

Where Is the Tipping Point? Bilateral Trade and the Diffusion of Human Rights

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Drawing on a panel of 136 countries over the period 1982–2004, we study a tipping point version of Vogel's 'California Effect' in the context of the diffusion of human rights practices. Because human rights practices are often deeply embedded in a society's customs and political institutions, we expect that a high level of pressure from the importing countries is needed to bring about changes in an exporting country's human rights records. We find strong empirical support for this threshold effect; provided that the average level of respect for human rights in importing countries is sufficiently high, trading relationships can operate as transmission belts for the diffusion of human rights practices from importing to exporting countries.

Whether international trade hurts or helps human rights has been extensively debated.¹ Most studies find that overall levels of trade dependence of exporting countries tend to be positively associated with higher levels of respect for human rights. Scholars note, however, that such results are often sensitive to the particular way in which measures of overall trade are operationalized.² We offer a new way to study the relationship between trade and human rights by focusing on the role of bilateral trade as a vehicle for the diffusion of human rights practices. Because much of the human rights literature focuses on the influence of overall trade on human rights, scholars overlook the possibility that trade with different partners might have different effects on an exporting country's human rights practices. We suggest that a more appropriate approach is to focus on bilateral trading relationships to understand how the varying human rights standards of the importing destinations might influence the human rights practices of the exporting

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¹ William H. Myers, 'Human Rights and MNCs: Theory versus Quantitative Analysis', *Human Rights Quarterly*, 18 (1996), 368–297; David L. Cingranelli and David L. Richards, 'Respect for Human Rights after the End of the Cold War', *Journal of Peace Research*, 36 (1999), 511–34; David L. Richards, Ronald D. Gellenny and David H. Sacko, 'Money with a Mean Streak? Foreign Economic Penetration and Government Respect for Human Rights in Developing Countries', *International Studies Quarterly*, 45 (2001), 219–39; Claire Apodaca, 'Global Economic Patterns and Personal Integrity Rights After the Cold War', *International Studies Quarterly* 45 (2001), 587–602; Thomas Cottier, 'Trade and Human Rights', *Journal of International Economic Law*, 5 (2002), 111–32.

² Emilie M. Hafner-Burton, 'Right or Robust? The Sensitive Nature of Repression to Globalization', *Journal of Peace Research*, 42 (2005), 679–98.

countries. Our approach is analogous to what Vogel has termed the ‘California Effect’.³ Its core idea is that international diffusion of policies and practices depends not on *how much* a country trades but *with whom* it trades.

One difficulty that states face in trying to restrict imports from countries with poor human rights practices is that the World Trade Organization constrains the ability of importing countries to regulate imports using process-based rules. Therefore, many observers expect that international trade will abet regulatory races to the bottom. In challenging this logic, Vogel has shown that under some conditions, increasing exposure to international trade will instead lead to a ratcheting up of environmental laws and regulatory standards. In the current study we extend this argument to the diffusion of human rights standards. Unlike the environmental regulations that were the focus of Vogel’s study, a country’s human rights standards involve a set of norms and practices that tends to be less formal and less strongly legalized. As a result of our analysis, we are able to develop further scope conditions concerning the ability of bilateral trade pressures from importing countries to bring about policy changes in exporting countries.

Drawing on prior research which shows non-linear effects of democracy on human rights,⁴ we hypothesize that the relationship between bilateral trade and human rights might also be non-linear. Unlike a number of studies of bilateral trade-induced changes such as regulatory standards regarding vehicle emissions,⁵ firms’ adoption of the ISO 14001 environmental standards,⁶ and labour rights,⁷ we begin with the assumption that given the wider political and social changes that are required to be made in order to improve a country’s respect for human rights, the relationship between bilateral trade and physical integrity rights will not follow the traditional linear path. To model the non-linearities in the hypothesized relationship, we draw on the notion of a ‘tipping point’. Our argument is that the effects that the human rights practices of a country’s export destinations will have on the exporting country’s own human rights practices will be apparent only above a certain threshold level of pressure. This is because human rights practices are deeply embedded in social customs and political institutions. Some actors have an interest in perpetuating the status quo. Without some minimum level of ‘push’ from importing destinations via the instrumentalities of bilateral trade, the resistance from such actors cannot be overcome.⁸

³ David Vogel, *Trading Up: Consumer and Environmental Regulation in a Global Economy*. (Cambridge, Mass.: Harvard University Press, 1995). Also see Thomas Bernauer and Ladina Caduff, ‘In Whose Interest? Pressure Group Politics, Economic Competition and Environmental Regulation’, *Journal of Public Policy*, 24 (2004), 99–126.

⁴ Christian Davenport and David A. Armstrong, ‘Democracy and the Violation of Human Rights: A Statistical Analysis from 1976 to 1996’, *American Journal of Political Science*, 48 (2004), 538–54.

⁵ Vogel, *Trading Up*.

⁶ Aseem Prakash and Matthew Potoski, ‘Racing to the Bottom? Globalization, Environmental Governance, and ISO 14001’, *American Journal of Political Science*, 50 (2006), 347–61.

⁷ Brian Greenhill, Layna Mosley and Aseem Prakash, ‘Trade-based Diffusion of Labor Rights: A Panel Study, 1986–2002’, *American Political Science Review*, 103 (2009), 669–90.

⁸ The notion of threshold effects in policy adoption has been discussed by Thomas C. Schelling, *Micromotives and Macrobehavior* (New York: W. W. Norton: 1978); Mark Granovetter, ‘Threshold Models of Collective Behavior’, *American Journal of Sociology*, 83 (1978), 1420–43; and, more recently, in Martha Finnemore and Kathryn Sikkink, ‘International Norm Dynamics and Political Change’, *International Organization*, 52 (1998), 887–917; Malcolm Gladwell, *The Tipping Point: How Little Things Can Make a Big Difference* (Boston, Mass.: Little, Brown, 2000); Beth A. Simmons and Zachary Elkins, ‘The Globalization of Liberalization: Policy Diffusion in the International Economy’, *American Political*

In addition to testing the notion of threshold effects empirically, our study provides a systematic treatment of the issue of endogeneity. While trade can influence the human rights practices of exporting countries, one might argue that human rights practices can also influence the exporters' decisions regarding trading destinations. In other words, while actors in importing countries might seek to influence human rights in exporting countries, it is possible that some exporting firms may choose to trade only with countries that are more concerned about the human rights practices of their trading partners. To address this potential selection problem, we employ a propensity score matching technique that allows us to draw more confident causal inferences from observational data. This technique requires the analyst to identify two subsets of the original dataset in a way that most closely resembles the 'treatment' and 'control' arms of a randomized experiment. In practical terms, this involves finding two sets of country-year cases: one whose value of the key independent variable (in our case, the trade-weighted average human rights performance of each country's trade partners) is high, and one whose value is low, while the distribution of all other variables between the groups are roughly the same. Once these matched sets have been found, the effect of the key independent variable can be estimated in a standard regression model.⁹ The use of the matching technique provides additional confidence that the effect of the bilateral trade context on the exporting countries' human rights practices can be observed only after a certain threshold is reached.

In the rest of the article, we first introduce the notion of a California Effect in human rights and explain the logic of the threshold model. We then discuss the key variables. The following section provides a discussion of matching analysis and uses this technique to test the relationship between bilateral trade and human rights. In the final section, we discuss various theoretical and policy implications of our study and comment on directions for future research.

BILATERAL TRADE, THE CALIFORNIA EFFECT AND HUMAN RIGHTS

In challenging the logic of the race-to-the-bottom hypothesis, Vogel coined the term 'California Effect' to describe the mechanism by which (importing) jurisdictions with higher standards are able to transmit their regulatory standards to (exporting) jurisdictions with lower standards.¹⁰ Vogel used the term to describe the way in which high air quality standards in the state of California (or Germany in the context of Europe) have led to the ratcheting-up of formal laws and environmental standards throughout the

(*F*note continued)

Science Review, 98 (2004), 171–89. These models examine threshold effects in the context of the international system; i.e. whether the probability of policy adoption in a given country increases once a certain number or percentage of states in the international system have adopted this policy. In this article we employ a somewhat different notion of a 'threshold effect'. We are interested in examining whether a critical level of pressure is required before a change is observed. For example, suppose that we can measure bilateral trade pressure applied by country B on country A on a scale of 1 to 10. Our argument is that this pressure from B will have observable behavioural changes in A only if this pressure exceeds a minimum value of, say, 6. Below this level, the pressure will not be expected to lead to behavioural changes in A.

⁹ Daniel E. Ho, Kosuke Imai, Gary King and Elizabeth A. Stuart, 'Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference', *Political Analysis*, 15 (2007), 199–236.

¹⁰ Vogel, *Trading Up*.

United States. The state of California, which represents a major market for the sale of cars produced elsewhere in the United States, has been a pioneer in the adoption of strict air quality standards. Vogel observes how, against the background of these strict standards, Californian regulators have been able to use the large relative market size of their state to induce car manufacturers located elsewhere in the United States to adopt standards that comply with Californian law. In this way, the combination of significant purchasing power and tough regulatory standards in one state has led the other states to engage in a 'race to the top' with respect to their own regulatory standards.¹¹

This article employs the notion of the California Effect in a slightly different way. Instead of looking at the legal and regulatory standards stipulated by public authorities, we examine the actual practices of states. That is, we do not examine how bilateral trade influences the passing of human rights legislation; rather, we are interested in examining how it influences human rights behaviour across countries. This is an important distinction to make, because a large gap exists between the formal laws protecting human rights and the actual practices of many countries. This was less of an issue for Vogel's study because compliance with product standards in the United States (and Europe) tends to be very high, and each state's formal laws on air quality can, therefore, serve as a reasonably reliable indicator of the actual emissions levels found within that state. Unfortunately, the same cannot be said of international human rights treaties; in many cases the ratification of international human rights treaties actually appears to be associated with worse, rather than better, human rights performance.¹²

Vogel made the original 'California Effect' argument in the context of environmental standards embodied in products (for example, the emissions standards of the cars), thereby raising the question of whether this argument can be extended to process standards (such as the various environmental issues that arise during the manufacture of the cars) given that the World Trade Organization (WTO) does not allow importing countries to subject their imports to process standards stipulated in public regulations.¹³ Activist groups commonly complain about this aspect of the WTO, and argue that by preventing importing countries from engaging in this type of discrimination, countries around the world will be forced to enter into a 'race to bottom' with respect to their environmental and human rights standards.¹⁴ However, there is obviously nothing to stop *non-governmental* actors from attempting to do so. Concerned customers and activist groups are still able effectively to limit imports via boycotts and name-and-shame campaigns. As a result, exporting countries might have incentives to improve certain process standards in response to demands from consumers, stakeholders and activists located in importing countries in spite of the WTO's restrictions on such action being taken at the inter-governmental level. Indeed, two recent studies of the diffusion of

¹¹ Vogel, *Trading Up*.

¹² Emilie M. Hafner-Burton and Kiyoteru Tsutsui, 'Human Rights in a Globalizing World: The Paradox of Empty Promises', *American Journal of Sociology*, 110 (2005), 1373–411; Oona A. Hathaway, 'Do Human Rights Treaties Make a Difference?' *Yale Law Journal*, 118 (2002), 1935–2042; James Raymond Vreeland, 'Political Institutions and Human Rights: Why Dictatorships enter into the United Nations Convention Against Torture', *International Organization*, 62 (2008), 65–101.

¹³ Unlike the vehicle emissions standards discussed in Vogel's study, the human rights practices of exporting countries would be an example of a process, rather than product, standard.

¹⁴ Stephan Gill, 'Globalisation, Market Civilization and Disciplinary Neoliberalism', *Millennium*, 24 (1995), 399–423; but see Daniel Drezner, 'Globalization and Policy Convergence', *International Studies Review*, 3 (2001), 53–78.

regulatory standards have suggested that the California Effect holds in the context of process standards – specifically, the ISO 14001 environmental management standard¹⁵ and collective labour rights.¹⁶

Once a trading relationship has been established between a pair of countries, firms located in the importing country can exert pressure on firms in the exporting country to adopt certain types of practices. In the case of environmental standards, this might take the form of firms in the importing country putting pressure on their foreign suppliers to improve their environmental performance in order to allow the importing firm to provide their stakeholders with evidence of an environmentally ‘clean’ supply chain leading all the way back to the fabrication or assembly site. In the case of human rights practices, a similar logic can be thought to operate: firms located in countries with strong levels of commitment to human rights are likely to want to demonstrate to their stakeholders that they are sourcing their supplies from countries with acceptable human rights standards. When the importing countries have a sufficiently high level of concern for human rights standards, we can expect the exporting countries to come under significant pressure to improve their own human rights practices. These pressures will be felt by both the exporting firms and their governments, given that both sets of actors have a shared interest in maximizing exports.

There is some qualitative evidence to support the claim that this sort of bilateral trade-based mechanism can be effective in influencing the human rights performance of exporting countries. Within importing countries, one frequently sees networks of issue-specific NGOs, trade unions and consumer groups mobilizing in an attempt to use trade as a means of coercing foreign governments to change their human rights practices. For example, the annual exercise which was conducted by the United States Congress to renew China’s Most Favoured Nation trading status (prior to China’s accession to the World Trade Organization in 2001) reflected the leverage that trading relations provide to groups based in the United States that sought to influence China’s human rights practices.¹⁷ Furthermore, activist groups in importing countries routinely make recourse to ‘private politics’ to influence the human rights practices of the countries in which goods are produced.¹⁸

Recent attempts by consumers in specific importing countries to boycott apparel manufacturers that were found to use prison labour or child labour or to employ other abusive practices in their overseas production facilities have achieved some important successes.¹⁹ A campaign targeted at the carpet industry within importing countries has resulted in a significant decline in the use of child labour among their suppliers.²⁰ Attempts by consumers in specific importing countries to boycott apparel manufacturers that were found to use prison labour or child labour or to employ other abusive practices

¹⁵ Prakash and Potoski, ‘Racing to the Bottom? Globalization, Environmental Governance, and ISO 14001’, *American Journal of Political Science*, 50 (2006), 347–61.

¹⁶ Brian Greenhill, Layna Mosley and Aseem Prakash, ‘Trade-based Diffusion of Labor Rights: A Panel Study, 1986–2002’, *American Political Science Review*, 103 (2009), 669–90.

¹⁷ Robert Drinan and Teresa Kuo, ‘The 1991 Battle for Human Rights in China’, *Human Rights Quarterly*, 14 (1992), 21–42.

¹⁸ David P. Baron, ‘Private Politics’, *Journal of Economics & Management Strategy*, 12 (2003), 31–66.

¹⁹ Ans Kolk and Rob Van Tulder, ‘Multinationality and Corporate Ethics: Codes of Conduct in the Sporting Goods Industry’, *Journal of International Business Studies*, 32 (2001), 267–83.

²⁰ Janet Hilowitz, ‘Social Labelling to Combat Child Labour: Some Considerations’, *International Labour Review*, 136 (1997), 215–23.

in their overseas production facilities have achieved some important successes. To illustrate, campaigns in importing markets to dissuade carpet exporters from employing child labour have resulted in a significant decline in the use of child labour. Concerns about the use of child labour in the carpet industry in the Indian subcontinent began receiving media attention in the 1980s. Several policy measures were proposed but these initiatives were not able to make a significant dent in the problem. Key actors, therefore, decided to make recourse to trade pressures by employing 'social labels' which allowed importers in developed countries to convey their preferences about human rights to manufacturers in the subcontinent. Two social labels, 'Rugmark' and 'Care & Fair', deserve particular mention.

Rugmark was an initiative of non-governmental organizations, while Care & Fair was initially an initiative of German carpet importers and retailers.²¹ While their programme designs vary, the key idea is that concerned buyers can use the information conveyed by the labels to create an economic incentive for carpet manufacturers to respect human rights, especially the rights of children. Actors in two key markets, Germany and the United States, mounted an effective campaign to ensure that carpets imported from the Indian subcontinent carry these labels. Indeed, recognizing the economic downside of ignoring this sensitive issue, some state or provincial governments in India became active on this issue themselves and launched initiatives such as establishing schools for children who had hitherto been working on carpet looms. While it is difficult to assess the exact contribution that these social labelling instruments have made to the decline in the use of child labour in the carpet weaving industry, anecdotal evidence suggests that these labels played an important role in transmitting preferences of the dominant import markets, Germany and the United States, and thereby shaping the policies and preferences of governments in the exporting countries, particularly India, Pakistan and Nepal.

More generally, American labour and human rights groups routinely oppose imports from countries that practise exploitative labour practices or violate human rights and thereby put pressure on exporting countries (such as Mexico or China) to improve their human rights performance. If these cases are reflective of a more general trend in which importing countries can exert upward pressure on the human rights standards of the exporting countries, we should expect to find evidence of a positive correlation between the human rights practices of a country and those of its export destinations, all else being equal.

However, our causal story differs from Vogel's in important ways. In the context of the environmental legislation that Vogel studied, economies of scale motivated companies to push for California's emissions standards to be adopted across the United States, and Germany's standards across Europe. In contrast, the human rights case that we examine does not involve the diffusion of specific pieces of legislation from the importing countries to the exporting countries. Rather, it pertains to the transmission of a more informal and less legalized set of norms concerning human rights. Because improvements in human rights practices require the exporting state to make changes that go beyond simply passing a new piece of legislation, we argue that the level of pressure required to bring about positive change is much higher (hence the threshold effect).

Moreover, in the case of a country-level standard such as the country's human rights practices, this economies of scale argument is less relevant because an exporting country

²¹ Rugmark (founded by Kailash Satyarthi in 1994) has now been renamed Goodweave; see goodweave.org. Care & Fair (care-fair.org) was also founded in 1994.

cannot pretend to have different human rights practices for each of its export markets. If a country frequently engages in acts of torture or extrajudicial killings, it cannot claim that the goods it produces for sale to, for example, the European market are not tainted by the same human rights violations as the goods that it produces for sale to other (less discriminating) markets. This is because human rights violations are associated with the country of origin, rather than the specific process through which the goods are produced. To put it differently, if consumers were concerned with specific process standards alone (such as the non-use of child labour in the South Asian carpet industry), then we could imagine a country like Pakistan being able to produce Rugmark-certified carpets for some markets while also producing non-certified carpets for other markets. However, when the consumers are concerned with human rights in the country as a whole, it is not possible for an exporting country to apply different sets of standards to the goods it produces for different markets.²²

In Figure 1, we provide a summary of the various possible causal mechanisms connecting the human rights practices of exporting countries to those of their export destinations. We identify the following potential causal pathways: first, consumers and activist groups in the importing country can lobby their own government to put pressure on the government of the exporting country to improve its human rights practices. Second, consumers and activist groups in the importing country can target multinational corporations (MNCs) located in their country that have business interests in the exporting country. Once these MNCs are sufficiently concerned about their ability to sell products in the importing country, they can bring pressure to bear on the government of the exporting country, either directly or via the threat of reducing business with local firms. Finally, local exporting firms in the exporting country that are concerned about losing access to foreign markets will also have strong incentives to lobby their own government to improve its human rights practices.

We take the theoretical argument of the California Effect forward by hypothesizing that while a California Effect might be observed in the context of broader society-wide standards such as human rights, the trade-based diffusion of such standards is unlikely to operate in a linear fashion. When the California Effect concerns regulatory standards that are not so closely connected with domestic politics and broader social practices and, therefore, can be easily implemented by the exporting firm themselves – as is the case for vehicle emission standards or firms' adoption of the ISO 14001 environmental management standard – we can expect the exporting firms' levels of adoption of the standards to be fairly responsive to the standards required of their export destinations. In cases such as these the key actor is the firm and its decision to adopt superior practices does not necessarily have significant ramifications for the wider society. Arguably, other societal actors lack the interest and opportunities to veto or influence such firm-level

²² There are other examples to support the claim that consumers use a strategy of boycotts in an attempt to change the human rights practices of a country as a whole, e.g. the consumer boycott of South Africa during the apartheid regime, the boycott of Israeli goods regarding the Palestinian issue, and the boycott of Chinese products in response to China's Tibet policies. For a listing of some consumer boycotts being practised in Britain, see <http://www.ethicalconsumer.org/Boycotts/currentboycottlist.aspx>. While effective labelling of the product or the process helps consumers and activist groups to target specific countries and firms, even in the absence of labelling, countries come under pressure to check human rights violations. This can be found in the case of petroleum, mining (see the recent initiatives on banning the import of materials from conflict zones), and even oil (Nigeria). By looking at total trade and not distinguishing between industries which may or may not have labelled products, this article sets a relatively hard test of the role of bilateral trade pressure on exporting countries' human rights standards.

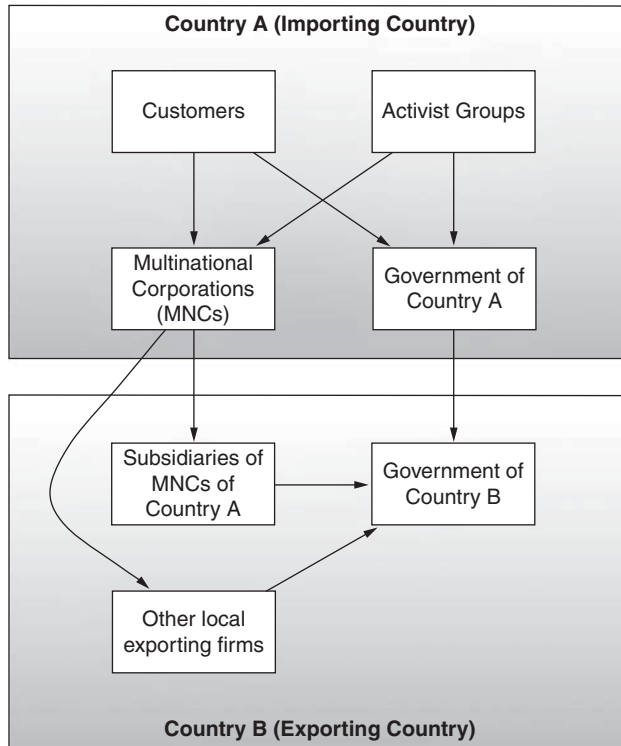


Fig. 1. California Effect in human right: Causal chains

Note: This casual diagram illustrates some of the potential causes chains connecting actors in importing countries to actors in exporting countries. Pressure for better human rights practices often begins with consumers and activist groups in the importing countries (represented by Country A in this diagram). One way for these actors to put pressure on the exporting countries (represented by Country B in this diagram) to improve their human rights standards is for the consumers and activist groups in Country A to lobby and pressure their own government directly, which, in turn, pressures the governments of the exporting countries. Alternatively, the consumers and activist groups in Country A can target multinational corporations (MNCs) located in their country that have business interests in Country B. Once they are sufficiently concerned about their ability to sell products in Country A, the MNCs can then bring pressure to bear on the government of Country B – either directly or via the threat of reducing business with local firms in Country B.

environmental policy level changes. As a result, the prevailing environmental standards/practices among the firms in the importing countries can be expected to be reflected in the standards of firms located in the exporting countries.

However, for changes in standards with wider social ramifications – such as human rights – that require behavioural changes on the part of the exporting country's government or the entire society (and not just its exporting firms), we can expect to find more significant resistance to change. Presumably, much greater pressure from the importing countries is needed to bring about changes in the existing patterns of behaviour that may be much more deeply embedded in the society's customs and political institutions. We would, therefore, expect to find a positive effect of trade-induced change in human rights behaviour only after a sufficiently high level of pressure has been exerted – in other words, after a particular threshold has been reached.

Our argument about a threshold effect is consistent with the notion of ‘tipping points’ that constructivist scholars have developed in the study of diffusion of norms.²³ Constructivist scholars have sought to explain change in international politics by emphasizing the role that ‘norm entrepreneurs’ play in promoting new ideas about appropriate forms of behaviour. These new ideas, however, tend to be met with significant resistance in the early phase and only gain traction in the international system once a critical mass of states has begun to adopt the norm. Once this tipping point is reached, near-universal transmission of the norm becomes much more likely. While Finnemore and Sikkink use the concept of the tipping point to refer to the critical proportion of states that needs to have adopted a norm before the so-called ‘norm cascade’ is triggered, we operationalize the concept of the tipping point slightly differently. For us, a tipping point is reached when the average level of respect for human rights in the importing countries is sufficiently strong to send an unambiguous signal to the exporting country about the importance that its importers attach to human rights standards.²⁴ Once this point is reached, exporting countries are far more likely to change their behaviour to bring their own human rights standards in line with those found among their export markets. Where this point lies is an empirical question that we address in the following sections of the article.

DATA

To investigate how bilateral trade influences exporting countries’ human rights practices, we examine a panel of 136 countries for the period 1982–2004. Our unit of analysis is the country-year. Our dependent variable is the Physical Integrity Rights Index developed by Cingranelli and Richards.²⁵ Our key explanatory variable is the average physical integrity rights index of a country’s export partners, weighted by the salience of each trading relationship in the country’s export basket. We call this variable the *Bilateral Trade Context*. If a California Effect exists in the realm of human rights, we should expect to find that, after controlling for the various factors that determine a country’s choice of trading partners, sufficiently high values of Bilateral Trade Context are associated with higher levels of respect for human rights at home, all else being equal.

The Physical Integrity Rights Index (hereinafter *PIR Score*) provides a composite measure of the extent to which a country respects four basic types of human right that are most closely associated with protecting the personal safety of its inhabitants.²⁶ The level

²³ Finnemore and Sikkink, ‘International Norm Dynamics and Political Change’. On the spiral model of norm diffusion, see Thomas Risse, Stephen C. Ropp and Kathryn Sikkink, eds, *The Power of Human Rights: International Norms and Domestic Change* (Cambridge: Cambridge University Press, 1999).

²⁴ Our measure of the average human rights practices of each country’s export destinations (*Bilateral Trade Context*) weights the human rights practices of each destination country by the proportion of total exports of the exporting country in each period. Thus a high value of *Bilateral Trade Context* reflects the fact that the most salient countries in a given country’s export basket have a high human rights score.

²⁵ The Cingranelli–Richards (CIRI) Human Rights Dataset, www.humanrightsdata.org. Dataset Version 2007.04.12, accessed June 2007.

²⁶ Given that we are interested primarily in the most egregious forms of human rights abuse – i.e., those that involve acts of physical harm carried out by the state against its inhabitants – we are faced with only two choices of worldwide data: Cingranelli and Richard’s 9-point ‘Physical Integrity Rights Index’ or Gibney and Dalton’s 5-point ‘Political Terror Scale’. Although both scales are compiled from the same underlying data sources (the annual country reports produced by Amnesty International and the US State Department), we chose to use the Physical Integrity Rights Index because it provides a more fine-grained

of respect for these rights is measured by the extent to which a country engages in the following four categories of rights violations:

- Torture;
- Imprisonment on the basis of ethnic identity, racial identity, religious practices, or involvement in non-violent political activities;
- Extra-judicial killings carried out either by the government or by private groups supported by the government; and
- Disappearances: undocumented acts of imprisonment or extra-judicial killing.

Drawing on the annual human rights reports published by both Amnesty International and the US State Department, Cingranelli and Richards have assigned a score of 0, 1 or 2 to each country for every year depending on whether violations of each of these four rights occurred more than fifty times in the year (resulting in a score of 0), between one and fifty times (resulting in a score of 1), or not at all (resulting in a score of 2). By adding together the score in each of the four categories, Cingranelli and Richards arrive at a composite score (ranging from 0 to 8) that measures each country's general level of respect for physical integrity rights in a given year.²⁷

The independent variable we use to test whether the California Effect operates in the context of human rights, *Bilateral Trade Context*, is constructed by calculating a weighted average of the *PIR Score* of each country's export destinations, where the PIR scores are weighted by the share of exports sent to each destination. This gives a measure of the trade weighted average levels of respect for physical integrity rights found among each country's trading partners. The value of this variable for country i at time t can be expressed as:

$$\text{Bilateral Trade Context}_{it} = \sum_j \text{PIR}_{jt} \times \text{Exports}_{ijt} / \text{Exports}_{it},$$

where PIR_{jt} is the PIR score of country j at time t , Exports_{ijt} is the level of exports from country i to country j at time t , and Exports_{it} is country i 's total exports at time t . Figure 2 displays the temporal variation in *Bilateral Trade Context* for eight countries from 1981 to 2004.²⁸

(*Note continued*)

measure of physical integrity rights. As a robustness check (detailed results are reported in an online appendix to this article), we re-estimated the model while replacing the Physical Integrity Rights Index with the Political Terror Scale. The results obtained using the variant of the Political Terror Scale derived from the US State Department's country reports were consistent with the results obtained from the original analysis; however, the results obtained using the variant of the Political Terror Scale derived from Amnesty International's country reports did not show a statistically significant effect for our key independent variable, *Bilateral Trade Context*. We suspect that this is due to the larger number of missing observations in the Amnesty International variant, rather than a qualitative difference between the two sources. For more on the relative merits of the Physical Integrity Rights Index and the Political Terror Scale, see David L. Cingranelli and David L. Richards, 'The Cingranelli and Richards (CIRI) Human Rights Data Project', *Human Rights Quarterly*, 32 (2010), 401–24; and Reed Wood and Mark Gibney, 'The Political Terror Scale (PTS): A Re-introduction and a Comparison to CIRI', *Human Rights Quarterly*, 33 (2010), 367–400.

²⁷ Subsequent cross-national comparison of countries' performance with respect to each of these categories of rights abuse suggests that the four separate categories of rights protections constituting the Physical Integrity Rights Index are hierarchically related and can on this basis be meaningfully aggregated into a single measure of how a country fares with respect to physical integrity rights (see David L. Cingranelli and David L. Richards, 'Measuring the Level, Pattern, and Sequences of Government Respect for Physical Integrity Rights', *International Studies Quarterly*, 43 (1999), 407–17).

²⁸ Data on bilateral trade were obtained from the IMF's Direction of Trade Statistics database.

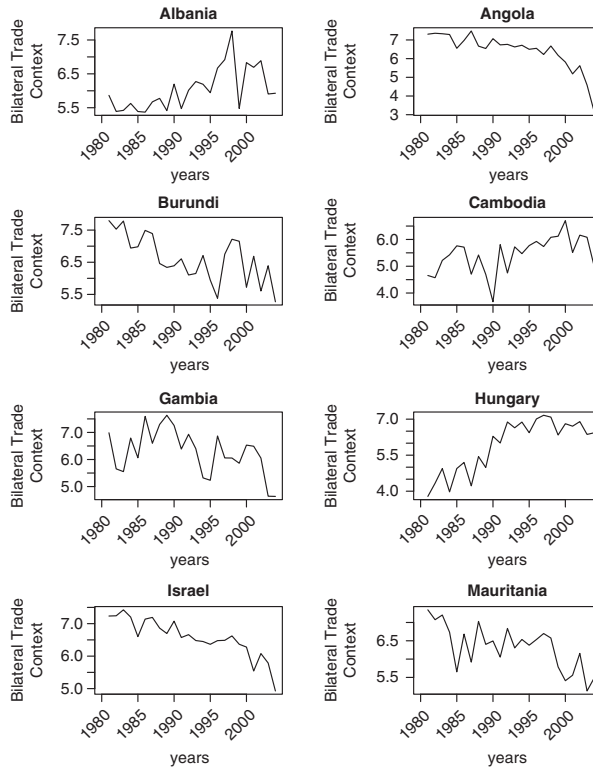


Fig. 2. Temporal variation in Bilateral Trade Context for a randomly selected sample of eight countries

Our model controls for a number of variables that could plausibly influence a country's human rights record. One of the most widely studied factors is a country's overall dependence on foreign markets.²⁹ This is operationalized as the country's total exports plus imports expressed as a percentage of its gross domestic product or GDP (*Total Trade*).³⁰ Analogous to trade, one might suspect that a country's human rights performance can be influenced by levels of foreign direct investment (FDI).³¹ As in the case of trade, conflicting arguments can be put forward regarding the effect of FDI on human rights practices. Globalization pessimists suggest that because foreign direct investors seek to lower their labour costs, they tend to gravitate towards jurisdictions that show scant respect for human rights and countenance exploitation of labour. According to this perspective, countries that seek to attract foreign investment are likely to engage in a 'race to the bottom' with respect to human rights

²⁹ Ronald B. Mitchell and James McCormick, 'Economic and Political Explanations of Human Rights Violations', *World Politics*, 40 (1988), 476–98.

³⁰ Our results hold even when we replace *Total Trade* by exports as proportion of GDP. Because most work in this area tends to employ total trade dependence as a covariate, to maintain consistency, we employ it as well.

³¹ Stephen H. Hymer, *The International Operations of National Firms: A Study of Direct Foreign Investment* (Cambridge, Mass.: MIT Press, 1976).

standards.³² These countries are likely to use their security apparatus to subdue labour and citizens' groups who might protest against their exploitative practices. Globalization optimists, by contrast, suggest that by facilitating economic development, foreign direct investment can help to raise countries' human rights standards. This more optimistic view of the effect of FDI on human rights standards finds some support in recent quantitative studies of human rights abuse. For example, Apodaca finds that during the period 1990–96, FDI (as well as trade) leads to a statistically significant decrease in levels of abuse of physical integrity rights as measured by an alternative indicator of physical integrity rights, the 'Political Terror Scale'.³³ Other results for FDI are mixed; Richards *et al.* report a positive association between FDI and their measure of 'political rights and civil liberties', but fail to find a statistically significant association between FDI and physical integrity rights.³⁴ Therefore, our model includes a measure of each country's exposure to FDI (*Inward FDI*) that is made up of inward flows of FDI expressed as a percentage of that country's GDP.

Our model also controls for the wealth of a country expressed in terms of GDP per capita. Previous studies have consistently reported wealth to be positively associated with improved human rights performance presumably because economic development empowers citizens to demand that their governments respect their basic human rights. At the same time, development provides resources to the government to supply institutions that can respond to such demands.³⁵ Data for *Total FDI* as well as *GDP per capita* (expressed in constant 2000 US dollars) were obtained from the World Bank's World Development Indicators Online database.

Domestic political institutions are also likely to have an important influence on a country's human rights practices. While activist groups have often led the way in demanding that governments improve their human rights practices, such groups need a

³² David L. Richards, Ronald D. Gellens and David H. Sacko, 'Money with a Mean Streak? Foreign Economic Penetration and Government Respect for Human Rights in Developing Countries'; Cingranelli and Richards, 'Respect for Human Rights after the End of the Cold War'.

³³ Apodaca, 'Global Economic Patterns and Personal Integrity Rights after the Cold War'; Mark Gibney, Linda Cornett and Reed Wood, *Political Terror Scale*. (available at <http://www.politicalterror scale.org> (2008)).

³⁴ Richards, Gellens and Sacko, 'Money with a Mean Streak?'

³⁵ Like many studies in the literature, we assume a linear relationship between wealth and human rights. However, as a robustness check we also tried testing for non-linearities in the relationship between GDP per capita and human rights by replacing the *GDP per capita* variable with a dummy variable indicating whether the country's GDP per capita exceeds \$1,000. This alternative specification did not lead to significant change in the estimated effect of our key independent variable, *Bilateral Trade Context*. As a separate robustness test, we also decided to check whether a country's human rights performance may be affected by both the mean income of the country and also its income distribution. We experimented with the inclusion of the Gini coefficient variable to control for this potential distribution effect of income (data are from the World Income Inequality Database: <http://www.wider.unu.edu/wiid/wiid.htm>). The Gini coefficient varies theoretically from 0 (perfectly equal distribution of income) to 100 (the society's total income accrues to only one person/household unit). We find that in some model specifications, the Gini coefficient has a statistically significant and negative relationship to human rights, suggesting that countries with higher income inequality tend to have worse human rights practices. However, this relationship is not robust across all model specifications. Moreover, a large number of missing observations are introduced by including the Gini coefficient: for the model before matching, the number of observations is reduced by 1,433 – more than 50 per cent of observations. For models after matching, this scale of loss in the number of observations makes model estimation difficult. Therefore, we choose not to include this inequality variable for models reported in this article.

certain amount of political space to be able to work effectively. If they are not allowed to voice their opinions freely, to organize and to protest, their ability to put pressure on the government will be severely diminished.³⁶ To operationalize political openness (*Democracy*), we employ the 'Polity 2' measure of democracy provided by the Polity IV database.³⁷ While scholars have tended to employ a variety of indicators to assess levels of political openness, Polity 2 is best suited to capture the institutional dimensions of political openness and, therefore, serve as a proxy for the political space available for the domestic implantation of human rights practices transmitted from abroad.³⁸ Furthermore, because Polity 2 is coded on a 21-point scale that ranges from -10 to $+10$ (reflecting the most autocratic and most democratic regimes respectively), it is sufficiently textured to pick up variations in domestic political institutions across countries.³⁹

³⁶ On how transnational advocacy networks can lessen the constraining effects of domestic opportunity structures, see Margaret E. Keck and Kathryn Sikkink, *Activists beyond Borders: Advocacy Networks in International Politics* (Ithaca, N.Y.: Cornell University Press, 1998).

³⁷ Monty G. Marshall and Keith Jagers, *Polity IV Data Set* [Computer file; version p4v2002] College Park, MD: Center for International Development and Conflict Management, University of Maryland, 2002. Human rights scholars have operationalized regime types differently. For example, while Kirkpatrick suggests that revolutionary autocracies are more likely to violate human rights than traditional autocracies, Howard and Donnelly find liberal regimes to be more respectful of human rights in relation to communitarian regimes. See Jeanne J. Kirkpatrick, *Dictatorships and Double Standards* (New York: Simon and Schuster, 1979); Rhoda E. Howard and Jack Donnelly, 'Human Dignity, Human Rights, and Political Regimes', *American Political Science Review*, 80 (1986), 801–18. In a detailed examination of the relationship between democracy and human rights, Davenport and Armstrong point out that many human rights scholars are wrong to assume a linear relationship between democracy and human rights (Christian Davenport and David A. Armstrong, 'Democracy and the Violation of Human Rights: A Statistical Analysis from 1976 to 1996', *American Journal of Political Science*, 48 (2004), 538–54). They show the relationship can be more accurately modelled as one in which democracy has a positive effect on human rights practices only once a certain threshold has been reached. In attempt to capture these non-linear effects of democracy, we tried replacing the Polity 2 measure of democracy with several variants of a three-point measure that indicates whether the Polity 2 score is low, medium or high. We found that the estimated effect of our key independent variable, *Bilateral Trade Context*, remained robust to these alternative specifications of the model.

³⁸ This article does not focus on how different components of democracy influence human rights. Bruce Bueno De Mesquita, Feryal Marie Cherif, George W. Downs and Alastair Smith, 'Thinking Inside the Box: A Closer Look at Democracy and Human Rights', *International Studies Quarterly*, 49 (2005), 439–58, provides evidence for the variations in the effects of different components of democracy on human rights practices. However, the contribution we seek to make pertains to the threshold effect of bilateral trade context on human rights while controlling for other variables such as democracy. Moreover, different components of democracy and the aggregate value of the Polity score are highly correlated: the correlations between the combined Polity score and its components, such as Regulation of Participation, the Competitiveness of Participation, the Regulation of Chief Executive Recruitment, the Competitiveness of Executive Recruitment, the Openness of Executive Recruitment, and the Executive Constraints, are over 0.90 for the countries and years covered by this study.

³⁹ In a series of robustness checks, we also tested whether our results are affected by the inclusion of dummy variables indicating the presence of a military regime or a 'leftist' regime. Poe and Tate and other studies (e.g., Linda Camp Keith, 'The United Nations International Covenant on Civil and Political Rights: Does It Make a Difference in Human Rights Behavior?', *Journal of Peace Research*, 36 (1999), 95–118; Linda Camp Keith, 'Constitutional Provisions for Individual Human Rights (1977–1996): Are They More Than Mere "Window Dressing?"', *Political Research Quarterly*, 55 (2002), 111–43.) have reported that governments run by the military and 'leftist' political authorities are more inclined to use repressive behaviour. We use the leftist regime variable from Poe and Tate and the military regime variable from Geddes, where she defines such a regime as where a 'group of officers decides who rules and influences policy.' The inclusion of these control variables did not significantly affect our estimate of the

We also control for the effect that civil conflicts have on a country's human rights performance. Governments often use threats to the security of the state as an excuse for engaging in widespread human rights violations. Strong evidence of this relationship has been reported in previous quantitative studies of human rights abuse.⁴⁰ Therefore, we include a dummy variable (*Civil War*) that indicates whether a civil war is occurring in each of the country-years in the sample. These data were obtained from Hafner-Burton and Tsutsui.⁴¹

Arguably, regime stability also has a separate effect on a country's human rights practices. Simply put, more stable regimes – irrespective of their democratic credentials – tend to engage in fewer violations of physical integrity rights; our model includes a variable called *Regime Durability* that is taken directly from the Polity IV database. This measures the number of years that have elapsed since the last time the country underwent a 'transition period', which is defined as a change of three points or more in the country's Polity 2 score that takes place within a period of three years or less.

Demographic factors can also be expected to bear upon human rights performance. Scholars have found a negative relationship between population density and levels of respect for human rights that is thought to reflect the fact that resource scarcities facilitate human rights violations.⁴² That is why our model includes a variable, *Population Density*, which provides a measure of the average number of persons per square kilometer in each country-year.⁴³ These data were obtained from the World Development Indicators Online.

The international institutional context in which a country is situated is also likely to influence its human rights practices.⁴⁴ There is an established debate in the field of international relations about the effectiveness of international inter-governmental regimes in changing the policies and practices of the signatory countries.⁴⁵ Hafner-Burton's examination of the ability of human rights treaties and Preferential Trade Agreements (PTAs) to influence actual human rights practices has found that while the ratification of international human rights treaties seems to make little difference to actual human rights behaviour, participation in certain types of PTA has a positive effect. In order to examine

(*F*note continued)

key independent variable *Bilateral Trade Context*. See Steven C. Poe and C. Neal Tate, 'Repression of Human Rights to Personal Integrity in the 1980s: A Global Analysis', *American Political Science Review*, 88 (1994), 853–72; Barbara Geddes, 'Authoritarian Breakdown: Empirical Test of a Game-Theoretic Argument' (paper presented at the annual meeting of the American Political Science Association, September 1999).

⁴⁰ Stephen C. Poe, Neal Tate and Linda Camp Keith, 'Repression of the Human Right to Personal Integrity Revisited: A Global Cross-National Study Covering the Years 1976–1993', *International Studies Quarterly*, 43 (1999), 291–313; Emilie M. Hafner-Burton and Kiyoteru Tsutsui, 'Justice Lost! The Failure of Human Rights Law to Matter where Needed Most', *Journal of Peace Research*, 44 (2007), 407–25.

⁴¹ Hafner-Burton and Tsutsui, 'Justice Lost!' Data were downloaded from http://www.princeton.edu/~ehafner/downloads/justice_lost.zip (accessed 7 September 2007). Hafner-Burton and Tsutsui had obtained raw data on civil wars from the Correlates of War dataset. Including a dummy variable for interstate war (also obtained from the replication data set of Hafner-Burton and Tsutsui) in addition to civil war did not lead to a significant change in our estimate of the effect of *Bilateral Trade Context*.

⁴² Emilie M. Hafner-Burton, 'Trading Human Rights: How Preferential Trade Agreements Influence Government Repression', *International Organization*, 59 (2005), 593–629.

⁴³ Using the natural log of *Population Density*, *Total Trade* and *Inward FDI* did not substantially affect the results.

⁴⁴ Ellen Lutz and Kathryn Sikkink, 'International Human Rights Law and Practice in Latin America', *International Organization*, 54 (2000), 633–59.

⁴⁵ Robert Keohane, 'After Hegemony' (Princeton, N.J.: Princeton University Press, 1984); John J. Mearsheimer, 'The False Promise of International Institutions', *International Security*, 19 (1994/95), 5–49.

whether a California Effect holds in the realm of human rights practices, we need to control for changes in human rights behaviour that stem from pressures exerted via a country's participation in certain types of PTA. Consistent with Hafner-Burton,⁴⁶ our model includes two dummy variables that measure the seriousness of the human rights conditions imposed by the PTAs to which a country belongs. *Hard PTA Membership* is coded as 1 for each country-year in which the country belongs to at least one PTA with enforceable human rights conditions, while *Soft PTA Membership* is coded as 1 for each country-year in which the country belongs to one or more PTAs in which human rights are mentioned, but only in a declaratory or promotional sense.⁴⁷ Information on PTA memberships was obtained from the World Trade Organization.⁴⁸

In addition to domestic drivers of human rights behaviour and a country's membership in preferential trading agreements, our model includes two variables that are designed to capture non-trade related influences on a country's human rights standards. The first of these, *Neighbourhood Effect*, is a spatially-lagged variable that reflects the average human rights standards found among a country's geographic neighbours. A large literature on geographic diffusion has shown that countries' domestic politics tend to be closely correlated with those of their geographic neighbours.⁴⁹ This may result from the fact that ideas and norms travel more easily between geographically proximate countries as a result of the greater flows of people (as well as media products) that take place between them, and/or the fact that geographically proximate countries tend to have a shared cultural history.⁵⁰

⁴⁶ Hafner-Burton, 'Trading Human Rights'.

⁴⁷ Following Hafner-Burton, we used the legal text of the agreements themselves to determine whether each PTA should be classified as 'hard' or 'soft'. Of the 2,528 total country-years included in our model, around 11 per cent were considered to belong to PTAs with 'hard' conditions, and around 21 per cent belonged to PTAs with 'soft' conditions.

⁴⁸ As a separate robustness test, we tried including a dummy variable indicating whether the state had ratified the International Covenant on Civil and Political Rights (ICCPR) – one of the most important human rights treaties dealing with physical integrity rights (see Beth A. Simmons, *Mobilizing for Human Rights: International Law in Domestic Politics* (New York: Cambridge University Press, 2009).) We found evidence of a slight *negative* effect of ICCPR ratification on physical rights, although this effect was only significant at the 0.10 level. This result is consistent with some studies that have suggested that states with worse human rights practices tend to be more willing to ratify human rights treaties with relatively weak enforcement mechanisms (see Oona A. Hathaway, 'Do Human Rights Treaties Make a Difference?'; Hafner-Burton and Tsutsui, 'Human Rights in a Globalizing World'; James Raymond Vreeland, 'Political Institutions and Human Rights: Why Dictatorships Enter into the Convention against Torture'). The inclusion of this variable did not, however, affect our estimate of the effect of our key independent variable, *Bilateral Trade Context*.

⁴⁹ Kristian Skrede Gleditsch and Michael D. Ward, 'Diffusion and the International Context of Democratization', *International Organization*, 60 (2006), 911–33; Beth A. Simmons and Zachary Elkins, 'The Globalization of Liberalization: Policy Diffusion in the International Economy', *American Political Science Review*, 98 (2004), 171–89; Jeffrey S. Kopstein and David A. Reilly, 'Geographic Diffusion and Transformation of the Post Communist World', *World Politics*, 53 (2000), 1–37.

⁵⁰ Data on the spatial relationships between pairs of countries were obtained from O'Loughlin *et al.* See John O'Loughlin, Michael D. Ward, Corey L. Lofdahl, Jordin S. Cohen, David S. Brown, David Reilly, Kristian S. Gleditsch and Michael Shin, 'The Diffusion of Democracy, 1946–1994', *Annals, Association of American Geographers*, 88 (1998), 545–74, p.554, and fn. 8. These data recognize the connections between pairs of countries that are physically contiguous (e.g., USA–Mexico), as well as those that involve island countries separated by relatively short distances (e.g., Cuba–USA, or Australia–New Zealand). Relatively remote islands, like Fiji, were coded as having no geographical neighbours.

Along with the geographical neighbourhood, one could also think of states as operating within cultural ‘neighbourhoods’ that produce isomorphic pressures and serve to diffuse human rights practices across countries. We use the presence of a shared language as an indicator of cultural similarities that could make countries more receptive to the influences of others. We hypothesize that both citizens and elites are likely to have greater exposure to, and a higher probability of being influenced by, the norms and practices of culturally-similar states. For example, we might expect the human rights practices of French-speaking countries to have a marginally greater influence on the human rights practices of a country like Niger than they would have on its primarily Arabic-speaking neighbours in North Africa. As with the geographical neighbourhood variable, our model controls for the linguistic neighbourhood effect by calculating the average PIR Score for groups of countries that share a common language (*Common Language*). Data on each country’s official language(s) were obtained from the CIA World Factbook.⁵¹

MATCHING ANALYSIS

The California Effect describes one type of mechanism that would produce a positive coefficient for the *Bilateral Trade Context* variable. However, self-selection among countries can also produce a positive relationship. Importing (or exporting) countries might choose their trading partners selectively; for example, one of the criteria for establishing a trading relationship might be the previous human rights records of the exporting country. So it is possible that a statistical association between one country’s human rights record and the *Bilateral Trade Context* variable is due to a selection effect rather than a California Effect. In order to distinguish between these two processes, we need to go beyond simple statistical association and consider a causal model. One way to define causality or causal effects follows the logic of counterfactuals. In our context, the causal effect of the *Bilateral Trade Context* variable can be considered as the difference between the human rights record of a country i when its value of *Bilateral Trade Context* is high and the human rights record of the same country when its value of *Bilateral Trade Context* is low. In causal modelling terminology, let us choose T as the treatment variable where $T=1$ denotes a high level of *Bilateral Trade Context* (i.e., good human rights practices among a country’s export destinations), and $T=0$ denotes a low value of the *Bilateral Trade Context* (i.e., poor human rights practices among a country’s export destinations). As before, the response variable is still country i ’s level of respect for human rights, as measured by its PIR Score. We shall use the term $Y_{T=1}$ to refer to country i ’s human rights level if this country receives the treatment (i.e., a high level of *Bilateral Trade Context*) and $Y_{T=0}$ otherwise (i.e., a low level of *Bilateral Trade Context*). The causal effect of the *Bilateral Trade Context* variable can therefore be inferred by comparing $Y_{T=1}$ and $Y_{T=0}$, which often involves simply taking the difference between the two: $Y_{T=1} - Y_{T=0}$.⁵²

⁵¹ We also tested whether the results were affected by the inclusion of a dummy variable indicating whether the state had previously been a colony of another state. Data on colonial histories were obtained from the *CIA World Factbook*. The inclusion of this variable did not significantly affect our estimate of the effect of *Bilateral Trade Context*.

⁵² Paul R. Rosenbaum and Donald B. Rubin, ‘The Central Role of the Propensity Score in Observational Studies for Causal Effects’, *Biometrika*, 70 (1983), 41–55; Donald B. Rubin, ‘Estimating Causal Effects of Treatments in Randomized and Nonrandomized Studies’, *Journal of Educational Psychology*, 66 (1974), 688–701.

Of course, in reality one can only observe either $Y_{T=1}$ or $Y_{T=0}$; a country, at one point in time, can only have one value of *Bilateral Trade Context*. One way to infer the causal effect is to calculate the average treatment effect, that is, $E(Y|T=1) - E(Y|T=0)$. This quantity can be easily estimated from the results of randomized experiments where units (i.e., countries in this study) are randomly assigned to treatment and control groups, thereby ensuring that the pre-treatment characteristics/covariates of the treatment group units and the control group units, X , are similar enough (in distribution) that the only difference between the two groups is the treatment itself. However, in the context of our study such randomization is impossible; we cannot randomly assign countries to two groups with different levels of *Bilateral Trade Context*. Nonetheless, what we can do is to select a subset of the observational data wherein the treatment units and control units are said to be ‘matched’ in that they have similar levels of all pre-treatment covariates, X , and differ only in their average level of the treatment variable, T . In this way, the link between pre-treatment covariates X and treatment assignment T can be broken (approximately) in a way that brings us much closer to the ideal situation where the treatment and control units had been assigned randomly from a single population. Imai and van Dyk have developed the broad notion of using propensity scores as a means of managing sample matching in parametric studies.⁵³ Once the matched subsamples are produced, one can simply calculate the average treatment effect ($E(Y|T=1) - E(Y|T=0)$); one can also proceed with normal parametric model fitting as we will do in the following analysis.⁵⁴

We use propensity score matching from the MatchIt library for the R programming language developed by Ho *et al.* to find subsamples of the data where the assignment of the treatment – a high average value of *Bilateral Trade Context* – is not correlated with the pre-treatment covariates X .⁵⁵ We look for subsamples of data based on matching on the propensity score (that is, the probability of receiving the ‘treatment’) which is a function of a number of variables that might possibly affect the countries’ level of *Bilateral Trade Context*. For example, among many other things, countries might select their trade partners based on the partner countries’ previous human rights records and political factors such as levels of democracy.⁵⁶ Specifically, the pre-treatment covariates, X , that we include are the countries’ previous year’s human rights records, their levels of democracy and regime durability, their levels of economic development (as measured by GDP per capita) and economic openness (measured by total trade as a percentage of GDP and inward FDI as a percentage of GDP), as well as other relevant factors such as civil war, population density, ‘hard’ and ‘soft’ PTA membership, and their neighbouring countries’ as well as same-language countries’ average human rights records. In other words, after matching, countries in the treatment group and countries in the control group are similar

⁵³ Kosuke Imai and David A. van Dyk, ‘Causal Inference With General Treatment Regimes: Generalizing the Propensity Score’, *Journal of the American Statistical Association*, 99 (2004), 854–66.

⁵⁴ Ho *et al.*, ‘Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference’. They argue that parametric analysis (such as the ordered probit regression that we use in the following analysis) with control variables is a better choice than simply taking the difference in means ($(E(Y|T=1) - E(Y|T=0))$) in the final matched sample. Indeed, standard parametric data analysis procedures only need to be changed when using subclassification, full matching, or matching with replacement. We use nearest neighbour matching without replacement in this study.

⁵⁵ Ho *et al.*, ‘Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference’.

⁵⁶ Edward Mansfield, Helen Milner and Peter Rosendorff, ‘Free to Trade: Democracies, Autocracies and International Trade Negotiations’, *American Political Science Review*, 94, (2000), 305–21.

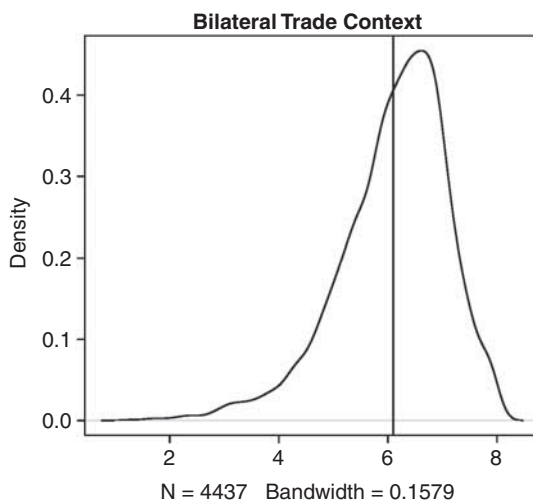


Fig. 3. Distribution of the *Bilateral Trade Context* variable in the full dataset
 Note: The vertical line represents the mean value.

enough in distribution in all of the dimensions of the pre-treatment covariates, and, assuming we have accounted for most of the factors that might affect a country's choice of trading partners, the only difference that remains is whether they receive the treatment, that is whether they are subject to a high or low value of *Bilateral Trade Context*.

Given that our treatment variable, *Bilateral Trade Context*, is not a binary variable, we have to dichotomize it in order to create separate treatment and control groups. However, there is no theoretical prior to tell us at what point along the range of possible values of *Bilateral Trade Context* the variable can be expected to have a significant effect. Figure 3 plots its density distribution. The mean is around 6.1 and the mode is around 6.5.⁵⁷ Figure 4 plots the human rights variable (jittered to avoid overlapping) against the bilateral trade context variable and includes a non-parametric Lowess line to show the relationship between *Bilateral Trade Context* and *PIR Score*.⁵⁸ Note that this scatter plot and Lowess line are based on the data before matching. However, they still give us some hints on where the thresholds might be; the effect of the *Bilateral Trade Context* variable only starts to show after it reaches the level around 6: the Lowess line starts to go up, revealing a positive association.

⁵⁷ Some examples of country-years that have a mean value for the *Bilateral Trade Context* variable include Bosnia and Herzegovina 1995 and 1999, Colombia 1999, Cote d'Ivoire 1989, Qatar 1994 and 1995, and Sri Lanka 1990. Examples of country-years that have a modal value for the *Bilateral Trade Context* variable include Algeria 1998, Chile 1988, India 1990, Malaysia 1981, and Tunisia 1988. Moreover, for most of the countries in our sample, there is enough temporal variation in the *Bilateral Trade Context* variable. To provide an illustration of how this variable changes over time, Table 2 includes a series of plots showing the levels of the variable over the 1981–2004 period for a randomly selected sample of eight countries.

⁵⁸ 'Jittering' is a procedure for improving the display of bivariate data. This involves introducing a trivial amount of random variation in the position of overlapping points on a scatterplot in order to make it easier for the reader to get a sense of the distribution of the data. (This is especially useful when one of the variables is categorical and where multiple data points may otherwise be represented by a single overlapping point on a scatterplot).

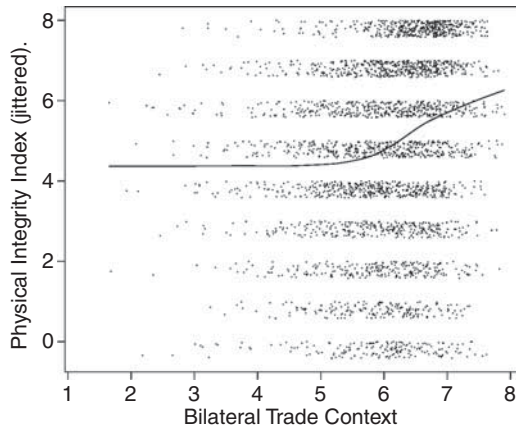


Fig. 4. Non-parametric lowess line showing the relationship between *Bilateral Trade Context* and *Physical Integrity Right* for all country-year observations in our sample

Note: The vertical positions of the points have been jittered to minimize the degree of overlap.

We start by trying *Bilateral Trade Context* values of around 6 as thresholds to dichotomize the treatment variable, find sub-samples in the data based on matching on the propensity score for receiving the treatment (that is, the possibility of having a high *Bilateral Trade Context*), and finally run a regression based on the matched sub-samples.⁵⁹ We start by using a threshold of 6.1 (the mean of the *Bilateral Trade Context* variable). The results are reported in Table 1: Model 1 where we find no significant effect of *Bilateral Trade Context*. When we increase the threshold to 6.5 (the modal value of *Bilateral Trade Context*), we begin to observe the causal effect of bilateral trade context becoming statistically significant (see Table 1: Model 2). We also try higher thresholds such as 7. As reported in Table 1: Model 3, at this point we find a significant causal effect of *Bilateral Trade Context*.

In Figure 5, we plot the 95 per cent confidence intervals of the treatment effects of *Bilateral Trade Context* across a wide range of different thresholds. We find that when *Bilateral Trade Context* takes on values between 6.5 and 7 the treatment effects are consistently statistically significant in the regression analysis on the matched data.⁶⁰ We also provide the estimates of *Bilateral Trade Context* and the other control variables from the same ordered probit regression but based on the full sample without matching (see Model 0 in Table 1). The purpose of presenting these estimates without matching is to demonstrate that estimated effects of other control variables are not a function of the restricted sample of data that results from the matching exercise. Indeed, the effects of

⁵⁹ We estimate ordered probit models because the dependent variable, *PIR Score*, takes on categorical values of 0 to 8. Moreover, because the independent variables cannot be expected to produce instantaneous changes in human rights practices, we lagged the independent variables by one year.

⁶⁰ Note that in Figure 5, when the threshold to dichotomize the *Bilateral Trade Context* variable is larger than 7, even though the mean estimates of the treatment effect (black dots in the middle of the confidence intervals) are all above zero, the 95% confidence intervals become so large that the treatment effect becomes insignificant. This is largely a function of small sample sizes after matching when the threshold to dichotomize is too high. For example, when we use 7.1 as the threshold, there are only 388 observations left (from both the treatment and the control group); when we use 7.2 as the threshold, the number of observations is further reduced to 274.

TABLE 1 *Explaining Levels of Physical Integrity Rights (PIR) across Space and Time*

	M0: without matching		M1: 6.1 as threshold		M2: 6.5 as threshold		M3: 7 as threshold	
	Coef.	Std. Error	Coef.	Std. Error	Coef.	Std. Error	Coef.	Std. Error
Bilateral Trade Context	0.019	0.025	0.026	0.052	0.117	0.052***	0.364	0.096***
Democracy	0.017	0.004***	0.016	0.004***	0.021	0.005***	0.036	0.009***
Total Trade (% of GDP)	0.003	0.001***	0.003	0.001***	0.004	0.001***	0.003	0.002***
Inward FDI (% of GDP)	-0.007	0.006	-0.012	0.007*	-0.006	0.008	-0.011	0.020
GDP per capita (log)	0.090	0.022***	0.090	0.023***	0.093	0.026***	0.039	0.048
Civil War	-0.430	0.088***	-0.429	0.092***	-0.411	0.100***	-0.485	0.163***
Regime Durability	0.003	0.001***	0.003	0.001***	0.004	0.001***	0.005	0.002**
Population Density	-0.000	0.000	0.000	0.000	-0.000	0.000	-0.000	0.001
'Hard' PTA membership	-0.058	0.085	-0.053	0.088	-0.115	0.101	-0.089	0.207
'Soft' PTA membership	0.049	0.064	0.035	0.071	0.078	0.079	0.187	0.148
Common Language	0.024	0.010**	0.017	0.011	0.026	0.012**	0.049	0.022**
Neighbourhood Effect	0.078	0.015***	0.087	0.017***	0.083	0.017***	0.068	0.031**
Lagged dependent variable	0.556	0.015***	0.564	0.016***	0.541	0.018***	0.543	0.033***
Number of observations		2,411		2,182		1,724		518

Notes: The table shows ordered probit regression estimates based on full sample without matching (M0) and on subsamples after matching (M1–M3). For M1 to M3, each column represents the results obtained when a different choice of threshold is used to dichotomize the Bilateral Trade Context variable. ***, **, * show significance levels 99%, 95%, and 90% respectively.

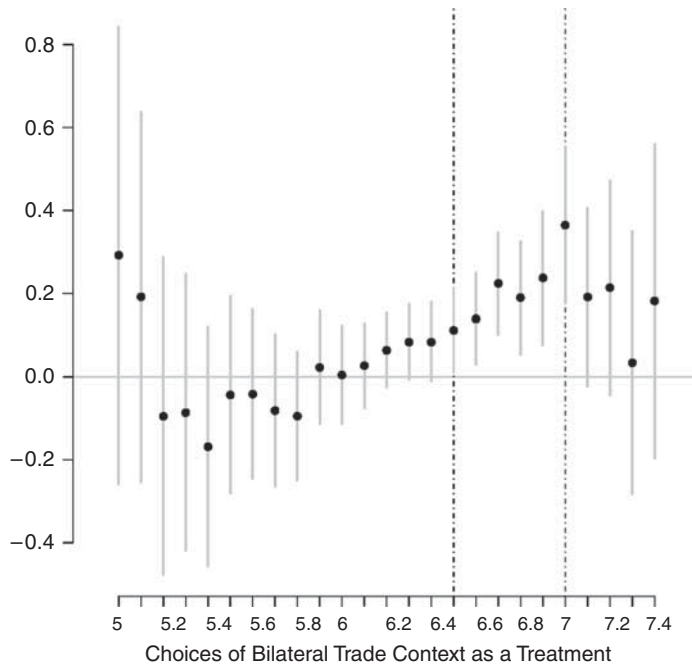


Fig. 5. Explaining levels of Physical Integrity Rights (PIR) across space and time

Notes: Estimated effect size of the Bilateral Trade Context variable in the regression analysis (after matching) when different thresholds are used to distinguish between 'low' and 'high' values of *Bilateral Trade Context*. The 95% confidence intervals around the coefficient estimates are indicated by the grey vertical lines around each point estimate.

most of our control variables included in the regression model are broadly compatible with the effects reported in earlier quantitative studies of human rights (with the exception of 'hard' and 'soft' PTAs). As expected, we find that civil wars are strongly associated with decreased levels of respect for human rights. We find that *Total Trade* and *Regime Durability* show a positive relationship to *PIR Scores*. The finding that *Total Trade* is positively related to *PIR Scores* is also consistent with many of these studies.⁶¹ This provides further evidence to support the arguments made by globalization optimists that exposure to global markets in the more general sense – that is, irrespective of the human rights performance of one's trading partners – tends to be associated with subsequent improvements in human rights performance. However, we do not find a consistent and significant effect of our measure of *Inward FDI*. The general intuition that richer countries show greater respect for physical integrity rights than poor countries holds in most of the model specifications we tried, except in Model 3 in which a *Bilateral Trade Context* of 7 is used to define the treatment and control groups.

Our analysis also suggests that the California Effect holds even when we control for a country's membership in PTAs. Importantly, contrary to Hafner-Burton, we find that neither a country's membership in a 'hard' PTA nor its membership in 'soft' PTAs is

⁶¹ Apodaca, 'Global Economic Patterns and Personal Integrity Rights after the Cold War'; Hafner-Burton and Tsutsui, 'Human Rights in a Globalizing World'; Poe, Tate and Camp Keith, 'Repression of the Human Right to Personal Integrity Revisited'.

positively associated with physical integrity rights.⁶² The inclusion of the two control variables makes very little difference to the size and significance of the *Bilateral Trade Context* variable.⁶³ In other words, the California Effect that we report in the context of human rights practices is not simply reflecting the fact that some of the better-performing countries belong to PTAs with strict human rights conditions.

Regarding the spatial variables, we find that the influence of a country's geographical neighbourhood (*Neighbourhood Effect*) is positive and highly statistically significant in all three models. This means that the human rights performance of a country's geographical neighbours appears to be closely related to the country's own human rights performance even after controlling for other domestic and international-level influences on that country's performance. However, our control for a common culture, *Common Language*, does not show a significant relationship with physical integrity rights in all the model specifications. Therefore, it appears that the effect of physical proximity is far stronger than the effect of cultural ties when it comes to considering other external influences on a country's human rights performance.

CONCLUSIONS

This study provides evidence that trading relationships can, under certain conditions, serve as 'transmission belts' for the diffusion of human rights standards from importing to exporting countries. In doing so, it contributes to a growing literature that is extending David Vogel's original concept of the California Effect to a number of different issue areas in international politics. Human rights practices provide an especially hard test of the California Effect because the causal chain that connects consumer behaviour in importing countries to the human rights practices of exporting states is less direct given that the actors that come under pressure to improve human rights standards (the exporting firms) are not the same as the actors responsible for changing the human rights practices of the target states (the governments of the exporting countries). Also, because human rights practices tend to be embedded in the wider political institutions and social practices of the country, attempts to change them are likely to generate strong resistance among other societal actors.

Given these obstacles to norm transmission, we find evidence for the existence of a California effect only when the average human rights practices of the export destinations exceeds a certain threshold. This finding has important theoretical and practical implications. From a theoretical point of view, it lends support to the view that trade-based

⁶² Hafner-Burton, 'Trading Human Rights'. We also tested for potential interaction effects between PTA membership and *Bilateral Trade Context*, but did not find evidence of a significant effect of PTA membership. The fact that we found no significant effect of PTA membership in any of these models raises important questions about the efficacy of including human rights conditions in PTAs. We had coded our PTA variables using the system described by Hafner-Burton. However, we realize that this coding method has two important limitations. First, there arguably is a selection bias because countries that already have good human rights practices might self-select into PTAs with more demanding human rights standards. Second, the membership variable is simply a binary indicator of PTA membership and does not distinguish between countries that are members of multiple PTAs and countries that only participate in a single PTA. We believe that further work needs to address the question of how PTA membership might or might not affect human rights practices more carefully. We want to thank one anonymous reviewer for suggesting the idea of a potential PTA–*Bilateral Trade Context* interaction effect.

⁶³ The correlation coefficients for *Bilateral Trade Context* with *Hard PTA Membership* and *Soft PTA Membership* are only 0.05 and -0.06 , respectively, in the unmatched data.

pressures can reach deeper into the domestic politics of an exporting state than previously thought. Trade not only connects the practices of importing jurisdictions to those of the exporting firms, but can also connect the practices of importing jurisdictions to those of the governments of exporting states. It also emphasizes the need for IR scholars to pay greater attention to the non-linear nature of the relationship between various international stimuli and domestic political outcomes. This is an idea that has been around for some time in the theoretical literature on norm diffusion, but has not, as far as we are aware, been subject to detailed testing in large-*n* studies.⁶⁴

This article suggests that in assessing the effect of trade on human rights practices, we ought to move beyond a simple one-dimensional measure of trade-induced globalization. Instead, we should also consider ways in which the practices of specific trading partners might shape the practices of exporting countries. From a practical point of view, our findings suggest that non-governmental organizations located in importing countries should reassess their opposition to international trade. Instead of viewing trade as a vehicle for instigating ‘races to the bottom’ in a variety of policy domains including human rights, they could perhaps think of leveraging trade to create ‘races to the top’. This is something that can be achieved through ‘private politics’, even though a country’s human rights performance represents a process-based standard without a clear label. This form of leverage is facilitated by the structure of world trade: the bulk of exports from developing countries is absorbed by developed countries that tend to have superior environmental, labour and social practices. Furthermore, non-governmental organizations tend to be more established and have greater political clout in developed countries. Hence, the inequitable structure of international trade whereby developing countries are dependent on developed countries for market access provides northern non-governmental organizations with an opportunity to manipulate trading relations to serve as conduits of their preferred practices and norms. Instead of developed countries being transformed in the image of practices of developing countries – as the race to the bottom literature suggests – non-governmental organizations could strategically leverage the structural inequity in trade to shape the practices and norms of developing countries.

This article should encourage systematic thinking about the emergence of new economic poles in the global economy. Particularly, how might China’s emerging trading relationships with certain African countries affect the human rights practices of these countries? Our model suggests that given China’s very low score on the Physical Integrity Rights Index (the Cingranelli-Richards dataset assigns China a score of 1 for 2004), bilateral trade with China is very unlikely to induce improvements in the human rights performance of China’s trading partners. Arguably, as long as OECD countries with fairly high levels of human rights practices dominated world trade, bilateral trade served to improve human rights standards in the developing world countries whose exports were dependent on these markets. As the structure of the world economy changes and new countries with less than stellar human rights performance emerge as important destinations for exports from developing countries, the positive effect of bilateral trade on human rights would most likely be compromised.

Finally, we believe that future research could benefit from considering some other possible ways in which this model of trade-related norm diffusion could be refined. While we provide evidence on how high levels of bilateral trade facilitate norm diffusion from

⁶⁴ Finnemore and Sikkink, ‘International Norm Dynamics and Political Change’.

importing to exporting countries, one could argue that such diffusion might also be predicated on other factors such as the gap between the exporting country's human rights score and the average score of its export destinations.⁶⁵ Moreover, future research might benefit from looking at the California Effect across different sectors of trade by disaggregating total bilateral trade flows. Another interesting question relates to the time dynamics of the California Effect diffusion mechanism. For example, one could ask whether the time taken for the trade-related diffusion of human rights practices is a function of various domestic variables. It is possible that even under the same level of bilateral trade pressure, some countries resist the pressure for longer than others prior to improving their human rights. Answering this question requires careful theorizing about the relevant domestic variables and mechanisms that moderate (or facilitate) the effect of bilateral trade pressure.⁶⁶ In short, we believe that there are many exciting theoretical and empirical opportunities for the creative examination of the role of bilateral trade in global diffusion processes.

⁶⁵ We thank one of the anonymous reviewers for bringing this to our attention.

⁶⁶ For a recent study of the conditional effects of domestic institutions on diffusion mechanisms, see Xun Cao and Aseem Prakash, 'Trade Competition and Environmental Regulations: Domestic Political Constraints and Issue Visibility', *Journal of Politics*, 74 (2012), 66–82.