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Does Institutional Change Spread Across Countries? Explaining Spatial Patterns in Human Rights*

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1. INTRODUCTION

IN 1945, the United Nations was founded to promote peace, security and human rights through international cooperation. In the wake of the horrors of the Second World War, protecting the rights laid down in the 1948 Universal Declaration of Human Rights became a prime goal of the UN. Other organisations, including many national governments, non-governmental organisations like Amnesty International and the American Library Association, religious organisations and trade unions put great effort into organising international support for the Declaration. Similarly, the provision of aid, trade concessions and investment treaties was linked to the observance of the Declaration. In 2000, all 189 member countries of the UN committed to jointly pursuing the Millennium Goals, of which human rights are a substantial pillar. Do these international efforts improve human rights integrity in poorly performing countries? They are not successful on all accounts: the respect for human rights is far from perfect in large parts of the world. Human right violations occur at a large scale over long stretches of time, in several regions of the world.

Interactions between neighbouring countries can frustrate international efforts to promote local human rights. Regional military interventions, trade and investment relations and political interactions with neighbouring countries probably determine local human rights. If countries thus reinforce each other's poor human rights, interventions for improvement may quickly be undone. Or more positively, local aid may have wider benefits in the region. Under such interactions, spatial clusters of good and poor human rights emerge. Figure 1 shows some tentative proof that human rights records follow spatial patterns, by mapping a human rights score, the Political Terror Scale (PTS). Visually,¹ there is indeed a clustering of high and low scores. Figure 1 suggests that violations of human rights in neighbouring countries are associated with a low domestic respect for human rights.

However, the spatial pattern in human rights scores does not prove that international interactions or spillovers play a large role in the development of human rights. Neighbouring countries often share a history and geography, which are replete with possible explanations of human rights performance. Such 'deep determinants' can include the physical geography: the soil and climate in sub-Saharan Africa may lag economic development, which often goes

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¹ And statistically: the Moran's I statistic for spatial autocorrelation is 0.123 and rejects the null of random distribution with a *p*-value beyond the second decimal. The Moran's I statistic is high, when scores are clustered in space. Section 4 contains an exact definition.

FIGURE 1
Human Rights Scores Across the Globe

Notes: US Department of State scores. Higher scores point to more violations of human rights

hand in hand with human rights violations (Sachs, 2001). Similarly, nearby countries often share history, including wars, the same colonisers and the same culture. The geography and history associated with a location affect overall institutional quality, thus determining the protection of minorities and the risk of conflicts. Indirectly, a good institutional quality contributes to economic development, possibly allowing for better human rights indirectly, too (Acemoglu et al., 2001, 2002, 2005; Rodrik et al., 2004). Therefore, even if current-day interactions between countries were absent, spatial patterns in human rights violations are still likely to form due to deep determinants.

But do Niger and Chad have the same imperfect human rights performance because they share a border; or because they both have large stretches of desert and saw French colonisation? While there is much anecdotal evidence, there are far fewer structural analyses that examine whether human rights benefit from, say, economic development in neighbouring countries. To design human rights policy, the interpretation of spatial human rights patterns is decisive. Local improvements in human rights (or violations of human rights) carry implications for other countries only, if international diffusion of the respect for human rights occurs. If the regime changes incited by the 'Arab Spring' in countries like Tunisia, Egypt and Yemen improve the national human rights performance, spatial spillovers could lead to parallel effects in North Africa and the Middle East. Conversely, serious deteriorations of human rights following political turmoil or conflict might lead to interventions in neighbouring countries. In this situation, human rights intervention may have indirect benefits elsewhere. Moreover, if countries reinforce each other's human rights violations, regional instead of national interventions are called for. However, if deep determinants like history and geography explain spatial patterns in human rights, such spatial spillovers are less realistic. Similarities among neighbouring countries like climate, diseases, legal heredity or soil quality cause similar outcomes, but will not lead changes in human right performance to affect neighbours. If such

deep determinants explain spatial human rights patterns, no external effects should be expected. This paper is one of the first to statistically examine human rights policy spillovers between countries in the light of such spatial patterns.

Analysing spatial human rights patterns, this paper contributes to the literature studying (international) change of institutions. Human rights, that include protection of property, equality before the law and freedom of movement and expression, are intimately related to indexes of institutional quality.² To the extent that human rights are part of broader institutions, this paper contributes to the large literature that studies institutions as an engine of long-run growth. Whereas most other measures of institutions vary little over time, the large variation in human rights indexes over time allows studying the dynamics by which these institutions change. Most other studies of institutional change via international channels are limited to cross-sections due to the time-invariant nature of most institutional indexes. Faber and Gerritse (2012), for example, document effects of the institutions in nearby countries on domestic institutional quality, such as the rule of law, voice and accountability and control of corruption. Claeys and Manca (2011) find no direct evidence of spatial patterns in institutional indexes. In cross-sectional data, it is hard to unravel locational features from interactions between locations. Exploiting larger time variation, our approach is better able to disentangle the potential causes of spatial patterns in human rights records into locational effects and contemporaneous interactions. The paper most closely related in this respect is Kelejian et al. (2013). Kelejian et al. document significant spatial patterns in their institutional indicators even when accounting for long-run (fixed) effects. The methodology and sample are not the same, but still the contrast with our result is rather sharp.

To our knowledge, this paper is among the first to document the international spatial patterns in human rights, and to exploit a long time horizon in institutions. In that respect, it builds on studies of intergovernmental organisations as spillover mechanisms (Greenhill, 2010), and of the sensitivity to human rights of foreign direct investments (Blanton and Blanton, 2007), aid flows (Carey, 2007), preferential trade agreements (Hafner-Burton, 2005b) and regional human rights agreements (Hathaway, 2002). However, while these papers highlight that many economic outcomes depend on the local human rights situation, our paper studies whether there is evidence of interaction regarding human rights between economies.

The results from our spatial panel methodology suggest that the role of interactions in human rights policy is limited: location matters, but events in neighbouring countries do not. In cross-sections, it is not possible to disentangle the two – the long-run spatial patterns may easily be mistaken for interactions (or sometimes called spatial interactions, network effects or peer effects). Therefore, the methodological step of exploiting time variation carries implications for other studies considering international spillovers in human rights, but also for studies in economic growth spillovers and tax competition that use similar methodologies. The results tie in with recent cautions that the statistical relevance of neighbouring countries' scores (or other peers for that matter) can only be interpreted as interactions in very specific circumstances (Abreu et al., 2005; Gibbons et al., 2015).

The paper is organised as follows. In the second section, we discuss the importance of neighbouring countries in explaining spatial human rights patterns and motivate our estimating equation. The third section discusses our data set and empirical strategy. Section 4 presents the results of the empirical analysis; a concluding section 5 completes the paper.

² Such as the popular World Governance Indicators (Kaufmann et al., 2010), that include the rule of law, political stability and absence of violence, and voice and accountability.

2. MOTIVATING THE ESTIMATING EQUATION

To structure our analysis, we shortly consider what reasons there could be for spatial patterns in human rights. In short, spatial patterns can be explained by interaction as well as by long-run determinants that bear similarity to neighbouring countries.

First, there are several channels through which human rights policies of other countries change the domestic human rights policies. We dub these ‘spillovers’, because they are the (unintended) consequence of changes in human rights policy elsewhere.

International trade is a first channel of diffusion of human rights policies. To many consumers, the degree of human rights violation is a quality of a product. For instance, consumers often dislike the idea that child labour was involved (Edmonds and Pavcnik, 2006), that workers had no right to organise (Kucera and Sarna, 2006), or that inputs have been acquired under ‘unfair’ circumstances (like coffee or textiles, see, e.g. Bacon, 2005). The pressure of consumers, activists and shareholders due to ‘shaming’ or ‘spotlights’ can substantially alter the strategies of importing firms (Greenhill et al., 2009).

Poorer human rights decrease prospective international sales. Policymakers who have exporters’ welfare in mind may therefore improve human rights; or lobbies and political contributions can change institutional quality, including human rights (Grossman and Helpman, 1994; Levchenko, 2010). In that case, human rights choices may depend on the human rights situation of neighbouring countries. An economy surrounded by countries with strong human rights records could see stronger condemnation of human rights violations in terms of lost revenue from its main trade partners. Equally, if a country’s neighbours are its main competitors for exports (e.g. due to proximity to the same third countries), then its human rights record relative to those of its neighbours determines sales. Both ethical considerations and competition from firms in other countries may therefore increase pressure on human rights policies if nearby countries have better human rights scores. Relatedly, Beugelsdijk et al. (2015) show that both in the importing and exporting country, a low quality of rule of law (which includes several human rights) impedes trade.

Similarly, international investments may give rise to policy competition in the sphere of human rights. Foreign direct investments are sensitive to various institutions, including contract enforcement, governance and labour laws and to likelihood of conflict (see the survey of Dixit, 2011). Dadasov and Lorz (2013) provide a theoretical rationale, showing that under the prospect of international investments, non-democratic elites may reduce the local risk of expropriation. Blanton and Blanton (2007) show that FDI responds strongly to local human rights policies, specifically. Wishing to attract investments, governments need to display good human rights policies relative to other potential investment locations: increased human rights performance may have externalities in competing countries.³ FDI also seems less sensitive to poor institutions in the host country if the home country has poor institutions (Darby et al., 2009). Poorly performing neighbours are therefore less likely to pressure the home country into a better human rights policy. Dharmapala and Hines (2009) show that the attractiveness of tax havens strongly correlates with the quality of local institutions, too. To compete for foreign direct investments, governments need a credible commitment to non-expropriation which requires high governance standards (in particular freedom of speech and property rights

³ The opposite argument could be made that human rights violations lower costs for multinationals, and as a result, poor human rights policies attract FDI. This similarly leads to a spatial dependence, but there is less evidence for this proposition (Harms and Ursprung, 2007).

captured in the ‘voice and accountability’ and ‘rule of law’ indexes that Dharmapala and Hines use). If the respect for human rights is part of such institutions, then changes in human rights policy could affect the tax base elsewhere – giving rise to an analogous competition in policies.

Other possible reasons for countries to respond to other countries’ human rights choices include the competition for aid flows and migration. The prospect of aid flows could encourage countries to outshine the neighbours in human rights performance, especially for conditional aid. It is not beyond discussion, however, whether aid flows respond to human rights practice: Carey (2007) shows that among European donors, human rights records hardly influence the choice of recipient, whereas aid from the United States may be more sensitive to human rights records (Demirel-Pegg and Moskowitz, 2009: 96). Fielding (2014) shows that aid inflows directed towards governance reform have no or even negative effects on institutional outcomes. Magesan (2013) finds that ratifying the UN Human Rights treaty may increase aid inflows, but that civil liberties typically decline afterwards. Lastly, Fleck and Hanssen (2013) argue that violations of rights are restricted by the situation in neighbouring countries: citizens may simply migrate to countries with better performances. Thus, migration to nearby countries can also restrict the local human rights policies.

To formalise the idea that the human rights policy choices in one country depend on the human rights situations elsewhere, we assume that:

$$HR_{it} = HR_{it}(HR_{jt}, X_{it}, \alpha_i, \epsilon_{it}). \quad (1)$$

In this set-up, the human rights quality HR_{it} in country i at time t depends directly on the human rights quality of a set of other countries j (akin to a reaction curve), local conditions X_{it} at time t and local innovations to the human rights situation ϵ_{it} . The term α_i reflects the initial conditions or time-invariant effects regarding human rights policy.

The reaction curves or cross-country dependence between HR_{it} and HR_{jt} can explain spatial patterns in human rights indicators. Most of the literature above predicts that nearby countries have stronger effects on local human rights. Consequently, the conditional expectation of the human rights score is higher, if nearby countries have higher human rights scores.

However, the initial condition α_i can equally explain spatial patterns in human rights scores. Even if HR_{jt} does not directly affect HR_{it} , the conditional expectation of the human rights score still increases in surrounding countries’ human rights scores if the initial condition is spatially autocorrelated. In that case, α_i is high if nearby α_j are high. Consequently, the conditional expectation of HR_{it} is higher when nearby HR_{jt} are higher, even if there is no direct interaction like a spillover of reaction curve (i.e. $HR_{it} = HR_{it}(X_{it}, \alpha_i, \epsilon_{it})$) still shows spatial autocorrelation.

It is hard to rule out that initial conditions or long-run processes captured in α have spatial components. For example, countries with a British legal origin often have fewer human rights violations. But colonies that share the same legal origin are often spatially clustered – North America has British legal origins while South America has (almost exclusively) French legal origins. Ethnic fractionalisation reduces workers’ rights and income and causes conflict (Barr and Oduro, 2002; Alesina et al., 2003). Ethnic fractionalisation, equally, shows strong spatial patterns: it is high in most of Africa but low in most of Europe and much of Asia. Sokoloff and Engerman (2000) argue that a country’s (natural) comparative advantage affects its subsequent institutional development – an advantage in sugar production, for instance, fosters slavery and hence promotes unequal institutions. If the respect for human rights can be traced back to growing conditions for different kinds of crops, this likely generates spatial patterns, too.

The literature on ‘deep determinants’ lends support to the idea that a country’s initial conditions are not randomly scattered over space. Long-run development, poverty and illiteracy may follow from physical geography, so geography may determine the human rights situation by implication. Diamond (1997) argues that the position and shape of continents has had a decisive impact on the development and subsequent spreading of agricultural production methods, contagious diseases, technology and the formation of states and political power. Sachs (2001) argues that temperate climates foster productivity-gear development. Gallup et al. (1999) find inhibitions for tropical regions due to higher disease burdens and a lower agricultural productivity; and for landlocked and remote countries. Congdon Fors (2014) documents that the isolation and small size of islands typically leads them to have high institutional quality, however. History and climate can also contribute to the formation of earlier institutions, which are often persistent over time. Acemoglu et al. (2001, 2002, 2005) show that the origin of the coloniser affects long-run growth of former colonies, and harsher climates can lead to the set-up of extractive institutions. Many of these deeper determinants follow spatial patterns.

As argued before, it is purposeful to understand whether spatial patterns in human rights result from direct interaction, or from spatial patterns in ‘deep determinants’. If international interactions or spillovers cause spatial dependence, human rights interventions might be effective and have indirect benefits. If initial conditions or other time-invariant causes explain the spatial patterns, the expected effects from interventions are correspondingly lower.

Our strategy is to exploit the time-variance of human rights indicators to isolate the effects of time-invariant processes from interaction effects. In linearised form, a ‘naïve’ version of the human rights equation that does not take into account the time-invariant explanation like history or geography is:

$$HR_{it} = \lambda \sum_{i \neq j} w_{ij} HR_{jt} + \beta x_{it} + \varepsilon_{it}, \quad (2)$$

where w_{ij} gives the weight of the relation between country i and country j (all bilateral links are considered, but not the i - i link), and λ is coefficient for the strength of cross-country dependence. Effects of local time-varying conditions are captured in β . If this equation is estimated, a positive estimate of λ points to interactions between countries. But that conclusion might be misjudged if the initial conditions α show spatial autocorrelation. In that case, HR_{jt} depends on α_j , and is correlated to α_i in the regression’s error term. Since these terms are time-invariant, estimating the equation in differences from the time-mean resolves the bias. Explicitly accounting for the time-invariant explanations of human rights, the equation is:

$$HR_{it} = \lambda \sum_{i \neq j} w_{ij} HR_{jt} + \beta x_{it} + \alpha_i + \varepsilon_{it}, \quad (3)$$

which clears the estimate of the cross-country dependence, λ , from any spatial patterns in the ‘deep determinants’ α_i .

3. EMPIRICAL STRATEGY AND DATA

a. Estimation Strategy

To investigate whether neighbouring countries’ human rights outcomes affect the local human rights score, we specify a statistical model that accounts for the effect of peer countries’ human rights infractions on the local human rights performance. To capture possible influences

of nearby countries in the local human rights situation, we estimate equation (3) including a spatial lag. The lag summarises the state of human rights in other countries, where a pre-specified weighting scheme assigns higher weights to nearby countries. In related literature, such a spatial lag is often used to identify interaction between different locations. Examples include the identification of tax reaction functions from tax competition models (i.e. how tax rates depend on tax rates in neighbouring jurisdictions, Egger et al., 2005); spillover effects in economic growth rates (Conley and Ligon, 2002), or international R&D spillovers (Coe et al., 2009).

The spatial lag allows us to consider whether human rights scores in geographically nearby countries determine local human rights scores statistically. For the weights of interaction, w_{ij} , we follow convention and use the standardised inverse distance between capitals as the intensity of interaction. The standardisation implies that all weights for a country sum to unity: $w_{ij} = \text{dist}_{ij}^{-1} / \sum_{i \neq j} \text{dist}_{ij}^{-1}$. Weighing other countries' human rights scores, the spatial lag is a cross-sectional average of human rights scores with larger weights for more nearby countries. An appealing alternative to using distances are trade flows. Given the simultaneity bias between institutions and trade flows (e.g. Méon and Sekkat, 2008; Nunn and Treffer, 2014), using trade flows to measure proximity gives rise to large endogeneity problems – spatial lag estimators assume exogeneity of the weights.

The linear regression model with a spatial lag is:

$$HR_{it} = \lambda \sum_{i \neq j} \frac{\text{dist}_{ij}^{-1}}{\sum_{i \neq j} \text{dist}_{ij}^{-1}} HR_{jt} + \sum_k \beta_k x_{kit} + \alpha_i + \gamma_t + \varepsilon_{it}, \quad (4)$$

which is the earlier model augmented with a set of local controls (indexed k) and a country fixed effect α_i .⁴

The term γ_t is a year fixed effect that absorbs global trends in the human rights indicators. The time fixed effects capture worldwide changes in human rights practice, but also makes the specification robust to any changes over time in the practice of coding human rights scores.

We estimate equation (4) without fixed effects as pooled model, or in deviations from the time averages with a fixed effect. The fixed effects rule out that we incorrectly identify similarity in geography, or location-induced similarities in history, like similar colonisers, as interactions between countries. In essence, we compare whether countries share similar patterns in their deviations from long-run averages – more colloquially: whether higher than normal human rights scores of neighbours are followed by higher than normal human rights scores locally. Thus, we exploit the within variation of the sample, and allow country-specific constant α_i to pick up time-invariant location effects. In alternative terminology, the location fixed effect isolates the spatial heterogeneity, correlated effects or common shocks from the interaction effects (Gibbons et al., 2015).

Equation (4) cannot be estimated consistently by regular OLS. There is a simultaneity bias in the spatial lag, because if outcomes in j affect i , it is also likely that the reverse occurs. To account for this reflection effect, we use a maximum-likelihood estimator that takes the

⁴ Apart from the spatial lag, the error term could follow a spatial process, especially if shocks are correlated over space. We have estimated the main specification allowing the error term to be correlated over space, such that $\varepsilon_{it} = \rho \sum_{i \neq j} w_{ij} \varepsilon_{jt} + u_{it}$, where u_{it} is an IID error term. Specification tests vindicate a lag model – either way the introduction of this error term does not affect any of the conclusions.

explicit structure of the spatial interaction into account. Additionally, the presence of fixed effects in this model renders the variance-covariance matrix singular, which requires a modification of the log-likelihood function to circumvent this issue. The fixed effects are filtered out using a transformation approach (Lee and Yu, 2010a), which does not require an asymptotically large number of observations in the time dimension.⁵

(i) *Descriptive Statistics*

Our data set uses human rights measures as well as various controls. Table 1 presents the descriptive statistics of our base sample, which runs over the years 1999 to 2009 and over 146 countries. The quantification of a human rights situation is not trivial; more than 50 human rights can be distinguished in international law. As a consequence, different measures of human rights exist (Landman and Carvalho, 2009), but our focus is on those that allow comparisons across countries. Given the spillover mechanisms outlined in the literature review above, measures of *de facto* human rights protection have preference over those that describe the statutory or *de jure* human rights respect. Both the Political Terror Scale (PTS; Gibney et al., 2010) and the Political Integrity Rights (PIR; Cingranelli and Richards, 2008) are indexes constructed from US State Department or Amnesty International's reporting for a large number of countries. They give higher scores for countries where human rights are violated more severely. These indexes take into account the extent to which the state uses extra-judicial killings, torture, political imprisonment and exile. Not surprisingly, the two measures are rather correlated (a coefficient of around 0.7, Wood and Gibney, 2010). The PTS arguably lends itself somewhat more for cross-country comparisons, because it scales the number of human rights violations to the country's size. Therefore, we use the PTS scale for most of our analysis. However, the PIR provides separate scores for its constituent parts, allowing analysis at a more disaggregated level. For that reason, we perform robustness checks with different elements of the PIR human rights score.

TABLE 1
Descriptive Statistics

	<i>Mean</i>	<i>SD</i>	<i>SD between</i>	<i>SD within</i>	<i>Moran's I test</i>
Human right index	2.54	1.05	0.95	0.46	0.12***
Log GDP	3.01	2.10	2.08	0.16	0.11***
Log population	2.31	1.60	1.60	0.05	0.03***
Urban population	53.52	22.11	22.12	1.45	0.17***
Inflation	0.01	0.02	0.01	0.01	0.02***
Food production	0.11	0.02	0.01	0.01	0.27***
Resources	0.66	1.77	1.67	0.60	0.01**
Life expectancy	67.43	10.36	10.34	1.15	0.32***
Conflict	0.14	0.35	0.30	0.17	0.03***

Notes:

(i) Observations: 1397.

(ii) *** $p < 0.01$, ** $p < 0.05$.

⁵ Our results do not differ between either way of introducing the fixed effects. The estimates reported are obtained using Stata code (xsmle).

The data on the onset of conflict stem from the Peace Research Institute in Oslo.⁶ All other controls are from the World Bank open data initiative.⁷ Note that time-invariant variables like climate do not contribute to the within variation, because they are subsumed in the fixed effects and therefore controlled for. Throughout the paper, we use bilateral distances based on the largest population agglomerations, which are from the CEPII data set (Mayer and Zignago, 2011).

The selection of controls is based on a reading of the literature that statistically explains human rights levels. Following Poe and Tate (1994), Lee et al. (2004), Landman (2005) and Hafner-Burton (2005a), we include variables that describe production, population size and conflicts in a country. Because we have a large share of developing countries in our data set we also include urbanisation rates and life expectancy, which describe the state of development. Also, as the onset of conflict may be traced back to food shortages or the presence of appropriable resources, we include a food production index and the natural resource rents as a share of GDP. We use consumer price inflation as a proxy of the quality of economic policy. In addition to these controls, the literature offers many variables that describe initial conditions, such as colonial history, language, ethnolinguistic features, judicial and political systems. Note that these are generally time-invariant and thus are absorbed in our fixed effects specification.

The variables in the data set show clear spatial patterns. Moran's *I* is a measure of spatial autocorrelation: it essentially tests whether the value of a variable in one country resembles its value in nearby countries (see footnote 15 for a formal definition). The Moran's *I* statistic rejects the null of no spatial autocorrelation with a *p*-value smaller than 5 per cent for all variables.

The time variation in the data set is substantial. Without time variation in the data, one would be likely to conclude that time-invariant processes explain spatial patterns. We report the decomposition of each variable's standard error in 'between' variation (cross-sectional) and 'within' variation (over time within each country). The standard error of the human rights indicator within countries over time is sizable, at around half of the standard deviation between countries. The within-variation standard deviation is 0.45 (instead of 0.46) when taking out time fixed effects to account for trends. The magnitude of within variation relative to between variation differs in the controls, with GDP, population and life expectancy showing variation between countries, but inflation and food production showing more variation within countries over time. In the 1999 to 2009 sample, we observe 492 year-on-year changes in the human rights indicator, which represents 39 per cent of all potential year-on-year changes.

4. RESULTS

The columns related to model 1 and 2 in Table 2 report the central regression results, using the PTS score as a dependent variable. Column (1) presents a pooled regression with a spatial lag. It is estimated using a maximum-likelihood function for spatial lag models (Lee, 2004). The spatial lag is statistically significantly different from zero, which apparently confirms our expectation that human rights situations in countries are at least partly the result of spatial spillovers. The signs of the other coefficients are as expected, and all except the coefficient for the food production index and life expectancy are statistically significant.

⁶ Freely available from <http://www.prio.no/CSCW/Datasets/Armed-Conflict/>

⁷ Accessible via <http://databank.worldbank.org/data/home.aspx>

TABLE 2
Effects on Human Rights index

Dependent: PTS	(1) Pooled	Indirect	Total	(2) FE	Indirect	Total	(3) Pooled	(4) FE	(5) Pooled 5 year	(6) FE 5 year
Log GDP	-0.32*** (0.02)	-0.23*** (0.07)	-0.55*** (0.08)	0.02 (0.23)	-0.00 (0.03)	0.02 (0.21)	-0.34*** (0.02)	0.02 (0.23)	-0.42*** (0.05)	-0.55*** (0.25)
Log population	0.50*** (0.03)	0.36*** (0.11)	0.87*** (0.12)	0.59 (0.70)	0.01 (0.08)	0.65 (0.80)	0.52*** (0.02)	0.59 (0.71)	0.55*** (0.05)	0.71 (0.44)
Urban population	0.00*** (0.00)	0.00* (0.00)	0.01** (0.00)	-0.02 (0.02)	-0.00 (0.00)	-0.02 (0.02)	0.00** (0.00)	-0.02 (0.02)	0.01*** (0.00)	0.01 (0.01)
Inflation	4.42*** (1.05)	3.18*** (1.21)	7.62*** (2.00)	2.52*** (1.04)	0.14 (0.24)	2.66** (1.05)	4.19*** (0.86)	2.53*** (1.05)	0.36*** (0.13)	0.30*** (0.15)
Food index	0.49 (1.29)	0.51 (0.89)	1.26 (2.09)	-0.27 (1.91)	0.02 (0.18)	0.06 (1.90)	1.12 (1.39)	-0.24 (1.92)	0.74 (0.68)	0.65 (0.42)
Resources	0.05*** (0.01)	0.04*** (0.01)	0.09*** (0.02)	-0.05*** (0.02)	-0.00 (0.01)	-0.05*** (0.02)	0.06*** (0.01)	-0.05*** (0.02)	0.11*** (0.03)	0.00 (0.08)
Life expectancy	0.00 (0.00)	0.00 (0.00)	0.00 (0.01)	-0.03 (0.03)	-0.00 (0.00)	-0.04 (0.03)	-0.00 (0.00)	-0.03 (0.03)	0.00 (0.01)	-0.01 (0.02)
Conflict	0.91*** (0.06)	0.65*** (0.22)	1.57*** (0.25)	0.55*** (0.14)	0.04 (0.06)	0.59*** (0.16)	0.92*** (0.07)	0.55*** (0.14)	1.15*** (0.09)	0.87*** (0.15)
Lag	0.42*** (0.08)			0.06 (0.09)					0.25*** (0.13)	-0.14 (0.12)
Observations	1,397	1,397	1,397	1,397	1,397	1,397	1,397	1,397	480	480
Number of countries	127	127	127	127	127	127	127	127	80	80

Notes:

(i) Robust standard errors in parentheses.

(ii) *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The second model reports the fixed effects specification with a spatial lag. The most striking result of this fixed effects specification is that the regression does not yield a statistically significant coefficient for the spatial lag – the point estimate falls from 0.42 to 0.06. The statistically insignificant spatial lag suggests little role for spatial interaction between countries once fixed effects are controlled for. Disentangling the effects into direct effects and indirect effects (LeSage and Pace, 2009; also discussed below) reveals that none of the indirect (i.e. spatial) effects in the second column of model 2 is statistically significant.⁸ For example, the indirect effect column of the pooled model suggests that a conflict in neighbouring countries leads to significant human rights violations in the home country (coefficient 0.65***), but the coefficient from the location fixed effects model (0.04) suggests no systematic spillover of conflict. Estimates for the benchmark models without spatial lags are reported in (3) and (4). They suggest that the addition of the spatial lag does not change other coefficients much, and, like their spatial counterparts, that controlling for location-specific time-invariant characteristics renders many explanatory variables insignificant. In the fixed effects estimates, the within R^2 is around 0.12, while the overall regression R^2 is around 0.45. Fifteen countries (just over 10 per cent of the sample) have no time variation in their human rights scores and cannot be used for identification when applying locational fixed effects. The estimates with and without fixed effects remain similar when dropping these countries from the sample.

The statistical insignificance of the spatial lag in a locational fixed effects model contrasts with the results in the pooled regression. While the pooled model includes time-invariant variation, the fixed effects model identifies coefficients from deviations from time averages. Our results thus suggest that time-invariant factors are responsible for spatial patterns in human rights. The fixed effects estimated from the baseline specification show a strong spatial pattern, too (the Moran's I of the estimated fixed effect is 0.09, with a p -value beyond the third decimal). The explanatory power of time-invariant variables is consistent with explanations of human rights performance from countries' historical settings or the physical geography, which also do not change over time. However, while consistent with historical or geographical explanations, the results are not evidence for such explanations. More importantly, our results suggest an absence of contemporaneous relations between neighbouring countries. That there is no evidence for spillovers in the time dimension (as in contrast to the cross-sectional dimension) is a key contribution of the paper. However, if interactions are not contemporaneous, one might wonder if they are relevant over other time horizons. Moreover, like most institutions, human rights practices show persistence over time. Possibly, yearly data have relatively much noise for such longer run processes.

Models (5) and (6) in Table 2 provide estimates based on a longer time period with more years in between observations. The regression uses six observations on five-year intervals from 1984 to 2009. If human rights practices have slow knock-on effects of several years or even decades, this panel is more likely to pick them up. Moreover, if noise from yearly observations obscures the identification of changes in human rights patterns, this panel should be more efficient. Using 5-year intervals leads to relatively low observation losses in the (balanced) panel. In a spatial setting, dropping countries from a geography may lead to a bias.

⁸ The matrix form of the estimating equation is $HR = \lambda WHR + X\beta + \varepsilon$ (11); its reduced form is $HR = (I - \lambda W)^{-1}(X\beta + \varepsilon)$. The direct effects are interpreted as the diagonal elements of the matrix $(I - \lambda W)^{-1}I\beta$, the indirect effects are the row sum average of off-diagonal elements.

Considering 5-year observations gives a balanced panel of 80 countries (as opposed to 127 in the 1999 to 2009 yearly sample), yearly observations over samples with longer time spans quickly reduce the number of countries. To further reduce any noise from the time dimension relative to the human rights indicator, we also estimate the same equation using three decadal observations per country from 1989 to 2009. This yields a spatial lag estimate of -0.01 (p -value 0.954).

The results based on a sample with longer time span similarly show no evidence of interactions between countries. The pooled model shows a significant and positive spatial lag, but the location fixed effects model shows no significant spatial lag or indirect effects. The longer panel shows some differences in coefficient estimates compared to the yearly panel in a shorter sample. The total effect estimates (LeSage and Pace, 2009) are comparable for yearly and five-year pooled estimates (model 1 and 5); only the inflation effects are estimated to be much lower in the longer sample. The model with location fixed effects shows more differences between the yearly panel and the five-year panel: the effects of resources and inflation are less pronounced in the five-year panel, but GDP is estimated to have significant effects in the five-year panel.

Our results differ from those of Kelejian et al. (2013), who find a spatial dependence in institutional indexes. There are (at least) two differences that could account for the variation in results: (i) the dependent variable differs (institutional indexes vs. human rights indicators); and (ii) Kelejian et al. use a generalised method of moments (GMM) set-up, foregoing the IV correction for lag endogeneity due to lack of time-varying instruments, whereas our model uses a maximum-likelihood estimator.⁹ The sample does not seem responsible for any differences: it is roughly the same size (nine-year observations over 1998 to 2008 in 129 countries). Our results do chime in with Fielding's (2014) results for the effects of aid on political rights: similarly, they show that the within-country variation paints a different picture than the between-country variation does. The difference in results with and without locational fixed effects is also consistent with earlier studies that find significant effects of globalisation on human rights and institutions when locational fixed effects are not controlled for (De Soysa and Vadlamannati, 2011; Badinger and Nindl, 2014; Law et al., 2015).

As a check on our results, we introduced a set of time-invariant controls that reflect the 'deep determinants' literature. They include legal origins, natural advantages in fuel and primary products and location and climate controls (from the World Development Indicators and the World Development Report). Table A2 in the Appendix reports the regression, along with regular pooled estimates and fixed effects estimates on the same sample. While this set of variables cannot hope to encompass the time-invariant explanations that fixed effects do, the results are similar. Estimates including the time-invariant controls based on historical and geographical conditions suggest that the spatial lag coefficient is not significantly different from zero (although somewhat higher than in the fixed effects specification). Consistently, the time-invariant controls show strong spatial patterns: for all, the Moran's I has p -value below 0.01

⁹ To compare methods, we estimated the model by instrumenting the spatial lag excluding the most likely endogenous variable (GDP, inflation, conflict). In that case, the lag is insignificant in both the pooled model (-0.09 , p -value 0.32) and the fixed effects model (0.13, p -value 0.89). In either case, the Kleibergen Paap under-identification test is easily passed, the Hansen J-test for overidentification is passed only in the fixed effect model (p -value 0.51). In the pooled estimates, dropping urban population from the spatial instruments leads to a passed Sargan test, but this yields a statistically insignificant lag. Using all instruments also yields an insignificant lag estimate.

with the exception of fuel exporters (p -value 0.07) and land area (insignificant), with the highest Moran's I scores for latitude (0.34), tropical climate (0.31) and ethnic fractionalisation (0.19).

So what are the consequences of ignoring the long-run determinants when studying human rights spillovers? Because the spatial lag model is not trivial to interpret, we provide a hypothetical experiment. We introduce the onset of a conflict in Brazil and compare the expected effects on human rights practice. We do not argue that a conflict in Brazil is likely to happen, but it illustrates the consequences of acknowledging long-run spatial patterns in human rights practice.¹⁰ We compare the models of Table 2's columns (1) and (2) in their expected shock to neighbouring economies. Figure 2 maps the differences in predicted human rights scores between the pooled and the fixed effects model. With the low estimate on the spatial lag coefficient in the fixed effects model, hardly any spillovers are expected. However, with the pooled model, significant spillovers are predicted. The mapped difference (spillover from the pooled model less spillover from the fixed effects model) almost perfectly reflects the pooled model's spillovers, as the spillovers from the fixed effects model are close to zero. The bias from the pooled model is around 0.1 in some of Brazil's neighbouring countries, especially Uruguay, Argentina and Paraguay. The bias (and the pooled model's absolute predicted spillover) is lower for countries on the Northern side of the continent, probably due to their larger distances from the most densely populated parts of Brazil. The bias is not large compared to

FIGURE 2
Bias in the Estimated Spillover from a Hypothetical Conflict in Brazil

¹⁰ In matrix form, the equation is $HR = \lambda WHR + X\beta + \varepsilon$, where W is the spatial weights matrix and λWHR is the spatial lag. In reduced form, this is $HR = (I - \lambda W)^{-1}(X\beta + \varepsilon)$. We compare the effects of a setting the conflict variable to 1 for Brazil on the (reduced-form) vector of HR scores.

the possible range of human rights scores, but it represents around 10 per cent of the decadal standard deviation in human rights performance of neighbouring countries; or in relative terms, the predicted spillover from the pooled model can be up to 12 times as large as the predicted spillover from the fixed effects model.

a. Sensitivity Analysis

The results from Table 2 suggest that there is no evidence for year-on-year spillovers in human rights practices. In this section, we discuss a number of concerns and the assumptions that the ML estimations make.

(i) Alternative Dynamics

The above model compares identification from time-variant and time-invariant variation in the data. Alternatively, we can condition the current human rights situation on that of the neighbours and the historical human rights situation. A time lag can then capture adjustment dynamics:

$$HR_{it} = \lambda \sum_{j \neq i} w_{ij} HR_{jt} + \tau HR_{it-1} + \sum_k \beta_k x_{kit} + \gamma_t + \alpha_i + \varepsilon_{it}. \quad (5)$$

This equation can be estimated using a maximum-likelihood function that takes into account both spatial and time lags (Lee and Yu, 2010b). An advantage compared to earlier approaches is that this specification allows disentangling the short and long-run effects, including differences in the short and long-run impact of neighbouring countries. A disadvantage compared to the fixed effects estimates is that the estimator relies on asymptotics along the time dimension. For the short-run direct and indirect effects, we construct the reduced form effects matrix $(I - \lambda W)^{-1} \beta$, where the average diagonal element is the direct effect, and the off-diagonal row sum average is the indirect effect. For the long-run effects, we similarly take the diagonal and non-diagonal elements of the matrix $((1 - \tau)I - \lambda W)^{-1} \beta$.

Table 3 reports the regressions using a time lag (Lee and Yu, 2010b). It estimates the model based on the same samples as Table 2. With a time lag included, the absence of spillovers persists. The coefficient on the time lag is significant in either specification, in line with high persistence of human rights scores. The time lag allows for partial adjustment, so that innovations to right-hand side variable like conflict or resource changes may take some time to materialise. To interpret the regression coefficients, we calculate the direct and indirect effects in the short run (immediate impact) and in the long run (when all time dynamics have worked out).

When location-specific attributes are controlled for, there is no evidence of spatial effects, either in the short or long run. The indirect effects of all variables are insignificant upon impact and after all dynamic adjustments have materialised. Without the fixed effects to control for location-specific characteristics, the estimates are consistent with spillovers (regressions are not reported). In the yearly sample without location fixed effects, we find short-run indirect impacts of log population (+, 29 per cent of total effect), inflation (+, 31 per cent of total) and conflict (+, 30 per cent of total); the long-run impacts are insignificant. In the (time-)lagged dependent variable model estimated using 5-year intervals without fixed effects, we find no evidence of indirect effects in the short or long run (the point estimates typically suggests lower than 1 per cent indirect effect even in the short run).

TABLE 3
Alternative Dynamics

<i>Dependent: PTS</i>	<i>(1)</i> <i>Yearly</i>	<i>Short Run</i>		<i>Long Run</i>		<i>(2)</i> <i>5 year</i>	<i>Short Run</i>		<i>Long Run</i>	
		<i>Direct</i>	<i>Indirect</i>	<i>Direct</i>	<i>Indirect</i>		<i>Direct</i>	<i>Indirect</i>	<i>Direct</i>	<i>Indirect</i>
Lagged dependent	0.21*** (0.04)					0.09 (0.06)				
Log GDP	0.10 (0.20)	0.03 (0.87)	0.00 (0.12)	-0.75 (18.49)	0.81 (18.13)	-0.60*** (0.25)	-0.61* (0.34)	0.03 (0.03)	-0.99* (2.23)	0.10 (1.01)
Log population	0.38 (0.67)	0.34 (0.76)	0.03 (0.10)	1.16 (12.77)	-0.35 (14.47)	0.81 (0.50)	0.81 (0.48)	-0.05 (0.04)	1.68 (5.66)	-0.24 (2.10)
Urban population	-0.03 (0.02)	-0.07 (0.53)	-0.01 (0.07)	-0.31 (5.16)	0.25 (5.47)	0.01 (0.02)	0.01 (0.39)	0.00 (0.03)	0.43 (3.35)	-0.09 (1.22)
Inflation	2.34*** (0.87)	2.37*** (0.88)	0.18 (0.26)	1.34*** (37.73)	1.29 (40.63)	0.25 (0.18)	0.25 (0.22)	-0.01 (0.02)	0.54 (1.92)	-0.06 (0.75)
Food index	-0.00 (1.82)	0.12 (1.45)	0.00 (0.19)	1.04 (24.23)	-0.94 (23.60)	0.51 (1.21)	0.50 (1.09)	-0.03 (0.07)	-0.43 (8.17)	0.17 (2.91)
Resources	-0.04*** (0.02)	-0.04*** (0.02)	0.00 (0.00)	0.00* (1.23)	-0.06 (1.21)	-0.01 (0.08)	-0.01 (0.08)	0.00 (0.01)	-0.03 (0.26)	0.01 (0.08)
Life expectancy	-0.03 (0.03)	-0.03 (0.04)	0.00 (0.01)	-0.08 (0.83)	0.05 (0.93)	-0.01 (0.02)	-0.01 (0.04)	0.00 (0.00)	-0.02 (0.14)	0.00 (0.05)
Conflict	0.44*** (0.13)	0.44*** (0.12)	0.03 (0.05)	0.05*** (13.36)	0.48 (13.42)	0.80*** (0.18)	0.80*** (0.18)	-0.05 (0.03)	1.31*** (3.29)	-0.16 (1.14)
Spatial lag	0.06 (0.09)					-0.20 (0.14)				
Observations	1,270					400				
Number of countries	127					80				

Notes:
(i) Robust standard errors in parentheses.
(ii) *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

(ii) Measurement

In line with most literature dealing with human rights indicators, we have used the PTS as if it is a cardinal scale. Even though the scale has been constructed on the assumption that moving 'from score 2 to 3 is the same as a move from a score of 4 to 5' (Landman and Carvalho, 2009, p. 76), the PTS basically is an ordinal scale (Wood and Gibney, 2010). This problem is circumvented in part by the use of fixed effects regressions that exploit the deviations from averages. Therefore, the initial level of human rights is accounted for in the fixed effects. A consistent but less efficient alternative is to divide the scores into 'high' and 'low' scores. This allows the model to be interpreted as a linear probability model, which does not suffer the problem of scaling the multiple scores. Even if this ignores much of the variation in outcomes, the results persist. Columns (1) and (2) of Table 4 repeat the baseline estimation with a transformed dependent variable. In particular, all observations for which the human rights score is assigned a 1 or 2 are transformed to a 0. All scores above 2 are transformed into a 1. Using the transformation, around 49 per cent of the data fall into the low score class. Using this transformation, a change in human rights score from 1 to 2 or from 3 to 4 is no longer recorded as a change in dependent variable – as a result, around two-thirds of the countries in the sample show time variation. The model gives the effect of each of the variables on the conditional probability of being in the class of high scores. Most importantly, the spatial lag is statistically significant in the pooled case, but not in the fixed effect case. This result in the linear probability model is consistent with the results in the untransformed models. Therefore, ignoring the ordinal nature of the data does not seem to affect our conclusions.

Additionally, some of our controls might be endogenous. For example, while we assume that GDP and conflicts affect behaviour of political leaders in charge, it is likely that lower human rights scores hamper economic growth or cause conflict. Unfortunately, there are no maximum-likelihood estimators that extend this spatial lag model to address the endogeneity of variables. To explore the sensitivity of the results to the endogeneity of control variables, we have excluded the variables that are most likely to generate endogeneity problems (GDP, conflict, inflation). The results are reported in column (3) of Table 4 and show no lag significantly different from zero. Hence, the potential endogeneity does not seem to affect our conclusions.

Next, it is plausible that the average coefficient on the spatial lags masks significant heterogeneity in the spatial patterns. If only a small enough share of the sample has significant interaction patterns, the full sample specification may not pick these up. Since the interaction effects are local, we divide the sample in geographical areas and check the stability of the coefficients for each of the subsamples. Following the World Bank classification of geographical zones, we have divided the sample in to: (i) Latin America and the Caribbean; (ii) Europe and Central Asia; (iii) sub-Saharan Africa, North Africa and the Middle East; and (iv) Asia. Our results show that no significant coefficient on the spatial lags is found in the fixed effects specification in any of the subsamples. This suggests that heterogeneity in the spatial dynamics does not conceal any significant effects in the full sample. In each of these subsamples, the specification without fixed effects leads to significant estimates on the spatial lags, as in the full sample.¹¹ In the Asian sample, a high correlation of controls with time fixed effects leads to unstable optimisation, so the reported model has no time fixed effects. The parameters of the control variables do show heterogeneity.¹²

¹¹ Spatial lag coefficients are statistically significant in the pooled case.

¹² Note that there is a residual World Bank category of 'Industrial countries' that have overlap with other categories and are geographically widely spread.

(iii) Normality

The maximum-likelihood function assumes normality of the errors. A Jarque-Bera test of this assumption rejects kurtosis of our error distribution as normal (the skewness, at $p = 0.54$, is not significantly different from a normal distribution). Non-normality is not surprising in our sample, because, if nothing else, the dependent variable's range is restricted, and the incidence of conflict variable hardly follows a normal distribution. In part, the non-normality seems to be driven by sample heterogeneity, as in all subsample estimations, the null that the disturbances are normally distributed cannot be rejected using the Jarque-Bera statistic. Since our findings are consistent throughout the subsamples, the non-normality of the errors does not seem to affect our conclusions much.¹³

To further investigate whether the normality assumption underlying the maximum-likelihood estimator could influence the results, we conduct additional Monte Carlo experiments. Seeing that our controls show no significant coefficients once time-invariant variation is removed, our test could effectively be run as a spatial autocorrelation measure. Hence, we run a Moran's I test of spatial autocorrelation on the levels as well as the first differences of the human rights variable.¹⁴ To avoid inference relying on the normality assumption, we randomly shuffle our observations across the locations in our sample each year to simulate the underlying distribution. This preserves the distribution of the human rights variable but compares the actual spatial correlation to one where the locations are random. Simulating 1,000 random spatial reallocations of the countries in our sample, we find that in levels, the bootstrapped Moran's I (far beyond $p = 0.01$) easily rejects random patterns in the level of human rights protection. For changes in the human rights score, we cannot reject randomness (the p -value is around 0.25). The insensitivity of the results to replacing the assumed normal distribution with the actual, simulated distribution suggests that our results are not very sensitive to the distributional assumptions of the maximum-likelihood estimator.

(iv) Alternative Human Rights Indexes

As argued before, the PTS scale may be more comparable across countries, but the PIR indicators are more disaggregated. We substituted the PTS for the scores for several elements of the PIR and some other human rights to check whether our results also hold at a disaggregated level.¹⁵ Notably, all individual elements contain sizable time variation. On average, the within standard deviation is at two-thirds of the between standard deviation, with the lowest within standard deviation at 44 per cent of the between standard deviation.

¹³ For visual inspection, Figure A1 in the Appendix provides a density plot of the errors of the full model.

¹⁴ The Moran's I statistic is:

$$I = \frac{N \sum_i \sum_j w_{ij} (HR_i - \overline{HR}) (HR_j - \overline{HR})}{\left(\sum_i \sum_j w_{ij} \right) \sum_i (HR_i - \overline{HR})^2},$$

with the null of no spatial correlation $I = -1/(N - 1)$. To generate a probability distribution, we simulated the statistic by randomly assigning countries to actual locations in the geography to generate the distribution of the test statistic under a random spatial pattern.

¹⁵ The disaggregated scores are for: the four PIR elements, assassinations, freedom of movement (both internally and externally), freedom of speech, electoral self-determination, religious freedom, workers' rights, women's rights (economic and political) and independence of the judiciary.

TABLE 4
Measurement

<i>Dependent: PTS</i>	(1) <i>LPM no FE</i>	(2) <i>LPM FE</i>	(3) <i>Exog</i>	(4) <i>Asia</i>	(5) <i>Europe</i>	(6) <i>Latin America</i>	(7) <i>Africa</i>
Log GDP	−0.16*** (0.03)	−0.02 (0.12)		0.51 (0.89)	0.11 (0.26)	0.68 (0.70)	−0.46 (0.50)
Log population	0.24*** (0.03)	1.03*** (0.37)	0.72 (0.72)	−3.55 (3.37)	−1.51 (2.01)	2.22 (2.12)	0.35 (1.65)
Urban population	0.00* (0.00)	−0.02** (0.01)	−0.03 (0.02)	0.01 (0.05)	−0.07*** (0.02)	−0.01 (0.05)	−0.06 (0.04)
Inflation	1.93*** (0.73)	−0.31 (0.58)		0.24 (3.10)	0.33 (0.67)	3.20 (2.57)	1.85 (1.17)
Food index	1.84 (1.22)	0.59 (1.05)	0.29 (1.75)	1.94 (4.00)	3.79* (2.05)	−7.39 (4.85)	−2.59 (3.64)
Resources	0.03*** (0.01)	−0.02** (0.01)	−0.05*** (0.02)	−0.09 (0.06)	−0.01 (0.01)	−0.00 (0.03)	−0.06** (0.03)
Life expectancy	0.00 (0.00)	−0.03** (0.01)	−0.05* (0.03)	0.14 (0.15)	−0.01 (0.05)	−0.05 (0.08)	−0.04 (0.03)
Conflict	0.16*** (0.05)	0.08 (0.07)		0.77** (0.30)	0.84*** (0.30)	−0.09 (0.38)	0.62*** (0.17)
Lag	0.39*** (0.12)	−0.10 (0.10)	0.04 (0.09)	−0.18 (0.17)	−0.21 (0.14)	0.12 (0.12)	−0.07 (0.14)
Observations	1,397	1,397	1,397	209	242	242	473
R-squared	0.42	0.28	0.42	0.46	0.17	0.32	0.03
Number of countries	127	127	127	19	22	22	43

Notes:

(i) Robust standard errors in parentheses.

(ii) *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

With few exceptions, the results are the same (see Appendix Table A1). When estimating the fixed effects model, no element of human rights practice shows spatial spillovers. Most regressions based on separate elements in the index do suggest spatial spillover in the pooled regression. This is in line with the results from the PTS scale. The indicators for disappearances and independence of the judiciary are the only exceptions: they do not show statistically significant autoregressive parameters both with and without fixed effects. Another potential concern with the PTS indicator is that it contains measurement errors. One reason is that the PTS score contains integers only, which implies rounding. If the use of fixed effects increases the attenuation bias (leading to larger estimated standard errors), the insignificance of the fixed effect spatial lag estimate could be attributed to an increase the fixed effect-induced bias instead the different source of variation used for identification. To gauge the size of the measurement bias, we have added extra noise to the dependent variable to check the sensitivity of the standard errors. To be conservative, we have added an error term to the human rights score that is uniformly distributed between -0.5 and 0.5 . The interval reflects all the numbers that could be rounded off to the observed human rights score, but the error term simply also represents a substantial increase in random variation in the dependent variable. Re-estimating the model with distorted human rights scores 1,000 times yields a standard error that is around 0.2 per cent higher than estimated in the original specification. To change the conclusions (reject that the spatial lag coefficient is zero), the standard error needs to be around 65

per cent lower. Given that substantial added measurement biases have a much lower impact on the standard error estimates, it seems implausible that the measurement error biases induced by location fixed effects explain the change in results.

(v) *Spatial Decay Structure*

Finally, a drawback of the spatial lag model is that a spatial decay structure needs to be imposed for identification. The spatial weights matrix's elements w_{ij} reflect the weights assigned on the relation between countries i and j . So far, we have followed a tradition of using inverse distance as weights (Anselin, 1988), which yields a power law: a doubling of distance implies that the weight is halved. A more general form would be $w_{ij} = \text{dist}_{ij}^{-\alpha}$, which allows for stronger decay effects by choosing higher values of α . Figure A2 in the Appendix provides spatial lag coefficients from the fixed effect model, including the confidence intervals. Varying the decay parameter around the inverse distance weights from $\alpha = 1/2$ upwards does not yield statistically significant estimates of the spatial lag, nor does the model fit increase much. Any misspecifications in the decay convexity therefore do not seem responsible for the change in conclusions between the pooled and the fixed effects model.¹⁶

An alternative decay structure assumes that spillovers occur between countries that share borders. This is reflected in a contiguity matrix as the weights matrix. However, 22 countries in our data set have no geographical borders with other countries, leaving them unconnected. Yet, using the CEPII border data in a row-standardised weights matrix yields qualitatively similar results. In the pooled model, the estimated autoregressive coefficient is 0.095*** (SE 0.02), while in the fixed effects model, the autoregressive coefficient is estimated to be -0.013 (SE 0.13).

5. CONCLUSION

This paper documents strong international spatial patterns in human rights practice. Such spatial patterns can form if human rights policy spills over, for instance through trade and investment interactions or policy competition. Spillovers would be relevant to international efforts to improve human rights, because beneficial external effects or 'domino' effects could imply large regional benefits. Similarly, human rights interactions could reinforce neighbouring countries' human rights violations, leading to pockets of poor human rights performance. However, the similarities between neighbouring countries may have other locational explanations: nearby countries often share colonisers, legal systems, ethnic diversity, climate and natural (dis)advantages.

Upon closer inspection, we find no evidence of interaction or spillovers. Once time-invariant locational properties are taken out of the equation, neighbouring countries seem to have

¹⁶ An alternative question in selecting weights, raised by a reviewer, is that the spillover effects may not be symmetric: the effects of Belarus on Russia may be incomparable to the mirrored effects. To weigh this issue, we have estimated the equation with inverse distance weights w_{ij} in the matrix if a country i 's start-of-sample population was smaller than country j 's – so that countries are only directly affected by larger countries. This yields qualitatively similar results: an autoregressive coefficient of 0.10 ($p = 0.02$) without fixed effects and -0.14 ($p = 0.50$) with fixed effects. However, this analysis assumes exogeneity of the initial population in the bilateral weights. That assumption is debatable, so spillover asymmetry could still be a caveat in the results.

little effect on domestic human rights records. There are no concurrent changes or, on average, countries do not seem to respond to their neighbours' human rights performance. The results reject the view that countries can easily be pressured from the outside to change their human rights situation.

Spillovers remain absent with variation in the sample length, identification in regional subsamples, and after robustness checks regarding measurement error and distributional assumptions. However, absence of evidence is not proof of absence. A potential dimension overlooked is the time span of any knock-on effects. The use of the time dimension is a contribution of the paper, but further study could be fitting still. Equally, if countries are in a strategic long-run equilibrium involving time-invariant human rights scores, the fixed effects methodology could overlook that interaction. Yet, our sample shows sizable time variation (the variation within countries over time is around half of the variation between countries). Spillovers may still be hard to identify if they do not occur within decades, but most of the trade and development policies that our paper relates to, assume spillovers that materialise within a few years, if not quicker.

The sensitivity of the results to locational attributes emphasises the importance of the choice of methodology (between pooled/cross-sectional and fixed effects models). Recently, the relevance of separating spatial heterogeneity and correlated effects from interactions has gained emphasis, and this study shows the importance of that separation. It is easy to mistake spatial processes that affect neighbours' similarly for one neighbour influencing the other. Methodologically, this is relevant to other research that studies long-run developments using spatial lags (Faber and Gerritse, 2012), but also to other studies of economic interaction in, for example economic growth (Conley and Ligon, 2002; Ertur and Koch, 2007), tax competition (Egger et al., 2005; Charlot and Paty, 2007) and international direct investments (Blonigen et al., 2007; Baltagi et al., 2008).

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APPENDIX A

FIGURE A1
Density Plot of Residual in the Benchmark Model (Table 2, column (4))

FIGURE A2
Spatial Lag Effects by Decay Parameter

TABLE A1
Spatial Lag Coefficient Estimates for Different Human Rights

	<i>Pooled Lag Coef.</i>	<i>SE</i>	<i>Fixed effects Lag Coef.</i>	<i>SE</i>
Physical integrity ^a	0.46***	0.07	−0.22	0.30
Disappearances	0.13	0.11	−0.04	0.22
Extra-judicial killings	0.54***	0.07	−0.01	0.25
Political prisoner	0.66***	0.07	−0.10	0.23
Torture	0.27*	0.10	−0.62	0.33
Assassinations	0.64***	0.07	0.06	0.26
Freedom foreign movement	0.77***	0.06	0.10	0.23
Freedom domestic movement	0.77***	0.06	0.03	0.29
Freedom of speech	0.55***	0.08	−0.15	0.38
Electoral self-determination	0.73***	0.06	−0.05	0.26
Religious freedom	0.82***	0.05	−0.06	0.23
Workers' rights	0.57***	0.08	0.18	0.26
Women's economic rights	0.28*	0.10	−0.20	0.24
Women's political rights	0.33**	0.10	−0.09	0.28
Independence judiciary	−0.03	0.10	0.12	0.20

Notes:

(i) ^aPhysical Integrity is the index that comprises disappearances, extra-judicial killings, political prisoners and torture.

(ii) *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A2
Time-Invariant Controls

<i>Dependent: PTS</i>	<i>(1) Pooled</i>	<i>(2) Pooled & Controls</i>	<i>(3) FE</i>
log GDP	−0.38*** (0.03)	−0.36*** (0.03)	−0.33 (0.39)
log population	0.53*** (0.03)	0.55*** (0.04)	1.31 (1.04)
Urban population	0.00*** (0.00)	0.01*** (0.00)	−0.04 (0.02)
Inflation	5.81*** (1.34)	3.10*** (1.31)	3.02*** (0.98)
Food index	−0.38 (1.70)	−0.25 (1.67)	−1.16 (2.26)
Resources	0.07*** (0.01)	0.01 (0.02)	−0.05*** (0.02)
Life expectancy	0.00 (0.00)	−0.01 (0.00)	−0.05 (0.03)
Conflict	0.92*** (0.06)	1.00*** (0.06)	0.49*** (0.15)
Landlocked		−0.12* (0.07)	
Ethnic fractionalisation		−0.46*** (0.10)	
Latitude		0.15 (0.12)	
Longitude		−0.19***	

TABLE A2 *Continued*

<i>Dependent: PTS</i>	<i>(1)</i> <i>Pooled</i>	<i>(2)</i> <i>Pooled & Controls</i>	<i>(3)</i> <i>FE</i>
Area		(0.04) 0.01 (0.01)	
Tropical climate		0.21*** (0.06)	
German legal origin		0.41*** (0.15)	
French legal origin		0.30** (0.12)	
British legal origin		0.48*** (0.12)	
Natural primary product exporter (non-fuel)		−0.16*** (0.06)	
Natural fuel/oil exporter		0.35*** (0.09)	
Spatial lag	0.20** (0.10)	0.11 (0.11)	−0.08 (0.12)
Observations	1,001	1,001	1,001
Number of country	91	91	91

Notes:

(i) Standard errors in parentheses.

(ii) *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.