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Essays on Law, Migration, and Economic Growth

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To my family,
those I was lucky to get,
and those added along the way.

"When a person can't find a deep sense of meaning,
they distract themselves with pleasure"
- Viktor Frankl

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I find it somehow reductive to summarize six years of PhD work and life experience into a single book. Nevertheless, I believe, it is worthwhile taking the step, both to myself (by clarifying thoughts and learning from the past), and for the reader (to hopefully find something of value or of interest). This dissertation is a product of 6 meaningful and highly volatile years, where I had the honor to be in an environment of many great and passionate people. Ultimately, this unforgettable journey made me a better researcher and person.

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Contents

General Introduction

The last decade has seen a growing recognition of access to justice (ATJ) as one of the major challenges in the development of peaceful and prosperous societies for the coming years (United Nations, 2015; OECD and Open Society Foundations, 2016). A study conducted by the World Justice Project (2019) estimates that as much as 5.1 billion people – or approximately two-thirds of humanity – face issues related to ATJ. The obstacles to reaching justice are numerous, ranging from time and costs factors to discrimination of all kinds; they impede the accurate delivery of justice in civil, administrative, and criminal matters. This has direct and strong implications for peoples' everyday decisions in the most fundamental economic operations. Chapter 1 tries to fill that gap in the literature by showing that ATJ is an important determinant of economic growth. As stated by the Sustainable Development Goal number 16 of the United Nations (2015) and reasserted by the World Justice Project (2019), societies, both developing and developed, are facing a considerable gap in the justice sector. This ATJ problem is key and needs to be addressed to have better functioning market economies. Whenever and wherever justice is denied, this has an economic cost that is sizeable and not yet fully measured and understood (OECD and World Justice Project, 2019).

The rise of international migration in the recent decades has revived interest among economists, political scientists and legal scholars on the effect of immigration on the labor market outcomes and its institutions. An extensive literature measuring the effect of immigration on natives labor market outcomes finds that, on average, immigration has a small or null effect, depending on the methodology and the context of the analysis (see Edo (2019) for a recent survey of the literature). However, one systematic feature of the literature is that labor market regulation is assumed to be, either implicitly or explicitly, exogenous to the presence of immigration. To the best of our knowledge, this assumption has never been tested. The labor market regulation is usually argued to be implicitly accounted for with the inclusion of fixed-effects, and relatively few papers explicitly investigate the impact of immigration depending on the existing labor market regulations, such as the presence of minimum wages or the rigidity of labor contracts (Angrist and Kugler, 2003; D'Amuri and Peri, 2014; Edo, 2016; Edo and Rapoport, 2019; Bächli and Tsankova, 2020). Nonetheless, general concerns related to the implications of immigration on countries' institutions have been raised in response to the growing inflows of immigrants from a broader set of institutionally and culturally different countries (Collier, 2013; Borjas, 2015), and recent evidence shows that immigrants' historical origin-specific preferences influence natives' preferences and institutions (Giuliano and Tabellini, 2020). If immigration indeed impacts labor market regulation, then the *overall* labor market effect of immigration has still not been completely unveiled. The second chapter fills this gap by studying the impact of immigration on labor market regulation (in terms of both size and composition), using a dataset on 36 labor laws for 70 countries over a period of 40 years.

Between June 2006 and June 2016, the reintroduction of border controls was announced 62 times; 56% of these were related to expected events, 35% to refugees, and 8% to terrorism-related events (see Appendix Figure 3.7). The expected events include various political summits and conferences (G8 summits, NATO summits, or UN conferences), international sports competition, or demonstrations. The refugee-related border controls started in September 2015 in Germany, and seven other Schengen countries — Austria, Slovenia, Hungary, Sweden, Norway, Denmark, and Belgium — followed in the next months. The explosion of Schengen suspensions was also related to terrorist attacks. The terrorist attacks

in Paris (November 2015) and Brussels (March 2016) led several countries to re-establish border controls within the Schengen Area. Even though Schengen Member States had followed the mechanism foreseen in the Schengen Agreement in the past (Home, 2016), the increased geographical scope and duration of the restrictions led some analysts to portray the situation as the "dislocation," the "dismantling", or even the "death" of the Schengen Area (Sénat, 2016). The last chapter aims to provide first systematic evidence on the temporary reintroduction of border controls.

1

Access to Justice and Economic Development: Evidence from an International Panel Dataset

Co-authored with Arnaud Deseau (UCLouvain and Universitè Saint-Louis) and Michèle Schmiegelow (UCLouvain)

Abstract

This paper evaluates the importance of access to justice (ATJ) for economic growth. We create a new database on the number of professional judges per 100,000 inhabitants by collecting data from various public institutions. We draw on these data as a macro-level proxy capturing the fundamental and historical evolution of ATJ from 1970 to 2014 for a wide range of developed and developing countries. Using an instrumental variable approach in a dynamic panel setting to deal with endogeneity, we show that ATJ is a positive and significant determinant of economic growth. The substantial aggregate effect of ATJ is inversely related to the initial level of income per capita, human capital, democracy, economic freedom, and the rule of law. These results hint at a stronger impact of ATJ on economic development in less developed societies. In terms of mechanisms, our results suggest that ATJ promotes growth via higher government accountability and improved quality of institutions.

1.1 Introduction

The last decade has seen a growing recognition of access to justice (ATJ) as one of the major challenges in the development of peaceful and prosperous societies for the coming years (United Nations, 2015; OECD and Open Society Foundations, 2016). A study conducted by the World Justice Project (2019) estimates that as much as 5.1 billion people – or approximately two-thirds of humanity – face issues related to ATJ. The obstacles to reaching justice are numerous, ranging from time and costs factors to discrimination of all kinds; they impede the accurate delivery of justice in civil, administrative, and criminal matters. This has direct and strong implications for peoples' everyday decisions in the most fundamental economic operations. Consequently, such obstacles to justice generate substantial economic costs and slows down economic development in both developed and developing societies.[1] The judiciary, both as a source of judge-made law and as a legal enforcement institution, shapes the incentives of economic actors and actively participates in the enforcement of contracts and the protection of property rights, which are recognized as the cornerstones of a well-functioning market economy (North, 1990; Acemoglu et al., 2005). Although existing contributions to the literature have stressed the importance of legal codes (La Porta et al., 2008) as well as effective or *de facto* judicial institutions for economic development (Djankov et al., 2003; Jappelli et al., 2005; Chemin, 2009a,b; Visaria, 2009; Chemin, 2012; Feld et al., 2015), surprisingly little is known about the structural importance of ATJ for economic development.

This paper fills that gap in the literature by showing that ATJ is an important determinant of economic growth. To do so, we construct a new database on the number of professional judges per 100,000 inhabitants by combining data from various public institutions. We use these data as a macro-level proxy capturing the fundamental and historical evolution of ATJ between 1970 and 2014 for a wide range of developed and developing countries. In a dynamic panel setting using an instrumental variable approach to deal with endogeneity, we show that ATJ is a positive and significant determinant of economic growth. The benchmark estimates imply that increasing ATJ by 1% increases the five-year growth rate of GDP per capita by 0.7pp. This substantial aggregate effect is inversely related to the initial level of GDP per capita, human capital, democracy, economic freedom, and rule of law, meaning that less developed societies are the ones that can gain the highest economic returns from improving ATJ. In terms of mechanisms, our results suggest that part of the effect of ATJ on growth comes from higher government accountability and quality of institutions. In particular, we show that ATJ negatively affects the share of government consumption in GDP, public corruption, and the shadow economy while it positively affects the protection of property rights and credit market regulations. These results are consistent with the existing literature showing that institutions and their quality is one of the main determinants for economic development (Acemoglu et al., 2005).

Our paper makes three primary contributions. First, we complement the existing literature on the effect of judicial institutions on economic development by providing the first study looking at the effect of ATJ on economic growth in a large and long panel of countries. Most of the existing studies evaluating the impact of ATJ-related measures on economic development are country-specific. For example, Lichand and Soares (2014) find that the creation of new special civil courts in Brazil foster entrepreneurship by increasing the geographic proximity and improving the speed of the justice system. Espinosa et al. (2018) evaluate a reform that closed a quarter of French labor courts, and show that cities which experienced an increase in the distance to their associated labor courts were creating less jobs and less firms, compared to unaffected cities. Our findings in an international setting are also complementary to country-specific studies revealing the importance of court effectiveness for entrepreneurship, credit, agricultural, and industrial activities (Jappelli et al., 2005; Chemin, 2009a,b; Visaria, 2009; Chemin, 2012; Amirapu, 2021). Our research is also related to other papers explaining cross-country differences in GDP per capita with *de facto* judicial indicators. For instance, Berkowitz et al. (2003a) find that the

[1] A study by OECD and World Justice Project (2019) estimates that the costs generated by legal problems are between 0.5% to 3% of GDP in most countries.

way a legal code is implemented matters more to national economic development than the legal family of the code. Feld and Voigt (2003) and Feld et al. (2015) show that *de facto* judicial independence is significantly correlated with economic growth while mere *de jure* judicial independence is not.

Second, we contribute to the existing literature by creating a new database on the number of professional judges per 100,000 inhabitants for a sample of 105 countries from 1970 through 2014. We draw on these data as a new structural indicator of ATJ, proxying its historical evolution at the country level. Compared to available measures of ATJ, our proxy has the great advantage of having a much longer time dimension, enabling us to capture and study the effect of long variations in ATJ. This is crucial for identification, as we exploit the within-country variation in ATJ to estimate its effect on economic development. We show that our new ATJ indicator is highly correlated with the more comprehensive (but shorter in the time dimension and using mostly qualitative data) access to civil justice score of the World Justice Project (2020); while it is not correlated with other dimensions of judicial performance at the country-level (such as the independence of the judiciary or the rule of law). We also produce a series of new stylized facts from our database, highlighting sizeable differences in regional trends as well as in the levels of the density of judges across countries.[2] The new dataset has the advantage of using only publicly available data sources with quantitative measures of ATJ, as opposed to relying only on qualitative measures made by legal experts opinions. The public data sources include various public institutions (national statistical offices, public archives, ministries of justice, and supreme courts), academic publications, and international organizations (UNOV, CEPEJ, UNODC, OAS, and Eurostat).[3]

Third, we advance the existing cross-country literature on the impact of *de facto* judicial institutions on economic development by addressing endogeneity issues in a more comprehensive way. Most of the studies rely solely on a fixed-effects estimator to evaluate the impact of various *de facto* judicial changes on economic development. This strategy solves omitted variable bias to a certain extent, but it does not address the reverse causality bias stemming from high correlation between economic development and *de facto* judicial institutions. Taking advantage of the long panel dimension of our new database, we use a difference-GMM approach (Arellano and Bond, 1991) which relies on first-differences and internal instrumentation to deal with endogeneity. As an additional piece of evidence, we use "politically motivated increase in the number of judgeships" from the V-Dem dataset (Coppedge et al., 2019) as an external instrument for ATJ, and we find similar results.

The rest of the paper is organized as follows. Section 1.2 discusses and tests the validity of our proxy for ATJ and describes the novel dataset on the number of judges along with stylized facts. Section 1.3 presents our identification strategy and discusses the main challenges for the empirical analysis. Section 1.4 presents our main results, robustness checks, heterogeneity, and the transmission channels through which ATJ can affect growth. Section 1.5 presents our conclusion.

1.2 Access to Justice: New Dataset and Stylized Facts

This section is organized in two parts. In Section 1.2.1, we discuss the concept of ATJ, explain how we measure it at a macro level and show that our proxy is both conceptually and empirically valid at that level. Then, in Section 1.2.2 we explain how we create our new ATJ dataset, and provide stylized facts on ATJ.

1.2.1 Theory and Measurement

ATJ is a broad and multidimensional concept. It is not surprising, therefore, that there is no commonly accepted definition of ATJ among legal scholars. Nevertheless, we can identify several core elements pertaining to the concept: (i) *Legal capability* – a citizen needs to know the law, legal procedures, and which person to contact in order to introduce a lawsuit or defend oneself against one; (ii) *Availability of*

[2]For brevity, we will sometimes refer to "judges" instead of "professional judges".

[3]See Appendix Table 1.9 for a detailed description of the sources used.

justice – a citizen needs to be able to rely on a sufficient and available judicial supply (infrastructures and staff); (iii) *Physical barriers* – a citizen needs to have access to means of transport to a court of justice and his or her legal advisors; (iv) *Social barriers* – a citizen needs to be free of the fear of incurring any discrimination during the judicial process; (v) *Cost* – a citizen needs to be able to pay the monetary cost of the procedure, to support the opportunity cost of the procedure and to endure the emotional cost of the procedure (see Barendrecht et al. (2006), Botero and Ponce (2011) and OECD and Open Society Foundations (2019) for a complete discussion about ATJ and its economic dimensions). Overall, this multidimensionality makes ATJ difficult to quantify due to the need to collect, aggregate, and make weighting decisions for many variables.[4] Moreover, even though various datasets on ATJ are available at the national level, there are no such datasets including both a large cross-country aspect and a long time dimension.[5] One example is the Rule of Law Index from The World Justice Project, which has access to civil justice as one of its sub-components. Despite a rich cross-country dimension, the time dimension for ATJ is short in that case, as data are available only from 2015. In addition, ATJ is very persistent on such a narrow time interval, making it hard to study potential effects on economic development.

In this paper, we focus on a specific and structural indicator of ATJ, i.e. the opportunity for citizens to have access to the judiciary. We measure and argue that the number of judges per 100,000 inhabitants is a good proxy for capturing the fundamental and historical evolutions of ATJ at the country-level. A higher number of judges relative to population directly increases the available judicial supply (component ii) and general awareness of the law (component i). An increase in the density of judges can also coincide with the creation of new courts, influencing the accessibility (iii), and cost dimensions (v) of ATJ. Our ATJ proxy has the advantage of being widely available and comparable across countries and through time by the fact that judges have the universal role of supplying justice: they resolve disputes in a court of law across all legal systems. Data on the number of judges we use are gathered from official sources such as public institutions, international organizations, and academic publications (see Section 1.2.2 and Appendix 1.6.2 for more details).

Staats et al. (2005) lists five dimensions, including ATJ (the accessibility dimension), that one can measure to assess judiciary performances: (i) *independence*, (ii) *accountability*, (iii) *efficiency*, (iv) *effectiveness*, and (v) *accessibility*. Because of judges' central position in the judicial decision-making process, one concern is that our proxy might reflect other judicial characteristics at the country-level, rather than just ATJ. We first empirically validate our proxy showing that there is a robust correlation between the density of judges and the accessibility dimension, while not being correlated with the other dimensions of judicial performance. To do so, Table 1.1 looks at the cross-country correlation between the number of judges per 100,000 inhabitants and variables capturing the aforementioned dimensions of judicial performance identified by Staats et al. (2005). Panel B, column 5 reveals a highly significant and positive relationship between our proxy of ATJ and the access to civil justice as measured by the World Justice Project for the year 2014.[6] Figure 1.1 depicts that relationship. Our proxy explains as much as one-third of the cross-country variation in access to civil justice. In the subsequent columns, we control for potential confounders such as income per capita, government spending, political regime, human capital, legal origin or geographical characteristics. In each case, the correlation between our proxy and the access to civil justice as measured by The World Justice Project remains robust. At the same time, we find that our ATJ proxy is not robustly correlated across other dimensions of judicial performance. Following Staats et al. (2005) classification, we find no robust correlation between the

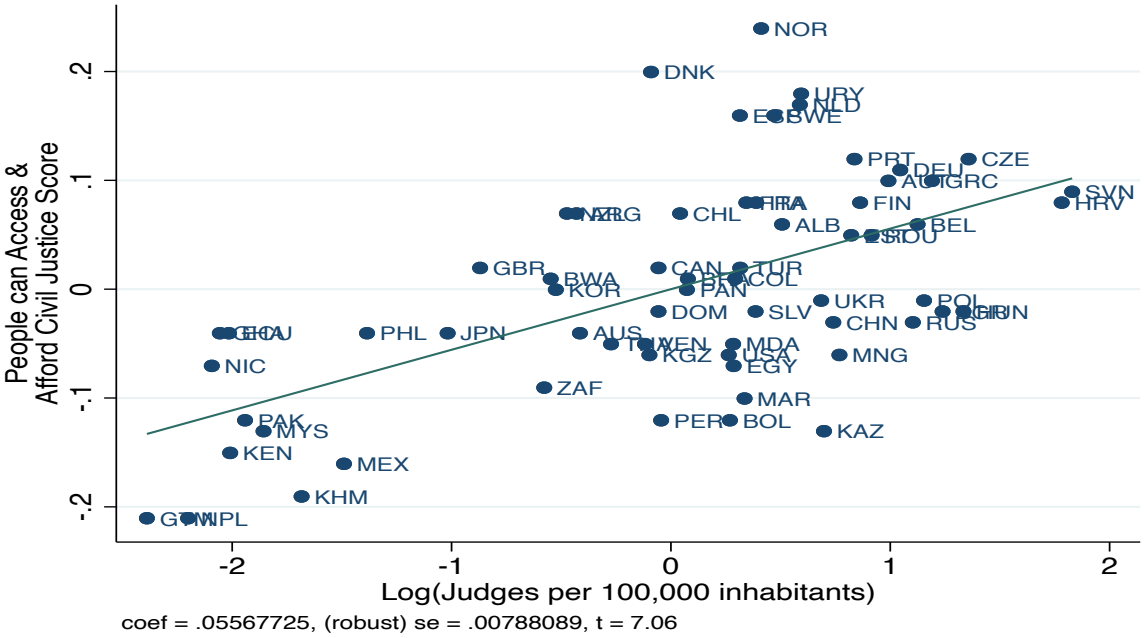
[4]The OECD published a note mapping 14 distinct dimensions and more than 40 sub-dimensions for a comprehensive ATJ indicator (OECD and Open Society Foundations, 2019).

[5]One of the main reasons is that the methodology used for each national survey is different, limiting the possibility of cross-country comparisons. See OECD and Open Society Foundations (2019) for a discussion about the most recent legal needs surveys.

[6]The World Justice Project defines access to civil justice as: "the accessibility and affordability of civil courts, including whether people are aware of available remedies; can access and afford legal advice and representation; and can access the court system without incurring unreasonable fees, encountering unreasonable procedural hurdles, or experiencing physical or linguistic barriers" (World Justice Project, 2020).

density of judges and independence (Panel A - columns 1-4), accountability (Panel A - columns 5-8) or effectiveness (Panel B - columns 1-4) of the judiciary.[7] We also find no robust correlation between our proxy and two measures of the overall quality of the judiciary from the World Bank (Panel C - columns 1-8).

Figure 1.1. Correlation between World Justice Project's Access to Civil Justice Index and the Density of Judges



Note: This figure plots the relationship between log number of professional judges per 100,000 inhabitants (averaged between 2000-2014) and the access to civil justice score of *The World Justice Project* (year 2014). This adjusted partial residual plot is based on the OLS regression in Table 1, Panel B, column 5.

By focusing our analysis on the density of judges, we essentially measure access to a formal court of justice and not alternative or informal dispute resolution mechanisms. Informal justice and out-of-court settlements play an important role in some developing and developed countries (Galanter, 1981; Platteau, 2000). This is a possible concern as we could underestimate the level of ATJ with our proxy in some countries. However, many alternative dispute resolution mechanisms, such as mediation, require court-backed enforcement and ultimately the intervention of formal justice. In the context of competition between informal and formal legal institutions, Aldashev et al. (2012) show that formal laws can act as an outside anchor, or a "magnet," moving the custom in a favorable direction for the marginalized groups: if access to formal justice is improved, access to informal justice will improve as well. For our empirical results, this means that the presence of informal justice will lead only to an attenuation bias on the estimated effect of ATJ on economic growth, as we tend to underestimate the level of ATJ in a given country.

Due to data availability, we focus on judges in all types of courts (first instance, second instance, and supreme courts) and dealing with all types of cases (civil, criminal, and administrative). This is not a concern for our analysis for several reasons. Firstly, judges often act as generalists: they adjudicate all relevant cases of civil, criminal, and public law. Only in large courts and developed economies do judges tend to specialize in one specific domain (e.g., marital matters for civil cases). Secondly, we cannot say a priori that judges in specific types of courts, dealing with certain types of cases, are not important

[7] We do not include a measure of judicial efficiency as there are no data available for a sufficient number of countries. For a complete discussion on judicial efficiency and judicial effectiveness, see Marciano et al. (2019).

Table 1.1. The Link between the Density of Judges and Judicial Performances

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A	**Judicial Independence**				**Corruption Free Judiciary**			
$log(Judges\ per\ 100k\ inh.)$	0.036*	-0.045	0.005	0.002	0.082***	-0.027	0.026	0.040*
	(0.021)	(0.028)	(0.025)	(0.027)	(0.020)	(0.023)	(0.021)	(0.024)
$log(GDP_{pc})$		0.192***	0.105***	0.109***		0.247***	0.218***	0.213***
		(0.020)	(0.032)	(0.028)		(0.017)	(0.024)	(0.024)
$Gov.\ Cons.$		0.049	-1.108**	-1.149**		0.243	-0.594	-0.651
		(0.513)	(0.456)	(0.461)		(0.491)	(0.501)	(0.478)
$Polity2$			0.013***	0.021***			0.007	0.015***
			(0.005)	(0.006)			(0.004)	(0.005)
$Schooling$			-0.018	-0.014			-0.028***	-0.019**
			(0.012)	(0.011)			(0.009)	(0.009)
Observations	64	64	64	64	64	64	64	64
adj. R-sq	0.02	0.49	0.69	0.71	0.11	0.73	0.83	0.85
PANEL B	**Log(Time Enforcing Contract)**				**Access to Civil Justice**			
$log(Judges\ per\ 100k\ inh.)$	-0.039	0.056	0.137**	0.114	0.056***	0.034***	0.036**	0.038**
	(0.046)	(0.054)	(0.068)	(0.072)	(0.008)	(0.013)	(0.015)	(0.018)
$log(GDP_{pc})$		-0.100***	-0.025	-0.032		0.059***	0.037**	0.036**
		(0.037)	(0.062)	(0.066)		(0.011)	(0.014)	(0.015)
$Gov.\ Cons.$		-1.951**	-0.780	-0.500		-0.130	-0.190	-0.337
		(0.889)	(0.909)	(1.003)		(0.284)	(0.282)	(0.283)
$Polity2$			0.004	-0.004			0.005	0.006*
			(0.008)	(0.010)			(0.003)	(0.003)
$Schooling$			-0.052*	-0.052			-0.002	0.000
			(0.029)	(0.032)			(0.006)	(0.006)
Observations	83	83	83	83	64	64	64	64
adj. R-sq	-0.00	0.06	0.16	0.20	0.33	0.55	0.65	0.63
PANEL C	**Rule of Law (WB)**				**Quality of Judicial Processes**			
$log(Judges\ per\ 100k\ inh.)$	0.338***	-0.049	0.035	0.046	1.041***	0.128	0.048	0.206
	(0.076)	(0.087)	(0.097)	(0.080)	(0.247)	(0.339)	(0.379)	(0.382)
$log(GDP_{pc})$		0.781***	0.511***	0.561***		1.400***	0.352	0.352
		(0.083)	(0.089)	(0.090)		(0.336)	(0.519)	(0.515)
$Gov.\ Cons.$		1.011	-2.335*	-2.704**		10.495	7.146	3.004
		(1.625)	(1.257)	(1.271)		(7.318)	(8.430)	(8.680)
$Polity2$			0.040***	0.054***			0.018	0.079
			(0.013)	(0.017)			(0.087)	(0.114)
$Schooling$			-0.057	-0.040			0.250	0.252
			(0.040)	(0.037)			(0.225)	(0.226)
Observations	83	83	83	83	83	83	83	83
adj. R-sq	0.10	0.62	0.76	0.80	0.11	0.27	0.33	0.31
Legal origin controls	No	No	Yes	Yes	No	No	Yes	Yes
Geographical controls	No	No	Yes	Yes	No	No	Yes	Yes
Continent fixed effects	No	No	No	Yes	No	No	No	Yes

Note: This table presents OLS estimates of the relationship between the density of judges and different measures of judicial performance at the country-level as defined by Staats et al. (2005). In all panels, the density of judges is defined as log number of professional judges per one hundred thousand inhabitants. Panel A uses an indicator of judicial independence (columns 1-4) and corruption free judiciary (columns 5-8) as dependent variables to assess the (i) independence and the (ii) accountability dimensions. Both indicators are from Gutmann and Voigt (2018). Panel B uses an indicator of judicial effectiveness defined as log number of days required to enforce a contract (columns 1-4) and an indicator of access to civil justice (columns 5-8) as dependent variables to assess the (iv) effectiveness and the (v) accessibility dimension. Data come respectively from World Bank's Doing Business indicators and from The World Justice Project - Rule of Law Index both in year 2014. Panel C uses proxies of the overall quality of justice defined as the rule of law index (columns 1-4) and a composite index of judicial quality (columns 5-8) as dependent variables. Indicators come respectively from the World Bank's Governance Indicators and the World Bank's Doing Business in year 2014 and 2018. Column 1 only includes log judges per 100,000 inhabitants as regressor. Column 2 adds log GDP per capita and the share of government consumption in GDP. Columns 3 adds Polity2 score of democracy, years of schooling, legal origin, and geographical characteristics (absolute latitude, total land area, standard deviation of elevation, and mean distance to coast or river). Finally, column 4 adds continent dummies. See Appendix 1.6.1 for further information on the variables. Standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

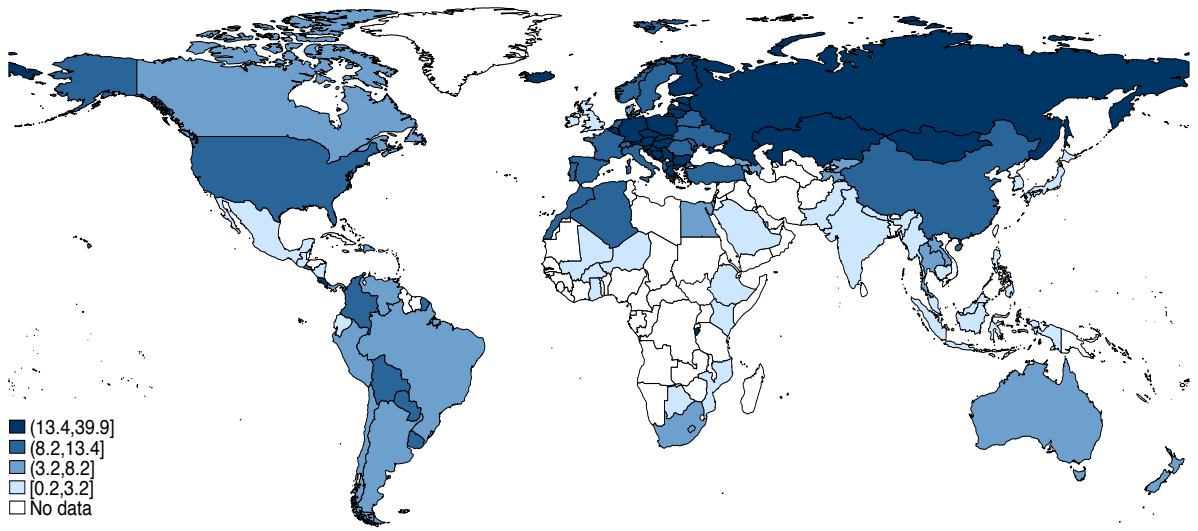
for economic growth. However, we can argue that judges dealing with civil cases are potentially the most important for economic development, since civil cases represent the majority of all legal cases, and they are directly related to economic matters (Pleasence et al., 2013). As previously discussed, Table 1.1 shows that our proxy effectively captures access to civil justice as measured by the World Justice Project, which is reassuring.

In our empirical exercise, we focus on the quantity rather than the quality of judges as measured by judges' education and experience. This can be a problem as the quality of judges can impact simultaneously ATJ and affect economic growth (through higher court productivity) (Ramseyer, 2012; Bielen et al., 2018). We tackle this omitted variable problem by explicitly controlling for the mean years of schooling in our regressions, and by including country fixed effects to capture time-invariant country specificities in terms of judges' education or training.

1.2.2 Data and Stylized Facts

We create a new database on the density of judges by collecting data from various public institutions. First, we gather data from ministries of justice, supreme courts and national statistical offices. We consider these sources as the most reliable, given that national institutions are often the first issuers of judicial data. As the second most reliable data source category, we consider international organizations that are long recognized for their expertise on various judicial indicators such as the United Nations Office in Vienna (UNOV), the Commission Européenne pour l'Efficacité de la Justice (CEPEJ), the United Nations Office on Drugs and Crime (UNODC), the Organization of American States (OAS), and the European Statistical Office (Eurostat). As the third most reliable data source category, we use individual academic publications. We merge these three data source categories and take care of the potential spurious variation implied by pooling different sources together. As a result, we end up with an unbalanced panel of judges for 105 countries from year 1970 through 2014. In Appendix 1.6.2, we provide a complete description of the merging process, sources and definitions (Appendix Table 1.9), and descriptive statistics (Appendix Table 1.10).

Figure 1.2. Average Density of Judges around the World between 1970-2014

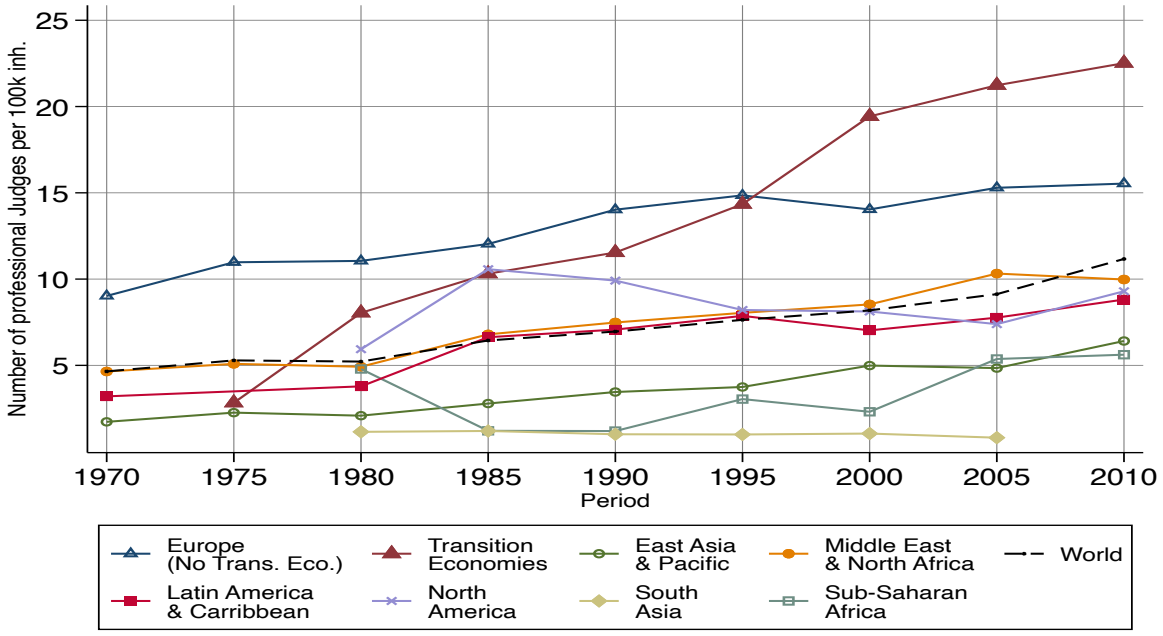


Note: The map presents the country-level distribution of the number of professional judges per 100,000 inhabitants (averaged between 1970-2014) for 105 countries. Each color represents a quartile from the first (light blue) to the fourth (dark blue).

Figure 1.2 gives an overview of our database by plotting the average density of judges over the whole 1970-2014 period by quartiles. We can see two things: (1) the 105 countries for which we managed to collect at least one country-year observation, and (2) the high cross-country variation in the average number of judges per 100,000 inhabitants. The highest average density is reached in Montenegro with 40

judges per 100,000 inhabitants, while the lowest density is in Ethiopia with only 0.25 judges per 100,000 inhabitants. Even within-continents, we observe sizeable variations. In Europe, for example, we find both countries belonging to the top quartile (e.g., Germany or Serbia) and countries belonging to the bottom quartile (e.g., United Kingdom or Ireland). An interesting finding is that the top decile of the distribution is composed almost entirely of central and southeastern European countries.[8] Cross-country variations in the density of judges are driven by differences in factors such as GDP per capita, legal origin, culture, and ethnic composition of the population.[9] The legal origin is one key variable that helps us understand the systematic difference in the density of judges between two countries – for instance Germany (civil law) and the United Kingdom (common law) – that have similar income per capita, population size and level of democracy.

Figure 1.3. The Evolution of the Density of Judges across Regions between 1970-2014



Note: This figure plots trends of the number of professional judges per 100,000 inhabitants between 1970-2014 in 5-year averages. The graph covers 105 countries categorized into eight regions plus the World. Country classifications are identical to the World Bank with the exception of Middle East & North Africa region for which we include Cyprus and Turkey, and the additional group of Transition Economies that are composed of Albania, Armenia, Azerbaijan, Belarus, Bosnia and Herzegovina, Bulgaria, Croatia, Czech Republic, Estonia, Georgia, Hungary, Kazakhstan, Kyrgyz Republic, Latvia, Lithuania, Macedonia, Moldova, Montenegro, Poland, Romania, Russia, Serbia, Slovak Republic, Slovenia, Tajikistan, and Ukraine, following the IMF (2000) classification.

Figure 1.3 depicts the time variation (in five-year averages) of the density of judges across different groups of countries and the world average. Even though during a period of 45 years (from 1970 through 2014) the world population has increased, the number of judges has increased even more. The world average of the number of judges per 100,000 inhabitants has more than doubled, increasing from 4.65 to 11.17 during the analyzed period. This doubling was achieved at a quite stable growth rate of 2.2% per year on average and with clear patterns across different regions. We confirm the significant heterogeneity in the average levels that we saw in Figure 1.2, where Europe and Transition Economies have the highest values, whereas East Asia & Pacific, Sub-Saharan Africa as well as South Asia have the lowest levels.

[8]The top decile accounts for more than 21.8 judges per 100,000 inhabitants and is composed of the following countries: Bosnia and Herzegovina, Croatia, Czech Republic, Germany, Hungary, Luxembourg, Macedonia, Montenegro, Serbia, Slovak Republic, and Slovenia.

[9]See Appendix Table 1.18 for more details on the determinants and correlates of the density of judges in our sample.

What is key for our identification strategy, is that we have significant differences in the growth rates of the density of judges, ranging from an threefold increase in East Asia & Pacific, to stagnation in South Asia.

Starting with Transition Economies, we document a remarkable doubling in the density of judges. Transition economies started from a density level close to the rest of European countries in the 1990 period, and reached the highest level among all the analyzed regions in the 2010 period with 22.51 judges per 100,000 inhabitants. The post-1990 doubling is mainly driven by two outcomes of the fall of the Soviet Bloc in 1991: (1) the new institutional framework and the transition to market economy created a demand for new courts and more judges;[10] (2) the population declined or stagnated in all Transition Economies since the 1990's. Considering only pre-1990 data should be treated with caution as the regional averages rely on less observations and countries compared to the post-1990 period.

The second most dynamic region is East-Asia & Pacific, where the density of judges has more than tripled, increasing from 1.73 in 1970 to 6.41 in the 2010 period. This is remarkable as the population has more than doubled during the same period in that region. Such a high investment in the justice sector was possible due to strong and stable economic growth since the 1980s, especially for East-Asian countries.

Four regions have grown in the pace of the world average, approximately doubling their density levels between the 1970 and 2010 period: Europe (excluding Transition Economies), Middle East & North Africa, Latin America & the Caribbean, and North America. European countries have the second highest level of density of judges in 2010, whereas Middle East & North Africa and Latin America & the Caribbean reached the world average levels during the considered period. As the North America country group is composed of only two countries (Canada and the United States), some variations are mechanical due to the unbalanced nature of our panel. Looking at the dates for which we observe both countries in North America (1980, 1995, and 2000), the level and trend are very close to the world average.

The remaining two regions (Sub-Saharan Africa and South Asia) have noted an overall stagnation. Sub-Saharan Africa has stagnated at around three judges per 100,000 inhabitants over the period 1980-2010. However, the pre-2000 trend relies on few countries due to data availability. Looking at the post-2000 period, we observe more countries, and we find that the density of judges has more than doubled, increasing from 2.31 to 5.62 judges per 100,000 inhabitants. As for South Asian countries (India, Nepal, and Pakistan), they have stagnated with the lowest density of judges with approximately one judge per 100,000 inhabitants. It is interesting to see the divergence between South Asia and East-Asia & Pacific, as the two regions had a similar density of judges in 1980. Even though both groups experienced similar population growth rates, East-Asian & Pacific countries managed to achieve a fivefold increase in the density of judges by 2005 compared to South Asian countries.

1.3 Empirical Strategy

Our goal is to empirically evaluate the impact of ATJ on economic development. We use a dynamic linear panel specification that is commonly used in the economic growth literature (Islam, 1995; Caselli et al., 1996; Barro, 2015; Acemoglu et al., 2019), and we adapt it to our setting with ATJ as the variable of interest. Our estimation strategy uses 5-year averages (nine time periods from 1970 through 2010) of all variables to smooth out the short-run fluctuations and to handle the annual gaps in the data. Furthermore, all our explanatory variables are lagged by one 5-year period as it takes time, particularly for ATJ, before the effect on economic activity materializes. Specifically, we focus on the following equation:

$$\ln\left(\frac{y_{i,t}}{y_{i,t-1}}\right) = \beta \ln(y_{i,t-1}) + \gamma \ln(ATJ_{i,t-1}) + \boldsymbol{\theta}' \boldsymbol{X}_{i,t-1} + \delta_t + (\alpha_i + \varepsilon_{i,t}) \tag{1.1}$$

[10] In Russia, for example, after the fall of the USSR a constitutional court and commercial courts have been created (Hendley, 2007). Similarly, after declaring independence from Yugoslavia in 1991, Slovenia has created its new first instance courts and administrative courts (Dimitrova-Grajzl et al., 2012).

where $i = 1, ..., N$ indicates a country and $t = 1, ..., T$ is a 5-year time period. Our left-hand side corresponds to a country's 5-year growth rate, with y denoting real GDP per capita. On the right-hand side, we have lagged GDP per capita y_{t-1}; our variable of interest $ATJ_{i,t-1}$ measured by the number of professional judges per 100,000 inhabitants; a vector of standard growth controls $\boldsymbol{X}_{i,t-1}$ such as schooling, investment, government expenditure, and political regime; a time fixed effect δ_t; a country fixed effect α_i; and an idiosyncratic error term $\varepsilon_{i,t}$. Our coefficient of interest is γ which captures the medium-run (5-year) effect of ATJ on economic growth.

Estimating equation (1.1) by OLS would produce inconsistent estimates due to endogeneity issues. First, ATJ and economic prosperity can be jointly affected by a third variable (e.g., government spending or political regime), leading to an omitted variable bias. Equation (1.1) treats part of the omitted variable problem by including country fixed effects which control for all time-invariant country characteristics affecting both economic growth and ATJ such as legal origin, culture, structural criminality, or geography. On top of country fixed effects, the vector $\boldsymbol{X}_{i,t-1}$ controls for time-varying growth determinants such as human capital, investment, government expenditures, and the level of democracy, further reducing the omitted variable bias. In addition, the inclusion of time fixed effects allows us to capture the world economy trends, global shocks, and the business cycle effects. Second, we face a reverse causality issue, as citizens with higher incomes have better ATJ. We mitigate this problem by lagging the ATJ variable in our specification. Another issue is the presence of a dynamic panel bias of order $1/T$, known as the Nickel bias (Nickell, 1981). In our case, this is a concern as we have a relatively short number of periods (T = 9).

To address the aforementioned biases, we estimate equation (1.1) using a two-step difference Generalized Method of Moments (GMM) (Arellano and Bond, 1991). This estimator differences away time-invariant country fixed effects, and relies on internal instrumentation to identify a plausible causal effect. Internal instruments are lagged levels of the endogenous variables used to instrument their current differences. The consistency of difference GMM crucially depends on valid moment conditions where instruments are not correlated with differenced error term, i.e. $\mathbb{E}\left[\left(\varepsilon_{i,t} - \varepsilon_{i,t-1}\right) ATJ_{i,s}\right] = 0 \; \forall \; s \leq t-2$. This assumption imposes that levels of ATJ observed in the past decades are not correlated with current shocks on economic growth. As this assumption is a priori hard to evaluate, we adopt a conservative approach by treating as endogenous not only ATJ but also the lagged dependent variable and the other classical growth determinants.[11] In particular, we use distant lags (fifth to eighth) to instrument ATJ and other growth determinants, as they typically display time persistence and are more likely to violate difference GMM moment conditions. The maintained assumption is that levels of the endogenous variables observed 25 to 40 years ago are valid instruments. Throughout the paper, we also keep the same set of instruments for transparency and to avoid selecting "ad hoc" moment conditions for each specification.[12] More specifically, we focus on the following moment conditions:

$$\mathbb{E}\left[\left(\varepsilon_{i,t} - \varepsilon_{i,t-1}\right)\left(\ln(y_{i,t-j}), \ln(ATJ_{i,t-k}), \boldsymbol{X}_{i,t-k}\right)\right] = 0 \quad \text{for all } 2 \leq j \leq 6 \text{ and } 5 \leq k \leq 8. \tag{1.2}$$

To evaluate the quality of our estimates, we systematically report p-values of the Hansen (1982) test and the Arellano-Bond test for AR2, two standard tests to detect instrumentation problems in GMM. The Hansen test is heteroskedasticity robust, and it evaluates the joint exogeneity of all the instruments. The Arellano-Bond test for AR2 evaluates the absence of second order autocorrelation of residuals. This is crucial as the presence of autocorrelation of residuals can yield to the invalidity of the set of instruments. Roodman (2009) shows that a usual source of weak instrumentation in the GMM framework comes from using "too many instruments."[13] Following his suggestions, we tackle this issue by keeping the instrument

[11] Treating lag-dependent as endogenous is a standard choice in the growth literature using GMM estimations (Voitchovsky, 2005; Hauk and Wacziarg, 2009; Acemoglu et al., 2019).

[12] In Appendix Table 1.13 we show that our results are robust to different moment condition choices.

[13] In a difference GMM estimation, the number of instruments is quadratic in T.

count well below the number of countries in our sample. We achieve this by collapsing the matrix of instruments in each specification.[14]

We use Arellano and Bond's (1991) difference GMM to estimate equation (1.1) as opposed to Blundell and Bond's (1998) system GMM, which is also widely used in empirical studies. System GMM requires an additional level equation in which the endogenous variables are instrumented with their past differences. Under the additional moment condition that country fixed effects are uncorrelated with lagged differences of endogenous variables, this estimator produces unbiased estimates. Hauk and Wacziarg (2009) and Roodman (2009) show that this additional moment condition is unlikely to hold in the context of growth regressions, like ours. One of the main concern is the correlation between country fixed effects and the speed of convergence for countries far from their steady-state position; this is typically the case for transition economies, of which our sample consist largely on.

As an additional source of evidence, we use a time-varying dummy variable indicating "politically motivated increase in the number of judgeships" from the V-Dem dataset (Coppedge et al., 2019) as an external instrument for ATJ. This represents plausibly exogenous variations in the density of judges, as those events are largely unanticipated and occur for political reasons. Moreover, the direct effect of the instrument on economic growth is ruled out by the fact that we control for political regime changes in our regressions using the Polity2 score. Using that external instrument for ATJ, we find similar results with respect to our benchmark (see Table 1.16). In Table 1.17, we also provide evidence on the quality of our external instrument with Kleibergen-Paap F-stats and several falsification tests.

To further explore the impact of ATJ on economic growth, we look at its heterogeneity by augmenting equation (1.1):

$$\begin{aligned} \ln\left(\frac{y_{i,t}}{y_{i,t-1}}\right) = \beta \ln(y_{i,t-1}) + \gamma \ln(ATJ_{i,t-1}) + \boldsymbol{\theta}' \boldsymbol{X}_{i,t-1} + \\ \boldsymbol{\zeta}' \boldsymbol{\mu}_{i,t-1} + \boldsymbol{\eta}' \ln(ATJ_{i,t-1}) \times \boldsymbol{\mu}_{i,t-1} + \delta_t + (\alpha_i + \varepsilon_{i,t}) \end{aligned} \tag{1.3}$$

where $\boldsymbol{\mu}_{i,t-1}$ is a vector of time-variant (e.g., level of democracy, the rule of law, or schooling) or time-invariant variables (e.g., the legal origin or geographical areas). The coefficient $\boldsymbol{\eta}'$ measures the effect of ATJ on economic growth at different levels of the variables contained in the $\boldsymbol{\mu}$ vector, meaning that we are looking at potential interaction effects with ATJ. When estimating equation (1.3), we instrument the new endogenous variables with the same lag structure as the other controls.

1.4 Results

The results are organized in three parts. In Section 1.4.1, we present our main results evaluating the impact of ATJ on economic growth, and we test its sensitivity to different sets of controls, placebos, and the exclusion of various sub-samples. In Section 1.4.2, we explore the heterogeneity of ATJ effect on growth across relevant macroeconomic dimensions. Section 1.4.3 investigates possible mechanisms through which the effect of ATJ on economic growth could materialize.

1.4.1 Main Results and Robustness Checks

In Table 1.2, we present our results based on the estimation of equation (1.1). We use 5-year averages in a panel of 83 countries (73 countries in the baseline specification) covering the 1970-2014 period. We start from a parsimonious specification in column 1 with OLS-FE where we include only our variable of interest and the lag dependent variable. In the next columns, we use a difference GMM estimation procedure to deal with endogeneity, and we gradually introduce control variables in order to avoid a potential problem of "bad controls" (Angrist and Pischke, 2008). In column 3, we report our main specification

[14] In the collapsed form the matrix of instruments contains one column per lag, instead of one column per lag and per time-period in the non-collapsed form. This allows us to significantly reduce the instrument count without losing information.

with standard growth controls: schooling, investment, government consumption, and political regime. In columns 4 to 6, we gradually include additional standard controls to help us further identify potential transmission channel candidates and to test for additional robustness.

Table 1.2. Effect of ATJ on Economic Growth

Estimation:	FE (1)	GMM (2)	GMM (3)	GMM (4)	GMM (5)	GMM (6)
$log(GDP_{pc})_{t-1}$	-0.488***	-0.294	-0.517***	-0.438***	-0.670***	-0.555***
	(0.073)	(0.177)	(0.168)	(0.155)	(0.217)	(0.204)
$log(ATJ)_{t-1}$	0.099*	1.077***	0.695***	0.494***	0.467***	0.365**
	(0.055)	(0.330)	(0.204)	(0.161)	(0.141)	(0.165)
$Schooling_{t-1}$			0.025	0.002	0.033	0.056
			(0.089)	(0.057)	(0.051)	(0.045)
$Investment_{t-1}$			-1.084	-0.855	0.008	-0.036
			(0.776)	(0.650)	(0.573)	(0.621)
$Gov.\ Cons._{t-1}$			-0.997**	-1.006**	-1.097*	-0.996**
			(0.424)	(0.481)	(0.554)	(0.415)
$Polity2_{t-1}$			-0.004	-0.007	-0.000	-0.007
			(0.015)	(0.012)	(0.016)	(0.013)
$log(Fertility)_{t-1}$				-0.543***	-0.371**	-0.379**
				(0.181)	(0.175)	(0.157)
$Openness_{t-1}$					0.908	0.833*
					(0.637)	(0.419)
$Inflation_{t-1}$						0.002
						(0.002)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
AR2	.	0.32	0.53	0.67	0.38	0.31
Hansen	.	0.89	0.75	0.45	0.61	0.53
Instruments	.	16	32	36	40	44
Countries	83	83	73	73	73	71
Observations	356	241	217	217	217	202

Note: This table presents estimates of the effect of ATJ on economic growth. Column 1 shows results using OLS-FE, while columns 2-6 present results using a two-step difference GMM estimator. In all specifications, we keep the same lag structure of instruments treating the lag dependent and other variables as endogenous. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. See Appendix 1.6.1 for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.2 shows that ATJ has a positive and statistically significant impact on economic development. This finding is robust to the inclusion of standard growth controls. In particular, column 3 reveals that increasing ATJ by 1% increases five-year GDP per capita growth by 0.7pp (0.14pp annually). This is a substantial effect, revealing the importance of ATJ in explaining historical differences in growth rates. Here, we capture the overall effect of increasing ATJ on economic development which, as we argue in Section 1.4.3, encompasses the effect of ATJ on other growth determinants. By adding additional standard growth controls, the magnitude of the effect is indeed consistently reduced from 1.08pp in column 2 to 0.36pp in column 6. This decrease in the magnitude suggests that part of the effect is potentially channeled through other significant variables such as government consumption, fertility, and trade openness (in Section 1.4.3 we test whether they are indeed transmission channels). The magnitude of the effect is similar to other studies regressing *de facto* judicial indicators on economic growth. For example, Feld et al. (2015) find that a one standard deviation increase in *de facto* judicial independence implies 0.3pp faster annual economic growth, while a country that switches from a completely dependent judiciary to a completely independent one would grow 1.3pp faster. Moreover, Melcarne and Ramello (2016) find that every extra year of judicial delay in the disposition of private litigation lowers annual economic growth by 1pp.

In terms of post-estimations tests, in all specifications we do not reject the null hypothesis of no

second order autocorrelation of the residuals (AR2 test) and of joint exogeneity of instruments (Hansen test). This is a clear and positive signal indicating the good quality of our instruments. Following Roodman's (2009) rule of thumb, we keep the number of instruments well below the number of countries in our sample by collapsing the matrix of instruments.

One can argue that the effect found for ATJ can be due to an increasing trend in upper-tail human capital, or can reflect the importance of public services, even after we control for government expenditure and education. To further address this concern, and to show the particular importance of the density of judges as a good macro-level proxy for ATJ, we conduct two placebo tests in Table 1.3. In Panel A, we replace our ATJ proxy with the number of physicians per 100,000 inhabitants, and in Panel B, with the number of public employees per 100,000 inhabitants. In both cases, after including the standard growth controls from column 2, we do not find any positive effect of the placebos on economic growth. Overall, this suggests that the effect of ATJ is not an artifact of a positive trend in top educated people, or in the employment size of the public sector.

Table 1.3. Placebo Tests

Panel A - Placebo	(1)	(2)	(3)	(4)	(5)
$log(GDP_{pc})_{t-1}$	-0.351*	-0.714***	-0.449***	-0.434***	-0.562***
	(0.197)	(0.195)	(0.146)	(0.155)	(0.170)
$log(Physicians)_{t-1}$	-0.958	0.053	-0.451*	-0.385	-0.342
	(0.592)	(0.204)	(0.234)	(0.237)	(0.250)
AR2	0.96	0.19	0.46	0.46	0.24
Hansen	0.10	0.01	0.20	0.39	0.30
Instruments	16	32	36	40	44
Countries	80	73	73	73	71
Observations	410	377	377	377	351
Panel B - Placebo	(1)	(2)	(3)	(4)	(5)
$log(GDP_{pc})_{t-1}$	-0.382**	-1.085***	-0.761**	-0.758***	-0.619***
	(0.166)	(0.294)	(0.303)	(0.273)	(0.157)
$log(Public\_emp)_{t-1}$	-0.905***	0.034	-0.129	-0.132	-0.048
	(0.214)	(0.208)	(0.120)	(0.102)	(0.092)
AR2	0.20	0.09	0.02	0.03	0.02
Hansen	0.01	0.04	0.10	0.17	0.55
Instruments	10	26	30	34	38
Countries	60	54	54	54	53
Observations	157	145	145	145	140

Note: This table presents estimates of the effect of log physicians per 100,000 inhabitants (Panel A) and log public employees per 100,000 inhabitants (Panel B) on economic growth. Columns 1-5 of both panels present results using two-step difference GMM estimator. In all specifications, we keep the same lag structure treating the lag dependent (instrumented with lags 2-6) and other variables (instrumented with lags 5-8) as endogenous. Control variables are included gradually from columns 2-5 and are the same as in Table 1.2. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.4 presents the robustness of our benchmark specification to the removal of different groups of countries. In column 1, we verify that our results are not driven by small or low population countries by removing countries with less than two million inhabitants in year 2000.[15] In columns 2 and 3, we check whether our results are influenced by countries with very low or very high values of ATJ by removing the bottom 10% (column 2) or the top 10% (column 3) of ATJ. The magnitude of the effect and the statistical significance of our variable of interest remains stable across the three columns. In column 4, we look at the potential influence of the unbalanced nature of our panel dataset by dropping countries with less than five periods per country for the ATJ variable. This is the most demanding robustness check that we perform, as we drop as much as 40% of the countries in our sample and 15% of the observations.

[15]Countries below two million inhabitants for year 2000 in our sample are: Botswana, Cyprus, Estonia, Luxembourg, Mauritius, Qatar, Slovenia, and Trinidad and Tobago.

Despite the large reduction in the sample size, the effect of ATJ on economic growth stays positive and significant at the 1% level, but the magnitude of the effect is reduced to 0.42pp.[16] Finally, in column 5 we check whether countries with highest variations of ATJ drive our benchmark results, since we exploit the within country variation in the identification strategy. Indeed, dropping the top 5% of countries with the highest standard deviation of ATJ decreases the magnitude of the effect from 0.7pp to 0.5pp, while remaining significant at the 1% level.

Table 1.4. Effect of ATJ on Economic Growth - Robustness by Subsample

Sub-sample:	No Small Countries (1)	No Bottom 10% of ATJ (2)	No Top 10% of ATJ (3)	No Small # of Time Periods (4)	No High Variation ATJ (5)
$log(GDPpc_{t-1})$	-0.524***	-0.571***	-0.446**	-0.685***	-0.641***
	(0.149)	(0.163)	(0.194)	(0.196)	(0.143)
$log(ATJ_{t-1})$	0.653***	0.569***	0.714***	0.421***	0.508***
	(0.200)	(0.202)	(0.217)	(0.154)	(0.173)
Controls	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
AR2	0.39	0.88	0.57	0.06	0.43
Hansen	0.80	0.41	0.66	0.28	0.61
Instruments	32	32	32	32	32
Countries	65	65	65	44	69
Observations	198	201	196	185	202

Note: This table presents estimates of the effect of ATJ on economic growth. In particular, we test the sensitivity of our benchmark (column 3 of Table 1.2) to the removal of different group of countries. In column 1, we drop countries with less than 2 million inhabitants. Columns 2 and 3 remove respectively countries in the bottom and top decile of ATJ. In column 4, we drop countries with less than 5 period observations of ATJ. Column 5 removes the top 5% of countries with the highest standard deviation of ATJ. In all specifications, we keep the same lag structure of instruments treating the lag dependent and other variables as endogenous. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. Control variables are the same as in our benchmark specification: schooling, investment, government consumption, and political regime. See Appendix 1.6.1 for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

As a further piece of evidence, in Appendix Table 1.16 we redo our main results from Table 1.2 using a plausible external instrument for ATJ, while keeping the same internal instrumentation for other endogenous variables. Our external instrument for ATJ is a dummy variable capturing whether a country faced a politically motivated increase in the number of judgeships (Coppedge et al., 2019). Once controlling for the direct effect of political regime changes on economic growth, that variable represents plausibly exogenous variations in the number of judges and ATJ. Using that external instrument, we find similar results with respect to the benchmark. More importantly for endogeneity issues, in all cases we are still not rejecting the null of both the AR2 and Hansen tests. To further analyze the validity of our external instrument, we implement tests for the weakness of instruments in Appendix Table 1.17 following Bazzi and Clemens (2013). In column 1, we replicate our most parsimonious specification, with Polity2 score as a control, in a 2SLS setting and find a strong effect of ATJ on economic growth. Crucially, we are able to reject the null hypotheses that the bias in the point estimates on the endogenous variables is greater than 30 percent of the OLS bias, indicating a good quality of our external instrument. In column 3, we perform a placebo test in which we lag our outcome and explanatory variables by two periods while keeping the ATJ variable in the same period; we find that ATJ does not impact past GDP growth, which provides evidence that our results are not spurious. Finally, in columns 5 and 6 we test if our instrument is correlated with the political regime which can be a sign of endogeneity. We do not find any correlation with Polity2 score, showing that our instrument is not a reflection of major political

[16] It is worth noting that in column 4 we are rejecting the null hypothesis of no second order autocorrelation of the residuals, signaling the failure of difference GMM moment condition. This is not surprising as we are changing the data structure and reducing the sample size significantly.

changes.

1.4.2 Heterogeneous Effects of ATJ on Economic Development

In Table 1.5, we test whether ATJ has a differential effect on economic growth depending on some key macroeconomic dimensions. In columns 1 to 4 of panel A, we look at the interaction effects of ATJ with the regions specified in Figure 1.3: Europe (excluding European Transition Economies), Transition Economies, MENA, and East-Asia and Pacific. These four regions are interesting for several reasons. European countries (excluding European Transition Economies) are characterized by a very high level of ATJ in the 1970-2010 period; Transition Economies have experienced a large increase in ATJ after the 1990s due to the political and institutional change resulting from the fall of USSR (Figure 1.3); MENA countries are mostly Muslim with Muslim law systems; East-Asia and Pacific countries comprise many high-growth economies (e.g., China, South Korea, and Japan) that have tripled ATJ despite high population growth over the same time period. Across all four regions, we find that ATJ is equally important for growth.[17] This also provides evidence that our results are not driven by specific geographic regions.

In columns 5 and 6, we include income level interaction terms. In contrast with interactions by regions, the effect of ATJ differs across income levels. Column 5 shows that the effect of ATJ on economic growth is significantly lower for advanced compared to non-advanced income countries.[18] Testing with a time-varying measure of economic development, column 6 confirms that the impact of ATJ on economic growth depends negatively on the initial level of GDP per capita. For example, column 6 predicts that increasing ATJ by 1% in Ethiopia raises five-year GDP per capita growth by 0.67pp; on the other hand, increasing ATJ by 1% in Belgium will have no effect on growth. Indeed, Figure 1.4 shows that there is no effect of increasing ATJ for countries with income levels higher than 36,680 2011US$ per capita, which corresponds to highly developed countries.[19] A direct and important policy implication is that the poorest countries are the ones that benefit the most from increasing ATJ, which is in line with other objectives promoted by the UN Sustainable Development Goals (SDGs).[20] This last result highlights, however, a limitation: our proxy is less likely to detect the effect of increasing ATJ where the basics of a well-functioning judicial system are already in place, which is a common feature in developed countries. However, we show in sub-section 1.4.3 that ATJ positively affects other determinants of economic growth such as the protection of property rights. Therefore, even in highly developed countries, ATJ can still have a positive effect on growth through other variables.

In column 7, we investigate the heterogeneous effect of ATJ with respect to human capital. In line with our results in columns 5 and 6 for GDP per capita, we see that highly educated societies are expected to benefit less from improving ATJ compared to low educated societies. This is not surprising considering the link between human capital and GDP per capita, as well as the link between education and legal capability, one of the ATJ components (see Section 1.2.1).

In panel B of Table 1.5, we start by investigating the effect of ATJ at different levels of political and economic freedom by using the Polity2 score of democracy (column 1) and the Economic Freedom of the World Index of the Fraser Institute (column 2). In both cases, we find that the effect of ATJ on economic growth decreases with the level of political or economic freedom. This is in line with expectations as

[17] In Appendix Table 1.12 we test for all other regions specified in Figure 1.3 and we find no heterogeneous effects.

[18] The advanced income countries in our sample as classified by the IMF are Australia, Austria, Belgium, Canada, Cyprus, Czech Republic, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Israel, Italy, Japan, Luxembourg, Netherlands, New Zealand, Norway, Portugal, Singapore, Slovak Republic, Slovenia, South Korea, Spain, Sweden, Switzerland, United Kingdom, and the United States.

[19] In our sample for the most recent period (2010-2014), the highly developed countries are Australia, Austria, Bahrain, Belgium, Canada, Denmark, Finland, France, Germany, Iceland, Ireland, Luxembourg, Netherlands, Norway, Qatar, Singapore, Sweden, Switzerland, United Kingdom, and the United States.

[20] As stated by the UN, ATJ is both an objective per se and a mean to achieve other SDGs such as “No Poverty" (SDG 1) or “Decent work and Economic growth" (SDG 8).

Table 1.5. Heterogeneous Effects of ATJ on Economic Growth

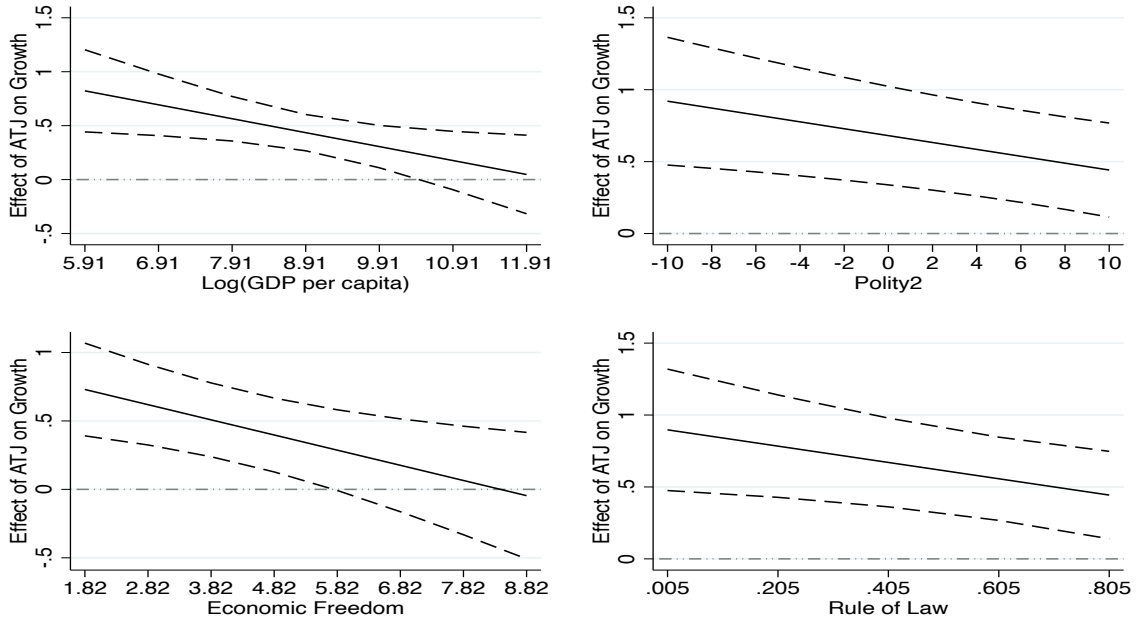
PANEL A	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$log(GDPpc_{t-1})$	-	-0.503**	-	-	-	-0.378**	-
	0.517***		0.509***	0.644***	0.543***		0.654***
	(0.132)	(0.206)	(0.168)	(0.172)	(0.153)	(0.167)	(0.165)
$log(ATJ_{t-1})$	0.571***	0.656***	0.621***	0.456**	0.555**	1.586**	0.847***
	(0.132)	(0.200)	(0.198)	(0.187)	(0.223)	(0.611)	(0.226)
$log(ATJ_{t-1}) \times EU\ No\ Trans.$	-0.461						
	(0.313)						
$log(ATJ_{t-1}) \times Trans.\ Econ.$		0.050					
		(0.210)					
$log(ATJ_{t-1}) \times MENA$			-0.408				
			(0.826)				
$log(ATJ_{t-1}) \times EAP$				0.150			
				(0.231)			
$log(ATJ_{t-1}) \times Adv.\ Income$					-0.478*		
					(0.273)		
$log(ATJ_{t-1}) \times log(GDPpc_{t-1})$						-0.129*	
						(0.067)	
$log(ATJ_{t-1}) \times Schooling_{t-1}$							-0.039*
							(0.021)
AR2	0.23	0.57	0.51	0.09	0.22	0.11	0.18
Hansen	0.55	0.82	0.66	0.66	0.47	0.26	0.44
Instruments	36	35	36	36	36	36	36
Countries	73	73	73	73	73	73	73
Observations	217	217	217	217	217	217	217
PANEL B	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$log(GDPpc_{t-1})$	-	-	-	-	-	-	-
	0.617***	0.673***	0.455***	0.508***	0.584***	0.588***	0.607***
	(0.182)	(0.175)	(0.137)	(0.151)	(0.199)	(0.178)	(0.173)
$log(ATJ_{t-1})$	0.681***	0.932***	0.727***	0.500**	0.574***	0.517***	0.900***
	(0.207)	(0.272)	(0.182)	(0.232)	(0.201)	(0.171)	(0.257)
$log(ATJ_{t-1}) \times Polity2_{t-1}$	-0.024**						
	(0.011)						
$log(ATJ_{t-1}) \times Eco.\ Freedom_{t-1}$		-0.111**					
		(0.051)					
$log(ATJ_{t-1}) \times LO_{UK}$			-0.682**				
			(0.305)				
$log(ATJ_{t-1}) \times LO_{FR}$				0.230			
				(0.220)			
$log(ATJ_{t-1}) \times LO_{GE}$					-0.192		
					(0.217)		
$log(ATJ)_{t-1} \times Indep.\ Judic_{t-1}$						-0.181**	
						(0.078)	
$log(ATJ_{t-1}) \times Rule\ of\ Law$							-0.567*
							(0.303)
AR2	0.13	0.28	0.77	0.63	0.26	0.14	0.14
Hansen	0.59	0.38	0.78	0.81	0.54	0.49	0.55
Instruments	36	40	36	36	36	40	40
Countries	73	67	73	73	73	70	73
Observations	217	193	217	217	217	204	217
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table presents estimates of the effect of ATJ on economic growth at different levels of some key macroeconomic variables. Columns 1-7 in Panel A and Panel B present results using two-step difference GMM estimator. In all specifications, we keep the same lag structure of instruments treating the lag dependent and other variables as endogenous. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. Control variables are the same as in our benchmark specification: schooling, investment, government consumption and political regime. See Appendix 1.6.1 for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

both dimensions are correlated with many measures of the quality of the judicial system, such as judicial independence or the rule of law.[21] Furthermore, this reflects the fact that more democratic and more

[21]Table 1.1 provides evidence by showing that the level of democracy is positively correlated with judicial independence

Figure 1.4. Marginal Effect of ATJ on Economic Growth



Note: This figure plots the marginal effect of ATJ on economic growth at different levels of log GDP per capita, democracy (Polity2 score), economic freedom, and rule of law. Values are calculated based on the results of Table 1.5. A 90 percent confidence interval is depicted with dashed lines.

economically free countries already possess higher levels of ATJ. In Figure 1.4, we depict the marginal effect of ATJ on growth for different levels of democracy and economic freedom. There is a significant positive effect of ATJ on growth for all levels of political freedom but with different magnitudes, as noted in Table 1.5. On the other hand, we do not find that the effect is significantly different from zero for high levels of economic freedom.

When delivering justice, the judge can have different constraints depending on the judicial system. In particular, the common law system is seen by legal scholars as giving less investigative power to the judge and more to lawyers, compared to the civil law system (Zweigert and Kötz, 1998; Schmiegelow and Schmiegelow, 2014). Consequently, the productivity of the judge is more constrained in common law systems compared to civil law systems. In line with the insights from the comparative law literature, column 3 reveals that the effect of ATJ on economic growth is significantly lower in common law countries compared to civil law countries. Moreover, the total effect is not significantly different from zero for common law countries. This emphasizes the fact that our proxy is less able to capture the evolution of ATJ in common law countries, as we focus on judges and not lawyers. In columns 4 and 5, we decompose further the civil law legal system by distinguishing between French and German legal systems as classified by La Porta et al. (2008). We do not find any significant difference in the effect for both French and German legal origins. Therefore, what matters more in terms of heterogeneity are the fundamental differences between the common law and civil law legal systems, rather than differences within the civil law legal system, which are less fundamental.

Lastly, we investigate the differential effect of ATJ on growth depending on judicial independence (column 6) and quality of the judicial system as measured by the rule of law (column 7). In both cases, we find evidence that the effect of ATJ on growth is stronger in less developed judiciaries. It is worth noting that the effect of ATJ on growth is statistically different from zero for all levels of rule of law as

(panel A), absence of corruption in the judiciary (panel A), access to civil justice (panel B), and the rule of law (panel C).

shown in Figure 1.4. This finding is in line with our previous results: developed judiciaries already have higher levels of ATJ; therefore, increasing ATJ in these judiciaries yields a smaller return in terms of growth.

1.4.3 Transmission Channels

In Table 1.2, we find that the positive effect of ATJ on economic growth diminishes when we include classical growth controls. This could be evidence that ATJ exerts its influence on economic growth via significant growth determinants such as government consumption, fertility, and trade openness. In Table 1.6, we take each of these transmission channel candidates and use them as new outcome variables. Additionally, we include other suspected candidates as outcomes such as public corruption, the size of the shadow economy, and the economic freedom index and its main sub-components.

Table 1.6. Effects of ATJ on Potential Mechanisms

Dependent var:	Government Cons.	log(Fertility)	Openness	Public Corruption	Shadow Economy	Economic Freedom	LegalSys. & Prop. Rights	Regulation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$log(GDPpc)_{t-1}$	0.118*	0.038	0.045	-0.013	-0.021	-0.051	0.309	-1.172
	(0.068)	(0.206)	(0.082)	(0.045)	(0.025)	(0.587)	(0.375)	(0.751)
$log(ATJ)_{t-1}$	-0.174***	-0.053	-0.154	-0.141***	-0.038**	2.672***	0.754**	1.926***
	(0.053)	(0.294)	(0.095)	(0.053)	(0.018)	(0.653)	(0.368)	(0.696)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
AR2	0.17	0.40	0.04	0.28	0.30	0.05	0.30	0.93
Hansen	0.70	0.00	0.48	0.90	0.14	0.66	0.53	0.43
Instruments	16	16	16	16	14	16	16	16
Countries	71	71	71	71	51	68	67	68
Observations	209	209	209	209	144	199	197	199

Note: This table presents estimates of the effect of ATJ on the different transmission channel candidates specified in the column labels. Columns 1-8 present results using two-step difference GMM estimator. In all specifications, we keep the same lag structure of instruments treating the lag dependent and other variables as endogenous. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. See Appendix 1.6.1 for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

We find that among the three classical growth control candidates from Table 1.2, only government consumption is a transmission channel through which ATJ affects GDP growth. Column 1 reveals that ATJ has a negative effect on the share of government consumption in GDP.[22] We interpret this finding as a positive effect of ATJ on government accountability as it provides a way for citizens to challenge government policies; one of the main transmission channels through which ATJ affects economic growth is therefore the effectiveness or quality of institutions, a critical notion to achieve sustained growth (Acemoglu et al., 2005).

In column 4, we find a negative effect of ATJ on public corruption that further reinforces the interpretation of the government expenditure channel. Corruption, by reducing incentives to produce and invest, is long recognized as a hindrance to growth especially in developing countries (Mauro, 1995). By increasing the share of population that can potentially prosecute corruption affairs, ATJ lowers corruption and shapes economic agents' attitudes in favor of growth. Furthermore, that last finding is particularly interesting in regard to the link found in other studies between corruption and non-productive (military) government spending (Gupta et al., 2001; d'Agostino et al., 2016). These studies point out

[22]This confirms once more that ATJ, as measured by the density of judges, is not simply a reflection of government spending. Columns 6 to 8 of Panel B in Table 1.1 show that government expenditures are not correlated with ATJ as measured by the World Justice Project while the density of judges is. Columns 3 to 6 of Table 1.2 show that both government expenditure and the density of judges are robust determinants of economic growth.

that public corruption, on top of exerting negative direct effects on growth, distorts the composition of public spending in favor of military spending. This confirms the negative impact of ATJ on government spending, which can be understood as a reduction in excessive military expenses.[23]

In columns 5-8, we include outcome variables that we think are also important for understanding the effect of ATJ on economic growth. We start with the size of the informal sector as a share of GDP in column 5. Many developing countries face a situation known as legal dualism, - i.e., the coexistence of formal and informal judicial systems. Informal justice can be easily thought to go hand in hand with informal economic activities, as it provides a way to resolve disputes while minimizing the probability of detection by the government and possibly with lower costs and delay. In such a case, better ATJ can reduce the informal economic activities as it lowers the cost for firms to operate in the formal sector relative to the informal sector, everything else being equal. In line with this statement, column 4 confirms that the share of informal activities in GDP is negatively affected by ATJ.

Recalling the heterogeneous effects of ATJ on growth, Table 1.5 reveals that the effect of ATJ is decreasing for higher levels of economic freedom. In column 6 of Table 1.6, we find a positive significant effect for economic freedom. This confirms that part of the effect of ATJ on growth is channeled through an improvement of institutions under which economic activities are undertaken. The magnitude of the effect is sizable as our estimates suggest that by increasing ATJ by 20%, a country can pass from the median to the 75th percentile of economic freedom. However, a cautionary causal statement should be made here, as we are rejecting the null hypothesis of no second order autocorrelation of the residuals.

To better understand the economic freedom channel, we decompose the economic freedom index as much as possible to have a more precise view of the exact transmission channel.[24] Among the five Economic Freedom Index components, we find that ATJ affects the legal systems and property rights (column 7) and regulations (column 8). Going one step further, we decompose these two sub-indexes and we find that ATJ affects positively the protection of property rights (column 1 of Appendix Table 1.14) and credit market regulations (column 1 of Appendix Table 1.15), which are both recognized as important growth determinants in the literature (North, 1990; Levine, 2005). Our result on the positive effect of ATJ on credit market regulations is in line with the findings from Visaria (2009) and Chemin (2009a,b, 2012) who show that judicial efficiency has positive effects on access to credit markets, investment, and entrepreneurship.

1.5 Conclusion

As stated by the Sustainable Development Goal number 16 of the United Nations (2015) and reasserted by the World Justice Project (2019), societies, both developing and developed, are facing a considerable gap in the justice sector. This ATJ problem is key and needs to be addressed to have better functioning market economies. Whenever and wherever justice is denied, this has an economic cost that is sizeable and not yet fully measured and understood (OECD and World Justice Project, 2019).

In this paper, we create a new database on the number of professional judges per 100,000 inhabitants by collecting data from various public institutions. We use these data as a macro-level proxy capturing the fundamental and historical evolution of ATJ between 1970 and 2014 for a wide range of developed and developing countries. This unprecedented historical coverage enable us to study the plausible causal effect of ATJ on economic development, using instrumental variable approach in a dynamic panel setting. In a panel of 73 countries, we show that ATJ is a significant and positive determinant of GDP per capita growth. The results are robust to the exclusion of various sub-samples and to different falsification exercises. Our estimates imply that increasing ATJ by 1% increases the GDP per capita five-year growth rate by 0.7pp. That substantial overall effect of ATJ depends negatively on the initial level of GDP per

[23]Column 2 of Appendix Table 1.14 provides further evidence in that direction by showing that ATJ has a negative effect on military interference in the rule of law and politics as measured by the Fraser Institute.

[24]Economic freedom is an index comprised of five sub-indexes: (i) size of government, (ii) legal system and property rights, (iii) sound money, (iv) freedom to trade internationally, and (v) regulation.

capita, human capital, democracy, economic freedom, and the rule of law. This has important policy implications, as it means that the poorest countries are the ones who can have the highest economic returns from improving ATJ. In terms of mechanism, our findings suggest that ATJ increases growth via higher government accountability and better quality of institutions. In particular, we find that ATJ leads to a lower share of government consumption in GDP, less public corruption, a smaller shadow economy, better protection of property rights, and better credit market regulation.

Taken as a whole, our results indicate that ATJ is one of the key elements when trying to understand the impact of effective, or *de facto*, judicial institutions on economic development. Given the interconnection between UN Sustainable Development Goals (SDGs) and ATJ, improving ATJ might help achieve other objectives such as no Poverty (SGD 1), gender equality (SDG 5) or decent work and economic growth (SDG 8). Research looking at the total economic effect of ATJ is still in its infancy. Retracing the historical evolution of ATJ over a longer period, with a larger sample of countries and with new ATJ measures, is a fruitful area for future research.

1.6 Appendix

1.6.1 Summary Statistics and Variable Definition

Table 1.7. Variable Definitions and Sources

Variable	Description and Sources
Judicial Independence	Judicial independence index. The score ranges from 0 to 1 with higher values indicating higher levels of judicial independence. Source: Gutmann and Voigt (2018).
$log(ATJ)$	Log number of professional judges per 100,000 inhabitants. Source: Authors' calculations.
$log(GDP_{pc})$	Log expenditure-side real GDP at chained PPPs (in mil. 2011US) divided by total population (in mil.). Source: Feenstra et al. (2015).
Gov. Cons.	Share of government consumption in GDP. Source: Feenstra et al. (2015).
Polity2	Political regime score. The score ranges from -10 to +10 with higher values indicating more democratic regimes. Source: Marshall et al. (2016).
Schooling	Years of schooling. Source: Barro and Lee (2013).
LO_{UK}	Dummy = 1 for English Legal Origin. Source: La Porta et al. (2008).
LO_{FR}	Dummy = 1 for French Legal Origin. Source: La Porta et al. (2008).
LO_{GE}	Dummy = 1 for German Legal Origin. Source: La Porta et al. (2008).
LO_{SC}	Dummy = 1 for Scandinavian Legal Origin. Source: La Porta et al. (2008).
Absolute Latitude	Absolute value of the latitude of a country's approximate geodesic centroid. Source: CIA's *World Factbook*.
Total Land Area	The total land area of a country in millions of square kilometers. Source: World Bank (2018b).
Std. Dev. Elevation	The standard deviation of elevation across the grid cells within a country in km above sea level. Source: Nordhaus (2006).
Mean Dist. Coast or River	The distance, in thousands of km, from a GIS grid cell to the nearest ice-freecoastline or sea-navigable river, averaged across the grid cells of a country. Source: Gallup et al. (1999).
Corruption Free Judiciary	Corruption of judiciary index. The score ranges from 0 to 1 with higher values indicating less corrupt judicial systems. Source: Gutmann and Voigt (2018).
$log(Time\ Enforcing\ Contract)$	Log number of days required to enforce a contract. Source: World Bank (2018a).
Access to Civil Justice	Access to civil justice index. The score ranges from 0 to 1 with higher values indicating higher levels of access to civil justice. Source: World Justice Project (2020).
Rule of Law (WB)	Rule of law index. The score ranges from -2.5 to 2.5 with higher values indicating higher rule of law levels. Source: World Bank (2018c).
Quality of Judicial Processes	Quality of the judicial processes index. The score ranges from 0 to 18 with higher values indicating better quality of the judicial processes Source: World Bank (2018a).
Investment	Share of investment in GDP. Source: Feenstra et al. (2015).
$log(Fertility)$	Log total births per woman. Source: World Bank (2018b).
Openness	Share of exports and imports in GDP. Source: Feenstra et al. (2015).
Inflation	Consumer Price Index. Source: IMF (2019).

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Variable	Description and Sources
$log(Physicians)$	Log of total number of physicians per 100,000 inhabitants (including generalist and specialist medical practitioners). Source: World Bank (2018b).
$log(Public\ Employees)$	Log of total number of public employees per 100,000 inhabitants. Source: ILO (2019).
$EU\ No\ Trans.$	Dummy = 1 for European economies (excluding transition economies). Source: IMF (2000).
$Trans.\ Econ.$	Dummy = 1 for Transition economies. Source: IMF (2000).
$MENA$	Dummy = 1 for Middle East and North Africa economies. Source: World Bank (2018b).
EAP	Dummy = 1 for East Asia and Pacific economies. Source: World Bank (2018b).
$Adv.\ Income$	Dummy = 1 for advanced income countries. Source: IMF (2019).
$Eco.\ Freedom$	Economic Freedom Index. The score ranges from 0 to 10 with higher values indicating higher economic freedom levels. Source: Gwartney et al. (2018).
$Indep.\ Judic.$	Dummy = 1 if there is an independent judiciary. Source: Dahlberg et al. (2018).
$Rule\ of\ Law$	Rule of law index. The score ranges from 0 to 1 with higher values indicating higher rule of law levels. Source: Coppedge et al. (2019).
$Public\ Corruption$	Public sector corruption index. The score ranges from 0 to 1 with higher values indicating higher levels of public corruption. Source: Coppedge et al. (2019).
$Shadow\ Economy$	Level of the shadow economy in % of GDP. Source: Dahlberg et al. (2018).
$Legal\ Sys.\ \&\ Prop.\ Rights$	Legal system and property rights index. The score ranges from 0 to 10 with higher values indicating better legal systems and higher protection of property rights. Source: Gwartney et al. (2018).
$Regulation$	Regulation index. The score ranges form 0 to 10 with higher values indicating better economic regulations. Source: Gwartney et al. (2018).
SSA	Dummy = 1 for Sub-Saharan Africa economies. Source: World Bank (2018b).
LAC	Dummy = 1 for Latin America and the Caribbean economies. Source: World Bank (2018b).
$North\ America$	Dummy = 1 for North America economies. Source: World Bank (2018b).
$South\ Asia$	Dummy = 1 for South Asia economies. Source: World Bank (2018b).
$Property\ Rights$	Property rights index. The score ranges from 0 to 10 with higher values indicating better protection of property rights. Source: Gwartney et al. (2018).
$Military\ Interference$	Military interference in rule of law and politics index. The score ranges from 0 to 10 with higher values indicating lower levels of interference. Source: Gwartney et al. (2018).
$Judi.\ Indep.\ (FI)$	Judicial independence index. The score ranges from 0 to 10 with higher values indicating higher levels of independence. Source: Gwartney et al. (2018).
$Impartial\ Courts$	Impartial courts index. The score ranges from 0 to 10 with higher values indicating higher levels of impartiality. Source: Gwartney et al. (2018).
$Legal\ System$	Integrity of the legal system index. The score ranges from 0 to 10 with higher values indicating higher integrity. Source: Gwartney et al. (2018).
$Contract\ Enforcement$	Legal enforcement of contracts index. The score ranges from 0 to 10 with higher values indicating better enforcement. Source: Gwartney et al. (2018).
$Regulatory\ Costs$	Regulatory costs of the sale of real property index. The score ranges from 0 to 10 with higher values indicating lower costs. Source: Gwartney et al. (2018).

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Variable	Description and Sources
Police Reliability	Reliability of police index. The score ranges from 0 to 10 with higher values indicating higher reliability. Source: Gwartney et al. (2018).
Business Cost of Crime	Business costs of crime index. The score ranges from 0 to 10 with higher values indicating higher lower costs. Source: Gwartney et al. (2018).
Credit Market Regulations	Credit market regulations index. The score ranges from 0 to 10 with higher values indicating more efficient regulations. Source: Gwartney et al. (2018).
Labor Market Regulations	Labor market regulations index. The score ranges from 0 to 10 with higher values indicating more efficient regulations. Source: Gwartney et al. (2018).
Business Regulations	Business regulations index. The score ranges from 0 to 10 with higher values indicating more efficient regulations. Source: Gwartney et al. (2018).
High Court Packing	Dummy = 1 for limited, politically motivated increase in the number of judgeships on very important courts. Source: Coppedge et al. (2019).
% *Tertiary Completed Educ.*	Percentage of complete tertiary schooling attained in population. Source: Barro and Lee (2013).
% *Pop. Risk Malaria*	Percentage of population at risk of contracting malaria. Source: Gallup and Sachs (2001).
Ethnic Fractionalization	Ethnic fractionalization index. Source: Alesina et al. (2003).
% *Euro. Descent Pop.*	Percentage of population of European descent. Source: Putterman and Weil (2010).
%*Protestant*	Percentage of a country's population belonging to the Protestant religion. Source: La Porta et al. (1999).
%*Catholic*	Percentage of a country's population belonging to the Roman Catholic religion. Source: La Porta et al. (1999).
%*Muslim*	Percentage of a country's population belonging to the Muslim religion. Source: La Porta et al. (1999).

Note: This table provides a description and the sources of all the variables used in our paper. Variables are displayed by order of apparition in tables.

Table 1.8. Summary Statistics

Variable	Mean	S.D.	Min.	Max.	Obs.
PANEL A					
$log(GDP_{pc})_{t-1}$	9.54	0.84	6.72	11.33	217
$log(ATJ)_{t-1}$	2.06	0.90	-0.38	3.94	217
$Schooling_{t-1}$	8.90	2.05	2.49	12.73	217
$Investment_{t-1}$	0.25	0.09	0.00	0.62	217
$Gov.\ Cons_{t-1}$	0.19	0.07	0.08	0.50	217
$Polity2_{t-1}$	6.80	5.18	-10.00	10.00	217
$log(Fertility)_{t-1}$	0.64	0.33	0.13	1.61	217
$Openness_{t-1}$	-0.05	0.13	-0.51	0.47	217
$Inflation_{t-1}$	63.99	27.67	0.01	102.50	207
$log(Physicians)_{t-1}$	-3.98	0.73	-7.33	-2.91	204
$log(Public\ Employees)_{t-1}$	8.90	0.56	7.05	10.43	123
$EU\ No\ Trans.$	0.50	0.50	0.00	1.00	217
$Trans.\ Econ.$	0.24	0.43	0.00	1.00	217
$MENA$	0.06	0.24	0.00	1.00	217
EAP	0.22	0.42	0.00	1.00	217
$Adv.\ Income$	0.49	0.50	0.00	1.00	217
$Eco.\ Freedom_{t-1}$	6.59	1.17	2.49	9.05	207
LO_{UK}	0.23	0.42	0.00	1.00	217
LO_{FR}	0.45	0.50	0.00	1.00	217
LO_{GE}	0.28	0.45	0.00	1.00	217
$Rule\ of\ Law_{t-1}$	0.77	0.26	0.06	1.00	217
$Indep.\ Judic._{t-1}$	0.65	0.46	0.00	1.00	204
$Gov.\ Cons._t$	0.18	0.06	0.08	0.50	217
$log(Fertility)_t$	0.60	0.30	0.13	1.49	217
$Openness_t$	-0.05	0.13	-0.54	0.47	217
$Shadow\ Economy_t$	0.27	0.13	0.08	0.71	144
$Eco.\ Freedom_t$	6.89	1.02	3.50	9.05	211
$Legal\ Sys.\ \&\ Prop.\ Rights_t$	6.23	1.31	2.40	8.86	208
$Regulation_t$	6.74	1.28	2.21	9.04	210
$Public\ Corruption_t$	0.28	0.26	0.00	0.91	217
PANEL B - Appendix Variables					
SSA	0.01	0.12	0.00	1.00	217
LAC	0.15	0.36	0.00	1.00	217
$North\ America$	0.03	0.16	0.00	1.00	217
$South\ Asia$	0.01	0.10	0.00	1.00	217
$Property\ Rights_t$	5.96	1.73	1.09	9.11	157
$Military\ Interference_t$	8.00	1.98	0.83	10.00	165
$Judicial\ Independence_t$	5.65	2.20	0.43	9.51	157
$Impartial\ Courts_t$	5.17	1.73	0.99	9.08	166
$Legal\ System_t$	7.16	1.98	1.67	10.00	162
$Contract\ Enforcement_t$	5.22	1.51	1.80	8.48	137
$Regulatory\ Costs_t$	7.85	1.48	3.30	9.94	137
$Police\ Reliability_t$	5.95	1.88	1.56	9.50	105
$Business\ Cost\ of\ Crime_t$	6.37	1.56	1.57	8.96	105
$Credit\ Market\ Regulation_t$	8.06	2.04	0.00	10.00	211
$Labor\ Market\ Regulation_t$	5.82	1.43	2.83	9.17	185
$Business\ Regulation_t$	6.51	1.15	2.72	9.50	157

Note: This table provides summary statistics for the main variables used in the core of our paper (Panel A) and in the appendix (Panel B).

1.6.2 A New Database on the number of Professional Judges: Method and Sources

This paper provides a new database on the number of professional judges for a large sample of developed and developing countries from 1970 through 2014. To do so, we collect and merge data on the number of judges from three different official sources: (i) *public institutions* (ministries of justice, supreme courts, and national statistical offices), (ii) *international organizations* and (iii) *academic publications*. The vast majority, almost 70%, of our data are from the following international organizations: UNOV, CEPEJ, UNODC, OAS, and Eurostat. These organizations are specialized in collecting data around the world: some, as CEPEJ or OAS, are focused on a specific area (respectively Europe and the Americas) while others, as UNOV, provide data for all continents.

In the making of our database, we use the following criteria:

1. We include data considering the following preference order of data sources: (i) *public institutions*, (ii) *international organizations*, and (iii) *academic publications*.

2. If we have two competing data sources from the same group (e.g., within international organisations, CEPEJ, and UNODC) and covering the exact same period, we keep data from the source which is the most closely related to the national administration of justice (e.g., CEPEJ for European countries).

3. If we have two competing data sources from the same group and covering some common dates (e.g., UNODC from 1970 through 2004 and CEPEJ from 2004 through 2014), we merge the data keeping the source which is the most closely related to the national administration of justice for the overlapping dates (e.g., CEPEJ for European countries in 2004).

4. If we have two complementary data sources (e.g., UNODC from 1970 through 2000, and CEPEJ from 2002 through 2014), we merge the data to create a longer time series.

5. We do not merge datasets when suspiciously high changes are observed (an annual change larger than 50% at the juncture of the two datasets). This is important to minimize false variations that might happen due to changes in definition and counting methods between two datasets. When observed, we remove completely the dataset with the lowest quality.

Table 1.9 details the sources, definitions, countries, and periods cover by each used source of data. Table 1.10 provides summary statistics for the 105 countries for which we have managed to collect at least one data point.

Table 1.9. Number of Judges: Sources, Definitions and Coverage

Source	Definition	Sample used
PANEL A - Public Institutions		
Ministries of Justice and Supreme Courts	Data for: Botswana, Belgium, Croatia, Estonia, Finland, Ireland, Latvia, Mali, Malta, New Zealand, Niger, Poland, Romania, Slovenia, South Korea, Sweden, Uruguay, and Venezuela.	18 countries, 1970-2014
National Statistical Offices	Data for: Australia, Armenia, Qatar, and the United States.	4 countries, 1980-2014
PANEL B - International Organizations		
UNOV (United Nations Office at Vienna - Crime Prevention and Criminal Justice Branch (n.d.))	Professional judges or magistrates may be understood to mean both full-time and part-time officials authorized to hear civil, criminal and other cases, including in appeal courts, and make dispositions in a court of law.	67 countries, 1973-2014 (online database and reports)
CEPEJ (n.d.) (European Commission for the Efficiency of Justice)	Total number of professional judges in all types of courts. Professional judges are "those who have been trained and who are paid as such" and whose main function is to work as a judge and not as a prosecutor; the fact of working full-time or part-time has no consequence for their status. It does not include the court clerks that exist in some member states.	34 European countries 2002-2014 (2002-2009 reports and 2010-2014 online database)
UNODC (n.d.) (United Nations Office on Drugs and Crime)	Professional Judges or Magistrates means both full-time and part-time officials authorized to hear civil, criminal and other cases, including in appeal courts, and to make dispositions in a court of law. Includes also authorized associate judges and magistrates.	24 countries, 2003-2015 (online database and reports)
OAS (n.d.) (Organization of American States)	Professional judges or magistrates understood as both full-time and part-time officials authorized to hear civil, criminal and other cases, including in appeal courts, and to make dispositions in a court of law. Including authorized associate judges and magistrates.	17 countries, 2003-2014 (online database and reports)
Eurostat (2016) (European Statistical Office)	Both full-time and part-time officials authorized to hear criminal and civil cases, including in appeal courts, and to make dispositions in a court of law. Authorized associate judges and magistrates are included.	7 European countries, 2002-2013 (online database and reports)
PANEL C - Publications		
Albers, Pim (2003)	Data for: Armenia, Botswana, Cambodia, Ghana, Mexico, Mozambique, Pakistan, Papua New Guinea, Peru, and Trinidad and Tobago.	10 countries, 2000
Pistor et al. (1999)	Data for: China, France, Germany, Japan, India, South Korea, and Malaysia.	7 countries, 1970-1995
Calleros-Alarcón (2008)	Data for: Bolivia, Brazil, Paraguay, and Venezuela.	4 countries, 1993-2001
Schmiegelow and Schmiegelow (2014)	Data for: Cambodia, Laos, and Tajikistan.	3 countries, 1995-2011
Contini (2000)	Data for: Austria and Spain.	2 countries, 1980-1990
Dakolias (1999)	Data for Ecuador.	1 country, 1998
Kühn (2011)	Data for Poland.	1 country, 1981
Turner (2009)	Data for Serbia.	1 country, 2002
IMF (2012)	Data for Burundi.	1 country, 2005-2010

Note: This table provides the source, definition and country-time data coverage we use for the number of judges. Sources are displayed according to the number of countries they provide within each category.

Table 1.10. Number of judges per 100.000 inhabitants - 105 countries, 1970-2014

Country	Mean	S.D.	Min.	Max.	Country	Mean	S.D.	Min.	Max.
Albania	11.29	2.04	8.55	13.07	**Kyrgyz Republic**	6.21	0.67	5.31	6.82
Algeria	8.72	3.53	3.89	12.32	Laos	5.92	.	5.92	5.92
Argentina	3.6	1.87	1.44	4.75	**Latvia**	16.5	8.08	6.81	25.71
Armenia	6.7	0.8	5.95	7.54	**Lithuania**	17.55	7.46	6.36	25.19
Australia	4.83	0.19	4.63	5	**Luxembourg**	34.65	6.01	25.87	39.21
Austria	19.6	0.97	18.22	20.92	**Macedonia**	28.63	4.57	20.79	31.52
Azerbaijan	4.62	1.79	2.49	6.48	**Malaysia**	1.31	0.37	0.83	1.67
Bahamas	9.16	1.49	8.11	10.21	Mali	2.58	.	2.58	2.58
Bahrain	11.44	2.27	9.83	13.05	**Malta**	5.97	2.76	2.69	10.07
Barbados	8.2	0.42	7.9	8.5	**Mauritius**	4.68	1.15	3.46	5.75
Belarus	8.85	2.1	5.75	10.32	**Mexico**	2.15	1.88	0.83	4.31
Belgium	19.66	2.65	16.31	22.81	**Moldova**	8.41	2.28	5.21	10.79
Bolivia	8.85	1.35	7.4	10.05	**Mongolia**	15.77	0.7	14.96	16.24
Bosnia and Herzegovina	21.75	4.25	18.25	26.47	**Montenegro**	39.9	1.04	39.26	41.11
Botswana	2.82	1.22	2.1	4.22	Morocco	10.22	.	10.22	10.22
Brazil	7.46	0.99	6.13	8.35	Mozambique	0.97	.	0.97	0.97
Bulgaria	14.24	10.49	2.82	30.41	**Myanmar**	2.72	0.23	2.46	2.86
Burkina Faso	1.81	0.12	1.73	1.9	**Nepal**	1	0.19	0.81	1.2
Burundi	14.41	6.77	6.62	18.88	**Netherlands**	9.96	3.74	5.77	14.45
Cambodia	1.11	0.71	0.47	1.86	**New Zealand**	3.76	0.87	2.17	4.81
Canada	5.3	3.1	0.68	7.39	Nicaragua	0.94	0.34	0.7	1.19
Chile	5.12	3.19	2.51	10.54	Niger	1.84	0.14	1.75	1.94
China	8.58	5.92	2.56	16.38	**Norway**	9.07	1.96	6.67	11.11
Colombia	9.93	0.37	9.4	10.34	Pakistan	1.05	.	1.05	1.05
Costa Rica	14.28	7.04	4.98	25.48	**Panama**	7.51	0.83	6.04	8.42
Croatia	36.98	9.57	22.2	44.51	Paraguay	10.39	0.16	10.27	10.51
Cyprus	9.06	2.94	5.67	12.88	**Peru**	4.73	2.13	2.99	7.58
Czech Republic	28.4	0.96	27.3	29.06	**Philippines**	2.17	0.47	1.73	2.85
Denmark	6.68	0.2	6.45	6.83	**Poland**	17.25	6.62	8.63	25.86
Dominican Republic	6.92	0.09	6.86	6.99	**Portugal**	12.07	4.88	5.33	18.67
Ecuador	0.97	0	0.97	0.97	**Qatar**	7.39	1.9	5.37	9.62
Egypt	7.11	3.7	4.5	9.73	**Romania**	13.36	6.1	5.15	20.44
El Salvador	10.75	0.08	10.69	10.8	**Russia**	18.42	7.33	7.54	22.87
Estonia	15.5	2.01	12.01	16.83	**Saudi Arabia**	3.17	0.17	3.05	3.3
Ethiopia	0.25	0.08	0.16	0.3	**Serbia**	34.54	2.56	32.31	37.34
Finland	16.42	1.52	13.7	18.07	**Singapore**	1.82	0.56	1.11	2.59
France	9.5	1.67	6.55	10.6	**Slovak Republic**	22.53	2.57	18.74	25.24
Georgia	6	0.95	4.39	6.71	**Slovenia**	38.96	10.87	25.72	51.58
Germany	22.13	3.15	18.07	26.25	**South Africa**	3.67	0.55	2.96	4.12
Ghana	0.93	.	0.93	0.93	**South Korea**	2.75	1.45	1.28	5.39
Greece	17.18	7.39	9.8	30.32	**Spain**	8.55	2.04	5.03	10.83
Guatemala	0.67	0.02	0.66	0.69	**Sweden**	11.48	0.73	10.75	12.35
Hungary	22.4	5.99	12.68	28.64	**Switzerland**	13.17	2.53	10.63	15.36
Iceland	16.26	1.14	15.33	17.74	Tajikistan	3.19	2.36	0.47	4.63
India	1.06	0.08	1	1.15	**Thailand**	4.23	1.71	2.24	6.35
Indonesia	1.62	0.03	1.6	1.64	**Trinidad and Tobago**	6.68	0.94	5.6	7.34
Ireland	2.59	0.48	2.08	3.17	**Turkey**	9.89	0.7	9.19	11.02
Israel	7.07	1.04	5.72	8.32	**Ukraine**	11.84	4.88	4.73	17.33
Italy	11.86	1.38	10.59	14.58	**United Kingdom**	2.73	0.93	1.58	3.55
Jamaica	2.57	1.01	1.07	3.25	**United States**	10.09	0.68	9.29	11.19
Japan	2.42	0.2	2.26	2.86	**Uruguay**	13.19	0.97	11.45	14.4
Kazakhstan	14.73	1.26	13.28	15.58	**Venezuela**	6.5	0.36	6.1	6.8
Kenya	1	0.24	0.83	1.17					

Note: This table provides summary statistics on the density of judges for the 105 countries for which we have at least one observation during the 1970-2014 period. In bold, we indicate the 83 countries that have sufficient time variations in the density of judges to be included in our analysis (see column 1 of Table 1.2). Countries with standard deviation = (.) have one observation during the 1970-2014 period.

1.6.3 Additional Analysis

Table 1.11. Effect of ATJ on Economic Growth - Robustness by Periods

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$log(GDP_{pc})_{t-1}$	-	-	-	-	-	-	-	-
	0.696***	0.705***	0.620***	0.589***	0.532***	0.502***	0.494***	0.590***
	(0.122)	(0.123)	(0.141)	(0.163)	(0.186)	(0.153)	(0.175)	(0.164)
$log(ATJ)_{t-1}$	0.384***	0.399***	0.631***	0.579***	0.682***	0.648***	0.722***	0.607***
	(0.133)	(0.123)	(0.187)	(0.217)	(0.219)	(0.187)	(0.205)	(0.188)
$log(ATJ_{t-1}) \times period2$	0.296							
	(0.534)							
$log(ATJ_{t-1}) \times period3$		0.315						
		(0.217)						
$log(ATJ_{t-1}) \times period4$			-0.250					
			(0.232)					
$log(ATJ_{t-1}) \times period5$				-0.005				
				(0.070)				
$log(ATJ_{t-1}) \times period6$					0.014			
					(0.055)			
$log(ATJ_{t-1}) \times period7$						0.051		
						(0.048)		
$log(ATJ_{t-1}) \times period8$							0.005	
							(0.036)	
$log(ATJ_{t-1}) \times post90s$								-0.028
								(0.171)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
AR2	0.09	0.23	0.51	0.33	0.52	0.62	0.60	0.33
Hansen	0.13	0.36	0.76	0.40	0.70	0.82	0.64	0.38
Instruments	35	35	34	33	32	32	32	33
Countries	73	73	73	73	73	73	73	73
Observations	217	217	217	217	217	217	217	217

Note: This table presents estimates of the effect of ATJ on economic growth. In particular, we test the sensitivity of our benchmark (column 3 of Table 1.2) to particular time periods. From column 1 to 7, we include an interaction term between ATJ and the corresponding 5-year time period dummy. In column 8, we include an interaction term between ATJ and post 1990 periods. In all specifications, we keep the same lag structure of instruments treating the lag dependent and other variables as endogenous. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. Control variables are the same as in our benchmark specification: schooling, investment, government consumption and political regime. See Appendix 1.6.1 for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.12. Effect of ATJ on Economic Growth - Additional Regional Interactions

	(1)	(2)	(3)	(4)
$L.log(GDP_{pc})$	-0.517***	-0.540***	-0.519***	-0.485***
	(0.163)	(0.144)	(0.175)	(0.170)
$L.log(ATJ)$	0.692***	0.570***	0.702***	0.759***
	(0.202)	(0.151)	(0.217)	(0.227)
$L.log(ATJ) \times SSA$	-0.072			
	(0.616)			
$L.log(ATJ) \times LAC$		-0.302		
		(0.286)		
$L.log(ATJ) \times N.\ America$			-0.468	
			(0.702)	
$L.log(ATJ) \times South\ Asia$				-1.455
				(2.944)
Controls	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
AR2	0.52	0.15	0.50	0.64
Hansen	0.74	0.51	0.79	0.76
Instruments	33	36	34	32
Countries	73	73	73	73
Observations	217	217	217	217

Note: This table presents estimates of the effect of ATJ on economic growth. In particular, we test the sensitivity of our benchmark (column 3 of Table 1.2) to particular geographical areas. From column 1 to 4, we include an interaction term between ATJ and the corresponding region. In all specifications, we keep the same lag structure of instruments treating the lag dependent and other variables as endogenous. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. Control variables are the same as in our benchmark specification: schooling, investment, government consumption and political regime. See Appendix 1.6.1 for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.13. Effect of ATJ on Economic Growth - Alternative Moment Conditions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$log(GDP_{pc})_{t-1}$	-0.519***	-0.523***	-0.533***	-0.524***	-0.535***	-0.462***	-0.603***	-0.513***
	(0.134)	(0.153)	(0.156)	(0.171)	(0.173)	(0.147)	(0.093)	(0.173)
$log(ATJ)_{t-1}$	0.624***	0.591***	0.630***	0.704***	0.634***	0.631***	0.526***	0.631***
	(0.153)	(0.153)	(0.148)	(0.194)	(0.154)	(0.220)	(0.106)	(0.143)
$Schooling_{t-1}$	-0.005	0.001	-0.000	0.027	-0.001	-0.012	0.062	-0.023
	(0.050)	(0.059)	(0.060)	(0.086)	(0.078)	(0.076)	(0.042)	(0.063)
$Investment_{t-1}$	-1.002	-0.885	-1.031	-1.090	-1.025	-0.544	-0.671	-0.848
	(0.617)	(0.746)	(0.742)	(0.780)	(0.935)	(0.853)	(0.548)	(0.809)
$Gov.\ Cons._{t-1}$	-1.133***	-1.255**	-1.123***	-0.970**	-1.105***	-0.951**	-0.975***	-1.207**
	(0.354)	(0.572)	(0.359)	(0.418)	(0.387)	(0.464)	(0.262)	(0.541)
$Polity2_{t-1}$	0.006	0.001	0.006	-0.004	0.006	0.005	0.005	-0.002
	(0.007)	(0.009)	(0.008)	(0.015)	(0.007)	(0.010)	(0.008)	(0.010)
$First\_log(GDP_{pc})_{t-1}$	2	2	3	3	4	4	5	5
$Last\_log(GDP_{pc})_{t-1}$	5	8	5	6	6	8	6	8
$First\_log(ATJ)_{t-1}$	3	4	3	5	3	3	3	4
$Last\_log(ATJ)_{t-1}$	6	6	6	8	6	5	8	6
AR2	0.38	0.38	0.38	0.54	0.41	0.52	0.19	0.53
Hansen	0.60	0.36	0.53	0.71	0.54	0.44	0.58	0.39
Instruments	31	29	30	31	30	27	39	26
Countries	73	73	73	73	73	73	73	73
Observations	217	217	217	217	217	217	217	217

Note: This table presents estimates of the effect of ATJ on economic growth. In particular, we test the sensitivity of our benchmark (column 3 of Table 1.2) to alternative lag structure for instruments. From column 1 to 8, we use diffrenent sets of lags specified in the corresponding lines for GDP_{pc} and ATJ (including other endogenous variables). We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. See Appendix 1.6.1 for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.14. Effects of ATJ on Potential Mechanisms - Decomposing the Legal System and Property Rights Indicator

Dependent var:	Property Rights	Military Interference	Judi. Indep. (FI)	Impartial Courts	Legal System	Contract Enforcement	Regulatory Costs	Police Reliability	Business Cost of Crime
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$log(GDPpc)_{t-1}$	0.438	0.000	1.588	2.044**	-0.549	-0.501	1.309	0.593	0.968
	(0.628)	(0.517)	(1.272)	(0.786)	(0.894)	(0.682)	(0.963)	(0.507)	(0.628)
$log(ATJ)_{t-1}$	2.375***	1.965***	-0.585	-0.049	-0.147	-0.333	0.382	-0.442	0.994
	(0.790)	(0.649)	(2.082)	(0.854)	(1.404)	(0.583)	(1.141)	(2.142)	(2.558)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
AR2	0.96	0.93	0.37	0.52	0.10	.	.	.	.
Hansen	0.16	0.69	0.16	0.11	0.35	0.34	0.79	0.80	0.52
Instruments	12	12	12	12	12	11	11	10	10
Countries	65	65	65	65	63	64	64	60	60
Observations	122	132	122	132	125	99	99	60	60

Note: This table presents estimates of the effect of ATJ on the different transmission channel candidates specified in the column labels. Columns 1-9 present results using two-step difference GMM estimator. In all specifications, we keep the same lag structure of instruments treating the lag dependent and other variables as endogenous. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. See Appendix 1.6.1 for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.15. Effects of ATJ on Potential Mechanisms - Decomposing the Regulation Indicator

Dependent var:	Credit Market Regulation (1)	Labor Market Regulation (2)	Business Regulation (3)
$log(GDPpc)_{t-1}$	-3.684**	0.059	-0.218
	(1.402)	(0.865)	(0.735)
$log(ATJ)_{t-1}$	5.675***	0.227	0.594
	(1.159)	(1.324)	(0.854)
Country FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
AR2	0.50	0.25	0.64
Hansen	0.43	0.09	0.14
Instruments	16	16	12
Countries	68	65	65
Observations	201	162	123

Note: This table presents estimates of the effect of ATJ on the different transmission channel candidates specified in the column labels. Columns 1-3 present results using two-step difference GMM estimator. In all specifications, we keep the same lag structure of instruments treating the lag dependent and other variables as endogenous. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. See Appendix 1.6.1 for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.16. Effect of ATJ on Economic Growth - with External Instrument for ATJ

Estimation:	FE (1a)	GMM (1b)	GMM (2)	GMM (3)	GMM (4)	GMM (5)
$log(GDP_{pc})_{t-1}$	-0.488***	-0.258	-0.494***	-0.476***	-0.702***	-0.568**
	(0.072)	(0.186)	(0.167)	(0.147)	(0.224)	(0.223)
$log(ATJ)_{t-1}$	0.099**	1.183***	0.735***	0.482***	0.477***	0.392**
	(0.044)	(0.327)	(0.190)	(0.159)	(0.156)	(0.153)
$Schooling_{t-1}$			-0.006	0.026	0.042	0.064
			(0.097)	(0.056)	(0.062)	(0.051)
$Investment_{t-1}$			-1.219	-1.065	-0.160	-0.116
			(0.837)	(0.782)	(0.774)	(0.658)
$Gov.\ Cons._{t-1}$			-0.951*	-1.105*	-1.221**	-1.085**
			(0.488)	(0.562)	(0.611)	(0.418)
$Polity2_{t-1}$			-0.007	-0.004	0.002	-0.007
			(0.014)	(0.011)	(0.015)	(0.014)
$log(Fertility)_{t-1}$				-0.539**	-0.340*	-0.347**
				(0.207)	(0.194)	(0.164)
$Openness_{t-1}$					0.881	0.859*
					(0.725)	(0.473)
$Inflation_{t-1}$						0.002
						(0.002)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
AR2		0.26	0.60	0.44	0.25	0.31
Hansen		0.85	0.74	0.43	0.55	0.38
Instruments		12	28	32	36	40
Countries	83	83	73	73	73	71
Observations	356	241	217	217	217	202

Note: This table presents estimates of the effect of ATJ on economic growth. In particular, we redo Table 1.2 using "politically motivated increase in the number of judges" (Coppedge et al., 2019) as an external instrument for ATJ. The instrument is a dummy variable indicating if the judiciary of a country experienced "massive, politically motivated increase in the number of judgeships across the entire judiciary" or "limited, politically motivated increase in the number of judgeships on very important courts." Column 1a presents results using fixed effects. Columns 1b-5 present results using two-step difference GMM estimator. In all specifications, we keep the same lag structure of instruments treating the lag dependent and other variables as endogenous. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. See Appendix 1.6.1 for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.17. External Instrument Validity

			Placebo			
Dependent var:	$D.growth_t$	$D.growth_t$	$D.growth_{t-2}$	$D.growth_{t-2}$	$D.polity2_t$	$D.polity2_t$
Estimation:	2SLS	OLS	2SLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
$D.ln\_ATJ_{t-1}$	1.094***		-0.101			
	(0.185)		(0.238)			
$D.log(GDP_{pc})_{t-1}$	-0.318**	-0.696***				0.108
	(0.121)	(0.048)				(0.629)
$D.log(GDP_{pc})_{t-3}$			-1.085***	-0.744***		
			(0.093)	(0.071)		
$D.Polity2_{t-1}$	-0.012	-0.001				
	(0.019)	(0.004)				
$D.Polity2_{t-3}$			-0.001	-0.002		
			(0.011)	(0.002)		
$High\ Court\ Packing_{t-1}$		0.490***		-0.020	-0.104	-0.154
		(0.024)		(0.066)	(0.120)	(0.280)
First stage						
$High\ Court\ Packing_{t-1}$	0.272***		0.215***			
	(0.041)		(0.048)			
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
KP LM p-val	0.05		0.58			
KP F-stat	6.52		1.30			
rel. OLS bias>30	0.0027		0.9664			
Countries	78	78	78	78	79	79
Observations	229	229	179	320	230	230

Note: This table test the validity of the external instrument presented and used in Table 1.16. In column 1, we use a standard 2SLS approach and instrument ATJ with "politically motivated increase in the number of judges" (Coppedge et al., 2019). The instrument is a dummy variable indicating if the judiciary of a country experienced "massive, politically motivated increase in the number of judgeships across the entire judiciary" or "limited, politically motivated increase in the number of judgeships on very important courts." Column 2 displays the reduced form relationship estimated by OLS. In columns 3 and 4 we make a placebo test by lagging the dependent and lag-dependent variable by two periods while keeping the ATJ variable in the same $t-1$ period. Columns 5 and 6 test if our instrument is correlated with the political regime variable. The KP LM row reports the p-value for a test of underidentification of the structural equation. The KP F-stat row reports the heteroskedasticity-robust first-stage F-statistics. Below, we report the p-value of a test that the bias in the point estimates on the endogenous variables is greater than 30 percent of the OLS bias following Bazzi and Clemens (2013). Considering a 30% maximal IV relative bias means that the critical value for the F-stat is around 4.5. In all specifications, we control for country and time fixed effects. See Appendix 1.6.1 for further information on the variables. Standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.18. Determinants and Correlates of the Density of Judges

Period covered:	1970-2014 (1)	1970-2014 (2)	1970-2014 (3)	1990-2014 (4)	1990-2014 (5)	1990-2014 (6)
$log(GDP_{pc})$	0.523***	0.439***	0.201**	0.521***	0.451***	0.185
	(0.082)	(0.081)	(0.099)	(0.078)	(0.081)	(0.111)
Gov. Cons.	2.793***	2.777***	0.862	3.341***	3.348***	0.949
	(0.949)	(1.000)	(0.993)	(1.019)	(1.121)	(1.105)
LO_{FR}	0.879***	0.902***	0.635***	0.905***	0.905***	0.598***
	(0.157)	(0.157)	(0.216)	(0.151)	(0.157)	(0.223)
LO_{GE}	1.064***	1.104***	1.017***	1.169***	1.205***	1.070***
	(0.258)	(0.287)	(0.263)	(0.241)	(0.279)	(0.272)
LO_{SC}	0.671***	0.657**	0.673*	0.614***	0.582**	0.674*
	(0.232)	(0.252)	(0.378)	(0.213)	(0.241)	(0.354)
Polity2		0.023**	0.012		0.016	-0.004
		(0.011)	(0.015)		(0.013)	(0.019)
% Tertiary Completed Educ.		-0.002	-0.001		0.000	-0.000
		(0.014)	(0.012)		(0.014)	(0.012)
Absolute Latitude			-0.002			-0.001
			(0.007)			(0.007)
% Pop. Risk Malaria			-0.238			-0.406
			(0.447)			(0.407)
Ethnic Fractionalization			0.275			0.212
			(0.356)			(0.378)
% Euro. Descent Pop.			0.011***			0.011***
			(0.003)			(0.003)
%Protestant			-0.003			-0.005
			(0.004)			(0.004)
%Catholic			-0.001			-0.001
			(0.002)			(0.003)
%Muslim			0.004			0.003
			(0.003)			(0.003)
Constant	-4.498***	-3.810***	-1.704*	-4.467***	-3.882***	-1.221
	(0.753)	(0.784)	(0.925)	(0.770)	(0.784)	(1.108)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
adj. R-sq	0.47	0.47	0.58	0.47	0.47	0.57
Countries (N)	102	91	89	102	91	89
Observations (NT)	459	416	409	359	321	314

Note: The aim of this table is to provide insights on the factors explaining the cross-country differences in the density of judges as observed in Figure 1.2. The presented results are based on an OLS estimation of: $\ln(Density.Judges_{i,t}) = \alpha + \beta_1 \ln(y_{i,t}) + \beta_2\ g_{i,t} + \boldsymbol{\zeta'} \boldsymbol{L}_i + \boldsymbol{\gamma'} \boldsymbol{X}_{i,t} + \delta_t + \varepsilon_{i,t}$ where $i = 1, ...N$ indicates a country and $t = 1, ...T$ is a 5-year time period. The dependent variable $Density.Judges_{i,t}$ is the number of judges per 100.000 inhabitants, y denotes GDP per capita, g stands for the share of government consumption in GDP, $\boldsymbol{L}$ is a vector of legal origin dummies, $\boldsymbol{X}$ is a vector of other explanatory variables, δ_t is a time fixed effect and $\varepsilon_{i,t}$ is an error term. In particular, the vector $\boldsymbol{X}$ contains, depending on the specification, the Polity2 score of democracy, the share of population having completed tertiary education, absolute latitude, percentage of population at risk of contracting malaria, ethnic fractionalization, percentage of population of European descent, and share of population of a given religion. All the mentioned variables are good candidates for explaining the cross-country variations of the density of judges, and are widely used in the macroeconomic literature. We uncover that a very robust predictor of the density of judges is the legal origin. In particular, we find that civil law countries of either French, German, or Scandinavian legal origin, have significantly higher density of judges compared to common law countries. Based on our preferred specification in column 3, French legal origin countries have on average 63% more judges per capita, and German legal origin countries more than double their density of judges compared to common law countries. Within the civil law legal family, we find that German legal origin countries have significantly higher density of judges compared to French legal origin countries, as we reject the null hypothesis of equality of the three civil law legal origins coefficients in column 3 and 6. These findings are consistent with the comparative law literature describing common law as an adversarial dispute resolution system, where the judge has less investigative powers than lawyers, while civil law countries dispute resolution system is inquisitorial, giving more power to the judge (Zweigert and Kötz, 1998). Common law legal system requires therefore less judges and more lawyers than civil law.[a] Differences inside the civil law family can be explained by specificities of the German legal origin in terms of judicial organisation (Schmiegelow and Schmiegelow, 2014). Finally, we confirm the fact that European countries have a significantly higher density of judges (Figure 1.3) as we find a positive and highly significant coefficient for the variable giving the percentage of population of European descent. Outside Europe, this variable might reflect the link between colonization patterns and current institutions (Acemoglu et al., 2005). Given the fact that many new countries gained independence after the 1990 period, columns 4-6 replicate the first three columns considering only data from the 1990-2014 period as a robustness check. Consistent with our previous findings, we confirm the highly significant and sizeable effect found for the three civil law legal origins. Standard errors are clustered at country-level. * p<0.1, ** p<0.05, *** p<0.01.

[a]Explaining the cross-country differences in the number of lawyers, Massenot (2012) finds that common law and French civil law countries have more lawyers than German and Scandinavian legal origins countries.

2

The Impact of Immigration on Workers' Protection

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Abstract

Even though the existing literature investigating the labor market impact of immigration assumes, implicitly or explicitly, that the law or labor market regulation is exogenous to immigration (in terms of both size and composition), this is not necessarily the case. To examine this link, we build a novel workers' protection measure based on 36 labor law variables over a sample of 70 developed and developing countries from 1970 to 2010. Exploiting a dynamic panel setting using both internal and external instruments, we establish a new result: immigration impacts workers' protection in the direction of the origin country workers' protection (composition channel), while we find a small negative or null effect for the immigrant population (size channel). The composition channel, or the law transfer effect, is particularly strong for two components of the workers' protection measure: worker representation laws and employment forms laws. We find suggestive evidence on one mechanism: the law transfer effect materializes through immigrants' distinctive unionization rate, which has an effect on unions' bargaining power and parties' political preference towards labor groups. Finally, calculations based on the estimated coefficients suggest that immigration, on average, contributes to a reduction in workers' protection, particularly in OECD high-income countries.

2.1 Introduction

The rise of international migration in the recent decades has revived interest among economists, political scientists and legal scholars on the effect of immigration on the labor market outcomes and its institutions. An extensive literature measuring the effect of immigration on natives labor market outcomes finds that, on average, immigration has a small or null effect, depending on the methodology and the context of the analysis (see Edo (2019) for a recent survey of the literature). However, one systematic feature of the literature is that labor market regulation is assumed to be, either implicitly or explicitly, exogenous to the presence of immigration. To the best of our knowledge, this assumption has never been tested. The labor market regulation is usually argued to be implicitly accounted for with the inclusion of fixed-effects, and relatively few papers explicitly investigate the impact of immigration depending on the existing labor market regulations, such as the presence of minimum wages or the rigidity of labor contracts (Angrist and Kugler, 2003; D'Amuri and Peri, 2014; Edo, 2016; Edo and Rapoport, 2019; Bächli and Tsankova, 2020). Nonetheless, general concerns related to the implications of immigration on countries' institutions have been raised in response to the growing inflows of immigrants from a broader set of institutionally and culturally different countries (Collier, 2013; Borjas, 2015), and recent evidence shows that immigrants' historical origin-specific preferences influence natives' preferences and institutions (Giuliano and Tabellini, 2020). If immigration indeed impacts labor market regulation, then the *overall* labor market effect of immigration has still not been completely unveiled.

This paper fills this gap by studying the impact of immigration on labor market regulation (in terms of both size and composition), using a dataset on 36 labor laws for 70 countries over a period of 40 years. Our analysis combines four innovative features compared to the existing literature. *First*, we build a novel labor regulation measure, focusing on workers' protection, which we call the *workers' protection index* (WPI). Compared to existing measures, such as Botero et al. (2004) or OECD (2013), our index covers a broader time span for a wide set of countries, allowing us to describe the evolution of labor regulation law across developed and developing countries. We show that WPI is highly correlated with alternative available measures of workers' protection and is related to economic and labor market outcomes. *Second*, we investigate the impact of immigration on workers' protection, in terms of both size and composition, due to their distinct effect on the labor market. The size of the immigrant population mechanically influences both labor supply and skill composition of the workforce, which could have far-reaching implications for labor regulation. As for the composition of immigrant population, we follow the literature using an epidemiological approach (Spilimbergo, 2009; Collier, 2013; Docquier et al., 2016; Valette, 2018). Immigrants' origin-country experience of regulation can shape their behavior in the destination country, which can in turn influence the functioning of the labor market in general, as well as the regulation of workers' protection. *Third*, by looking at the past four decades (medium-run), our empirical set-up distinguishes this paper from the majority of literature, which focuses mostly on short-run or historical events. The medium-run time horizon is distinctive and important for identification, since labor regulation is persistent over time, and it slowly adapts to changes in the country-specific conditions. *Fourth*, we find suggestive evidence on a new mechanism: the composition effect of immigration (or the law transfer effect) materializes through immigrants' distinctive unionization rate, which has an effect on unions' bargaining power and parties' political preference towards labor groups.

To identify the causal effect of immigration, we adopt a dynamic panel specification over four decades - accounting for the high persistency of worker's protection regulation - and estimate it with a system GMM with both internal and external instruments. The internal instruments are used to remove the Nickel bias (Nickell, 1981) in the persistency term. Being aware of immigrants' non-random sorting across countries and the potential presence of time-varying omitted factors that could influence simultaneously workers' protection regulation and migration location, we use external instruments relying on well-known strategies. Namely, the shift-share approach (Card, 2001; Moriconi et al., 2021) to instrument the composition effect of immigration, and the gravity-model based approach (Alesina et al., 2016; Docquier et al., 2020) to instrument the size effect of immigration. In terms of exclusion restriction, the

common critiques to the shift-share approach indicate a possible threat from persistent local conditions (Jaeger et al., 2018; Goldsmith-Pinkham et al., 2020) and potential correlations in the error terms due to similar initial distributions of immigrants by country of origin across destination countries (Adao et al., 2019). These concerns are minor in our setting because of the wide set of historical events that undermine persistent local factors (e.g., the fall of the Soviet Union in 1989, the 1965 amendments to the Immigration and Nationality Act in the US, the constitution of the European Union, and the activation of the Schengen Area in 1995). Nonetheless, following Goldsmith-Pinkham et al. (2020), we show that the initial shares of the most relevant origin countries for our instrument are not related to the initial conditions in the destination countries. Moreover, we find no correlation between our predicted measures and pre-period economic and social trends, nor do we find any significant variations in the error terms after clustering countries by a similar distribution of immigrants' origin countries in the 1960s. As for the exclusion restriction of the gravity-model approach, concerns would arise if the included gravity controls were to affect the evolution of labor regulation through other channels, such as trade or foreign direct investment. However, the highly parsimonious estimated gravity model makes us more likely to satisfy the exclusion restriction and allows us for a more causal interpretation of the results once the predicted stocks are used.

Our paper provides three main findings. We *first* find a strong and positive effect of migrants' level of workers' protection in their origin countries—measured by an epidemiological term *à la* Spilimbergo (2009)—on workers' protection in destination countries. Namely, having immigrants from countries with high levels of workers' protection (such as France or Germany) increases the workers' protection in destination countries; and conversely, having immigrants from countries with low levels of workers' protection (such as UK or US) decreases the workers' protection in destination countries. This is in line with the concept of legal transplants from the comparative legal studies literature. An increase in the epidemiological term of one standard deviation increases the workers' protection index by 7.8% of WPI standard deviations. The effect is robust to our demanding specification, a battery of robustness checks, falsification tests, and alternative competing effects such as diversity, polarization, or skill selection. Additionally, we find that the immigration size effect has a small negative or null impact on workers' protection. *Second*, our results show that two areas of labor regulation are particularly influenced by immigration: worker representation laws (e.g., laws concerning the right to unionize or allowing collective bargaining) and employment forms laws (e.g., laws concerning the flexibility of contracts). Additionally, our paper provides suggestive evidence that immigrants' origin-specific level of labor regulation influences their propensity to unionize: immigrants from countries with higher level of workers' protection are less inclined to seek protection via unions in the destination country, thereby reducing the size of the unionized worker population. We then show that political parties' favorable position on labor groups is negatively related to the size of the unionized workforce, which suggests political actors' attempts to counterbalance the loss of unions' bargaining power as a result of a decreased rate of unionization. *Finally*, taking the baseline coefficients as the "true" ones and an actual variation in the immigrant population in terms of size and composition over the period 1970–2010, we provide back-of-the-envelope computations to evaluate the magnitude of the response of labor regulation to immigration. On average, the computations predict that immigration reduces the WPI by 4.2% standard deviations over the analyzed period. Although the effect is highly heterogeneous across countries, depending on both the size and composition of the immigrant population, the average negative response for OECD high-income countries is 72% stronger compared with non-OECD countries.

This paper contributes to three broad strands of literature. The first is on the overall impact of migration on the labor market in destination countries' and more specifically on natives' labor market outcomes. The overall consensus from a broad set of evidence using different assumptions and methodological approaches is that, on average, immigration has a small or null effect on natives' wages and employment (Borjas, 2003; Peri and Sparber, 2009; Ottaviano and Peri, 2012; Manacorda et al., 2012; Edo, 2019), although it can have some relevant redistributional effects depending on immigrants' location and education level (Card, 2009; Borjas, 2016). More closely related to our work, part of the literature investigates the labor market effect of immigration by exploring the heterogeneity in the effects across

labor market institutions and regulations. Angrist and Kugler (2003) and D'Amuri and Peri (2014) reveal that in more rigid labor markets, natives take more time to adjust to the immigration supply shock by moving toward more complex and less manual tasks, compared with natives in less rigid labor markets. Using French data, Edo (2016) shows that an immigration shock reduces the wages of natives covered by fixed-term contracts and the employment of natives covered by indefinite-term contracts. However, exploiting the non-linear distribution of minimum wages across 51 US states, Edo and Rapoport (2019) provide evidence that higher minimum wages can reduce the negative effect of low-skilled immigration on less educated native workers. Moreover, when protected by labor regulations, natives hold more benign attitudes toward immigrants, which also influences their voting preferences (Bächli and Tsankova, 2020). Even though the literature recognizes the importance of labor regulation for various outcomes, to the best of our knowledge, none of the previous works looked at the effect of immigration on workers' protection, or more broadly on labor regulation law. So far, the literature has treated immigration as exogenous to labor regulation, fixed over time, and "absorbed" by eventual geographic time-invariant fixed effects. Our paper aims to fill this gap by investigating the impact of immigration on labor regulation.

The second broad strand of literature we contribute to investigates the legal institutions' determinants and how they respond to the international movement of factors. Although the literature provides evidence in a cross-sectional setting for the relevance of legal origin and the process of legal transplants in explaining cross-country differences in legal regulation (Botero et al., 2004; Berkowitz et al., 2003b), less is known about the determinants of its evolution over time. Focusing on financial sector regulation, Abiad and Mody (2005) show that economic shocks or political ideology can explain the global process of financial liberalization. Globalization, with a particular focus on trade and capital liberalization, has been linked to the process of labor deregulation (Boulhol, 2009). Facchini and Willmann (2005) provide a theoretical model based on the common agency problem explaining the regulation reaction to the international movement of factors, such as trade or immigration. Concerning the demand for labor market regulation and welfare state, evidence shows the relevance of family ties (Alesina et al., 2015) and generalized trust (Algan et al., 2016) as potential determinants. Additionally, concerns about the response of institutions to immigration have been raised with respect to the potential disruptive effect of norms and habits brought by immigrants coming from distant countries on well-functioning Western institutions (Collier, 2013; Borjas, 2015). However, the available set of evidence shows that immigrants have a small positive or no effect on market-functioning institutions and economic freedom (Clark et al., 2015; Powell et al., 2017; Baudassé et al., 2018). With an origin-country perspective, few authors provide evidence that immigrants' experience of institutions and productive capacity in destination countries has an effect on origin countries' institutions (Spilimbergo, 2009; Docquier et al., 2016; Valette, 2018). More related to our paper, Giuliano and Tabellini (2020) shows that the exposure to European immigrants, with stronger preferences for redistribution, during the Age of Mass Migration have long-lasting effect on Americans political ideology. Focusing on a broader set of countries and a decennial time-span, our paper contributes to this rising literature by providing the first set of evidence on the impact of immigration on an important part of institutions, which is the law or labor regulation.

Third, we contribute to the comparative legal studies literature and more specifically to the literature on legal transplants. The concept of legal transplants was introduced in the seminal work of Watson (1974). As one of the examples, the author provides an insight that the private law of many countries is fundamentally based on the reception of Roman law and argues that society's laws do not usually develop as a logical outgrowth of solely its own experience. Moreover, the author argues that the law cannot be used as a tool to understand societies without taking legal transplants into account. In the contemporary legal transplant literature, comparative lawyers agree that a country's legal culture can be transplanted through legal education, methods, and mentalities (Twining, 2009; Del Duca and Levasseur, 2010; Graziadei, 2006). Kalantry (2020) presents a case study on reverse legal transplants from poor to rich countries, with the movement of restrictions on abortion from India to the United States. Hans (2017) explains the importance of connections between international scholars and other research networks for transplantation of trial by jury. We contribute directly to this strand of literature by providing the first empirical evidence on how immigrants can be a source of legal transplant or law transfer.

The rest of the paper is organized as follows. Section 2.2 presents the data, the construction of our workers' protection index and immigration variables. Section 2.3 shows our empirical approach, the identification strategies, and potential alternative effects driven by immigration. Section 2.4 shows the main results of the analysis, the robustness checks, and the falsification tests. Section 2.5 explores the potential mechanism on the different subcomponents of the workers' protection index and on immigrants' participation in unions. Section 2.6 discusses the magnitude of the effect after back-of-the-envelope computations of the effects. Finally, Section 2.7 concludes.

2.2 Data and Stylized Facts

This paper combines different data sources for 70 countries in 5-year periods over a long time span from 1970 to 2010. Section 2.2.1 describes the data associated with workers' protection, explains the construction of the workers' protection index, and shows relevant correlations with alternative measures of workers' protection and economic outcomes. In Section 2.2.2, we present the immigration data, the construction of the epidemiological term based on the level of workers' protection that immigrants experience in their origin countries and the evolution of the immigrant population over time.

2.2.1 Workers' Protection Index

To construct the novel workers' protection index, we use the Leximetric dataset developed by legal scholars (Adams et al., 2017). This dataset quantifies the level and evolution of labor law and workers' protection based on the "law-in-the-books". The Leximetric data on workers' protection covers 117 countries over the 1970–2013 period. For few post-socialist countries, the data are available only after 1990.[1] The dataset documents the degree of legal protection associated with permanent and part-time workers. However, when the law sets different standards across different groups of workers (e.g., blue-collar and white-collar workers), the dataset enlists the degree of protection associated with the least protected group. This aspect implies that the dataset is capturing either the average or the minimum degree of protection guaranteed to the least protected workers.

The dataset includes 36 relevant variables associated with different aspects of workers' protection.[2] We assign a value between zero and one to each of those variables, where zero stands for no protection/lowest protection possible, while one stands for the maximum protection available in that area. All the variables are categorized into five broad areas related to workers' protection: employment forms laws ($EmptForm$), working time laws ($WorkTime$), worker dismissal laws ($WkrDismis$), worker representation laws ($WkrRepr$), and industrial action laws ($IndAction$). The first area represents the law governing the definition of the employment relationship and employment forms, which accounts for the legal difference across different employment forms and their maximum duration. It has maximum value, for instance, when workers that have temporary/fixed-term contracts are protected. $WorkTime$ covers issues related to holidays, extra compensation in case of overtime working hours, and the duration of working weeks. The variables take the value of one when holidays and overtime activities are well compensated and when workers have a reasonable amount of working hours per week/day. The third area ($WkrDismis$) is related to the length of notice before dismissal, the constraints that employers have to face before firing an employee, and eventual compensation after dismissal. The fourth area ($WkrRepr$) provides information related to workers' right of unionization and collective bargaining and on unions' right to nominate representatives for companies' boards of directors. Variables take the value of one when the workers have a right to be represented and voice their concerns through unions. The last area ($IndAction$) is related to the rights of industrial actions and striking. This area covers the part of legislation that grants workers' rights to strike and reduces employers' rights to lockout. These five broad areas have a high degree of correspondence with the

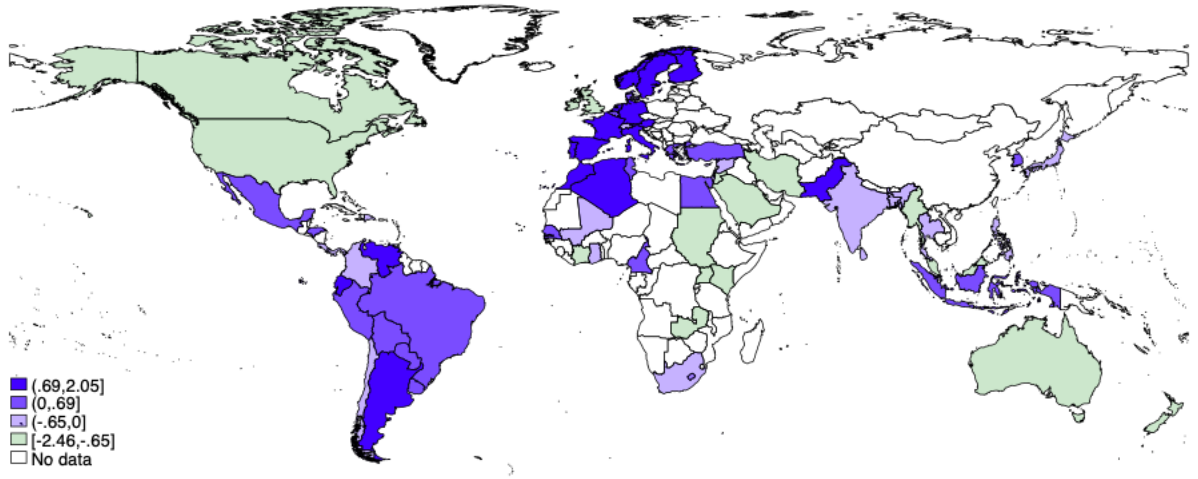
[1] To have a more balanced sample, we keep only the countries that have data from 1970 on in our final sample.

[2] A list of all workers' protection variables is available in Table 2.11.

categories analyzed by Botero et al. (2004) in a cross-sectional setting, which later provided the methodological basis for the World Bank's Doing Business reports.

The wide range of legal issues covered by the Leximetric data gives us a comprehensive measurement of different aspects related to workers' protection. Nonetheless, to have a general overview of the evolution of labor law regulation, we perform the following steps to construct one synthetic measure of workers' protection at the country level. First, following the structure of the data, we build five indicators associated with the five areas of working protection measured by the Centre for Business Research (CBR) researchers. Following Preacher and MacCallum (2003) guidelines, we aggregate the variables associated with each area through a factor analysis, and we standardize them with mean zero and a standard deviation equal to one.[3] Second, we perform a second factor analysis over the five aggregated indicators associated with the five legislative areas of workers' protection to build one synthetic indicator. We define the first standardized component of this latter factor analysis as our *workers' protection index* (WPI).

Figure 2.1. Workers Protection Index - Geographical Distribution



Note: Authors' calculations on CBR Leximetric data. The figure plots the average standardized workers' protection index by quartile at the country level over the 1970–2010 period.

Figure 2.1 shows the geographical distribution of the average workers' protection index. European countries (excluding the United Kingdom) are characterized by a high level of WPI, with Portugal having the highest average WPI (2.05). Pakistan is the only country in Asia with a WPI comparable to Continental Europe, which has a WPI of 1.12. Among high-income developed societies, countries with a common law legal system (e.g., the United States, United Kingdom, Australia, and New Zealand) are characterized by a systematically lower level of workers' protection. The United States in particular is characterized by the lowest WPI level in our sample (-2.46). This is not surprising, since common law legal systems are on average less codified and more protective on the side of investors (La Porta et al., 1997, 2008).[4] Concerning developing countries, Latin American countries are characterized by a high level of workers' protection overall. The only exception is Chile (-0.35), which is characterized by a lower level of WPI compared to its neighboring countries, as a result of the changes in law and institutions

[3]The results of the factor analysis are available in Appendix 2.8.2. Since the structure of the data and the relation among variables are theoretically well defined by the CBR researchers, we follow Preacher and MacCallum (2003), who suggest implementing factor analysis in these cases to identify the sources of common variation, as opposed to a principal component analysis, which aims to explain as much variance as possible. The indexes are the first standardized component from each specific legislative area.

[4]In Table 2.34, we look at the possible determinants of the values shown in Figure 2.1 using a simple OLS. We confirm a strong negative and highly significant relationship between common law legal origin and WPI. Depending on the set of controls, we find positive correlations of WPI with the epidemiological term, GDP p.c. and democracy, whereas we find a negative correlation with the size of immigration.

under the Pinochet regime (Borzutzky, 2005). A large degree of heterogeneity in WPI is reported in African and Middle Eastern countries, from countries with a reasonably high degree of WPI, such as Algeria (1.59) and Morocco (0.76), to countries with a low level of WPI, such as Saudi Arabia (-1.06) and Kenya (-1.2).

Figure 2.2. Workers' Protection Index - Evolution over Time

(a) WPI by development level and legal origin

(b) WPI and its sub-components

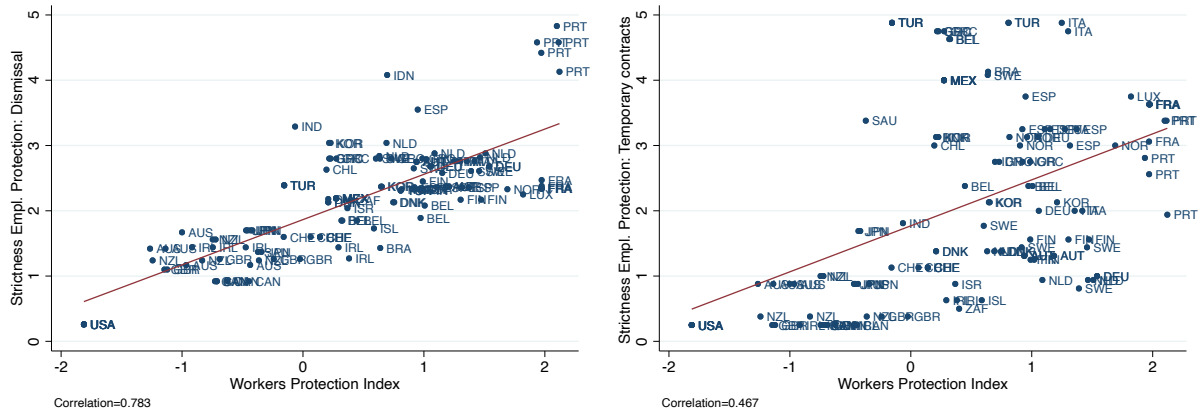
Note: Authors' calculations on CBR Leximetric data. Figure (a) plots the average standardized workers' protection index by destination countries' level of development and legal origin. Figure (b) plots the average standardized workers' protection index and its five subcomponents.

The historical evolution of the workers' protection index is available in Figure 2.2. The dotted blue line in both Figure 2.2(a) and 2.2(b) shows that the degree of workers' protection is on an increasing trend from 1970 to 2010, moving from an average level of -0.6 to 0.5. However, some heterogeneous trends depending on countries' economic development and legal origin are also evident. Figure 2.2(a) shows that OECD high-income and non-OECD countries were experiencing similar levels of WPI until 1975. However, from 1980 on, the WPI grows faster in OECD high-income countries compared to others. In terms of legal origin, even though the trends are parallel from 1970 to 2010, countries with a civil law legal system experienced a substantially higher degree of WPI compared to countries with a common law legal system. This evidence is in line with the literature that recognizes systematic differences in the level of regulation across countries (La Porta et al., 1997). Figure 2.2(b) plots the WPI with its five subcomponents. During the whole analyzed period, worker dismissal, worker representation, and employment forms protection laws have increased as the aggregated index. However, the industrial action laws and working time protection laws have rather stagnated.

The Leximetric data are able to tap into multiple aspects of the legislation associated with workers' rights. Moreover, as the authors of the database point out, the data aim to capture how the law protects the labor relations between employers and employees, rather than the actual cost that the legislation imposes on employers.[5] However, the Leximetric dataset is not the only one available that encompasses the legal aspects related to workers' protection. Over a smaller sample of countries and a reduced time span, the OECD Employment Protection database provides some indicators of employment protection (OECD, 2013). In particular, we focus on two indicators that are available for 33 countries over the 1990–2010 period. The first is the index of the strictness of employment regulation on individual and collective dismissal (DI^{OE}). It captures procedures and costs involved in dismissing workers, such as procedural inconveniences that employers have to face through the dismissal process, notice period, and

[5]The CBR researchers specify three main reasons for this. First, it is not possible to infer from the existence of a given legal rule any effect on behaviors that will affect firms' costs. Second, the existence of a law-in-the-books does not imply the degree of its actual observation in practice. Third, an increase in workers' protection can also have beneficial effects on firms' costs, such as the reduction in transaction costs after introducing collective bargaining.

Figure 2.3. Workers' Protection Index - Correlations with OECD Employment Protection Data



(a) Individual and Collective Dismissal

(b) Temporary Contracts

Note: Authors' calculations on CBR Leximetric data (x-axis) and OECD Employment Protection Database (y-axis). The figure plots the country-period level of the standardized workers' protection index on the country-period level of the index of the strictness of employment regulation on individual and collective dismissal (DI^{OE}) (Figure (a)) and the country-period level of the index of the strictness of employment regulation on temporary contracts (TC^{OE})(Figure (b)).

severance pay. Second is the index of the strictness of employment regulation on temporary contracts (TC^{OE}). It measures the duration, regulation, and conditions associated with temporary contracts compared to permanent contracts. Both indicators have high values when workers have a higher degree of protection (i.e., more hindrances to employers when they want to fire a worker and when they want to hire a temporary/fixed-term worker). Figure 2.3 shows the correlations between the WPI and the two OECD indicators. In both Figures 2.3(a) and 2.3(b), the WPI is positively correlated with the OECD indicators, and the correlations are statistically significant at a 1% level. Additionally, in Figure 2.11 we cross-validate each of the five WPI subcomponents with the related cross-sectional labor regulation measures available Botero et al. (2004), and we find strong and positive correlations between the proxies. Overall, we provide evidence on correlations across different related data sources for our measure of workers' protection and its main subcomponents, which is reassuring for the external validity of our measure.

2.2.2 Immigration Data and the Epidemiological Term

We combine two different data sources to have a more comprehensive picture of the immigrant population over a broad sample of destination countries. First, we rely on the Global Migration data by Özden et al. (2011), which combines several censuses and population registers. This dataset provides decennial matrices of bilateral migration stocks between 1960 and 2000. Owing to its long time span and coverage of over 200 destination and origin countries, this dataset has been used in several cross-country and panel studies (e.g., Beine and Parsons (2015); Cattaneo and Peri (2016)). We combine and harmonize it with the World Bank Bilateral Migration Matrix of 2010 (World Bank, 2010), such that we have a decennial coverage from 1960 to 2010. Finally, to deal with the mixed frequency of the data (the outcome is in five-year periods), we interpolate the decennial bilateral migration stocks to five-year periods.[6]

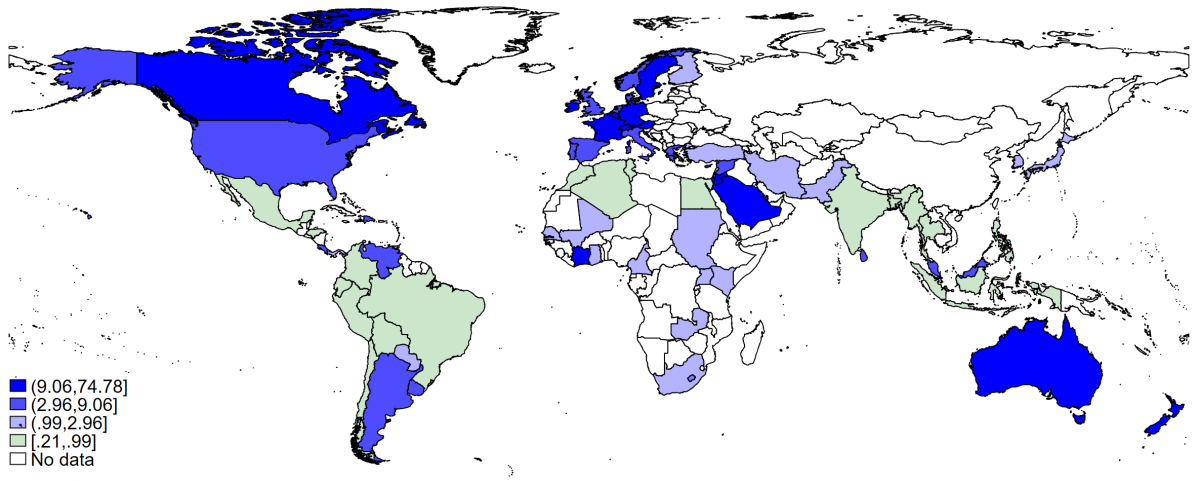
To measure the size of the immigrant population, we first compute for each country of destination d at year t the share of immigrants in the total population of 2000 as follows:

[6]We perform such interpolation to have more data points, which will be relevant for the strength of our estimation strategy. Apart from using a simple average, we do recognize there are better imputation methods that take into account multiple demographic dimensions (Standaert et al., 2022). Nevertheless, when we remove interpolated observations (i.e., 1975, 1985, 1995, and 2005), the main results remain unchanged, as column (4) in Table 2.2 shows.

$$ShareMig_{d,t} = \frac{MIG_{d,t}}{Pop_{d,2000}}, \tag{2.1}$$

where $ShareMig_{d,t}$ is the share of immigrants and $MIG_{d,t}$ is the total stock of immigrants. Following Moriconi et al. (2021), we compute the share of immigrants using the population in a fixed year as the denominator to uniquely identify the source of variation in the changes in the immigrant stocks.[7] Such measure is a proxy of immigrant population size, which can influence countries' economy and legislative aspects. For instance, a higher share of immigrants would imply a higher labor supply, which can have a direct effect on wages and employment (see Borjas (2003); Edo (2019)). To avoid potential unwanted effects on native workers' economic outcomes, institutions could react by changing labor market institutions and laws.

Figure 2.4. Share of Immigrants - Geographical Distribution



Note: Authors' calculations on Özden et al. (2011) and World Bank data. The figure plots the average share of immigrants over the 2000 population by quartile at country level over the 1970–2010 period.

Figure 2.4 shows the geographical distribution of the average share of immigrants as computed in equation (2.1) over the period 1970–2010. OECD high-income countries are characterized by a sizeable migration share. However, Qatar has the highest value in our sample (74.78), followed by Israel (30.15), Luxembourg (26.05), and Singapore (24.59).[8] Developing countries in Latin America, Africa, and Asia are characterized by a lower share of immigrants. Looking at the time variation of the share of immigrants, Figure 2.5 shows that the average share of immigrants evolves with a similar trend both across countries' level of development (Figure 2.5(a)) and across countries' legal origin (Figure 2.5(b)).

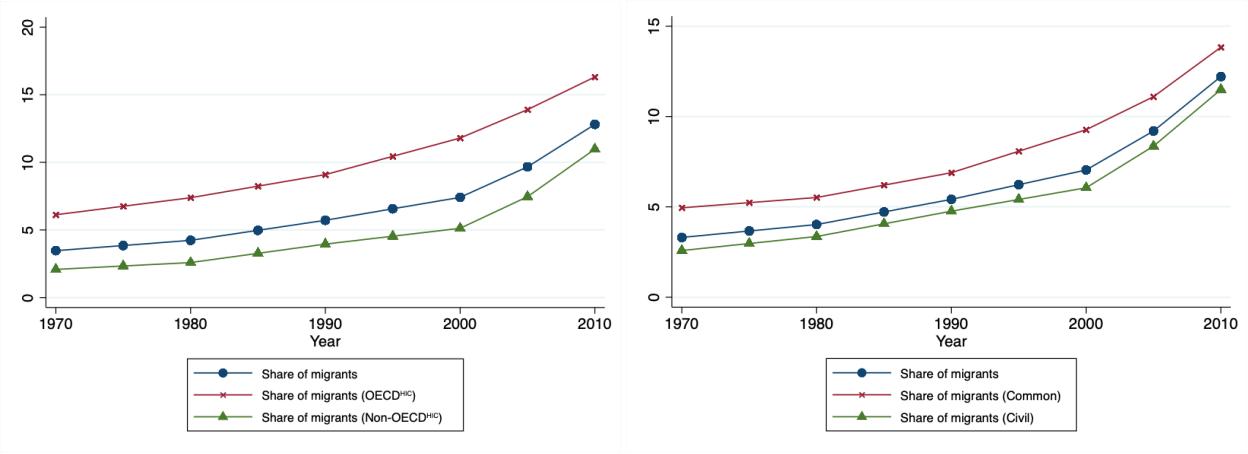
Migration can influence destination countries not only because of its size, but also because of its composition. Aspects such as immigrants' education and their capacity to expand the knowledge set of a given country as a result of their novel competences and skills are just a few examples of how immigrants' characteristics could affect natives' behavior and countries' economies (e.g., Borjas (2019); Bahar et al. (2020); Moriconi et al. (2019); Docquier et al. (2020)).[9] In terms of the composition effect, Collier (2013) points out that immigrants can convey to destination countries the institutions and social norms of their country of origin. However, our paper differs from Collier (2013) and other related

[7] In Table 2.31 in the Appendix, we test our main results using the share of immigrants over the current population rather than the share of immigrants over a fixed population, as shown in equation (2.1). The main results remain unchanged.

[8] Those countries are characterized by a large immigrant population because of the structure of the labor market and institutions (see De Bel-Air (2014) for Qatar). The large size of the immigrant population, for example in Israel, is related to historical reasons, such as the exodus of soviet Jews from Russia to Israel in the 1990s after the collapse of the USSR (Smooha, 2008).

[9] Even though aspects such as diversity, polarization, and skill selection are not the main focus of our paper, we test for them in Table 2.3.

Figure 2.5. Share of Immigrants - Evolution over Time



(a) OECD^{HIC} vs. Non-OECD^{HIC} (b) Civil Law vs. Common Law

Note: Authors' calculations on Özden et al. (2011) and World Bank data. Figures (a) and (b) plot the country average share of immigrants over the 2000 population by destination countries' level of development (Figure (a)) and legal origin (Figure (b)).

studies on the transfer of broadly defined institutions, norms and values. Namely, we address empirically the question of immigrant legal transplants, which is something that has been discussed among legal scholars but never systematically tested to the best of our knowledge. Giuliano and Tabellini (2020) show that European immigrants' preferences toward redistribution could have influenced US natives' political preferences during the age of mass migration. We follow Spilimbergo (2009) and Valette (2018) to account for potential origin-specific and epidemiological effects by computing the following index:

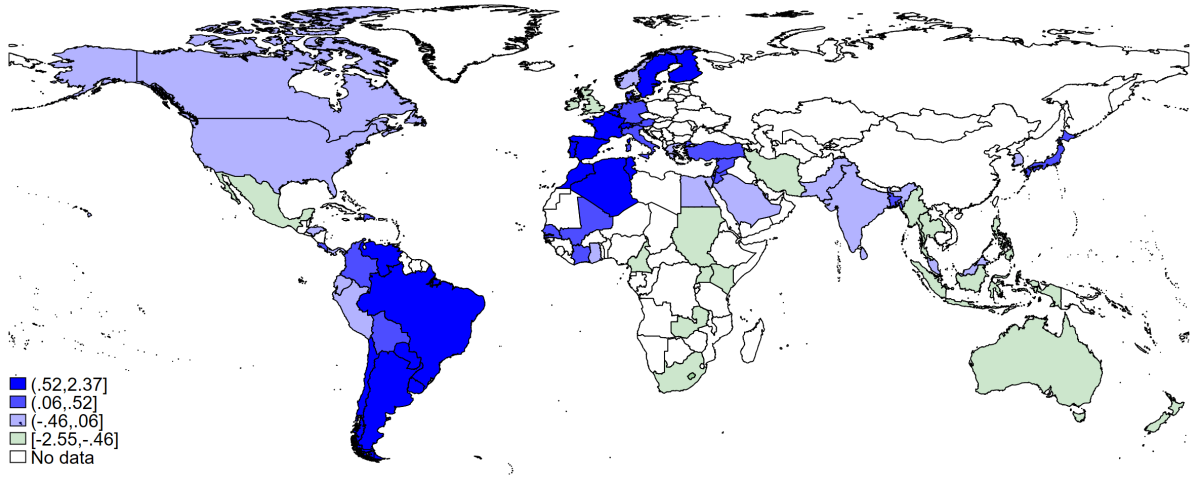
$$Epid_{d,t} = \sum_{o} \frac{MIG_{o,d,t}}{\sum_{o}^{O} MIG_{o,d,t}} * WPI_{o,2000} = \sum_{o} \overline{mig}_{o,d,t} * WPI_{o,2000}. \tag{2.2}$$

The index $Epid_{d,t}$ captures for a country of destination d at year t the degree of workers' protection experienced by its immigrant population in their origin country. It is measured as a weighted average of the WPI in the origin countries in the year 2000, using as weights the share of immigrants coming from country of origin o and living in country d over the total immigrant population in country d.[10] We proxy for the degree of workers' protection in the country of origin with the WPI in the year 2000 for two reasons. First, by fixing the level of WPI at the year 2000, variations of the epidemiological effects are driven *exclusively* by changes in the composition of the immigrant population, rather than changes in the labor regulation in origin countries. That allow us to clearly identify the source of variation of the epidemiological effect. Second, a lot of countries enter the Leximetric data starting only from 1990. To have the broadest geographical coverage in terms of WPI for origin countries, we take as a reference the year 2000. For such year, we are able to compute the WPI for 116 countries of origin. Although we cover the majority of the countries in terms of size and population, for a few countries of origin, we still do not know the level of WPI. We then impute the missing countries with the average level of WPI in 2000 based on their legal origin.[11] Nonetheless, Table 2.32 in the Appendix provides the results after including an epidemiological effect constructed with a time-varying WPI. The main results are confirmed

[10] Alternatively, Table 2.3 provides the results after computing the epidemiological effect using as weights the share of immigrants coming from country of origin o and living in country d over the total population in destination country d.

[11] As Figure 2.2(b) and Table 2.34 show, the legal origin rather than the level of development is one of the best predictors of the level of workers' protection. We are aware that this imputation procedure might affect our results. For this reason, we perform in Table 2.2 two robustness checks. First, we drop from the sample countries characterized by a high percentage of imputed WPI within their epidemiological term (i.e., more than 30% of the immigrant population coming from a country with an imputed WPI). Second, we compute the epidemiological term with different imputation methods: (i) not imputing the values of missing countries (*Epid strict*); (ii) imputing the missing countries with the minimum value by legal origin

Figure 2.6. Epidemiological Term - Geographical Distribution

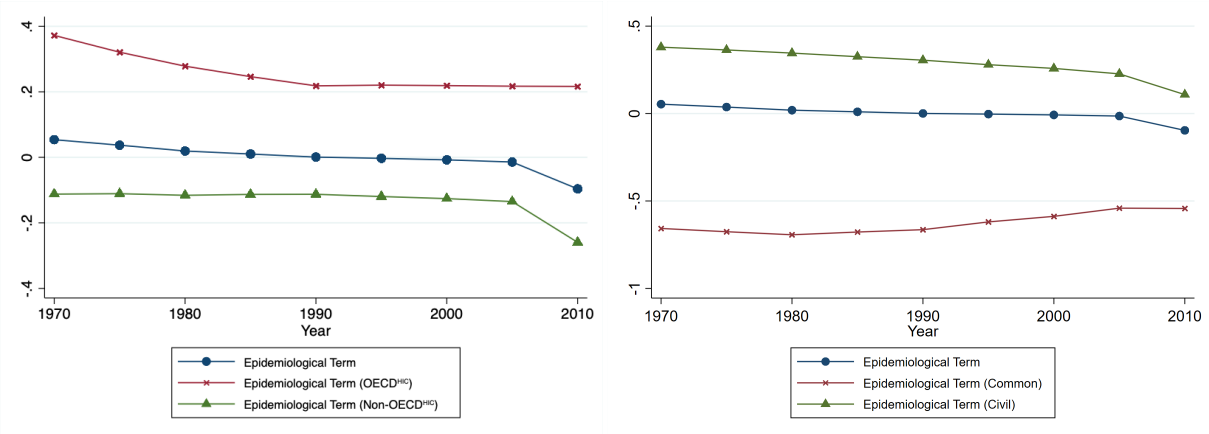


Note: Authors' calculations on Özden et al. (2011), World Bank data, and CBR Leximetric data. The figure plots the country average epidemiological term (as we compute in equation (2.2)) by quartile at country level over the 1970–2010 period.

both in terms of magnitude and significance, which is not surprising, given the high persistence of the WPI variable.

Figure 2.6 presents the geographical distribution of the average epidemiological term over the period 1970–2010. The distribution is rather heterogeneous across continents. The country characterized by the highest epidemiological term is Morocco (2.37), followed by Luxembourg (1.97), and Tunisia (1.76). On the other end, countries with the highest share of immigrants from countries with a low WPI are Mexico (-2.55), Indonesia (-1.9), and Ireland (-1.89).

Figure 2.7. Epidemiological Term - Evolution over Time



(a) OECD^{HIC} vs. Non-OECD^{HIC} (b) Civil Law vs. Common Law

Note: Authors' calculations on Özden et al. (2011), World Bank data, and Leximetric data. Figures (a) and (b) plot the country average epidemiological term (as we compute in equation (2.2)) by destination countries' level of development (Figure (a)) and legal origin (Figure (b)).

Figure 2.7 provides some more clear evidence on the evolution and average value of the epidemiological term across countries' level of development and legal origin. First, as the dotted blue line shows, the

(*Epid min*), and (iii) imputing the missing countries with the maximum value by legal origin (*Epid max*). The results remain unchanged across these different robustness tests.

epidemiological term experienced a slow decline from the 1970s to 2010. This trend shows that, among our sample of countries, through time new immigrants were coming from countries with a lower level of workers' protection compared to the ones already in the destination countries. Second, developed and civil law countries are characterized by a higher proportion of immigrants from countries with high WPI, compared to developing and common law countries. Third, common law countries are the only ones that experienced a small but positive trend in the epidemiological term.

2.3 Empirical strategy

Our goal is to empirically evaluate the impact of immigration on workers' protection, in terms of both size and composition by countries of origin. Section 2.3.1 describes our linear dynamic panel model specification and the system GMM estimation technique we employ. In Section 2.3.2 we present our shift-share approach and the gravity model, which are necessary to instrument our variables of interest within the system GMM framework. Last, Section 2.3.3 discusses alternative and competing effects driven by immigration: diversity, polarization, and skill selection.

2.3.1 Empirical model and estimation technique

Our estimation strategy uses five-year periods of all variables (from 1970 to 2010) to address the medium-run perspective of law changes, to rule out short-run (e.g., annual frequency) fluctuations in the data, and to better harmonize the occurrence of gaps from the mixed frequency of the data.[12] All of our explanatory variables are lagged with a one five-year period, as it usually takes a few years before the labor laws respond to changes induced by our explanatory variables. Since labor law is highly persistent over time, we use a linear dynamic specification.[13] Other studies used the same dynamic panel specification to measure the effect of migration on different institutional outcomes (Spilimbergo, 2009; Docquier et al., 2016). We estimate the following model:

$$WPI_{d,t} = \alpha + \beta WPI_{d,t-1} + \gamma ShareMig_{d,t-1} + \delta Epid_{d,t-1} + \theta \mathbf{X_{d,t-1}} + \eta_t + \zeta_d + \epsilon_{d,t}, \tag{2.3}$$

where $WPI_{d,t}$ is the workers' protection index in destination country d at year t. The $WPI_{d,t-1}$ is one-period lag of the outcome variable that allows us to account for the persistence in the workers' protection index. Our variables of interest are both $Epid_{d,t-1}$ and $ShareMig_{d,t-1}$, which are accordingly the epidemiological term and the share of immigrants at the destination country d in period $t-1$. The vector $\mathbf{X_{d,t-1}}$ includes controls (such as GDP, political regime, and human capital) that could affect simultaneously our variables of interest and the outcome. In addition, ζ_d denotes a country fixed effect, η_t is a time fixed effect, and $\epsilon_{d,t}$ is the error term.

The set of controls that we include are borrowed from the economic growth literature and are ex-ante important for both the workers' protection index and immigration: economic development level, political regime, and human capital level. More developed countries, on average, have higher values of WPI (recall Figure 2.2). The political regime is another relevant control because we observe that labor regulation may be influenced by political shocks—for instance, as was the case under the Pinochet regime (Borzutzky, 2005). Finally, we can reasonably argue that human capital, contributing to the overall development of a country and being correlated with a country's institutions (Acemoglu et al., 2014; Faria et al., 2016), can also contribute to the development of labor market institutions.

Our analysis employs a system GMM estimator to estimate equation (2.3). This estimation technique accounts for the unobserved heterogeneity, persistence, and potential endogeneity of other regressors.

[12] To further scrutinize the empirical analysis, we experiment with alternative specifications and different time periods (i.e. 10-year instead of 5-year periods) in Table 2.2.

[13] We can see the high persistency of the WPI in Figure 2.2, as well as in our main results in Table 2.1 where our autoregressive coefficient is always above 0.8.

Blundell and Bond (1998) and Bond et al. (2001) suggest that system GMM is the most adequate estimator in a dynamic panel setting if the time series are highly persistent, as they are in our case. This estimation strategy allows us to circumvent the dynamic panel bias stemming from the inclusion of the lagged dependent variable in a within-group estimator with a short time period, also known as the Nickel bias (Nickell, 1981). We use a two-step system GMM procedure, which is asymptotically more efficient than the one-step procedure, but it has potentially downward-biased standard errors in small samples (Bond et al., 2001). Consequently, we use Windmeijer (2005) finite sample correction, which gives more accurate estimates in small samples. The validity of the estimator relies on crucial Arellano and Bover (1995) conditions, which are tested with Hansen's J and difference-in-Hansen tests along with each regression. Furthermore, we follow Bazzi and Clemens (2013) by performing various weak instrument diagnostics to ensure that the estimated coefficients are unbiased.

Within the system GMM framework, we use both internal and external instruments to obtain consistent estimates. For the internal instruments part, the instruments used in the difference equation are lagged levels, whereas the instruments in the level equation are lagged differences of the corresponding variables; the regressions in both differences and levels are then combined into a single system. To avoid arbitrary exogeneity assumptions, we treat all right-hand-side variables as endogenous, as is most common in the literature. However, this decision leads to numerous instruments that can potentially overfit the instrumented variables. We handle this by collapsing the matrix of instruments and reducing the lag structure to have fewer instruments than countries, as suggested by Roodman (2009). We keep the same instrument set across all regressions to be transparent and to avoid the "ad hoc" choice of internal instruments for each specification. More specifically, we instrument $WPI_{d,t-1}$ always with its third to seventh lag, and $X_{d,t-1}$ with its second to fourth lag.[14] As for our variables of interest, $ShareMig_{d,t-1}$ and $Epid_{d,t-1}$, we instrument them using external instruments which we create using the shift-share and gravity approach. In the next subsection, we explain the construction of our external instruments in more detail.

2.3.2 Identification strategy: shift-share and gravity-model based instruments

Estimating γ and δ from equation (2.3) allows us to retrieve the partial correlation between immigration (share of immigrants and the epidemiological term) and countries' workers' protection, after accounting for other controls and the persistency of the dependent variable. However, the estimated partial correlations could be affected by two main sources of bias. First, unobserved time-varying country characteristics, captured by the error term, could influence a country's labor law and immigrants' destination country choice at the same time. The direction of such bias is unclear, since different unobserved factors could play a role. Second, the correlation between immigration and labor law could suffer from reverse causation. For instance, if immigrants are attracted by countries with higher or similar levels of workers' protection compared to their origin country, then both γ and δ would suffer from an upward bias. Using a GMM estimator with internal instruments partially accounts for both sources of bias. However, if unobserved time-varying factors and reverse causation influence not only current but also past immigrant decisions, the source of bias could persist over time. To strongly mitigate these potential biases and estimate the true causal relationship of immigration on workers' protection, we rely on two well-known instrumental variable approaches to build valid external instruments.

Our first instrumental variable approach is based on the shift-share methodology (Card, 2001; Ottaviano and Peri, 2006; Moriconi et al., 2019). The intuition behind this methodology is to use past settlements of immigrants by country of origin as a predictor of subsequent migration flows arising

[14] In Table 2.26, we test for different lag structures of the internal instruments, and our main results remain robust to various lag structures.

from network effects.[15] We then allocate the aggregate immigration flows by country of origin, mainly driven by push factors, to the sample of destination countries following a historical distribution of the population of immigrants by country of origin. If the historical distributions of immigrants are uncorrelated (or weakly correlated) with the current unobserved factors and the country's labor law, then the predicted migration stocks are also uncorrelated (or, at the very least, less correlated). To compute the predicted bilateral stocks, we first use Özden et al. (2011) data and compute the historical distribution of immigrants from country of origin o in destination country d in the year 1960 as follows:

$$sh_{o,d,1960} = \frac{MIG_{o,d,1960}}{\sum_d^D MIG_{o,d,1960}}. \tag{2.4}$$

Equation (2.4) computes the share of immigrants from country o in destination country d in year 1960 over the total stock of immigrants from the same country of origin. We then compute the total aggregate stocks of immigrants from country of origin o for the years $t \in \{1970, 1975, ..., 2010\}$ as follows:

$$TM_{o,t} = \sum_d^D MIG_{o,d,t}. \tag{2.5}$$

Finally, we can compute the predicted bilateral stocks of immigrants from country of origin o to destination country d in year t as follows:

$$\widetilde{MIG}_{o,d,t}^{SS} = TM_{o,t} * sh_{o,d,1960} \tag{2.6}$$

Relying on the literature studying the effect of diversity in the immigrant population on countries' economic growth, the shift-share approach is a good predictor of the immigrant population composition as opposed to its size (Alesina et al., 2016; Bahar et al., 2020; Docquier et al., 2020); we use the predicted bilateral stocks computed in equation (2.6) to compute a predicted measure of the epidemiological effect ($\widetilde{Epid}_{d,t}^{SS}$). We employ this variable as our external instrumental variable for the epidemiological effect.[16]

The second instrumental variable approach is based on Alesina et al. (2016) and Docquier et al. (2020), which estimate a gravity model to predict the bilateral stocks of immigrants. Following their methodology, we propose a parsimonious gravity model that (i) minimizes the potential violation of the exclusion restriction and (ii) includes year dummies interacted with the geographical distance between origin and destination country. As Feyrer (2019) suggests, these interactions should capture the declining cost of displacement due to a reduction in transportation costs. The gravity model is specified as follows:

$$MIG_{o,d,t} = \beta Dist_{o,d} * I_t + \theta_d + \gamma_t + \epsilon_{o,d,t}, \tag{2.7}$$

where $MIG_{o,d,t}$ is the stock of immigrants from country of origin o to the country of destination d in year t. The set of controls includes interactions between bilateral distance (weighted by population size) and year dummies ($Dist_{o,d} * I_t$), year fixed effects (γ_t), and destination country fixed effects (θ_d).[17] As Figure 2.10 in the Appendix shows, bilateral geographical distance is unrelated with bilateral labor regulation distance, mitigating concerns of a potential correlation with origin-destination institutions. Given the high number of zeros due to empty bilateral corridors, we estimate equation (2.7) using a Poisson pseudo maximum likelihood (PPML) estimator, as suggested by Silva and Tenreyro (2006), and we cluster the standard errors at the country level. Table 2.24 shows the estimated coefficients of the gravity model. We then use the predicted coefficients from the estimated gravity model to compute

[15] Bertoli and Ruyssen (2018) show that intending migrants are more likely to move to countries where they have peers and friends.

[16] We also compute a predicted measure of the share of immigrants $\widetilde{ShareMig}_{d,t}^{SS}$; however, it appears to be a weak instrument.

[17] The measure of bilateral weighted distance comes from Head et al. (2010), and it is based on distances among the biggest cities of the countries weighted by their share of population. Year fixed effects captures common time trends, while country fixed effects captures the time-invariant unobserved heterogeneity in destination countries.

the predicted bilateral stocks ($\widehat{MIG}^{G}_{o,d,t}$). Since the estimated predicted bilateral stocks are less driven by reverse causation and unobserved factors, we use them to compute predicted immigration shares ($\widehat{ShareMig}^{G}_{d,t}$). We use this latter variable as an external instrument for our GMM approach.

We follow Bahar et al. (2020) and use both IV strategies simultaneously to instrument our migration variables: shift-share approach to instrument the epidemiological effect and the gravity-based approach to instrument the share of immigrants. Both instruments pass the Bazzi and Clemens (2013) test on weak instruments in a system GMM context.[18] Despite their common use in the literature, both approaches have some drawbacks. Even though extremely parsimonious, our gravity model could violate the exclusion restriction if countries' geographical closeness has an effect not only on migration but also on the degree of workers' protection. However, if such effect is related to any kind of economic channels, the inclusion of GDP per capita as a control should account for it. Moreover, the average level of countries' workers' protection index is uncorrelated with the number of neighboring countries, which is a proxy for geographical closeness.

Concerning the shift-share approach, criticisms have been raised related to the role of persistent factors: if persistent local conditions influence immigrants' location and workers' protection, then an omitted variable bias could arise (Jaeger et al., 2018; Goldsmith-Pinkham et al., 2020). Relying on the assumption of exogenous initial shares for the validity of the instrument, we follow Goldsmith-Pinkham et al. (2020) to provide evidence in Section 2.8.3 in the Appendix that the initial distribution of immigrants by origin across destination countries in the 1960s is exogenous to destination countries' specific factors. By computing the Rotemberg weights across different periods in time, Table 2.27 identifies the origin countries that identify the highest variation in the IV.[19] Then, in Table 2.28, we show the correlation between the top origin-specific shares identified by the highest Rotemberg weights and a set of country characteristics in 1960. We do not find any significant correlations, both across different origins and in the variation of the predicted epidemiological term, suggesting that our initial shares are exogenous to destination country factors. Finally, using historical data from the Maddison Project (Bolt et al., 2018) and following Moriconi et al. (2021), we check the correlations between the instrument and the pre-1960 economic trends. Table 2.29 in the Appendix shows the coefficients from regressing the growth of the predicted epidemiological term on the pre-1960 growth of GDP per capita (Panel A) and population (Panel B) over different time periods. None of the correlations are statistically significant. This is also the case when we regress the growth of our external IV on countries' legal origin (Panel C). This evidence suggests that our predicted epidemiological effect is not correlated with pre-existing national trends, and therefore increases the validity of the instrument.[20]

[18] Table 2.25 in the Appendix provides the values of the F-test on weak instruments comparable to the values suggested by Stock et al. (2005).

[19] The size of the Rotemberg weights proxy for the importance of each specific origin group. As suggested by Goldsmith-Pinkham et al. (2020), we report the top-five origin countries in terms of Rotemberg weights, which accounts for, on average, 40% of the total weights, which is not far from the results provided in the canonical migration setting (Goldsmith-Pinkham et al., 2020).

[20] Adao et al. (2019) point out that another source of bias could be driven by a correlation of the errors due to a similar initial distribution of immigrants by country of origin in destination countries. Countries with a similar initial historical distribution of immigrants by country of origin will suffer similar shocks, which will appear in a correlation in the standard errors. We do not think that this bias could affect our results. First, the two-step GMM estimator implemented in our analysis is robust to any pattern of heteroskedasticity and cross-correlation (Roodman, 2009). Second, we perform a correction in the spirit of Adao et al. (2019) by first dividing the sample of destination countries by different quantiles based on the initial share of immigrants coming from the top-origin countries identified by the Rotemberg Weights in Table 2.27. Then we perform our system GMM analysis and cluster the standard errors over cells corresponding to the quantiles of the initial distribution of each of these shares. Table 2.30 in the Appendix shows that standard errors associated with the epidemiological effect remain fairly similar across the different clustering, minimizing potential concerns arising from unobserved correlations in the standard errors.

2.3.3 Testing for alternative immigration effects

Our benchmark specification explores the impact of immigration on labor regulation. Even though we test for the implications of the size and composition of the immigrant population, immigration is a complex phenomenon that can influence receiving countries in different ways through its effect on human capital, culture, productive knowledge, and other factors (Borjas, 2016). In this section, we try to take into account some alternative effects.

One of the strongest and also undoubted results of the migration literature is that several effects of immigration are skill specific: college-educated immigrants tend to be more beneficial to the destination countries' economy relative to less educated immigrants (Docquier et al., 2014; Borjas, 2019). Investigating the skill-specific effect of immigration on countries' labor regulation would be intriguing, also given the different interactions that less educated and highly educated immigrants have on the labor markets. However, the only source of data that provides skill-specific bilateral migration stocks for a wide set of destination countries is Artuç et al. (2014), which combines several censuses for only two years: 1990 and 2000. Having only two years' worth of data is insufficient for obtaining GMM estimates in our dynamic panel specification.[21] To account for the skill composition of immigrants and the potential self-selection on education, we then follow Alesina et al. (2016) and compute an index of immigrant population skill selection for each country d at year t as follows:

$$Skill\ Selection_{d,t} = \sum_{O} \frac{\frac{HS\ TM_{o,2000}}{TM_{o,2000}}}{\frac{HS\ NAT_{o,2000}}{NAT_{o,2000}}} * \frac{MIG_{o,d,t}}{\sum_{o} MIG_{o,d,t}} = \sum_{o} MigSel_{o,2000} * \overline{mig}_{o,d,t} \tag{2.8}$$

Using Artuç et al. (2014) and Barro and Lee (2013) data, we first compute, for each country of origin o in year 2000, the relative share of highly educated migrants abroad compared to highly educated natives in their origin countries' native population ($MigSel_{o,2000}$). If $MigSel_{o,2000}$ is above one, it means that for the specific country of origin o, the relative share of highly educated individuals abroad is higher than at the origin countries, suggesting positive selection on education. The selection index ($Skill\ Selection_{d,t}$) is then a weighted average of immigrants' relative self-selection on education, using the share of immigrants by origin as weights, and proxies for immigrants' level of self-selection on education.

One of the implications of international migration is its contribution to population diversity. Several studies find positive economic effects of migration diversity measures (mainly immigrants' birthplace diversity) on economic performance at different levels of aggregation (Ottaviano and Peri, 2006; Ortega and Peri, 2014; Alesina et al., 2016; Docquier et al., 2020). More recently, Bahar et al. (2020) show in a cross-country analysis that this positive effect is due to the expansion of the productive knowledge of receiving countries. The increasing variety of competences and knowledge as a result of immigration can also have implications for labor regulation. To account for the potential impact of migration diversity on labor regulation, we compute for each country of destination d an index of immigrant birthplace diversity as follows:

$$Diversity_{d,t} = \sum_{o} \overline{mig}_{o,d,t}(1 - \overline{mig}_{o,d,t}), \tag{2.9}$$

where $\overline{mig}_{o,d,t}$ is the share of immigrants from country of origin o over the total immigrant population in destination country d at year t. This index ranges between 0 and 1, and it measures the probability of randomly drawing two individuals born in different countries from the immigrant population. Including the immigrant birthplace diversity index in the main regression with the share of immigrants allows us to control for the size and the diversity of the immigrant population simultaneously.[22]

[21] We also followed Bahar et al. (2020) and combined Artuç et al. (2014) data with the Database on Immigrants in OECD Countries (DIOC), provided by the OECD, to expand the time dimension of the analysis for a subset of countries. Nevertheless, the total number of country-period observations was too small to obtain any reliable estimates.

[22] Alesina et al. (2016) show that the immigration share and the birthplace diversity index computed for the total population (rather than for the immigrant population) are highly correlated. Including the immigration share and the

An alternative approach to account for the effect of immigration on the variety of the population is to compute a polarization index rather than a diversity index. Intuitively, a polarized population is less cohesive, which could affect social trust, public good provision, and potential conflict (Montalvo and Reynal-Querol, 2005; Ager and Brückner, 2013). Since the most polarized population is characterized by only two groups of equal size, it is intuitive to see polarization as the other side of the coin of diversity. We follow Montalvo and Reynal-Querol (2005) and compute an immigrants' birthplace polarization index as follows:

$$Polarization_{d,t} = 1 - \sum_{o} \left(\frac{0.5 - \overline{m}_{o,d,t}}{0.5} \right)^2 \overline{m}_{o,d,t}. \tag{2.10}$$

For each country of destination d and year t, the polarization index measures how much the immigrant population is close to a bimodal distribution: if it is equal to one, then the immigrant population is composed of only two groups of equal size.

Finally, immigrants can bring with them their experience and habits not only of their labor regulation but also of other relevant aspects. In particular, immigrants coming from developing and poor countries can behave differently from immigrants coming from more developed countries in the labor market, since they experienced a different productive system. We then perform two alternative falsification tests to verify whether the epidemiological effect is driven by the labor regulation experienced by the immigrants and not by other factors. First, we compute an epidemiological effect as a weighted average of the GDP per capita at the origin rather than the WPI (*Epid GDP*). If the epidemiological effect is driven by any aspect related to the level of development of the origin countries, it should be captured by this term. Second, we compute an epidemiological effect after randomly assigning the level of WPI to the origin countries (*Epid Random*). Finally, our epidemiological effect is driven by the level of labor regulation in the origin countries and not by the difference between origin and destination country labor regulation. However, what could matter could be the distance between the legal systems, rather than the level. To test this potential alternative story, we compute an epidemiological effect using not the level of the WPI in the origin country, but rather a normalized Manhattan distance between the origin country and the destination country (*Epid Distance*).[23] Destination countries characterized by immigrants from countries with similar labor law regulation should experience an extremely small *Epid Distance* value.

2.4 Results

The results are organized in two parts. In Section 2.4.1, we present our main results measuring the impact of immigration on WPI. In Section 2.4.2, we examine the robustness of our main results by its various subsamples and to other alternative effects such as diversity, polarization, and skill selection of immigrants. Additionally, we explore the heterogeneity of our main results by countries' economic development and legal system.

2.4.1 Main Results

We estimate the baseline model of equation (2.3) with system GMM using external instruments (shift-share and gravity) for our two variables of interest. Our regression sample covers a panel of 70 countries with five-year periods, from 1970 to 2010. We keep the same number of observations across

birthplace diversity index computed over the immigrant population will account for both overall diversity and diversity within the immigrant population.

[23]The normalized Manhattan distance of WPI between origin country o and destination country d is computed as follows: $WPI^{MAN}_{o,d,2000} = \frac{|WPI_o - WPI_d|}{max_d|WPI_o - WPI_d|}$. It takes a value of 0 when country o and d have the same level of WPI, while it takes the value of 1 when country o and d have the highest distance over the whole sample of countries in analysis.

all specifications to maximize the comparability of results.[24] We start from a parsimonious specification in columns (1) and (2) of Table 2.1, in which we include the lag of the outcome variable and the two immigration variables of interest separately. To avoid simultaneity bias due to "bad controls" (Angrist and Pischke, 2008), we then gradually include the control variables until we reach our main specification in column (6).

Table 2.1. Workers' Protection and Immigration

	(1)	(2)	(3)	(4)	(5)	(6)
Estimation:	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM
Time:	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010
Dep var:	WPI	WPI	WPI	WPI	WPI	WPI
$Prot_{t-1}$	0.895***	0.854***	0.852***	0.860***	0.858***	0.847***
	(0.056)	(0.064)	(0.066)	(0.053)	(0.055)	(0.055)
$Share\ Mig_{t-1}$	-0.002		-0.003	-0.002	-0.005	-0.006*
	(0.002)		(0.002)	(0.003)	(0.003)	(0.003)
$Epid_{t-1}$		0.094**	0.094**	0.085**	0.085***	0.078***
		(0.041)	(0.040)	(0.034)	(0.030)	(0.028)
$ln(GDP)_{t-1}$				-0.009	0.031	0.054
				(0.048)	(0.045)	(0.058)
$Polity2_{t-1}$					-0.101	-0.012
					(0.107)	(0.100)
$ln(HC)_{t-1}$						-0.011
						(0.077)
Year FE	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓
AR1 p-val	0.00	0.00	0.00	0.00	0.00	0.00
AR2 p-val	0.57	0.57	0.57	0.56	0.50	0.55
Hansen p-val	0.37	0.41	0.40	0.55	0.74	0.78
Diff-Hansen p-val	0.27	0.59	0.38	0.90	0.88	0.83
Instruments	15	15	16	20	24	28
Countries	70	70	70	70	70	70
Observations	554	554	554	554	554	554

Note: Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01. The dependent variable is our standardized workers' protection index. As controls, we include the logarithm of GDP per capita, the polity2 index, and the logarithm of human capital. Our main variables of interest are the share of immigrants and the epidemiological effect: the former is instrumented using the predicted immigration share from a gravity model and the latter with the epidemiological effect using predicted bilateral immigration stocks from a shift-share approach. See Appendix 2.8.1 for further information on the variables.

In our main results in Table 2.1, we find a positive and statistically significant effect of the epidemiological term capturing the composition of immigration on the WPI. The coefficient is stable to the inclusion of relevant controls. This result suggests that destination countries' labor regulation is responsive to the level of workers' protection that immigrants experience in their origin country. As for the size of migration, we find a null or negative effect that is significant at a 10% level in our main specification (column 6), but it is not robust across other specifications. Hence, we do not find that the size of immigrant population is a key factor for changing labor regulation, which minimizes the concern about positive reverse causation (i.e., immigrants attracted by protected labor markets). To gain some intuition into the economic magnitude of these effects, we take the face values of the benchmark estimates in column (6). An increase in the migration share by one standard deviation (around 10 percentage points) induces a 6% standard deviation decrease in the WPI over a five-year period. An increase in the epidemiological term by one standard deviation leads to a 7.8% standard deviation increase in the WPI in the destination country—a substantial effect. Our lag dependent variable, as expected, is highly significant across specifications with a coefficient above 0.8, confirming the high

[24] Six missing country-period observations prevent us from achieving a balanced panel regression sample. This omission is due to the polity2 variable: there is one missing observation for Bangladesh and one for Qatar in 1970, and the other four observations are for Germany before its reunification from 1970 to 1985.

persistency of the WPI. We do not find any statistically significant effect associated with the gradually included controls: the level of development, human capital, and the degree of democratization.[25]

To assess the validity of our results, we perform all standard post-estimation test statistics. The first is the absence of a second-order serial correlation in the residuals, which we satisfy as AR(2) p-value is always greater than 0.1. For the Hansen J-test of overidentifying restrictions and the difference-in-Hansen tests, we never reject the null hypothesis, indicating that the moment conditions are satisfied and that the instruments are valid across the specifications. Additionally, we perform various tests for the validity of instruments following Bazzi and Clemens (2013), and in Table 2.25 we test the weakness of instruments in both levels and differences; the Kleibergen-Paap F-stats are significantly greater than 10, and the Kleibergen-Paap Wald-type statistics indicate that we do not suffer from a weak instrument problem.

Finally, Tables 2.31 and 2.32 in the Appendix show that our results are not driven by the time-invariant components of our variables of interest: the WPI at the origin for the epidemiological effect and the population in the destination country for the share of migrants. Estimates associated both to the epidemiological effect computed with a time-variant WPI at the origin (*Epid Tvar*) and to the share of migrants over the actual population (*Share Mig Tvar*) remain qualitatively unchanged.

2.4.2 Robustness and Heterogeneity

In Table 2.2 we perform robustness checks by subsamples, using different measures of the epidemiological term, and by testing the relevance of imputations. Using the benchmark specification presented in column (6) of Table 2.1, we first investigate whether our results are driven by a specific set of countries or years. In column (1), we remove the last two periods (corresponding to 10 years) of our sample to avoid potential spurious estimates due to the 2008 financial crisis and the inclusion of 2010 migration data from a different source compared to Özden et al. (2011). In columns (2) and (3), we drop countries belonging to the upper quantile of the migration share and workers' protection index accordingly.[26] This is to address the concern about potential outliers by removing the right tail of the variable distribution. In the second part, we test whether our results are being driven by imputations and different measures of the epidemiological term. In column (4), we run our analysis in 10-year periods to verify whether our results are being driven by the five-year interpolations made in the bilateral stocks of immigrants. In column (5), we drop from the sample countries characterized by a high percentage of imputed WPI within their epidemiological term (i.e., more than 30% of the immigrant population coming from a country with an imputed WPI). In the next three columns, we test the robustness of the epidemiological term with different imputation methods: (6) imputing the missing countries with the minimum value by legal origin (*Epid min*), (7) imputing the missing countries with the maximum value by legal origin (*Epid max*), and (8) not imputing the values of missing countries (*Epid strict*). In the last column, we test a specification that simultaneously does not use imputed values for the epidemiological and migration terms, by using *Epid strict* and 10-year periods.

The estimated coefficient for the epidemiological term is always positive and highly significant across all subsamples and imputations. These estimates suggest that the positive effect of the epidemiological effect on WPI is not driven by particular outliers or imputation methods. Moreover, the magnitude of the coefficient is very stable, except for columns (4) and (9).[27] Apart from the Hansen test in column (9), all of our estimates satisfy the standard Hansen, difference-in-Hansen, and AR2 post-estimation tests. As for the estimated coefficient for the share of immigrants, it is not significant when looking at subsamples in columns (1) to (3) and in column (9). However, it is significant and negative when looking

[25] One explanation for finding no effects in the additional controls is that we only use internal instruments to mitigate endogeneity problems. In an ideal case, we would instrument each of the additional controls with valid external instruments.

[26] In our sample, we removed 14 countries based on the values in the year 2000.

[27] Re-estimating our results over a longer time span (10-year periods rather than 5-year periods) may increase the variation and consequently the estimated coefficients; additionally, we reduce the sample size by half, which reduces the precision of the estimates.

Table 2.2. Workers' Protection and Immigration - Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Estimation:	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM
Time:	1970-00	1970-10	1970-10	1970-10	1970-10	1970-10	1970-10	1970-10	1970-10
Dep var:	WPI	WPI	WPI	WPI	WPI	WPI	WPI	WPI	WPI
Robustness to:	2005-10	Top20%Mig	Top20%WPI	10-year	Epid imp	Epid min	Epid max	Epid stric	Epid stric &10-year
WPI_{t-1}	0.834***	0.777***	0.787***	0.792***	0.853***	0.849***	0.857***	0.846***	0.742***
	(0.047)	(0.074)	(0.074)	(0.113)	(0.059)	(0.054)	(0.055)	(0.055)	(0.154)
$Share\ Mig_{t-1}$	-0.001	0.007	-0.004	-0.024*	-0.008**	-0.006*	-0.006*	-0.006*	-0.026
	(0.004)	(0.012)	(0.004)	(0.013)	(0.004)	(0.003)	(0.003)	(0.004)	(0.017)
$Epid_{t-1}$	0.090***	0.074**	0.067**	0.137**	0.070**				
	(0.031)	(0.032)	(0.031)	(0.061)	(0.028)				
$Epid\ min_{t-1}$						0.073***			
						(0.026)			
$Epid\ max_{t-1}$							0.057**		
							(0.027)		
$Epid\ stric_{t-1}$								0.079***	0.304**
								(0.028)	(0.123)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
AR1 p-val	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
AR2 p-val	0.58	0.37	0.55	0.57	0.51	0.56	0.53	0.55	0.55
Hansen p-val	0.74	0.81	0.63	0.10	0.84	0.80	0.76	0.79	0.09
DiffHansen p-val	0.54	0.90	0.73	0.48	0.81	0.80	0.84	0.83	0.47
Instruments	24	28	28	17	28	28	28	28	17
Countries	70	56	56	70	56	70	70	70	70
Observations	414	447	446	278	442	554	554	554	278

Note: Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01. The dependent variable is our standardized workers' protection index. As controls, we include the logarithm of GDP per capita, the polity2 index, and the logarithm of human capital. Our main variables of interest are the share of immigrants and the epidemiological effect: the former is instrumented using the predicted immigration share from a gravity model and the latter with the epidemiological effect using predicted bilateral immigration stocks from a shift-share approach. See Appendix 2.8.1 for further information on the variables. Columns (1) to (5) report the estimates after dropping from the sample: the years 2005–2010 (col. 1), countries belonging to the top quintile in terms of share of immigrants (col. 2) and in terms of workers' protection index (col. 3), the five-year interpolated observations (col. 4), and countries with more than 30% of immigrants with imputed WPI at the origin. Columns (6) to (8) include as main variable of interest instead of the standard epidemiological effect: an epidemiological effect where all the imputed WPI measures at the origin are equal to the lowest value available (*Epid min*), an epidemiological effect where all the imputed WPI at the origin are equal to the highest value available (*Epid max*), and an epidemiological effect where missing values are not imputed (*Epid strict*). Column (9) reports results with no imputation in the epidemiological term and with 10-year periods.

at robustness to imputations in columns (4) to (8). Therefore, the conclusion for the size of migration is in line with Table 2.1: the size of migration has a null or small and negative impact on the WPI.

After verifying that our results are not driven by sample selection and data imputations, we investigate whether alternative effects due to immigration could replace or complement our estimates. As Section 2.3.3 presents, the literature identifies that diversity and polarization of the immigrant population can have a direct effect on countries' productive knowledge and economic development (Ager and Brückner, 2013; Bahar et al., 2020; Docquier et al., 2020). We include an index of diversity and an index of polarization among immigrants in columns (1) and (2) of Table 2.3, respectively. The estimates show that neither immigration diversity nor polarization outruns the epidemiological effect as a relevant channel for explaining WPI variation. Moreover, the estimates for both indexes are not statistically different from zero. To test whether our effects are not entirely driven by the absence of the skill composition and self-selection of immigrants, we follow Alesina et al. (2016) and include as additional control an index of immigrants' self-selection on education. Column (3) of Table 2.3 shows estimates that are similar to

the baseline results associated with the epidemiological term and the migration share after including the self-selection index computed in equation (2.8); the coefficient associated with the index of self-selection is negative and smaller compared to the epidemiological term and only significant at the 10% level. To explore whether the epidemiological effect is influenced by the relative size of the immigrant population compared to the native population in the destination country, we replace in column (4) the standard epidemiological effect with one using as origin-specific weights the share of immigrants by origin over the destination country population (*Epid PopDe*). The estimates are consistent with our previous results, suggesting that the modified epidemiological term is not affected by the relative size of the immigrant population.

To test whether the epidemiological effect is driven not only by the level of WPI experienced in the origin countries, but also by other factors, in column (5) of Table 2.3 we first replace the standard epidemiological effect with the one using WPI origin-destination distances rather than levels (*Epid Distance*). We do not find a significant effect associated with the latter index, suggesting that the difference between the immigrants' and natives' WPI is far less important for the change in WPI at destination compared to the degree (level) of WPI that immigrants experienced in the origin country. Additionally, since immigrants bring with them the origin-country experience of not only labor market regulation but also a broader set of competences and skills, we perform two falsification tests by building two different epidemiological effects based on immigrants' GDP at origin instead of WPI (*Epid GDP*) as well as completely randomizing the level of WPI in the origin countries (*Epid Random*). After replacing our main epidemiological effect with the two alternative constructions in columns (6) and (7) of Table 2.3, we confirm that the epidemiological effect is driven by the immigrants' origin-country experience of labor regulation. Finally, we test out whether the epidemiological effect is not driven by other relevant factor highlighted by the literature: Alesina et al. (2015) identifies the importance of family ties in driving demand for labor market regulation, while Algan et al. (2016) highlights individual generalized trust and civicness as in determining the size of the welfare state. We followed the above-mentioned works by computing the shares of individual living with parents and the shares of individuals who trust others. Then we build two epidemiological effects using family ties (*Epid Family*) and trust (*Epid Trust*) as weights. Columns (8) and (9) show that the newly constructed epidemiological effects are not statistically significant, suggesting that family ties and trust are less relevant in explaining the impact of immigration on labor regulation, at the very least, during the analyzed period.

Beyond the controls included in the benchmark specification, we test the robustness of our results by including additional controls in the appendix in Table 2.33. First, we include a measure of de facto law proxied by the rule of law index, which can serve as a complementary variable to our de jure measure of the WPI (Coppedge et al., 2020). Next, we add a civil liberties index, which is a combination of de facto and de jure questions that are important for maintaining the rights of citizens (House, 2016). Third, we include a measure of the size of the informal market or the shadow economy, which could affect the size and the composition of immigrants as well as the degree of workers' protection (Elgin et al., 2012). Afterward, we include the economic freedom index and trade (exports plus imports) as a share of GDP (Gwartney et al., 2018; World Bank, 2018b). Finally, we control for countries' membership in the European Union, the International Labor Organization (ILO), and the World Trade Organization (WTO). Overall, after including these additional controls separately, the coefficients of interest remain in line with our benchmark results.

In Table 2.4, we test for the heterogeneous effects of immigration on WPI across destination countries by looking at the level of development (measured by being a high-income OECD country) and at the legal origin. After including destination-specific dummies and interaction terms with our variables of interest, we do not find a differential impact depending on economic development or legal origin. Columns (2) and (4) reveal that what matters is immigrants' workers' protection at the origin country, regardless of the destination country's legal system and level of economic development. Finally, looking at the main terms, we confirm the stylized facts in Figure 2.2: high-income OECD countries and civil law countries are characterized by a higher level of WPI (although such difference in levels is not significant for the legal origin). Legal origin seems to better explain the cross-sectional variation rather than the within-country

Table 2.3. Workers' Protection and Immigration - Alternative Immigration Effects and Falsification tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Estimation:	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM
Time:	1970-10	1970-10	1970-10	1970-10	1970-10	1970-10	1970-10	1970-10	1970-10
Dep var:	WPI	WPI	WPI	WPI	WPI	WPI	WPI	WPI	WPI
WPI_{t-1}	0.849***	0.853***	0.847***	0.865***	0.869***	0.858***	0.868***	0.869***	0.871***
	(0.054)	(0.053)	(0.055)	(0.054)	(0.052)	(0.053)	(0.053)	(0.052)	(0.054)
$Share\ Mig_{t-1}$	-0.006*	-0.006*	-0.006*	-0.007	-0.006*	-0.005	-0.006**	-0.006*	-0.006**
	(0.003)	(0.003)	(0.003)	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
$Epid_{t-1}$	0.079***	0.079***	0.072***						
	(0.028)	(0.029)	(0.027)						
$Diversity_{t-1}$	0.018								
	(0.030)								
$Polar\ Mig_{t-1}$		0.003							
		(0.030)							
$Selection_{t-1}$			-0.022*						
			(0.013)						
$Epid\ PopDe_{t-1}$				0.087**					
				(0.035)					
$Epid\ Distance_{t-1}$					0.043				
					(0.590)				
$Epid\ GDP_{t-1}$						0.068			
						(0.049)			
$Epid\ Random_{t-1}$							-0.018		
							(0.028)		
$Epid\ Family_{t-1}$								-0.005	
								(0.027)	
$Epid\ Trust_{t-1}$									0.003
									(0.024)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
AR1 p-val	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
AR2 p-val	0.55	0.56	0.55	0.56	0.54	0.54	0.54	0.54	0.53
Hansen p-val	0.81	0.76	0.78	0.76	0.76	0.81	0.77	0.77	0.78
Diff-Hansen p-val	0.83	0.85	0.86	0.63	0.78	0.67	0.79	0.79	0.75
Instruments	29	29	29	28	28	28	28	28	28
Countries	70	70	70	70	70	70	70	70	70
Observations	554	554	554	554	554	554	554	554	554

Note: Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01. The dependent variable is our standardized workers' protection index. As controls, we include the logarithm of GDP per capita, the polity2 index, and the logarithm of human capital. Our main variables of interest are the share of immigrants and the epidemiological effect: the former is instrumented using the predicted immigration share from a gravity model and the latter with the epidemiological effect using predicted bilateral immigration stocks from a shift-share approach. See Appendix 2.8.1 for further information on the variables. Columns (1) to (3) include as additional controls: birthplace diversity index among immigrants (*Diversity*), polarization index among immigrants (*Polarization*), and human capital selection index of immigrant population (*Skill Selection*). Columns (4) to (9) include as main variables of interest instead of the standard epidemiological effect: an epidemiological effect using as origin-specific weights the share of immigrants over the total population of the destination country (*Epid PopDe*), an epidemiological effect based on the origin-destination WPI distances (*Epid Distance*), an epidemiological effect using the level of GDP per capita at the origin as weight (*Epid GDP*), an epidemiological effect where the WPI at the origin is randomly distributed (*Epid Random*), an epidemiological effect using the share of individuals living with the family at the origin as weight (*Epid Family*), and an epidemiological effect using the share of individuals in the origin who trust others as weight (*Epid Trust*). All the additional included variables are instrumented using predicted bilateral immigration stocks from a shift-share approach.

evolution over time.[28]

[28]Table 2.34 in the Appendix shows the cross-sectional determinants in our sample of countries (average between 1970 and 2010). The legal origin seems to explain a large portion of WPI disparities across countries.

Table 2.4. Workers' Protection and Immigration - Heterogeneity Analysis

	(1)	(2)	(3)	(4)
Estimation:	S-GMM	S-GMM	S-GMM	S-GMM
Time:	1970-2010	1970-2010	1970-2010	1970-2010
Dep var:	WPI	WPI	WPI	WPI
WPI_{t-1}	0.838***	0.831***	0.822***	0.819***
	(0.054)	(0.055)	(0.064)	(0.062)
$Share\ Mig_{t-1}$	-0.005	-0.005	-0.005	-0.006*
	(0.004)	(0.004)	(0.004)	(0.003)
$Epid_{t-1}$	0.078***	0.067**	0.055**	0.068**
	(0.029)	(0.029)	(0.026)	(0.028)
$Share\ Mig_{t-1} * OECD^{HIC}$	-0.004			
	(0.005)			
$Epid_{t-1} * OECD^{HIC}$		0.042		
		(0.051)		
$Share\ Mig_{t-1} * Common\ Law$			-0.004	
			(0.005)	
$Epid_{t-1} * Common\ Law$				-0.068
				(0.091)
$OECD^{HIC}$	0.177*	0.160**		
	(0.096)	(0.080)		
$Common\ Law$			-0.096	-0.164
			(0.077)	(0.100)
Controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Country FE	✓	✓	✓	✓
AR1 p-val	0.00	0.00	0.00	0.00
AR2 p-val	0.54	0.54	0.55	0.55
Hansen p-val	0.83	0.85	0.77	0.76
Diff-Hansen p-val	0.85	0.87	0.84	0.84
Instruments	30	30	30	30
Countries	70	70	70	70
Observations	554	554	554	554

Note: Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01. The dependent variable is our standardized workers' protection index. As controls, we include the logarithm of GDP per capita, the polity2 index, and the logarithm of human capital. Our main variables of interest are the share of immigrants and the epidemiological effect: the former is instrumented using the predicted immigration share from a gravity model and the latter with the epidemiological effect using predicted bilateral immigration stocks from a shift-share approach. Additionally, we include dummies capturing the level of development and legal origin of destination countries. See Appendix 2.8.1 for further information on the variables.

2.5 Mechanisms

In this section, we explore potential mechanisms via which the immigrants' law transfer can impact labor regulation. In Section 2.5.1, we decompose our dependent variable into its five main subcomponents to better identify which dimension of WPI is the most affected by immigration. In Section 2.5.2, we describe a stylized theory to rationalize the changes in labor regulation due to immigration by exploring the implications for unions' bargaining power and political actors. Section 2.5.3 provides empirical suggestive evidence of the stylized theory.

2.5.1 WPI Subcomponents

Table 2.5 presents the estimates of the response of the five subdimensions of the WPI to immigration: (i) industrial action laws ($IndAction$), (ii) worker representation laws ($WkrRepr$), (iii) worker dismissal laws ($WkrDismis$), (iv) working time laws ($WorkTime$), and (v) employment forms laws ($EmptForm$). Each specification includes the same set of controls and variables of interest as our benchmark specification. Even though the variation of each subcomponent over time is smaller than the overall WPI, we find that the level of workers' protection that immigrants experienced in their origin countries (and proxied by the

epidemiological effect) has a positive and statistically significant effect on two subcomponents: worker representation laws and employment forms laws. Both coefficients are statistically significant at the 5% level, and the magnitude of the effect is similar to the aggregate WPI. The estimates associated with the size of immigration are small and close to zero. These areas of labor regulation have important implications for the labor market. Worker representation laws, such as unionization and collective bargaining rights, are extremely relevant because of their direct impact on wage dispersion: a broad set of evidence shows that unions reduce wage dispersion, in particular for male workers (Lemieux, 1998; Card et al., 2004, 2020). Moreover, the degree of rigidity of the employment forms not only has a general effect on wages and employment, but also influences how natives react to a supply shock in the workforce as a result of immigration flows (D'Amuri and Peri, 2014; Edo, 2016).

Table 2.5. WPI Subcomponents and Immigration

	(1)	(2)	(3)	(4)	(5)
Estimation:	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM
Time:	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010
Dep var: WPI Subcomponent	$IndAction$	$WkrRepr$	$WkrDismis$	$WorkTime$	$EmptForm$
Dep_{t-1}	0.885***	0.894***	0.817***	0.903***	0.922***
	(0.082)	(0.044)	(0.070)	(0.059)	(0.061)
$Share\ Mig_{t-1}$	-0.010	-0.002	0.004	-0.000	-0.005
	(0.007)	(0.004)	(0.005)	(0.003)	(0.004)
$Epid_{t-1}$	-0.030	0.070**	0.044	0.031	0.061**
	(0.034)	(0.030)	(0.038)	(0.028)	(0.031)
Controls	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓
AR1 p-val	0.00	0.00	0.00	0.00	0.00
AR2 p-val	0.69	0.79	0.39	0.11	0.86
Hansen p-val	0.89	0.50	0.54	0.57	0.34
Diff-Hansen p-val	0.83	0.39	0.37	0.53	0.36
Instruments	28	28	28	28	28
Countries	70	70	70	70	70
Observations	554	554	554	554	554

Note: Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01. Each column has as a dependent variable a different subcomponent of the workers' protection index: industrial action and striking ($IndAction$), employment representation and unionization ($WkrRepr$), workers' dismissal regulation ($WkrDismis$), working time regulation ($WorkTime$), and employment relationship and employment forms ($EmptForm$). As controls, we include the logarithm of GDP per capita, the polity2 index, and the logarithm of human capital. Our main variables of interest are the share of immigrants and the epidemiological effect: the former is instrumented using the predicted immigration share from a gravity model and the latter with the epidemiological effect using predicted bilateral immigration stocks from a shift-share approach. See Appendix 2.8.1 for further information on the variables.

To capture whether the estimates are driven by immigrants' experience of the specific area of labor law regulation, in Table 2.6 we recalculate the epidemiological term by replacing the immigrants' aggregate level of workers' protection at origin with one of the five WPI subcomponents at a time. Then we estimate the subcomponent-specific epidemiological effect on the same destination country subcomponent of workers' protection. For instance, in column (2), we see that the epidemiological term constructed with immigrants' worker representation laws at origin ($Epid^{WkrRepr}$) affects the worker representation laws in the destination country. The magnitude of the effect is reduced from 7% to 5.9% standard deviations compared to the aggregate epidemiological effect presented in Table 2.5, and the significance level remains unchanged. We also find a similar magnitude of the effect for the subcomponent-specific epidemiological effect ($Epid^{EmptForm}$) in column (5), which is now significant only at the 10% level. We do not find any significant estimates associated with either the epidemiological effect or the share of immigrants on the other three dimensions (industrial action laws, worker dismissal laws, and working time laws). It is worth noting, however, that the tested subcomponents exhibit lower variation over time

Table 2.6. WPI Subcomponents and Immigration - Subcomponent-Specific Epidemiological Term

	(1)	(2)	(3)	(4)	(5)
Estimation:	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM
Time:	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010
Dep var: WPI Subcomponent	$IndAction$	$WkrRepr$	$WkrDismis$	$WorkTime$	$EmptForm$
Dep_{t-1}	0.877***	0.900***	0.820***	0.884***	0.924***
	(0.086)	(0.045)	(0.067)	(0.069)	(0.055)
$Share\ Mig_{t-1}$	-0.011	-0.002	0.004	-0.000	-0.004
	(0.007)	(0.003)	(0.004)	(0.003)	(0.004)
$Epid_{t-1}^{IndAction}$	-0.017				
	(0.043)				
$Epid_{t-1}^{WkrRepr}$		0.059**			
		(0.029)			
$Epid_{t-1}^{WkrDismis}$			0.035		
			(0.035)		
$Epid_{t-1}^{WorkTime}$				0.057	
				(0.037)	
$Epid_{t-1}^{EmptForm}$					0.052*
					(0.027)
Controls	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓
AR1 p-val	0.00	0.00	0.00	0.00	0.00
AR2 p-val	0.70	0.81	0.38	0.10	0.86
Hansen p-val	0.90	0.55	0.54	0.55	0.35
Diff-Hansen p-val	0.84	0.37	0.35	0.50	0.38
Instruments	28	28	28	28	28
Countries	70	70	70	70	70
Observations	554	554	554	554	554

Note: Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01. Each column has as a dependent variable a different subcomponent of the workers' protection index: industrial action and striking ($IndAction$), employment representation and unionization ($WkrRepr$), workers' dismissal regulation ($WkrDismis$), working time regulation ($WorkTime$), and employment relationship and employment forms ($EmptForm$). As controls, we include the logarithm of GDP per capita, the polity2 index, and the logarithm of human capital. Our main variables of interest are the share of immigrants and the subcomponent-specific epidemiological effect: the former is instrumented using the predicted immigration share from a gravity model and the latter with the epidemiological effect using predicted bilateral immigration stocks from a shift-share approach. See Appendix 2.8.1 for further information on the variables.

compared to the full index, and we are capturing a lot of variation in our demanding specification with the persistency term included. Overall, our results show that some areas of the labor law regulation are more likely to react to immigration, in particular, laws concerning labor unions' rights.

2.5.2 A Stylized Theory of Unions' Bargaining Power

The previous set of results shows that there is heterogeneity in the impact of immigration on different areas of labor regulation. The laws concerning workers' rights to unionize and unions' power to bargain for wages appear to be particularly affected by the composition of the immigrant population. In this section, we want to provide some theoretical arguments behind these results by first distinguishing the *direct effect* from the *indirect effect* of immigration on labor law regulation.

A *direct effect* on the legislators can occur as a result of immigrants' participation in the political system through voting. If immigrants have the right to vote, they could, for instance, vote for parties that support a degree of workers' protection that is similar to what they experienced in their origin countries. Then, a higher epidemiological term captures a higher support for parties with a focus on more protective labor regulation. In a sample of 32 European countries, Luttmer and Singhal (2011) show that immigrants who share stronger origin-specific preferences toward redistribution are more prone to support and vote for redistributive parties. However, since only a small share of immigrants obtain

voting rights after a long period of residence in a subset of destination countries in our sample, this potential direct effect is expected to be small. Alternatively, another direct effect could appear as a result of interaction between immigrants and local households. Giuliano and Tabellini (2020) suggest that US natives' exposure to more welfare-state-oriented European immigrants have influenced natives' political preferences toward redistribution. Although the interaction between households holding different preferences and views might play an important role, we are unable to test for direct interactions between natives and migrants in our framework.

Alternatively, labor law regulation's reaction can be rationalized by an *indirect effect* of immigration on workers' protection. By participating in the labor market, immigrants can influence both the supply of labor and the bargaining power of different actors in the labor market, particularly through immigrants' participation in trade unions. If immigrant workers' propensity to unionize differs from that of natives, unions' bargaining power may be affected: unions that represent a bigger (smaller) share of the labor force have stronger (weaker) bargaining power (Mishel, 1986). Moreover, if political actors (i.e., parties) react to changes in union density and unions' bargaining power, then immigration could contribute to changes in labor law regulation.

To rationalize our arguments and the hypotheses that we want to test concerning the indirect effect, we can first describe a stylized theory of unions' bargaining power (BP^u). The degree of unions' bargaining power influences the economic gains for union members (i.e., the union wage premium) and for the society as a whole, by reducing wage dispersion (Card et al., 2020). For our purposes, we can describe it as a function of two factors: (i) union density (u^d) (i.e., the share of unionized employees over the employed population) and (ii) unions' supportive labor law regulation (l^u):

$$BP^u = F(u^d, l^u). \tag{2.11}$$

Higher union density implies that unions are able to represent a wider share of the employed population; hence, they would hold stronger bargaining power once they face employers and firms ($\frac{\delta BP^u}{\delta u^d} > 0$). Similarly, a more protective labor law regulation toward unions would positively influence unions' bargaining power ($\frac{\delta BP^u}{\delta l^u} > 0$). The literature shows that, although negatively affected by the rise of more competitive markets and unions' difficulties in monopolizing the supply of labor, unions' bargaining power is still strong enough (that is, sufficient above a certain threshold $BP^u > \widetilde{BP^u}$) to generate a union wage premium and reduce discrimination in labor markets (Bryson, 2014).

How can immigration influence unions' bargaining power? We can first formalize that the workforce is composed of natives (n) and immigrants (m), and we can define workers' propensity to unionize as p^i. If immigrants hold a different propensity to unionize compared to natives (i.e., $p^n \neq p^m$) and such propensity is related to immigrants' origin-country labor regulation experience, then changes in the composition of the immigrant population could affect unions' bargaining power.[29].

Our hypothesis is that origin-country labor regulation experience may influence immigrants' propensity to unionize. Defining as l_o the labor law regulation experienced in country of origin o, we can describe two potential implications for immigrants' propensity to unionize. Immigrants used to weak labor protection may be prone to seek protection in destination countries, thereby enhancing the propensity to unionize ($\frac{\delta p_o^m}{\delta l_o} < 0$), and conversely, immigrants used to strong labor protection may not be prone to seek protection in destination countries, thus diminishing the propensity to unionize. We define this behavior as a *responsive position* toward the origin-specific labor regulation experience. On the other hand, immigrants may stick to their origin-country experience: seeking protection if they experienced strong and effective labor regulation, and renouncing union protection if they experienced a weakly regulated labor market. We define this second behavior of immigrants as a *passive position*

[29]Combining the World Values Survey (WVS) longitudinal data from 1993 and the European Social Survey (ESS) data from 2000, Figure 2.9 in the Appendix shows that immigrants are less unionized than natives: on average, only 8% (WVS) or 12.2% (ESS) of the immigrant workforce is unionized, compared to 9.8% (WVS) or 16.3% (ESS) of the native workforce. This discrepancy in union density is in line with the literature on European and Nordic European countries (Kranendonk and De Beer, 2016; Hagen and Jensen, 2019)

toward the origin country's labor regulation. Testing how origin-specific experience affects immigrants' union density would be the first step of our indirect effect analysis.

After describing how immigration can influence unions' bargaining power through union density, we can rationalize how legislators could react to a change in unions' bargaining power. Relying on Botero et al. (2004), we can identify two theories that could explain political actors' (i.e., parties) position toward unions and labor law regulation after variations in union density. The *political power theory* claims that changes in the law are driven by the political gains from potential voters: if unions become stronger and bigger, parties will promote legal changes to increase their bargaining power (i.e., more protective labor law) to obtain their support during elections. Alternatively, the *efficiency theory* argues that changes in the regulations are driven by efficiency considerations (i.e., to maximize social welfare). By defining an optimal level of union bargaining power ($\widehat{BP^u} > \widetilde{BP^u}$), political actors will counterbalance any variation above or below such optimal level with changes in labor law regulation in the opposite direction.

The next section provides suggestive evidence of the above-described stylized theory, testing (i) the relation between immigrants' origin-specific experience and union density, and (ii) the relation between union density and parties' political preferences.

2.5.3 Testing the theory

The first step in our analysis is to test the relation between the degree of workers' protection that immigrants experience in their origin countries (captured by the epidemiological term) and their degree of unionization. To retrieve information on immigrants' unionization preferences, we rely on the European Social Survey (ESS), which provides information on both migration status (native or immigrant) and whether respondents belong to a trade union.[30] Our focus is on 14 European countries that are part of our initial sample and for which we have multiple observations over the period 2000–2010, in order to partially overlap our period of analysis starting from the first wave available from the ESS (2000). We drop the waves in which immigrants are just a small minority of the respondents (i.e., less than 80 observations). After computing the share of unionized workers at the country level, we estimate the partial correlation between the share of unionized workers and immigration. Our analysis includes both country and year fixed effects in order to capture time-invariant unobserved factors and common trends.

Table 2.7 shows the estimates using as a dependent variable the overall share of unionized workers (col. (1) and (4)), its decomposition between unionized native workers (col. (2) and (5)), and unionized immigrant workers (col. (3) and (6)) over the total employed population. Immigration (in terms of both size and composition) is not correlated with either the overall or native share of unionized workers. In column (3), the size of immigration is positively related to the share of unionized immigrants: by construction, having more immigrants increases the probability of having more unionized immigrants. Concerning the composition of the immigrant population, column (3) presents a negative relation between the epidemiological term and the share of unionized workers. Being aware that these estimates are suggestive partial correlations,[31] these results suggest a *responsive position* of immigrants after experiencing origin-country labor regulation: immigrants that are used to less protective labor regulation tend to seek protection from unions, and so they are more unionized. This intuition is somewhat confirmed in column (6), when we estimate the partial correlations between the share of unionized immigrants and the epidemiological term using the workers' employment representation subcomponent. This finding is in line with Table 2.6, where a higher share of immigrants coming from countries with strong unionization rights is negatively related to the share of unionized immigrant workers.

Table 2.7 estimates suggest that an immigrant population that is used to strong labor regulation in their origin countries tend to be less unionized. Consequently, the share of workers represented by unions is affected by immigrants' origin experience, which eventually influences their bargaining power. These

[30] An alternative dataset that can be used to retrieve information on immigrants' participation in unions is the World Values Survey; however, it is available for only a few countries that belong to our sample of countries over multiple periods.

[31] For such a small set of countries and the short time span, our IV approaches produce weak instruments, generating unreliable estimates. Results are available upon request.

Table 2.7. Participation in Unions

	(1)	(2)	(3)	(4)	(5)	(6)
Estimation:	OLS	OLS	OLS	OLS	OLS	OLS
Time:	2000-2010	2000-2010	2000-2010	2000-2010	2000-2010	2000-2010
Dep var: $\frac{Unionized}{Workers}$	All	Natives	Migrants	All	Natives	Migrants
$Share\ Mig_t$	0.063	-0.125	0.189**	-0.135	-0.199	0.065
	(0.343)	(0.306)	(0.086)	(0.190)	(0.177)	(0.052)
$Epid_t$	-0.150	-0.039	-0.111***			
	(0.171)	(0.146)	(0.034)			
$Epid_t^{WkrRepr}$				-0.047	0.068	-0.115**
				(0.191)	(0.158)	(0.041)
Observations	38	38	38	38	38	38
Countries	14	14	14	14	14	14
Adj. R-Square	0.98	0.98	0.92	0.98	0.98	0.90
Country FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Note: Authors' calculations on ESS data. * p<0.1, ** p<0.05, *** p<0.01. Standard errors are clustered at the country level. Our main variables of interest are the share of immigrants and the epidemiological effect (computed with the overall WPI at the origin and the $WkrRepr$ specific subcomponent). The dependent variable is the overall share of unionized workers (col. (1) and (4)), the share of native unionized workers (col. (2) and (5)), and the share of immmigrant unionized workers (col. (3) and (6)) over the overall worker population. The sample of countries includes Austria, Belgium, Denmark, France, Germany, Greece, Ireland, Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, and United Kingdom. Waves of the survey are dropped when the size of the immigrant population is not well represented (i.e., less than 80 observations).

effects can affect labor regulation (i.e., law-in-the-books) if parties react to such variation in union size by either proposing laws in line with the potential electoral gains from the pool of unionized workers or proposing laws to balance out the bargaining power of unions. The first reaction is in line with the *political power theory*, which predicts a positive relation between union density and parties' support of protective labor law, while the second reaction can be explained by an *efficiency theory*, which predicts a negative relation between union density and parties' support of protective labor law (Botero et al., 2004).

To test this potential relationship between union size and parties' political preferences, we use the Manifesto Project Database (MPD) (Volkens et al., 2019), which explores parties' political platforms through a content analysis of their political manifesto. For each party that won at least one seat in a national election, the MPD data quantify the party's political preferences by counting the number of quasi-sentences related to a specific political issue over the total length of the political manifesto.[32] For our purpose, we focus on parties' political preferences on labor groups: this measure captures parties' preferences toward the working class, labor unions, unemployed workers, and labor groups in general. Since MPD provides for each party the measure for the share of both positive and negative sentences toward labor groups, we built a measure of parties' net political preferences by subtracting the share of negative mentions from the share of positive mentions. We then explore over the same sample of countries and period in Table 2.7 the partial correlation between the share of unionized workers (overall, natives and immigrants) and parties' net political preferences toward labor groups. In our linear regression, we include year and party fixed effects in order to capture common trends and the time-invariant ideological position of the parties, and we weight each party for the share of votes gained in each electoral event.

Table 2.8 shows that the share of the unionized workforce is negatively related to parties' net favorable position toward labor groups. Although not statistically significant, the point estimates associated with the unionized migration workforce are the biggest. The estimates are robust in terms of magnitude and

[32] Although being a proxy of parties' political platforms, measuring parties' political preferences through a content analysis of the political manifesto has been shown to be a good proxy for parties' ideological spectrum. Focusing on issues related to nationalism, Moriconi et al. (2021) show that proxies with MPD data are closely related to other available data sources, such as the Chapel Hill Expert Survey.

Table 2.8. Unions and Party Position Toward Labor Groups

	(1)	(2)	(3)	(4)	(5)	(6)
Estimation:	OLS	OLS	OLS	OLS	OLS	OLS
Time:	2000-2010	2000-2010	2000-2010	2000-2010	2000-2010	2000-2010
Dep var: Political Pref.	Lab. Groups	Lab. Groups	Lab. Groups	Lab. Groups	Lab. Groups	Lab. Groups
$Unions_{tot}$	-0.354*			-0.370**		
	(0.180)			(0.153)		
$Unions_{nat}$		-0.404**			-0.413**	
		(0.173)			(0.159)	
$Unions_{mig}$			-0.749			-1.106
			(0.781)			(0.797)
$Share\ Mig_t$				0.030	-0.034	0.222
				(0.224)	(0.241)	(0.230)
$Epid_t$				-4.791	-1.314	-11.458
				(9.358)	(10.262)	(10.877)
Observations	505	505	505	505	505	505
Countries	14	14	14	14	14	14
Adj. R-Square	0.37	0.37	0.34	0.37	0.36	0.34
Party FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Note: Authors' calculations on ESS and Manifesto Project Database (MPD) data. * p<0.1, ** p<0.05, *** p<0.01. Standard errors are clustered at the country level. The dependent variable is the net positive position of parties toward labor groups. Each party is weighted with the share of votes obtained in the election. The main variable of interest is the share of unionized workers. The sample of countries includes Austria, Belgium, Denmark, France, Germany, Greece, Ireland, Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, and United Kingdom. Waves of the survey are dropped when the size of the immigrant population is not well represented (i.e., less than 80 observations).

significance after controlling for the size and composition of the immigrant population. Recalling that these estimates should be interpreted cautiously because of the endogenous nature of the variables, we note that the direction of the relation is against the explanation of the *political power theory*: a reduction in the unionized workforce (i.e., a reduction in union bargaining power) fosters parties' favorable net position toward labor groups and unions. That would imply, from a legal point of view, a stronger and favorable regulation toward unions, which would balance out the reduction in union bargaining power, in line with the *efficiency theory*.

2.6 Discussion

In this section, we provide some back-of-the-envelope calculations based on our estimates to have a sense of the magnitude of the immigration effect on WPI. Even though these simulations have a descriptive purpose, since they cannot consider all general equilibrium effects, they can provide an intuitive picture of the magnitude of the effect under various assumptions. Moreover, these simulations do not aim to explain the whole evolution of WPI presented in Figure 2.2, which is determined by many factors, but rather to attempt to highlight the contribution in a partial equilibrium of immigration on the evolution of WPI.

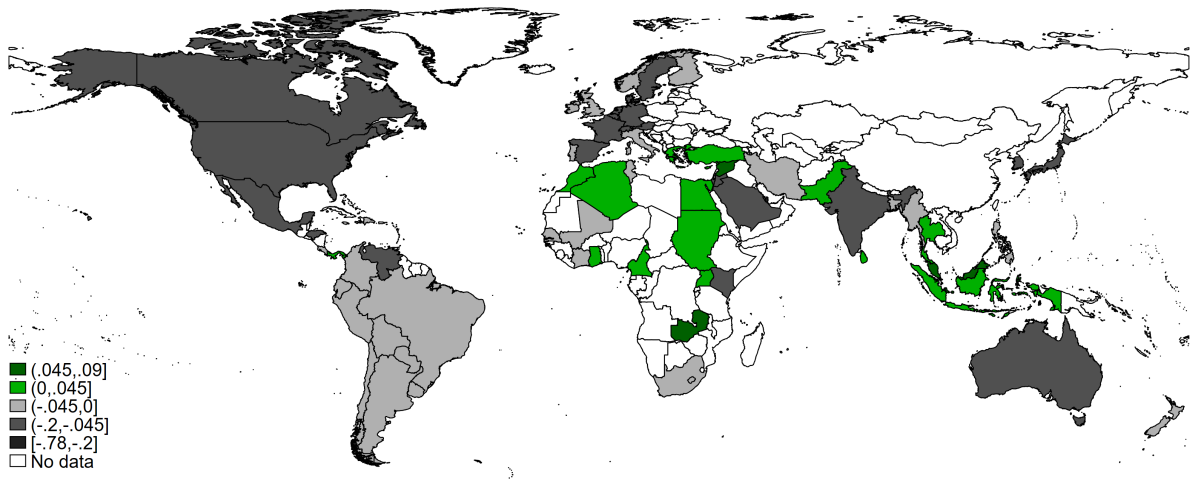
We first compute the observed long-run differences between two time periods (1970 and 2005) in the share of immigrants and in the epidemiological term. Then, using the estimated coefficients $\hat{\gamma}$ and $\hat{\delta}$ of our benchmark model (Table 2.1, column (6)), we compute the country-specific predicted variation in the WPI over the 1975–2010 period as follows:

$$\Delta\widehat{WPI}_{d,75-10} = \hat{\gamma}\Delta ShareMig_{d,70-05} + \hat{\delta}\Delta Epid_{d,70-05} \tag{2.12}$$

Figure 2.8 plots the results and shows a large degree of heterogeneity in the predicted variation for our

sample of countries. The general pattern shows that immigration decreases WPI in high-income OECD societies. This effect is explained both by the increase in the share of immigrants and by receiving immigrants from less regulated labor markets. Greece is the only exception, where the negative effect of the increase in the share of immigrants is counteracted by a positive effect from immigrants coming from countries with high levels of WPI. Concerning developing countries, the results are much more heterogeneous: African and Middle Eastern countries experienced, on average, a predicted increase of WPI as a result of immigration, whereas almost all Latin American countries experienced a predicted decrease.

Figure 2.8. WPI Simulation Based on 1975-2010 Variation



Note: Authors' calculations on CBR Leximetric data and World Bank data. The figure plots the predicted variation in our standardized measure of workers' protection due to migration. Predictions are based on equation (2.12).

To provide a better idea of the aggregate effect, column (1) of Table 2.10 presents the predicted average immigration effect on WPI for the standard scenario presented in Figure 2.8 and other four different scenarios, characterized by the following: (i) countries' epidemiological effect increased by 20% (Epid (+20%)); (ii) all the countries in our sample experienced the same variation in the epidemiological effect of the UK (Epid (UK)); (iii) all the countries in our sample experienced the same variation in the epidemiological effect of France (Epid (FR)); and (iv) countries experienced a variation in both the immigration share and the epidemiological effect following their 2000–2010 trend (Constant trends). We take France and the United Kingdom as two representative countries that are similar in terms of population and economic development while being geographically close, but significantly different in terms of their legal system (different levels of labor regulation) and immigration patterns. Although both countries experienced a sizeable increase in the share of immigrants during the analyzed period, the composition of the immigrant population was rather different: France attracted mainly immigrants from low WPI countries, producing a negative variation of -0.73 in the epidemiological effect, while the UK experienced a surge of immigrants from more regulated labor markets (increase of 0.16 in the epidemiological effect). Finally, to take into account the differences between the levels of development, the table shows the results for the whole sample (Panel A), for OECD high-income countries (Panel B), and for non-OECD countries (Panel C).

The standard scenario provides an average decrease in the workers' protection index by 4.2% of WPI standard deviations. Since the variation in the WPI over the period 1970–2010 is around one standard deviation (see Figure 2.2), the predicted effect is small albeit not negligible. The effects of immigration are smaller once countries experience an increase in their epidemiological effect (by receiving more immigrants from countries with high levels of WPI) or experience the same positive variation in the epidemiological term of the UK. On the other hand, experiencing the same change in the composition of immigrants between 1970 and 2005 as in France, or assuming that the same recent trends as in 2000–2010 will

persist in the future, generates even more negative effects. The predicted effects are even more negative for OECD countries compared to non-OECD in the standard scenario: the predicted WPI decrease in OECD countries is around 5.8% standard deviations, compared to the prediction of 3.4% for non-OECD countries.

Table 2.9. Workers' Protection and Labor Market Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Estimation:	FE	FE	FE	FE	FE	FE	FE	FE
Time:	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010
Dep var:	Unemp rate	Labor produc	Hrs worked pc	Gini	Unemp rate	Labor produc	Hrs worked pc	Gini
WPI_t	2.052***	4.958***	-47.747**	-0.010**				
	(0.708)	(1.743)	(19.301)	(0.004)				
WPI_{t-1}					2.019***	3.840**	-37.270**	-0.007
					(0.740)	(1.596)	(16.159)	(0.004)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓	✓	✓
Adj. R-Square	0.61	0.92	0.92	0.96	0.62	0.93	0.94	0.96
Countries	68	47	47	69	68	47	47	69
Observations	322	394	394	495	302	353	353	466

Note: Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01. The dependent variables are unemployment rate, labor productivity per hour worked, annual hours worked per worker, and Gini index after tax. See Appendix 2.8.1 for further information on the variables.

What would be the economic implications of these predicted effects on economic outcomes, such as wages and employment? First, we explore the relation between labor regulation and various economic outcomes. Table 2.9 shows the partial correlation between WPI and four different labor market outcomes: unemployment rate, labor productivity per hour worked, annual hours worked per worker, and the Gini index after taxes and transfers.[33] Overall, the WPI is indeed related to relevant labor market outcomes, and the partial correlations are on average statistically significant at a 5% level. Moreover, the direction of the relationship is heterogeneous across labor market outcomes. The change in the workers' protection index is positively associated with the unemployment rate and labor productivity, whereas it is negatively associated with the total number of hours worked and inequality.

Then, we compute the potential effect of the predicted WPI change by multiplying the predicted variation in WPI with the estimated coefficients associated with each labor market outcome presented in Table 2.9. The results are presented in columns (2) to (5) of Table 2.10, and each column shows the predicted effect of the variation in WPI resulting from migration on the variation in the unemployment rate (col. 2), labor productivity per hours worked (col. 3), annual hours worked per worker (col. 4), and the Gini index after tax (col. 5). In the standard scenario, the predicted reduction in WPI over the 1975–2010 period resulting from immigration should lead to a reduction in the unemployment rate of around 0.09 percentage points, a decrease in labor productivity per hours worked by 0.21, an increase of 2.02 hours worked per worker in a year, and an increase of the Gini index by 0.04 percentage points.[34] The predictions almost double when we assume a constant trend in the migration variables. Finally, the

[33]We perform our analysis using the majority of countries depicted in Figure 2.1 with five-year time periods from 1970 to 2010, and we investigate the relationship between WPI and economic outcomes in the same year and with a one-period lag. To avoid the issue of bad controls (see Angrist and Pischke (2008)), our specification includes only country and year fixed effects in order to capture time-invariant unobserved heterogeneity and common trends.

[34]These results are in line with Botero et al. (2004), who suggest in a cross-sectional setting at the country level that protective labor law regulation is negatively related to employment in the formal sector while enhancing employment in the informal one.

Table 2.10. WPI and Labor Market Outcomes Simulations

Scenarios:	(1) $\Delta\overline{WPI}$	(2) $\Delta\overline{Unemp}$	(3) $\Delta\overline{Prod}$	(4) $\Delta\overline{Hrs}$	(5) $\Delta\overline{Gini}$
Panel A - All Countries					
Standard	-0.0425	-0.0872	-0.2107	2.0291	0.0004
Epid (+20%)	-0.0367	-0.0753	-0.1819	1.7515	0.0004
Epid (UK)	-0.0247	-0.0507	-0.1224	1.1786	0.0002
Epid (FR)	-0.0943	-0.1934	-0.4673	4.5007	0.0009
Constant trends	-0.0817	-0.1677	-0.4053	3.9027	0.0008
Panel B - OECD^{HIC}					
Standard	-0.0587	-0.1205	-0.2912	2.8047	0.0006
Epid (+20%)	-0.0520	-0.1066	-0.2576	2.4805	0.0005
Epid (UK)	-0.0341	-0.0701	-0.1693	1.6304	0.0003
Epid (FR)	-0.1037	-0.2128	-0.5143	4.9525	0.0010
Constant trends	-0.0980	-0.2011	-0.4858	4.6783	0.0010
Panel C - Non-OECD^{HIC}					
Standard	-0.0340	-0.0698	-0.1687	1.6245	0.0003
Epid (+20%)	-0.0287	-0.0589	-0.1424	1.3711	0.0003
Epid (UK)	-0.0197	-0.0405	-0.0979	0.9429	0.0002
Epid (FR)	-0.0893	-0.1833	-0.4429	4.2650	0.0009
Constant trends	-0.0733	-0.1503	-0.3632	3.4981	0.0007

Note: Authors' calculations on World Bank and CBR Leximetrics data. Column (1) shows the average country variation in workers' protection due to migration over the 1975–2010 period. Columns (2) to (5) show the average country variation in different labor market outcomes due to the variation in workers' protection. Each row presents a different scenario: (i) "Standard" shows the average country variation as presented in equation (2.12); (ii) "Epid (+20%)" shows the average country variation after an increase in the epidemiological term in each country by 20%; (iii) "Epid (UK)" shows the average country variation if all the countries have the same variation in the epidemiological term of the United Kingdom; (iv) "Epid (FR)" shows the average country variation if all the countries have the same variation in the epidemiological term of France; (v) "Constant trends" shows the average country variation if all the countries have an increase in the epidemiological term and migration share as the 2010–2000 trend. Panel A presents the results for the whole sample of countries in our analysis, while Panel B and Panel C show the estimates for OECD high-income countries and non-OECD high-income countries, respectively.

economic predictions in OECD countries are twice as large as the ones in non-OECD countries. Being aware that these values have a descriptive purpose and should not be overemphasized, we note that these results aim to provide intuitive magnitudes of the non-negligible economic implications of WPI changes resulting from immigration.

2.7 Conclusions

Mainly focusing on outcomes such as wages and employment, researchers have made an extensive effort to understand the impact of immigration on natives' labor market outcomes. The mixed set of evidence based on different methodological assumptions and datasets converges toward an overall growing consensus that the effect of immigration, on average, is small or null and that labor market institutions play an important role in the process of shaping natives' reactions to the inflow of immigrants in the labor market. In these studies, labor regulations are assumed to be exogenous to the presence of immigrants—but is this the case?

This paper answers this question by using a comprehensive dataset on labor law regulation, covering 40 years for 70 countries around the world. We build a novel measure of workers' protection based on 36 different aspects of labor law regulation and explore the reaction of labor regulation to immigration, in terms of both size and composition. The paper shows that immigration is a source of legal transplants: receiving immigrants from countries with high levels of workers' protection increases the level of workers' protection, and conversely, receiving immigrants from countries with low levels of workers' protection decreases the workers' protection in the destination country. As for the size of immigration, we find that it has a small negative or null effect on the workers' protection in destination countries. These

results are robust after controlling for other competing or complementary effects of immigration, such as diversity, polarization, and skill selection. Moreover, the paper shows that the origin-specific effect is neither driven by immigrants' experience of the economic situation, level of trust or importance of family ties in the origin country, nor by the relative distance between workers' protection in the origin and destination countries, but only by the level of workers' protection in the origin country.

Across different domains of the regulation, we find that worker representation laws and employment forms laws are the ones that are mostly influenced by immigration. Moreover, we provide suggestive evidence of immigrants' participation in unions as one of the potential mechanisms explaining the results: countries characterized by a higher share of immigrants from countries with high levels of workers' protection are associated with a lower share of unionized immigrant workers. We argue that this correlation could suggest that immigrants with high levels of workers' protection in the origin country are less prone to seek union protection in the destination country. The paper shows that changes in unions' capacity to represent the workforce are related to political parties' position toward labor groups, which may ultimately influence the evolution of labor law.

Lastly, we discuss the magnitude of the effect on workers' protection with back-of-the-envelope computations. Being aware of the partial nature of this exercise, we note that the results show that, on average, immigration contributes to a reduction in WPI of 4.2% standard deviations over the 1970–2010 period. The negative effects are 72% stronger in high-income OECD countries compared to non-OECD countries.

The set of evidence provided in this paper shows that immigrants are a source of law transfer which shapes labor regulation, and we provide one new mechanism explaining this effect. We hope that this paper will contribute to future research on the labor market impact of immigration, accounting for endogenous labor regulation, and stimulate further research on the potential effect of immigration on destination countries' labor market institutions and the main mechanisms at play.

2.8 Appendix

2.8.1 Summary of Data and Variable Definition

Table 2.11. Variable Definitions and Sources

Variable	Description	Source
PANEL A - WPI		
Workers' protection, compacted (2SFA, S.D.)	Calculated using factor analysis composed from 5 subindexes below	Adams et al. (2017)
Working time laws (FA, S.D.)	The index includes following variables: 1) Annual leave entitlements; 2) Public holiday entitlements; 3) Overtime premia; 4) Weekend working; 5) Limits to overtime working; 6) Duration of the normal working week; 7) Maximum daily working time	
Worker dismissal laws (FA, S.D.)	The index includes following variables: 1) Legally mandated notice period; 2) Legally mandated redundancy compensation; 3) Minimum qualifying period of service for normal case of unjust dismissal; 4) Law imposes procedural constraints on dismissal; 5) Law imposes substantive constraints on dismissal; 6) Reinstatement normal remedy for unfair dismissal; 7) Notification of dismissal; 8) Redundancy selection; 9) Priority in re-employment	
Worker representation laws (FA, S.D.)	The index includes following variables: 1) Right to unionization; 2) Right to collective bargaining; 3) Duty to bargain; 4) Extension of collective agreements; 5) Closed shops; 6) Codetermination: board membership; 7) Codetermination and information/consultation of workers	
Industrial action laws (FA, S.D.)	The index includes following variables: 1) Unofficial industrial action; 2) Political industrial action; 3) Secondary industrial action; 4) Lockouts; 5) Right to industrial action	
Employment forms laws (FA, S.D.)	The index includes following variables: 1) The law, as opposed to the contracting parties, determines the legal status of the worker; 2) Part-time workers have the right to equal treatment with full-time workers; 3) The cost of dismissing part-time workers is equal in proportionate terms to the cost of dismissing full-time workers; 4) Fixed-term contracts are allowed only for work of limited duration; 5) Fixed-term workers have the right to equal treatment with permanent workers; 6) Maximum duration of fixed-term contracts; 7) Agency work is prohibited or strictly controlled; 8) Agency workers have the right to equal treatment with permanent workers of the user undertaking	
PANEL B - Country Level Variables		
Share of immigrants	Share of immigrants over 2000 population (%)	Artuç et al. (2014); World Bank (2010)
Epidemiological Effect	Epidemiological term (see Eq. (2.2))	Adams et al. (2017); Artuç et al. (2014); World Bank (2010)
GDP per capita (log)	Real GDP/capita at constant 2011 national prices (in mil. 2011US$)	Feenstra et al. (2015)
Polity2	Measure of political regime. Time-varying dummy =1 (Democratic regime) for polity score greater or equal to 5 and otherwise =0 (Autocratic regime)	Marshall et al. (2002)
Human capital (log)	Years of schooling	Barro and Lee (2013)
Rule of Law	Rule of Law index	Coppedge et al. (2020)
Shadow Economy	Size of the shadow economy	Elgin et al. (2012)
Civil Liberties	The civil liberties index	House (2016)
Economic Freedom Index	The Economic Freedom Index	Gwartney et al. (2018)
Trade / GDP	Trade (Exports plus Imports) as a share of GDP	World Bank (2018b)
ctfp	TFP level at current PPPs (USA=1)	Feenstra et al. (2015)
EU	Dummy = 1 if country is a member of European Union	
ILO	Dummy = 1 if country is a member of the International Labour Organization	
WTO	Dummy = 1 if country is a member of the World Trade Organization	
PANEL C - Labor Market Outcomes Variables		
Unemp rate	Share of unemployed in the total labor force (national estimate)	ILO (2019)
Labor productivity	Labor productivity per hour worked in 2017 USD (converted to 2017 price level with updated 2011 PPPs)	Total Economy Database (2019)
Hrs Worked Pc	Annual hours worked per worker	Total Economy Database (2019)
Gini	Estimate of Gini index of inequality in equivalized (square root scale) household disposable (post-tax, post-transfer) income, using Luxembourg Income Study data as the standard	Solt (2016)
Common law	Time invariant. Dummy = 1 for common law and civil law otherwise	La Porta et al. (2008)

Table 2.12. Variable Definitions and Sources Part2

Variable	Description	Source
PANEL D - Gravity Model Variables		CEPII (2010), Head et al. (2010)
Bilateral weighted distance	Geodesic distance in km	
Colonial relationship	Dummy = 1 for pair ever in colonial relationship	
Common ethnic language	Dummy = 1 for pair with language shared by at least 9% of populations	
Common official language	Dummy = 1 for pair with same official language	
Horizontal Time difference	Difference in time zones in hours	
PANEL E - Unions and Parties Political Preferences Variables		
Unions	Share of unionized workers over the total workers population	European Social Survey
Lab. Groups	Difference between the percentage of quasi-sentence in favor and against labour groups in parties' political manifesto	Manifesto Project Database, Volkens et al. (2019)

Table 2.13. Summary Statistics - 70 Countries, 1970-2010

Variable	Mean	S.D.	Min.	Max.	Obs.	Corr
PANEL A - Workers Protection Index						
$WPI_{\ t}$ (2SFA)	0.00	1.00	-2.55	2.69	630	1.00***
$EmptForm_{\ t}$ (FA)	-0.00	1.00	-1.31	2.79	630	0.74***
$WorkTime_{\ t}$ (FA)	-0.00	1.00	-2.87	1.61	630	0.50***
$WkrDismis_{\ t}$ (FA)	-0.00	1.00	-2.23	1.99	630	0.65***
$WkrRepr_{\ t}$ (FA)	-0.00	1.00	-1.81	2.35	630	0.78***
$IndAction_{\ t}$ (FA)	-0.00	1.00	-1.35	2.30	630	0.29***
PANEL B - Country Level Variables						
Share of migrants $_{t-1}$	5.71	10.19	0.06	135.43	554	
Epidemiological effect $_{t-1}$	0.01	1.01	-3.02	2.68	554	
GDP per capita (log) $_{t-1}$	-4.68	1.11	-7.28	-1.68	554	
Polity2 $_{t-1}$	0.58	0.49	0.00	1.00	554	
Human capital (log) $_{t-1}$	1.61	0.68	-1.63	2.57	554	
Rule of Law $_{t-1}$	0.61	0.32	0.03	1.00	554	
Shadow Economy $_{t-1}$	31.15	14.59	8.11	71.99	495	
Civil Liberties $_{t-1}$	4.73	1.85	1.00	7.00	487	
Economic Freedom Index $_{t-1}$	5.91	1.39	2.45	9.05	499	
Trade / GDP $_{t-1}$	65.32	50.23	1.24	423.28	532	
$OECD^{HIC}$	0.34	0.47	0.00	1.00	554	
Common law	0.32	0.47	0.00	1.00	554	
PANEL C - Labor Market Outcomes Variables						
Unemp rate $_{t}$	7.00	5.00	0.20	31.84	322	
Labor productivity $_{t}$	28.54	20.13	0.64	95.33	394	
Hrs Worked Pc $_{t}$	1951.26	275.32	1389.88	2746.89	394	
Gini $_{t}$	0.38	0.09	0.20	0.59	495	
PANEL D - Gravity Model Variables						
Bilateral weighted distance $_{t}$	8249.82	4622.22	1.00	19781.39	388287	
Colonial relationship $_{t}$	0.01	0.10	0.00	1.00	388287	
Common ethnic language $_{t}$	0.16	0.36	0.00	1.00	388287	
Common official language $_{t}$	0.17	0.37	0.00	1.00	388287	
Common border $_{t}$	0.01	0.12	0.00	1.00	388287	
Horizontal Time difference $_{t}$	4.86	3.46	0.00	12.00	388287	

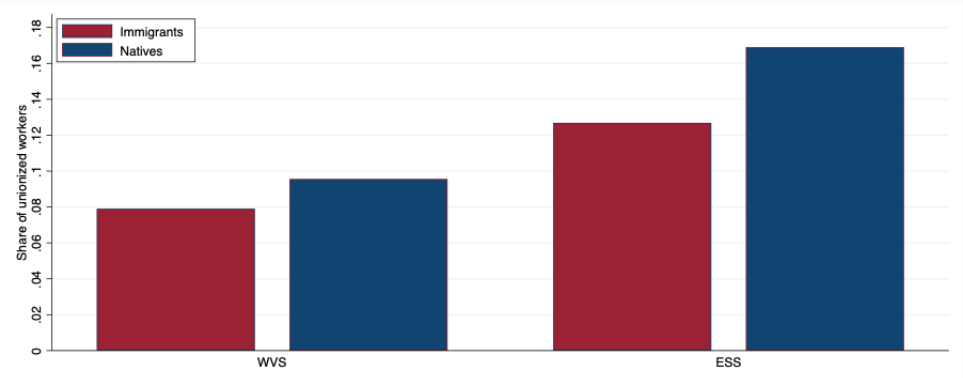
Note: For detailed sources and definitions, see Appendix Table 2.11. * p<0.1, ** p<0.05, *** p<0.01.

Table 2.14. Workers Protection Index - Summary Statistics by Country

Country	Mean	Min	Max	Country	Mean	Min	Max	Country	Mean	Min	Max
Algeria	1.59	0.31	2.34	Honduras	0.18	0.01	0.2	Portugal	2.05	-0.21	2.69
Argentina	0.86	0.18	1.21	India	-0.01	-0.37	0.17	Qatar	-0.68	-0.77	-0.63
Australia	-1.38	-1.68	-0.61	Indonesia	0.36	0.11	0.85	Saudi Arabia	-1.06	-1.15	-0.53
Austria	1	0.13	1.48	Iran	-0.87	-1.4	-0.35	Senegal	0.69	0.24	1.07
Bangladesh	-0.34	-0.38	-0.05	Ireland	-0.86	-1.63	0.44	Singapore	-0.9	-0.99	-0.66
Belgium	0.39	-0.39	1.25	Israel	-0.08	-0.39	0.43	South Africa	-0.27	-0.76	0.47
Bolivia	0.49	0.35	1.18	Italy	1.63	1.52	1.78	South Korea	0.36	-0.39	1.51
Brazil	0.38	-0.04	0.78	Japan	-0.58	-0.65	-0.49	Spain	1.11	-0.32	1.72
Cameroon	0.06	-0.36	0.24	Jordan	-0.75	-0.95	-0.2	Sri Lanka	-0.05	-0.14	0.08
Canada	-0.93	-1.16	-0.62	Kenya	-1.2	-1.53	-0.43	Sudan	-1.13	-1.73	-0.69
Chile	-0.35	-0.8	0.2	Luxembourg	1.2	-0.96	2.36	Sweden	0.79	-1.29	1.83
Colombia	0	-0.19	0.22	Malaysia	-0.65	-1.05	-0.43	Switzerland	-0.23	-0.57	0.13
Costa Rica	-0.56	-0.57	-0.53	Mali	-0.35	-0.6	-0.01	Syria	-0.57	-0.62	-0.21
Cote d'Ivoire	-0.92	-1.58	0.16	Mexico	0.31	0.31	0.31	Thailand	-0.41	-1.91	0.07
Cyprus	0	-0.21	0.75	Morocco	0.76	-0.07	1.38	Tunisia	0.32	-0.43	1.12
Denmark	0.29	-0.79	0.92	Myanmar	-1.19	-1.2	-1.1	Turkey	0.02	-0.26	0.99
Dominican Republic	-0.53	-0.6	-0.21	Netherlands	0.89	0.19	1.9	Uganda	-1.29	-1.7	-0.3
Ecuador	0.81	0.07	1.57	New Zealand	-0.93	-1.65	-0.52	United Kingdom	-1.15	-1.83	-0.08
Egypt	0.46	-0.14	0.84	Norway	1	-0.04	2.13	United States	-2.46	-2.55	-2.38
Finland	0.77	-0.56	1.85	Pakistan	1.12	-0.25	1.35	Uruguay	0.27	-0.63	0.67
France	1.88	-0.1	2.5	Panama	-0.25	-1.45	0.09	Venezuela	0.79	0.26	1.64
Germany	1.45	1.16	1.94	Paraguay	0.28	0.16	0.44	Zambia	-1.4	-2.09	-1.07
Ghana	-0.44	-0.89	0.32	Peru	0.65	-0.22	1.21				
Greece	0.09	-1.12	1.22	Philippines	-0.57	-2.3	0.16				

Note: List of all 70 countries used in the analysis. Balanced panel with nine five-year periods for each country. The values presented are the average, minimum, and maximum values for the standardized measures of the workers' protection index during the 1970–2010 period.

Figure 2.9. Union density - Immigrants and Natives



Note: Authors' calculations on WVS and ESS data. The figure plots the share of unionized workers over the overall employed population by immigration status and by data source.

2.8.2 Factor Analysis

Table 2.15. Factor Analysis - Employment Forms & Working Time

Employment Forms

Factor	(1) Eigenvalue	(2) Difference	(3) Proportion
Factor1	2.047	1.576	0.910
Factor2	0.475	0.168	0.211
Factor3	0.307	0.187	0.136
Factor4	0.120	0.144	0.053

Working Time

Factor	(1) Eigenvalue	(2) Difference	(3) Proportion
Factor1	1.338	0.887	1.049
Factor2	0.451	0.351	0.353
Factor3	0.100	0.104	0.079

Table 2.16. Factor Loadings - Employment Forms & Working Time

Employment Forms

Variable	Factor 1	Factor 2	Factor 3	Factor 4
$EmptForm_1$	0.394	0.341	-0.084	-0.109
$EmptForm_2$	0.537	-0.338	-0.154	-0.036
$EmptForm_3$	0.139	-0.004	0.087	0.275
$EmptForm_4$	0.517	0.276	-0.232	-0.015
$EmptForm_5$	0.701	-0.308	-0.084	-0.010
$EmptForm_6$	0.544	0.188	-0.073	0.163
$EmptForm_7$	0.410	0.175	0.322	-0.047
$EmptForm_8$	0.602	-0.088	0.315	-0.050

Working Time

Variable	Factor 1	Factor 2	Factor 3
$WorkTime_1$	0.335	0.409	-0.073
$WorkTime_2$	0.343	0.160	0.068
$WorkTime_3$	0.369	-0.052	0.170
$WorkTime_4$	0.487	-0.008	0.178
$WorkTime_5$	0.621	-0.159	-0.120
$WorkTime_6$	0.050	0.453	-0.020
$WorkTime_7$	0.589	-0.158	-0.123

Table 2.17. Correlations - Employment Form & Working Time

Employment Forms

	$EmptForm_{Ind}$
$EmptForm_1$	0.452***
$EmptForm_2$	0.615***
$EmptForm_3$	0.159***
$EmptForm_4$	0.592***
$EmptForm_5$	0.804***
$EmptForm_6$	0.624***
$EmptForm_7$	0.470***
$EmptForm_8$	0.690***

Working Time

	$WorkTime_{Ind}$
$WorkTime_1$	0.420***
$WorkTime_2$	0.430***
$WorkTime_3$	0.463***
$WorkTime_4$	0.610***
$WorkTime_5$	0.778***
$WorkTime_6$	0.0633***
$WorkTime_7$	0.737***

Table 2.18. Factor Analysis - Workers' Dismissal & Employment Representation

Workers Dismissal

Factor	(1) Eigenvalue	(2) Difference	(3) Proportion
Factor1	1.878	1.259	0.905
Factor2	0.619	0.374	0.298
Factor3	0.245	0.101	0.118
Factor4	0.143	0.183	0.069

Employment Representation

Factor	(1) Eigenvalue	(2) Difference	(3) Proportion
Factor1	1.047	0.507	0.874
Factor2	0.540	0.320	0.451
Factor3	0.221	0.221	0.184

Table 2.19. Factor Loadings - Workers' Dismissal & Employment Representation

Workers Dismissal

Variable	Factor 1	Factor 2	Factor 3	Factor 4
$WkrDismis_1$	0.024	0.174	0.152	0.064
$WkrDismis_2$	0.046	-0.092	0.246	-0.173
$WkrDismis_3$	0.121	-0.091	0.044	0.254
$WkrDismis_4$	0.261	-0.093	-0.268	-0.151
$WkrDismis_5$	0.254	-0.189	0.167	-0.071
$WkrDismis_6$	0.192	-0.157	-0.013	0.167
$WkrDismis_7$	0.191	-0.001	-0.052	-0.049
$WkrDismis_8$	0.166	0.386	-0.137	-0.029
$WkrDismis_9$	0.176	0.277	0.188	0.020

Employment Representation

Variable	Factor 1	Factor 2	Factor 3
$WkrRepr_1$	0.185	0.199	0.028
$WkrRepr_2$	0.209	0.242	0.060
$WkrRepr_3$	0.101	0.083	0.311
$WkrRepr_4$	0.147	0.207	-0.107
$WkrRepr_5$	0.036	-0.202	0.209
$WkrRepr_6$	0.250	-0.238	0.036
$WkrRepr_7$	0.390	-0.223	-0.178

Table 2.20. Correlations - Workers' Dismissal & Employment Representation

Workers Dismissal

	$WkrDismis_{Ind}$
$WkrDismis_1$	0.0864***
$WkrDismis_2$	0.190***
$WkrDismis_3$	0.439***
$WkrDismis_4$	0.725***
$WkrDismis_5$	0.711***
$WkrDismis_6$	0.615***
$WkrDismis_7$	0.640***
$WkrDismis_8$	0.510***
$WkrDismis_9$	0.525***

Employment Representation

	$WkrRepr_{Ind}$
$WkrRepr_1$	0.496***
$WkrRepr_2$	0.528***
$WkrRepr_3$	0.237***
$WkrRepr_4$	0.411***
$WkrRepr_5$	0.138***
$WkrRepr_6$	0.634***
$WkrRepr_7$	0.798***

Table 2.21. Factor Analysis - Industrial Actions & Workers' Protection Index

Industrial Actions

Factor	(1) Eigenvalue	(2) Difference	(3) Proportion
Factor1	0.922	0.477	1.134
Factor2	0.445	0.577	0.547

Workers Protection Index

Factor	(1) Eigenvalue	(2) Difference	(3) Proportion
Factor1	1.186	1.176	1.450
Factor2	0.010	0.043	0.012

Table 2.22. Factor Loadings - Industrial Actions & Workers' Protection Index

Industrial Actions

Variable	Factor 1	Factor 2
$IndAction_1$	0.449	-0.252
$IndAction_2$	0.562	0.058
$IndAction_3$	0.587	-0.116
$IndAction_4$	0.185	0.410
$IndAction_5$	0.163	0.443

Workers' Protection Index

Variable	Factor 1	Factor 2
$EmptForm_{Ind}$	0.305	-0.039
$WorkTime_{Ind}$	0.179	0.024
$WkrDismis_{Ind}$	0.245	-0.015
$WkrRepr_{Ind}$	0.334	0.001
$IndAction_{Ind}$	0.081	0.095

Table 2.23. Correlations - Industrial Actions & Workers' Protection Index

Industrial Actions		**Workers' Protection Index**	
	$IndAction_{Ind}$		WPI
$IndAction_1$	0.613***	$EmptForm_{Ind}$	0.752***
$IndAction_2$	0.767***	$WorkTime_{Ind}$	0.522***
$IndAction_3$	0.801***	$WkrDismis_{Ind}$	0.661***
$IndAction_4$	0.253***	$WkrRepr_{Ind}$	0.796***
$IndAction_5$	0.223***	$IndAction_{Ind}$	0.255***

2.8.3 Identification Strategy - Additional Results

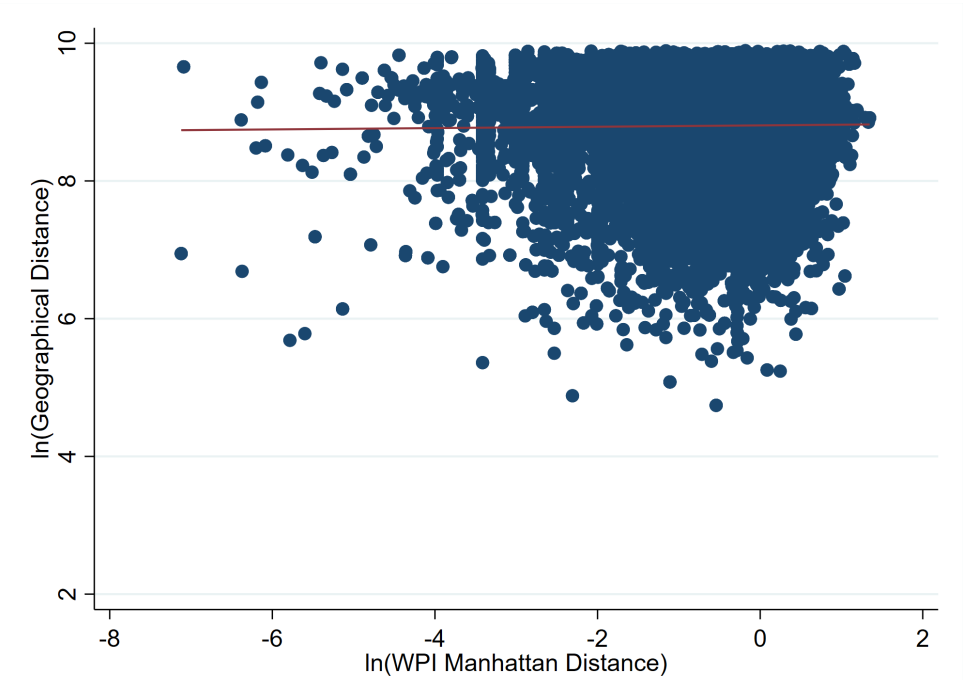
Table 2.24. Predicted Stocks through Gravity Model

	(1)
Estimation:	PPML
Time:	1970-2010
Dep var:	$Stock_{c,j,t}$
$Dist^w_{c,j} * I_{1970}$	-0.038***
	(0.009)
$Dist^w_{c,j} * I_{1975}$	-0.037***
	(0.008)
$Dist^w_{c,j} * I_{1980}$	-0.035***
	(0.008)
$Dist^w_{c,j} * I_{1985}$	-0.034***
	(0.007)
$Dist^w_{c,j} * I_{1990}$	-0.033***
	(0.006)
$Dist^w_{c,j} * I_{1995}$	-0.031***
	(0.006)
$Dist^w_{c,j} * I_{2000}$	-0.030***
	(0.005)
$Dist^w_{c,j} * I_{2005}$	-0.029***
	(0.005)
$Dist^w_{c,j} * I_{2010}$	-0.029***
	(0.004)
Observations	137970
Countries	70
Partial R-Square	0.44

Note: Authors' calculations on World Bank data. Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01. The set of controls includes the interactions between bilateral distance (weighted by population size) and year dummies, year fixed effects, and destination country fixed effects. The dependent variable is the bilateral migration stock.

2.8.4 Additional Analysis

Figure 2.10. Bilateral Geographical and WPI Distance



Note: Authors' calculations on Head et al. (2010) data. The figure plots the scatterplot and the regression line between the logarithm of geographical distance and the logarithm of workers' protection index distance, computed as a Manhattan distance.

Table 2.25. Weak Instrument Tests

	(1)	(2)
Estimation:	IV	IV
Time:	1970-2010	1970-2010
Dep Var:	WPI	WPI
Equation:	Levels	Differences
$Prot_{t-1}$	0.922***	0.004
	(0.020)	(0.044)
$Share\ Mig_{t-1}$	-0.000	0.002
	(0.001)	(0.005)
$Epid_{t-1}$	0.057***	-0.400
	(0.021)	(0.471)
Year FE	✓	✓
Instruments	10	9
Observations	560	490
KP LM test p-val	0.00	0.00
KP F-stat	812.85	17.00
KP rel bias>30%	0.00	0.00

Note: The table reports weak instrument diagnostics. For the KP p-val, since critical values do not exist for the KP statistic, we follow the approach suggested by Bazzi and Clemens (2013) and use the Stock et al. (2005) 30% of the OLS bias critical values for the multivariate statistic. Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01.

Table 2.26. Robustness Checks: Lag Structure of Instruments

	(1)	(2)	(3)	(4)	(5)	(6)
Estimation:	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM
Time:	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010
Dep var:	WPI	WPI	WPI	WPI	WPI	WPI
$Prot_{t-1}$	0.848***	0.822***	0.848***	0.851***	0.829***	0.829***
	(0.054)	(0.081)	(0.054)	(0.054)	(0.047)	(0.048)
$Share\ Mig_{t-1}$	-0.006*	-0.004	-0.006*	-0.006	-0.006*	-0.007*
	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
$Epid_{t-1}$	0.085***	0.106***	0.081***	0.076***	0.077***	0.077***
	(0.028)	(0.039)	(0.029)	(0.028)	(0.028)	(0.028)
Controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓
AR1 p-val	0.00	0.00	0.00	0.00	0.00	0.00
AR2 p-val	0.52	0.48	0.54	0.55	0.57	0.57
Hansen p-val	0.78	0.11	0.61	0.80	0.49	0.57
Diff-Hansen p-val	0.91	0.30	0.91	0.79	0.74	0.83
Instruments	22	28	23	27	31	33
Countries	70	70	70	70	70	70
Observations	554	554	554	554	554	554
$First\ lag\ WPI_{t-1}$	3	3	3	3	4	4
$Last\ lag\ WPI_{t-1}$	4	4	5	6	5	7
$First\ lag\ Controls_{t-1}$	2	4	2	2	2	2
$Last\ lag\ Controls_{t-1}$	3	7	3	4	6	6

Note: Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01. The dependent variable is our standardized workers' protection index. As controls, we include the logarithm of GDP per capita, the polity2 index, and the logarithm of human capital. Our main variables of interest are the share of immigrants and the epidemiological effect: the former is instrumented using the predicted immigration share from a gravity model and the latter with the epidemiological effect using predicted bilateral immigration stocks from a shift-share approach. Each column includes a different set of lags. See Appendix 2.8.1 for further information on the variables.

Table 2.27. Rotemberg Weights

	1975				1990				2010			
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Origin	$\hat{\alpha_k}$	$\sum \hat{\alpha_k}$	Ratio	Origin	$\hat{\alpha_k}$	$\sum \hat{\alpha_k}$	Ratio	Origin	$\hat{\alpha_k}$	$\sum \hat{\alpha_k}$	Ratio
Portugal	0.052	1.199	0.434	Portugal	0.079	1.652	0.405	Morocco	0.049	1.582	0.412
Germany	0.057	1.199	0.434	Philippines	0.084	1.652	0.405	Puerto Rico	0.056	1.582	0.412
Mexico	0.083	1.199	0.434	Puerto Rico	0.090	1.652	0.405	Italy	0.065	1.582	0.412
Italy	0.122	1.199	0.434	Italy	0.128	1.652	0.405	Philippines	0.091	1.582	0.412
Pakistan	0.206	1.199	0.434	Mexico	0.288	1.652	0.405	Mexico	0.390	1.582	0.412

Note: Authors' calculations following (Goldsmith-Pinkham et al., 2020) methodology. The table shows the top-five Rotemberg weights ($\hat{\alpha}$) by origin and year (1975, 1990, and 2010). Columns (3), (7), and (11) report the sum of the positive Rotemberg weights, while columns (4), (8), and (12) report the ratio between the sum of the top-five Rotemberg weights by origin and the total positive weights.

Table 2.28. Origin Country Shares and Destination Countries' Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Time	1960	1960	1960	1960	1960	1960	1960	1960	1960	1970-2010
	Origin Shares									$\widetilde{Epid}^{SS}_{70-10}$
Origin	GER	ITA	MEX	MOR	PAK	PHIL	POR	PUE	*All*	
$ln(GDP)$	0.005	0.015	0.001	-0.007	-0.042	0.003	0.008*	0.001	-0.016	0.237
	(0.01)	(0.01)	(0.01)	(0.01)	(0.04)	(0.01)	(0.00)	(0.01)	(0.06)	(0.17)
$ln(Col)$	0.005	0.009	0.022	0.007	0.001	0.015	0.001	0.022	0.083	-0.239*
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.00)	(0.02)	(0.08)	(0.14)
$ln(PopD)$	-0.002	-0.002	-0.004	0.005	0.014	-0.003	-0.006	-0.004	-0.002	-0.078
	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.07)
$Polity2$	0.000	0.000	-0.001	-0.001	0.002	-0.001	0.002	-0.001	0.001	0.011
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Common Law	0.023	0.009	0.051	0.011	0.059	0.043	-0.015	0.052	0.233	0.015
	(0.02)	(0.02)	(0.05)	(0.02)	(0.06)	(0.04)	(0.02)	(0.05)	(0.19)	(0.29)
$OECD^{HIC}$	0.023*	-0.013	0.019	0.026	-0.002	0.016	-0.025	0.018	0.063	0.042
	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.11)	(0.43)
Observations	62	62	62	62	62	62	62	62	62	62
R-Square	0.18	0.19	0.10	0.11	0.14	0.10	0.05	0.10	0.12	0.04

Note: Authors' calculations on World Bank and Maddison Project data. * p<0.1, ** p<0.05, *** p<0.01. Columns (1) to (8) report results of a single regression of a 1960 origin-specific share on 1960 destination countries' characteristics. Column (9) reports the results using as a dependent variable the sum of the origin-specific shares identified in Table 2.27. Column (10) reports the estimates on the predicted growth of the epidemiological term using the predicted stocks constructed through our shift-share strategy between 1970 and 2010.

Table 2.29. Correlation Between pre-1960 Indicators and Shift-Share-Based Epidemiological Term Growth

	(1)	(2)	(3)	(4)
Estimation:	OLS	OLS	OLS	OLS
Dep var:	$\widetilde{Epid}^{SS}_{70-80}$	$\widetilde{Epid}^{SS}_{70-90}$	$\widetilde{Epid}^{SS}_{70-00}$	$\widetilde{Epid}^{SS}_{70-10}$
Panel A - GDP per capita				
	0.235	0.361	-0.121	-0.032
	(0.570)	(0.577)	(0.300)	(0.251)
Countries/Observations	68	43	43	40
Panel B - Population				
	1.492	0.033	0.190	0.121
	(0.931)	(0.618)	(0.365)	(0.298)
Countries/Observations	70	45	45	44
Panel C - Legal origin: Common Law				
	0.081	0.065	0.110	0.072
	(0.310)	(0.312)	(0.277)	(0.307)
Countries/Observations	70	70	70	70

Note: Authors' calculations on World Bank and Maddison Project data. * p<0.1, ** p<0.05, *** p<0.01. The dependent variable is our standardized measure of workers' protection. In Panel A (GDP per capita) and Panel B (Population) the table shows the predicted coefficients regressing the growth rate of macro indicators between: 1950 to 1960 (col. 1), 1940 and 1960 (col. 2), 1930 and 1960 (col. 3), and 1920 and 1960 (col. 4) on the national predicted epidemiological effect with our shift-share strategy over different periods. Panel C shows the predicted coefficients of common law legal origin on the national predicted epidemiological effect with our shift-share strategy over different periods.

Table 2.30. Robustness Checks: Adao Standard Error Correction

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Estimation:	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM
Time:	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010
Dep var:	WPI	WPI	WPI	WPI	WPI	WPI	WPI	WPI	WPI
SE clustered at:	CNT	GER	ITA	MEX	MOR	PAK	PHL	POR	PUE
$Prot_{t-1}$	0.863***	0.863***	0.863***	0.863***	0.863***	0.863***	0.863***	0.863***	0.863**
	(0.057)	(0.065)	(0.057)	(0.049)	(0.076)	(0.060)	(0.052)	(0.057)	(0.060)
$Share\ Mig_{t-1}$	-0.006	-0.006	-0.006*	-0.006	-0.006*	-0.006	-0.006	-0.006	-0.006
	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.003)	(0.002)
$Epid_{t-1}$	0.080***	0.080**	0.080**	0.080*	0.080**	0.080**	0.080***	0.080**	0.080*
	(0.030)	(0.033)	(0.031)	(0.038)	(0.038)	(0.032)	(0.027)	(0.034)	(0.006)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
AR1 p-val	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
AR2 p-val	0.51	0.51	0.51	0.51	0.51	0.51	0.51	0.51	0.51
Hansen p-val	0.79	0.98	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Diff-Hansen p-val	0.84	0.93	0.98	1.00	0.98	0.98	0.95	0.98	(.)
Instruments	28	28	28	28	28	28	28	28	28
Clusters	70	20	20	16	19	19	19	18	2
Countries	70	70	70	70	70	70	70	70	70
Observations	554	554	554	554	554	554	554	554	554

Note: * p<0.1, ** p<0.05, *** p<0.01. The dependent variable is our standardized workers' protection index. As controls, we include the logarithm of GDP per capita, the polity2 index, and the logarithm of human capital. Our main variables of interest are the share of immigrants and the epidemiological effect: the former is instrumented using the predicted immigration share from a gravity model and the latter with the epidemiological effect using predicted bilateral immigration stocks from a shift-share approach. See Appendix 2.8.1 for further information on the variables. The table reports system GMM results across different standard error clusters: country level (col. 1), and grouping countries with similar initial shares of immigrants in the year 1960 from Germany (col. 2), Italy (col. 3), Mexico (col. 4), Morocco (col. 5), Pakistan (col. 6), Philippines (col. 7), Portugal (col. 8), and Puerto Rico (col. 9).

Table 2.31. Workers' Protection and Immigration with all Time-varying Components of the Share of Immigrants

	(1)	(2)	(3)	(4)	(5)	(6)
Estimation:	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM
Time:	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010
Dep var:	WPI	WPI	WPI	WPI	WPI	WPI
$Prot_{t-1}$	0.894***	0.854***	0.852***	0.845***	0.842***	0.815***
	(0.057)	(0.064)	(0.067)	(0.063)	(0.062)	(0.060)
$Share\ Mig\ Tvar_{t-1}$	-0.003		-0.003**	-0.004	-0.006*	-0.008**
	(0.002)		(0.002)	(0.004)	(0.003)	(0.003)
$Epid_{t-1}$		0.094**	0.093**	0.088**	0.087***	0.084***
		(0.041)	(0.040)	(0.035)	(0.031)	(0.030)
$ln(GDP)_{t-1}$				0.006	0.046	0.086
				(0.060)	(0.051)	(0.055)
$Polity2_{t-1}$					-0.109	-0.018
					(0.105)	(0.096)
$ln(HC)_{t-1}$						-0.039
						(0.069)
Year FE	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓
AR1 p-val	0.00	0.00	0.00	0.00	0.00	0.00
AR2 p-val	0.57	0.57	0.57	0.56	0.49	0.55
Hansen p-val	0.38	0.41	0.41	0.58	0.72	0.79
Diff-Hansen p-val	0.31	0.59	0.53	0.88	0.91	0.81
Instruments	15	15	16	20	24	28
Countries	70	70	70	70	70	70
Observations	554	554	554	554	554	554

Note: Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01. This table reproduces Table 2.1 while changing the epidemiological measure in which the WPI is time-varying. The dependent variable is our standardized workers' protection index. As controls, we include the logarithm of GDP per capita, the polity2 index, and the logarithm of human capital. Our main variables of interest are the epidemiological effect and the recalculated share of immigrants with all time-varying components: the former is instrumented using the predicted immigration share from a gravity model and the latter with the epidemiological effect using predicted bilateral immigration stocks from a shift-share approach. See Appendix 2.8.1 for further information on the variables.

Table 2.32. Workers' Protection and Immigration with all Time-varying Components of the Epidemiological Term

	(1)	(2)	(3)	(4)	(5)	(6)
Estimation:	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM
Time:	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010
Dep var:	WPI	WPI	WPI	WPI	WPI	WPI
$Prot_{t-1}$	0.895***	0.839***	0.838***	0.838***	0.840***	0.835***
	(0.056)	(0.073)	(0.075)	(0.062)	(0.062)	(0.060)
$Share\ Mig_{t-1}$	-0.002		-0.003	-0.003	-0.005	-0.007*
	(0.002)		(0.002)	(0.004)	(0.003)	(0.004)
$Epid\ Tvar_{t-1}$		0.103**	0.103**	0.099**	0.095***	0.089***
		(0.045)	(0.045)	(0.038)	(0.035)	(0.034)
$ln(GDP)_{t-1}$				-0.001	0.030	0.048
				(0.051)	(0.048)	(0.057)
$Polity2_{t-1}$					-0.080	-0.014
					(0.108)	(0.095)
$ln(HC)_{t-1}$						-0.005
						(0.072)
Year FE	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓
AR1 p-val	0.00	0.00	0.00	0.00	0.00	0.00
AR2 p-val	0.57	0.58	0.57	0.57	0.51	0.56
Hansen p-val	0.37	0.38	0.35	0.52	0.70	0.80
Diff-Hansen p-val	0.27	0.40	0.27	0.57	0.50	0.65
Instruments	15	15	16	20	24	28
Countries	70	70	70	70	70	70
Observations	554	554	554	554	554	554

Note: Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01. This table reproduces Table 2.1 while changing the epidemiological measure in which the WPI is time-varying. The dependent variable is our standardized workers' protection index. As controls, we include the logarithm of GDP per capita, the polity2 index, and the logarithm of human capital. Our main variables of interest are the share of immigrants and the recalculated epidemiological effect with a time-varying WPI: the former is instrumented using the predicted immigration share from a gravity model and the latter with the epidemiological effect using predicted bilateral immigration stocks from a shift-share approach. See Appendix 2.8.1 for further information on the variables.

Table 2.33. Robustness Checks: Additional Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Estimation:	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM
Time:	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010
Dep var:	WPI	WPI	WPI	WPI	WPI	WPI	WPI	WPI
$WPIt-1$	0.834***	0.806***	0.779***	0.857***	0.864***	0.796***	0.815***	0.822***
	(0.073)	(0.068)	(0.079)	(0.065)	(0.066)	(0.082)	(0.070)	(0.065)
$Share\ Mig_{t-1}$	-0.007	-0.003	-0.006**	-0.009	-0.004	-0.005	-0.005	-0.007
	(0.004)	(0.003)	(0.003)	(0.006)	(0.004)	(0.006)	(0.005)	(0.004)
$Epid_{t-1}$	0.079**	0.091**	0.098**	0.071**	0.077**	0.088**	0.077**	0.087**
	(0.031)	(0.035)	(0.038)	(0.029)	(0.034)	(0.042)	(0.030)	(0.034)
$Rule\ Of\ Law_{t-1}$	-0.134							
	(0.305)							
$Civil\ Liberties_{t-1}$		0.122**						
		(0.054)						
$Shadow\ Econ_{t-1}$			0.003					
			(0.007)					
$Econ\ Freedom_{t-1}$				0.015				
				(0.044)				
$Trade/GDP_{t-1}$					-0.002*			
					(0.001)			
EU						0.269*		
						(0.144)		
ILO							0.365	
							(0.491)	
WTO								0.075
								(0.084)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓	✓	✓
AR1 p-val	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
AR2 p-val	0.53	0.34	0.78	0.91	0.81	0.59	0.56	0.61
Hansen p-val	0.80	0.83	0.85	0.62	0.92	0.06	0.42	0.21
Diff-Hansen p-val	0.81	0.62	0.70	0.56	0.87	0.12	0.42	0.81
Instruments	32	31	32	32	32	31	32	31
Countries	70	70	64	67	70	70	70	70
Observations	554	487	495	499	532	554	554	554

Note: Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01. The dependent variable is our standardized workers' protection index. As controls, we include the logarithm of GDP per capita, the polity2 index, and the logarithm of human capital. Our main variables of interest are the share of immigrants and the epidemiological effect: the former is instrumented using the predicted immigration share from a gravity model and the latter with the epidemiological effect using predicted bilateral immigration stocks from a shift-share approach. As additional controls, we include rule of law index, size of the shadow economy, civil liberties, economic freedom index, trade (exports plus imports) as a share of GDP, European Union membership, International Labour Organization membership, and World Trade Organization membership. See Appendix 2.8.1 for further information on the variables.

Table 2.34. Cross-sectional Determinants of Workers' Protection Index

	(1)	(2)	(3)	(4)	(5)	(6)
Estimation:	OLS	OLS	OLS	OLS	OLS	OLS
Dep var:	WPI	WPI	WPI	WPI	WPI	WPI
Share Mig	-0.027***	-0.018**	-0.018	-0.029***	-0.031***	-0.010
	(0.009)	(0.008)	(0.013)	(0.010)	(0.010)	(0.014)
Epid	0.386***	0.256***	0.358***	0.383***	0.394***	0.150
	(0.092)	(0.090)	(0.094)	(0.093)	(0.089)	(0.094)
$ln(GDP)$	0.332**	0.211	0.313	0.335**	0.361**	0.308
	(0.145)	(0.139)	(0.253)	(0.144)	(0.149)	(0.294)
Polity2	0.478*	0.548*	0.567*	0.551*	0.368	0.820**
	(0.272)	(0.293)	(0.287)	(0.309)	(0.271)	(0.364)
$ln(HC)$	-0.454	-0.340	-0.583	-0.509	-0.434	-0.677
	(0.296)	(0.306)	(0.472)	(0.329)	(0.278)	(0.472)
Common Law		-0.684***				-0.949***
		(0.233)				(0.266)
ctfp			-0.313			-0.457
			(0.717)			(0.786)
Trade/GDP				0.002		0.002
				(0.002)		(0.002)
unemp rate					-0.027	-0.003
					(0.019)	(0.025)
Adj. R-Square	0.32	0.42	0.25	0.32	0.31	0.40
Observations	70	70	61	70	68	59

Note: Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01. The dependent variable is the country average workers' protection index over the entire time span. As controls, we include logarithm of GDP per capita, the polity2 index, the logarithm of human capital, a dummy associated with country legal origin, the logarithm of productivity, trade as a share of GDP, and the share of unemployed workers. Our main variables of interest are the share of immigrants and the epidemiological effect. See 2.8.1 for further information on the variables.

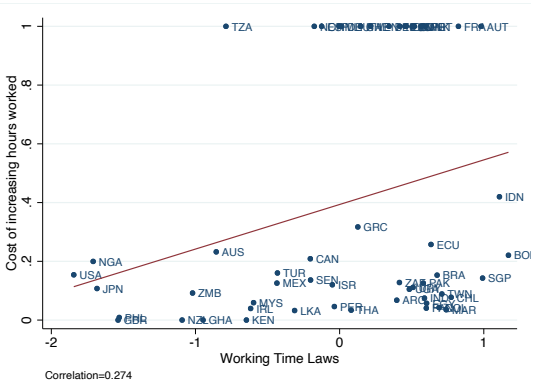
Table 2.35. WPI Subcomponents and Labor Market Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Estimation:	FE	FE	FE	FE	FE	FE	FE	FE
Time:	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010	1970-2010
Dep var:	Unempl	Labor Produc	Hours worked pc	Gini	Unempl	Labor Produc	Hours worked pc	Gini
Panel A								
$CollectAct_t$	-0.158	0.038	-25.701	-0.006				
	(0.335)	(1.626)	(18.836)	(0.004)				
$CollectAct_{t-1}$					0.376	-0.106	-26.358	-0.005
					(0.304)	(1.435)	(20.139)	(0.004)
Panel B								
$WkrRepr_t$	1.374*	0.752	-27.223*	-0.004				
	(0.704)	(1.218)	(15.984)	(0.004)				
$WkrRepr_{t-1}$					1.788**	0.633	-24.772*	-0.004
					(0.782)	(1.128)	(13.353)	(0.003)
Panel C								
$WkrDismis_t$	0.947	4.182**	-33.619*	-0.004				
	(0.719)	(1.995)	(17.996)	(0.005)				
$WkrDismis_{t-1}$					0.932*	4.103**	-27.838*	-0.001
					(0.525)	(1.712)	(15.891)	(0.005)
Panel D								
$WorkTime_t$	0.131	-0.295	18.508	-0.007				
	(0.718)	(1.328)	(18.508)	(0.004)				
$WorkTime_{t-1}$					-0.458	-0.007	13.557	-0.008*
					(0.604)	(1.096)	(17.743)	(0.004)
Panel E								
$EmptForm_t$	0.986**	4.266***	-31.317**	-0.004				
	(0.405)	(1.037)	(13.138)	(0.003)				
$EmptForm_{t-1}$					0.877**	3.373***	-22.104*	-0.002
					(0.382)	(1.024)	(11.788)	(0.004)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓	✓	✓
Countries	68	47	47	69	68	47	47	69
Observations	322	394	394	495	302	353	353	466

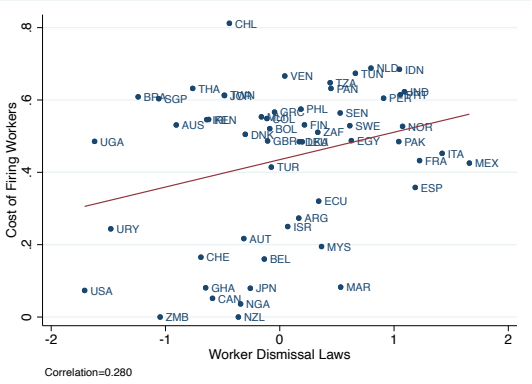
Note: Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01. The dependent variables are the unemployment rate, labor productivity per hours worked, annual hours worked per worker, and Gini index after tax. Each panel provides the estimates associated with the following variables of interest: industrial action laws ($IndAction$), worker representation laws ($WkrRepr$), dismissal laws ($WkrDismis$), working time laws ($WorkTime$), and employment forms laws ($EmptForm$). See Appendix 2.8.1 for further information on the variables.

Figure 2.11. Workers' Protection Index Subcomponents - Correlations with Botero et al. (2004)

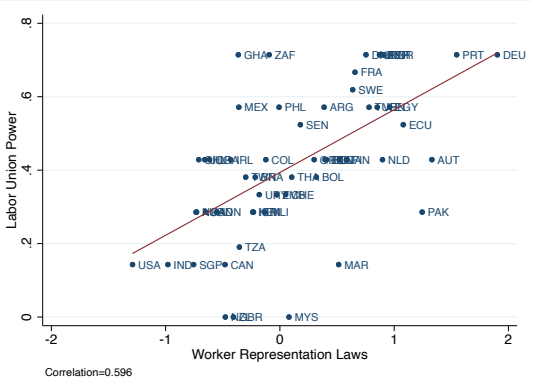
(a) Employment Forms Laws and Alternative Employment Contracts



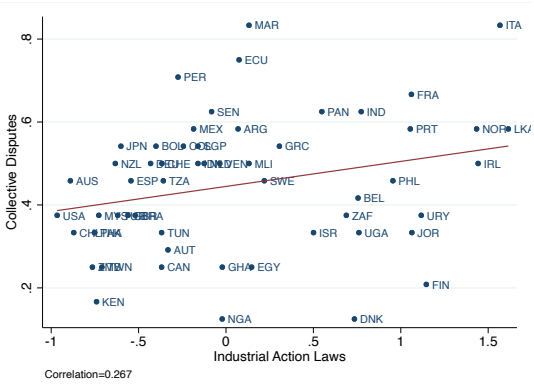
(b) Working Time Laws and Cost of increasing hours worked



(c) Worker Dismissal Laws and Cost of Firing Workers



(d) Worker Representation Laws and Labor Union Power



(e) Industrial Action Laws and Collective Disputes

Note: Authors' calculations on CBR Leximetric data (x-axis) and Botero et al. (2004) (y-axis). The figure plots the 1970–2010 average at the country level of the standardized workers' protection subcomponents on the related cross-sectional measures of workers' protection by Botero et al. (2004).

3

The Cost of Temporarily Suspending Schengen? Evidence From European Stock Markets

Co-authored with Jean-François Maystadt (UCLouvain)

Abstract

There is little systematic evidence on the economic cost of reintroducing border controls. We study the impact of temporary border controls on stock market returns. To do so, we first document a unique dataset on the reintroduction of border controls constructed from official documents. Then, we consider two complementary research designs: an event study exploiting the first refugee-induced border control in Germany in September 2015, and a difference-in-difference analysis covering all border controls in the Schengen area between 2006 and 2016. In both analyses, we find a negative and short-lived effect on daily stock returns. These results are confirmed across various sectors, causes of border controls, and firm sizes. The temporary reintroduction of border controls does not seem to have affected firms' expectations in a persistent manner.

3.1 Introduction and Context

Since the creation of the Schengen Area, freedom of movement and the removal of internal borders have been fundamental legal principles in the European Union (Barbero, 2020). The signing of the Schengen agreement on June 14^{th}, 1984, and its implementation agreement in June 1990 have facilitated the mobility of workers and consumers, as well as the transportation of goods across borders (Davis and Gift, 2014).[1] However, during the last decade, the EU and its member states have experienced frequent exceptions to the agreed-upon principles.

According to the Schengen Border Code Regulation, border controls can be temporarily reintroduced under three main circumstances. First is the case of "foreseeable events" (European Parliament and European Council (2016b), article 24) (e.g., international political summits or international sports competitions); second are circumstances of "a serious threat to public policy or internal security" (European Parliament and European Council (2016b), article 23); third is when Member States report "cases requiring urgent actions" (European Parliament and European Council 2016a, article 25). In any of these cases, Member States must provide their reasons for border controls to the EU Commission and other EU Member States. However, in the first case, Member States have to inform the EU about "the scope of the proposed reintroduction, specifying where border control is to be reintroduced; the names of the authorized crossing-points" (European Parliament and European Council (2016a), article 24 (1)). In the other two cases, Member States do not have to provide such detailed information and can leave neighboring countries with an unclear picture of the extent to which cross-border flow will be affected. For example, if a given Member State decides to reintroduce border controls by applying articles 23 and 25 of the Schengen Code, it means that the neighboring country faces disrupted freedom of movement for people, goods, and services (e.g., disrupted workforce commuting, traffic congestion). Controls usually appear in an unannounced and occasional manner. They are managed by the police and customs services, usually at the most active border crossing points.

Between June 2006 and June 2016, the reintroduction of border controls was announced 62 times; 56% of these were related to expected events, 35% to refugees, and 8% to terrorism-related events (see Appendix Figure 3.7). The expected events include various political summits and conferences (G8 summits, NATO summits, or UN conferences), international sports competition, or demonstrations. The refugee-related border controls started in September 2015 in Germany, and seven other Schengen countries — Austria, Slovenia, Hungary, Sweden, Norway, Denmark, and Belgium — followed in the next months. The explosion of Schengen suspensions was also related to terrorist attacks. The terrorist attacks in Paris (November 2015) and Brussels (March 2016) led several countries to re-establish border controls within the Schengen Area. Even though Schengen Member States had followed the mechanism foreseen in the Schengen Agreement in the past (Home, 2016), the increased geographical scope and duration of the restrictions led some analysts to portray the situation as the "dislocation," the "dismantling", or even the "death" of the Schengen Area (Sénat, 2016).

3.2 German Case Study

3.2.1 Data and Descriptive Statistics

The event we focus on took place on September 13^{th}, 2015, the day Germany became the first Schengen member to announce the reintroduction of border controls due to an alleged "big influx of persons seeking international protection". The announcement made late Sunday afternoon by German Interior Minister, Thomas de Maziere, was considered a "surprise move" (Karnitschnig, 2015). Patrolling by the German

[1]The Schengen Member States should not be confused with the European Union (EU) Member States. Most EU Member States belong to Schengen, together with few a non-EU countries. There are 26 Schengen Member States: Austria, Belgium, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Italy, Latvia, Liechtenstein, Lithuania, Luxembourg, Malta, Netherlands, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Sweden, and Switzerland.

police was immediately witnessed at the road crossing points with Austria, at 5:30pm on the same day (Harding, 2015).

Appendix Figure 3.3 illustrates the timeline of the event. One month prior, Germany had an open-door policy towards newcomers. They suspended the Dublin procedure for Syrian asylum-seekers, due to the wars in the Middle East, which meant that those migrants would no longer need to be sent back to the first EU country they entered. On August 31^{st}, 2015, German Chancellor Angela Merkel declared "We can do this" as Europe was confronting the refugee crisis. The German government granted protection to hundreds of thousands of refugees, and Merkel declared it a "national duty" to do so. On September 4^{th}, Germany and Austria started to take in asylum-seekers who were stuck at the Hungarian border (Appendix Figure 3.4). Germany had a welcoming culture, and it became the most desirable destination for asylum seekers in Europe. Only 9 days later, on September 13^{th}, the situation completely reversed politically and Germany began to strengthen controls on its border with Austria.

For our outcome, we use historical stock returns for all publicly listed German stocks from Eurofidai (Eurofidai, 2021). The returns are multiplied by 100 in order to be expressed as percentages. We remove the top and bottom 1% of all observed returns from our sample in order for our results not to be driven by possible measurement error in the outliers.[2]

3.2.2 Identification

To outline more clearly our identification strategy and the interpretation of our results, we first study the effect of a single major event: the German border controls implemented on September 13^{th}, 2015, due to the alleged "big influx of persons seeking international protection". Among all documented border controls, we argue that Germany provides an ideal case study to quantify the causal impact of border controls on stock market returns for several reasons. Germany was the first country to introduce border controls due to the refugee crisis. In addition, this was an unexpected reintroduction of border controls, which was implemented immediately on a Sunday, before the European stock markets re-opened. Furthermore, the presence of asylum-seekers is likely to have a negligible direct effect on stock markets, as opposed to other events such as terrorist attacks. Following Germany, on September 16^{th} Austria became the next country to notify the European Commission of the reintroduction of border controls, due to the same alleged "big influx of persons seeking international protection". Therefore, the first two trading days of the week, Monday 14^{th} and Tuesday 15^{th}, are of interest particularly as they are not contaminated by spillover effects from neighboring countries.

We assess the impact of the reintroduction of border controls on daily stock returns within a constant mean return model (MacKinlay, 1997). Our baseline model is structured as follows:

$$R_{it} = \beta EventDay1_t + \alpha_i + X_t'\gamma + \epsilon_{it}, \tag{3.1}$$

where the outcome R_{it} denotes the average daily stock market return for a given security i at day t. $EventDay1_t$ is a dummy equal to one for the first trading day when the event occurred (Monday, September 14^{th}). α_i stands for security fixed effects. X_t' captures day-of-the-week fixed effects. Standard errors are clustered at the security level. We restrict our estimation window to two months prior to the announcement of the German reintroduction of border controls and one month after the announcement.[3] We exploit information on 7,119 securities traded by German companies.

The use of fixed effects makes our estimation model equivalent to the constant mean return model in the event study literature, as we are effectively demeaning returns at the security level, akin to abnormal returns.[4] However, event study in a two-way fixed effects setup does not necessarily capture the average

[2] In Appendix Figure 3.2, we plot the distribution of returns for Germany. Our main results are similar if we use different trimming or winsorizing cut-off points.

[3] Results are shown to be similar when we define an estimation window over alternative two- and three-month periods.

[4] We cannot apply the so-called market model, where the variation in market returns is removed from the security return, since our treatment is defined at the country level and not the security level.

treatment effect (De Chaisemartin and d'Haultfoeuille, 2020; Gardner, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2020). The estimates of fixed effects in Eq. 3.1 can be biased if the effects are heterogeneous but assumed to be homogeneous. For example, if the effects of border control are stronger in first time periods, but the estimated impact of border control is assumed to be constant, then the day-of-the-week fixed effects in the first periods will be biased since we assume a smaller treatment effect than the reality. To address the above-mentioned concerns, we follow Gardner (2021) and apply the proposed two-step approach. In the first stage, we estimate all coefficients apart from the event study coefficients, for all non-treated data. More specifically, we estimate the following first stage:

$$R_{it} = \alpha_i + X_t'\gamma + \nu_{it}, \tag{3.2}$$

where the sample is restricted to countries that have not yet reintroduced border controls. Therefore, the estimates in the first stage are not contaminated by the effects of border controls, as discussed above. In the second stage, we estimate the following equation:

$$R_{it} = \beta EventDay1_t + \widehat{\alpha_i} + X_t'\widehat{\gamma} + \epsilon_{it}, \tag{3.3}$$

where all the parameters, $\widehat{\alpha_i}$, $X_{i,t}'\widehat{\gamma}$, are estimates from Eq.3.2. We cluster the standard errors at the security level.

3.2.3 Results

The results for the German case study are presented in Table 3.1. Panel A focuses on daily stock returns, while Panel B focuses on stock return volatility. Column (1) shows that the reintroduction of border controls in Germany decreases daily returns by 0.25 percentage points. Compared to major events such as a civil war (Guidolin and La Ferrara, 2007, 2010), the Brexit referendum (Oehler et al., 2017), or US-backed military coups (Dube et al., 2011), the magnitude seems fairly small. However, it is about six times larger than the average daily return (–0.04) over the period of investigation.[5] Columns (2) to (4) demonstrate that the impact is short-lived, however. While the reintroduction of border controls on the Sunday still had some negative effects on the second trading day (Tuesday), the financial markets corrected for the initial dip on the third (Wednesday) and fourth trading days (Thursday). Consistent with daily returns, the market increased in volatility also for only the first two days only, as shown in Panel B. The initial short-lived reaction of the stock market can be interpreted as the markets internalizing border controls as a path towards the so-called "death" of the Schengen Area.

In Table 3.2, we explore the heterogeneity of the effect across securities by looking at the industries in which they operate and their size. Panel A focuses on daily stock returns, while Panel B focuses on stock return volatility. Overall, we find that the transport, materials, industrials, discretionary, and staples sectors saw a significant fall on the first trading day after the event.[6] In terms of firm size, the impact of the reintroduction of border controls is mostly concentrated among securities with medium or large valuations (this is consistent with the consensus that large firms are more likely to be involved in cross-border activities (Bernard et al., 2007, 2018)). Contrary to the previous findings regarding the aggregate effects, the heterogeneous effects are less pronounced when looking at volatility across industries and firm sizes (Panel B).

[5] Descriptive statistics are provided in Appendix Table 3.5. Although lower in magnitude, similar results are found using a classical two-way fixed-effects estimator (see Appendix Table 3.6). Our results are qualitatively unaltered when we modify the estimation window to 2 or 3 months before or after the event (Appendix Tables 3.7 and 3.8) or when we use alternative outcomes or winsorizing cut-off points (Appendix Table 3.9).

[6] In Appendix Table 3.10 we test all the remaining sectors, and we additionally find a significant decrease in the energy, financials, and IT sectors, whereas, we find no effect for the telecommunications sector.

Table 3.1. The Effect of Border Controls in Germany on Daily Returns and Volatility

	(1)	(2)	(3)	(4)
Dependent Var:	Panel A: Daily Returns			
EventDay1Mon	-0.250***	-0.241***	-0.234***	-0.238***
	(0.027)	(0.025)	(0.025)	(0.025)
EventDay2Tue		-0.104***	-0.098***	-0.101***
		(0.027)	(0.027)	(0.027)
EventDay3Wed			0.275***	0.273***
			(0.027)	(0.026)
EventDay4Thu				0.187***
				(0.027)
Securities	7,119	7,119	7,119	7,119
Observations	356,841	356,841	356,841	356,841
Tbeg	13jul2015	13jul2015	13jul2015	13jul2015
Tend	13oct2015	13oct2015	13oct2015	13oct2015
SecurityFE	Yes	Yes	Yes	Yes
DayOfWeekFE	Yes	Yes	Yes	Yes
Dependent Var:	Panel B: Volatility			
EventDay1Mon	0.082***	0.082***	0.082***	0.082***
	(0.015)	(0.015)	(0.015)	(0.015)
EventDay2Tue		0.032***	0.032***	0.032***
		(0.005)	(0.005)	(0.005)
EventDay3Wed			-0.043***	-0.043***
			(0.007)	(0.007)
EventDay4Thu				-0.011
				(0.008)
Securities	5,808	5,808	5,808	5,808
Observations	27,816	27,816	27,816	27,816
Tbeg	14sep2015	14sep2015	14sep2015	14sep2015
Tend	18sep2015	18sep2015	18sep2015	18sep2015
SecurityFE	Yes	Yes	Yes	Yes
DayOfWeekFE	Yes	Yes	Yes	Yes

Note: The table reports the effect of the reintroduction of border controls in Germany in the first 4 trading days on daily stock returns (Panel A), and its 5-day volatility (Panel B). It estimates Eq. 3.1 using a two-stage event study approach. The units of analysis are securities. Standard errors in parentheses are clustered at the security level. * p<0.1, ** p<0.05, *** p<0.01.

3.3 Schengen Area Analysis

3.3.1 Data and Descriptive Statistics

Since all reintroductions of border controls have to be reported to the Commission (or potentially, by the Commission to the Member States), we document the universe of all temporary reintroductions of border controls at internal borders from official Commission documents (Commission, 2019). We firstly encode the duration of each temporary border control event indicated by a Member State. Afterwards, we classify each event according to the reasons given by each Member State for why the borders were reintroduced: due to the refugee crisis, a terrorist attack, or foreseeable events (G7 or NATO meetings, visits by the Pope, demonstrations, etc.).

In Appendix Figure 3.6 we depict the frequency and duration of all 117 temporary border controls for each Schengen Member State, from the first occurrence in October 2006 until June 2016. Overall, 19 out of 26 Schengen Member States instated border controls at least once during our period of investigation. In terms of frequency, Hungary, Latvia, the Netherlands, Portugal, and Slovenia reintroduced border controls only once. At the other extreme, France did it 11 times between June 2006 and June 2016.[7] As for the duration, border controls can be reintroduced for one day up to 6 months. For instance,

[7]See Appendix Figure 3.7.

Table 3.2. Border-Control Effect Heterogeneity by Industry and Firm Size

	Industry					Firm Size		
Sub-sample	Transport	Materials	Industrials	Discretionary	Staples	Small Cap	Medium Cap	Large Cap
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Var:	Panel A: Daily Returns							
*EventDay*1*Mon*	-0.455***	-0.264***	-0.377***	-0.261***	-0.338***	0.046	-0.492***	-0.315***
	(0.118)	(0.070)	(0.056)	(0.089)	(0.079)	(0.058)	(0.050)	(0.029)
Securities	234	686	1,602	692	390	1,983	2,039	2,091
Observations	11,254	33,145	78,601	34,154	20,177	106,592	106,575	106,583
Tbeg	13jul2015	13jul2015	13jul2015	13jul2015	13jul2015	13jul2015	13jul2015	13jul2015
Tend	13oct2015	13oct2015	13oct2015	13oct2015	13oct2015	13oct2015	13oct2015	13oct2015
SecurityFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DayOfWeekFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Var:	Panel B: Volatility							
*EventDay*1*Mon*	0.001	0.116***	0.049	0.049	-0.090**	0.181***	0.110***	-0.012
	(0.067)	(0.032)	(0.030)	(0.045)	(0.046)	(0.032)	(0.024)	(0.025)
Securities	180	522	1,273	561	328	1,719	1,698	1,716
Observations	861	2,515	6,080	2,651	1,590	8,098	8,098	8,097
Tbeg	14sep2015	14sep2015	14sep2015	14sep2015	14sep2015	14sep2015	14sep2015	14sep2015
Tend	18sep2015	18sep2015	18sep2015	18sep2015	18sep2015	18sep2015	18sep2015	18sep2015
SecurityFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DayOfWeekFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table reports heterogeneous effects of border controls on daily stock returns by industry and firm size (Panel A) and its 5-day volatility (Panel B). We follow the Eurofidai (2021) classification, where for each company the information about its industry sector is provided. The transport industry consists of companies conducting activities connected to traffic and transportation (column 1). The materials industry refers to commodity chemicals, agricultural chemicals, construction materials, paper, and forest products (column 2). The industrials industry is composed of capital goods, commercial and professional services and transportation (column 3). The discretionary industry is composed of automobiles and components, consumer durables, apparel, consumer services, and media (column 4). The staples industry includes food, beverages, tobacco, food and staples retailing, and household and personal products (column 5). Firm size is calculated by classifying all the companies into 3 quantiles based on their market capitalization. The results are estimated using Eq. 3.1, with a two-stage event study approach. The units of analysis are securities. Standard errors in parentheses are clustered at the security level. * p<0.1, ** p<0.05, *** p<0.01.

the reintroduction of border controls due to demonstrations by young Basques in Bayonne, France, on October 21st, 2006, lasted for 1 day. The longest controls were related to the refugee crisis and lasted around 6 months in Germany and Austria and 5 months in Norway and Sweden. The longest border control related to a terrorist attack occurred in France in December 2015 and lasted for over 5 months (165 days).

Similarly to the German case study, we use historical stock returns from Eurofidai for all European stocks in the Schengen area (Eurofidai, 2021). The returns are multiplied by 100 in order to be expressed as percentages. We remove the top and bottom 1% of all observed returns from our sample in order for our results not to be driven by possible measurement error in the outliers.[8] In order to avoid possible confounding variables as a result of Brexit, we restrict our data collection from June 1st, 2006, to June 1st, 2016, since the Brexit referendum took place on June 23rd, 2016. Finally, we also use additional data to deal with identification threats, namely, data on asylum seekers from Eurostat (Eurostat, 2021) and on terrorist attacks from the Global Terrorism Database (GTD, 2021).

[8]In Appendix Figure 3.5, we plot the distribution of returns for the whole Schengen area. Our main results are similar if we use different trimming or winsorizing cut-off points.

3.3.2 Identification

To assess the external validity of the results established for the German case study, we estimate a difference-in-difference strategy for all reintroductions of border controls in the Schengen area from 2006 to 2016. Our previous model in Eq. 3.1 is further enriched by the country dimension, as follows:

$$R_{ct} = \beta Events_{ct} + \alpha_c + \delta_t + X'_{ct}\gamma + \epsilon_{ct} \tag{3.4}$$

where the outcome R_{ct} denotes the capitalization-weighted stock market return in country c at day t. $Events_{c,t}$ is our variable of interest, and is equal to one if a country reintroduced border controls. α_c and δ_t designate country and day fixed effects, respectively. $X'_{c,t}$ is a vector of controls variables, such as the log number of asylum applications and the occurrence of terrorist attacks. Standard errors are clustered at the country level.

In order for our difference-in-difference results to be interpreted as causal, we need to deal with a number of challenges. The first challenge is related to the parallel trends assumption. Second, we ought to investigate whether some border controls are plausibly exogenous and whether there are confounding factors simultaneously affecting the reintroduction of border controls and daily returns. Third, our estimates may be affected by spillover effects endured by firms whose business take places in a "bordering" country, for which the circulation of goods and people is indirectly impacted. Lastly, possible anticipation effects, particularly for expected events, can lead to an attenuation bias of the estimated coefficient.

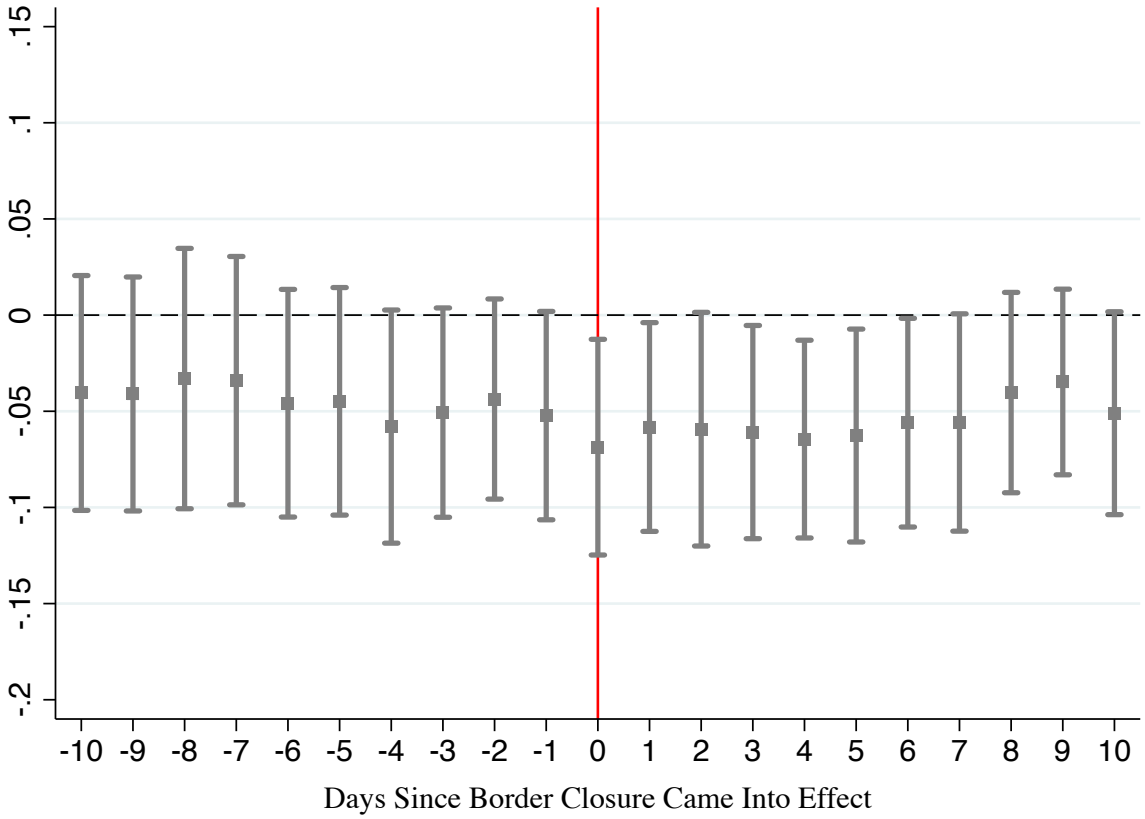
The core identification assumption of our difference-in-difference design requires conditional parallel trends (conditional on country fixed effects, day fixed effects, and the additional controls). The average stock return among countries with border controls (treated) should have followed parallel trends compared to countries without border controls (control), absent of border controls. A potential violation of our identification assumption would occur if the treatment group experienced an additional stock return decrease contemporaneous with the border controls (such as the direct effect of the terrorist attacks). In the case of the refugee-induced border controls, this is not a threat to identification as the flow of asylum-seekers did not appear suddenly, but rather, as illustrated in the German case study, it happened progressively over time (Appendix Figure 3.4). Additionally, in Figure 3.1 we show that there are no significant pre-trends.

We next turn to the questions of treatment exogeneity and omitted-variable bias. We have classified three main causes of border controls: refugees, terrorists attacks, and expected events. Refugee-induced border controls can be argued to not be fully exogenous, as countries could predict or observe large flows of asylum-seekers in neighboring countries. In other words, the higher the number of refugees entering a country, the higher the probability of reintroducing border controls. To deal with this potential source of bias, we control for the number of asylum applications in a given country. We compile data at the country and month level from Eurostat (Eurostat, 2021).

For border controls that are induced by terrorist attacks, we use the Global Terrorism Database. We construct an indicator capturing the exact date of a terrorist attack. The actual incidence of a terrorist attack clearly confounds both the Schengen suspension and the stock market returns. It is well established in the literature that terrorist attacks affect financial markets' expectations (Papakyriakou et al., 2019).

Beyond the use of controls, we keep track of the three different causes invoked for temporarily suspending the Schengen agreement. Controlling for the direct impact, events induced by terrorist attacks are the most unexpected. Likewise, the first refugee-induced border controls were also unexpected. On the one the hand, border controls that are induced by expected events are the least exogenous as they are usually planned long in advance. Examples of expected events are the high-level governmental meetings of the G7 or NATO, or other large events (European Football Championship, Tour de France). We should not expect much of a market reaction to expected events as they are already priced into stock prices long before the reintroduction of border controls. Finally, the role of spillover effects is investigated by considering the impact of the reintroduction of border controls by a neighboring country.

Figure 3.1. Event Study Estimates of Border Controls Within The Schengen Area - Lags and Leads



Note: The figure reports lags and leads of Eq. 3.4 using two-stage difference-in-difference estimates (and their 95% confidence intervals). The dependent variable is the capitalization-weighted daily return. The controls include the log number of asylum applications, the occurrence of a terrorist attack, country, and day fixed effects. The units of analysis are countries. Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01.

Given the variation in the treatment timing, we are concerned about the shortcomings of estimating Eq. 3.4 with a standard difference-in-difference estimation. We cannot exclude that the treatment effect is heterogeneous across groups and time, in which the standard difference-in-difference estimator will not necessarily capture the average treatment effect (De Chaisemartin and d'Haultfoeuille, 2020; Gardner, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2020). Therefore, similarly to the German case study, we follow Gardner (2021) and adopt a two-stage difference-in-difference method.

3.3.3 Results

Table 3.3 presents the results of the Schengen-wide analysis. Panel A focuses on daily stock returns, while Panel B focuses on stock return volatility. Exploiting within-country variation, column (1) shows a significant and negative average impact of the reintroduction of border controls between June 2006 and June 2016. Border controls related to terrorist attacks (col. 3) and refugee inflows (col. 2) have the strongest impact, in this order. As expected, foreseeable events have no significant effect on daily returns since they are already priced in before they occur (col. 4). Economically, we find that the reintroduction of refugee-induced border controls reduces daily returns by 0.06 percentage points, on average. This magnitude is slightly below the mean daily return (0.08).[9]

The fact that the magnitude is far below that found in the German case study suggests that financial markets do not interpret the various border controls as a slow path towards the "death" of the Schengen agreement. Another possible explanation is that all firms within the Schengen area interpret any

[9]Descriptive statistics are provided in Appendix Table 3.11.

reintroduction of border controls in the same way in terms of future expectations. If this were the case, control securities assumed to offer a good counterfactual would be contaminated. We do not believe this explanation to be plausible, given results presented in Appendix Table 3.12.[10] On the contrary, similar to the German case study, financial markets seem to correct themselves after the initial fall in daily returns. As shown in Table 3.4, the effect is significantly negative only for the first time refugee-induced border controls. For subsequent events, financial markets correct themselves after the initial decline. In terms of volatility, similarly to the German case study, stock return volatility increases significantly following the reintroduction of border controls in the Schengen area (Panel B of Table 3.3).

Table 3.3. The Effect of Border Controls in the Schengen Area on Daily Returns and Volatility

	(1)	(2)	(3)	(4)	(5)
Dependent Var:	Panel A: Daily Returns				
All Events	-0.069**				
	(0.029)				
Refugees		-0.055***			-0.053***
		(0.011)			(0.012)
Terrorist Attacks			-0.124***		-0.126***
			(0.028)		(0.028)
Expected Events				-0.073	-0.073
				(0.114)	(0.113)
Countries	26	26	26	26	26
Observations	65,396	65,396	65,396	65,396	65,396
Tbeg	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006
Tend	01jun2016	01jun2016	01jun2016	01jun2016	01jun2016
CountryFE	Yes	Yes	Yes	Yes	Yes
DayFE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	No	Yes
Dependent Var:	Panel B: Volatility				
All Events	0.237***				
	(0.058)				
Refugees		0.250***			0.247***
		(0.076)			(0.073)
Terrorist Attacks			0.431***		0.427***
			(0.120)		(0.124)
Expected Events				0.090	0.078
				(0.068)	(0.071)
Countries	26	26	26	26	26
Observations	93,633	93,633	93,633	93,633	93,633
Tbeg	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006
Tend	31may2016	31may2016	31may2016	31may2016	31may2016
CountryFE	Yes	Yes	Yes	Yes	Yes
DayFE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	No	Yes

Note: The table reports the effects of different types of border controls: all types (column 1), refugee-related (column 2), terrorism-related (column 3), and expected-events-related (column 4). The dependent variable is the capitalization-weighted daily stock return (Panel A), and its 5-day volatility (Panel B). The estimates are computed using Eq. 3.4 with a two-stage difference-in-difference approach. Controls include the log number of asylum applications for the refugee-related variables, and the occurrence of a terrorist attack for the terrorism-related variables. The units of analysis are countries. Standard errors in parentheses are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01.

We conduct a battery of robustness checks. Results are broadly robust to the use of the standard

[10] Additionally including securities from twelve European countries that border the Schengen area (Bosnia and Herzegovina, Bulgaria, Croatia, Cyprus, Ireland, Macedonia, Romania, Russia, Serbia, Turkey, Ukraine, and the United Kingdom), we augment Eq. 3.4 with an indicator equal to 1 when there is a reintroduction of border controls anywhere in the Schengen area and interact it with whether the country is a Schengen Member State. If general equilibrium or contamination effects exist, we would find that the markets to react to an event wherever it takes place. We test for this in Appendix Table 3.12, and we do not find such effects.

Table 3.4. Cumulative Effect of Border Controls in The Schengen Area

Dependent Var:	Daily Returns							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Event 1st Shock	-0.021	-0.021	-0.021					
	(0.147)	(0.147)	(0.147)					
Event 2nd Shock		-0.152***	-0.152***					
		(0.054)	(0.054)					
Event 3rd Shock			0.034					
			(0.029)					
Refugees 1st Shock				-0.276***	-0.276***	-0.276***		
				(0.087)	(0.087)	(0.087)		
Refugees 2nd Shock					0.088**	0.088**		
					(0.039)	(0.039)		
Refugees 3rd Shock						0.243**		
						(0.112)		
Terrorist Attack 1st Shock							-0.105***	-0.103***
							(0.020)	(0.021)
Terrorist Attack 2nd Shock								-0.332***
								(0.007)
Countries	26	26	26	26	26	26	26	26
Observations	65,396	65,396	65,396	65,396	65,396	65,396	65,396	65,396
Tbeg	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006
Tend	01jun2016	01jun2016	01jun2016	01jun2016	01jun2016	01jun2016	01jun2016	01jun2016
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DayFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table reports estimates for countries that experienced a given type of border control for the first time (columns 1, 4, and 7), for the second time a given type of border control (columns 2, 5, and 8), and for the third time a given type of border control (columns 3, and 6). The dependent variable is the capitalization-weighted daily stock return. The estimates are computed using Eq. 3.4 with a two-stage difference-in-difference approach. Controls include the log number of asylum applications for the refugee-related variables, and the occurrence of a terrorist attack for the terrorism-related variables. The unit of analysis are countries. Standard errors in parentheses are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01.

difference-in-difference with two-way fixed effects setup (Appendix Table 3.13) or to gradually dropping initial years of our sample from June 1^{st}, 2006, up to June 1^{st}, 2014 (Appendix Figure 3.8). More fundamentally, spillover events — border controls reintroduced by a neighboring country — have similar effects on daily returns and do not alter our main results (Appendix Figure 3.9). Our results are also similar when removing additional controls from the baseline specification: the number of asylum applications or the occurrence of a terrorist attack (Appendix Table 3.14), redefining volatility periods from 2 to 6 days (Appendix Table 3.15), using alternative measures of daily returns (Appendix Table 3.16), or using a different cut-off point for winsorizing (Appendix Table 3.17). Although we lose precision, when stratifying the sample by industry the only negative effect is observed in the financial sector, with increased volatility observed across the majority of sectors (Appendix Table 3.18). Results are confirmed only for an increased volatility in large-valuation firms (Appendix Table 3.19).

3.4 Conclusions

Financial markets react to the introduction of unexpected border controls, but the effect on daily returns is short-lived. Firms do not seem to interpret the reintroduction of border controls as a sign of the dislocation of the Schengen agreement. However, our study faces two main limits in further interpreting our results from a policy perspective. First, the results do not imply that financial markets do not value *per se* the free circulation of people or goods offered by the Schengen agreement. We are investigating the impact of temporary suspensions of the Schengen agreement, not its permanent disintegration. Second, we cannot exclude the possibility that financial markets are not very good at interpreting the reintroduction of border controls as a signal regarding the viability of a political agreement. Further

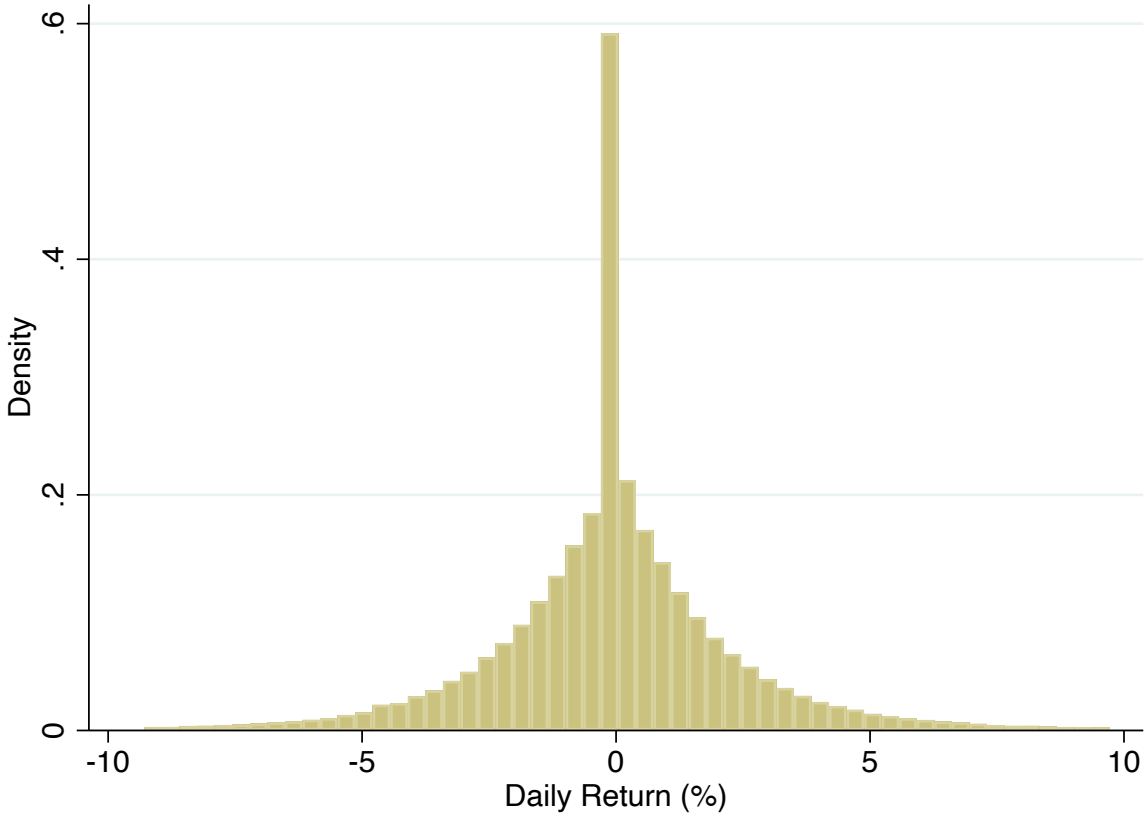
research should certainly aim to disentangle the immediate effect of border controls on transaction costs from the changes in firms' expectations.

3.5 Appendix

3.5.1 German Case Study

Descriptive Statistics

Figure 3.2. Distribution of the Daily Stock Market Returns



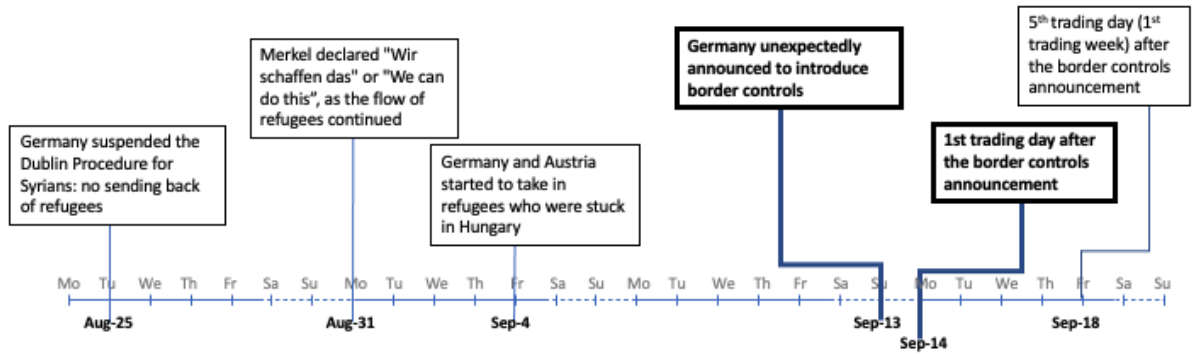
Note: the plot shows the distribution for all daily stock market returns in Germany between July13th2015 and October13th2015, after trimming top and bottom 1% from the raw data.

Table 3.5. Summary Statistics

Variable	Mean	S.D.	Min.	Max.	Obs.
Daily Returns	-0.042	2.370	-9.29	9.72	356841
Average 5-day Volatility	1.674	1.400	0.00	12.86	26777
Small Cap Returns	-0.069	2.487	-9.28	9.72	106592
Medium Cap Returns	-0.008	2.353	-9.29	9.72	106575
Large Cap Returns	-0.048	2.193	-9.27	9.72	106583
Transport Sector Returns	0.031	2.183	-9.25	9.71	11254
Materials Sector Returns	-0.105	2.204	-9.29	9.66	33145
Industrials Sector Returns	-0.066	2.426	-9.28	9.72	78601
Discretionary Sector Returns	-0.018	2.382	-9.28	9.71	34154
Staples Sector Returns	-0.037	2.106	-9.23	9.71	20177

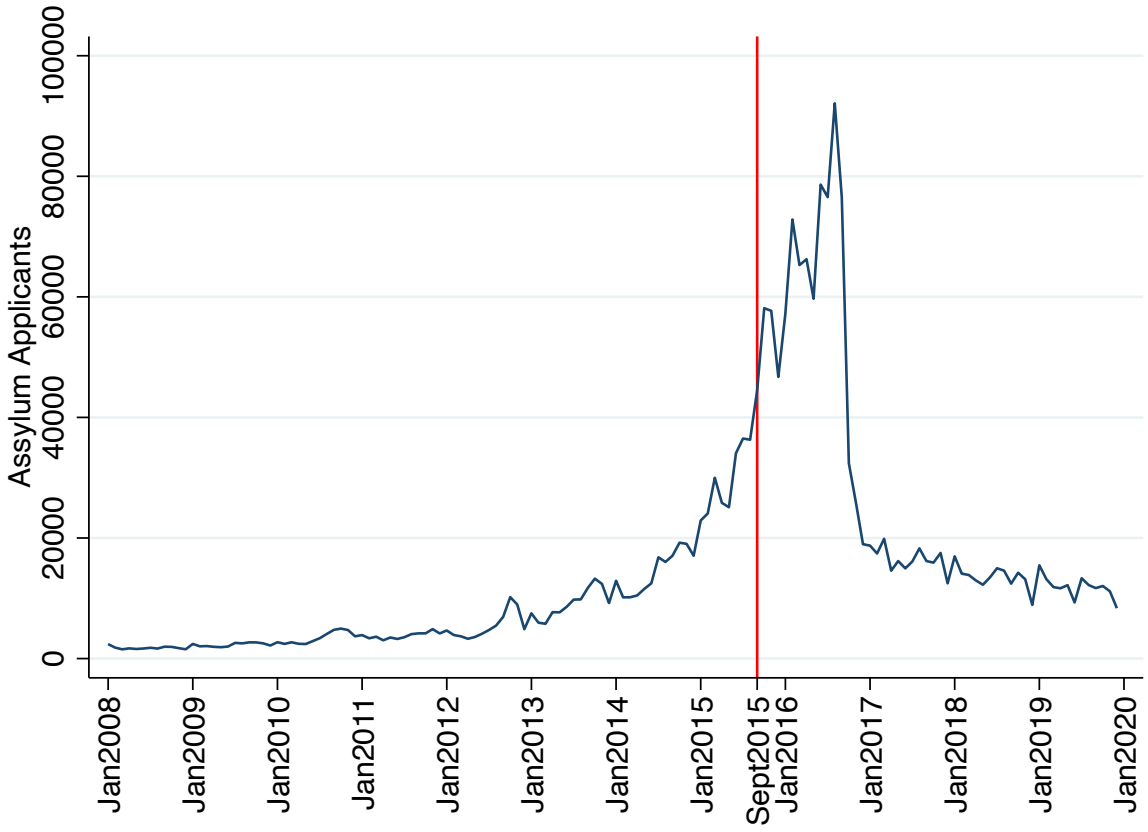
Note: the table shows summary statistics for the baseline sample.

Figure 3.3. Timeline of The First Refugee Induced Border Controls



Note: the plot shows the timeline from August to September 2015 of the refugee crisis related events in Germany and the reintroduction of border controls. Additionally, the plot shows the day of the week to track the opening of the stock exchange around the event date.

Figure 3.4. Monthly Number of Asylum Applicants in Germany



Note: the plot shows the number of asylum applicants in Germany using Eurostat data (Eurostat, 2021)

Robustness

Table 3.6. Baseline Results Using a Standard Two-Way Fixed Effects Estimator

Dependant Var:	Daily Returns			
	(1)	(2)	(3)	(4)
$EventDay1Mon$	-0.148***	-0.148***	-0.148***	-0.148***
	(0.027)	(0.027)	(0.027)	(0.027)
$EventDay2Tue$		-0.117***	-0.117***	-0.117***
		(0.028)	(0.028)	(0.028)
$EventDay3Wed$			0.180***	0.180***
			(0.028)	(0.028)
$EventDay4Thu$				0.043
				(0.028)
Securities	6,860	6,860	6,860	6,860
Observations	356,582	356,582	356,582	356,582
Tbeg	13jul2015	13jul2015	13jul2015	13jul2015
Tend	13oct2015	13oct2015	13oct2015	13oct2015
SecurityFE	Yes	Yes	Yes	Yes
DayOfWeekFE	Yes	Yes	Yes	Yes

Note: The table reports the effect of the reintroduction of border controls in Germany in the first 4 trading days on daily stock returns. It estimates Eq. 3.1 using a standard two-way fixed effects estimator. The units of analysis are securities. Standard errors in parentheses are clustered at the security level. * p<0.1, ** p<0.05, *** p<0.01.

Table 3.7. Alternative Estimation Window - 2 Months Before and 2 Months After the Event

Dependant Var:	Daily Returns			
	(1)	(2)	(3)	(4)
$EventDay1Mon$	-0.269***	-0.263***	-0.257***	-0.260***
	(0.026)	(0.025)	(0.025)	(0.025)
$EventDay2Tue$		-0.136***	-0.131***	-0.133***
		(0.027)	(0.027)	(0.027)
$EventDay3Wed$			0.232***	0.230***
			(0.027)	(0.026)
$EventDay4Thu$				0.133***
				(0.027)
Securities	7,397	7,397	7,397	7,397
Observations	486,110	486,110	486,110	486,110
Tbeg	13jul2015	13jul2015	13jul2015	13jul2015
Tend	13nov2015	13nov2015	13nov2015	13nov2015
SecurityFE	Yes	Yes	Yes	Yes
DayOfWeekFE	Yes	Yes	Yes	Yes

Note: The table reports the effect of the reintroduction of border controls in Germany in the first 4 trading days on daily stock returns with a different estimation window. The estimation window used is 2 months before and 2 months after the event. It estimates Eq. 3.1 using a two-stage event study approach. The unit of analysis are securities. Standard errors are clustered at the security level. * p<0.1, ** p<0.05, *** p<0.01.

Table 3.8. Alternative Estimation Window - 3 Months Before and 2 Months After the Event

Dependant Var:	Daily Returns			
	(1)	(2)	(3)	(4)
$EventDay1Mon$	-0.190***	-0.186***	-0.182***	-0.185***
	(0.026)	(0.025)	(0.025)	(0.025)
$EventDay2Tue$		-0.096***	-0.092***	-0.094***
		(0.027)	(0.027)	(0.027)
$EventDay3Wed$			0.235***	0.233***
			(0.027)	(0.026)
$EventDay4Thu$				0.100***
				(0.027)
Securities	7,649	7,649	7,649	7,649
Observations	594,946	594,946	594,946	594,946
Tbeg	15jun2015	15jun2015	15jun2015	15jun2015
Tend	13nov2015	13nov2015	13nov2015	13nov2015
SecurityFE	Yes	Yes	Yes	Yes
DayOfWeekFE	Yes	Yes	Yes	Yes

Note: The table reports the effect of the reintroduction of border controls in Germany in the first 4 trading days on daily stock returns with a different estimation window. The estimation window used is 3 months before and 2 months after the event. It estimates Eq. 3.1 using a two-stage event study approach. The unit of analysis are securities. Standard errors are clustered at the security level. * p<0.1, ** p<0.05, *** p<0.01.

Table 3.9. Alternative Outcomes and Winsorizing

Dependant Var:	3-day Volatility	Cap Daily	Cap Yearly	Winsorizing 1%	Winsorizing 5%
	(1)	(2)	(3)	(4)	(5)
$EventDay1Mon$	0.168***	-0.315***	-0.319***	-0.206***	-0.217***
	(0.022)	(0.000)	(0.000)	(0.029)	(0.023)
Securities	5,653	9,882	9,882	7,150	7,150
Observations	16,444	606,835	606,835	364,010	364,010
Tbeg	14sep2015	13jul2015	13jul2015	13jul2015	13jul2015
Tend	16sep2015	13oct2015	13oct2015	13oct2015	13oct2015
SecurityFE	Yes	Yes	Yes	Yes	Yes
DayOfWeekFE	Yes	Yes	Yes	Yes	Yes

Note: The table reports the effect of the reintroduction of border controls in Germany on alternative outcomes and winsorized daily stock returns. Column 1 reports the 3-day average volatility of daily returns. Column 2 reports daily returns weighted with the contemporaneous capitalization rate. Column 3 reports daily returns weighted with the capitalization rate from the last trading day in the previous year. Columns 4 and 5 winsorize top and bottom 1% and 5% of daily returns accordingly. The results estimate Eq. 3.1 using a two-stage event study approach. The unit of analysis are securities. Standard errors are clustered at the security level. * p<0.1, ** p<0.05, *** p<0.01.

Heterogeneity

Table 3.10. Border Control Effect Heterogeneity by Industries

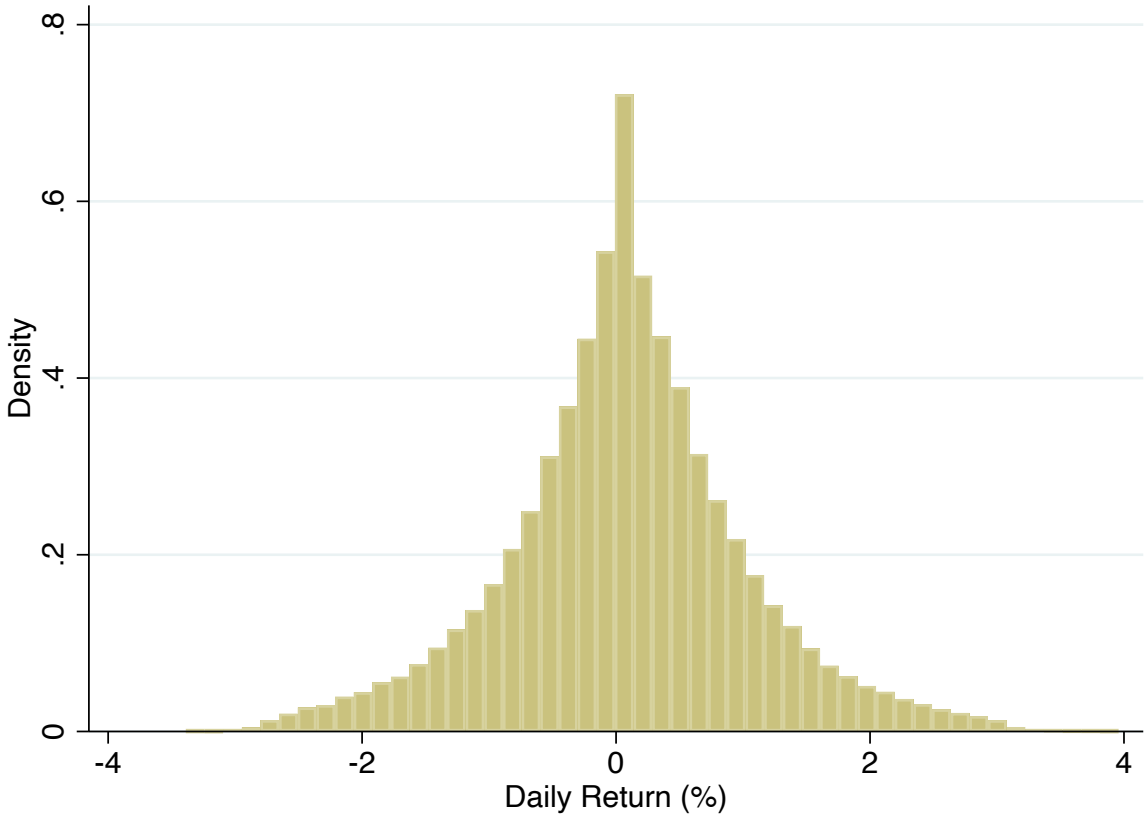
Sub-sample:	Energy	Financials	IT	Telecommunication
	(1)	(2)	(3)	(4)
Dependant Var:	Panel A: Daily Returns			
$EventDay1Mon$	-0.506***	-0.262***	-0.209***	0.128
	(0.184)	(0.055)	(0.073)	(0.213)
Securities	40	1,311	1,058	222
Observations	2,109	69,332	53,618	11,094
Tbeg	13jul2015	13jul2015	13jul2015	13jul2015
Tend	13oct2015	13oct2015	13oct2015	13oct2015
SecurityFE	Yes	Yes	Yes	Yes
DayOfWeekFE	Yes	Yes	Yes	Yes
Dependant Var:	Panel B: Volatility			
$EventDay1Mon$	0.211***	0.286***	0.008	0.316***
	(0.068)	(0.030)	(0.043)	(0.094)
Securities	33	1,129	889	182
Observations	164	5,452	4,225	876
Tbeg	14sep2015	14sep2015	14sep2015	14sep2015
Tend	18sep2015	18sep2015	18sep2015	18sep2015
SecurityFE	Yes	Yes	Yes	Yes
DayOfWeekFE	Yes	Yes	Yes	Yes

Note: The table reports heterogenous effects by industry of the reintroduction of border controls in Germany on daily stock returns (Panel A), and its 5-day volatility (Panel B). We follow the Eurofidai (2021) classification, where for each company, the information about its industry sector is provided. The energy industry consists of petroleum/oil and natural gas firms (column 1). Financials sectors is composed of, real estate, banks and other credit institutions, mortgage and funding institutions, and insurance companies (column 2). The IT industry is related to electronics and semiconductors, internet, software and IT services, computer hardware and networking (column 3). Telecommunication industry consists of telecommunication related services (column 4). It estimates Eq. 3.1 using a two-stage event study approach. The units of analysis are securities. Standard errors in parentheses are clustered at the security level. * p<0.1, ** p<0.05, *** p<0.01.

3.5.2 Schengen Area Analysis

Descriptive Statistics

Figure 3.5. Distribution of the Daily Stock Market Returns



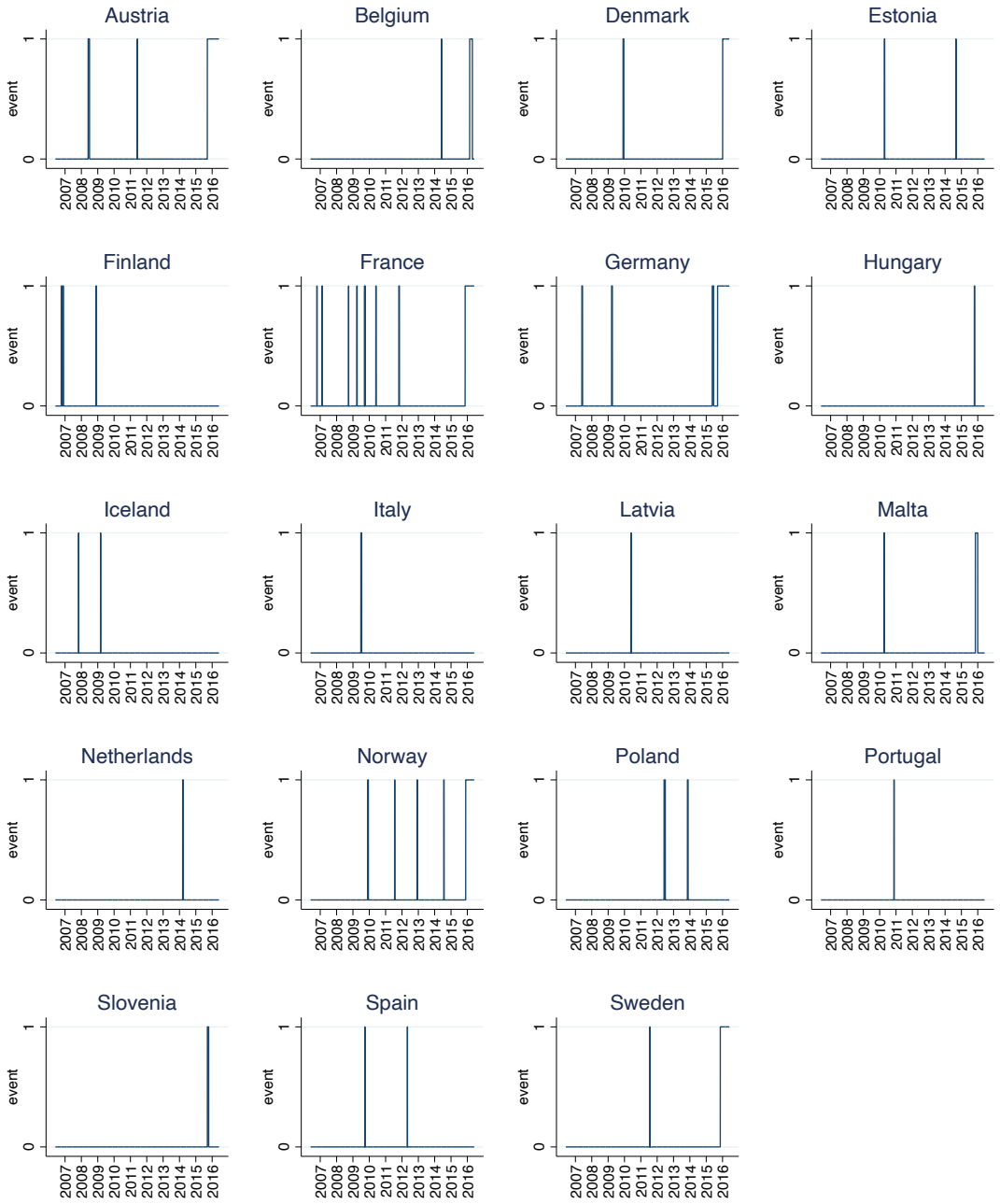
Note: the plot shows the distribution of capitalization-weighted daily stock market returns at the country level, after trimming top and bottom 1% from the raw data.

Table 3.11. Summary Statistics - 26 Countries, June1st2006 - June1st2016

Variable	Mean	S.D.	Min.	Max.	Obs.
Daily Returns Capitalization-Weighted	0.082	0.978	-3.39	3.96	63107
5-day Volatility	0.761	0.495	0.00	4.54	62381
All Events	0.019	0.136	0.00	1.00	63169
Refugees	0.012	0.111	0.00	1.00	63169
Terrorist Attacks	0.002	0.048	0.00	1.00	63169
Expected Events	0.004	0.064	0.00	1.00	63169
log(AssylNo.Applicants)	3.860	3.238	0.00	11.27	63169
Presence of Terrorist Attack	0.004	0.066	0.00	1.00	63169

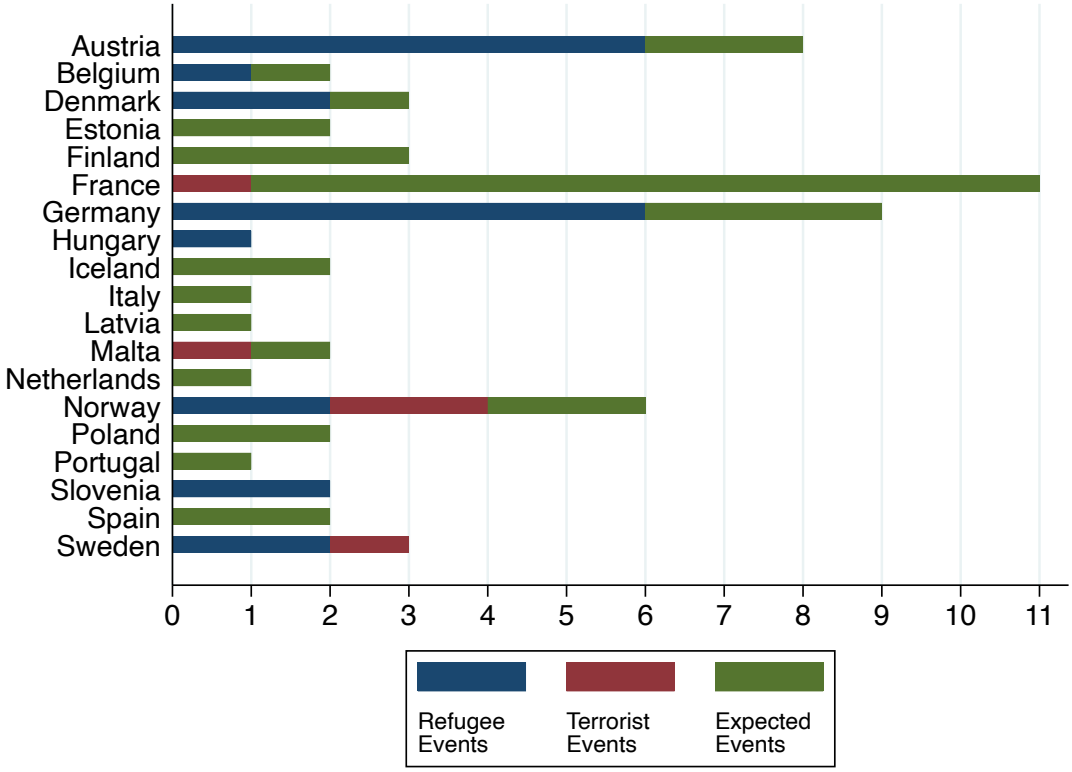
Note: the table shows summary statistics for the baseline sample.

Figure 3.6. Frequency and Duration of all Border Controls in Schengen



Note: Authors' calculations based on the official documents from the European Commisssion. The figure plots the frequency and duration of all temporary border closings for each Schengen member state from June 2006 to June 2016.

Figure 3.7. Frequency by Type of Border Control in Schengen



Note: Authors' calculations based on the official documents from the European Commisssion. The time sample covered is from June 2006 until June 2016.

Robustness

Table 3.12. Comparing Aggregate Returns of the Entire Schengen Area to its Neighbouring Countries

Dependant Var:	Daily Returns		
	(1)	(2)	(3)
All Events	-0.068**	-0.065**	
	(0.030)	(0.030)	
$Event_t$	-0.001	-0.017	-0.017
	(0.007)	(0.020)	(0.020)
$Event_t * Schengen$		0.018	0.010
		(0.022)	(0.022)
Countries	26	38	38
Observations	65,396	94,174	94,174
Tbeg	01jun2006	01jun2006	01jun2006
Tend	01jun2016	01jun2016	01jun2016
CountryFE	Yes	Yes	Yes
DayFE	Yes	Yes	Yes
Controls	Yes	Yes	Yes

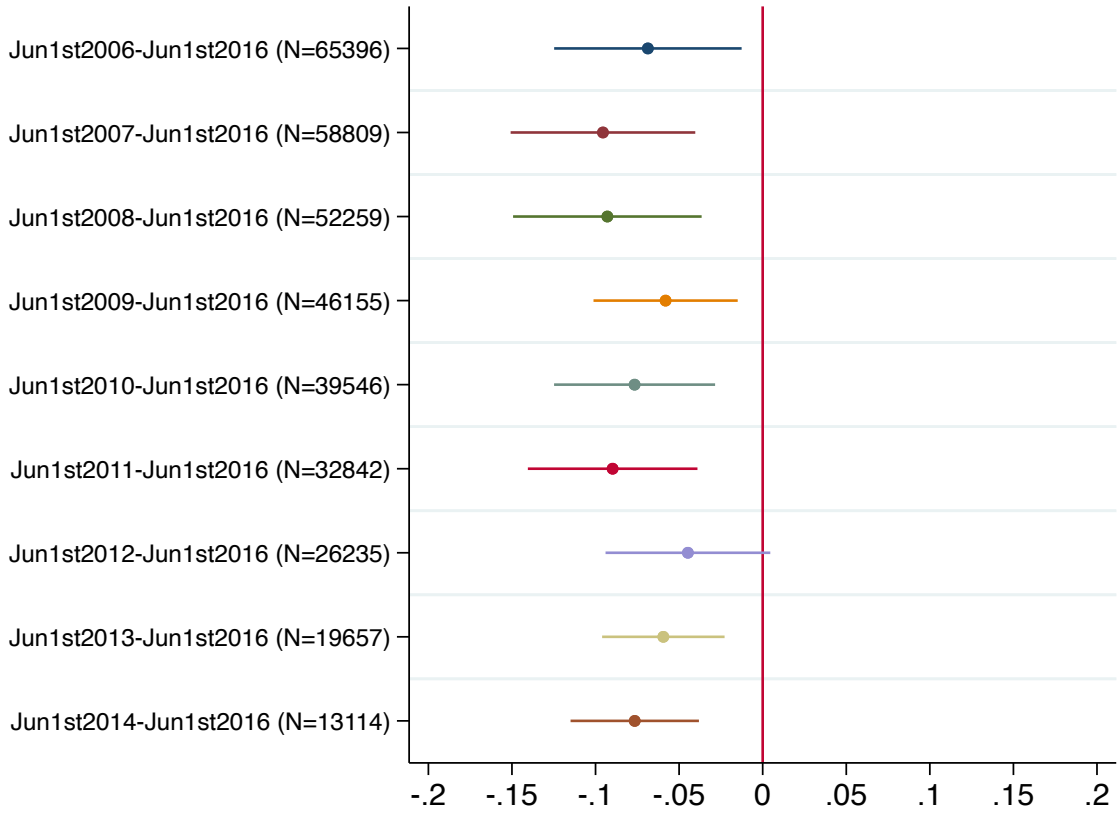
Note: The table reports the border control effect on capitalization-weighted daily returns in the Schengen area compared to its neighboring countries. It estimates Eq. 3.4 using a two-stage difference-in-difference approach. Controls include log number of asylum applications and presence of terrorist attacks. The units of analysis are countries. Standard errors in parentheses are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01.

Table 3.13. Baseline Results Using a Standard Two-Way Fixed Effects Estimator

Dependant Var:	Daily Returns				
	(1)	(2)	(3)	(4)	(5)
All Events	-0.075**				
	(0.031)				
Refugees		-0.062**			-0.068**
		(0.025)			(0.026)
Terrorist Attacks			-0.118***		-0.131***
			(0.028)		(0.030)
Expected Events				-0.062	-0.065
				(0.075)	(0.076)
Countries	26	26	26	26	26
Observations	65,390	65,390	65,390	65,390	65,390
Tbeg	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006
Tend	01jun2016	01jun2016	01jun2016	01jun2016	01jun2016
CountryFE	Yes	Yes	Yes	Yes	Yes
DayFE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	No	Yes

Note: The table reports the effect of the reintroduction of border controls in the Schengen area on capitalization-weighted daily stock returns. It estimates Eq. 3.4 using a standard two-way fixed effects estimator. Controls include log number of asylum applications for the refugee-related variables, and presence of terrorist attack for the terrorism-related variables. The units of analysis are countries. Standard errors in parentheses are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01.

Figure 3.8. Event study plot with different sample lengths



Note: did2s coefficient estimates (and their 95% confidence intervals) are reported. Following the baseline specification, the dependent variable is the daily stock market return. Controls include log number of asylum applications, presence of terrorist attack, country, and day of the week fixed effects.

Table 3.14. Baseline Results Without Controls

	(1)	(2)	(3)	(4)	(5)
Dependant Var:	Panel A: Daily Returns				
All Events	-0.069**				
	(0.028)				
Refugees		-0.054***			-0.054***
		(0.010)			(0.010)
Terrorist Attacks			-0.126***		-0.127***
			(0.028)		(0.029)
Expected Events				-0.073	-0.073
				(0.114)	(0.113)
Countries	26	26	26	26	26
Observations	65,396	65,396	65,396	65,396	65,396
Tbeg	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006
Tend	01jun2016	01jun2016	01jun2016	01jun2016	01jun2016
CountryFE	Yes	Yes	Yes	Yes	Yes
DayFE	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No
Dependant Var:	Panel B: Daily Volatility				
All Events	0.255***				
	(0.060)				
Refugees		0.266***			0.266***
		(0.078)			(0.078)
Terrorist Attacks			0.437***		0.445***
			(0.119)		(0.118)
Expected Events				0.090	0.092
				(0.068)	(0.068)
Countries	26	26	26	26	26
Observations	93,633	93,633	93,633	93,633	93,633
Tbeg	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006
Tend	31may2016	31may2016	31may2016	31may2016	31may2016
CountryFE	Yes	Yes	Yes	Yes	Yes
DayFE	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No

Note: The table reports the effect of different types of border controls: all types (column 1), refugee-related (column 2), terrorism-related (column 3), and expected events related (column 4). The dependent variable is the capitalization-weighted daily stock returns (Panel A), and its 5-day volatility (Panel B). It estimates Eq. 3.4 using a two-stage difference-in-difference approach. The unit of analysis are countries. Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01.

Table 3.15. Robustness to Different Volatility Time Spans

Dependant Var:	2-day volatility	3-day volatility	4-day volatility	5-day volatility	6-day volatility
	(1)	(2)	(3)	(4)	(5)
All Events	0.233***	0.228***	0.233***	0.237***	0.229***
	(0.054)	(0.055)	(0.057)	(0.058)	(0.059)
Countries	26	26	26	26	26
Observations	51,069	65,316	91,493	93,633	94,581
Tbeg	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006
Tend	31may2016	31may2016	31may2016	31may2016	31may2016
CountryFE	Yes	Yes	Yes	Yes	Yes
DayFE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Note: The dependent variables are from 2-day (column 1) to 6–day (column 5) average volatilities of equally weighted daily stock market returns. The variable of interest, all events, is the universe of all border closures. It estimates Eq. 3.4 using a two-stage difference-in-difference approach. The unit of analysis are countries. Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01.

Table 3.16. Yearly Capitalization-Weighted Returns

Dependant Var:	Daily Returns Weighted by Yearly Capitalization Rate				
	(1)	(2)	(3)	(4)	(5)
All Events	-0.069**				
	(0.031)				
Refugees		-0.047***			-0.045***
		(0.010)			(0.011)
Terrorist Attacks			-0.156***		-0.157***
			(0.014)		(0.015)
Expected Events				-0.080	-0.079
				(0.119)	(0.118)
Countries	26	26	26	26	26
Observations	65,401	65,401	65,401	65,401	65,401
Tbeg	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006
Tend	01jun2016	01jun2016	01jun2016	01jun2016	01jun2016
CountryFE	Yes	Yes	Yes	Yes	Yes
DayFE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	No	Yes

Note: The table reports the effect of different types of border controls: all types (column 1), refugee-related (column 2), terrorism-related (column 3), and expected events related (column 4). The dependent variable is the daily return weighted by the capitalization rate from the last trading day in the previous year. It estimates Eq. 3.4 using a two-stage difference-in-difference approach. The unit of analysis are countries. Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01.

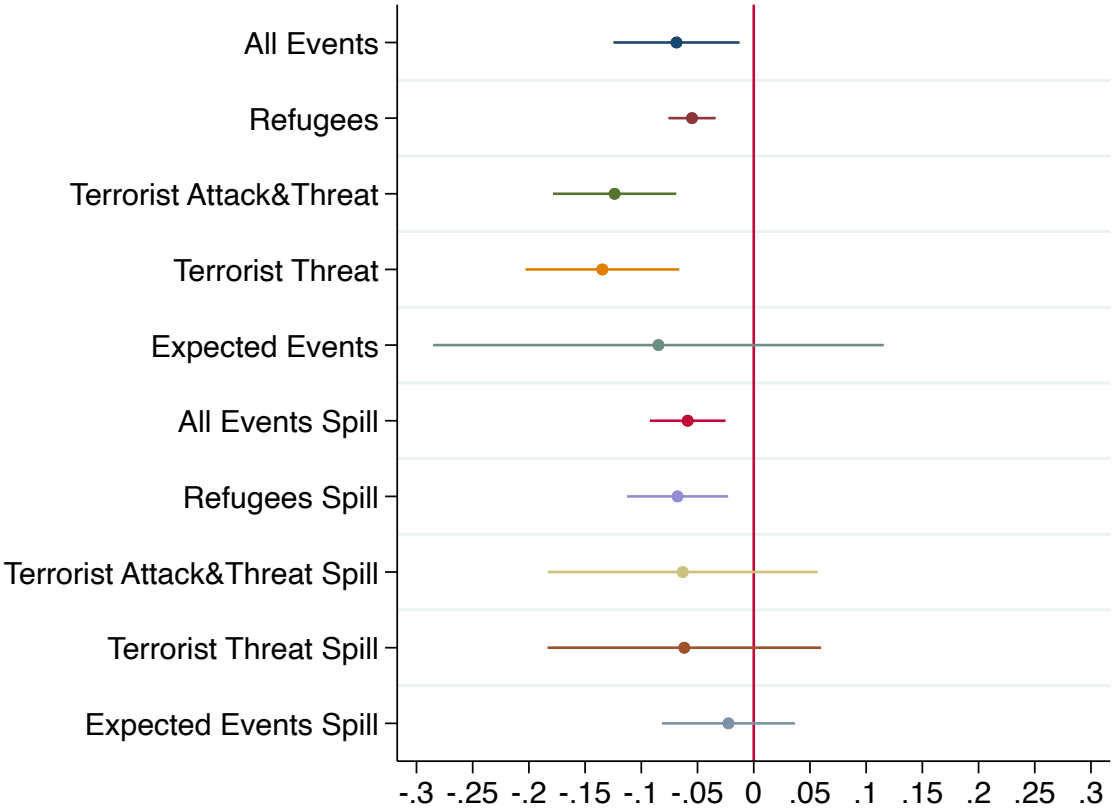
Table 3.17. Benchmark - Winsorizing 1%

Dependant Var:	Daily Returns Winsorized at 1%				
	(1)	(2)	(3)	(4)	(5)
All Events	-0.058**				
	(0.026)				
Refugees		-0.063***			-0.061***
		(0.010)			(0.010)
Terrorist Attacks			-0.070***		-0.072***
			(0.018)		(0.018)
Expected Events				-0.038	-0.037
				(0.109)	(0.108)
Countries	26	26	26	26	26
Observations	66,542	66,542	66,542	66,542	66,542
Tbeg	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006
Tend	01jun2016	01jun2016	01jun2016	01jun2016	01jun2016
CountryFE	Yes	Yes	Yes	Yes	Yes
DayFE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	No	Yes

Note: The table reports the effect of different types of border controls: all types (column 1), refugee-related (column 2), terrorism-related (column 3), and expected events related (column 4). The dependent variable is the capitalization-weighted daily return winsorized at 1% (top and bottom of the distribution). It estimates Eq. 3.4 using a two-stage difference-in-difference approach. The unit of analysis are countries. Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01.

Heterogeneity

Figure 3.9. Treatment Heterogeneity and Spillovers



Note: did2s coefficient estimates (and their 95% confidence intervals) are reported. Following the baseline specification, the dependent variable is the daily stock market return. The controls include log number of asylum applications, occurrence of a terrorist attack, country and day of the week fixed effects.

Table 3.18. Border Control Effect Heterogeneity by Industries

Sub-sample:	Transport	Energy	Materials	Industrials	Discretionary	Staples	Financials	IT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependant Var:	Panel A: Daily Returns							
All Events	-0.044	0.008	0.043	-0.003	-0.046	0.050	-0.081**	0.000
	(0.033)	(0.040)	(0.033)	(0.034)	(0.035)	(0.035)	(0.033)	(0.042)
Countries	23	21	24	25	25	25	26	23
Observations	54,761	47,835	56,964	59,424	56,403	60,348	62,189	51,593
Tbeg	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006
Tend	01jun2016	01jun2016	01jun2016	01jun2016	01jun2016	01jun2016	01jun2016	01jun2016
countryIndustryFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DateFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependant Var:	Panel B: Volatility							
All Events	0.213**	0.261***	0.139***	0.123	0.011	0.094	0.173*	0.274***
	(0.084)	(0.070)	(0.052)	(0.113)	(0.059)	(0.061)	(0.103)	(0.084)
Countries	23	21	24	25	25	25	26	23
Observations	864,260	781,444	907,248	934,534	896,713	945,210	944,391	836,550
Tbeg	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006	01jun2006
Tend	31may2016	31may2016	31may2016	31may2016	31may2016	31may2016	31may2016	31may2016
countryIndustryFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DateFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The dependent variable is the capitalization-weighted daily stock returns (Panel A), and its 5-day volatility (Panel B), across different industries. We follow the Eurofidai (2021) classification, where for each company, the information about its industry sector is provided. The transport industry consists of companies for which the activity is connected to traffic and transportation (column 1). Energy industry is composed of petroleum/oil and natural gas firms (column 2). Materials industry is related to commodity chemicals, agricultural chemicals, construction materials, paper, and forest products (column 3). Industrials industry is composed of capital goods, commercial and professional services and transportation (column 4). Discretionary industry is composed of automobiles and components, consumer durables, apparel, consumer services, media (column 5). Staples industry includes food, beverages, tobacco, food and staples retailing, household and personal products (column 6). Financials sectors is composed of, real estate, banks and other credit institutions, mortgage and funding institutions, and insurance companies (column 7). The IT industry is related to electronics and semiconductors, internet, software and IT services, computer hardware and networking (column 8). The variable of interest, all events, is the universe of all border closures. It estimates Eq. 3.4 using a two-stage difference-in-difference approach. The unit of analysis are countries. Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01.

Table 3.19. Border Control Effect Heterogeneity by Firm Size

Sub-sample:	Small Cap (1)	Medium Cap (2)	Large Cap (3)
Dependant Var:		Panel A: Daily Returns	
All Events	0.000	-0.012	-0.018
	(0.015)	(0.027)	(0.020)
Countries	26	26	26
Observations	63,033	63,559	65,209
Tbeg	01jun2006	01jun2006	01jun2006
Tend	01jun2016	01jun2016	01jun2016
countryFirmSizeFE	Yes	Yes	Yes
DateFE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Dependant Var:		Panel B: Volatility	
All Events	-0.062	-0.006	0.081**
	(0.047)	(0.038)	(0.040)
Countries	26	26	26
Observations	188,502	189,374	192,090
Tbeg	01jun2006	01jun2006	01jun2006
Tend	31may2016	31may2016	31may2016
countryFirmSizeFE	Yes	Yes	Yes
DateFE	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Note: The dependent variable is the capitalization-weighted daily stock returns (Panel A), and its 5-day volatility (Panel B), across different firm sizes. Firm size is calculated by classifying all the companies into 3 quantiles based on their market capitalization. It estimates Eq. 3.4 using a two-stage difference-in-difference approach. The unit of analysis are countries. Standard errors are clustered at the country level. * p<0.1, ** p<0.05, *** p<0.01.

4

General conclusion

This PhD dissertation aims to increase the understanding of different law dimensions and their connection with migration, economic growth, and the stock market.

In the first chapter we investigate the impact of access to justice on economic growth. In a panel of 73 countries, we show that ATJ is a significant and positive determinant of GDP per capita growth. The results are robust to the exclusion of various sub-samples and to different falsification exercises. Our estimates imply that increasing ATJ by 1% increases the GDP per capita five-year growth rate by 0.7pp. That substantial overall effect of ATJ depends negatively on the initial level of GDP per capita, human capital, democracy, economic freedom, and the rule of law. This has important policy implications, as it means that the poorest countries are the ones who can have the highest economic returns from improving ATJ. In terms of mechanism, our findings suggest that ATJ increases growth via higher government accountability and better quality of institutions. Taken as a whole, our results indicate that ATJ is one of the key elements when trying to understand the impact of effective, or *de facto*, judicial institutions on economic development.

In the second chapter we analyse if immigration (in terms of both size and composition) can affect the labor market regulation in destination country. To examine this link, we build a novel workers' protection measure based on 36 labor law variables over a sample of 70 developed and developing countries from 1970 to 2010. Exploiting a dynamic panel setting using both internal and external instruments, we establish a new result: immigration impacts workers' protection in the direction of the origin country workers' protection (composition channel), while we find a small negative or null effect for the immigrant population (size channel). The set of evidence provided in this paper shows that immigrants are a source of law transfer which shapes labor regulation, and we provide one new mechanism explaining this effect.

The third and final chapter investigates the impact of temporary border controls on stock market returns. To do so, we first document a unique dataset on the reintroduction of border controls constructed from official documents. Then, we consider two complementary research designs: an event study exploiting the first refugee-induced border control in Germany in September 2015, and a difference-in-difference analysis covering all border controls in the Schengen area between 2006 and 2016. In both analyses, we find a negative and short-lived effect on daily stock returns. These results are confirmed across various sectors, causes of border controls, and firm sizes. We find that financial markets react to the introduction of unexpected border controls, but the effect on daily returns is short-lived. Firms do not seem to interpret the reintroduction of border controls as a long-term sign of the dislocation of the Schengen agreement.

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