximately 23% over 12 months, as the BBC (2018) reported.[13] With the ensuing riots drawing the participation of 282,000 protesters at its peak, the Yellow Vest Protests ultimately forced the French government to concede by announcing a halt on planned fuel tax increases and freezing electricity and gas prices for 2019. The economic damage of these civil outbursts amounted to 0.2 percentage points of quarterly growth – approximately 5 billion euros – according to Reuters (2019)[14], and was commented on by the Finance Minister of France, Bruno Le Maire, with these words:

> "It's a catastrophe for business, it's a catastrophe for our economy." —
> *BBC, 2018*

Considering the far-reaching implications of mass mobilization like the Yellow Vest Protests, the question arises: what precise dynamics unfold behind protests? In this study, we investigate the factors driving individual protest decisions, drawing from a standard model of a single network good. In particular, we focus on how network effects influence individual participation and how these effects translate into aggregate outcomes.

On the individual level, one rationale for participating in a protest is deriving protest-dependent utility by either satisfying a sense of moral duty or benefiting from an underlying objective. While both causes seem plausible, two issues related to the latter must be mentioned. First, most protests are directed toward the greater good of the public, leading to the classic free-rider problem among potential participants (Apolte, 2012; Olson, 1971). The second issue is related to the paradox of voting. It is present even if the free-rider problem is disregarded: with the individual's participation being non-pivotal in achieving the underlying objective of the protest, the cost of participation should outweigh the benefit of contribution (Downs, 1957). This cost is particularly substantial in the case of protests since, depending on the context or country, individuals might face not only minor transportation and opportunity costs, but also severe repercussions like physical violence, incarceration, torture, and death.

A more recent literature focuses on the role of networks in explaining individual

[13] "Yellow vest protests 'economic catastrophe' for France", BBC, December 9, 2018, `https://www.bbc.com/news/world-europe-46499996`, last accessed: May 5, 2024.

[14] "French 'Yellow Vest' protests cost 0.2 percentage points of growth - Le Maire", Reuters, February 28, 2019, `https://www.reuters.com/article/idUSKCN1QH17O/`, last accessed: May 5, 2024.

participation decisions in protests. Similar to decisions regarding network goods, like joining a social media platform, an individual's value from protesting is assumed to increase as more people join (Economides, 1996). This insight originates in the seminal work of Kuran (1989) and Granovetter (1978), who emphasize interdependencies among protesters and analyze their implications for collective action. Such network effects can be understood as direct and indirect benefits an individual gains from participating, which increase as the network attracts further participants. In the context of protests, network benefits include peer effects through social ties. These peer effects may stem from social utility obtained from friendships made or maintained through joint participation in protest events (Bursztyn et al., 2021). They may also be the result of an individual signaling compliance with social norms to their peers to avoid social sanctions (Enikolopov et al., 2020b), a mechanism that has also been emphasized in the related literature on voting behavior (Gerber et al., 2008; DellaVigna et al., 2016). Larger protests will also attract more media coverage, which can further amplify these social incentives (Enikolopov et al., 2020a). However, the realization of these network benefits crucially depends on others' participation. The individual's decision to participate is thus determined by their ex ante expectation about the protest. While Carter and Carter (2020) and Truex (2019) identify focal points as a mechanism to increase individual expectations about protest size, the general problem of coordination remains.

Combining these well-known insights – (1) the individual's utility from participating is subject to protest-dependent attitudes; (2) the individual's participation is non-pivotal to achieving the protest objective; (3) networks affect the individual's participation decision while allowing for multiple equilibria due to coordination problems – we formalize a single theoretical framework and empirically test its implications. The model is in that way a formalization of network dependencies classical to models of protest coordination and theory of revolutions as established by Kuran (1989) and Granovetter (1978). In the framework, we consider a continuum of heterogeneous individuals whose utility from protesting consists of two components: (i) the interaction between an individual's type and the standalone benefit, and (ii) the network benefit that increases with the expected number of protest participants. Following the rational-expectation approach in the static model, we derive equilibrium outcomes for protest participation depending on the relation between the standalone and the network benefit. If the standalone benefit exceeds the network benefit, which we define as *weak network effects*, a unique equilibrium exists for any exogenous participation cost. If the network benefit exceeds the standalone benefit, which we define as *strong network effects*, two stable equilibria coexist for intermediate participation costs, namely the extreme outcomes of full and no participation. This result highlights the coordination problem individuals face when deciding to attend a protest, for which the network benefit is the determining factor for participation. In the dynamic version of the model, we further show the precise adjustment processes of protest participation over discrete time, which lead to the same outcomes in the limiting case as in the static model.

Based on this theoretical framework, we present descriptive empirical evidence on the relative influence of standalone and network benefits on the decision to protest, thereby quantifying effects introduced in the literature by authors such as Granovetter (1978) and Kuran (1989). We use data on more than 6000 individual respondents from the American National Election Studies (ANES) 2020 survey wave (ANES, 2021). In a robustness check, we use the 2016 survey wave. We first test the hypotheses (i) that the standalone benefit determines an individual's protest participation and (ii) that the network benefit determines an individual's protest participation. The spatial structure of our model then allows us to compare direct and indirect effects of the covariates on the decision to protest. From this comparison, we draw conclusions about the relative magnitudes of standalone and network benefits. We go on to explore heterogeneity in these findings across different political groups and protest movements.

The main empirical challenges consist of identifying standalone and network benefits and clustering individuals according to their political attitudes. Incorporating individuals' participation decisions within a social network into a simple ordinary least squares (OLS) regression leads to endogeneity due to the inherent interdependencies among them. Individual A's protest decision affects individual B's protest decision and vice versa. Hence, regressing individual A's protest decision on individual B's protest decision leads to simultaneity in the equation, resulting in biased and inconsistent estimates. To overcome this problem, we rely on a generalized spatial two-stage least squares (GS2SLS) estimator proposed by Drukker et al. (2013), in which the protest decisions of an individual's social network are instrumented by their individual characteristics. The standalone and network benefits are measured by interpreting the direct and indirect effects of a set of covariates. To explore heterogeneity across different societal groups, we rely on the clustering algorithm proposed by Kaufman and Rousseeuw (2009) for political groups and a decision rule for more granular protest groups.

The results from the baseline regression suggest that both the standalone and network benefits are driving forces behind individual protest decisions. To explore heterogeneity, we divide individuals into three clusters based on their political opinions. These clusters can be interpreted as groups of individuals within the sample holding left-leaning, moderate, and right-leaning political views. We find that the standalone and network benefits are significant determinants of the protesting decision within each cluster, though with varying magnitudes. Interestingly, network benefits seem to dominate standalone benefits for all clusters.

In a finer-grained specification, we pool individuals based on causes they consider important and identify four relevant potential protest movements: Black Lives Matter (BLM), healthcare, environment, and pro-immigration (these clusters are no longer mutually exclusive). We find strong network effects across all four protest movements. In an extension, we consider the model framework in combination with these benefit compositions to provide an explanation for their observed protest sizes.

This paper contributes to the literature by providing a better understanding of the determinants of protest participation. Drawing on a model of network goods applied to the context of protests, we find empirical evidence for the important role of network effects alongside standalone benefits in individuals' decisions to participate in protests. Specifically, we can disentangle individual-level effects from network-related effects and make statements about their relative influences. Methodologically, we contribute to the existing literature by employing a spatial identification strategy that involves building a synthetic network of United States (US) citizens. To the best of our knowledge, this approach to identifying network effects is novel to the protest literature.

The paper is structured as follows. After discussing the related literature in the following paragraphs, we introduce the framework in Section 4.2. In Section 4.2.1, we derive the potential equilibria of protest participation under strong and weak network effects in the static model. Next, we enrich the model by including discrete time in Section 4.2.2, deriving the precise protesting processes under weak and strong network effects. Thereafter, in Section 4.2.3, we formulate two hypotheses based on the model's implications. In Section 4.3, we introduce the empirical identification strategy used to test our hypotheses. The subsequent Section 4.4 presents the results for a baseline regression, as well as for political clusters in Section 4.4.2 and protest-specific clusters in Section 4.4.3. Finally, we conclude in Section 4.5. All proofs, additional figures, and tables are deferred to Appendices 4.A.1–4.A.3, respectively.

Related literature

Acemoglu and Robinson (2001, 2005) establish a first game-theoretic, formal model on the occurrence of protests by considering distribution battles among the population as the primary cause of protests and potential subsequent revolutions. Disadvantaged citizens utilize protests and the threat of revolution to pressure elites into creating a more equal distribution of wealth and income. These elites, who run the state, act under a revolution constraint that allows them to strategically distribute resources. According to the authors, protests and revolutions occur when elites fail to adhere to the revolution constraint, and disadvantaged citizens respond by raising their voices via protests. This theoretical perspective aligns with the broader framework introduced by Gurr (1970), who attributes the origins of protest to relative deprivation, defined as the perceived gap between expected and actual conditions.

The approach formulated by Gurr (1970) has been challenged by several scholars who emphasize the role of mobilizing resources as the primary driver of protest mobilization (McCarthy and Zald, 1977; Zald and McCarthy, 1987; Jenkins, 1983). Mobilizing resources are broadly defined as the tangible and intangible assets available to groups for coordination and collective action. Within this literature, particular emphasis is placed on organizational structures as key mechanisms enabling collective engagement in protest (e.g., B. Edwards et al., 2018). A similar point was made as a response to Acemoglu and Robinson (2001, 2005), as some authors see an inconsistency between the explanation of Acemoglu and Robinson (2001, 2005) and fundamental insights from Olson's (1971) Theory of Collective Action (Egorov and Sonin, 2024; Apolte, 2012). In particular, Apolte (2012) claims that inequality among a nation's citizens is neither sufficient nor necessary for enlarged protests or revolutions. Instead, the ability to overcome the free-rider problem should explain protest events and revolutions.

The fundamental conflict between these two strands of theoretical literature emerges due to the different characterization of the underlying objective of protests: *club good* vis-à-vis *public good*. Under the notion of a club good, citizens protest to benefit from expected political changes (e.g., Acemoglu and Robinson, 2001, 2005). In contrast, under the assumption that the protest objective is characterized as a public good, the benefits from political changes are distributed among all citizens, regardless of the individual participation decision. Consequently, individuals would decide to free-ride – i.e., not protest – if the participation cost is sufficiently high. We do not impose any characterization on the nature of protests or the outcome. Instead, the microfoundation of our model is related to the literature on the voting paradox, which states that the individual's action is non-pivotal to the success of the underlying objective (Downs, 1957). This approach allows us to consider an individual's choice for protesting as the choice for a single network good, where protest participation is driven by a standalone and a network benefit. Thus, the conception of our model is also closely related to the industrial organization literature addressing network effects (Belleflamme and Peitz, 2015; Economides, 1996).

Later developments in the protest literature acknowledge how central the coordination problem is for explaining protests. This literature focuses on informational aspects, as, for example, Ellis and Fender (2011) extend the model from Acemoglu and Robinson (2001, 2005) by introducing information transmission via informational cascades based on a model proposed by Lohmann (1994). This informational exchange facilitates coordination. Another approach to overcoming the coordination problem is proposed by De Mesquita (2010), who models a difference in the timing of movements. The model relies on a protest vanguard that moves first, thereby shaping the movement's trajectory. This move is observed by other citizens, who can subsequently join the protest. Both models incorporate informational exchange as a device for switching from a low to a high participation equilibrium. However, they maintain a club good characterization of protests, which makes them prone to the above-mentioned criticism, too. In our proposed model, we incorporate the presented informational aspects via expectation formation. In particular, in the dynamic specification, informational cascades are modeled explicitly, and we provide an account of the pace at which they materialize.

In this regard, our paper is based on the theoretical literature that centers on threshold models of collective behavior, as introduced by Granovetter (1978). In this framework, individuals choose to participate in collective actions – such as protests – when the number of prior participants exceeds a certain threshold. This idea provides a behavioral microfoundation for how participation cascades can emerge from heterogeneous individual preferences. Building on this, Wiedermann et al. (2020) present a network-based extension of Granovetter's model, showing how individual thresholds interact with social network structures to produce tipping points in protest dynamics. These models emphasize how even small shifts in expectations or participation can lead to rapid, large-scale mobilization, offering a complementary perspective to coordination-based and network benefit approaches.

Our paper is closely related to the work of Kuran (1989), who proposes a model in which revolutionary dynamics are driven by a threshold mechanism. In this model, individuals' privately held preferences regarding the regime or the opposition may diverge from their publicly expressed preferences. The utility an individual receives from expressing a preference depends on the expected share of other individuals supporting the regime (or the opposition) and on integrity costs incurred from expressing preferences that differ from privately held preferences. Due to the interdependence of public preferences, a shock that changes expectations of expressed preferences can set off a bandwagon, leading to more individuals finding it preferable to adapt their public stance, which may culminate in revolution. Kuran's (1989) insight that multiple equilibria exist when individuals' protesting decisions are interdependent is reflected in this paper.

Lastly, one recent strand of literature discusses the formation and effects of networks in overcoming the free-rider problem. The fundamental insight is that protest participation among social peers is strategically complementary. Such effects are identified by Bursztyn et al. (2021) in a field study with incentivized students in Hong Kong's anti-authoritarian protests and by Enikolopov et al. (2020a) using a natural experiment with social media data in Russia. Importantly, these approaches are capable of explaining protest participation irrespective of whether the desired outcome is a club good or a public good. We aim to contribute to this literature by preserving the central role of networks in overcoming the free-rider problem while explicitly modeling the strategic complementarity and testing its resulting hypotheses with a spatial model. The spatial model is a promising technique for identifying network effects, as highlighted by Souza (2024), and, to the best of our knowledge, has not been applied in the protest literature so far. Thereby, we provide complementary evidence in favor of Bursztyn et al. (2021) and Enikolopov et al. (2020a) while also contributing to the question of whether protest participation is a strategic complement or substitute as discussed by Cantoni et al. (2019) and Shadmehr (2021).

4.2 The framework

4.2.1 Static model

To establish the baseline model for participation in demonstrations, we follow the textbook approach by Belleflamme and Peitz (2015) on markets for a single network good.[15] Consider an economy with a continuum of heterogeneous individuals of mass 1. Each individual decides whether to participate in the demonstration ($d = 1$) or not ($d = 0$), such that the action set is $d \in \{0, 1\}$. If an individual protests, the individual's utility is given by

$$U_1(\theta) = \theta a + vn^e - c\,, \tag{4.1}$$

where the individual's type θ is uniformly distributed on the interval $[0, 1]$. The utility from demonstrating consists of two components: (i) the interaction between the individual's type and the standalone benefit $a > 0$, which can represent but is not limited to a feeling of moral obligation, sympathy for the protest, or the benefit of the potential policy implementation; and (ii) the network benefit $v > 0$, which, for simplicity, increases linearly with the expected number of participants n^e. This network benefit can encompass both direct effects (e.g., social status, identification with a movement) and indirect effects (e.g., larger venues, media representation). Participation comes at a non-monetary cost $c > 0$, capturing opportunity costs or personal repercussions. If the individual decides not to protest ($d = 0$), the individual receives the outside option

$$U_0 = \underline{\mathrm{U}}\,, \tag{4.2}$$

[15] A related version of the model is put forward by Economides (1996) in the industrial organization literature. Regarding the literature on protests and revolutions, there is a large number of models that take insights from Granovetter (1978) and Kuran (1989) and model protest as a network-dependent decision.

where, without loss of generality, we assume that the outside option is normalized to zero.[16]

For a given participation cost c and an expected network size n^e, the individual being indifferent between demonstrating or not is characterized by

$$\theta a + vn^e - c \geq 0\,, \tag{4.3}$$

where

$$\theta \geq \frac{c - vn^e}{a} := \hat{\theta} \tag{4.4}$$

denotes the critical type threshold, i.e., individuals with types lower than $\hat{\theta}$ choose not to participate.

Restricted to the outcome of no or full participation if $\hat{\theta}$ is not in the interval $[0, 1]$, the actual number of individuals demonstrating is

$$n(c, n^e) = \begin{cases} 0 & \text{if } n^e < (c-a)/v\,, \\ 1 - \frac{c - vn^e}{a} & \text{if } (c-a)/v \leq n^e \leq c/v\,, \\ 1 & \text{if } n^e > c/v\,. \end{cases} \tag{4.5}$$

We follow the rational-expectation approach to continue the analysis: if individuals form rational expectations in equilibrium, it must be that $n^e = n$. From the demand function in (4.5), it is straightforward that equilibria of no or full participation occur if either $c > a$ (nobody participates with $n = n^e = 0$) or if $c < v$ (everyone participates with $n = n^e = 1$). It follows that both equilibria coexist if $v > a$. Furthermore, the interior equilibrium with $0 < n^e = n < 1$ is

$$n(c) = \frac{a - c}{a - v}\,. \tag{4.6}$$

First, suppose we are in the scenario where the standalone benefit exceeds the benefit of the network, $v < a$, which we define as *weak network effects*. In that case, the number

[16]In Appendix 4.A.1, we show that incorporating a non-zero outside option—which can reflect free-riding incentives—has no qualitative effect on the equilibrium outcome.

of participants $n(c)$ is a decreasing function of the cost incurred. Thus, there is a unique participation level for any (exogenous) cost c: $n = 0$ for $c > a$, $n = (a - c)/(a - v)$ for $v \leq c \leq a$, and $n = 1$ for $c < v$.

Second, suppose we are in the scenario where the benefit of the network exceeds the standalone benefit, $v > a$, which we define as *strong network effects*. In that case, the number of participants is an increasing function of the cost incurred. As an intuition, the marginal willingness to incur costs increases with a marginal increase in participation numbers. Furthermore, there coexist three rational-expectations equilibria for intermediate costs ($a < c < v$): $n = 0$, $n = (c - a)/(v - a)$, and $n = 1$. By the argument of a dynamic adjustment process, i.e., a marginal cost increase (decrease) drives the equilibrium towards full (no) participation, we can disregard the unstable interior equilibrium (see Rohlfs, 1974). In contrast, the two remaining equilibria depict precisely the coordination problem individuals face when deciding on a protest for which the network benefit v is the determining participation factor. While both equilibria are stable, it is straightforward that by following the Pareto criterion, the no-participation equilibrium ($n = 0$) is Pareto-dominated: each individual would be better off coordinating to end up in the $n = 1$ equilibrium.

An example of the potential equilibria, i.e., the unique equilibrium under weak network effects and the multiple equilibria under strong network effects, for an exogenous cost $\hat{c}$ is depicted in Figure 4.1.

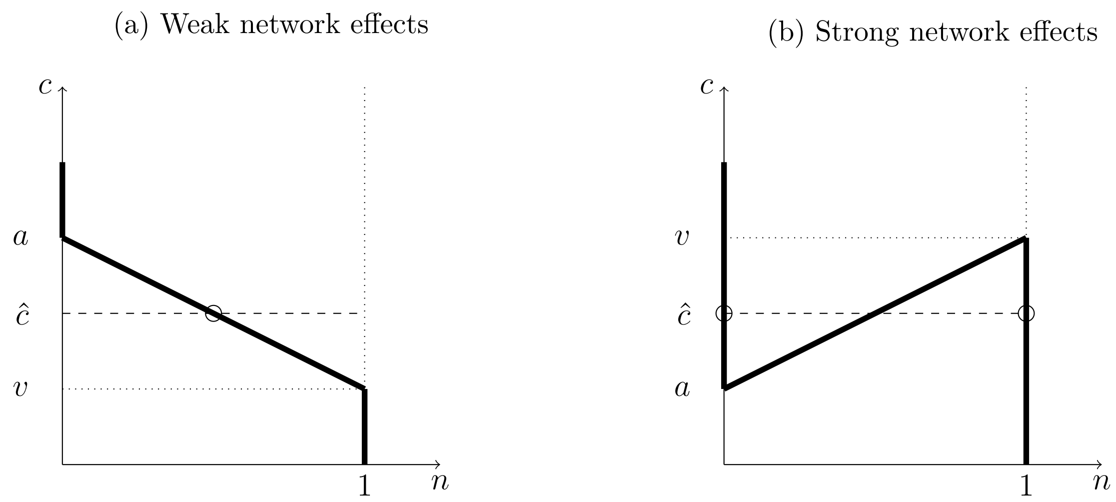


Figure 4.1: Equilibria under weak and strong network effects for given cost $\hat{c}$.

We can now characterize the following equilibria.

Proposition 4.1 (Static Network effects)**.** *In equilibrium, the number of individuals participating in the demonstration*

(i) under weak network effects ($v < a$) is denoted by

$$n(c) = \begin{cases} 0 & \text{if } c > a\,, \\ \frac{a-c}{a-v} & \text{if } v \leq c \leq a\,, \\ 1 & \text{if } c < v\,; \end{cases} \tag{4.7}$$

(ii) under strong network effects ($v > a$) is denoted by

$$n(c) = \begin{cases} 0 & \text{if } c > v\,, \\ 0 \veebar 1 & \text{if } a \leq c \leq v\,, \\ 1 & \text{if } c < a\,. \end{cases} \tag{4.8}$$

Conveying Proposition 4.1 to the real world, weak network effects may relate to protests about topics tied to goods (policies) and personal goals rather than social conditions and norms. Examples would be union wage talks, farmers' protests, or gun control, where the standalone benefit of the good, a, is the participant's priority, and higher private costs arguably lead to fewer participation numbers.

Examining the theoretical underpinnings of strong network effects in the context of protest participation, the BLM Movement, the 2020–2021 Belarusian Protests, and the Iranian Women's Rights Movement may be compelling examples. Despite the potential for high personal repercussions, these movements demonstrate how network effects can be powerful enough to motivate more individuals to participate (as long as the cost satisfies $c \leq v$). Furthermore, the coordination problem that arises under strong network effects, with its two stable corner equilibria, provides a theoretical foundation for the concept of focal points: despite no inherent structural change, there can be an equilibrium switch from no participation ($n = n^e = 0$) to full participation ($n = n^e = 1$) when instances, e.g., the killing of George Floyd, lead to higher expectations for protest.

4.2.2 Dynamic model

To further analyze individuals' protest participation and its underlying processes, consider now the dynamic version of the model in discrete time $t \geq 1$, where the utility from demonstrating ($d = 1$) is given by

$$U_{1,t}(\theta) = \theta a + v n_t^e - c \,. \tag{4.9}$$

For simplicity, assume adaptive expectations, where the expected number of participants at time t is the actual number of participants from the previous period with $n_t^e = n_{t-1}$, and that the initial participation level is exogenous with $n_0 \in [0, 1]$.[17] Holding the outside option fixed ($d = 0$) and normalized to $\underline{\text{U}}_{0,t} = 0 \;\forall\; t \geq 1$, the critical type threshold at time t denotes

$$\theta = \frac{c - v n_{t-1}}{a} := \hat{\theta}_t \,, \tag{4.10}$$

where the number of participants is $n_t = 1 - \hat{\theta}_t$.

We can then show the following.[18]

Proposition 4.2 (Dynamic Network effects)**.** *The number of individuals participating in the demonstration at time $t \geq 1$ evolves according to*

$$n_t(c, n_0) = \begin{cases} 0 & \text{if } n_0 < \frac{c-a}{v} \,, \\ n_0 \left(\frac{v}{a}\right)^t + \left(1 - \frac{c}{a}\right) \sum\limits_{i=0}^{t-1} \left(\frac{v}{a}\right)^i & \text{if } \frac{c-a}{v} \leq n_0 \leq \frac{c}{v} \,, \\ 1 & \text{if } n_0 > \frac{c}{v} \,, \end{cases} \tag{4.11}$$

where in the interior, the change in the number of participants per period is defined by

$$n_t - n_{t-1} = \left(\frac{v}{a}\right)^{t-1} (n_1 - n_0) := \Delta_t \,. \tag{4.12}$$

Thus,

[17] The assumption of naïve individuals, as in Kuran (1989), avoids unrealistic jumps from no to full participation.

[18] The proof is deferred to Appendix 4.A.1.

(i) under weak network effects ($v < a$), the number of participants converges to

$$\lim_{t\to\infty} n_t(c, n_0) = \frac{a-c}{a-v}\,; \tag{4.13}$$

(ii) under strong network effects ($v > a$), the number of participants converges to

$$\lim_{t\to\infty} n_t(c, n_0) = \begin{cases} 0 & \textit{if}\, n_0 < \frac{c-a}{v-a}\,, \\ 1 & \textit{if}\, n_0 > \frac{c-a}{v-a}\,. \end{cases} \tag{4.14}$$

Proposition 4.2 highlights the outcome of protest participation under weak and strong network effects, subject to the adjustment process in protest participation per period. Equation (4.11) states that protests where the initial protest size is characterized by full (no) participation remain at this extreme outcome. In contrast, protests where the initial participation number satisfies the interval $(c-a)/v \leq n_0 \leq c/v$ follow the dynamic adjustment process in Equation (4.12): Under weak network effects ($v < a$), the change in protest participation, Δ_t, decreases per period, and the protest size ultimately converges to its interior limit $(a-c)/(a-v)$. With the standalone benefit being the determinant factor of protest participation, the marginal benefit of one more individual joining the protest on the critical type $\hat{\theta}_t$ exactly outweighs the individual's participation cost. Under strong network effects ($v > a$), the change in protest participation, Δ_t, increases per period. Here, the limit outcome of the protest size crucially depends on its initial participation number n_0, where Equation (4.14) shows that the protest size drives to the full (no) participation equilibrium if $n_0 > (c-a)/(v-a)$ (if $n_0 < (c-a)/(v-a)$). The large (small) initial protest size in connection with the strong network benefit gradually increases (decreases) the critical type threshold $\hat{\theta}_t$, eventually leading to the extreme outcome.

This interplay between initial protest size and network effects may very well illustrate the workings of focal points. Focal points, when combined with strong network effects, can spur mass mobilization by attracting a sufficiently large number of initial protesters.

4.2.3 Model implications and hypotheses

Our theoretical model posits that individuals decide to participate in protests based on a utility function that includes both private (a) and social components (v). While the model does not yield directly testable implications, it provides a structure for identifying key mechanisms. We empirically test whether these mechanisms, standalone and network benefits, are significant predictors of participation. The following hypotheses guide our empirical analysis.

The first two hypotheses are direct premises of the model and address the cornerstone of our framework: the choice of a single network good can represent an individual's rationale for participating in a protest.

Hypothesis 1a. *The standalone benefit, as modeled in the utility function, increases an individual's protest participation.*

Hypothesis 1b. *The network benefit, as modeled in the utility function, increases an individual's protest participation.*

Turning to the implications of the model, the relationship between standalone and network benefits determines the equilibrium protest size. Under the assumption that participation costs are similar across different protests, strong network effects lead to extreme outcomes – that is, either small or large protests – whereas weak network effects lead to medium-sized protests.

Consistent with these insights, we expect substantial heterogeneity across societal subgroups in the effect sizes and relative importance of the standalone and network benefits. To test this, we formulate Hypothesis 2, which implies differences in the model's key parameters across different political and protest clusters. The second empirical challenge is thus to appropriately cluster individuals into more homogeneous groups and explore heterogeneities in the drivers of their protest participation.

Hypothesis 2. *The relative strength of standalone and network benefits differs across political and protest clusters.*

Potential heterogeneity implies that individuals' motivation for joining protests, and thus the relationship between standalone and network benefits, is protest-dependent. For instance, protests related to racial issues may have a different composition of standalone and network benefits than those related to healthcare policies. By identifying and comparing standalone with network benefits across different protests, this approach enables us to identify underlying strong or weak network effects.

4.3 Empirical strategy

Turning to the empirical strategy, our primary objective is to statistically identify both direct and indirect effects in the context of contemporary protest behavior in the US. We do not aim to estimate the structural model parameters a and v directly. Instead, we employ a spatial autoregressive (SAR) model that enables us to disentangle direct (individual-specific) and indirect (network-based) effects on personal protest decisions. Specifically, we regress the individual protest indicator on a set of covariates and a network component. This approach allows us to decompose the total effect of each variable into a direct effect at the individual level and an indirect effect operating through the social network. While this estimation strategy does not yield structural estimates of a and v, it provides an empirical approximation of their influence. Comparing the magnitudes of direct and indirect effects enables us to assess the relative importance of individual-level versus network-level determinants of protest behavior.

After estimating the spatial autoregressive model on the full ANES sample, we proceed to assess standalone and network effects within more homogeneous subgroups. To this end, we implement two clustering strategies. First, we construct political clusters comprising three groups: left-wing, moderate, and right-wing individuals. Second, we form issue-specific protest clusters corresponding to BLM, immigration, environmental, and healthcare-related protests. Analyzing these subgroups separately allows us to compare the composition of standalone and network effects and to relate our findings to actual protest participation levels observed in 2020, when the survey was conducted. Details on the clustering algorithm are provided in the Appendix.

4.3.1 Data

The ANES 2020 survey wave fulfills the crucial requirements for the selected empirical identification strategy. First, the database provides information on a representative sample from the US with extensive socio-demographic background information. More than 6000 individuals[19] participated in the 2020 survey wave, which is a sufficiently large sample for investigating social networks based on individual characteristics. Most importantly, the database provides us with a combination of information on (1) protest behavior,

[19] After dropping observations with missing values.

(2) socio-demographic characteristics, (3) associational memberships, and (4) political attitudes. Having this information in a representative sample of the US allows us to build an empirical model for individual protest decisions.

Variable	N	Mean	Std. Dev.	Min	Pctl. 25	Pctl. 75	Max
Protest Variable							
Protest	6100	0.10	0.30	0.00	0.00	0.00	1.00
Socio-Demographic Characteristics							
Age (years)	6100	50.95	17.05	18.00	36.00	65.00	80.00
Education	6100	4.63	2.03	1.00	3.00	6.00	8.00
Income	6100	11.96	6.71	1.00	6.00	18.00	22.00
Black (=1)	6100	0.087	0.28	0.00	0.00	0.00	1.00
Male (=1)	6100	0.47	0.50	0.00	0.00	1.00	1.00
Health Status	6100	2.40	1.02	0.00	2.00	3.00	4.00
Married (=1)	6100	0.53	0.50	0.00	0.00	1.00	1.00
Number of Children	6100	0.62	1.04	0.00	0.00	1.00	4.00
Personal Attitudes							
Left-right-scale	6100	5.56	2.77	0.00	4.00	8.00	10.00
Deviation Left-right-scale	6100	2.22	1.74	0.00	1.00	4.00	5.00
Importance Religion	6100	2.88	1.50	1.00	1.00	4.00	5.00
Trust in others	6100	2.21	0.90	0.00	2.00	3.00	4.00
Longitude	6100	−91.62	15.65	−157.49	−97.33	−79.68	−69.76
Latitude	6100	37.79	5.01	21.11	34.61	41.31	61.41
Associational Participation							
Volunteering	6100	0.30	0.46	0.00	0.00	1.00	1.00
Community Participation	6100	0.19	0.39	0.00	0.00	0.00	1.00
Church Attendance	6100	0.47	0.50	0.00	0.00	1.00	1.00
Political Attitudes Towards...							
Labor Unions	6100	58.74	24.09	0	50	75	100
Conservatives	6100	53.54	27.74	0	40	70	100
Liberals	6100	51.03	29.28	0	30	70	100
Feminists	6100	59.40	26.84	0	50	85	100
Christian Fundamentalists	6100	45.74	28.94	0	25	60	100
Police	6100	70.05	25.38	0	60	85	100
Black Lives Matter	6100	54.02	35.50	0	15	85	100

Table 4.1: Summary Statistics. The data source is the American National Election Study Wave 2020; the list contains all variables employed in the empirical analysis. Longitude and latitude of respondents' locations are summarized at the state level. All variables, their corresponding survey questions, and the authors' recoding procedures are documented in Table 4.6.

The empirical strategy relies on four categories of data included in the ANES database. Table 4.1 presents an overview of the variables used and their summary statistics. The binary protest variable indicates whether an individual respondent protested at least once during the past year. This binary protest variable serves as the dependent variable throughout this paper. Socio-demographic characteristics, personal attitudes, and associational memberships are used as control variables and to calculate statistical proximity (network ties) between survey participants. The details of this procedure are explained below. The political attitudes category contains *thermometer* scores in which participants rate their feelings towards several societal groups (the higher the score, the friendlier the attitude). Some examples are attitudes towards conservatives, liberals, BLM protesters, and feminists. These attitudes form the basis for analyzing political clusters separately and comparing their respective standalone and network benefits.

To address potential concerns regarding the time frame of the primary analysis, we re-estimate the model using data from 2016 as a robustness check. Since survey respondents were asked between November 8, 2020, and January 4, 2021, whether they had participated in a protest within the past 12 months, all had the opportunity to protest during the summer, when COVID-19 restrictions were generally relaxed. However, while protest activity was legally permissible during the summer of 2020, some individuals may have remained hesitant to participate. To ensure that our findings are not driven by pandemic-related constraints, we replicate the baseline analysis for 2016. The model specification remains unchanged, with the exception of the religious importance variable, which is measured on a five-point scale in 2020 and as a binary indicator in 2016.

4.3.2 Identification strategy

The empirical model is designed to separate standalone and network benefits in protest participation to assess Hypotheses 1a and 1b. To identify the standalone benefit – meaning the *utility* an individual receives from protesting, excluding any social network elements – we rely on *direct effects* of a set of predictor variables. Specifically, we assess the utility an individual receives merely because of protesting and expressing themselves without any social interaction before, during, or after the protest event. There is a variety of potential standalone protest determinants. To proxy individuals' intrinsic motivation to protest, we include their ideological distance from the political center on a left-right scale. The underlying hypothesis is that individuals with more extreme political views, i.e., those farther from the center, are ideologically more involved in the underlying protest objective and thus experience a larger intrinsic benefit (Marinthe et al., 2025).[20]

Expressive benefits are proxied by education, and psychological benefits by trust in others and religiosity. It is commonly argued that more educated individuals are more likely to be politically active and engage in protest behavior (Olcese et al., 2014; Dahlum and Wig, 2019). Trust in others is expected to positively affect protest participation, as

[20]It is also plausible that moderates are more likely to protest (Michaeli and Spiro, 2025). In this case, we would expect more extreme values on the left-right scale to have a negative effect on protesting likelihood.

Benson and Rochon (2004) argue that it may induce optimistic estimates of the uncertain components of the individual's utility from participation. That is, all other things being equal, a more trusting individual will expect a lower cost and higher benefit from protest participation.

To include additional variation that is not fully proxied through this set of variables, we control for general socio-demographic variables like sex, age, race, income, health status, marital status, and number of children. We expect younger individuals to have a higher intrinsic motivation to protest, as it has been argued that they are typically less constrained by family- or career-related obligations – thus giving them more time to protest – and that they are more prone to taking risks associated with protest participation than older individuals (Wiltfang and McAdam, 1991; Schussman and Soule, 2005). Being married and having children is expected to negatively affect protest participation, as these attributes come with obligations constraining an individual's time and risk-taking capabilities, a mechanism referred to by McAdam (1986) as 'biographical availability'. The effect of income on protest participation is theoretically ambiguous. Similar to the previous point, individuals with higher income may be more constrained by their career and may have more to lose from engaging in risk-taking behavior. Also, individuals with higher income may be more satisfied with the status quo and thus less intrinsically motivated to promote a policy change. On the other hand, higher income may proxy resources that enhance individuals' organizational capacity in accordance with the resource model of political participation (Brady et al., 1995). To account for associational memberships as contributing to individual protest participation, we include volunteering behavior, participation in community meetings, and church attendance. We expect associational memberships to be positively related to protest participation, as it can be argued that such civic participation reflects certain ideals or a sense of duty, which may also give rise to an intrinsic motivation to protest. In sum, these are the covariates of the model for predicting individual participation and, importantly, their direct effects exclude any network-related utility as this is controlled for using the spatial term. Note that we control for a wide range of variables that may proxy individual propensity to protest. If there were spatially correlated omitted variables affecting the likelihood of protesting, then their effect would be falsely attributed to the network effect. This concern is mitigated by our inclusion of control variables as specified above.

The identification strategy for the network benefit relies on a spatial autoregressive model. As discussed, for example, by Souza (2024), a spatial model is appropriate to identify network effects among individuals. That is, if individual A is a close friend of individual B, then A's decision to protest should influence B's decision and vice versa. The spillover is not limited to positive reinforcement. A's decision not to protest should consequently make individual B less likely to join the protest. Such spillover effects are identifiable via spatial regressions (Souza, 2024).[21] Thus, including a spatial term in a regression analysis in combination with suitable individual-level covariates allows for the identification of the effects explored in the protest decision model from Section 4.2.

The estimated spatial autoregressive parameter captures the degree of network-based interdependence in protest participation. While this spatial autoregressive parameter itself is not directly interpretable, it reflects the equilibrium relationship among individuals in the network (LeSage, 2015). Therefore, we disaggregate the total effect of the covariates into direct and indirect effects. The direct effect of an increase in, for example, income is how much more likely the individual is to protest when the income increases by one unit, excluding feedback effects of the network. Once the individual is more likely to protest as a consequence of the income shock, their peers are also more likely to protest, and thus they have a second-order effect on the previously shocked individual. These higher-order effects are captured by the indirect effect, which we estimate for all covariates. In the SARAR (spatial autoregressive model with autoregressive disturbances) setting, where the dependent variable and disturbances are spatially lagged, the ratio of direct to indirect effects is the same for all covariates[22]. The ratio provides the crucial insight into how much an individual is affected by changing standalone benefits (direct effect) and how much by network benefits (indirect effects). In sum, the spatial autoregressive model allows for the disaggregation of the standalone and network benefits and, thereby, allows us to approximate the relative magnitudes of the parameters of the proposed model.

The first step in building spatial models is to grasp the social network structure within the sample. For this, we construct a quantitative measure of social proximity between individuals. We rely on the concept of homophily – meaning that individuals who share

[21]Throughout this section, we refer to the estimated spillover effects or peer effects, which are transmitted via a synthetic network as "network effects". Thus, we keep the terminology consistent with the theoretical model.

[22]The size of the direct and indirect effects is determined by the weighting matrix, β coefficients, and the spatial autoregressive parameter.

similarities are more attracted to and exposed to each other and, therefore, are more likely to build friendships and partnerships (McPherson et al., 2001). The first empirical challenge is thus to measure how similar each individual is to all other individuals in the sample. We rely on the inverse of the Mahalanobis distance, which measures how different two individuals are based on a set of variables. The variables used to construct the Mahalanobis distance include: a ten-item left-right political attitude scale, importance of religion, trust in others, age, education, income, volunteering behavior, community participation, church attendance, and the geographic coordinates (longitude and latitude) of the respondent's state of residence. Unlike the Euclidean distance, the Mahalanobis distance accounts for the variables' covariances and hence does not weight highly correlated variables too strongly (Grimm and Yarnold, 2000). All variables are standardized prior to distance calculation.

Given the sample size, we rely on statistical distances between individuals in our sample to construct a synthetic network for the entire dataset. A database containing just over 6000 individuals across the US is, unsurprisingly, too small to identify actual social networks within the population. A solution is to rely on statistical distances among individuals. The distance between individuals A and B thus does not indicate how likely it is that both have a real network tie – e.g., a friendship – but rather indicates that individual A has individuals in their network that are similar to B. On this account, each individual within the sample has a synthetic network based on the whole ANES sample. This approach allows us to investigate whether an individual's synthetic network affects the individual's protest decision.

Equation (4.15) presents the regression model, including covariates, a spatial term, regional fixed effects, and an error term. The dependent variable P_i is a binary variable equal to 1 if individual i participated in at least one protest event during the past 12 months and zero otherwise.[23] The matrix X_i includes control variables for sex, marital status, children, age, race, income, education, health, extremism, importance of religion, trust in others, volunteering behavior, community participation, and church attendance. Details about these variables are available in Table 4.6. Spatial autocorrelation is assessed using

[23]Understood as protest events are street protests such as protest marches, rallies, and demonstrations, as per the wording of the question on which this variable is based (see Table 4.6). Other forms of voicing discontent are not covered by this variable. We cannot differentiate between non-violent and violent protests in this study even though we acknowledge that they may differ in their determinants (Chenoweth and Stephan, 2011; Gomez-Ruiz et al., 2025).

the spatial term $W_{invdist}\, P_{-i}$. The first element is a spatial weighting matrix indicating the inverse distances between each individual i and all other individuals -i, and the second element denotes the other individuals' protest decisions P_{-i}. Because individuals are unlikely to be influenced by their whole network, we restrict spillover effects to their fifteen closest neighbors[24]. $\delta_{r(i)}$ denotes a fixed effect for the census region of the individual's state (Northeast, South, Midwest, or West) and ϵ_i is the spatially correlated error term as depicted in Equation (4.16).

$$P_i = \alpha\, \mathbf{X}_i + \lambda\, \mathbf{W}_{invdist}\, P_{-i} + \delta_{r(i)} + \epsilon_i \tag{4.15}$$

$$\epsilon_i = \rho\, \mathbf{W}_{invdist}\, \epsilon_i + u_i \tag{4.16}$$

Simple OLS estimation of the model equation above would lead to biased estimates due to the simultaneity between the dependent variable from individual i and all other individuals -i. Therefore, we rely on the GS2SLS estimator proposed by Drukker et al. (2013). This estimator is based on spatial models derived earlier by Kelejian and Prucha (1998, 1999, 2010). The estimators instrument the simultaneous spatially lagged dependent variable with the remaining exogenous variables of each individual. Thereby, they allow for consistent estimation of spatial autoregressive models. To account for spatially correlated errors, we extend the SAR model to a SARAR specification, which explicitly allows for error terms to be correlated across the network. This ensures that our estimates remain robust in the presence of network-based error dependence.

Our regression framework provides the basis for estimating both the direct and indirect effects of each explanatory variable, which are central to our identification strategy for separating standalone and network benefits. Specifically, we interpret a one-unit increase in the variable of interest as leading to a predicted increase in the outcome variable, which can be decomposed into two components: a direct, individual-level effect and an indirect, network-related effect. The direct effect captures the immediate impact on the individual, while the indirect effect reflects the cumulative influence transmitted through the network, where the initial change affects peers, who in turn influence the original

[24] In the weighting matrix, the fifteen nearest neighbors are assigned a value of 1, while all others are set to 0. Then the weighting matrix is row normalized, a standard procedure in estimating spatial models (LeSage, 2015). The number of fifteen closest neighbors is varied in a sensitivity analysis to demonstrate the robustness of the results.

individual, generating recursive spillovers. When network interactions are reinforcing, this feedback amplifies the initial impact. Accordingly, we interpret the direct effect as an indication of the standalone benefit and the indirect effect as the network benefit. Importantly, effects are driven by the same underlying variation in the explanatory variable x.

Equations (4.17) to (4.19) define the direct, total, and indirect effects following LeSage (2015). These effects are derived from the spatial regression model and estimated to interpret the influence of explanatory variables in the presence of spatial dependence. Specifically, the direct effect, $\overline{M}(r)_{\text{direct}}$, captures the average impact of a change in a covariate on the outcome without taking the network into consideration. The total effect, $\overline{M}(r)_{\text{total}}$, measures the average overall impact of a change in a covariate for one individual on the outcome variable across all individuals in the network. The indirect effect, $\overline{M}(r)_{\text{indirect}}$, is then obtained as the difference between the total and direct effects and reflects the average spillover effect, i.e., how a change in one individual's covariate affects others in the network through spatial or social connections.

These effects are computed using the matrix $S_r(W) = (I_n - \lambda W)^{-1} I_n \alpha_r$, where I_n is the identity matrix of size n, λ is the spatial autoregressive parameter, W is the spatial weighting matrix, and α_r is the coefficient of the r-th covariate. The term $(I_n - \lambda W)^{-1}$ captures the spatial multiplier effect, indicating spillover effects throughout the network. The trace (tr) operator in the direct effect formula averages the diagonal elements of the impact matrix, while the total effect is computed as the average of all elements (l_n being a $n \times 1$ vector of ones). The spatial regression, the decomposition into direct and indirect effects, and the associated test statistics are estimated using the `spatialreg` R package provided by Bivand et al. (2021).

$$\overline{M}(r)_{\text{direct}} = n^{-1}\,\mathrm{tr}(S_r(\mathbf{W})) \tag{4.17}$$

$$\overline{M}(r)_{\text{total}} = n^{-1} l_n' S_r(\mathbf{W}) l_n \tag{4.18}$$

$$\overline{M}(r)_{\text{indirect}} = \overline{M}(r)_{\text{total}} - \overline{M}(r)_{\text{direct}} \tag{4.19}$$

where

$$S_r(\mathbf{W}) = (I_n - \lambda \mathbf{W})^{-1} I_n \alpha_r \tag{4.20}$$

4.3.3 Heterogeneity analysis

After having obtained average results across a representative sample of the US population, we are interested in exploring heterogeneities among different societal groups across the US. Considering the model dynamics, these differences in the composition of benefits directly translate into different equilibrium outcomes for protests. The key objective of forming groups across the US population is thus to explore potential differences in the causes of their protest participation. To do so, we build clusters and assess the standalone and network benefits for them separately. There are two subsets we investigate. First, different individuals are clustered into political groups, and we examine whether the protest participation determinants (i.e., standalone and network benefits) vary across these political groups. Second, individuals are clustered based on their likelihood of participating in different protest movements, and then we assess whether the protest determinants vary across these clusters. Both specifications contribute to testing Hypothesis 2, as they explore heterogeneities among protest determinants that help explain differences in protest sizes across movements.

I. Political Clusters

The first heterogeneity analysis consists of differentiating protests according to their *political directions*. We aim to elicit differences between political groups in terms of the underlying reasons for their protest decisions – i.e., are there substantial differences in the standalone and network benefits across different political clusters? To construct political clusters, we rely on a clustering algorithm by Kaufman and Rousseeuw (2009). This algorithm is based on several protest-sensitive political attitudes and forms clusters according to similarity between individuals. We use this algorithm to minimize subjective assumptions in the clustering process. A thorough description of the clustering methodology is available in Section 4.A.2 in the Appendix.

Our approach relies on survey respondents' attitudes towards a set of stylized groups of people within US society. The groups are listed in Table 4.1 under "Political Attitudes Towards...". These variables are scores from 0 (very low opinion) to 100 (very high opinion) for the following groups: labor unions, conservatives, liberals, feminists, Christian fundamentalists, police, and BLM participants. Clustering individuals according to their attitudes towards these groups should result in groups that share concerns about similar political issues and hold relatively similar political views. For example, individuals with a very low opinion of feminists and a high opinion of conservatives and Christian fundamentalists are more likely to attend the same protest movements. The same applies to individuals with opposing views, who are likely to participate in different types of protest events.

II. Protest Clusters

The final empirical specification explores further heterogeneities by disaggregating the analysis to specific *protest movements*. An in-depth description of the methodology followed is presented in Section 4.A.2 in the Appendix. This approach is intended to shed further light on Hypothesis 2, which claims that there are dominant network effects in (at least some) protest movements. To further identify the conditions under which network effects are most pronounced, we construct clusters of individuals based on the protest causes they support. This allows us to examine how the relative importance of the network and standalone benefits varies across these groups.

The survey includes a question asking respondents to identify what they consider the most pressing problems facing the US. Participants were allowed to name multiple issues. Based on the frequency of responses, we focus on four topics that emerged most prominently: healthcare (42.7%), race relations (19.3%), the environment (6.6%), and immigration (4.3%). These categories serve as the basis for forming issue-specific protest clusters, which allow us to analyze protest behavior in a more targeted way. Importantly, individuals may belong to multiple groups, as participation in one protest does not preclude involvement in another. Therefore, the clusters in this specification are not mutually exclusive.

4.4 Results

In the following section, the theory-deduced hypotheses are tested by relying on the presented empirical identification strategy. As a first step, Hypotheses 1a and 1b are tested for all individuals across the US sample. The hypotheses indicate that standalone and network benefits shape individual protest participation. Sections 4.4.2 and 4.4.3 explore heterogeneities among different societal groups regarding the benefit composition. Thereby, these specifications shed light on Hypothesis 2, which indicates that, given the stark contrast of aggregate protest outcomes, we expect substantially different benefit compositions between different protest movements.

4.4.1 Baseline estimation

The baseline estimation follows the identification strategy described in Section 4.3.2, where a spatial weighting matrix is defined via which each individual is affected by their peers. To demonstrate robustness to methodological choices, we present the results for different specifications in which spillovers are given by the 10, 15, or 20 nearest neighbors. Furthermore, we include a set of covariates commonly applied in the literature to estimate their effects on an individual's likelihood to participate in protests. The model is estimated using the presented GS2SLS estimator.

Table 4.2 presents the spatial regression output for the baseline regression, indicating the presence of robust network effects and the significance of the included covariates. Importantly, the regression coefficients are not yet interpretable because a shock to a particular individual has an impact on the spatial (network) equilibrium, which in turn has feedback effects (LeSage, 2015). We can merely state that some of the covariates, like age, marital status, or the number of children, are likely to affect individual protest participation. As we observe a statistically significant λ and ρ, we conclude that there is indeed substantial spatial (network) dependence within the analyzed sample. Hence, OLS estimation of the same model is biased and inconsistent, while the SARAR specification effectively accounts for spillover effects across the network.

A possible concern for our interpretation of the significant spillover effects is that they may be the result of joint shocks. That is, individuals who are similar to one another according to our distance measure might be simultaneously exposed to a common shock affecting their likelihood to protest. Such a shock could be a reduction in the cost of protesting or a policy making a sub-group of our sample more likely to protest. While we cannot completely rule out this concern, it is mitigated to an extent by also including the variables that compose the Mahalanobis distance measure as control variables. Moreover, we later repeat the analysis for more homogeneous sub-groups of our sample. With the sample being more homogeneous regarding their political views or their most important issues, it becomes more likely that a common shock affects all individuals in the sample and not just some subset.

Table 4.2: Baseline spatial regression results.

	Model (1) Neighbors: 10	Model (2) Neighbors: 15	Model (3) Neighbors: 20
Intercept	0.216***	0.179***	0.148***
	(0.047)	(0.043)	(0.039)
Male (=1)	−0.002	−0.002	−0.002
	(0.007)	(0.007)	(0.007)
Age (years)	−0.005***	−0.004***	−0.003***
	(0.001)	(0.001)	(0.001)
Age (years)^2	0.000**	0.000*	0.000*
	(0.000)	(0.000)	(0.000)
Black (=1)	0.016	0.017	0.017
	(0.014)	(0.014)	(0.013)
Income	0.000	0.000	0.000
	(0.001)	(0.001)	(0.000)
Education	0.003*	0.002	0.000
	(0.002)	(0.001)	(0.001)
Left-right-scale	−0.015***	−0.012***	−0.010***
	(0.002)	(0.002)	(0.002)
Deviation Left-right-scale	0.023***	0.021***	0.018***
	(0.003)	(0.003)	(0.002)
Health	0.001	0.000	0.000
	(0.004)	(0.004)	(0.004)
Married (=1)	−0.026***	−0.025***	−0.025***
	(0.008)	(0.008)	(0.008)
Number of children	−0.018***	−0.017***	−0.016***
	(0.004)	(0.004)	(0.003)
Importance Religion	0.006**	0.004*	0.002
	(0.003)	(0.003)	(0.002)
Trust in others	0.004	0.003	0.002
	(0.003)	(0.003)	(0.003)
Volunteering	0.039***	0.029***	0.020***
	(0.009)	(0.008)	(0.007)
Community Participation	0.072***	0.050***	0.028**
	(0.015)	(0.015)	(0.013)
Church Attendance	0.014**	0.015**	0.016***
	(0.007)	(0.006)	(0.005)
North East	−0.010	−0.006	−0.004
	(0.010)	(0.010)	(0.009)
Mid West	−0.025***	−0.019**	−0.015*
	(0.009)	(0.009)	(0.008)
South	−0.028***	−0.024***	−0.021***
	(0.009)	(0.008)	(0.007)
Spatial Lag Y: λ	0.366***	0.576***	0.778***
	(0.123)	(0.123)	(0.112)
Spatial Lag ϵ: ρ	−0.358***	−0.535***	−0.759***
	(0.130)	(0.145)	(0.151)
Observations	6100	6100	6100

Heteroskedasticity robust spatial SE in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Baseline SARAR regression results are based on Equation (14). The full ANES sample is used. Columns differ by the number of nearest neighbors used in the spatial weighting matrix (10, 15, and 20, respectively). All variables, their corresponding survey questions, and the authors' recoding procedures are documented in Table 4.6.

Once we obtain the spatial regression output, we estimate the direct, indirect, and total effects of each covariate based on the spatial parameter λ, the weighting matrix W, and the variable's spatial point estimate α. The results of this estimation are presented in Table 4.3. As can be seen in the table, both the direct and the indirect effects are identified at a statistically significant level. A one-unit increase in *Left Right Scale* (equivalent to becoming "more right-wing") decreases one's likelihood to participate in a protest by 1.5 percentage points directly and by 0.8 percentage points indirectly, amounting to a total effect of 2.3 percentage points. The indirect effect is driven by the individual's effect on the network equilibrium, where the individual is influenced by the network's feedback effects. A more substantial total effect is observed for the deviation from the political center. If *Left Right Deviation* increases by one unit, the individual is, altogether, 3.7 percentage points more likely to join a protest event.

Since protest behavior is typically modeled without accounting for spatial dependencies, we include an OLS regression of Equation (4.15), omitting the spatial term. The results are presented in Table 4.7 in Appendix 4.A.3. As the spatial term is statistically identified previously, the OLS estimates are biased due to the exclusion of spatial spillovers and do not allow for a decomposition of the total effect into direct and indirect components. Nonetheless, the direction of the estimated effects is broadly consistent with the results reported in Table 4.3. However, the magnitude of several coefficients is substantially affected by the inclusion of the spatial term. For example, the estimated effect of having an additional child in the household is a 1.8 percentage point decrease in the probability of protesting in the OLS specification, which changes to a 2.8 percentage point decrease in the spatial specification (GS2SLS).

Interpreting these results in light of the theory-deduced hypotheses, we find evidence for Hypotheses 1a and 1b as both standalone (direct effects) and network (indirect effects) benefits affect protest participation. In Proposition 4.1, we distinguish between two equilibria defined by the relative magnitude of standalone (individual-level) and network (network-level) benefits. The results suggest that the individual-level, direct effect is larger than the network-level, indirect effect, as it accounts for a greater share of the total effect. Expressed as a ratio, the estimated indirect effects are approximately 0.56 of the direct effects (ratio given by indirect effect divided by direct effect size). This comparison provides suggestive evidence that, for the full sample, protest behavior exhibits weak network effects, aligning with equilibrium outcome (a) in Proposition 4.1.

The findings connect to a broader literature by indicating that protests are games of strategic complements. This fact becomes clear when interpreting the consistently positive statistical identification of λ, indicating positive autocorrelation within the network. The

Table 4.3: Baseline impacts.

Effect	Direct	Indirect	Total
Male (=1)	−0.002	−0.001	−0.002
	(0.007)	(0.005)	(0.012)
Age (years)	−0.005***	−0.003*	−0.007***
	(0.001)	(0.001)	(0.002)
Age (years)^2	0.000**	0.000	0.000**
	(0.000)	(0.000)	(0.000)
Black (=1)	0.016	0.009	0.025
	(0.014)	(0.012)	(0.024)
Income	0.000	0.000	0.000
	(0.001)	(0.000)	(0.001)
Education	0.003	0.002	0.004
	(0.002)	(0.001)	(0.003)
Left-right-scale	−0.015***	−0.008**	−0.023***
	(0.002)	(0.004)	(0.003)
Deviation Left-right-scale	0.023***	0.013**	0.037***
	(0.003)	(0.007)	(0.006)
Health	0.001	0.000	0.001
	(0.004)	(0.003)	(0.006)
Married (=1)	−0.026***	−0.015	−0.041**
	(0.008)	(0.011)	(0.017)
Number of Children	−0.018***	−0.010*	−0.028***
	(0.004)	(0.006)	(0.008)
Importance Religion	0.006**	0.004*	0.010**
	(0.003)	(0.002)	(0.004)
Trust in others	0.004	0.002	0.007
	(0.003)	(0.003)	(0.006)
Volunteering	0.040***	0.022**	0.062***
	(0.009)	(0.010)	(0.012)
Community Participation	0.073***	0.041***	0.114***
	(0.015)	(0.016)	(0.015)
Church Attendance	0.015**	0.008	0.023**
	(0.006)	(0.006)	(0.011)
North East	−0.010	−0.005	−0.015
	(0.011)	(0.007)	(0.017)
Mid West	−0.025***	−0.014*	−0.039***
	(0.009)	(0.008)	(0.014)
South	−0.029***	−0.016*	−0.045***
	(0.009)	(0.009)	(0.014)

Heteroskedasticity robust spatial SE in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Impact estimation of baseline SARAR regression results is based on Equation (14). The effect ratio is 0.56 (indirect effect/direct effect). The full ANES sample is used. Columns differ by the estimated effect type. Results are obtained for the specification that includes the 10 nearest neighbors in the weighting matrix. All variables, their corresponding survey questions, and the authors' recoding procedures are documented in Table 4.6.

paper thus confirms findings from experimental studies about positive network effects in field experiments (Bursztyn et al., 2021), quasi-experimental studies (Enikolopov et al., 2020a), and studies about online networks (Larson et al., 2019; Mundt et al., 2018). The contribution to these existing studies is the novel application of constructing a synthetic network and applying a spatial autoregressive model to estimate network dependence.

4.4.2 Political clusters

In the previous section, we found evidence for Hypotheses 1a and 1b as both the standalone and network benefits were found to increase an individual's protest participation, as predicted by the proposed model. For the full sample, direct effects seem to exceed indirect effects, indicating a situation of weak network effects formulated in case (a) in Proposition 4.1. This result may change once we collect more fine-grained evidence in more homogeneous groups. To explore such heterogeneities, we apply the same estimation techniques to political clusters built according to the algorithm presented in the Appendix in Section 4.A.2. Within these clusters, individuals show much stronger homogeneity as they are sorted according to their political attitudes.

Table 4.4 presents the estimated spatial coefficients and the ratio of indirect to direct effects in the estimation of the political clusters. The parameter λ should, by definition, be an element between -1 and 1. As can be seen, for example, for the right-wing specification with 20 neighbors, the point estimate exceeds this range. Considering the standard errors, the 95% confidence interval clearly includes values below 1, and hence the exceeding value is likely due to statistical imprecision. Therefore, we interpret the parameter nonetheless. However, calculating the indirect to direct effect ratio is no longer informative, as the Leontief Expansion is only applicable when ρ is an element between -1 and 1.[25]

Table 4.4 provides consistent evidence of network dependencies. Across all specifications except 10 nearest neighbors for right-wing individuals, the estimated spatial autoregressive parameter (λ) exceeds 0.7, indicating substantial spillover effects among individuals within political clusters. Drawing on the indirect-direct effect ratios, the evidence points to strong network benefits within the political clusters. Except for the specification with 10 nearest neighbors for right-wing individuals, all other specifications show ratios above 2, implying that the indirect effect is at least twice as large as the direct effect, excluding specifications where this comparison is not calculated due to $|\rho| > 1$.

Applying this evidence to the proposed model, we argue that *within* political clusters, protest behavior is characterized by strong network effects, as opposed to the weak network effects found in the previous subsection for the entire sample. This leads us to conclude

[25] If $|\rho| < 1$, the series of feedback loops can be simplified: $(I_n - \rho W)^{-1} = I_n + \rho W + \rho^2 W^2 + \rho^3 W^3 + ...$ (LeSage, 2015, p. 34). Once $|\rho| \geq 1$, this transformation is no longer possible and, hence, we omit displaying the indirect to direct effect ratio.

Table 4.4: Political cluster impacts

Political cluster	Right-wing	Moderates	Left-wing
	Neighbors: 10		
Spatial Lag Y: λ	0.683	0.693***	1.106***
	(0.189)	(0.139)	(0.164)
Spatial Lag ϵ: ρ	−0.653	−0.831***	−0.900***
	(0.179)	(0.187)	(0.177)
Indirect effect/direct effect	2.02	2.11	*omitted*
	Neighbors: 15		
Spatial Lag Y: λ	0.950***	0.736***	1.209***
	(0.214)	(0.169)	(0.187)
Spatial Lag ϵ: ρ	−0.900***	−0.882***	−0.900***
	(0.227)	(0.237)	(0.226)
Indirect effect/direct effect	16.90	2.64	*omitted*
	Neighbors: 20		
Spatial Lag Y: λ	1.161***	0.733***	1.464***
	(0.253)	(0.189)	(0.213)
Spatial Lag ϵ: ρ	−0.900***	−0.900***	−0.900***
	(0.305)	(0.278)	(0.238)
Indirect effect/direct effect	*omitted*	2.64	*omitted*

Heteroskedasticity robust spatial SE in parentheses; * p < 0.1, ** p < 0.05, *** p < 0.01.

Note: Political cluster SARAR regression results are based on Equation 4.15. Each column corresponds to a distinct political cluster. Covariate coefficients are omitted for brevity.

that, in relation to Proposition 4.1, protests organized within more homogeneous political groups correspond to case (b), strong network effects. The resulting model prediction is that, for these groups, equilibria with either zero or full participation can emerge. Hence, extreme cases of mobilization are predicted to prevail in protests within these groups. To get an idea of whether this is actually the case, we analyze even more homogeneous groups in the following subsection, where we specifically shed light on four protest movements.

4.4.3 Protest clusters

While political clusters already help investigate network effects in more homogeneous groups of individuals, the objective of this subsection is to assign individuals to protest movement clusters. The advantage of doing so is that we can compare the measured effects to the aggregate protest outcome during 2020 and relate them to the model predictions. To do so, four topics are chosen: BLM, immigration, health, and environment, which have been particularly salient in 2020. The detailed procedure is described in Section 4.A.2.

Interpreting the results presented in Table 4.5, we find robust evidence of strong network effects for protests related to BLM, immigration, health, and the environment—except in the specification using the 10 nearest neighbors, where the effects for health and immigration are not statistically significant. These results are driven by the high values of the λ-parameter for these protest movements, which subsequently lead to high ratios of indirect to direct effects. As a result, we predict the aggregate protest outcome of BLM, immigration, health, and environment to either show very high or very low participation, as described in the case differentiation *strong network effects* in Proposition 4.1.

Table 4.5: Protest cluster impacts

Protest cluster	BLM	Immigration	Health	Environment
	Neighbors: 10			
Spatial Lag Y: λ	0.935***	1.145***	0.313	0.011
	(0.158)	(0.292)	(0.196)	(0.256)
Spatial Lag ϵ: ρ	−0.900***	−0.900**	−0.188	−0.068
	(0.205)	(0.387)	(0.215)	(0.300)
Indirect effect/direct effect	12.159	*omitted*	0.446	0.011
	Neighbors: 15			
Spatial Lag Y: λ	0.853***	1.324***	0.553***	0.731***
	(0.187)	(0.487)	(0.178)	(0.283)
Spatial Lag ϵ: ρ	−0.900***	−0.900	−0.468**	−0.730*
	(0.258)	(0.596)	(0.211)	(0.378)
Indirect effect/direct effect	5.402	*omitted*	1.198	2.594
	Neighbors: 20			
Spatial Lag Y: λ	1.073***	1.261**	0.727***	0.931***
	(0.210)	(0.581)	(0.159)	(0.325)
Spatial Lag ϵ: ρ	−0.900***	−0.900	−0.731***	−0.852**
	(0.288)	(0.641)	(0.212)	(0.422)
Indirect effect/direct effect	*omitted*	*omitted*	2.568	2.594
Observations	882	263	2600	401

Heteroskedasticity robust spatial SE in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Protest cluster SARAR regression results are based on Equation 4.15. Each column corresponds to a distinct protest cluster. Covariate coefficients are omitted for brevity.

While the theoretical insight of protest benefits being composed of standalone and network benefits can be assumed to hold universally, the heterogeneity in their relative magnitudes across political groups and protest movements is likely to be country-specific. This is because the strength of networks and social capital as well as the demographic composition within a certain political movement may differ between countries, leading to different relative magnitudes of standalone and network benefits. Thus, the insights we gain in the analysis of heterogeneities for the case of the US may not be valid for other countries.

When considering the aggregate protest outcomes in Figure 2, we can interpret them in light of the identified strong network effects. (Anti-)Racism, as a protest topic, was by far the most salient during 2020. From the modeling perspective, individuals managed to coordinate into the high participation equilibrium for this category of protests. By contrast, protests related to environmental and health issues remained sparse and small in scale, suggesting that despite the presence of strong network effects, individuals failed to coordinate and thus remained in a low-participation equilibrium. Finally, immigration-related protests exhibited a moderate level of participation, suggesting that network effects may not have been fully realized.

The connection of the individual-level findings to the aggregate protest outcome should be understood as an illustrative analysis rather than a statistical test, as there are some limitations to this endeavor. There is no measure of the individual participation cost for any of the protests. Partly, these costs are proxied by the covariates, and generally, they may not differ too much, given the democratic right to protest in the US. However, the varying observational units and different participation rates within the different clusters do not constitute a limitation of this analysis. Spillovers are measured in both positive and negative directions, and hence, there can be strong network effects even if only a few individuals protest in the cluster. Also, cluster size should have no impact on the coefficient size; if anything, small clusters may show larger standard errors due to lower statistical power.

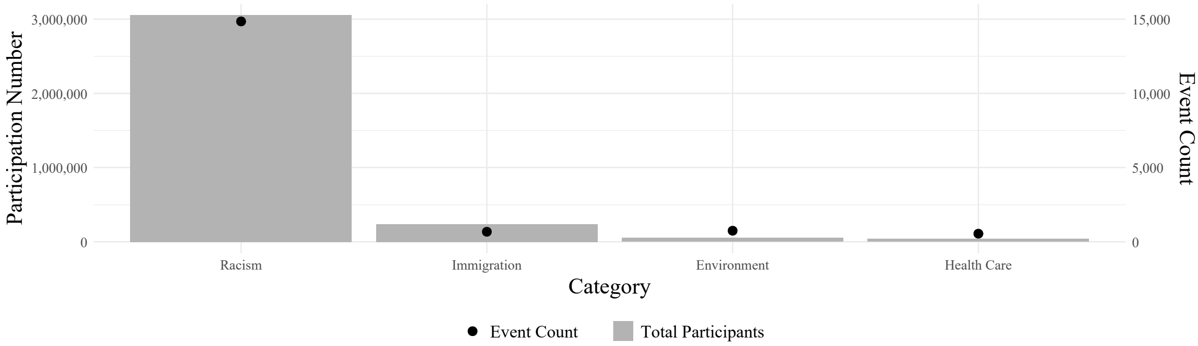


Figure 4.2: Illustration of protest size and frequency grouped by protest issues for 2020 according to the Crowd Counting Consortium database.

As discussed previously, we find strong evidence for the strategic complementarity of protests for BLM, immigration, health, and the environment. Protest mobilization dynamics for these categories can thus be modeled as coordination games in which both the high and low participation equilibria may materialize, similar to De Mesquita (2010). The murder of George Floyd on May 25, 2020, sparked large protests, shifting participation from low to high. In the context of coordination games, this event qualifies as a Focal Point, which makes a certain equilibrium salient and thus leads to coordination. Such mechanisms are first described by Schelling (1960) from a game-theoretical perspective. Other scholars investigate the role of focal points, mostly interpreted as particular days associated with a country's culture and history, in protest coordination (Truex, 2019; Carter and Carter, 2020; Ketchley and Barrie, 2020). For environmental and healthcare-related protests, no sufficiently salient event occurred to trigger large-scale protests.

Another explanation for the large volume of anti-racism protests lies in the interplay between political opportunity structures (POS) and emancipative values. POS theory posits that protest activity becomes more likely when political systems exhibit openness to change (Tarrow, 1996; Kriesi, 1995). In the context of 2020, the anti-racism movement gained momentum following the murder of George Floyd, a pivotal event that may have expanded perceived opportunities for influencing policy. This shift likely facilitated the coordination of anti-racism protests, while mobilization around other issues—such as immigration, environmental protection, and healthcare—remained comparatively limited. More recent research highlights the role of emancipative values in driving protest behavior (Welzel and Deutsch, 2012; Donni et al., 2021). Specifically, Welzel and Deutsch (2012) argue that values centered on freedom and equality precede institutional change and can be activated through protest. Similarly, Donni et al. (2021) find that even when controlling for institutional factors, emancipative values significantly increase the likelihood of protest participation. These cultural foundations offer an additional explanation for the scale and intensity of anti-racism protests relative to other movements.

4.5 Conclusion

This paper builds on influential models developed by Kuran (1989) and Granovetter (1978) and empirically tests the relevance of the interdependencies in protesting decisions which they propose. First, we apply a model of a single network good by Belleflamme and Peitz (2015) to model individuals' protest decisions. We argue that the individual's protest participation is non-pivotal to the success of the underlying protest objective and that the utility derived from protesting consists of two components: (i) the interaction between an individual's type and the standalone benefit, and (ii) the network benefit, which increases linearly with the expected number of further participants. We then derive equilibrium outcomes where the relation between the standalone benefit and the network benefit determines the aggregate outcome: Under *weak network effects*, a unique equilibrium exists for any exogenous participation cost. Under *strong network effects*, two stable equilibria coexist for intermediate participation costs, namely the extreme outcomes of full and no participation, which illustrates the coordination problem of protests.

The results from the empirical investigation provide evidence on the relative importance of standalone and network benefits as determinants of protest participation. Using a spatial auto-regressive model, we find that both standalone and network benefits are drivers of individual protest participation in the whole US sample. We compare estimated indirect and direct effects of the model's covariates to draw conclusions about whether the protesting decision is characterized by strong or weak network effects. We find evidence for strong network effects in three clusters of individuals with similar political attitudes. Furthermore, our results suggest strong network effects across different protest movements related to race relations, immigration, healthcare, and the environment. Given the observed differences in size and mobilization success of actual protests, this finding suggests heterogeneity in protesters' ability to solve the coordination problem across protest movements.

4.A Appendix

4.A.1 Mathematical appendix

Proof (Outside Option $\underline{U}$). First, suppose that an individual's outside option ($d = 0$) is type-dependent with

$$\underline{U}_0 = \theta \underline{u}\,, \tag{4.21}$$

where θ is uniformly distributed on the interval $[0, 1]$. Then, the critical type threshold denotes

$$\hat{\theta} := \frac{c - vn^e}{a - \underline{u}}\,, \tag{4.22}$$

where it is straightforward to see that the type-dependent outside option only affects the level and slope of the function $n(c)$.

Second, suppose that an individual's outside option scales with the expected number of participants, i.e.,

$$\underline{U}_0 = \phi n^e \tag{4.23}$$

with base utility $\phi > 0$. Then, the critical type threshold denotes

$$\hat{\theta} := \frac{c - (v - \phi)n^e}{a}\,, \tag{4.24}$$

where we can readily see that the scaling outside option, ϕn^e, only affects the steepness of the function $n(c)$.

With the above cases being variable transformations, it is without loss of generality to normalize an individual's outside option to $\underline{U}_0 = 0$. □

Proof (Proof of Proposition 4.2). Recall the utility function $U_{1,t}(\theta) = \theta a + vn^e_t$, where $n^e_t = n_{t-1}$ and the initial participation level $n_0 \in [0, 1]$. Recall further that with an outside option $\underline{U}_{0,t} = 0$, the critical type threshold at time t denotes $\hat{\theta}_t = (c - vn_{t-1})/a$.

At $t = 1$, the number of participants at a protest denotes

$$n_1(c, n_0) = \begin{cases} 0 & \text{if } n_0 < \frac{c-a}{v}\,, \\ 1 - \frac{c - vn_0}{a} & \text{if } \frac{c-a}{v} \leq n_0 \leq \frac{c}{v}\,, \\ 1 & \text{if } n_0 > \frac{c}{v}\,, \end{cases} \tag{4.25}$$

where we can readily see that $n_1(c, n_0) \geq n_0$ if $n_0 \geq (c - a)/(v - a)$ and that this property holds for any time $t \geq 1$, i.e., $n_t(c, n_0) \geq n_{t-1}(c, n_0)$ if $n_{t-1} \geq (c - a)/(v - a)$.

Lemma 4.1. *For any time $t \geq 1$, the number of participants weakly increases if n_0 is sufficiently large, i.e.,*

$$n_t(c, n_0) \geq n_{t-1}(c, n_0) \quad \textit{if} \quad n_0 \geq \frac{c - a}{v - a}\,, \tag{4.26}$$

Considering the corner intervals in (4.25), Lemma 4.1 implies that if the initial participation satisfies $n_0 \geq c/v$ (if $n_0 < (c - a)/v$), the number of participants remains at the full-participation (no-participation) outcome with $n_t = 1$ ($n_t = 0$) for any time $t \geq 1$.

We can now focus on the interior where $(c - a)/v \leq n_0 \leq c/v$. To solve the first-order linear difference equation, $n_t = 1 - \hat{\theta}_t$, explicitly, we begin by expressing the number of participants at a protest at time $t = 1$ as

$$n_1(c, n_0) = 1 - \frac{c}{a} + n_0 \frac{v}{a}\,. \tag{4.27}$$

Next, consider the number of protest participants at time $t = 2$. By writing n_2 in terms of n_0 through the substitution of n_1, we obtain

$$\begin{aligned} n_2(c, n_1) &= 1 - \frac{c - vn_1}{a} \\ \Rightarrow n_2(c, n_0) &= 1 - \frac{c}{a} + \frac{v}{a}\left(1 - \frac{c - vn_0}{a}\right) \\ \Rightarrow n_2(c, n_0) &= \left(1 - \frac{c}{a}\right)\left(1 + \frac{v}{a}\right) + n_0 \left(\frac{v}{a}\right)^2\,. \end{aligned} \tag{4.28}$$

Analogously, the number of protest participants at time $t = 3$ is given by

$$\begin{aligned}
n_3(c, n_2) &= 1 - \frac{c - vn_2}{a} \\
\Rightarrow n_3(c, n_0) &= 1 - \frac{c}{a} + \frac{v}{a}\left[\left(1 - \frac{c}{a}\right)\left(1 + \frac{v}{a}\right) + n_0\left(\frac{v}{a}\right)^2\right] \\
\Rightarrow n_3(c, n_0) &= \left(1 - \frac{c}{a}\right)\left(1 + \frac{v}{a} + \left(\frac{v}{a}\right)^2\right) + n_0\left(\frac{v}{a}\right)^3
\end{aligned} \tag{4.29}$$

and more generally, for any subsequent time $t = k$, we have

$$\begin{aligned}
n_k(c, n_{k-1}) &= 1 - \frac{c - vn_{k-1}}{a} \\
\Rightarrow n_k(c, n_0) &= \left(1 - \frac{c}{a}\right)\left(1 + \frac{v}{a} + \left(\frac{v}{a}\right)^2 + ... + \left(\frac{v}{a}\right)^{k-1}\right) + n_0\left(\frac{v}{a}\right)^k .
\end{aligned} \tag{4.30}$$

Thus, the number of participants at time $t \geq 1$ can be written as

$$n_t(c, n_0) = n_0\left(\frac{v}{a}\right)^t + \left(1 - \frac{c}{a}\right)\sum_{i=0}^{t-1}\left(\frac{v}{a}\right)^i , \tag{4.31}$$

and the change in the number of participants per period, for ease of expression, is denoted by $\Delta_t = \left(\frac{v}{a}\right)^{t-1}(n_1 - n_0)$.

(i) Under weak network effects ($v < a$), Equation (4.31) constitutes a geometric series in the limit as $t \to \infty$, converging to its supremum with $\lim_{t\to\infty} n_t(c, n_0) = (a - c)/(a - v)$.

(ii) Under strong network effects ($v > a$), the change in the number of participants Δ_t implies that in the limiting case in t, the participation level n_t converges to a no- or full-participation outcome. From Lemma 4.1, it must follow that $\lim_{t\to\infty} n_t(c, n_0) = 0$ if $n_0 < (c - a)/(v - a)$ and $\lim_{t\to\infty} n_t(c, n_0) = 1$ if $n_0 > (c - a)/(v - a)$, which finishes the proof. $\square$

4.A.2 Identifying political and protest clusters

Identifying political clusters

The individuals are clustered according to the partitioning around medoids (PAM) algorithm by Kaufman and Rousseeuw (2009). The authors developed the K-medoids algorithm, which is more robust to outliers than the more common K-Means algorithm (Kassambara, 2017). A medoid is an observation that is representative of a whole cluster, and all other observations sort themselves around k-medoids. The algorithm determines the choice of medoids; the clustering algorithm works as follows (Kaufman and Rousseeuw, 2009).

1. Randomly select k individuals from the sample. These are the first cluster-medoids.
2. Calculate the Euclidean dissimilarity matrix for all individuals based on the variables "Political Attitudes Towards..." from Table 4.1.
3. Assign each individual to the cluster-medoid to which it is most similar.
4. Within each cluster, check if there is an alternative medoid that minimizes the average dissimilarity coefficient of the cluster. If this is the case, this alternative medoid is the new cluster-medoid.
5. In case one or more medoids change, return to (3); otherwise, end the algorithm.

Since the ANES database contains more than 6000 individuals, the PAM algorithm is computationally too expensive to estimate. Therefore, we rely on the Clustering Large Applications (CLARA) extension to the k-medoids algorithm, which is also based on Kaufman and Rousseeuw (2009). The CLARA algorithm splits the sample into multiple subsets – 50 in our case. For each subgroup, the k medoids are determined using the PAM algorithm. Thus, we end up with 50 sets of potential medoid combinations. All sets are assessed using the full dataset. The remaining observations are assigned to whichever medoid is closest to them, separately for each set of medoids. Then, the average dissimilarity between the observations and their respective medoid is calculated. The set of medoids that minimizes this number is selected.

Applying the CLARA algorithm based on the seven political attitudes, we identify three clusters as illustrated in Figure 3. Individuals in cluster 1 have, on average, relatively high opinions about conservatives, the police, and Christian fundamentalists. This cluster

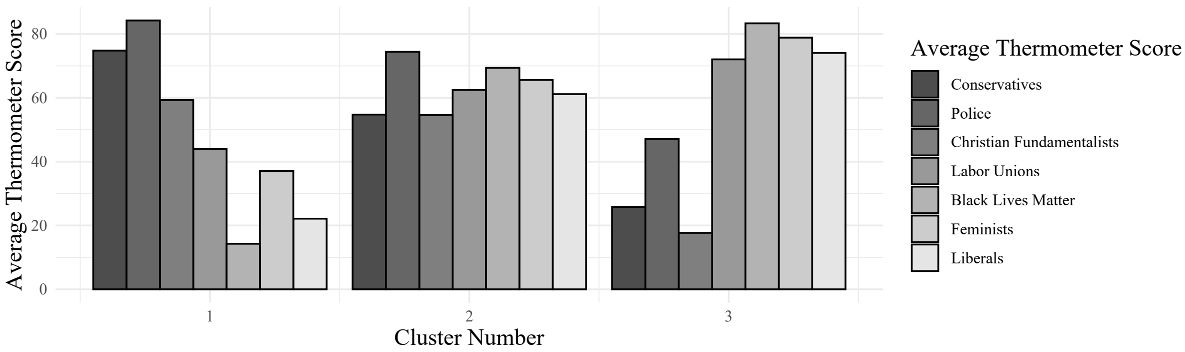


Figure 4.3: Average Thermometer Scores for Political Clusters. Cluster 1 includes 2,455 individuals, cluster 2 includes 2,520 individuals, and cluster 3 includes 1,897 individuals.

thus seems to include individuals who align themselves with conservative ideologies in the US. The average scores for cluster 3 are roughly the opposite. Individuals in cluster 3 have, on average, relatively high opinions about liberals, feminists, BLM protesters, and labor unions. Thus, this cluster seems to include left-leaning individuals. Cluster 2 seems to collect all individuals who do not fit into both political camps. Therefore, this cluster contains individuals who deviate atypically in their opinions from left- or right-leaning individuals or who score each group on the variables equally well. We call this cluster the "moderates", as their attitudes toward the different groups are less extreme on average.

These cluster interpretations are helpful for assessing heterogeneity in standalone and network benefits. Assigning individuals to different clusters enables separate assessment of these effects. This approach aims to shed light on the drivers of left-wing versus right-wing protests and whether protest motivations differ between left- and right-leaning individuals. We re-estimate the model separately for each of the three clusters, using the same specification as in Equation (4.15).

Identifying protest clusters

The first step in building protest clusters involves assessing participants' awareness of political issues. The survey includes a question that asks participants to identify what they perceive as the most pressing problems facing the US. Respondents could name multiple issues. Based on respondents' answers, we identified four topics that were mentioned most frequently: healthcare (42.7%), race relations (19.3%), environment (6.6%), and immigration (4.3%).

Relying solely on these categories may be problematic because some topics do not directly translate to policy implications. For example, individuals skeptical of BLM protests can name race relations as the biggest problem in the US today, but pro-BLM individuals may also name the same problem. Both groups, however, derive very different policy conclusions from the same issue and are, therefore, very unlikely to join the same protest or to belong to the same protest movement's group of potential protesters. Thus, we combine the naming of the problem with the thermometer scores about political groups. From the group that judges race relations to be one of the most significant problems in the US, only those who indicated a thermometer score of 50 or higher toward the group "BLM protesters" remain in the group. All other individuals are excluded. Thus, this group represents people who see race relations as one of the most pressing issues in the country, while being in line with BLM demands. The individuals are thereby grouped according to their problem awareness and their attitudes toward these protest movements. Based on individuals' survey responses, we thus assign 396 individuals to the group "Environment", 877 individuals to the group "Black Lives Matter", 260 individuals to the group "Immigration", and 2580 individuals to the group "Healthcare". We consider an individual who is a member of one of these groups to be a potential protester for the corresponding issue.

We estimate the model specified in Equation (4.15) separately for four subsamples, each corresponding to a distinct protest movement. This second extension enables us to examine whether standalone and network benefits vary across movements. A key advantage of this approach is that individuals may belong to multiple groups, or none at all, depending on their political attitudes. This allows us to define potential protesters with precision and to quantify the magnitude of both standalone and network benefits within this subset. In a subsequent analysis, we relate the ranking of these estimated effects to the observed levels of participation across the four protest movements.

4.A.3 Variables and robustness appendix

This section provides complementary tables with additional details that were omitted from the main text for brevity. Specifically, Table 4.6 documents all variables and survey questions from the ANES 2020, Table 4.7 reports the corresponding OLS estimation results, Table 4.8 presents the spatial regression results for the political clusters, and Table 4.9 provides robustness checks for the 2016 baseline specification.

Table 4.6: Variable description ANES 2020.

Variable	Question
Protest Variable	
Protest	During the past 12 months, have you joined in a protest march, rally, or demonstration, or have you not done this in the past 12 months? ANSWER: 1: yes; 0: no
Socio-Demo. Characteristics	
Age (years)	What is the month, day, and year of your birth? ANSWER: Range 18–80, 80 indicating 80 or older
Education	What is the highest level of school you have completed or the highest degree you have received? ANSWER: 1: Less than high school credential; 2: High school graduate; 3: Some college, no degree; 4: Associate degree - vocational; 5: Associate degree - academic; 6: Bachelor's degree; 7: Master's degree; 8: Professional school/Doctoral degree
Income	Please mark the answer that includes the income of all members of your family during the past 12 months before taxes. ANSWER: Range 1–22, indicating income groups from 0-10k USD to >250k USD
Black (=1)	What do you consider your main ethnic group or nationality group? *Recoded to dummy variable indicating 1 if respondent declares to be Black, non-Hispanic.* ANSWER: 1. yes; 0. no
Male (=1)	What is your sex? 1. Male; 0. Female
Health status	Would you say that in general your health is [excellent, very good, good, fair, or poor/poor, fair, good, very good, or excellent]? ANSWER: 0: poor; 1: fair; 2: good; 3: very good; 4: excellent (recoded)
Married (=1)	Are you now married, widowed, divorced, separated or never married? *Recoded to dummy variable indicating 1 if respondent declares to be married.* ANSWER: 1. yes; 0. no.
Number of Children	How many children or teenagers age 0 to 17 live in this household? ANSWER: 0. None; 1: one; 2: two; 3: three; 4: four or more
Personal Attitudes	
Left-right-scale	Where would you place yourself on a scale from 0 to 10 where 0 means the left and 10 means the right? ANSWER: Range 0–10, low values indicating left and high values right.
Deviation Left-right-scale	Absolute number of 5 - answer to left-right-scale question. ANSWER: Range 0–5, where high values indicate extremeness of political attitudes.
Importance Religion	How important is religion in your life? ANSWER: 1: Extremely; 2: Very; 3: Moderately; 4: A little; 5: Not at all
Trust in others	Generally speaking, how often can you trust other people? ANSWER: 0: never; 1: some of the time; 2: about half the time; 3: most of the time; 4: Always
Longitude	Longitude of the respondent's state
Latitude	Latitude of the respondent's state
Associational Participation	
Volunteering	Were you able to devote any time to volunteer work in the past 12 months or did you not do so? ANSWER: 1: yes; 0: no
Community Participation	During the past 12 months, did you attend a meeting about an issue facing your local community or schools? ANSWER: 1: yes; 0: no
Church Attendance	Thinking about your life these days, do you ever attend religious services, apart from occasional weddings, baptisms or funerals? ANSWER: 1: yes; 0: no
Political Attitudes Towards...	
Labor Unions	Feelings towards some of [these groups]. *Scale 0–100.*
Conservatives	Feelings towards some of [these groups]. *Scale 0–100.*
Liberals	Feelings towards some of [these groups]. *Scale 0–100.*
Feminists	Feelings towards some of [these groups]. *Scale 0–100.*
Christian Fundamentalists	Feelings towards some of [these groups]. *Scale 0–100.*
Police	Feelings towards some of [these groups]. *Scale 0–100.*
Black Lives Matter	Feelings towards some of [these groups]. *Scale 0–100.*

Note: Variable Description. Questions from the American National Election Survey 2020.

Table 4.7: OLS estimation results

	Model (1) Full sample	Model (2) Right-wing	Model (3) Moderates	Model (4) Left-wing
Male (=1)	−0.001	0.002	0.002	0.014
	(0.007)	(0.010)	(0.022)	(0.009)
Age (years)	−0.006***	−0.004**	−0.006	−0.003
	(0.002)	(0.002)	(0.004)	(0.002)
Age (years)^2	0.00004***	0.00003	0.00001	0.00002
	(0.00001)	(0.00002)	(0.00004)	(0.00002)
Black (=1)	0.017	0.030*	0.022	0.023
	(0.014)	(0.016)	(0.041)	(0.035)
Income	−0.0004	−0.001	0.001	−0.0004
	(0.001)	(0.001)	(0.002)	(0.001)
Education	0.005**	0.002	0.010	−0.001
	(0.002)	(0.003)	(0.006)	(0.003)
Left-right-scale	−0.020***	−0.009***	−0.021***	0.002
	(0.002)	(0.002)	(0.005)	(0.004)
Deviation Left-right-scale	0.029***	0.008**	0.019**	0.011***
	(0.002)	(0.003)	(0.007)	(0.004)
Health Status	0.001	0.009*	−0.001	−0.005
	(0.004)	(0.005)	(0.011)	(0.005)
Married (=1)	−0.027***	−0.014	−0.065**	−0.022**
	(0.008)	(0.011)	(0.026)	(0.011)
Number of Children	−0.018***	−0.007	−0.035***	−0.006
	(0.004)	(0.005)	(0.012)	(0.004)
Importance Religion	0.010***	0.010**	0.005	0.001
	(0.003)	(0.004)	(0.009)	(0.004)
Trust in others	0.006	0.003	0.010	−0.004
	(0.004)	(0.005)	(0.013)	(0.006)
Volunteering	0.058***	0.060***	0.084***	0.042***
	(0.010)	(0.014)	(0.026)	(0.013)
Community Participation	0.111***	0.067***	0.223***	0.042**
	(0.012)	(0.017)	(0.030)	(0.017)
Church Attendance	0.013	0.022*	0.026	0.011
	(0.009)	(0.012)	(0.030)	(0.011)
North East	−0.016	0.003	−0.062*	−0.012
	(0.013)	(0.016)	(0.033)	(0.017)
Mid West	−0.034***	−0.011	−0.083***	−0.021
	(0.011)	(0.014)	(0.031)	(0.014)
South	−0.036***	−0.012	−0.099***	−0.023*
	(0.010)	(0.013)	(0.029)	(0.013)
Constant	0.290***	0.168***	0.415***	0.119**
	(0.044)	(0.058)	(0.124)	(0.059)
Observations	6103	2558	1367	2178
R^2	0.128	0.058	0.165	0.033
Adjusted R^2	0.126	0.051	0.153	0.024

Heteroskedasticity robust spatial SE in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: OLS regression results for the year 2020 as a linear probability model. Model (1) includes the full sample, while Models (2) to (4) differentiate between different political clusters. All variables, their corresponding survey questions, and the authors' recoding procedures are documented in Table 4.6.

Table 4.8: Political cluster spatial regression results

Cluster	Right-wing	Moderates	Left-wing
Intercept	0.085*	0.137	0.074*
	(0.051)	(0.108)	(0.043)
Male (=1)	0.002	−0.001	0.009
	(0.009)	(0.022)	(0.008)
Age (years)	−0.003	0.000	−0.002
	(0.002)	(0.004)	(0.002)
Age (years)^2	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Black (=1)	0.027*	0.027	0.022
	(0.015)	(0.038)	(0.032)
Income	-0.001	0.002	0.000
	(0.001)	(0.001)	(0.001)
Education	0.000	0.004	−0.002
	(0.002)	(0.004)	(0.002)
Left-right-scale	−0.006***	−0.009**	−0.001
	(0.002)	(0.004)	(0.003)
Deviation Left-right-scale	0.006**	0.015***	0.006**
	(0.003)	(0.005)	(0.003)
Health	0.009**	−0.002	−0.006
	(0.005)	(0.011)	(0.005)
Married (=1)	−0.012	−0.060**	−0.018*
	(0.011)	(0.024)	(0.010)
Number of children	−0.007	−0.032***	−0.008*
	(0.005)	(0.011)	(0.004)
Importance Religion	0.006*	−0.001	0.001
	(0.003)	(0.006)	(0.002)
Trust in others	0.003	0.008	0.000
	(0.003)	(0.008)	(0.004)
Volunteering	0.029***	0.033*	0.012
	(0.011)	(0.019)	(0.007)
Community Participation	0.026	0.079**	0.006
	(0.016)	(0.034)	(0.010)
Church Attendance	0.011	0.030*	0.000
	(0.008)	(0.017)	(0.006)
North East	0.007	−0.031	−0.005
	(0.012)	(0.023)	(0.011)
Mid West	−0.005	−0.055**	−0.009
	(0.010)	(0.024)	(0.009)
South	−0.009	−0.061***	−0.016*
	(0.010)	(0.022)	(0.009)
Spatial Lag Y: λ	0.653***	0.693***	1.102***
	(0.193)	(0.139)	(0.163)
Spatial Lag ϵ: ρ	−0.628***	−0.831***	−0.900***
	(0.182)	(0.187)	(0.177)

Heteroskedasticity robust spatial SE in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Political cluster full SARAR regression results are based on Equation 4.15. Each column corresponds to a distinct political cluster. Results are obtained for the specification that includes the 15 nearest neighbors in the weighting matrix. All variables, their corresponding survey questions, and the authors' recoding procedures are documented in Table 4.6.

Table 4.9: Robustness baseline 2016

	Model (1) Neighbors: 10	Model (2) Neighbors: 15	Model (3) Neighbors: 20
Intercept	0.023	0.032	0.031
	(0.027)	(0.030)	(0.030)
Male (=1)	−0.003	−0.002	−0.003
	(0.006)	(0.006)	(0.006)
Age (years)	−0.001	−0.001	−0.001
	(0.001)	(0.001)	(0.001)
Age (years)^2	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Black (=1)	0.021*	0.026**	0.026**
	(0.012)	(0.013)	(0.013)
Income	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Education	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)
Left-right-scale	−0.003*	−0.004**	−0.004**
	(0.002)	(0.002)	(0.002)
Deviation Left-right-scale	0.005***	0.007***	0.007***
	(0.002)	(0.002)	(0.002)
Health	0.004	0.003	0.003
	(0.003)	(0.003)	(0.003)
Married (=1)	0.002	0.002	0.001
	(0.007)	(0.007)	(0.007)
Number of children	−0.006*	−0.006*	−0.006*
	(0.003)	(0.003)	(0.003)
Importance Religion	0.002	0.002	0.002
	(0.002)	(0.002)	(0.002)
Trust in others	0.000	0.000	0.001
	(0.002)	(0.002)	(0.002)
Volunteering	−0.007*	−0.006	−0.006
	(0.004)	(0.004)	(0.004)
Community Participation	0.004	0.004	0.003
	(0.007)	(0.008)	(0.009)
Church Attendance	0.007*	0.006	0.006
	(0.004)	(0.004)	(0.004)
North East	0.006	0.006	0.005
	(0.007)	(0.007)	(0.008)
Mid West	0.005	0.005	0.006
	(0.006)	(0.006)	(0.006)
South	−0.002	−0.003	−0.003
	(0.005)	(0.005)	(0.005)
Spatial Lag Y: λ	1.161***	1.136***	1.186***
	(0.152)	(0.182)	(0.198)
Spatial Lag ϵ: ρ	−0.900***	−0.900***	−0.900***
	(0.173)	(0.231)	(0.266)
Observations	3257	3257	3257

Heteroskedasticity robust spatial SE in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Robustness for 2016 for the baseline SARAR regression results based on Equation (14). The full ANES sample is used, with a much smaller sample size for 2016 compared to 2020. Columns differ by the number of nearest neighbors used in the spatial weighting matrix (10, 15, and 20, respectively). All variables, their corresponding survey questions, and the authors' recoding procedures are documented in Table 4.6.

Chapter 5

Why is Europe Lagging Behind in High Tech Sectors? The Role of Institutional and Regulatory Quality[26]

Abstract

This paper investigates the relationship between institutional and regulatory quality, and high-tech sector investment. Using data from 25 European Union (EU) countries from 2004 to 2019 (extended to 2023 for artificial intelligence-specific analyses), the study examines how institutional governance, labour market regulations, and business regulations influence investments in innovative, high-tech, and artificial intelligence-intensive sectors. The findings reveal that better institutional quality and less burdensome regulations are associated with higher investment shares in innovative, high-tech, and artificial intelligence industries. Raising EU countries' institutional and regulatory quality to the level of the current EU frontier could raise the share of investment in high-technology sectors by as much as 50%, hence notably narrowing the existent EU-US investment gap. These results highlight the importance of effective governance and efficient regulations in fostering investment, innovation, and therefore long-term productivity growth.

Keywords: *Institutional quality, regulatory frameworks, risky technology, innovation, artificial intelligence, investment*

[26]This chapter is joint work with Paloma Lopez-Garcia, Daphne Momferatou and Ralph Setzer. I conducted the empirical analysis. All authors contributed equally to the conceptual development, interpretation of the results, and drafting of the paper. A version of this chapter has been published in the ECB Working Paper Series as ECB Working Paper No. 3185 (Bothner et al., 2026). All views expressed are those of the authors and do not necessarily reflect the views of the ECB.

5.1 Introduction

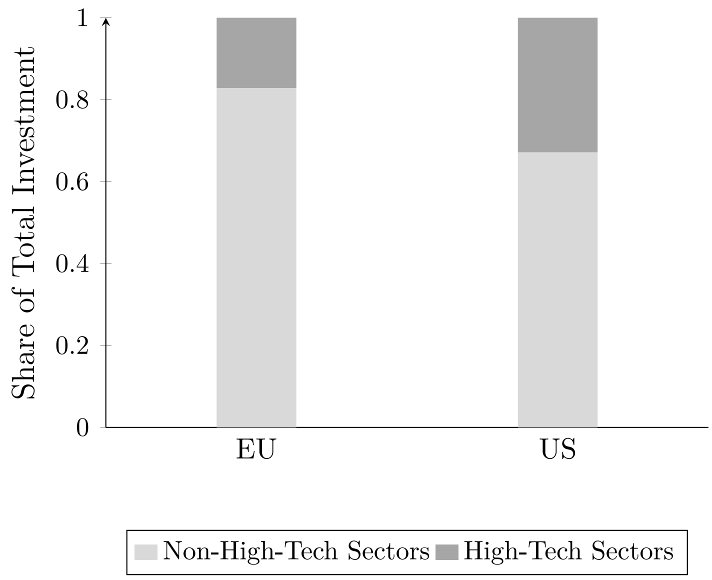
Since the mid-1990s, European Union (EU) countries have experienced slower productivity growth than the United States (US). This productivity growth gap is closely linked to an innovation gap (Draghi, 2024). Notably, research and development (R&D) expenditures in Europe, particularly from the private sector, lag behind those in the US. Furthermore, there is a stark difference in the composition of investment and R&D expenditures between the two regions. The US prioritises innovative high-tech sectors like ICT, AI, cloud computing or biotech, which boosts productivity across the economy. By contrast, Europe continues to concentrate on mature mid-tech sectors, which limits the potential for positive productivity spillovers across the economy (Fuest et al., 2024).

Indeed, Figure 5.1 illustrates that in 2021 approximately 17 % of market sector gross fixed capital formation in EU countries was concentrated in sectors classified as high-tech by Eurostat. The corresponding share in the United States was nearly double, reaching 33 %, in the same year.

These differences in sectoral composition significantly contribute to the divergence in productivity growth between Europe and the US. Data from EU KLEMS illustrate that the ICT sector alone explains about 48 % of the average annual hourly productivity growth gap between the EU and the US from 2000–2019 (see Appendix Table 5.7). This is the result of both the ICT sector's higher productivity growth and its greater size in the US than in the EU. These insights raise an important question: Why are EU countries lagging behind in disruptively innovative high-tech sectors?

One potential explanation lies in Europe's institutional and regulatory environment. High-tech sectors are inherently risky, characterized by trial-and-error and high rates of project and firm failures (Coatanlem and Coste, 2024, 2025). The expected profitability of investment in such sectors depends crucially on the costs of failure and restructuring (Bartelsman et al., 2016; Coatanlem and Coste, 2025; Saint-Paul, 2002). Institutions and regulations which influence these costs play therefore a critical role in determining investment in innovative high-tech sectors, more so than in traditional sectors. Coatanlem and Coste (2024) argue that restrictive employment protection legislation, which increases restructuring costs for European firms, can explain the smaller size of the high-tech sector in Europe and the relative scarcity of breakthrough innovations compared to the US. This

Figure 5.1: High-tech vs non-high-tech sector investment share of market sector investment in the EU and in the US in 2021.



Note: Authors' own calculations using EU KLEMS data on gross fixed capital formation at 2-digit NACE industry level. Market sectors are all NACE sectors except NACE sectors O (public administration, defence and compulsory social security), P (education), Q (human health and social work activities) and T (activities of households as employers). We further exclude NACE sectors A (agriculture, forestry and fishing) and L (real estate activities) from the definition of market sectors. For the purpose of this figure, high-tech sectors include NACE sectors C20–C21 (chemicals and pharmaceuticals), C26 (Manufacturing of computers) and J (Media and information and communication technology). The EU high-tech sector investment share is based on the 17 EU countries for which data on gross fixed capital formation is available for these three NACE codes in the EU KLEMS dataset. These countries are Austria, Belgium, Bulgaria, Czechia, Germany, Denmark, Finland, France, Greece, Hungary, Italy, Luxembourg, Latvia, Netherlands, Portugal, Romania and Slovakia.

is in line with the model by Saint-Paul (2002) which suggests that countries with high firing costs will specialize in secondary innovation, i.e. innovation which improves existing products, whereas countries with low firing costs will specialize in primary innovation which introduces new products. Similarly, Bartelsman et al. (2016) develop a model in which high-risk activity depends on firing costs and they provide empirical evidence that risky sectors are larger in countries with less restrictive labour market regulation.

This paper seeks to contribute to the existing literature by empirically examining how investment in innovative sectors, relative to investment in less innovative sectors, responds to measures of institutional and regulatory quality in EU countries over the period 2004 – 2019 (until 2023 for some AI-specific analyses). In particular, we explore the relative effect of the quality of institutions, summarised by the Institutional Delivery Index (which is composed of four Worldwide Governance Indicators by the World Bank), labour market regulations, as captured by the OECD Employment Protection Legislation (EPL) index, and business regulations, proxied by the World Bank's Starting a Business score. While the comparison with the US in the introduction serves to benchmark and illustrate the magnitude of the EU's investment gap in innovative sectors, the empirical analysis in this paper focuses on determinants of relative sectoral investment allocation within the EU, in innovative versus non-innovative sectors.

Our results indicate that better institutional governance and less burdensome regulations are associated with higher investment shares of high-tech sectors, sectors with high patent activity and AI-intensive sectors. Specifically, we estimate that raising all EU countries' institutional or regulatory delivery to the EU frontier could increase the investment share of high-tech sectors by up to 7.9 percentage points, equivalent to about 50% of the 2021 investment share of high-tech sectors. This would then close the respective investment gap with the US by almost 50%. For AI intensive sectors, the corresponding increase in the investment share could reach over 7 percentage points.

These findings have important policy implications. First, our results highlight the critical role of the institutional and regulatory frameworks in promoting investment in high-tech and innovative sectors. Institutional reforms that strengthen governance and reduce regulatory burden can help create a more favourable environment for innovation and investment in high-tech activities.

The remainder of the paper is structured as follows: Section 5.2 reviews the relevant literature, situating our analysis within the existing research. Section 5.3 outlines the methodology applied, details the different approaches used for the classification of sectors according to their innovativeness, and presents our measures of institutional and regulatory quality. Section 5.4 presents the results of the estimations using an instrumental variable (IV) approach to address potential endogeneity concerns, different sector classifications and different institutional indicators. It also includes simple back-of-the-envelope calculations to gauge the economic implications of these estimates. Section 5.5 assesses the robustness of the results to alternative control variables, dependent variables, and country samples. Section 5.6 concludes by summarising the key findings and discussing policy implications.

5.2 Literature

Institutional frameworks have been extensively discussed as an important determinant of economic growth (Rodrik et al., 2004; Acemoglu et al., 2005). North (1991) emphasizes the role of institutions in decreasing uncertainty and thus promoting investment and innovation which leads to growth. Institutional frameworks thus alter the incentive structure of individuals, encouraging or discouraging different kinds of behavior.

One important determinant of growth affected by institutional quality is innovation. Several studies have examined how institutions promote or hinder Schumpeterian 'creative destruction'. A prominent strand of literature focuses on institutions which regulate competition and market entry of firms. There are two lines of argument regarding the effect of competition on innovation. One argument focuses on the reduced monopoly rents an innovator can expect with increased competition. If regulatory entry barriers are low, successful innovation will be succeeded by imitators, decreasing the innovator's rents. This decreases the incentive to innovate (Aghion and Howitt, 1992). On the other hand, a competitive environment may induce a firm to be innovative in order to gain an advantage over its competitors. This would imply a positive effect of competition on innovative activity (Aghion et al., 2001). Aghion et al. (2009) examine how firm entry affects the innovation incentives of incumbents. They use firm-level data from the UK and exploit policy reforms at the UK and at the EU level which affect entry of foreign firms. Their findings suggest that foreign firm entry is related to increased innovation incentives in industries which are close to the technology frontier but not in industries which are less technologically advanced.

One of the most important institutions governing the functioning of the labour market is the Employment Protection Legislation (EPL) and has therefore received particular attention in the literature. The relationship between EPL and innovation is complex and remains widely debated. On the one hand, stringent EPL has been argued to foster innovation by promoting job security and reducing the risk of opportunistic behavior by employers. On the other hand, EPL can increase labor market rigidity, increase operational costs, and reduce firms' incentives to engage in risky investments in R&D.

On the positive side, Acharya et al. (2013, 2014) argue that dismissal laws—by limiting employers' ability to arbitrarily fire employees or expropriate the returns from innovative activities—build trust and collaboration between employers and employees. This trust, they claim, encourages employees to engage in innovative activities. Their empirical evidence, based on country-level changes in dismissal laws in the United States, the United Kingdom, France, and Germany, suggests that stringent dismissal laws particularly benefit innovation-intensive industries. On the negative side, however, EPL can impose costs that dampen innovation. Riphahn (2004) shows that protections against layoffs during probationary periods reduce the productivity of new hires, as they weaken incentives for performance, thereby undermining innovation. Bradley et al. (2017) find that unionisation which often results in strong termination protection, creates misaligned incentives and hinders corporate innovation by discouraging R&D investment, fostering employee shirking, and reducing wage inequality. Furthermore, stricter EPL can limit firms' ability to adjust their workforce in response to economic shifts or evolving technological demands, increasing operational rigidity and costs. This disincentivizes firms from pursuing high-risk, high-reward innovation projects. Francis et al. (2018) provide firm-level evidence that enhanced labour protections negatively impact innovation through mechanisms such as inventor shirking and distortions in labor mobility. They also find that the negative impact is more pronounced in firms that heavily rely on external financing and firms that have high R&D intensity. Saint-Paul (2002) highlights the importance of examining the differential impact of EPL across sectors, arguing that EPL may benefit industries with stable employment structures while hindering innovation in sectors that rely on high labor mobility and frequent workforce reallocation. Similarly, Bozkaya and Kerr (2014) use firm-level data to demonstrate that stringent EPL in European countries reduces venture capital investment, a critical driver of innovation, particularly in high-volatility sectors (energy, biotech, computers). Bartelsman et al. (2016) further explore the differential effects of EPL across sectors, developing a model that shows how firing costs reduce the sorting of firms into sectors with risky technology. They corroborate their findings with empirical evidence for OECD countries for the 1995–2005 period, using a diff-in-diff approach.

Our study is closely related to the literature emphasizing the differential sectoral impacts of regulations, like EPL, as they might be better suited for certain types of activities than for others. Specifically, we investigate how institutional and regulatory burdens which could be related to the cost of adjustments in the face of failure impact relative investment allocation across sectors. This research question is linked to the recent public policy discussion about structural factors underlying the productivity growth gap between Europe and the US (Draghi, 2024). Several authors attribute this gap partly to differences in the sectoral allocation of economic activity. Fuest et al. (2024) point out that in the US, a higher share of R&D expenditures occurs in innovative high-tech industries than in the EU. This, they argue, leads to disruptive innovations which boost productivity taking place in US high-tech sectors. In contrast, innovation in EU countries is incremental in nature and takes place in mature mid-tech industries, a phenomenon that is referred to as the 'middle-technology trap' (Dietrich et al., 2024; Fuest et al., 2024).

We contribute to the literature by examining how institutional and regulatory frameworks influence the decision to invest in high-technology or disruptive sectors, rather than in mature technologies across sectors in EU countries. While our empirical approach is close to that of Bartelsman et al. (2016), we build upon their work by using a broader range of institutional and regulatory indicators and of innovativeness classifications. Specifically, we use three different institutional indicators and three distinct classification methods for high-tech sectors to inquire how various institutional dimensions affect investment in innovative sectors, relative to investment in other sectors. Moreover, we apply an IV approach, instrumenting institutions using legal origins, to address potential issues of endogeneity. By looking at differential impacts on innovative sectors in particular, we intend to support an ongoing policy discussion with empirical evidence.

5.3 Data and Methodology

5.3.1 Empirical methodology

We estimate the following model:

$$Inv_{cit} = \alpha + \beta IF_{ct} + \gamma IF_{ct} \times Innovative_i + \theta X_{cit} + \delta_i + \lambda_t + \epsilon_{cit}. \tag{5.1}$$

Models of similar form have been used in previous studies to explore the differential impacts of country-specific structural factors across sectors (Rajan and Zingales, 1996; Bartelsman et al., 2016).

Here, Inv_{cit} is the gross fixed capital formation in sector i in country c in year t as a percentage share of total market sector gross fixed capital formation in country c in year t.

We use data on gross fixed capital formation in current prices disaggregated at the 2-digit NACE sector level.

Two potential sources for data on gross fixed capital formation are the 2025 release of the EU KLEMS dataset (Bontadini et al., 2023) and Eurostat. The two datasets differ slightly in their coverage of countries, sectors and years. While the EU KLEMS data have longer reporting lags and are less disaggregated for some non-manufacturing sectors, we use the gross fixed capital formation data from EU KLEMS for most of our analysis. This choice is motivated by the fact that Eurostat investment data in manufacturing is available at a disaggregated sector level for only 14 EU countries.

The sample includes observations on up to 28 market sectors in 25 EU countries[27] over the time period from 2004 until 2019. As specified further below, some of the AI-specific analyses are extended to 2023. We consider market sectors as defined by the EU KLEMS, i.e. all sectors excluding real estate activities, public administration and defence, compulsory social security, education, human health and social work activities, activities of households as employers and activities of extraterritorial organizations and bodies. We further exclude agriculture, forestry and fishing. Table 5.8 in the Appendix lists all the sectors included in the baseline analysis. The choice of sectors to be included and the levels of sector aggregation are based on data availability in the EU KLEMS dataset.

IF_{ct} is a measure of an institutional factor in country c in year t and $Innovative_i$ is a variable that indicates the innovativeness of sector i which is used to moderate the impact of the institutional variable. We discuss the innovativeness classifications and the institutional indicators in detail in Sections 5.3.2 and 5.3.3, respectively. By estimating the coefficient γ on the interaction term and controlling for sector- and year-fixed effects, we can estimate the differential impact of an increase in the institutional and regulatory quality on the investment share of an innovative sector relative to a non-innovative sector.

Our unit of observation is the country-sector-year cell. The institutional factor IF_{ct} varies both across countries and over time, while $Innovative_i$ varies across sectors and is constant across countries and years. The interaction term $IF_{ct} \times Innovative_i$ thus varies across countries, sectors and years. The parameter which is primarily of interest to us is γ and it is identified by cross-country variation and within-country over-time variation of

[27] Cyprus and Malta are excluded due to limited data availability.

the institutional variable.

The vector X_{cit} includes control variables. Keep in mind that our dependent variable is the within-country share of a sector's investment and our variable of interest – the interaction term $IF_{ct} \times Innovative_i$ – varies at the country-, sector- and year-level. Any omitted variable raising concerns about endogeneity should also exhibit variation at the country- and sector level. That is, since the dependent variable is expressed as a sector's within-country share, variables varying at the country-level may give rise to endogeneity concerns if they affect the investment in sectors classified as innovative differently than investment in other sectors.

In our baseline specification of the model, we control for the value added growth of sector i in country c in year t to account for the sector's cyclical position in a given year. To further address potential omitted variable bias, we conduct robustness checks by including further control variables. First, we account for the possibility that investment in innovative sectors is disproportionately attracted by favourable taxation policies. Second, since institutional quality is correlated with economic development, our estimates on the institutional measures might partially capture the effect of other characteristics of wealthier economies on investment in innovative sectors.

To address these concerns, we re-estimate the models while incorporating additional controls for a country's corporate tax rate and its gross domestic product (GDP) per capita, as well as the interactions between these variables and the respective $Innovative_i$ measures. Historical data on corporate tax rates is obtained from the Tax Foundation (2024). Data on GDP per capita is sourced from the World Development Indicators of the World Bank (2025). Summary statistics on all variables used in the empirical analysis are provided in Table 5.9 in the Appendix.

5.3.2 Classifying Sectors by Innovativeness

We employ three distinct methods to classify the innovativeness of sectors. The first method categorizes sectors as either high-tech or non-high-tech, based on the Eurostat high-tech aggregation by NACE Rev. 2 at 2-digit level. A sector is considered high-tech if it is either classified as a high-technology manufacturing sector or as a high-tech knowledge-intensive service sector. Specifically, the following sectors are classified as high-tech: C21 (Manufacture of basic pharmaceutical products and pharmaceutical preparations), C26 (Manufacture of computer, electronic and optical products), J58–J60 (Publishing activities; motion picture, video and television programme production, sound recording; Programming and broadcasting activities), J61 (Telecommunications) and J62–J63 (Computer programming, consultancy and related activities; Information service activities)[28]. In this first approach, we estimate the model specified in Equation (5.1) using a dummy variable as the moderating innovativeness classification. The dummy takes a value of one if a sector is classified as high-tech, and zero otherwise.

In our second classification approach, we assess sectoral innovativeness on the basis of patenting activity. We rely on patent data from the US to mitigate possible endogeneity issues. Using data on 18,085,696 patent applications from the United States PatentsView database of the U.S. Patent and Trademark Office (2025) we match the International Patent Classification (IPC) codes of the patent applications with 15 NACE codes employing the concordance scheme by Van Looy et al. (2014). For each sector, we calculate the average patent share by dividing the number of patents attributed to its NACE code in a given year by the total number of patent applications which could be matched to any NACE code in the respective year. Next, we calculate the average patent share for each sector over the period from 2004 to 2024 and use this as the innovation measure in Equation (5.1).

As a third classification approach, we focus on the specific case of AI-intensive sectors. To examine the differential impact of institutional variables on investment in sectors with high AI intensity relative to sectors with low AI intensity, we rely on the taxonomy developed by Calvino et al. (2024). Calvino et al. (2024) conduct their classification by ranking 2-digit NACE sectors in OECD countries with respect to four measures capturing different dimensions of AI intensity: (1) AI human capital demand, measured by AI-related online vacancies, (2) AI innovation, measured by AI-related patents, (3) AI use, measured by firms' AI adoption and (4) sectoral AI exposure based on an indicator initially developed by Felten et al. (2021) which measures the extent to which AI can perform the tasks associated with occupations in a sector. Under this taxonomy, a sector

[28] As the EU KLEMS dataset only provides data on the aggregate across the three media sectors J58–J60 we consider the entire aggregate to be high-tech, even though J58 (Publishing activities) is not considered high-tech by the Eurostat categorization. Moreover, NACE code M72 (Scientific research and development) is also classified as high-tech by Eurostat. However, since the sector M (Professional, Scientific and Technical Activities) is not disaggregated any further in the EU KLEMS data, we cannot consider it a high-tech sector in our baseline analysis.

is considered highly AI-intensive if it ranks in the top quartile for at least two of these four indicators and avoids falling into the bottom quartile for any of them[29]. Since the AI-part of our analysis requires a different sector aggregation that is not available in the EU KLEMS dataset, we use data from Eurostat which allows us to extend the sample until 2023.

Note that Calvino et al. (2024) refine the AI exposure measure by considering sector-specific barriers to AI adoption, such as cost, skills, and regulatory constraints. However, since our analysis aims to investigate how institutional quality influences investment in AI-intensive sectors, including regulatory barriers in the measure of AI intensity could introduce endogeneity issues. Regulatory barriers themselves reflect the institutional environment, potentially biasing our results. To address this concern, we reconstruct the AI-intensity classification by excluding the regulatory barriers component from the measure. The composition of sectors identified as highly AI-intensive remains unchanged with this adjustment. Additionally, we perform a supplementary exercise in which we estimate the model in Equation (5.1) using the sectoral rankings of each of the four subindices individually as the variable moderating the impact of institutions on sectoral investment shares. This approach allows us to examine how different aspects of AI intensity affect the link between institutions and investment. In this exercise we rely on the non-adjusted AI exposure measure by Felten et al. (2021) excluding regulatory barriers, as previously explained.

5.3.3 Institutional indicators

We consider three indicators of institutional quality and regulatory stringency in our analysis. The first is the "Institutional Delivery" Index (Helliwell and Huang, 2008; Masuch et al., 2017) which is the simple mean of the four Worldwide Governance Indicators (WGI) "Rule of Law", "Control of Corruption", "Regulatory Quality" and "Government Effectiveness" (Kaufmann et al., 2011; Kaufmann and Kraay, 2024). The Institutional Delivery Index is a broad institutional indicator and reflects how well national institutions

[29]The seven NACE sectors classified as highly AI intensive are C26 (Manufacture of computer, electronic and optical products), J58–J60 (Media), J61 (Telecommunications), J62–J63 (Computer programming, consultancy and related activities; Information service activities), K (Financial and Insurance Activities), M69–M71 (Legal and accounting activities; Activities of head offices; management consultancy activities; Architectural and engineering activities; technical testing and analysis) and M72 (Scientific research and development).

deliver a level playing field for all economic actors, prevent rent extraction and waste of resources, and ensure sound economic incentives to invest, innovate, and provide public goods.

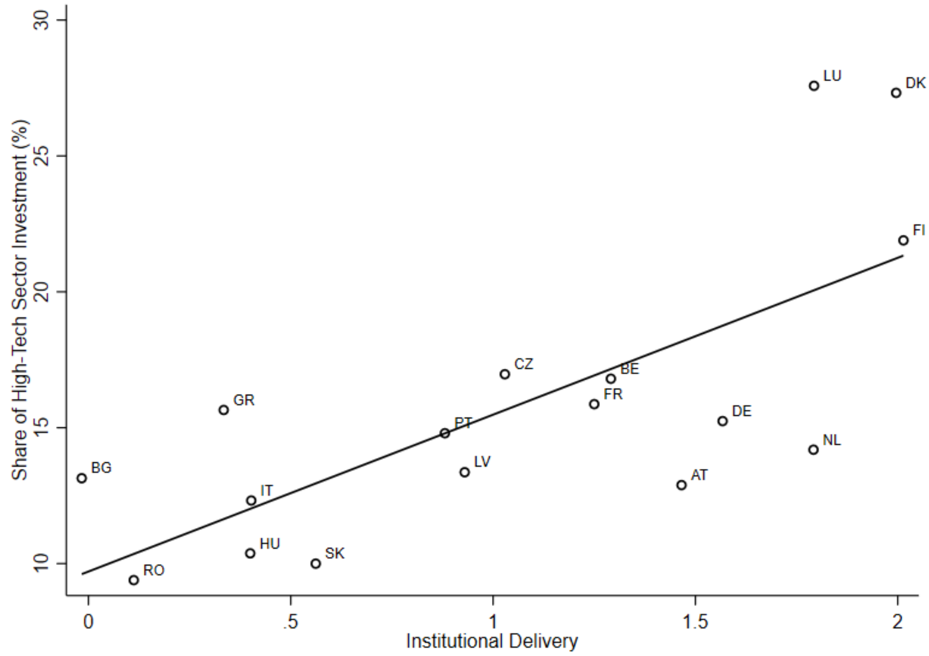
In addition, we look at two more narrowly defined, regulatory quality indicators, which capture more specific aspects of a country's regulatory framework. The first is the EPL index by the OECD (2020)[30]. This indicator measures the strictness of regulations on collective and individual dismissals. The second is the Starting a Business score by the World Bank (2020). This indicator measures the administrative burden placed on entrepreneurs when starting a business.

For a first visual inspection, Figure 5.2 shows the correlation between the institutional delivery index and the share of a country's investment in high-tech sectors for EU countries in 2021. The positive relation lends support to the idea that investment in innovative high-tech sectors is fostered by a high-quality institutional environment. Figures 5.5 and 5.6 in the Appendix present similar relationships showing how institutional quality is positively associated with investment in sectors characterised by high patent activity and in AI-intensive sectors, respectively.

Table 5.10 in the Appendix shows the variations of the institutional indicators between and within countries. Institutional conditions tend to change only slowly over time, hence the institutional delivery index varies much more across countries than within countries over time. However, the EPL index and especially the Starting a Business Score also exhibit substantial variation within a country over time. These patterns of within-country over-time variation are also illustrated by the developments of the EU-wide averages of the indicators over time, shown in Figures 5.7, 5.8, and 5.9 in the Appendix. While the average Institutional Delivery index exhibited little variation over the time period from 2004 until 2019, there are substantial increases discernible for the other two indicators. In our approach, we exploit both sources of variation of the institutional indicators, the variation over countries and the variation over time.

[30]We use the second version of the EPL index as it is the most recent version with data for all years covered by our sample.

Figure 5.2: Institutional Delivery and high-tech sector investment share in 2021.



Note: The shares of high-tech sector investment are calculated using EU KLEMS data on gross fixed capital formation. High-tech sectors include NACE sectors C21 (Pharmaceuticals), C26 (Manufacturing of computers) and J (Media and information and communication technology). The figure includes the 17 EU countries for which data on gross fixed capital formation is available for these three NACE codes in the EU KLEMS dataset.

5.3.4 Instrumenting Institutional Factors

We apply an instrumental variable (IV) approach to address endogeneity concerns and try to identify the causal differential effect of institutions on sectoral investment shares. We instrument the institutional measures using binary legal origin indicators. The idea behind this approach is that countries' legal systems differ with regard to their underlying ideologies and the intentions with which they were set up.

La Porta et al. (1999) classify countries by five legal origins: English common law, French civil law, German civil law, Scandinavian law and Socialist law. Legal origin theory typically argues that the historic intentions of a legal system affect the laws and regulations of countries that have adopted them (La Porta et al., 1997, 1998, 1999). Socialist law is said to be aimed at securing the power of the state and at the extraction of resources from the population. Civil law, with its various sub-classifications, is also intended to maintain the state's power, though with more restraint than Socialist law. English common law, on the other hand, intends to restrain government intervention and emphasizes individual liberties and property rights (La Porta et al., 1999). In particular, English common law and French civil law are often juxtaposed due to their quite different proclivities towards state intervention and regulation (Glaeser and Shleifer, 2002).

The studies by La Porta et al. (1997, 1998, 1999, 2008) provide evidence that legal origin can indeed explain variation in countries' institutional frameworks. Several studies have thus used legal origins as IVs for institutions (Glaeser et al., 2004; Acemoglu and Johnson, 2005; C. R. Williamson and Kerekes, 2011; Masuch et al., 2017). Legal origin has also been shown to explain cross country differences in labour market regulation (Botero et al., 2004) and regulatory entry barriers Djankov et al. (2002).

We define four dummy variables indicating four different legal origins of the countries in our sample. The legal origins are classified in accordance with La Porta et al. (1999)[31]. Since we are concerned about endogeneity of the institutional indicators, two regressors in the model in Equation (5.1) need to be instrumented: IF_{ct} and $IF_{ct} \times Innovative_i$. We thus have two first stage regressions which take the following forms:

[31] The four legal origin indicators are French (Belgium, Spain, France, Greece, Italy, Luxembourg, Netherlands, and Portugal), German (Austria and Germany), Scandinavian (Denmark, Finland, and Sweden) and Socialist (Estonia, Poland, Lithuania, Latvia, Slovenia, Slovak Republic, Bulgaria, Czechia, Croatia, and Hungary). The only country in our sample with its legal origin in English common law is Ireland, hence there is no separate indicator for this legal family.

$$IF_{ct} = \alpha + \beta_1 French_c + \beta_2 German_c + \beta_3 Socialist_c + \beta_4 Scandinavian_c$$
$$+\gamma_1 French_c \times Innovative_i + \gamma_2 German_c \times Innovative_i + \gamma_3 Socialist_c \times Innovative_i$$
$$+\theta X_{cit} + \delta_i + \lambda_t + \epsilon_{ct}. \tag{5.2}$$

$$IF_{ct} \times Innovative_i = \alpha + \beta_1 French_c + \beta_2 German_c + \beta_3 Socialist_c + \beta_4 Scandinavian_c$$
$$+\gamma_1 French_c \times Innovative_i + \gamma_2 German_c \times Innovative_i + \gamma_3 Socialist_c \times Innovative_i$$
$$+\theta X_{cit} + \delta_i + \lambda_t + \epsilon_{ct}. \tag{5.3}$$

The second stage regression then takes the following form with $\hat{IF}_{ct}$ being the value of IF_{ct} predicted by the model in Equation (5.2) and $IF_{ct} \times In\hat{n}ovative_i$ being the value of $IF_{ct} \times Innovative_i$ predicted by the model in Equation (5.3):

$$Inv_{cit} = \alpha + \beta \hat{IF}_{ct} + \gamma IF_{ct} \times In\hat{n}ovative_i + \theta X_{cit} + \delta_i + \lambda_t + \epsilon_{cit}. \tag{5.4}$$

There are no country fixed effects in Equations (5.1) through (5.4) as these would absorb the effects of the legal origin variables in the first stage regressions. However, the omission of country fixed effects is not likely to be a concern since our dependent variable Inv_{cit} is the share of investment of a sector within a country.

Throughout this analysis we cluster standard errors at the country level. Alternatively, one could cluster at the sector level. In our case, clustering at the country level is essential in the 2SLS estimations, since the dependent variable in the first stage indicated by Equation (5.2) varies by country but not by sector. Clustering by country is also appropriate given the second stage: our dependent variable is expressed as a sector's within-country share of gross fixed capital formation and hence the residuals will be correlated within country cells.

The two requirements for our instruments to be valid are that they are relevant and exogenous. Relevance implies that the instruments must be correlated with the endogenous regressors in Equation (5.1), while exogeneity requires that the instruments are not correlated with the error term.

To address the concern of weak instruments, we report the relevant F-statistics for the 2SLS estimations in this study. Since we have multiple endogenous variables, we rely on the corrected conditional F-statistic suggested by Sanderson and Windmeijer (2016) to evaluate the strength of each instrument individually. Additionally, we report the Kleibergen and Paap (2006) F-statistic to assess the overall strength of both instruments for each 2SLS regression. As suggested by Baum et al. (2007) we follow the conventional rule of thumb and consider instruments to be sufficiently strong if the relevant F-statistics are larger than 10. The results of the first stage regressions for our baseline estimations, using the binary high-tech classification, the patent share and the AI intensity classification, respectively, are presented in Tables 5.11, 5.12, and 5.13 in the Appendix. The F-statistics are all above the conventional threshold, providing reassurance about the relevance of the instruments.

The second requirement for instrument validity, the exogeneity condition, may be violated if the legal origin of countries affects our dependent variable through other channels which are correlated with institutional quality. To mitigate this concern, we ensure the robustness of our main results by incorporating additional covariates to control for potential confounding factors. Specifically, in a robustness check, we further control for a country's GDP per capita and corporate tax rate as well as their interactions with the innovativeness measures.

5.4 Empirical Results

Table 5.1 presents the results of estimating the model specified in Equation (5.1) with OLS and 2SLS, applying the Eurostat high tech sector classification. Columns (1) and (2) display the results for the Institutional Delivery index, columns (3) and (4) show the results for the EPL index and columns (5) and (6) present the findings for the Starting a Business score. Each institutional indicator is defined such that higher values represent more efficient institutions. To facilitate interpretation, all institutional variables are standardized to have a mean of zero and a standard deviation of one. This means the estimated coefficients on the interaction of the respective institutional indicator with the high-tech dummy variable can be interpreted as the average differential impact of a one-standard deviation increase in the institutional variable on the investment share of high-tech sectors relative to non-high-tech sectors. The results consistently show that more efficient institutional frameworks, as captured by all three institutional indicators, are positively associated with a relatively higher share of investment in high-tech sectors.

Table 5.1: Results for Eurostat High-Tech Sector Classification

	Institutional Delivery		Employment Protection		Starting a Business	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)
Institutional Factor	-0.135***	-0.211***	-0.122**	-0.309***	-0.094**	-0.518***
	(0.042)	(0.056)	(0.053)	(0.080)	(0.041)	(0.146)
Institutional Factor×High-Tech	0.793***	0.924***	0.674***	1.098***	0.449**	2.315***
	(0.197)	(0.216)	(0.193)	(0.202)	(0.173)	(0.552)
Value Added Growth	0.510***	0.494***	0.566***	0.576***	0.525***	0.493***
	(0.135)	(0.134)	(0.103)	(0.108)	(0.123)	(0.122)
Intercept	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	9315	9315	7745	7745	9267	9267
R-squared	0.61	0.66	0.62	0.62	0.60	0.58
First stage, dependent variable: Institutional Factor						
SW multivariate F test of excluded instruments		43.74		37.41		1828.42
First stage, dependent variable: Institutional Factor ×High-Tech						
SW multivariate F test of excluded instruments		191.38		40.23		6153.63
Kleibergen-Paap Wald rk F statistic		51.63		33.67		4411.16

Notes: Standard errors in parentheses are clustered at the country level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

We conduct a back-of-the-envelope calculation to illustrate the economic implications of the differential response of investment to changes in the institutional factors reported in Table 5.1. The 2SLS estimate of 0.924 on the interaction of the institutional indicator with the high-technology dummy in column (2) implies that a one-standard deviation increase in institutional delivery is, on average, associated with a 0.924 percentage-point increase in a high-tech sector's investment share relative to that of a non-high-tech sector.

The coefficient on the non-interacted institutional indicator of -0.211 reflects the marginal impact of a one standard deviation increase in institutional delivery on a non-high-tech sector's share of investment. Therefore, the estimated marginal impact of a one standard deviation increase of institutional delivery on a high-tech sector's share of investment, reported in Table 5.14 in the Appendix, is $-0.211 + 0.924 = 0.713$ percentage points. Using the same method, the marginal impacts of a one-standard-deviation increase in the EPL index and the Starting a Business score on a high-tech sector's investment share are 0.789 and 1.797 percentage points, respectively.

These marginal impacts represent the average impact per high-tech sector. Since our sample includes five sectors classified as high-tech for this analysis, a one standard deviation increase in institutional delivery is associated with a total increase of $0.713 \times 5 = 3.565$ percentage points in the investment share across all high-tech sectors. Similarly, a one standard deviation increase in the EPL index or the Starting a Business score corresponds, on average, to an increase in the high-tech sector investment share of $0.789 \times 5 = 3.945$ or $1.797 \times 5 = 8.985$ percentage points, respectively.

To provide a more concrete interpretation of the results, we simulate the effects of raising each EU country's institutional indicators to the EU frontier, as summarised in Table 5.2. The analysis uses 2021 as the reference year, as it is the most recent year with sufficient data on sectoral gross fixed capital formation from the EU KLEMS dataset. For 17 EU countries with complete data on high-tech sector investment – defined by NACE codes C21, C26, and J – we calculate each country's high-tech sector investment share and total investment in high-tech sectors (in EUR). Using the estimated marginal effects, we evaluate the increase in each country's high-tech investment share resulting from aligning its institutional indicators with the EU frontier.

The predicted post-reform high-tech investment shares are then converted into absolute investment amounts (in EUR), which are summed across all countries to estimate the total increase in high-tech sector investment. The results suggest that raising each EU country's Institutional Delivery Index to the EU frontier would increase the high-tech sector investment share by 4.59 percentage points.

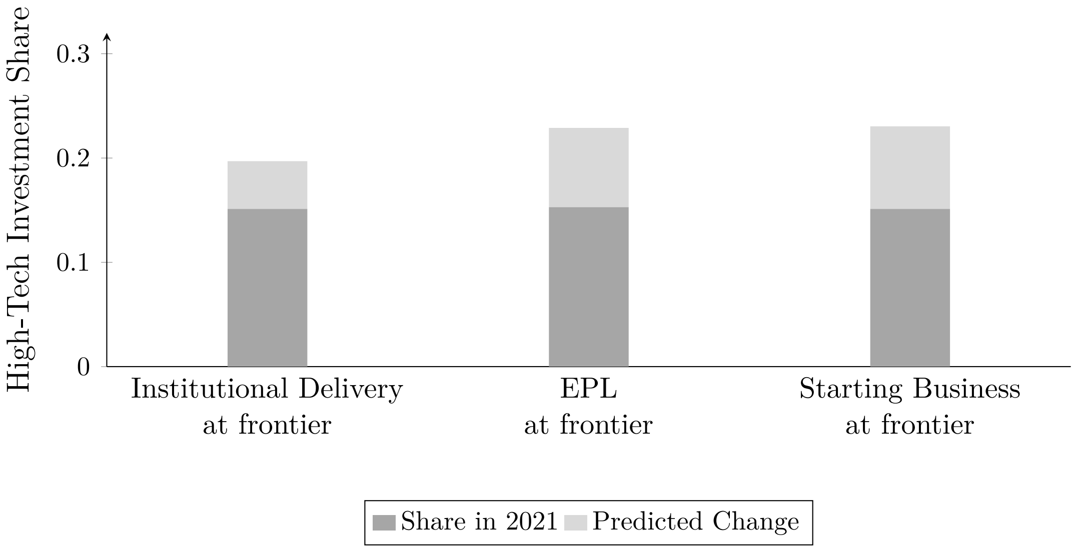
Similarly, aligning each country's EPL index and Starting a Business score with their respective EU frontiers is estimated to raise the high-tech sector investment share by 7.60 and 7.92 percentage points, respectively.

Table 5.2: Back-of-the-envelope calculation of the estimated increases in the high-tech investment share due to institutional and structural improvements in percentage points.

	Per-sector Impact (1 SD)	**Total Impact (1 SD)**	**Improving Institutions to the EU frontier**
Institutional Delivery	0.713	$0.713 \times 5 = 3.565$	4.59
EPL	0.789	$0.789 \times 5 = 3.945$	7.60
Starting a Business	1.797	$1.797 \times 5 = 8.985$	7.92

A comparison to the investment shares of high-tech sectors in EU countries puts this into perspective: In 2021, high-tech sectors' investment accounted for 15.12% of total market sector investment across 17 EU countries[32]. Thus, the estimated increases due to raising each country's institutional indicators to the frontier are substantial, ranging from about 30% of the 2021 high-tech investment share (for the Institutional Delivery index) to about 50% of the 2021 share (for the Starting a Business score). Figure 5.3 illustrates these projected increases in high-tech sectors' investment shares for each of the three institutional indicators.

Figure 5.3: High-tech sectors' investment share and predicted change under institutional and structural reforms



Next, we examine the role of institutional factors for investment in sectors which are more innovative, as indicated by their share of patent applications filed at the USPTO. Table 5.3 presents the results. The 2SLS estimate on the interaction term between the institutional variable and the patent share is positive and statistically significant at the 1 % level for Institutional Delivery and the EPL index. For the Starting a Business score, the 2SLS estimate is significant at the 10 % level.

Table 5.15 in the Appendix reports the corresponding marginal effects of an increase

[32]The high-tech sector investment share is the authors' own calculation using data from EU KLEMS. The investment share was computed considering only those EU countries for which there was data available on investment in all high-tech sectors in 2021, which are Austria, Belgium, Bulgaria, Czechia, Germany, Denmark, Finland, France, Greece, Hungary, Italy, Luxembourg, Latvia, Netherlands, Portugal, Romania, and Slovakia. The high-tech sector investment share reported here differs slightly from the share shown in Figure 5.1 due to a difference in the composition of high-tech sectors. In Figure 5.1 NACE sector C20 (chemicals) is considered a high-tech sector along with NACE sector C21 (pharmaceuticals), even though according to the Eurostat classification and in our analysis only NACE sector C21 is considered as high-tech. This is because there is no disaggregation of these two sectors in the US data, hence in Figure 5.1 we summed them both up under high-tech to make the comparison between Europe and the US possible.

Table 5.3: Results for Patent Shares

	Institutional Delivery		Employment Protection		Starting a Business	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Institutional Indicator	-0.089	-0.083	0.115	0.139	-0.107	-0.119
	(0.104)	(0.122)	(0.100)	(0.156)	(0.126)	(0.267)
Institutional Indicator × Patent Share	0.059***	0.066***	0.044*	0.081***	0.025	0.110*
	(0.016)	(0.022)	(0.021)	(0.030)	(0.015)	(0.063)
Value Added Growth	0.532***	0.530***	0.693***	0.752***	0.587***	0.652***
	(0.153)	(0.146)	(0.109)	(0.095)	(0.138)	(0.113)
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	4323	4323	3688	3688	4314	4314
R-squared	0.34	0.34	0.29	0.27	0.30	0.22
First stage, dependent variable: Institutional Factor						
SW multivariate F test of excluded instruments		64.03		38.01		889.97
First stage, dependent variable: Institutional Factor ×Patent Share						
SW multivariate F test of excluded instruments		51.80		43.49		857.52
Kleibergen-Paap Wald rk F statistic		104.21		34.42		518.72

Notes: Standard errors in parentheses are clustered at the country level. The Patent Share is centered around its sample-mean so the estimate on the non-interacted institutional indicator is the estimated effect at the mean. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

in the various institutional indicators on the investment shares of the lowest-ranking and the highest-ranking sectors, in terms of patent shares. The differential impact on the highest-ranking sector relative to the lowest-ranking sector is reported as well.

The results of the 2SLS estimations suggest that an increase in any of the three institutional indicators is associated with a statistically significant relative increase of the share of investment allocated to the sector with the highest patent share. These findings suggest that improvements in institutional frameworks have a disproportionately stronger effect on investment in sectors with higher patenting activity.

Third, we examine the differential impact of institutional frameworks on investment in AI intensive sectors compared to less AI-intensive sectors. As described in Section 5.3.2, we estimate the model in Equation (5.1) using the binary AI intensity classification by Calvino et al. (2024) as the moderating variable. Table 5.4 presents the results.

For two out of the three indicators, we find the estimate on the interaction of the institutional indicator with the AI-intensity dummy to be positive and statistically significant at the 1% level. For the EPL index, the estimate on the interaction is also positive; however, it is not significant at the 10% level.

Table 5.16 reports the corresponding estimated impacts on the investment share of

Table 5.4: Results for AI Intensity

	Institutional Delivery		Employment Protection		Starting a Business	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Institutional Indicator	-0.182**	-0.256***	-0.051	-0.247*	-0.235***	-0.596***
	(0.061)	(0.088)	(0.084)	(0.126)	(0.068)	(0.159)
Institutional Indicator×AI Intensive	0.845***	0.967***	0.227	0.415	0.618***	2.019***
	(0.193)	(0.204)	(0.206)	(0.344)	(0.176)	(0.537)
Value Added Growth	0.928***	0.913***	1.200***	1.194***	1.021***	1.161***
	(0.188)	(0.186)	(0.262)	(0.266)	(0.212)	(0.202)
Sector-FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	7182	7182	6038	6038	7125	7125
R-squared	0.60	0.60	0.60	0.60	0.60	0.58
First stage, dependent variable: Institutional Factor						
SW multivariate F test of excluded instruments		45.47		43.18		2411.50
First stage, dependent variable: Institutional Factor ×AI-intensive						
SW multivariate F test of excluded instruments		29.75		9.12		1040.70
Kleibergen-Paap Wald rk F statistic		34.47		20.70		231.23

Notes: Standard errors in parentheses are clustered at the country-level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

AI-intensive and non-AI-intensive sectors. To gauge the economic implications of these estimates, in Table 5.5 we conduct another back-of-the-envelope calculation of the expected change in the seven AI-intensive sectors' investment shares resulting from elevating EU countries' institutional and structural indicators to frontier levels. The calculation follows the same methodology as the one presented in Table 5.2 which analysed the expected increase in high-tech sector investment. Our results suggest that raising institutional and regulatory frameworks to frontier levels would lead to an increase of AI-intensive sectors' investment share between 2.14 and 7.60 percentage points. With AI intensive sectors accounting for approximately 28.5% of total investment in 2021, these estimates indicate significant relative increases of roughly 7.5%, 24.4%, or 26.7% in the investment share, depending on whether the EPL index, the Starting a Business score, or the institutional delivery index is raised to the frontier level. Figure 5.4 shows the magnitudes of these impacts in relation to AI intensive sectors' investment share, across 18 EU countries in 2021[33].

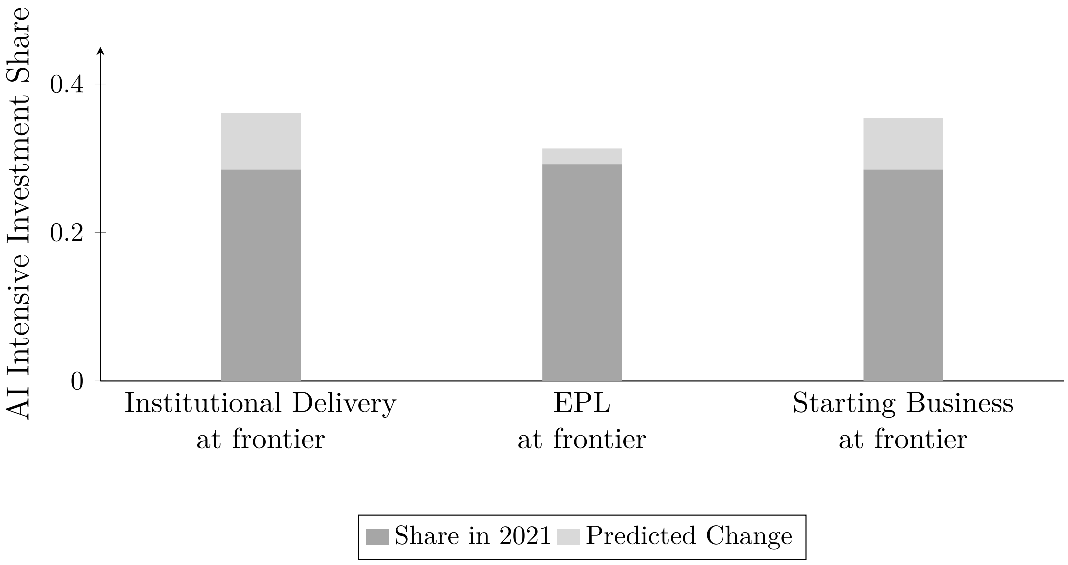
Calvino et al. (2024) base the AI-intensity classification used above on four subindices. Those sectors which rank consistently high across four indices measuring different aspects

[33]The 18 EU countries for which there was data available from Eurostat on investment in all seven AI intensive sectors in 2021 and which could thus be considered in this calculation are Austria, Belgium, Bulgaria, Czechia, Denmark, Finland, France, Greece, Hungary, Ireland, Italy, Luxembourg, Latvia, Netherlands, Portugal, Romania, Sweden, and Slovakia

Table 5.5: Back-of-the-envelope calculation of the estimated increase in AI intensive sectors' investment share changes due to institutional and structural improvements in percentage points.

	Per-sector Impact (1 SD)	**Total Impact (1 SD)**	**Improving Institutions to the EU frontier**
Institutional Delivery	0.711	$0.711 \times 7 = 4.977$	7.60
EPL	0.168	$0.168 \times 7 = 1.176$	2.14
Starting a Business	1.422	$1.422 \times 7 = 9.954$	6.96

Figure 5.4: AI intensive sectors' investment share and predicted change under institutional and structural reforms



of AI intensity – AI human capital, AI innovation, AI exposure and AI use – get classified as highly AI intensive. To see whether our main results vary across sub-dimensions of AI intensity, we estimate the model in Equation (5.1) using the sectoral ranking of the four subindices reported by Calvino et al. (2024) as the variable moderating the effect of the institutional variable. The four panels of Table 5.17 in the Appendix show the results of this exercise for the four AI intensity subindices[34].

For three out of the four AI intensity indicators – AI Human Capital, AI Exposure, and AI Use – the estimated coefficients on the interaction term of interest are positive and statistically significant at the 5% level for all three institutional indicators. An exception is the AI Innovation indicator. Here, the 2SLS estimate on the interaction of the institutional

[34]For the specification using the AI exposure ranking, we use the sectoral ranking with respect to the non-barrier-adjusted AI exposure measure by Felten et al. (2021) instead of the barrier-adjusted exposure measure by Calvino et al. (2024) to mitigate the issue that the absence of regulatory barriers determines the AI intensity classification, as discussed in Section 5.3.2.

indicator with the intensity rank is not statistically different from zero for the EPL index. This could reflect the fact that AI-related patenting might be more affected by factors other than labor market regulation, like human capital or access to capital.

Similar to Table 5.17, the four panels of Table 5.18 in the Appendix show the estimated marginal effects of the different institutional indicators on the lowest ranking sector and on the highest ranking sector according to each of the four AI intensity rankings. Also, each panel shows the respective estimated differential impact on the highest ranking sector relative to the lowest ranking sector. The significance levels of the estimated differential impacts mirror those of the institutional interaction terms in Table 5.17. Overall, the results indicate that sectors with a higher AI intensity are more strongly affected by institutional framework conditions than sectors with low AI intensity. This appears to be particularly the case for sectors in which AI applications are used rather than invented, as indicated by the insignificant estimate for the EPL index in the specification using the AI Innovation indicator.

One might argue that in the time period from 2004 to 2019, over which the baseline regressions were conducted, AI technology had not yet reached the level of relevance it holds in more recent years. Consequently, the concept of AI intensity which we allow to moderate the impact of the institutional and structural indicators may have been less meaningful during that earlier timeframe. To address this concern, we re-estimate the model in Equation (5.1), focusing on the effects of institutional indicators on AI intensity, while using a more recent sample covering the years 2019 to 2023. This period captures a time in which the emergence of AI technology was further advanced than in earlier years and non-regulatory barriers to firms' adoption were lower. The investment data for these regressions is from Eurostat since the EU KLEMS data only extends to 2021.

Moreover, since the most recent years for which the EPL index and the Starting a Business score are available are 2019 and 2020, respectively, we assume the institutional indicators to be non-time-varying in these specifications. Specifically, we use the average value of each indicator for a country across the entire sample period and apply this average to all five years under consideration.

Table 5.6 shows the results of these estimations. The 2SLS estimates on the interaction term of interest are positive and statistically significant at the 1% level for the Institutional Delivery index and the Starting a Business score. Column (4) shows that the 2SLS estimate

Table 5.6: Results for AI Intensity (2019 – 2023)

	Institutional Delivery		Employment Protection		Starting a Business	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Institutional Indicator	-0.311***	-0.540***	-0.266	-0.637***	-0.312**	-0.747***
	(0.108)	(0.165)	(0.174)	(0.245)	(0.136)	(0.274)
Institutional Indicator×AI intensive	0.931***	1.254***	0.382	0.682*	0.710**	1.605***
	(0.212)	(0.318)	(0.280)	(0.356)	(0.309)	(0.498)
Value added growth	0.451	0.408	0.831	0.759*	0.388	0.309
	(0.477)	(0.456)	(0.487)	(0.459)	(0.483)	(0.438)
Sector-FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	1676	1676	1484	1484	1676	1676
R-squared	0.60	0.59	0.62	0.61	0.59	0.58
First stage, dependent variable: Institutional Factor						
SW multivariate F test of excluded instruments		23.23		63.54		5387.57
First stage, dependent variable: Institutional Factor × AI-intensive						
SW multivariate F test of excluded instruments		11.42		6.15		685.85
Kleibergen-Paap Wald rk F statistic		13.26		18.51		118.08

Notes: Standard errors in parentheses are clustered at the country-level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

on the interacted EPL index is significant at the 10% level, while it was insignificant in the baseline estimations presented in Table 5.4, underscoring the importance of institutional quality in fostering investment in AI-intensive sectors during the more recent period, when AI technology has become increasingly prominent.

5.5 Robustness and Additional Results

5.5.1 Controlling for Confounding Variables

In this section we examine the robustness of the results by controlling for additional covariates, using alternative dependent variables, and modifying the sample of countries.

First, we address concerns that the exogeneity restriction of our IVs may be violated in our baseline estimations. The exogeneity restriction requires that our IVs, that is the legal origin dummies and their interactions with the sectoral innovativeness measures, affect the differential sectoral investment shares of innovative sectors only through their effect on the institutional measures and their respective interactions. It is plausible that legal origins have an effect on investment conditions through other channels than the institutional variables which we use. This would raise concerns about the validity of our instruments. To address this issue, we re-estimate the model in Equation (5.1) while controlling for GDP per capita and the corporate tax rate, as well as for these variables' respective interactions with the three innovativeness measures.

Table 5.19 shows the results for the high-tech classification. The 2SLS estimates on the interaction of the institutional variables with the high-tech indicator remain similar in magnitude to the baseline estimates. For the Institutional Delivery index in column (1), the precision of the estimate on the interaction decreases due to the inclusion of the additional covariates, yet it remains significant at the 10% level.

For the EPL index in column (2), the relevant F-statistics are below the conventional threshold of 10, raising concerns about weak instruments in this specific specification. Nevertheless, the results are overall qualitatively consistent with the baseline results.

Tables 5.20 and 5.21 present analogous results using the sectoral patent share and the AI intensity indicator, respectively, as the moderating variable. In both cases, the baseline results prove robust, with the exception of the estimate on the interacted Institutional Delivery index when using the AI intensity indicator (column 1 in Table 5.21) which, while still positive, is not statistically significant at the 10% level.

5.5.2 Alternative Dependent Variables

We further test the robustness of our main results by employing alternative dependent variables in the estimation. First, we use the value added of sector i in country c in year t as a share of total market sector value added in country c in year t.

This approach shifts the focus from examining the differential impact of institutional factors on investment in innovative sectors to analysing whether the relative economic size of innovative sectors compared to less innovative sectors is larger in countries with more efficient institutional environments. We obtain the data on sectoral value added in current prices from Eurostat.

The results, presented in Tables 5.22, 5.23 and 5.24 in the Appendix for the three different sector innovativeness classifications, are consistent with our main results. Specifically, the value added in innovative and disruptive sectors increases relative to other sectors in countries with more efficient institutions and better regulatory frameworks.

As a second variation to the dependent variable, we use the R&D expenditure share of sector i in country c in year t as a share of total market sector R&D expenditure. The R&D expenditure data is obtained from the EU KLEMS dataset. This specification allows us to investigate whether more efficient institutions lead to a greater concentration of innovative activity—proxied by R&D expenditures—in disruptive sectors compared to traditional industries. Tables 5.25, 5.26 and 5.27 in the Appendix report the results of estimating the model in Equation (5.1) with this alternative dependent variable using as the moderating innovativeness variable the Eurostat high-tech classification, the patent share and the AI intensity classification, respectively.

Table 5.25 suggests that the results obtained using the Eurostat high-tech classification are in line with our main results. The positive and significant estimates on the interaction term suggest that more efficient institutions are associated with increased R&D expenditures in sectors classified as high-tech relative to other sectors. However, we do not find evidence that R&D expenditures in sectors with high patenting activity and in AI intensive sectors are affected more strongly by these institutional and regulatory factors, as the estimates on the interaction terms reported in Tables 5.26 and 5.27 are not statistically different from zero.

5.5.3 Resolving Insolvency Score

Thus far we have used three institutional and structural indicators (the institutional delivery index, the EPL index and the Starting a Business score) to examine the differential impact of institutions and regulations on investment in innovative sectors. These indicators can be interpreted as determinants of firms' start-up and rescaling costs and thus of the costs of project failure which according to Coatanlem and Coste (2024) strongly determine firms' profitability in disruptively innovative sectors.

In this section we re-estimate the model in Equation (5.1), incorporating the Resolving Insolvency score of the World Bank (2020) as the structural indicator. This index measures the ease of resolving insolvency in a country and is composed of two subindices: the recovery rate in insolvency proceedings and the strength of the legal insolvency framework. The ease of resolving insolvency may serve as a proxy for firms' exit costs, complementing indicators such as starting and scaling a business. It may also capture the cost of project failure leading to a firm's exit.

Table 5.28 in the Appendix presents the results of these estimations for the three classifications of innovativeness. The results suggest a positive differential impact of the insolvency indicator. The estimate on the interaction of the resolving insolvency score with the innovativeness measure is statistically significant at the 1% level in all three specifications.

Our results underline the importance of efficient insolvency frameworks for investment in innovative sectors. This is in line with the argument of Coatanlem and Coste (2024, 2025) that activity in these sectors is highly responsive to the costs of failure.

5.5.4 Excluding Ireland and Luxembourg

In a final robustness check, we drop Ireland and Luxembourg from the sample, due to their role as financial hubs. The investment data of these countries need to be interpreted with caution, since they are heavily affected by the activities of multinational enterprises and a large financial sector which, in the case of Ireland, show only limited links with domestic economic activity. Table 5.29 in the Appendix shows the results of the 2SLS estimations without these two countries using the Eurostat high-tech sector classification as the moderating innovativeness classification. The estimates on the interactions of the high tech dummy with the institutional variables remain very similar in magnitude to those of the baseline estimates in Table 5.1.

Similarly, Tables 5.30 and 5.31 in the Appendix show the results of the estimations using the sectoral patent share and the AI intensity classification as moderating innovativeness variables. Overall, the estimates confirm that the main findings of the analysis are not materially affected by the exclusion of Ireland and Luxembourg.

5.6 Conclusion

This study has provided evidence for the relation between institutional and regulatory quality and the share of investment in innovative sectors in EU countries. Stronger institutions and more efficient regulatory frameworks are associated with significantly higher investment shares in high-tech, patent-intensive, and AI-intensive sectors.

Our findings suggest that the EU's lag in technological dynamism and productivity goes beyond industrial composition or sectoral preferences. It is deeply rooted in the quality of governance and regulatory frameworks. This is particularly the case for highly innovative sectors, such as those relying heavily on AI, R&D, and disruptive technologies, which face a significantly higher probability of failure. These sectors are disproportionately affected by the costs and constraints of institutional setups, as they depend on adaptive and supportive frameworks to effectively manage risk, scale operations, and recover from setbacks.

Qualitatively, illustrative back-of-the-envelope calculations suggest that aligning the institutional and regulatory quality of all EU member states with the corresponding current EU frontier could raise the share of investment in high technology sectors by up to 50%, closing the respective gap to the US by about the same amount.

These findings are policy-relevant, as a significant portion of the productivity growth gap between the EU and the US can be attributed to the smaller size and slower productivity growth of innovative sectors in the EU (Draghi, 2024). Easing labor market rigidities, enhancing the rule of law, and reducing administrative burdens could significantly improve the investment climate for cutting-edge industries. In particular, reforming employment protection legislation and simplifying business start-up procedures could help reduce barriers to innovation and entrepreneurial risk-taking, enabling a more dynamic reallocation of resources toward frontier technologies. Such improvements in the regulatory and institutional framework will need to be complemented by other important enablers of innovation, such as access to finance, education and skill-upgrading systems, and robust digital infrastructure, to further close the investment gap to the US.

Looking ahead, future research could further build on this work by exploring the interaction between institutional reforms and these complementary enablers. Moreover, as AI and other general-purpose technologies continue to evolve rapidly, gaining deeper insights into the specific institutional factors that facilitate their diffusion will be critical for improving Europe's competitiveness on the global stage.

5.A Appendix

Table 5.7: Value added weighted average

Productivity annual average growth, 2000–2019, %	EU11	US	US minus EU11
Total Economy	0.9	1.6	0.62
Total excluding contribution from ICT	0.8	1.1	0.33
Total excluding contribution from ICT and Financial Services	0.7	0.9	0.22
ICT, of which	3.8	7.8	3.97
Manufacturing of computers and electronics	4.2	11.8	7.54
Information and Communication services	2.7	6.7	3.99
Financial and Insurance Services	1.3	1.8	0.48
Memo items, VA shares			
1999			
ICT, of which	3.8	3.6	-0.17
Manufacturing of computers and electronics	1.2	0.5	-0.69
Information and Communication services	2.6	3.2	0.52
Financial and Insurance Services	4.8	8.0	3.15
2019			
ICT, of which	6.2	9.5	3.23
Manufacturing of computers and electronics	1.6	1.6	0.03
Information and Communication services	4.6	7.8	3.21
Financial and Insurance Services	4.3	7.6	3.30

Source: EU KLEMS and ECB staff computations; EU11: sum of AT, CZ, BE, DE, DK, ES, FI, FR, IT, NL, SE. Sectoral contributions are based on corresponding shares of value added.

Table 5.8: NACE Sectors Included in the Analysis

NACE Code	**Sector Description**
B	Mining and Quarrying
C10–C12	Manufacture of food products, beverages and tobacco products
C13–C15	Manufacture of textiles, wearing apparel, leather and related products
C16–C18	Manufacture of wood, wood products (except furniture) and paper; Printing
C19	Manufacture of coke and refined petroleum products
C20	Manufacture of chemicals and chemical products
C21	Manufacture of basic pharmaceutical products and pharmaceutical preparations

NACE Code	Sector Description
C22–C23	Manufacture of rubber, plastic products and other non-metallic mineral products
C24–C25	Manufacture of basic metals and fabricated metal products (except machinery and equipment)
C26	Manufacture of computer, electronic and optical products
C27	Manufacture of electrical equipment
C28	Manufacture of machinery and equipment n.e.c.
C29–C30	Manufacture of motor vehicles, trailers, semi-trailers and other transport equipment
C31–C33	Manufacture of furniture and other manufacturing; Repair and installation of machinery and equipment
D	Electricity, gas, steam and air conditioning supply
E	Water supply; sewerage; waste management and remediation activities
F	Construction
G	Wholesale and retail trade
H	Transport and warehousing
I	Accommodation and food service activities
J58–J60	Publishing activities; motion picture, video and television programme production, sound recording; Programming and broadcasting activities
J61	Telecommunications
J62–J63	Computer programming, consultancy and related activities; Information service activities
K	Financial and Insurance Activities
M	Professional, Scientific and Technical Activities
N	Administrative and Support Service Activities
R	Arts, Entertainment and Recreation
S	Other Service Activities

Table 5.9: Summary Statistics

Variable	N	Mean	SD	Min	Max
Investment share (EU KLEMS)	9315	3.89	4.03	-4.59	35.41
Investment share (Eurostat)	9545	3.56	3.98	-4.59	43.65
High-tech	9315	0.15	0.36	0	1
Patent share	4323	6.39	9.39	0.16	38.54
AI intensive	7182	0.28	0.45	0	1
AI human capital rank	7182	12.07	6.92	1	24
AI innovation rank	7182	12.70	6.74	1	24
AI exposure rank	7182	12.28	7.07	1	24
AI use rank	6798	11.4	6.71	1	23
Institutional delivery	9315	1.09	0.63	-0.13	2.18
EPL	7745	1.90	0.45	0.61	2.89
Starting a Business score	9267	84.48	8.68	51.5	95.3
Value added growth	9315	0.02	0.14	-2.01	2.13
GDP per capita	9315	46.32	19.79	16.38	138.68
Corporate tax rate	9315	23.98	7.11	9	38.90
Resolving insolvency	9267	68.16	15.31	36.4	93.9
R&D investment share	7863	0.04	0.08	-0.50	1.15
Value added share	12137	3.26	3.90	-0.12	39.22

Notes: The summary statistics of the institutional indicators are those before standardization.

Table 5.10: Standard deviations of institutional indicators

Variable	SD Overall	SD Between	SD Within
Institutional delivery	0.63	0.62	0.10
EPL	0.45	0.42	0.17
Starting a Business score	8.68	5.89	6.37

Notes: The summary statistics of the institutional indicators are those before standardization.

Table 5.11: First Stage Results for Eurostat High-Tech Sector Classification

	Institutional Delivery	Employment Protection	Starting a Business
Dep. Var.: Institutions	(1)	(2)	(3)
French	-0.667***	-2.339***	-0.838***
	(0.290)	(0.239)	(0.249)
German	0.164***	-1.944***	-1.265***
	(0.034)	(0.265)	(0.033)
Socialist	-1.572***	-1.965***	-1.021***
	(0.164)	(0.292)	(0.197)
Scandinavian	0.668***	-0.676*	0.011
	(0.056)	(0.400)	(0.052)
French × High-Tech	-0.040	-0.114***	0.037
	(0.035)	(0.042)	(0.044)
German × High-Tech	-0.004	-0.045*	0.003
	(0.005)	(0.024)	(0.005)
Socialist × High-Tech	-0.113	-0.252	0.018
	(0.097)	(0.176)	(0.114)
Value added growth	0.114	-0.046	0.055
	(0.101)	(0.069)	(0.124)
Intercept	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Number of Observations	9315	7745	9267
R-squared	0.64	0.46	0.45
SW multivariate F test of excluded instruments	43.74	37.41	1828.42
	Institutional Delivery	Employment Protection	Starting a Business
Dep. Var.: Institutions × High-Tech	(1)	(2)	(3)
French	0.001*	-0.002	-0.002
	(0.000)	(0.001)	(0.003)
German	0.000	-0.001	-0.000
	(0.000)	(0.001)	(0.000)
Socialist	-0.000	-0.006	-0.000
	(0.000)	(0.004)	(0.000)
Scandinavian	0.001*	-0.001*	-0.000
	(0.000)	(0.001)	(0.001)
French × High-Tech	-1.377***	-1.772***	-0.801***
	(0.312)	(0.439)	(0.248)
German × High-Tech	-0.508***	-1.313***	-1.272***
	(0.043)	(0.456)	(0.044)
Socialist × High-Tech	-2.352***	-1.516***	-1.013***
	(0.212)	(0.541)	(0.261)
Value added growth	0.035**	-0.040**	0.008
	(0.018)	(0.019)	(0.023)
Intercept	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Number of Observations	9315	7745	9267
R-squared	0.66	0.44	0.20
SW multivariate F test of excluded instruments	191.38	40.23	6153.63
Kleibergen-Paap Wald rk F statistic	51.63	33.67	4411.16

Notes: Standard errors in parentheses are clustered at the country level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.12: First Stage Results for Patent Shares

	Institutional Delivery	Employment Protection	Starting a Business
Dep. Var.: Institutions	(1)	(2)	(3)
French	-0.649**	-2.341***	-0.839***
	(0.294)	(0.247)	(0.262)
German	0.213***	-1.898***	-1.299***
	(0.069)	(0.276)	(0.075)
Socialist	-1.576***	-2.085***	-1.039***
	(0.170)	(0.341)	(0.215)
Scandinavian	0.712***	-0.583	0.008
	(0.081)	(0.389)	(0.085)
French × Patent Share	-0.001	-0.005	0.005
	(0.002)	(0.004)	(0.005)
German × Patent Share	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Socialist × Patent Share	-0.001	-0.002	0.000
	(0.001)	(0.002)	(0.001)
Value added growth	0.136*	-0.074	0.073
	(0.076)	(0.089)	(0.145)
Intercept	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Number of Observations	4323	3688	4314
R-squared	0.67	0.45	0.44
SW multivariate F test of excluded instruments	64.03	38.01	889.97
	Institutional Delivery	Employment Protection	Starting a Business
Dep. Var.: Institutions × Patent Share	(1)	(2)	(3)
French	-4.231***	3.036	0.407
	(0.593)	(2.447)	(0.723)
German	-4.145***	3.496	0.029
	(0.490)	(2.398)	(0.518)
Socialist	-4.218***	3.280	0.065
	(0.548)	(2.426)	(0.606)
Scandinavian	-4.126***	3.499	0.021
	(0.491)	(2.409)	(0.516)
French × Patent Share	-1.374***	-1.801***	-0.755***
	(0.327)	(0.463)	(0.252)
German × Patent Share	-0.491***	-1.265***	-1.304***
	(0.044)	(0.480)	(0.043)
Socialist × Patent Share	-2.285***	-1.454***	-1.040***
	(0.204)	(0.558)	(0.263)
Value added growth	0.561*	-1.064**	-0.597
	(0.339)	(0.475)	(0.480)
Intercept	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Number of Observations	4323	3688	4314
R-squared	0.68	0.45	0.17
SW multivariate F test of excluded instruments	64.03	43.49	857.52
Kleibergen-Paap Wald rk F statistic	104.21	34.42	518.72

Notes: Standard errors in parentheses are clustered at the country level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.13: First Stage Results for AI Intensity

	Institutional Delivery	Employment Protection	Starting a Business
Dep. Var.: Institutions	(1)	(2)	(3)
French	-0.662**	-2.344***	-0.771***
	(0.305)	(0.213)	(0.233)
German	0.098***	-1.718***	-1.310***
	(0.018)	(0.198)	(0.012)
Socialist	-1.602***	-1.986***	-1.049***
	(0.176)	(0.313)	(0.230)
Scandinavian	0.609***	-0.669*	-0.008
	(0.046)	(0.370)	(0.047)
French × AI Intensive	-0.011	-0.039	0.028
	(0.015)	(0.024)	(0.029)
German × AI Intensive	0.003	0.062	-0.000
	(0.006)	(0.072)	(0.005)
Socialist × AI Intensive	-0.044	-0.068	0.000
	(0.029)	(0.051)	(0.036)
Value added growth	0.177	-0.212*	0.137
	(0.178)	(0.117)	(0.236)
Intercept	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Number of Observations	7182	6038	7125
R-squared	0.63	0.53	0.45
SW multivariate F test of excluded instruments	45.47	43.18	2411.50
	Institutional Delivery	Employment Protection	Starting a Business
Dep. Var.: Institutions × AI Intensive	(1)	(2)	(3)
French	0.160***	-0.184*	-0.005
	(0.036)	(0.103)	(0.013)
German	0.159***	-0.180*	0.001
	(0.036)	(0.102)	(0.012)
Socialist	0.158***	-0.187*	0.002
	(0.036)	(0.102)	(0.012)
Scandinavian	0.190***	-0.216*	0.001
	(0.016)	(0.116)	(0.014)
French × AI Intensive	-1.175***	-1.824***	-0.720***
	(0.334)	(0.382)	(0.236)
German × AI Intensive	-0.398***	-1.108***	-1.302***
	(0.111)	(0.342)	(0.037)
Socialist × AI Intensive	-2.142***	-1.482***	-1.037***
	(0.219)	(0.464)	(0.249)
Value added growth	0.047	-0.086	-0.082
	(0.047)	(0.054)	(0.072)
Intercept	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Number of Observations	7182	6038	7125
R-squared	0.63	0.52	0.17
SW multivariate F test of excluded instruments	29.75	9.12	1040.70
Kleibergen-Paap Wald rk F statistic	34.47	20.70	231.23

Notes: Standard errors in parentheses are clustered at the country level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.14: Marginal Effects for Eurostat High-Tech Sector Classification

	Institutional Delivery		Employment Protection		Starting a Business	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Impact of 1 SD increase at:						
High-Tech = 0	-0.135***	-0.211***	-0.122**	-0.309***	-0.094**	-0.518***
	(0.042)	(0.056)	(0.053)	(0.080)	(0.041)	(0.146)
High-Tech = 1	0.658***	0.713***	0.552***	0.789***	0.355**	1.797***
	(0.176)	(0.180)	(0.156)	(0.174)	(0.144)	(0.448)
Differential Impact	0.793**	0.924***	0.674***	1.098***	0.449**	2.315***
	(0.197)	(0.216)	(0.193)	(0.202)	(0.173)	(0.552)

Notes: Standard errors in parentheses are clustered at the country level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.15: Marginal Effects for Patent Shares

	Institutional Delivery		Employment Protection		Starting a Business	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Impact of 1 SD increase at:						
Lowest Patent Share (C19)	-0.455***	-0.491***	-0.162	-0.367**	-0.261**	-0.803***
	(0.105)	(0.108)	(0.121)	(0.147)	(0.125)	(0.313)
Highest Patent Share (C26)	1.800***	2.022***	1.541**	2.747***	0.681	3.408
	(0.584)	(0.778)	(0.309)	(1.056)	(0.530)	(2.191)
Differential Impact	2.255***	2.514***	1.703*	3.114***	0.942	4.211*
	(0.629)	(0.828)	(0.823)	(1.134)	(0.562)	(2.411)

Notes: Standard errors in parentheses are clustered at the country level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.16: Marginal Effects for AI Intensity Classification

	Institutional Delivery		Employment Protection		Starting a Business	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Impact of 1 SD increase at:						
AI Intensive = 0	-0.182***	-0.256***	-0.051	-0.247*	-0.235***	-0.596***
	(0.061)	(0.088)	(0.084)	(0.126)	(0.068)	(0.159)
AI Intensive = 1	0.663***	0.711***	0.176	0.168	0.384***	1.422***
	(0.156)	(0.142)	(0.133)	(0.237)	(0.136)	(0.413)
Differential Impact	0.845**	0.967***	0.227	0.415	0.618***	2.019***
	(0.193)	(0.204)	(0.206)	(0.344)	(0.176)	(0.537)

Notes: Standard errors in parentheses are clustered at the country level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.17: Results for AI Intensity Subindicator Rankings

	Institutional Delivery	Employment Protection	Starting a Business
	2SLS (1)	2SLS (2)	2SLS (3)
AI Human Capital			
Institutional Indicator	0.007	-0.160**	-0.035
	(0.053)	(0.062)	(0.099)
Institutional Indicator × Rank	0.072***	0.060***	0.152***
	(0.014)	(0.017)	(0.038)
Value Added Growth	0.905***	1.251***	1.146***
	(0.184)	(0.258)	(0.212)
Sector- & Year-FE	Yes	Yes	Yes
Number of Observations	7182	6038	7125
R-squared	0.60	0.60	0.57
First stage, dependent variable: Institutional Factor			
SW multivariate F test of excluded instruments	53.22	238.98	3985.17
First stage, dependent variable: Institutional Factor × Rank			
SW multivariate F test of excluded instruments	18.30	12.00	517.97
Kleibergen-Paap Wald rk F statistic	37.56	25.83	369.76
AI Innovation			
Institutional Indicator	0.020	-0.126**	-0.041
	(0.055)	(0.056)	(0.082)
Institutional Indicator × Rank	0.042***	0.013	0.080***
	(0.010)	(0.016)	(0.023)
Value Added Growth	0.937***	1.193***	1.030***
	(0.173)	(0.268)	(0.207)
Sector- & Year-FE	Yes	Yes	Yes
Number of Observations	7182	6038	7125
R-squared	0.60	0.60	0.59
First stage, dependent variable: Institutional Factor			
SW multivariate F test of excluded instruments	47.61	1576.00	4096.43
First stage, dependent variable: Institutional Factor × Rank			
SW multivariate F test of excluded instruments	56.95	672.56	2994.06
Kleibergen-Paap Wald rk F statistic	31.66	287.58	2549.64
AI Exposure			
Institutional Indicator	0.0512	-0.128**	-0.020
	(0.054)	(0.057)	(0.095)
Institutional Indicator × Rank	0.098***	0.046**	0.147***
	(0.015)	(0.021)	(0.044)
Value Added Growth	0.890***	1.196***	1.110***
	(0.171)	(0.263)	(0.223)
Sector- & Year-FE	Yes	Yes	Yes
Number of Observations	7182	6038	7125
R-squared	0.61	0.60	0.57
First stage, dependent variable: Institutional Factor			
SW multivariate F test of excluded instruments	50.95	168.28	2706.37
First stage, dependent variable: Institutional Factor × Rank			
SW multivariate F test of excluded instruments	10.56	16.78	345.98
Kleibergen-Paap Wald rk F statistic	40.76	23.11	238.99
AI Use			
Institutional Indicator	-0.043	-0.133**	-0.083
	(0.049)	(0.062)	(0.093)
Institutional Indicator × Rank	0.088***	0.064***	0.156***
	(0.019)	(0.022)	(0.046)
Value Added Growth	0.928***	1.191***	1.117***
	(0.182)	(0.261)	(0.220)
Sector- & Year-FE	Yes	Yes	Yes
Number of Observations	6798	5716	6744
R-squared	0.62	0.61	0.58
First stage, dependent variable: Institutional Factor			
SW multivariate F test of excluded instruments	59.15	529.04	9453.92
First stage, dependent variable: Institutional Factor × Rank			
SW multivariate F test of excluded instruments	37.67	32.16	263.52
Kleibergen-Paap Wald rk F statistic	39.93	23.38	922.15

Notes: Standard errors in parentheses are clustered at the country level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Each AI intensity ranking is centered around its respective mean so the estimates on the non-interacted institutional indicator are the estimated effects at the mean.

Table 5.18: Marginal Effects for AI Intensity Subindicator Rankings

	Institutional Delivery	Employment Protection	Starting a Business
	2SLS	2SLS	2SLS
	(1)	(2)	(3)
AI Human Capital			
Impact of 1 SD increase at:			
Lowest Rank (I)	-0.790***	-0.829***	-1.719***
	(0.171)	(0.223)	(0.396)
Highest Rank (C26)	0.867***	0.559***	1.777***
	(0.177)	(0.178)	(0.498)
Differential Impact	1.657**	1.388***	3.495***
	(0.330)	(0.387)	(0.874)
AI Innovation			
Impact of 1 SD increase at:			
Lowest Rank (C22-C23)	-0.468***	-0.274	-0.974***
	(0.122)	(0.203)	(0.239)
Highest Rank (J58-J60)	0.491***	0.017	0.858***
	(0.139)	(0.189)	(0.314)
Differential Impact	0.960***	0.291	1.832***
	(0.237)	(0.375)	(0.535)
AI Exposure			
Impact of 1 SD increase at:			
Lowest Rank (F)	-1.097***	-0.648***	-1.683***
	(0.162)	(0.245)	(0.460)
Highest Rank (J62-J63)	1.163***	0.412*	1.706***
	(0.207)	(0.244)	(0.565)
Differential Impact	2.260***	1.060**	3.388***
	(0.356)	(0.475)	(1.012)
AI Use			
Impact of 1 SD increase at:			
Lowest Rank (I)	-0.965***	-0.796***	-1.707***
	(0.197)	(0.261)	(0.454)
Highest Rank (J62-J63)	0.980***	0.601**	1.718***
	(0.232)	(0.244)	(0.578)
Differential Impact	1.944***	1.397***	3.425***
	(0.418)	(0.492)	(1.018)

Notes: Standard errors in parentheses are clustered at the country level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.19: Controlling for GDP p.c. and Taxes (High-Tech Classification)

	Institutional Delivery	Employment Protection	Starting a Business
	2SLS	2SLS	2SLS
	(1)	(2)	(3)
Institutional Factor	-0.305***	-0.335***	-0.481***
	(0.063)	(0.097)	(0.126)
Institutional Factor × High-Tech	0.763*	0.918***	1.901***
	(0.402)	(0.231)	(0.485)
GDP per capita	0.005	0.000	0.001
	(0.004)	(0.005)	(0.004)
GDP per capita × High-Tech	0.026	0.036**	0.020
	(0.022)	(0.014)	(0.020)
Corporate Tax Rate	0.011	-0.007	-0.003
	(0.010)	(0.011)	(0.012)
Corporate Tax Rate × High-Tech	-0.034*	0.003	0.026
	(0.020)	(0.022)	(0.045)
Value added growth	0.518***	0.559***	0.501***
	(0.143)	(0.106)	(0.130)
Sector-FE	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes
Number of Observations	9315	7745	9267
R-squared	0.61	0.62	0.59
First stage, dependent variable: Institutional Factor			
SW multivariate F test of excluded instruments	23.06	6.46	51.00
First stage, dependent variable: Institutional Factor × High-tech			
SW multivariate F test of excluded instruments	32.16	5.08	27.40
Kleibergen-Paap Wald rk F statistic	18.18	3.91	20.32

Notes: Standard errors in parentheses are clustered at the country-level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.20: Controlling for GDP p.c. and Taxes (Patent Shares)

	Institutional Delivery	Employment Protection	Starting a Business
	2SLS	2SLS	2SLS
	(1)	(2)	(3)
Institutional Indicator	0.461*	0.137	0.057
	(0.258)	(0.132)	(0.217)
Institutional Indicator×Patent Shares	0.132**	0.080**	0.047
	(0.062)	(0.036)	(0.051)
GDP per capita	-0.034*	-0.004	-0.002
	(0.018)	(0.006)	(0.007)
GDP per capita ×Patent Shares	-0.005	0.001	0.002**
	(0.004)	(0.001)	(0.001)
Corporate Tax Rate	-0.008	-0.015	-0.019
	(0.019)	(0.016)	(0.015)
Corporate Tax Rate ×Patent Shares	-0.000	0.001	0.000*
	(0.002)	(0.003)	(0.002)
Value added growth	0.444***	0.735***	0.569***
	(0.157)	(0.083)	(0.119)
Sector-FE	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes
Number of Observations	4323	3688	4314
R-squared	0.32	0.28	0.31
First stage, dependent variable: Institutional Factor			
SW multivariate F test of excluded instruments	21.02	4.57	32.48
First stage, dependent variable: Institutional Factor × Patent Share			
SW multivariate F test of excluded instruments	28.73	4.21	27.65
Kleibergen-Paap Wald rk F statistic	7.09	3.41	13.16

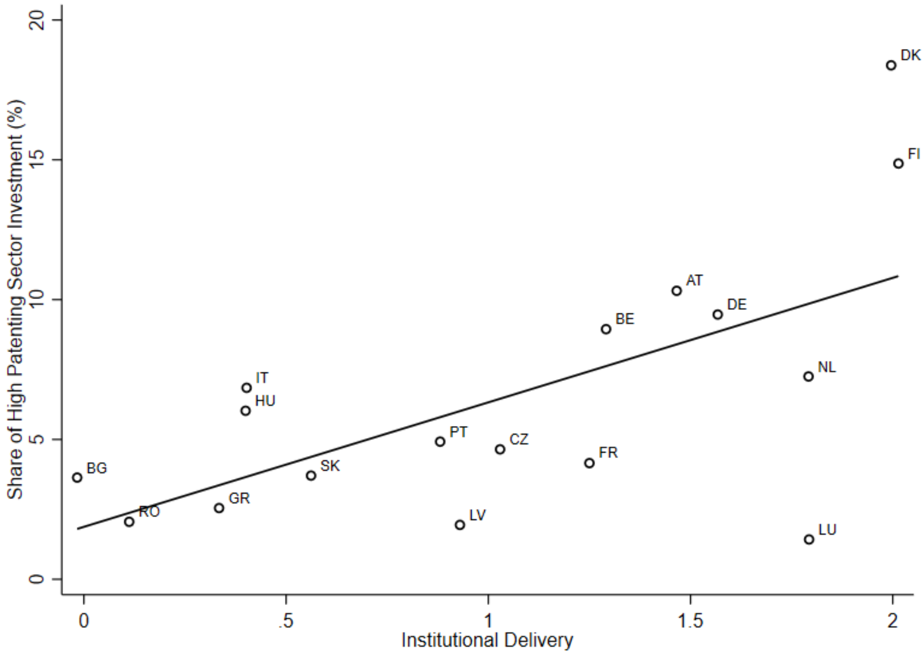
Notes: Standard errors in parentheses are clustered at the country-level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.21: Controlling for GDP p.c. and Taxes (AI Intensity)

	Institutional Delivery	Employment Protection	Starting a Business
	2SLS	2SLS	2SLS
	(1)	(2)	(3)
Institutional Indicator	-0.304***	-0.329**	-0.639***
	(0.115)	(0.146)	(0.137)
Institutional Indicator×AI Intensity	0.493	0.553	1.641***
	(0.358)	(0.355)	(0.457)
GDP per capita	0.004	0.001	0.004
	(0.003)	(0.003)	(0.003)
GDP per capita ×AI Intensity	0.020**	0.022***	0.009
	(0.010)	(0.006)	(0.012)
Corporate Tax Rate	0.000	-0.021	-0.012
	(0.012)	(0.016)	(0.011)
Corporate Tax Rate ×AI Intensity	0.045	0.075**	0.076***
	(0.031)	(0.038)	(0.027)
Value added growth	0.989***	1.192***	1.198***
	(0.202)	(0.251)	(0.217)
Sector-FE	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes
Number of Observations	7182	6038	7125
R-squared	0.60	0.60	0.59
First stage, dependent variable: Institutional Factor			
SW multivariate F test of excluded instruments	22.33	4.42	63.07
First stage, dependent variable: Institutional Factor × AI intensity			
SW multivariate F test of excluded instruments	20.03	3.55	55.88
Kleibergen-Paap Wald rk F statistic	13.54	3.82	36.67

Notes: Standard errors in parentheses are clustered at the country-level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 5.5: Institutional delivery and high patenting sector investment share in 2021.



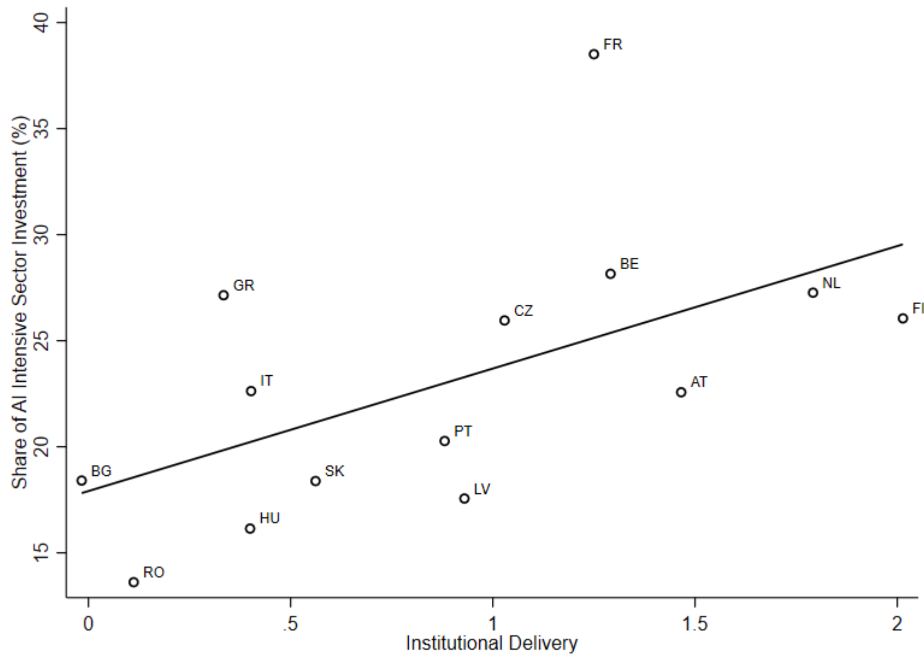
Note: For the purpose of this figure, we consider the 20 % of sectors with the highest patent shares to be high-patenting sectors. These are the sectors with NACE codes C21, C26 and C28. The figure includes all EU countries for which data on gross fixed capital formation is available for all three of these NACE codes in the EU KLEMS dataset.

Table 5.22: Results for Eurostat High-Tech Sector Classification (Value Added Share as Dependent Variable)

	Institutional Delivery	Employment Protection	Starting a Business
	2SLS (1)	2SLS (2)	2SLS (3)
Institutional Indicator	-0.095**	-0.225***	-0.309**
	(0.048)	(0.077)	(0.122)
Institutional Indicator × High-Tech	0.430**	0.759***	1.242***
	(0.189)	(0.201)	(0.358)
Value added growth	0.356***	0.482***	0.353***
	(0.100)	(0.116)	(0.101)
Number of Observations	12,137	9786	12,062
R-squared	0.77	0.78	0.77
First stage, dependent variable: Institutional Factor			
SW multivariate F test of excluded instruments	408.52	34.59	100,000.00
First stage, dependent variable: Institutional Factor × High-Tech			
SW multivariate F test of excluded instruments	76.90	30.19	558.50
Kleibergen-Paap Wald rk F statistic	489.30	18.30	359.03

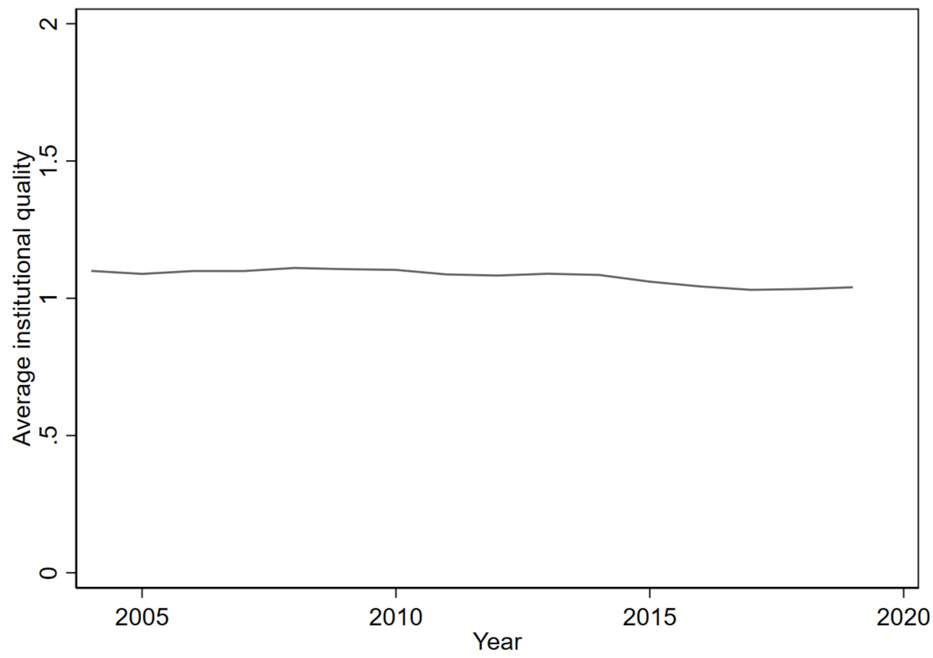
Notes: Standard errors in parentheses are clustered at the country level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 5.6: Institutional delivery and AI intensive sector investment share in 2021.



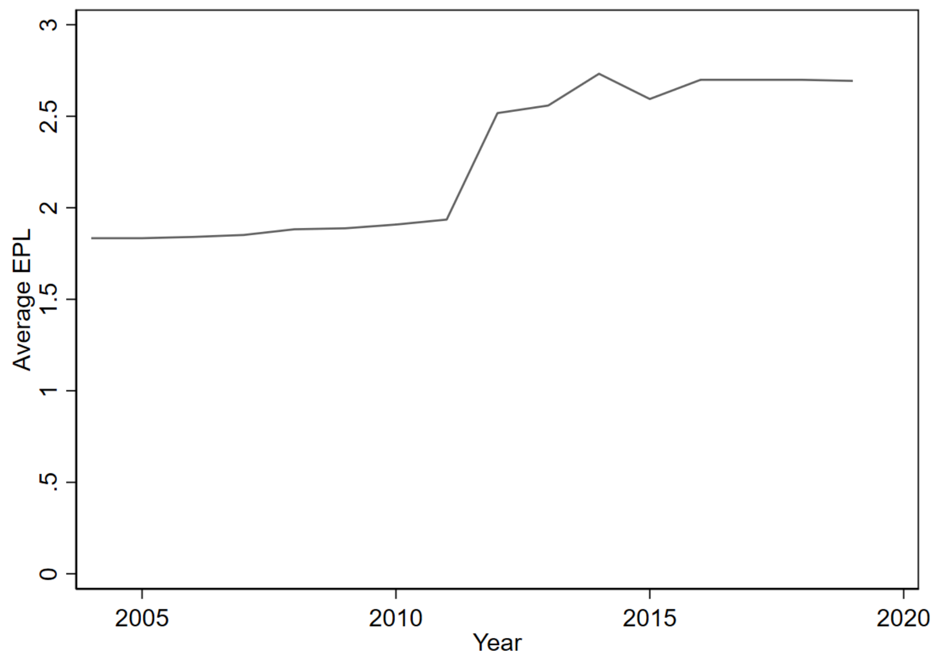
Note: The shares of AI intensive sector investment are calculated using data on gross fixed capital formation from Eurostat. We consider those sectors as AI intensive which are classified as highly AI intensive by Calvino et al. (2024). These include NACE sectors C26 (Manufacturing of computers) and J (Media and information and communication technology), K (Finance and insurance), M69–M71 (Legal, head office, architectural and engineering activities) and M72 (Scientific research and development). The figure includes the 14 EU countries for which data on gross fixed capital formation is available for all these NACE codes from Eurostat.

Figure 5.7: Average institutional delivery over time.



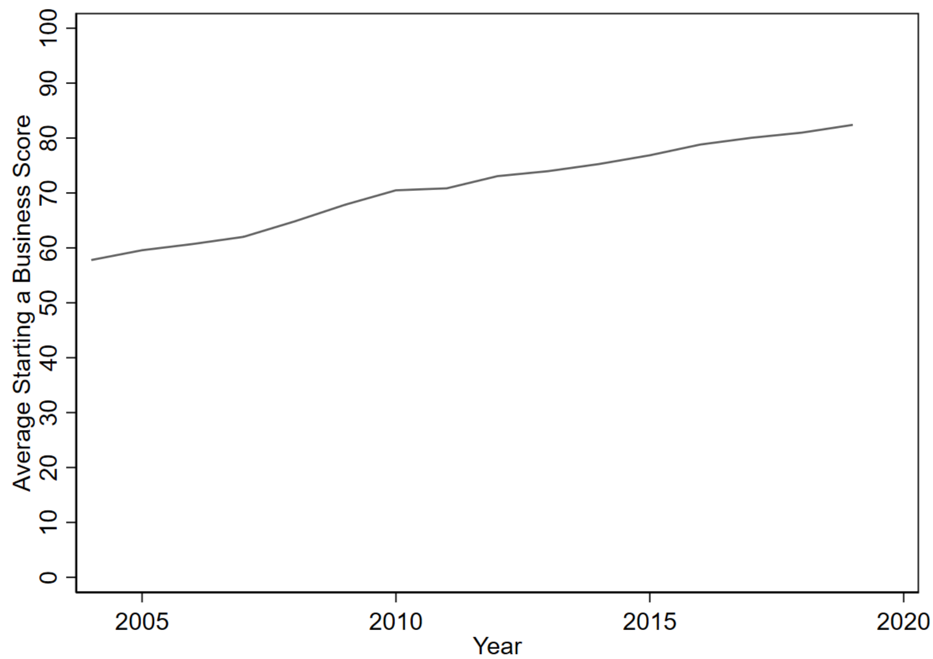
Note: Average of the institutional delivery index across all 27 EU countries from 2004 until 2019.

Figure 5.8: Average EPL over time.



Note: Average EPL across 24 EU countries from 2004 until 2019. There is no EPL data over this time period for Bulgaria, Cyprus and Romania. Estonia and Luxembourg are included from 2008 onwards. Slovenia is included from 2009 onwards. Latvia is included from 2012 onwards. Lithuania is included from 2014 onwards. Croatia is only included in 2015.

Figure 5.9: Average Starting a Business Score over time.



Note: Average Starting a Business Score across 27 EU countries from 2004 until 2019. Luxembourg and Malta are included from 2007 onwards and Cyprus is included from 2009 onwards.

Table 5.23: Results for Patent Shares (Value Added Share as Dependent Variable)

	Institutional Delivery	Employment Protection	Starting a Business
	2SLS (1)	2SLS (2)	2SLS (3)
Institutional Indicator	-0.038	0.203	-0.065
	(0.096)	(0.127)	(0.197)
Institutional Indicator × Patent Share	0.037***	0.039***	0.069***
	(0.009)	(0.010)	(0.026)
Value added growth	0.392***	0.538***	0.471***
	(0.134)	(0.133)	(0.165)
Number of Observations	5779	4656	5749
R-squared	0.66	0.64	0.60
First stage, dependent variable: Institutional Factor			
SW multivariate F test of excluded instruments	277.41	34.35	21225.87
First stage, dependent variable: Institutional Factor × Patent Share			
SW multivariate F test of excluded instruments	73.42	29.72	1863.30
Kleibergen-Paap Wald rk F statistic	203.89	18.93	1124.80

Notes: Standard errors in parentheses are clustered at the country level. The Patent Share is centered around its sample-mean so the estimate on the non-interacted institutional indicator is the estimated effect at the mean. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.24: Results for AI Intensity (Value Added Share as Dependent Variable)

	Institutional Delivery	Employment Protection	Starting a Business
	2SLS	2SLS	2SLS
	(1)	(2)	(3)
Institutional Indicator	-0.174**	-0.142	-0.441***
	(0.072)	(0.104)	(0.146)
Institutional Indicator × AI Intensive	0.662***	0.135	1.292***
	(0.227)	(0.381)	(0.402)
Value Added Growth	0.942***	1.201***	1.037***
	(0.151)	(0.174)	(0.180)
Number of Observations	9424	7596	9367
R-squared	0.77	0.77	0.77
First stage, dependent variable: Institutional Factor			
F test of excluded instruments	323.73	36.71	72551.97
SW multivariate F test of excluded instruments	351.42	34.37	80643.01
First stage, dependent variable: Institutional Factor × AI intensity			
F test of excluded instruments	371.31	29.65	71270.31
SW multivariate F test of excluded instruments	24.36	15.13	554.22
Kleibergen-Paap Wald rk F statistic	148.30	19.74	322.10

Notes: Standard errors in parentheses are clustered at the country level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.25: Results for Eurostat High-Tech Sector Classification (R&D Share as Dependent Variable)

	Institutional Delivery	Employment Protection	Starting a Business
	2SLS	2SLS	2SLS
	(1)	(2)	(3)
Institutional Indicator	-0.005**	-0.006***	-0.009***
	(0.002)	(0.001)	(0.003)
Institutional Indicator × High-Tech	0.020**	0.031***	0.046**
	(0.010)	(0.005)	(0.018)
Value added growth	-0.003	0.003	-0.002
	(0.005)	(0.004)	(0.005)
Number of Observations	7863	6757	7815
R-squared	0.51	0.53	0.48
First stage, dependent variable: Institutional Factor			
SW multivariate F test of excluded instruments	48.16	3.95	226.31
First stage, dependent variable: Institutional Factor × High-Tech			
SW multivariate F test of excluded instruments	105.33	35.61	482.92
Kleibergen-Paap Wald rk F statistic	39.03	2.05	147.68

Notes: Standard errors in parentheses, two-way clustered at the sector and country-year levels. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.26: Results for Patent Shares (R&D Share as Dependent Variable)

	Institutional Delivery	Employment Protection	Starting a Business
	2SLS (1)	2SLS (2)	2SLS (3)
Institutional Indicator	0.003	0.005**	-0.001
	(0.003)	(0.002)	(0.006)
Institutional Indicator × Patent Share	0.002	0.002**	0.003
	(0.001)	(0.001)	(0.002)
Value added growth	0.004	0.011***	0.006
	(0.005)	(0.004)	(0.004)
Number of Observations	3723	3326	3714
R-squared	0.34	0.33	0.29
First stage, dependent variable: Institutional Factor			
SW multivariate F test of excluded instruments	69.54	5.07	809.94
First stage, dependent variable: Institutional Factor × Patent Share			
SW multivariate F test of excluded instruments	50.65	5.77	192.32
Kleibergen-Paap Wald rk F statistic	96.94	3.73	161.28

Notes: Standard errors in parentheses are clustered at the country level. The Patent Share is centered around its sample-mean so the estimate on the non-interacted institutional indicator is the estimated effect at the mean. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.27: Results for AI Intensity (R&D Share as Dependent Variable)

	Institutional Delivery	Employment Protection	Starting a Business
	2SLS (1)	2SLS (2)	2SLS (3)
Institutional Indicator	-0.000	-0.001	-0.007
	(0.003)	(0.004)	(0.006)
Institutional Indicator × AI Intensity	0.011	0.013	0.028
	(0.009)	(0.013)	(0.020)
Value added growth	0.004	0.015	0.009
	(0.009)	(0.011)	(0.009)
Number of Observations	5772	5020	5721
R-squared	0.32	0.30	0.29
First stage, dependent variable: Institutional Factor			
SW multivariate F test of excluded instruments	57.12	7.17	183.68
First stage, dependent variable: Institutional Factor × AI intensity			
SW multivariate F test of excluded instruments	146.12	53.38	203.17
Kleibergen-Paap Wald rk F statistic	39.57	20.22	154.04

Notes: Standard errors in parentheses, two-way clustered at the sector and country-year levels. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.28: Results for the Resolving Insolvency Index

	(1) High-Tech Classification	(2) Patent Share	(3) AI Intensive
Resolving Insolvency	-0.243***	-0.144	-0.308***
	(0.072)	(0.152)	(0.093)
Resolving Insolvency × Innovative	1.036***	0.073***	1.213***
	(0.319)	(0.021)	(0.218)
Value added growth	0.480***	0.534***	1.015***
	(0.122)	(0.134)	(0.203)
Number of Observations	9267	4314	7125
R-squared	0.60	0.31	0.59
First stage, dependent variable: Institutional Factor			
SW multivariate F test of excluded instruments	30.45	27.58	27.65
First stage, dependent variable: Institutional Factor × Innovative			
SW multivariate F test of excluded instruments	27.15	28.82	22.04
Kleibergen-Paap Wald rk F statistic	21.83	21.02	11.60

Notes: Standard errors in parentheses are clustered at the country level. The Patent Share is centered around its sample-mean so the estimate on the non-interacted institutional indicator is the estimated effect at the mean. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.29: Results for Eurostat High-Tech Classification (Excluding Ireland and Luxembourg)

	Institutional Delivery	Employment Protection	Starting a Business
	2SLS (1)	2SLS (2)	2SLS (3)
Institutional Indicator	-0.184***	-0.214***	-0.414***
	(0.050)	(0.051)	(0.103)
Institutional Indicator × High-Tech	0.896***	1.087***	2.239***
	(0.224)	(0.196)	(0.548)
Value added growth	0.416***	0.499***	0.430***
	(0.122)	(0.085)	(0.114)
Sector FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Number of Observations	8867	7361	8867
R-squared	0.64	0.66	0.61
First stage, dependent variable: Institutional Factor			
SW multivariate F test of excluded instruments	51.09	2.74	247.21
First stage, dependent variable: Institutional Factor × High-Tech			
SW multivariate F test of excluded instruments	757.57	34.46	376.29
Kleibergen-Paap Wald rk F statistic	53.07	2.47	146.59

Notes: Standard errors in parentheses are clustered at the country-level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.30: Results for Patent Shares (Excluding Ireland and Luxembourg)

	Institutional Delivery	Employment Protection	Starting a Business
	2SLS (1)	2SLS (2)	2SLS (3)
Institutional Indicator	-0.053	0.203	-0.018
	(0.116)	(0.157)	(0.234)
Institutional Indicator × Patent Share	0.064***	0.077***	0.104
	(0.022)	(0.029)	(0.063)
Value added growth	0.526***	0.747***	0.646***
	(0.147)	(0.098)	(0.114)
Sector FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Number of Observations	4259	3636	4259
R-squared	0.34	0.27	0.23
First stage, dependent variable: Institutional Factor			
SW multivariate F test of excluded instruments	85.42	4.87	1069.05
First stage, dependent variable: Institutional Factor × Patent Share			
SW multivariate F test of excluded instruments	70.52	5.39	193.24
Kleibergen-Paap Wald rk F statistic	127.39	4.07	160.93

Notes: Standard errors in parentheses are clustered at the country-level. The Patent Share is centered around its sample-mean so the estimate on the non-interacted institutional indicator is the estimated effect at the mean. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.31: Results for AI Intensity (Excluding Ireland and Luxembourg)

	Institutional Delivery	Employment Protection	Starting a Business
	2SLS (1)	2SLS (2)	2SLS (3)
Institutional Indicator	-0.265***	-0.269*	-0.662***
	(0.096)	(0.143)	(0.172)
Institutional Indicator × AI Intensive	0.959***	0.535	2.244***
	(0.223)	(0.391)	(0.579)
Value Added Growth	0.815***	1.010***	1.079***
	(0.196)	(0.268)	(0.221)
Sector FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Number of Observations	6672	5604	6672
R-squared	0.64	0.65	0.61
First stage, dependent variable: Institutional Factor			
SW multivariate F test of excluded instruments	51.51	4.32	177.54
First stage, dependent variable: Institutional Factor × AI-intensive			
SW multivariate F test of excluded instruments	57.82	5.55	190.19
Kleibergen-Paap Wald rk F statistic	69.06	4.21	150.39

Notes: Standard errors in parentheses are clustered at the country level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Chapter 6

Conclusion

Institutions determine the incentive structures of individuals. The cooperation and coordination of individuals' actions, which can be induced or deterred by incentive structures, are important factors in enabling economic transactions between individuals. In the aggregate, the quality of institutions can thus affect observed economic outcomes.

This thesis examines the interplay between institutions and economic outcomes in various settings. The relation between institutions and investment is examined in two contexts. One is focused on developing countries with a differential analysis for natural-resource-endowed countries. The other focuses on EU countries and examines how institutions shape the allocation of investment to high tech sectors. Also investigated in this thesis are the international interdependence of institutional quality through trade flows and the translation of individual incentives into collective action.

The thesis highlights the important role institutions play in determining aggregate economic outcomes by shaping individual incentives. It is shown that institutions matter for investment patterns. Here, it is suggested that investment in economic activities involving a high degree of uncertainty, irreversibility, and innovation is particularly responsive to the design of the institutional framework. The institutions of a country are further shown to matter for the institutions of the countries with which it trades. The findings suggest institutional spillovers from importing OECD countries to exporting non-OECD countries. Lastly, the thesis shows how aggregate outcomes depend on individuals' incentive structures. For the collective action case of protest movements, it is shown how differences in the relative importance of incentives specific to an individual and selective incentives derived externally from other individuals can determine the number and stability of protest participation equilibria.

Across various empirical settings, institutions are shown to matter for economic outcomes because they affect individuals' uncertainty about others' behavior and can thus induce cooperation and coordination.

The findings of this thesis are of high relevance for the public policy debate in various contexts. They emphasize the importance of institutions for developing countries in attracting foreign investment to encourage economic growth. For EU countries, institutions are shown to be an important determinant of the sectoral composition of investment. It is suggested that efficient institutional frameworks are necessary for private investment to occur in innovative sectors that promise the greatest increases in productivity growth. Further, it is implied that trade between countries characterized by differing institutions can induce spillovers of institutional quality, a finding that is highly relevant for foreign policy and ethical questions of international trade. Lastly, the thesis shows the importance of network effects in collective action, in the context of protest movements. This highlights a mechanism through which selective incentives channel individual behavior and thus affect aggregate outcomes.

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