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Tax revenues and the fiscal cost of trade liberalization, 1792–2006[☆]



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A B S T R A C T

This article examines the impact of trade liberalization on government revenues. Using a new dataset on tax revenues for 130 countries between 1792 and 2006, we identify ninety-nine episodes of trade liberalization defined as a large fall in trade tax revenues not accompanied by a decrease in trade. Seven took place before World War One, seven in the interwar period, eighteen in the Bretton Woods period and the remainder after 1970. We examine the extent to which countries were able to recover the tax revenues lost by liberalizing trade by using other sources of revenue. We find that historical (pre-1970) trade liberalization episodes were unlikely to be accompanied by decreases in tax revenues, especially during the Bretton Woods era. In the recent period however, over 40% of the developing countries in our sample experience a fall in total tax revenues that lasts more than ten years after an episode of trade liberalization. Overall, trade liberalization led to larger and longer-lived declines in tax revenues in developing countries since 1970 than in today's rich countries in the 19th and 20th centuries. Results are similar when we consider government expenditures, suggesting decreases in trade tax revenues negatively affect governments' capacity to provide public services in many developing countries.

1. Introduction

How do trade liberalizations affect macroeconomic outcomes? Previous research has shown that a long-run perspective on this question is useful for exploring how trade liberalizations influence growth, employment, and living standards (Jacks, 2006; Lampe and Sharp, 2013; O'Rourke, 2000); however, comparatively less is known about their impact on fiscal revenues and hence on governments' capacity to provide public services. Using a new panel dataset on tax revenues and government expenditures covering 130 countries between 1792 and 2006, we analyze whether countries in which trade tax revenues decrease experience a simultaneous fall in total tax revenues – an effect we call “the fiscal cost of trade liberalization”. When fiscal costs of trade liberalization are realized, we examine how long the decline in revenue lasts. We compare the characteristics of trade liberalization episodes over different historical periods, starting with the ‘first wave’ of trade liberalization (see Estevadeordal et al., 2003; O'Rourke and Williamson, 1999; Tena-Junguito et al., 2012) and up to the recent experiences of today's developing countries. For each of the 130 countries in our data we go as far back in time as possible: our dataset starts in 1792 for one country (the United States), covers nine countries in the 19th century, and thirty-five countries in the first half of the 20th century. It is to the best of our knowledge the most exhaustive data on tax revenues available to researchers.

^{*} The dataset constructed for this paper is available on the authors' websites. We are grateful to the Editor (Kris Mitchener) and to three anonymous referees for insightful comments. We gratefully acknowledge helpful comments and suggestions from Alberto Alesina, Sascha Becker, Tim Besley, Denis Cogneau, Emmanuel Farhi, Walker Hanlon, Wojciech Kopczuk, Marc Melitz, Petra Moser, Nathan Nunn, Thomas Piketty, and Romain Rancière. We also thank Thomas Baunsgaard, Michael Clemens, Doug Irwin, Michael Keen, Markus Lampe, Kevin O'Rourke, Moritz Schularick, and Alan Taylor for sharing their data or helping us access data, as well as seminar participants at Harvard University, Columbia University and the Paris School of Economics for useful comments and suggestions. A previous version of this paper was circulated under the name ‘Tax Capacity and the Adverse Effects of Trade Liberalization’.

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We develop and implement a method to identify episodes of trade liberalization and measure the contemporary change in total tax revenues. The difficulties of constructing a measure of trade liberalization that is consistent across countries in the recent period, or among a subset of countries going back in time, are well known (see [Lampe and Sharp, 2014](#), for a recent review).¹ The aim of our method is not to examine the fiscal impact of all changes in trade policy, but to document the fiscal cost of trade liberalization when trade liberalization could potentially have a fiscal cost – when it takes the form of a decrease in trade tax revenues. We therefore choose to use a fiscal definition of trade liberalization which we define as a large and prolonged fall in trade tax revenues over GDP. To avoid capturing decreases in trade tax revenues that are not the consequence of trade policy, we restrict our analysis to episodes that were not accompanied by a decrease in trade. Our method will not therefore capture trade liberalization episodes during which countries lowered non-tariff barriers or decreased tariffs set above the revenue-maximizing rate as these will not lead to decreases in tax revenues. By definition it is better suited to study episodes when fiscal (or revenue) tariffs decline than episodes when purely protectionist tariffs levied on goods with high trade elasticities are reduced (see [Tena-Junguito, 2006](#), for the distinction between fiscal and protectionist tariffs). We consider the evolution of trade policy measures (weighted and unweighted average tariffs and trade agreements), whenever available, to confirm that our episodes are indeed related to changes in trade policy. We then study whether countries are able to compensate for the fall in trade tax revenues by an increase in other (domestic) tax revenues and argue that there is a fiscal cost of trade liberalization when total tax revenues fall after the start of the episode.

We find ninety-nine episodes of trade liberalization thus defined. Seven occur between the mid-19th century and 1914, another seven during the interwar period, eighteen during the Bretton Woods period, and the remaining sixty-seven in the contemporary period (post-1970); the sharp increase in the number of episodes over time in large part reflects the increase in the size of our sample, as countries that used to belong to colonial empires obtain independence and enter our data. Trade taxes fall by 3 GDP percentage points on average during these episodes. 45% of the countries that experience a trade liberalization episode have not recovered the lost tax revenues five years after the start of the episode and we never observe a fiscal recovery in over 20% of the countries.

Comparing across historical periods, we find that the fiscal cost of trade liberalization experienced by today's rich countries at early levels of economic development is smaller and shorter-lived than the one experienced by developing countries since 1970. Episodes in the 19th century and first half of the 20th century are characterized by a relatively smaller drop in trade tax revenues and we always observe a fiscal recovery for those occurring during the Bretton Woods period. There are also clear differences by level of development in the period since 1970. The few rich countries that experience a trade liberalization episode never experience any fiscal cost whilst over 50% of developing countries do. Moreover nearly a third of developing countries are never observed recovering the lost trade tax revenues through other tax instruments. Overall, episodes of trade liberalization are associated with larger decreases in tax revenues in poorer countries, particularly so since the 1970s.

Our results are robust to the choices made in defining a trade liberalization episode. In particular, we find very similar results when normalizing tax revenues by population instead of GDP (to avoid capturing potential changes in GDP due to trade liberalization), when we change our measure of trade and when we exclude episodes for which we observe an increase in tax revenues prior to the onset of the episode (suggesting they may have chosen to preempt the fall in trade taxes) and episodes for which there are no trade data available. Varying the GDP thresholds or the trade variable used to define an episode similarly does not affect the results.

Countries that do not recover the lost trade tax revenues through an increase in other taxes may nevertheless maintain their level of public spending through an increase in non-tax revenues. We look at the evolution of government expenditures during trade liberalization episodes to test whether this is the case. Slightly fewer countries experience a fall in government expenditures than a fall in total tax revenues during trade liberalization episodes, suggesting that the decrease in trade tax revenues may indeed have been compensated for by an increase in non-tax revenues in some countries.² The overall patterns, however, are strikingly similar. Nearly one-third of developing countries are never observed recovering the lost government expenditures, and poorer countries are more likely to experience an expenditure cost of trade liberalization than rich countries, particularly since the 1970s. Our research does not attempt to assess the overall welfare impact of the trade liberalization episodes we identify; it may be that the fiscal cost we document was more than offset by the non-fiscal welfare gains from trade liberalization. This last set of results, however, confirms that in most cases this fiscal cost did lead to less government spending on the ground.

This paper contributes to a large literature on the history of trade liberalization. In particular a number of papers have investigated the relationship between tariffs and growth ([Clemens and Williamson, 2001](#); [Estevadeordal and Taylor, 2013](#); [Irwin, 2002](#); [Lampe and Sharp, 2013](#); [O'Rourke, 2000](#); [Schularick and Solomou, 2011](#)), and more broadly questioned the costs of protectionism (for a discussion see [Krueger, 1997](#)). We focus on a consequence of trade liberalization – its impact on government revenues – that has attracted relatively little attention in the literature. One exception is [Baunsgaard and Keen \(2010\)](#) who first identified the existence of a trade-off between tax revenues and trade liberalization. They estimate how domestic tax revenues react to changes in trade tax revenues in the short run using post-1975 data and, like us, find an incomplete replacement of lost trade tax revenues in low-income-countries. We build on and complement their work in several ways by taking a more historical perspective. First, we consider the experience of trade liberalization in the history of early developers during the 19th and early 20th centuries and elaborate on the differences between today's developing countries and the historical experience of early developers to discuss potential explanations for the fiscal cost of trade liberalization. Second, our data allow us to generalize their results for today's developing countries to their

¹ Recent work has been able to construct relatively long time series on tariffs for a large set of countries, but this is only feasible for a subset of commodities – see for example [Tena-Junguito et al. \(2012\)](#).

² These non-tax revenues could come from development aid, natural resources or borrowing; we cannot document which of these revenues is the most relevant here as data on non-tax revenues are not available.

complete fiscal history since independence. Third, our method abstracts from short-term co-movements between domestic tax and trade tax revenues possibly unrelated to structural changes in countries' tax structures.

In the last section of the paper we discuss possible explanations for why the fiscal cost of trade liberalization differs across countries and periods by drawing on historical and contemporary examples of trade liberalization. In particular, we try to assess whether the difference between the experience of today's developing countries and that of early developers may be explained by the fact that the former liberalized trade before they had developed sufficient fiscal capacity to levy revenues through taxes on domestic production. This relates our work to the political science literature on the relationship between fiscal capacity building and protectionism. [Queralt \(2015\)](#) in particular develops a theory explaining how mercantilism can pave the way for economies endogenously embracing free trade if mercantilist revenues are invested in increasing fiscal capacity; [Brambor and Lindvall \(2014\)](#) argue that domestic fiscal capacity is a necessary prerequisite for trade liberalization (see also [Queralt, 2017](#)).

Our results also speak more generally to the literature considering how state capacity, and in particular tax levels and tax structures, changes as countries develop (see [Johnson and Koyama, 2017](#), for a recent review). Recent examples include the work by [Besley and Persson \(2009, 2013\)](#) in which countries' decisions to invest in fiscal capacity allows them to increase their tax to GDP ratios over time and to decrease their dependence on trade taxes. Others argue that as economies develop they undergo structural changes which make transactions easier to monitor and allow governments to rely less on less efficient but easier to levy taxes like taxes on trade (see e.g. [Aizenman, 1987](#); [Kleven et al., 2016](#); [Riezman and Slemrod, 1987](#)). These theories imply that countries will decrease trade taxes once they find themselves capable of levying domestic taxes but they cannot rationalize the fiscal cost of trade liberalization. We return to this literature when discussing possible explanations for our results. A smaller literature discusses the conditions under which revenue-neutral reforms replacing taxes on trade by domestic taxes such as the VAT will be optimal ([Emran and Stiglitz, 2005](#); [Keen and Ligthart, 2001; 2005](#)). Our results show that the typical trade liberalization reform in developing countries since 1970 during which trade tax revenues declined was not revenue-neutral but instead lead to a decrease in total revenues.

2. Data and descriptive statistics

2.1. Data

We assemble historical and contemporary data on tax revenues in a coherent way that allows for meaningful comparison across countries and over time. We primarily rely on three data sources: [Mitchell \(2007\)](#)'s *International Historical Statistics*, the dataset constructed by [Baungsgaard and Keen \(2010\)](#) and the International Monetary Fund's *Government Finance Statistics* (GFS). As explained in the data appendix, additional data on tax revenues may be available for some countries and time periods not covered by our dataset. Please see the online appendix for more details on the data sources, the methodology used to combine them, and examples of other data sources available; in particular, Appendix Table 1 lists the countries in our sample and the data sources used for every country and time period.³

Our aim is to detect and compare changes in total and trade tax revenues within countries over time so we combine these three datasets in a way that does not allow for within country 'jumps' in the series which could be due to changes in data sources. We determine which dataset contains the largest number of observations for each country and use only data from this source for each country, unless we see a clear continuity across sources. We do a linear interpolation when a variable is missing for one country for less than three years. When the gaps are longer (typically during wars), we drop the years for which the variable is missing and create another country identifier when the series start again. Finally we exclude from our sample countries for which we have data for less than seven consecutive years. [Fig. 1](#) shows the number of countries in our sample in each year (red line, right y-axis). Our data start before 1925 for 13% of the countries in our sample, before 1950 for 27% and before 1980 for 75% of the countries; it ends after 2000 for 88% of the countries.

We complement our analysis of tax revenues by using data on the share of government expenditures in GDP. Information on expenditures is available from [Mitchell \(2007\)](#) for most of the observations for which there is also tax information from this source. The dataset compiled by [Baungsgaard and Keen \(2010\)](#) however contains no information on expenditures and the GFS dataset very little information. We therefore use the IMF's *World Economic Outlook* (WEO) database to complement our dataset.

Our main measure of trade is imports as a share of GDP as most trade tax revenues come from tariffs levied on imports, but we also use total trade (sum of imports and exports of goods and services as a share of GDP) as a robustness check. We use trade data from the World Development Indicators for 1960 and from [Mitchell \(2007\)](#) for the pre-1960 period. Comparing these two variables in the post-1960 period suggests they could be measuring slightly different types of flows so we never combine the two sources when looking at the evolution of trade during an episode.

We collect data on countries' tariffs to complement our analysis of trade tax revenues. We use data on average (unweighted) tariff on all products available from 1988 in the *World Development Indicators*. This variable is a good proxy for the type of trade instrument directly controlled by policy makers and helps us assess the extent to which our method detects decreases in trade taxes that are really due to trade liberalization, as discussed below. It is available for 34 countries in 1998 and 150 countries in 2006. We also use data on 'average ad valorem equivalent tariff rate' (hereafter AVEs), the weighted average tariff rate (typically computed as the ratio of trade tax revenues to imports) from four sources: the *World Development Indicators* for the post-1988 period, [Lampe and Sharp \(2013\)](#), [Clemens and Williamson \(2004\)](#) and [Schularick and Solomou \(2011\)](#) for the earlier periods.

³ The data constructed for this paper can be found in [Cagé and Gadenne \(2018\)](#).

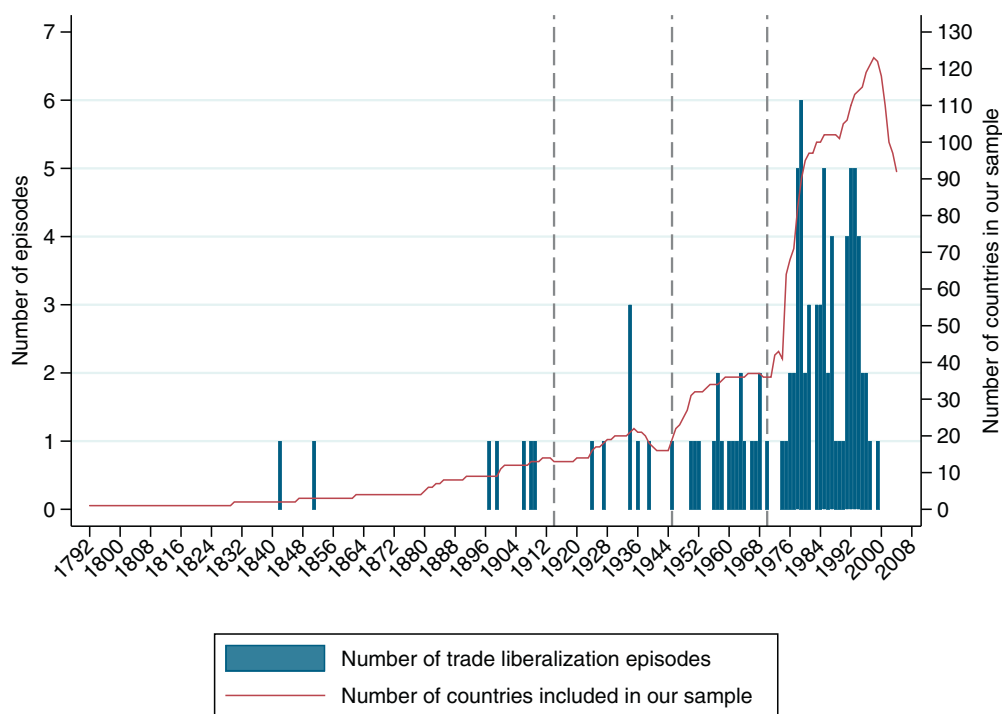


Fig. 1. Number of countries in our sample and trade liberalization episodes by year *Notes:* The blue bars, labelled on the left y-axis depict the number of trade liberalization episodes each year - see the text for a description of the method used to define an episode. The red line, labelled on the right y-axis presents the total number of countries that are included in our sample. The dashed lines indicate the limits of the different time periods we consider in the text. The first dashed line (1914) represents the start of World War One, the second (1945) the end of World War Two and the third (1970) the end of the Bretton Woods era. Appendix Table 1 lists the countries in our sample and the time period covered for each country and Appendix Table 6 lists episodes by country and start date. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

Finally, we use the GDP per capita data constructed by Maddison (2008) to classify countries by level of development following the earliest available country classification from the World Bank (1987).⁴ The United States for example is a low-income-country (LIC) until 1856, a middle-income-country (MIC) until 1941 and a high-income-country (HIC) after that. We sometimes classify countries with respect to their GDP in 2006, when we refer to ‘today’s developing countries’, we include all countries that are a LIC or a MIC in 2006. Countries are listed by their 2006 income group in the online Appendix Table 1.

Our main constraint in constructing our sample is the availability of data on total and trade tax revenues. We exclude from our sample countries and time periods of potential interest because of a lack of tax data comparable over time and across countries; we highlight important omissions below to clarify which historical episodes of trade liberalization our data will enable us to consider.

First, note that we do not have data on countries in Asia (except for Japan), Africa and the Middle East prior to 1945. This is because many countries in these regions were part of colonial empires at least until the end of World War Two, which gives rise to two problems when attempting to look for episodes of trade liberalization. First, comprehensive country-level tax revenue data are not available for most countries prior to independence; the metropolises levied taxes in the colonies but often did not record systematically colony-level tax revenues.⁵ Second, whilst some colonies were nominally free to set their trade policy in practice most trade policy decisions were controlled, directly or indirectly, by the colonizers. The metropolises typically set up preferential trading relationships within the empire through trading agreements or empire-based custom and currency unions (see for example Estevadeordal et al., 2003; Jacks, 2014; Mitchener and Weidenmier, 2008). In the 19th century moreover the trade policy of some formally independent countries was also controlled indirectly, through diplomatic and military means, by Western powers (see Bairoch, 1989, for a discussion of enforced commercial liberalism). Both these factors explain why we can only start looking for trade liberalization episodes in Asia, Africa and the Middle East after 1945. Asian and Middle Eastern countries typically enter our sample in the late 1940s and 1950s, African countries in the mid to late 1970s.

⁴ GDP is measured in 1990 Geary–Khamis dollars and is available for all countries for our period of interest.

⁵ When consistent time-series on colonial tax revenues are available the unit of observation is often a group of colonies within an empire, and allocating revenues (in particular trade tax revenues) to post-independence country units is conceptually difficult (see for example Cogneau et al., 2016).

Second, our data on Latin American countries start in the 20th century (starting with Brazil in 1900). This is again due to the lack of comprehensive tax data in these countries in the 19th century.⁶ The literature on Latin American trade policy in this period however suggests that this data limitation probably does not lead us to overlook important trade liberalization episodes: tariffs typically remained very high in Latin America in the second half of the 19th century, in part because tariff revenues were needed to finance recurring regional wars (see for example [Coatsworth and Williamson, 2004](#)). Most of the episodes of trade liberalization we find in the region occur during the late 20th century wave of globalization, as we discuss below.

Finally, data on tax revenues and expenditures are particularly poor for countries from the ex-Soviet bloc. Tax data for all but the very recent period are hard to come by for these countries, and harder still to compare with other countries because the wide-ranging intervention of the state in the economy until 1991 blurs the distinction between public and private income. The statistical system used by countries during the Soviet era differed greatly from statistical standards used in the rest of the world and convergence to global standards was slow in the 1990s and early 2000s ([World Bank, 1992](#)).⁷ Our dataset therefore only contains data on countries from the ex-Soviet bloc for which sufficiently long series are available from either GFS or [Baunsgaard and Keen \(2010\)](#); this notably excludes Russia itself.

2.2. Descriptive statistics

Panel A of [Table 1](#) shows the evolution of total and trade tax revenues as a share of GDP, GDP per capita and tariff rates since the 1830s for countries that are HICs, MICs or LICs in 2006.⁸ It highlights several stylized facts of interest regarding taxation and development. First, we see that tax ratios increase with GDP per capita, in line with Wagner's Law. This is particularly evident in [column 1](#) depicting the evolution of tax ratios for today's HICs. In the 1830s the two countries for which data are available (the UK and the US) are what we would today call LICs and levy less than 7% of their GDP in taxes. Tax ratios then increase in the second half of the 19th century to 9% as countries become MICs and keep increasing by roughly 4–5 GDP points every twenty years until today. The trend of the first half of the 20th century, well-documented and often explained by higher demand for public spending during wars (see for example [Lindert, 2004](#)), is maintained in the second half of the century. These findings are robust to considering only countries for which data are available from the 1890s to the 1990s, as shown in [Appendix Table](#). The cross-sectional comparison between HICs, MICs and LICs in 2000–2006 also shows a positive, albeit weaker, correlation between economic development and tax ratios. HICs are today on average 16 times richer than LICs and levy twice as much taxes as a share of GDP.

[Table 1](#) also illustrates a lesser-known stylized fact about taxation and development, the ‘tax transition’. Countries at an early stage of development rely on taxes on trade to levy a large share of their revenues, as they develop this share becomes smaller.⁹ Trade taxes represent nearly 50% of total taxes on average in the HICs we observe in the 1830s. This share falls to 18% in the 1920s, 12% in the 1950s and decreases in the last 50 years to around 2% today. We observe a similar decrease in the share of trade taxes in total taxes in developing countries, where trade taxes represent more than 25% (MICs) and nearly 40% (LICs) of total taxes in the 1970s. This share decreases to less than 15% (22% for LICs) in the 2000s. The correlation between the share of trade taxes in total tax revenues and development also holds in the cross-section: in 2000–2006, the share of trade taxes in total tax revenues is ten times bigger in LICs than in HICs. We see a similar pattern when looking at average tariff rates: in 2000–2006, tariffs are more than six times higher in LICs than in HICs.

Of particular interest for the question asked in this paper we see that the tax transition took a very different form in early developers compared to today's developing countries. In HICs the decrease in the share of trade taxes in total taxes is mostly due to an increase in non-trade tax revenues: the share of trade taxes in GDP remains roughly constant over more than a century (1860 to 1980) while the tax ratio strongly increases. The share of trade taxes in GDP only clearly decreases from 1980 onwards, at which date trade taxes already represent a negligible share of total revenues. In developing countries on the contrary the tax transition is driven by a decrease in the share of trade taxes over GDP more than by an increase in tax ratios.

Changes in the number of countries in each group may lead to spurious changes in average values over time. In [panel B of Table 1](#) we focus on the recent period for which more data for developing countries are available and see a similar pattern when we only consider the 87 countries for which we have data in each decade from 1970 to 2006. In contrast with the steady increase in tax ratios over time seen in [panel A](#) we see that tax ratios have decreased in both MICs and LICs during the 1980s and 1990s: they fall by 2 GDP points in LICs over the period. The share of trade taxes in GDP falls by 25–50% in all country groups over the same period. This fall is more than enough to explain the decrease in total tax ratios over time in MICs and LICs but does not halt the increase in tax ratios in HICs.¹⁰ The last column in [panel B](#) shows the evolution of the average unweighted tariff, available only for the last two

⁶ [Mitchell \(2007\)](#) has no data on tax revenues by tax instrument prior to 1895 for this region.

⁷ As a result, the two contemporary datasets we use do not report any data on countries in the ex-Soviet bloc prior to the mid 1990s; [Mitchell \(2007\)](#) reports some historical tax data for Russia and the USSR but for most periods does not report trade tax revenues separately and points to problems in comparing even total tax revenues over time due to statistical inconsistencies.

⁸ We only use the AVE measure of tariffs in [Table 1](#) as the average unweighted tariff isn't available for most of the period under consideration.

⁹ This stylized fact was first documented by [Hinrichs \(1966\)](#).

¹⁰ Part of the very large increase in tax ratios in HICs may be due to changes in the data sources used over time. Our educated guess from comparing our data to official numbers released by countries statistical institutes is that social security contributions are not included in the [Mitchell \(2007\)](#) data but they are in the [Baunsgaard and Keen \(2010\)](#) data – see the online appendix for a discussion of this issue. Online appendix [Table 4](#) shows that the increase is smaller when we use only data from [Baunsgaard and Keen \(2010\)](#). This particularity of the data cannot affect our definition of episodes, thanks to our rule for combining data from different sources described above.

Table 1
Evolution of tax ratios, tax structures and GDP per capita since 1830 by level of development.

	High-income countries					Middle-income countries					Low-income countries				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	Total tax	Trade tax	GDP	AVE	Tariff	Total tax	Trade tax	GDP	AVE	Tariff	Total tax	Trade tax	GDP	AVE	Tariff
<i>A. Across historical periods</i>															
1830–1839	6.6	3.0	1674.5												
1860–1869	9.1	1.4	2161.2												
1890–1899	8.6	1.9	2334.6	11.7											
1920–1929	12.7	2.3	3504.6	6.7		11.5	2.3	1658.5	18.1						
1950–1959	16.9	2.0	5702.8	9.1		9.5	2.2	1734.2	15.9						
1970–1979	22.2	1.6	10754.0	7.1		15.9	4.2	3286.7	15.5		15.6	5.9	970.3	18.6	
2000–2006	27.6	0.6	16219.8	1.5		16.1	2.3	4069.0	6.5		14.0	3.1	928.9	9.7	
<i>B. Contemporary period</i>															
1970–1979	22.5	1.6	11016.0	7.1		16.3	4.4	3196.3	16.0		15.6	5.9	970.3	18.6	
1980–1989	25.0	1.1	12741.4	5.0		15.4	3.6	3346.2	14.0		15.1	5.3	946.5	16.7	
1990–1999	28.4	0.6	15430.2	3.6	5.9	15.1	2.8	3732.3	14.2	30.9	13.7	3.9	877.0	19.1	28.0
2000–2006	31.0	0.4	18272.8	1.2	4.7	15.7	2.3	4115.9	6.7	14.0	14.9	3.4	928.3	15.0	17.0

Notes: Each value is a mean over a decade. The table presents descriptive statistics on total tax revenues as percentage shares of GDP (columns 1, 6 and 11); trade tax revenues as percentage shares of GDP (columns 2, 7 and 12); GDP per capita in 1990 Geary–Khamis dollars (columns 3, 8 and 13); average weighted tariff rates (AVEs) in percent (columns 4, 10 and 14); and average unweighted tariff (*Tariff*) in percent (columns 5, 10 and 15). Countries are categorized by their level of economic development in 2006, see the text for a description of the data and the country income groups. Columns 1 to 5 present the data for High-Income Countries; columns 6–10 for Middle-Income-Countries; and columns 11–15 for Low-Income Countries. Each country is given equal weight in the mean. In panel A the number of observations in each decade from the top to bottom is (i) High-Income Countries (HICs): 3, 4, 9, 12, 18, 25; (ii) Middle-Income-Countries (MICs): 5, 12, 30, 43; (iii) Low-Income Countries (LICs): 30, 40. In panel B the sample consists of the 29 HICs, 28 MICs and 30 LICs for which we have data in all decades.

decades. We see this is highly correlated with the AVE measure of tariff; the average unweighted tariff falls in all country groups but more so in developing countries, where the average tariff in the 2000s is nearly half that in the 1990s.¹¹

3. Defining trade liberalization episodes

We use a fiscal definition of trade liberalization: we define trade liberalization episodes by a fall in trade tax revenues as a percentage of GDP of at least 1 GDP point from a local maximum to the next local minimum that is accompanied by a non-decrease in the volume of trade (imports) as a share of GDP.¹²

Our aim is to capture periods during which countries open up to trade through large decreases in tariffs levied on imports (or less frequently exports). We follow the trade economic history literature and use data on trade tax revenues to proxy for changes in tariffs (see [Lampe and Sharp, 2014](#), for a recent review). Formally, the variable we use to define trade liberalization episodes is the following: for each country i and year t we measure

$$t_{it} = \frac{\sum_j \tau_{jit} m_{jit}}{Y_{it}} \quad (1)$$

where τ_{jit} is the tariff rate on imports m_{jit} of commodity j and Y_{it} is the country's GDP.¹³ There are three well-known types of shortcomings of using changes in revenue data to measure changes in trade openness. First, this measure does not account for nontariff barriers – prohibitions or restrictions like red-tape requirements that discourage trade. Removing non-tariff barriers does not lead to a decrease in revenues – there is no potential fiscal cost – so these are not of interest here. Second, it is biased downward because it cannot capture the effect of extremely high tariffs in the average – at the limit a product with a 100% tariff will not be traded and the removal of such a tariff could lead to an increase in trade tax revenues. More generally, increases in tariffs set above their revenue-maximizing rates could lead to *decreases* in t_{it} as the fall in imports would more than compensate the increase in tax. Third, scaling trade tax revenues by GDP implies that declines in trade volumes m , unrelated to trade policy, could lead to decreases in t_i . We address both of these concerns by looking at the evolution of import volumes (also scaled by GDP) during falls in t_{it} and define episodes by a simultaneous fall in t_{it} and a non-decrease in $\frac{\sum_j m_{jit}}{Y_{it}}$. We use import volumes because most trade tax revenues are levied on imports but consider episodes defined by a decrease in trade tax revenues and a non-decrease in total trade (exports plus imports scaled by GDP) as a robustness check.

Ratios of tax revenues to GDP experience short-run fluctuations that may come from exchange rate volatility, changes in the reporting period or business cycles and be unrelated to change in tax policy. We isolate the trends in our data on total tax, trade tax and expenditure as a share of GDP to avoid confounding episodes of trade liberalization with short-run correlations. Our main method uses the Hodrick–Prescott filter; we follow [Ravn and Uhlig \(2002\)](#) in using a 6.25 smoothing parameter.¹⁴ We define the *size* of an episode by the difference between the local maximum value of trade tax revenues as a percentage of GDP at the start of the episode (year s) and the following local minimum value of trade tax revenues at the end of the episode (year e). The distance between year e and year s is the *length* of the episode.

We measure the potential fiscal cost of trade liberalization by looking at the evolution of total tax revenues as a percentage of GDP. By definition, total tax revenues are expected to fall during an episode unless countries are able to increase their tax collection from other (domestic) sources of tax revenues by an amount large enough to compensate for the fall in trade tax revenues. In the absence of such an increase, we say that countries experience a fiscal cost of trade liberalization. More precisely, we measure for each episode of decrease in trade tax revenues (i) whether total tax revenues as a share of GDP fall at the start of the episode; and (ii) if they do, the number of years before total tax revenues come back to their level before the start of the episode. Formally, we define the *revenue recovery* year (r) as the first year in which total tax revenues as a percentage of GDP are at least equal to their value in year s . We call the distance between year r and year s the (fiscal) *recovery time*.

[Fig. 2](#) illustrates graphically how we construct the episodes, the fiscal cost and the fiscal recovery variables using the example of Chile in the 1975–2006 period. The vertical red lines show the start of the episodes, the blue lines their end and the green lines the year of recovery. The first episode starts in 1975 and corresponds to the first phase of Chile's unilateral trade liberalization reform during which the country eliminated all quantitative trade restrictions and radically reduced all tariffs by replacing them with a uniform 10% tariff. We observe rapid fiscal recovery for this episode, as early as 1978. This episode ends in 1980 at the start of a severe economic crisis during which the government chose to raise tariffs to 35% to correct external and fiscal imbalances. This return to protectionism was short lived and the process of trade liberalization resumed in 1985 when tariffs were decreased again: we observe a second episode that starts in 1985 and ends in 1993, an eight-year period during which the uniform tariff was eventually reduced to 11% (see [Dornbusch and Edwards, 1994](#); [Edwards and Lederman, 2002](#), for a discussion of Chile's trade liberalization reforms). We observe no fiscal recovery for this episode.

¹¹ The correlation between AVE and unweighted tariff in our data is 0.69, significant at the 1% level. This is in line with a large literature that has found a high correlation between AVEs and more policy consistent measures of tariffs, see for example [Irwin \(2010\)](#) and [Kee et al. \(2008\)](#).

¹² We say that an observation is a local maximum (minimum) if it is higher (lower) than the preceding and following observations.

¹³ Some countries also levy tariffs on exports, in practice we measure $t_{it} = \frac{\sum_j \tau_{jit} m_{jit} + \sum_k z_{kit} x_{kit}}{Y_{it}}$ where z_{kit} is the tariff rate on exports x_{kit} of commodity k .

¹⁴ This corresponds to a value of 1600 for quarterly data. [Ravn and Uhlig \(2002\)](#) show that the smoothing parameter should be adjusted according to the fourth power of a change in the frequency of observations.

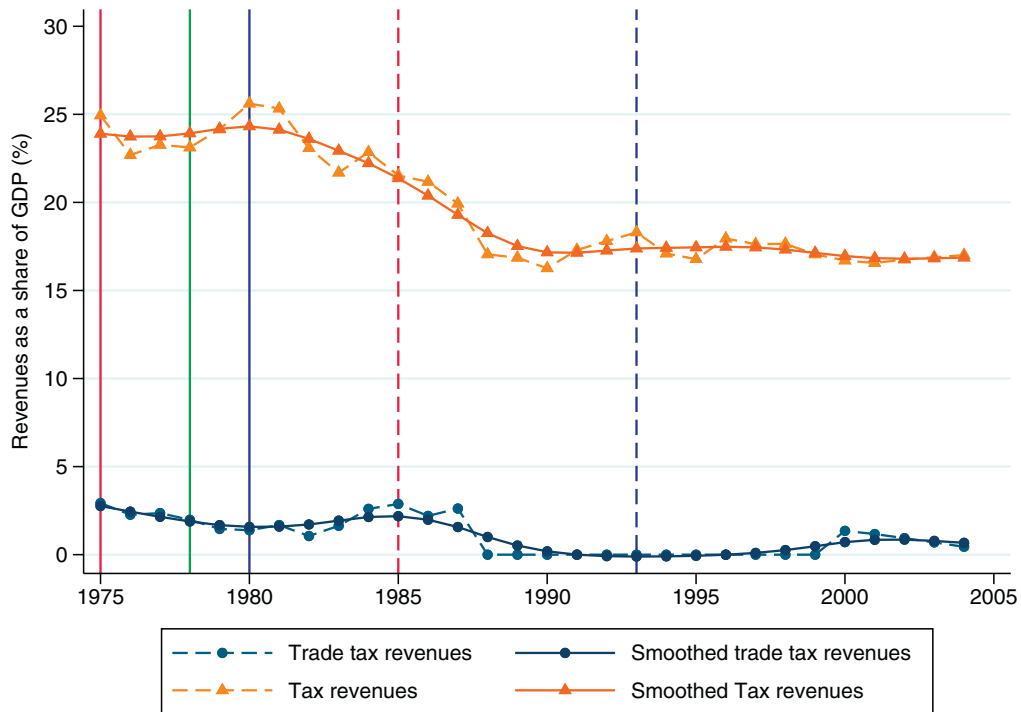


Fig. 2. Definition of trade liberalization episodes and fiscal recovery: example of Chile *Notes:* The figure illustrates our method for constructing episodes of trade liberalization and the fiscal recovery variable. The solid lines illustrate the 1975 episode of trade liberalization, and the dashed lines the 1985 episode of trade liberalization. The vertical red lines show the start of the episodes, the blue lines their end and the green lines the years of recovery. See the text for a description of the dataset used. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

Our method only considers trade liberalization episodes characterized by decreases in trade tax revenues. This is justified by our interest in the fiscal consequences of trade liberalization – if trade tax revenues do not decrease during an episode there will by definition be no fiscal cost – but leads us to ignore other forms of trade liberalization. We do not consider for example liberalization reforms that remove quantitative constraints on trade, such as outright prohibitions or strict licensing rules. We discuss an example of such reforms in Spain in the 1960s below.

Likewise, we do not classify as episodes decreases in truly prohibitive tariffs that lead to an increase in trade and have a positive impact on trade tax revenues; this is a plausible outcome mostly for decreases in tariffs levied on commodities for which domestically-produced substitutes are widely available, which typically have high trade elasticities. We note however that even during the highly protectionist late 19th century period in Latin America average tariffs were always much lower than the revenue-maximizing rate: Coatsworth and Williamson (2004) estimate a revenue-maximizing rate of roughly 50% and report average unweighted tariffs always below 35%. Overall, whilst there clearly were over the course of history some episodes of decreases in tariffs initially set above the revenue-maximizing rate (see Irwin, 1998, for an example), historical evidence on trade elasticities suggests average tariffs were typically lower than the revenue-maximizing rate.¹⁵ It is therefore not unreasonable to think that decreases in tariffs, more often than not, led to decreases in trade tax revenues. This being said all our results in what follows should be interpreted as relevant regarding trade liberalization that leads to a decrease in trade tax revenues, and not regarding all possible forms of trade liberalization.

Two other types of changes unrelated to trade liberalization could lead to decreases in t_{it} . First, negative shocks to countries' capacity to collect taxes could lead to decreases in trade and non-trade tax revenues, this would lead us to confound a fiscal cost of trade liberalization with increases in tax evasion. The administration of customs tax collection is, historically and across countries today, often separated from general tax collection (Alink and van Kommer, 2016) so this scenario is fairly unlikely, but it remains a concern. Second, changes in the structure but not level of trade volumes, away from heavily taxed imports, could lead to a decrease in t_{it} . We consider whether the episodes we identify are accompanied by changes in trade policy instruments – decreases in average tariff rates ($\sum_j \tau_{jit}$) – to assess the validity of these concerns for the sub-sample of episodes for which data on these instruments are available.

¹⁵ See Tena-Junguito (2006) for estimates of trade elasticities in the 19th century.

Finally, note that our definition of fiscal “recovery” assumes that tax-GDP ratios would have remained constant in the absence of a trade liberalization episode. This may lead us to over-estimate the extent to which countries are able to recover the lost tax revenues as the literature (and our own evidence in Table 1) has found that tax-GDP ratios tend to increase over time and as countries develop (see for example Lindert, 2004).

4. Trade liberalization episodes

We find 99 episodes of decreases in trade tax revenues and no decrease in trade, we list episodes by country and date in Appendix Table 6. Fig. 1 presents the distribution of these episodes and the number of countries in our sample over time. We see a sharp increase in the number of episodes over time which in large part reflects the increase in sample size: two thirds of the episodes occur during the contemporary period (post 1970) for which we have more data.

We find seven episodes between the mid-19th century and 1914, during the “first wave” of trade liberalization characterized by falling trade costs (Jacks et al., 2010; Mohammed and Williamson, 2004) and the rise of the gold standard which facilitated trade (Estevadeordal et al., 2003; Mitchener et al., 2010; Mitchener and Weidenmier, 2008). More surprisingly perhaps we also find seven episodes during the interwar period, including four during the Great Depression – we discuss these in detail below. Another eighteen episodes occur during the “second wave” of trade liberalization (1945–1969), an era characterized by multilateral trade negotiations under the auspices of the GATT and the Bretton Woods system of fixed exchange rates, as well as technological changes that further decreased trade costs (Hummels, 2007). Finally, most of the remaining episodes occur in developing countries during the most recent wave of globalization since the late 1970s, this time in a context during which countries gradually shifted from currency pegs (typically to the US dollar or French franc) to more flexible exchange rate arrangements (Aziz and Caramazza, 1998). Many of these episodes take place during the Uruguay Round (1986–1994) of trade negotiations, the first GATT round during which developing countries committed to significant policy changes towards trade liberalization (Estevadeordal and Taylor, 2013).

Table 2 presents descriptive statistics on our entire sample of episodes and Table 3 focuses on the historical (pre-1970) period. The first column in Table 2 shows that the average loss in tax revenues due to an episode is large: trade tax revenues fall on average by 2.9 GDP points during the episode (column 1). This fall represents 58% of the average trade tax revenues and 17% of total tax revenues at the start of the episode; the typical episode lasts 12 years. This section first considers the episodes we find in the historical (pre-1970) period and then turns to the comparison between historical and contemporary episodes.

4.1. Before world war 1

We observe seven episodes of trade liberalization prior to 1914: two in the United Kingdom (1842 and 1851), one in Denmark in 1897, in Sweden in 1899, in Brazil in 1906, in Finland in 1908 and in Norway in 1909. Column 2 in Table 3 shows that these episodes are relatively small (never larger than 2.5 GDP points) and spread out over a relatively long time period. The 1842 episode in the UK takes place immediately before the famous repeal of the Corn Laws; we return to the circumstances in which this first episode occurred in section 8. The 1851 episode corresponds to the completion by the then Chancellor of the Exchequer William Gladstone of the change in tariff reductions initiated by Prime Minister Robert Peel in 1842. This episode was particularly long: we observe a continuous decline in trade tax revenues as a share of GDP in the UK from 1851 to 1874, the year in which Gladstone’s Liberals were replaced by the Conservatives. Trade tax revenues then remain stable around 1.6% of GDP for the remainder of the 19th century.

The 1897 episode in Denmark is a priori surprising given Denmark’s reluctance to take part in the post Cobden–Chevalier network of bilateral treaties in the second half of the nineteenth century (Lampe and Sharp, 2011); this reluctance may however explain the slight increase in trade tax revenues as a share of GDP we observe in the 1890s in Denmark, prior to a long decrease starting in 1897 and continuing more or less uninterrupted until 1914.¹⁶

The absence of France from the list of early trade liberalization episodes may also seem surprising. Settling the “Fortress France” debate (see Irwin, 1993; Nye, 1991) is beyond the scope of this paper, but three elements help explain why we see no episode in France in the 19th century.¹⁷ First, our data for France start in 1847 so we cannot consider potential earlier episodes. Second, we do see a decline in trade tax revenues as a share of GDP from 1860 onwards (the year in which the Cobden–Chevalier Treaty was signed) but starting from a low level of less than 1 GDP point: the share of trade tax revenues to GDP in 19th century France was too low for us to detect an episode, in line with the discussion in Nye (1991). Third, a major component of the Cobden–Chevalier Treaty – the abolition of import prohibitions – was an example of a trade liberalization policy that cannot lead to a decrease in trade tax revenues and hence is not captured by our method. France indeed prohibited the importation of a large number of product varieties before 1860 (see e.g. Tena-Junguito et al., 2012).

Germany is also absent from our list of pre-1914 liberalization episodes. Our data for Germany begin in 1872, at the start of the German Empire which was then committed to free trade. Following the general elections in 1877 and 1878, Germany was “at the forefront of what would become a large-scale setback for globalization in the period before World War I” (Lehmann, 2010), in particular

¹⁶ This decrease in the share of trade tax revenues in GDP is mirrored by a decrease in the AVE variable constructed by Clemens and Williamson (2004) which drops from 9.5% at its 1897 peak to 7% in 1906 and 4.5% in 1913.

¹⁷ While Nye (1991) argues that French average tariff levels were consistently below those of Britain throughout most of the nineteenth century, contradicting the common view that Britain was the great nation of free trade at the time, Irwin (1993) claims that the tariff revenue figures used by Nye (1991) may not be appropriate to measure free trade policy. According to his interpretation of the commercial policies implemented at the time by both countries, tariff protection was much higher in France than in Britain in the second half of the nineteenth century.

Table 2
Episodes of decreases in trade tax revenues: comparison by period and level of development.

	1792–1969					1970–2006				Diff. p-values	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Entire sample	All countries	HICs	MICs	LICs	All countries	HICs	MICs	LICs	Rich/dvp	Pre/post
Size of the episode (GDP points)	2.9 (2.0)	2.4 (1.4)	1.9 (0.9)	2.5 (1.5)	2.4 (1.2)	3.1 (2.2)	2.4 (1.7)	2.6 (1.7)	3.5 (2.5)	0.32	0.06
Length of the episode (years)	12 (6)	14 (7)	17 (8)	15 (7)	10 (4)	12 (6)	15 (1)	10 (5)	13 (7)	0.25	0.05
Total tax revenues (% GDP)	16.8 (6.8)	15.3 (6.6)	19.7 (8.2)	15.4 (6.9)	12.5 (3.7)	17.5 (6.8)	23.3 (9.2)	19.3 (6.0)	16.1 (7.2)	0.08	0.03
Trade tax revenues (% GDP)	5.0 (4.0)	4.0 (1.7)	3.0 (0.9)	4.0 (1.8)	4.6 (1.4)	5.5 (4.6)	2.4 (1.7)	4.5 (2.7)	6.5 (5.6)	0.33	0.05
% Pre-empted episodes	6.9 (25.6)	7.1 (26.2)	25.0 (50.0)	5.0 (22.4)	0.0 (0.0)	6.8 (25.5)	. (.)	4.8 (21.8)	8.7 (28.8)		
Number of episodes	99	32	4	21	7	67	2	28	37		
Number of countries	85	25	4	18	7	63	2	27	34		
Number of observations	5206	1862	178	1152	532	3344	927	1224	1193		

Notes: Mean (standard error). The table presents descriptive statistics on our sample of 99 episodes of decrease in trade tax revenues. HICs, MICs and LICs stand for, respectively, High-Income, Middle-Income and Low-Income countries. The last two columns present two-sided p-values for the difference between developing countries (LICs and MICs) in 1970–2006 and i) HICs in 1970–2006 (column 10) ii) developing countries in 1792–1969 (column 11). The number of observations refers to the number of observations in our data set for the period and country income group under consideration. Column 1 presents results for the entire sample of episodes. In columns 2–5 (respectively 6–9), we present results considering only the period 1792–1969 (respectively 1970–2006). HICs (columns 3 and 7), MICs (columns 4 and 8) and LICs (columns 5 and 9) are defined using the country's income group at the start of the episode. The last two columns present p-values for the difference between developing countries (LICs and MICs) in 1970–2006 and i) HICs in 1970–2006 (column 10) ii) developing countries in 1792–1969 (column 11). See the text for a description of the dataset and the method used to construct episodes.

Table 3
Episodes of decreases in trade tax revenues pre-1970.

	(1)	(2)	(3)	(4)
	1792–1969	Pre-WWI	Interwar period	Bretton-Woods period
Size of the episode (GDP points)	2.4 (1.4)	2.3 (1.0)	1.6 (0.5)	2.7 (1.6)
Length of the episode (years)	14 (7)	15 (7)	8 (2)	16 (7)
Total tax revenues (% GDP)	15.3 (6.6)	9.6 (2.7)	12.3 (6.0)	18.6 (6.0)
Trade tax revenues (% GDP)	4.0 (1.7)	4.1 (1.6)	3.7 (1.5)	4.1 (1.8)
Number of episodes	32	7	7	18
Number of countries	25	6	6	16
Number of observations	1862	525	443	894

Notes: Mean (standard error). The table presents descriptive statistics on our sample of 32 episodes of decrease in trade tax revenues that occur prior to 1970. Column 1 presents results for the entire sample of episodes. In columns 2–4 we present results considering only the pre-World War I period (1792–1914), the interwar period (1915–1939), and the Bretton Woods period (1945–1969). See the text for a description of the dataset and the method used to construct episodes.

with the adoption of the “iron and rye tariffs” in 1879 (Wolf, 2009). Hence what we observe in the data at the end of the 19th century for Germany is an increase rather than a decrease in trade taxes over GDP, from 1,26% in 1878 to 2,6% in 1891.

4.2. The interwar period and the great depression

We observe seven episodes during the interwar period, these episodes are substantially smaller and shorter lived than the average (third column of Table 3). The first two occur during the 1920s: Bulgaria in 1924 and Chile in 1927. The Chilean episode is particularly interesting as an example of a country that liberalized trade after a major investment in tax capacity: trade taxes were the main source of public revenues in Chile in the 19th century and the early 1920s (Lederman, 2001) but the introduction of an income tax in 1924–1925 meant tariffs became less essential and could be decreased; we indeed observe immediate fiscal recovery in Chile in 1927 (see next section).

We also observe four episodes of trade liberalization during the Great Depression (1929–1938): in Belgium, Ireland and Switzerland in 1934, and in the Netherlands in 1936. To better understand the mechanisms at play for these particular episodes, online Appendix Fig. 1 plots the evolution of total and trade tax revenues as well as the AVE variable from Lampe and Sharp (2013) over the period for these four countries. We see a clear increase in trade tax revenues as a share of GDP and/or AVE in Belgium, Switzerland and the Netherlands prior to the start of the episodes. These three countries were still part of the gold standard until the mid 1930s and were therefore amongst those that initially resorted to increases in tariffs the most to respond to the economic crisis, as highlighted in O’Rourke (2017) (see also Eichengreen and Irwin, 2010; Eichengreen, 1992). The start of the episodes coincide with the dates at which these three countries abandoned the gold standard and hereafter relaxed their protectionist policy.

The Irish episode similarly follows a period of very protectionist policy: Ireland increased its tariffs substantially during the early 1930s Anglo-Irish Trade War (Whitaker, 1974). The “war” lasted until 1938 but the tensions began to ease off from 1935 and the Coal-Cattle Pact – around the time of the start of the episode. This first Irish episode is substantially smaller than the two that occur in this country in the post World War II period and during which the country definitely moved from protectionism to free trade.¹⁸ Overall, the trade liberalization episodes we observe during the Great Depression are perhaps best understood as re-liberalization episodes that follow protectionist policies (see also Williamson, 2006), we consider the robustness of our results to dropping these episodes from our sample below.

4.3. The Bretton Woods period

We observe eighteen episodes during the Bretton Woods period, defined here as 1945–1969.¹⁹ One well-documented case of trade liberalization is absent from the list: Spain’s gradual loosening, in the early 1960s, of the strict trade controls of the early years of the Franco regime does not lead to a trade liberalization episode. This is a good example of a trade liberalization case that did not come at the expense of trade tax revenues – and therefore isn’t detected by our method – simply because the pre-liberalization policies were characterized by non-tariff barriers to trade: strict currency controls and quasi-prohibition of most imports. The trade liberalization reforms of the early 1960s if anything increased trade tax revenues (which we find represent less than 2% of GDP for Spain even

¹⁸ The last Irish episode happens in 1967 and coincides with the 1966 start of the Anglo-Irish Free Trade Area.

¹⁹ Through a misuse of language; the Bretton Woods agreement was effectively brought to an end in 1971.

at the start of the 20th century), in part because quantitative restrictions on foreign trade were replaced by tariffs (see Prados de la Escosura et al., 2012; Lampe and Sharp, 2013).²⁰

The average episode during this period, described in the last column of Table 3, looks extremely similar to the average episode in the contemporary (post 1970) period, described in column six of Table 2. The main difference with the contemporary period therefore lies in the type of countries in which they occur: all of the episodes between 1945 and 1969 except two – Indonesia in 1951 and Thailand in 1964 – occur in high-income-countries whereas only two of the sixty-seven post-1970 episodes do. By 1970, trade tax revenues in high-income countries represented a very small share of GDP, too small for us to detect more episodes of trade liberalization (see Table 1). The post-1970 episodes thereby represent mostly the experience of developing countries.

4.4. Trade liberalization episodes from 1970 onwards

In Table 2, we consider separately the pre- and post-1970 periods and countries that were HICs, MICs and LICs at the start of the episode to investigate whether today's developing countries experienced a different fiscal cost of trade liberalization from today's HICs when they decreased trade tax revenues in the 19th and early 20th centuries.²¹ Our main focus in this table is on the comparison between the trade liberalization experiences of today's developing countries and (i) that of rich countries since 1970, and (ii) that of rich countries at earlier stages of development. We therefore present for each variable the p-value of the differences between developing countries (MICs and LICs) in the post-1970 period and on the one hand (i) rich countries in the same period (column 10), and on the other hand (ii) developing countries in the pre-1970 period (column 11).

We see that episodes are significantly deeper (bigger sizes spread out over shorter periods) since 1970. Looking at differences by income group at the start of the episode we find that poorer countries are much more likely to experience episodes since 1970: there are 0.2 episodes per 100 observations among HICs, 2.3 among MICs and 3.1 among LICs. They also have significantly lower total tax-to-GDP ratios and experience slightly deeper episodes though the difference is not statistically significant. Fig. 3 plots the distribution of episode sizes by country income group (for all periods). We see that not only do poorer countries experience bigger episodes on average, the entire distribution of episodes is shifted to the right for LICs versus MICs, and MICs versus HICs.

Categorizing a decrease in total tax revenues after the start of an episode as a “fiscal cost of trade liberalization” is not appropriate if the decrease in trade tax revenues has been pre-empted. Countries may decide to increase tax revenues from domestic sources before lowering tariffs precisely to anticipate the coming fall in trade tax revenues. We consider the evolution of domestic tax revenues in the years prior to the start of the episode to investigate whether such pre-emptive measures occur: the penultimate line of Table 2 presents the share of countries in which total tax revenues increase in the 3 years prior to the episode by at least as much as the size of the episode. We see that on average few countries – 6.9% – preempt the loss in trade tax revenues (column 1).²²

Table 4 presents the number of episodes of decreases in trade tax revenues of more than 1 GDP point, the number of episodes obtained using our definition (decrease of more than 1 GDP and non-decrease in trade) and the evolution of average unweighted and weighted tariff rates during the episodes, when data are available. We note that there are 30% more cases of decreases in trade tax revenues of more than 1 GDP than episodes, defined by a decrease in trade tax revenues of that magnitude and a non-decrease in trade. Online Appendix Table 6 lists the episodes, their start dates and whether or not they were accompanied by a fall in average weighted or unweighted tariff, we include in the list decreases in trade tax revenues that are accompanied by a fall in trade.²³ Overall we see that cases for which trade tax revenues and trade decline simultaneously occur as expected in periods of contraction of both GDP and trade – the majority take place in sub-Saharan Africa in the late 1970s and 1980s, the region's “lost decade” characterized by stagnating GDP and trade (Easterly, 2001), some occur in Europe and Latin America during the trade contraction years of the Great Depression.²⁴

For the post-1988 period we check whether the episodes occurred during periods of changes in the main trade policy variable available to governments. We find that, out of the 30 episodes that occur in that period, 23 are accompanied by a decrease in tariffs, 5 occur in countries for which data are not available and only 2 are accompanied by a small increase in tariffs.²⁵ The average change in tariffs during an episode is –29%. The value of this exercise is limited by the small number of episodes under consideration but we find this evidence reassuring: for the post-1988 period at least it seems that most of our episodes are at least partially driven by changes in policy variables.

²⁰ Lampe and Sharp (2013) also find that their AVE measure is not a good proxy for trade openness in the case of Spain as it increases after the 1959 “Stabilization and Liberalization Plan”.

²¹ The choice of the 1970 year to split our sample is driven by the fact that for the majority of developing countries in our sample, data only become available a few years after independence.

²² Excluding these episodes from our sample does not change the overall picture in Table 2 (see Appendix Table 10).

²³ To look at the 99 episodes included in our baseline sample, one must only consider the episodes for which the variable in the first column on the Online Appendix Table 6 – “Fall in imports” – is not equal to 1.

²⁴ We also report the liberalization dates used in Wacziarg and Welch (2008) to compare our episodes with a measure of trade openness commonly used in the literature. These authors use a very different definition of openness from ours (the measure first constructed by Sachs and Warner (1995) based on data on tariffs, nontariff barriers, exchange rate black market premia, socialist economic systems and export marketing boards); we did not expect our dates to match precisely. We find that, out of the 47 episodes for which Wacziarg and Welch (2008) also report a potential liberalization dates, our episode start date and theirs are 5 years apart or less in 50% of cases. Overall, our trade liberalization episodes tend to occur before the openness dates of Wacziarg and Welch (2008), in line with the differences between their definition and ours: they categorize a country as open once it meets 5 criteria, only one of which is low tariff rates.

²⁵ These 2 episodes take place in Chad in 1999 and Mozambique in 1989, all our results below are unchanged if we exclude these 2 episodes.

Table 4
Evolution of tariff measures during episodes .

	1792–1969					1970–2006			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Entire sample	All	HICs	MICs	LICs	All	HICs	MICs	LICs
Nb decreases in trade taxes > 1 GDP point	140	38	4	25	9	102	3	44	55
Nb episodes	99	29	3	21	5	70	2	28	40
<i>Evolution of average unweighted tariff</i>									
Nb episodes with data	25	0	0	0	0	25	2	15	8
Nb episodes with decrease	23	0	0	0	0	23	2	15	6
Average change (%)	–28.59 (50.77)	–28.59 (50.77)	–25.49 (13.01)	–41.22 (22.07)	–11.37 (52.86)
Predicted size (GDP points)	7.27–15.31 (9.62)–(8.93)	12.67–13.70 (7.69)–(8.16)	5.86–16.77 (12.16)–(9.74)	7.85–12.20 (4.59)–(7.26)
<i>Evolution of average weighted tariff (AVE)</i>									
Nb episodes with data	84	25	3	19	3	59	2	27	30
Nb episodes with decrease	78	24	3	18	3	54	2	27	25
Average change (%)	–39.42 (39.05)	–59.17 (33.41)	–80.8 (10.9)	–62.17 (33.11)	–18.57 (12.17)	–35.14 (42.5)	–81.74 (25.56)	–52.03 (23.40)	–16.83 (48.47)
Number of countries	110	30	4	22	8	85	3	39	46
Number of observations	5206	1862	178	1152	532	3344	927	1224	1193

Notes: Mean (standard error). From line 2 onwards the results refer to our sample of 99 episodes. We say that variable x (average unweighted or average weighted tariff) falls during the episode if variable x decreases between the episode start and end dates. The number of observations refers to the number of observations in our data set for the period and country income group under consideration. HICs, MICs and LICs stand for, respectively, High-Income, Middle-Income and Low-Income countries. Column 1 presents results for the entire sample of episodes. In columns 2–5 (respectively 6–9), we present results considering only the period 1792–1969 (respectively 1970–2006). HICs (columns 3 and 7), MICs (columns 4 and 8) and LICs (columns 5 and 9) are defined using the country's income group at the start of the episode. See the text for a description of the dataset and the method used to construct episodes.

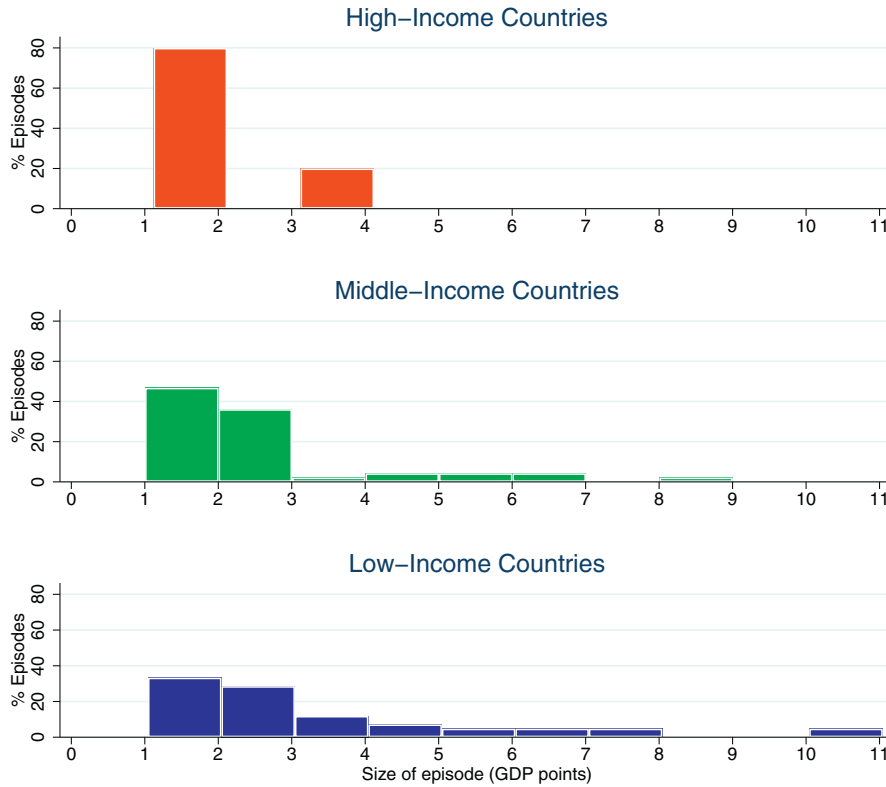


Fig. 3. Distribution of episode sizes Notes: Each bar represents the share of episodes of a given size, the sample includes all 99 episodes (5 in High-Income Countries, 49 in Middle-Income Countries and 45 in Low-Income Countries).

We use the observed change in average unweighted tariff to compute two measures of the “potential size” of the episode: the decrease in trade tax revenues we would have seen if the decrease in the average tariff rate had been applied to all imports. This helps us assess whether what we define as the size of the episode could be driven only by the observed decrease in tariffs; a smaller “potential size” than real size would suggest we are capturing other trends, unrelated to trade liberalization, that lead to decreases in trade tax revenues. Our first measure assumes the imports-GDP ratio remains the same throughout the episode (this is equivalent to assuming that elasticities of both imports and GDP with respect to trade are zero); this gives us a likely upper bound on the true potential size. Our second measure uses the import elasticity estimates in Kee et al. (2008) to construct the change in imports due to the decrease in the average tariff rate; we apply this elasticity to import-GDP ratios so this likely gives us a lower bound on the true potential size (this assumes that imports increase during the episodes but GDP remains constant).²⁶ We see that even the lower bound on the potential size is on average orders of magnitude larger than the effective decrease in trade tax revenues observed during the episode (and larger for all episodes considered). Finally, the third panel of Table 4 presents the evolution of average ad valorem equivalent tariff rate (AVE). We find that there is a decline for 93% of the 84 episodes for which AVE data are available. This confirms that our trade liberalization episodes are unlikely to be driven by changes in trade volumes, which enter both the numerator and the denominator of this variable.

5. The fiscal cost of trade liberalization

Table 5 presents our measure of the fiscal cost of trade liberalization across the three (pre-1970) historical periods and Table 6 considers whether this cost differs between the historical and contemporary periods and by level of development. We find several patterns of interest.

First, roughly one-fourth of the countries are never observed going back to their pre-episode level of tax revenues – we say that they experience no fiscal recovery (second line of Table 6). This is significantly less likely in the historical period. In Table 5 we see

²⁶ Formally, writing m_i the import-GDP ratio at the start of the episode in country i , t_i the average tariff rate at the start of the episode and $\Delta(t_i)$ the decrease in the average tariff rate, our upper bound is defined as $\Delta(t_i)m_i$ and our lower bound as $\Delta(t_i)m_i(1 + \epsilon_{mi} \frac{t_i}{1+t_i})$ where ϵ_{mi} is the (average) import elasticity estimate in Kee et al. (2008) for country i and t_i the average tariff rate at the start of the episode.

Table 5
The fiscal cost of trade liberalization pre 1970.

	(1)	(2)	(3)	(4)
	1792–1969	Pre-WWI	Interwar period	Bretton-Woods period
% Episodes with no fall in total tax revenues	62.5 (49.2)	42.9 (53.5)	57.1 (53.5)	72.2 (46.1)
% Episodes with no fiscal recovery	9.4 (29.6)	28.6 (48.8)	14.3 (37.8)	0.0 (0.0)
Actual recovery time (years)	5 (7)	7 (9)	4 (6)	5 (7)
Potential recovery time (years)	41.9 (22.1)	60.3 (33.5)	38.7 (21.4)	36.1 (12.5)
% Episodes with recovery under 5 years	64.5 (48.6)	42.9 (53.5)	66.7 (51.6)	72.2 (46.1)
% Episodes with recovery under 10 years	71.0 (46.1)	42.9 (53.5)	83.3 (40.8)	77.8 (42.8)
% Episodes with recovery under 20 years	88.9 (32.0)	66.7 (51.6)	100.0 (0.0)	93.8 (25.0)
Number of episodes	32	7	7	18
Number of countries	25	6	6	16

Notes: Mean (standard error). The table presents descriptive statistics on fiscal recovery for the 32 episodes of decrease in trade tax revenues that occur prior to 1970. Column 1 presents results for the entire sample of episodes. In columns 2 to 4 we present results considering only the pre-World War I period (1792–1914), the interwar period (1915–1939), and the Bretton Woods period (1945–1969). See the text for a description of the dataset and the method used to construct episodes.

that quick fiscal recovery was extremely common for episodes during the Bretton Woods period (column 4) which coincides both with post-war trade liberalization and expansions of the welfare state. All countries eventually recover during this period, nearly three-fourths experience no fiscal cost at all. Earlier episodes in columns 2 and 3 were more likely to lead to a fiscal cost and less likely to be seen to ever recover, though this lack of recovery amongst early episodes can partially be explained by data constraints: we do not measure recovery when a data series for a country is interrupted (because of no overlap between data sources, or a long period with no data) before the tax ratio goes back to its pre-episode level, this happens for some of the countries in which we observe episodes prior to 1945.²⁷

Second, developing countries experience a fiscal cost more often than rich countries. The first line of the table presents the share of countries for which we do not observe any fall in total tax revenues at the start of an episode: in these countries trade tax revenues fall but domestic sources of tax revenues immediately increase more than enough to compensate this fall. This occurs for half of the episodes and more often in the pre-1970 period than in the recent period. Developing countries are more than twice as likely as rich countries to experience a fall in total tax revenues in the recent period. None of the HICs for which we see an episode after 1970 also experience a fiscal cost of trade liberalization, while nearly 50% of the MICs and 60% of the LICs do. There are also significant differences in the probability of fiscal recovery by level of economic development. All the HICs experience a fiscal recovery but a third of the developing countries for which the episode starts after 1970 do not. In the pre-1970 period some of the countries that were MICs or LICs at the time also do not experience a fiscal recovery but they were a lot less likely to do so than developing countries in the recent period.

Third, the countries for which we observe both a fiscal cost and a fiscal recovery return to their pre-episode level of tax revenues slightly faster since 1970 than before 1970 (see “actual recovery time” in the third line of the Table). Rich countries that recover also typically do so faster than developing countries in the post 1970 period, though the difference is not statistically significant.

The fact that countries that experience episodes since 1970 are less likely to recover fiscally but recover faster when they do suggests that part of the differences across periods may be due to data truncation. We observe countries for a smaller number of years in the post-1970 than in the pre-1970 period and may not have long enough time series after the most recent episodes to observe fiscal recoveries. Similarly, we could be observing less recovery in developing countries because data series are typically shorter for these countries than for HICs. We check that this is not what is driving our results by considering the number of years for which we observe countries in the data after the start of the episode (see ‘potential recovery time’, fourth line of the Table). This number is indeed higher in the pre- than in the post-1970 period, but if anything poorer countries are observed for slightly longer after the start of the episodes. Results are moreover similar when we consider the probability of recovery amongst only countries which we observe for five, ten or twenty years after the start of the episode (see the last three lines of the Table). Regardless of the time period used we see developing countries recovering more in the pre- than in the post-1970 period (though differences are not always statistically significant); developing countries are significantly less likely to recover than HICs in the post-1970 period.

²⁷ Norway is a good example. We observe an episode starting in 1909, and no recovery, but this is because our first data series for Norway ends in 1939. Our second data series for Norway starts in 1946, at which point the tax ratio more than exceeds its level at the start of the 1909 episode, but our method does not classify this as a recovery.

Table 6

The fiscal cost of trade liberalization: comparison by period and level of development.

	1792–1969					1970–2006				Diff. <i>p</i> -values	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Entire sample	All countries	HICs	MICs	LICs	All countries	HICs	MICs	LICs	Rich/dvp	Pre/post
% Episodes with no fall in total tax revenues	52.5 (50.2)	62.5 (49.2)	50.0 (57.7)	71.4 (46.3)	42.9 (53.5)	47.8 (50.3)	100.0 (0.0)	53.6 (50.8)	40.5 (49.8)	0.07	0.05
% Episodes with no fiscal recovery	24.2 (43.1)	9.4 (29.6)	0.0 (0.0)	9.5 (30.1)	14.3 (37.8)	31.3 (46.7)	0.0 (0.0)	32.1 (47.6)	32.4 (47.5)	0.15	0.01
Actual recovery time (years)	4 (6)	5 (7)	6 (6)	3 (5)	10 (10)	4 (5)	1 (0)	3 (5)	5 (6)	0.22	0.29
Potential recovery time (years)	25.5 (17.9)	41.9 (22.1)	30.8 (6.5)	41.1 (21.1)	50.7 (29.1)	17.6 (7.2)	14.5 (0.7)	14.2 (5.8)	20.3 (7.2)	0.27	0.00
% Episodes with recovery under 5 years	55.1 (50.0)	64.5 (48.6)	50.0 (57.7)	75.0 (44.4)	42.9 (53.5)	50.7 (50.4)	100.0 (0.0)	53.6 (50.8)	45.9 (50.5)	0.08	0.06
% Episodes with recovery under 10 years	63.7 (48.3)	71.0 (46.1)	75.0 (50.0)	80.0 (41.0)	42.9 (53.5)	60.0 (49.4)	100.0 (0.0)	65.2 (48.7)	54.3 (50.5)	0.10	0.15
% Episodes with recovery under 20 years	76.9 (42.5)	88.9 (32.0)	100.0 (0.0)	94.4 (23.6)	60.0 (54.8)	64.0 (49.0)	. (.)	100.0 (0.0)	55.0 (51.0)	.	0.03
Number of episodes	99	32	4	21	7	67	2	28	37		
Number of countries	85	25	4	18	7	63	2	27	34		

Notes: Mean (standard error). The table presents descriptive statistics on fiscal recovery for all episodes of decreases in trade tax revenues. HICs, MICs and LICs stand for, respectively, High-Income, Middle-Income and Low-Income countries. The last two columns present two-sided *p*-values for the difference between developing countries (LICs and MICs) in 1970–2006 and i) HICs in 1970–2006 (column 10) ii) developing countries in 1792–1969 (column 11). The number of observations refers to the number of observations in our data set for the period and country income group under consideration. The last three lines restrict the sample to episodes for which we have data for at least 5, 10 or 20 years after the start of the episode. Column 1 presents results for the entire sample of episodes. In columns 2–5 (respectively 6–9), we present results considering only the period 1792–1969 (respectively 1970–2006). HICs (columns 3 and 7), MICs (columns 4 and 8) and LICs (columns 5 and 9) are defined using the country income group at the start of the episode. Episodes are obtained on data smoothed using a HP filter with a smoothing parameter of 6.25. See the text for a description of the dataset and the method used to construct episodes.

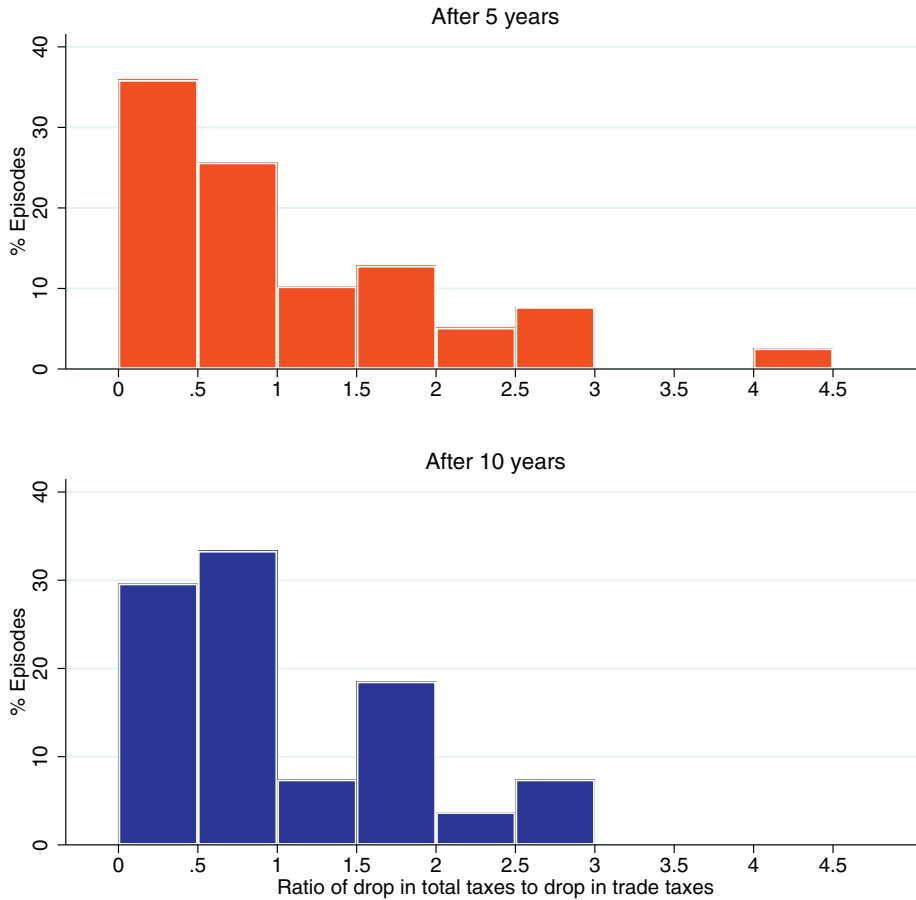


Fig. 4. Distribution of decrease in total tax revenues during an episode *Notes:* Each bar represents the share of episodes with a ratio of fall in total taxes to fall in trade taxes (episode size) of a given size 5 years (top graph) or 10 years (bottom graph) after the start of the episode. The fall in total taxes and in trade taxes are expressed in GDP points. The sample includes the 41 episodes for which there is no tax recovery after 5 years (top graph) or the 30 episodes for which there is no tax recovery after 10 years (bottom graph).

How large was the fall in total taxes during episodes? Fig. 4 presents the distribution of the fall in total tax revenues divided by the fall in trade tax revenues after 5 and 10 years for the episodes without fiscal recovery after 5 or 10 years. On average the fall in total tax revenues is smaller than the fall in trade taxes (the average ratio is 0.79 after 5 years, 0.82 after 10 years) but 30% of countries that have not recovered after 5 years experience a fall in total tax revenues that is at least as big as the decrease in trade tax revenues during the episode, that number is slightly larger (close to 40%) after 10 years.

6. Effects on government expenditures

We do not attempt to discuss the potential net welfare gain or cost of trade liberalization here. Our results suggest that trade liberalization has, in some cases, led to a decrease in tax revenues, but this potential welfare loss may be more than offset by non-fiscal welfare gains from trade liberalization. Moreover, a fall in tax revenues does not directly translate into a welfare cost if it does not lead to a decrease in public spending. If it is compensated for by an increase in other sources of revenues – development aid, revenues from natural resources or borrowing, for example – the main potential reason to worry about the fiscal cost of trade liberalization becomes moot. This is particularly meaningful in the post-1970 period given the importance that the “aid for trade” paradigm has taken during this period. This paradigm advocates poverty alleviation via aid aimed at expanding export opportunities and domestic complementarities to trade (see for example Balat et al., 2009). We therefore turn to data on the share of government expenditures to GDP and consider whether (i) government expenditures fall at the start of episodes and (ii) when they do, the number of years before government expenditures come back to their pre-episode level.

Table 7 presents our results regarding government expenditures. The shares of countries that experience a decrease in expenditures or are never observed to recover are slightly smaller than in Table 6 for most income and period groups, particularly for LICs. This suggests that non-tax sources of public revenues may sometimes have been used to compensate for the fall in trade taxes; we cannot

Table 7
Trade liberalization and government expenditures.

	1792–1969					1970–2006				Diff. <i>p</i> -values	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Entire Sample	All countries	HICs	MICs	LICs	All countries	HICs	MICs	LICs	Rich/dvp	Pre/post
% Episodes with no fall in expenditures	60.6 (49.1)	65.6 (48.3)	75.0 (50.0)	71.4 (46.3)	42.9 (53.5)	58.2 (49.7)	50.0 (70.7)	53.6 (50.8)	51.3 (49.2)	0.40	0.30
% Episodes with no expenditure recovery	19.19 (39.58)	3.13 (17.68)	0.00 (0.00)	4.76 (21.82)	0.00 (0.00)	26.87 (44.66)	0.00 (0.00)	35.71 (48.80)	21.62 (41.73)	0.19	0.01
Actual recovery time (years)	4 (8)	6 (11)	3 (3)	5 (12)	10 (10)	2 (3)	4 (4)	2 (4)	2 (3)	0.22	0.02
Potential recovery time (years)	25.5 (17.9)	41.9 (22.1)	30.8 (6.5)	41.1 (21.1)	50.7 (29.1)	17.6 (7.2)	14.5 (0.7)	14.2 (5.8)	20.3 (7.2)	0.27	0.00
% Episodes with recovery under 5 years	64.3 (48.2)	71.0 (46.1)	75.0 (50.0)	80.0 (41.0)	42.9 (53.5)	56.2 (49.1)	50.0 (70.7)	57.1 (50.4)	56.8 (48.4)	0.37	0.21
% Episodes with recovery under 10 years	73.6 (44.3)	77.4 (42.5)	100.0 (0.0)	80.0 (41.0)	57.1 (53.5)	68.7 (45.4)	100.0 (0.0)	65.2 (48.7)	64.9 (44.3)	0.19	0.27
% Episodes with recovery under 20 years	86.5 (34.5)	88.9 (32.0)	100.0 (0.0)	94.4 (23.6)	71.4 (54.8)	78.0 (37.4)	. (.)	92.8 (0.0)	80.0 (41.0)	.	0.38
Number of episodes	99	32	4	21	7	67	2	28	37		
Number of countries	85	25	4	18	7	63	2	27	34		

Notes: Mean (standard error). The table presents descriptive statistics on expenditure recovery for all episodes of decreases in trade tax revenues. HICs, MICs and LICs stand for, respectively, High-Income, Middle-Income and Low-Income countries. The last two columns present two-sided *p*-values for the difference between developing countries (LICs and MICs) in 1970–2006 and i) HICs in 1970–2006 (column 10) ii) developing countries in 1792–1969 (column 11). The number of observations refers to the number of observations in our data set for the period and country income group under consideration. The last three lines restrict the sample to episodes for which we have data for at least 5, 10 or 20 years after the start of the episode. Column 1 presents results for the entire sample of episodes. In columns 2 to 5 (respectively 6 to 9), we present results considering only the period 1792–1969 (respectively 1970–2006). HICs (columns 3 and 7), MICs (columns 4 and 8) and LICs (columns 5 and 9) are defined using the country income group at the start of the episode. Episodes are obtained on data smoothed using a HP filter with a smoothing parameter of 6.25. See the text for a description of the dataset and the method used to construct episodes.

determine which sources due to the lack of comprehensive data on non-tax revenues. The key patterns remain the same however: today's developing countries are more likely than both rich countries and developing countries in the pre-1970s period to experience an expenditure cost and less likely to recover in five, ten or twenty years, though the differences between country groups are less likely to be statistically significant. Trade liberalization episodes lead to a fall in government expenditures that is permanent in our data nearly 30% of the time in developing countries since 1970 whilst rich countries always recover the lost government expenditures.

7. Robustness checks

All the robustness check results in this sub-section can be found in the paper's online appendix unless specified otherwise. We first show that our results are not driven by the episodes that occurred during the Great Depression (see Section 4 above) in Appendix Tables 16, 26 and 31. The fiscal recovery characteristics of the average historical episode are extremely similar when we exclude these four episodes, indicating that these episodes did not have different fiscal consequences from other pre-1970 episodes despite happening after periods of increases in protectionism.

Our definition of trade liberalization episodes by a decrease in trade taxes and a non-decrease in trade volumes, whilst allowing us to exclude decreases in trade tax revenues due to the imposition of very high tariff rates or a decrease in trade, may lead us to exclude some cases of genuine decreases in tariff revenues that happen to be contemporaneous to decreases in trade volumes. One can imagine a situation in which a country decreases its tariffs but there is a fall in trade, for example during a recession.²⁸ We therefore reproduce Tables 2, 6 and 7 for the sample of 140 decreases in trade tax revenues of more than 1 GDP point in the online appendix (Tables 7, 17 and 27). We see that these episodes are slightly deeper than those using our main definition and they are slightly more likely to lead to a fiscal or expenditure cost, though the differences are small (36% of developing countries do not experience a fiscal recovery compared to 32% using our main definition). We also consider results excluding the few episodes for which there is no imports data (online Appendix Tables 9, 19 and 29), and consider episodes defined by a non decrease in trade (exports + imports as a share of GDP) instead of imports (online Appendix Tables 8, 18 and 28). Our main results are unaffected.

Results obtained using tax revenues (or expenditures) as a share of GDP may partially capture changes in GDP growth. This is a potential cause for concern here as trade liberalization may itself increase GDP (see e.g. Lee et al., 2004; Wacziarg and Welch, 2008). Estimating the impact of trade liberalization on economic growth is beyond the scope of this paper but we consider whether growth increases following an episode by computing the average growth rate before and after the start of episodes, using a number of different time spans. There is no evidence that our sample of episodes were accompanied by increases in GDP growth rates (online Appendix Table 5).

We consider an alternative definition of episodes using data on tax revenues per capita to further address this concern: we abstract from using GDP data altogether and say there is an episode when we observe a large fall in (smoothed) trade tax revenues per capita and look for fiscal recovery of the total tax revenues per capita variable.²⁹ The results are again very similar to those obtained using tax data normalized by GDP. The difference between the pre- and post-1970 periods is even stronger as all countries which experience an episode prior to 1970 are observed making a fiscal recovery at some point in the sample period (see online Appendix Tables 15 and 25).

The method we use to define episodes may not get rid of all noisy short-run variations in tax revenues – in which case some of our episodes are spurious – or may get rid of too much variation, leading us to exclude informative episodes. We consider episodes defined using a higher (2 GDP points) threshold for the fall in trade tax revenues and check for the robustness of the results to the choice of filter by considering episodes obtained using different smoothing parameters for the HP filter.³⁰ A known concern with the HP filter is its “end-point bias” (Baxter and King, 1999) as the last point of the series has an exaggerated impact on the trend. We use the Christiano-Fitzgerald band-pass filter to check that this bias is not driving some of our results (Christiano and Fitzgerald, 2003). Results are presented in the online Appendix Tables 11–14 and 21–24. We obtain more episodes (111) when using the Christiano-Fitzgerald method and less episodes when using higher values of the HP filter (90 and 94 episodes) or a higher threshold for the definition of the episodes (57 episodes), as expected. The main patterns found using our baseline definition of episodes are unaffected, and similarly unaffected if we only consider non pre-empted episodes – those for which we know that the fall in trade tax revenues was not compensated for “ex-ante” by an increase in tax revenues (Table 20).

Our findings therefore indicate that developing countries i) are more likely to experience a fiscal cost of trade liberalization, and ii) experience it for longer, than both rich countries today and rich countries when they were at similar stages of economic development. This suggests that the fall in tax ratios in these countries over the period 1970–2000 presented in Table 1 can partly be explained by the contemporary decrease in trade tax revenues also observed in the Table. Moreover, we show that the fall in tax revenues during trade liberalization episodes in many developing countries is hardly compensated for by increases in other types of government revenues: we observe similar patterns when we consider the evolution of government expenditures after episodes. In the next section we elaborate on the difference between today's developing countries and the historical experience of rich countries to discuss potential explanations for the fiscal cost of trade liberalization that we observe.

²⁸ Our use of post-1988 tariff data suggests this was the case in some countries as we find that average unweighted tariffs decrease during three of the four cases of simultaneous decreases in trade taxes and in trade for which tariff data are available, see Appendix Table 6 for more details.

²⁹ We choose a 50% threshold to obtain a number of episodes that is similar to the one obtained using our main definition.

³⁰ We consider values of 8.25, as in Ravn and Uhlig (2002) and 10, as in Hassler et al. (1992) and Baxter and King (1999).

8. A historical perspective on the tax transition

Why are some countries able to recover the tax revenues lost from liberalizing trade through domestic sources of taxation when others are not? To answer this question one must first understand why trade taxes are such an important tax handle for countries at an early stage of economic development - more than 30% of total tax revenues in early developers in the 1830s and in LICs in the 1970s. The consensus in the literature is that while the Diamond–Mirrlees (1971) production efficiency theorem implies that taxes on international trade are inferior to most forms of domestic taxation (for a review see Dixit, 1985), the former are easier to levy or more “revenue-efficient” to follow the terminology in Best et al. (2015). Optimal tax theory therefore predicts that countries will only tax trade if they cannot raise sufficient revenues through taxes on domestic transactions. This may be the case in countries at pre- or early industrializing stages of economic development if the characteristics of their economies are such that governments have to spend more on enforcement and monitoring activities to raise a given amount of revenues – we know for example that agricultural incomes are harder to observe (and therefore tax) than incomes in the manufacturing sector and that small firms are less likely to be tax compliant than large firms (Kleven et al., 2016). Relatedly, and following the concepts developed in Besley and Persson (2009, 2013), we can think that developing countries have less fiscal capacity and that less fiscal capacity is needed to levy trade taxes than broader-based domestic taxes: to levy tariffs governments only need to observe a few large transactions that are typically concentrated geographically.

These theories explain the tax transition observed in our data: as countries develop they decrease their revenues from taxes on trade and increase taxation from other sources. They are also consistent with historical evidence on rich countries which suggest that they gradually lowered tariffs once they had developed a fiscal administration which made it possible to raise tax revenues through other means (Ardant, 1971). A good example is the earliest episode in our sample, in 1842 in the United Kingdom – a low-income-country at the time. In 1842, over a third of the UK’s tax revenues came from export and import duties. Prime minister Robert Peel implemented a large over-the-board decrease in tariffs, and financed the budget overhaul by re-introducing the income tax and mobilizing the country’s modern tax bureaucracy built during the Napoleonic Wars – in other words by utilizing pre-existing fiscal capacity. The extra tax revenue raised was more than expected, allowing for further tariff reforms starting in 1846, the famous repeal of the Corn Laws (Bairoch, 1989). We observe immediate revenue recovery (no fiscal cost) for this episode.

9. Why is the fiscal cost of trade liberalization higher since 1970?

These theories cannot however explain why we often observe a fiscal cost of trade liberalization in developing countries since 1970. On the contrary they predict that tax ratios will *increase* when tariffs decrease: as countries shift their tax mix away from inefficient taxes on trade (because of structural economic change or of increases in fiscal capacity) the marginal cost of raising taxes falls, leading to an increase in (optimal) tax ratios. To explain the decrease in tax ratios that we observe one therefore has to assume that trade taxes decrease for exogenous – non fiscal – causes, and ask whether these potential causes were more likely to be relevant in developing countries since 1970 than in rich countries at earlier stages of development, explaining the patterns we see in the data. We consider several such potential causes in turn.

A first potential explanation is that governments were pressured to liberalize trade, regardless of the fiscal cost, by potential trading partners. Antràs and Padró i Miquel (2011) argue for example that powerful governments often succeed in changing the tariff policies of their economically smaller trade partners. Such external pressures indeed influenced the tariff policies of Asian and African economies in the 19th century: as explained above the trade policies of colonies were often determined by the needs of metropolises (Mitchener and Weidenmier, 2008), but external pressures to liberalize trade also played a key role in setting the tariffs in formally independent countries such as China and Japan. These pressures did not necessarily end with decolonization as newly-independent developing countries remained heavily dependent on trade with their ex-colonizers in the first decades post independence (see for example Head et al., 2010). This dependence made them particularly vulnerable to pressure from their main trading partner, which in many cases was also their main source of foreign aid. This hypothesis suggests the fiscal cost may have been particularly severe for episodes that are accompanied by trade agreements. To test this we consider whether episodes can be linked to countries entering trade agreements using information on the dates of entry of the different countries in our sample in regional and international trade agreements from the World Trade Organization’s Regional Trade Agreements Information System and historical sources. We are able to link 41 episodes to trade agreements (11 in the pre-1970 period, 30 in the later period); results presented in the online Appendix Table 33 show that these episodes are indeed characterized by a slightly higher fiscal cost in both the contemporary and historical periods, though the differences with our baseline sample of episodes are not statistically significant.³¹

Another source of external pressure to liberalize trade may have come from international organizations that often advocate trade openness. This seems consistent with the fact that many of our episodes occur during the 1980s and 1990s, a period during which international institutions often steered developing countries towards lowering tariffs through the conditions on their loans (Edwards, 1997; International Monetary Fund, 2001; Rodrik, 1992). A good example is the case of Argentina for which we find a trade liberalization episode starting in 1984 – the year in which the country, confronted with a mounting debt crisis, finally reached an agreement on financial assistance with the IMF. We assess the plausibility of this hypothesis in Table 8. Panel A compares countries that experienced a fiscal cost to those that experienced no fiscal cost, Panel B restricts the sample to episodes for which there was

³¹ This is only an imperfect test of this hypothesis as the pressure from trading partners to liberalize trade often occurs outside of formal trade agreements and fundamentally cannot be observed.

Table 8
Characteristics of episodes by existence of fiscal cost and revenue recovery.

Episodes in developing countries since 1970			Episodes pre 1970			
<i>A. All episodes, by existence of fiscal cost</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
	Fiscal cost	No fiscal cost	Diff. <i>p</i> -value	Fiscal cost	No fiscal cost	Diff. <i>p</i> -value
GDP per capita	2357.35	2528.96	0.37	4068.03	4947.05	0.24
Total tax revenues (% GDP)	18.43	16.30	0.10	13.82	16.15	0.17
Trade tax revenues (% GDP)	5.87	5.38	0.34	3.90	4.06	0.40
IMF program	0.57	0.56	0.48	0	0	.
Democracy index	−1.34	0.15	0.19	3.33	4.79	0.27
Number of episodes	35	30	65	12	20	32
<i>B. Episodes with a fiscal cost and 10 years of data, by recovery after 10 years</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
	No recovery	Recovery	Diff. <i>p</i> -value	No recovery	Recovery	Diff. <i>p</i> -value
GDP per capita	1993.15	1350.41	0.20	3048.21	9167.14	0.03
Total tax revenues (% GDP)	18.21	18.81	0.44	13.89	13.41	0.46
Trade tax revenues (% GDP)	6.50	5.62	0.35	4.07	3.06	0.22
IMF program	0.58	0.50	0.36	0	0	.
Democracy index	−1.26	1.33	0.20	2	10.00	0.03
Number of episodes	24	6	30	2	11	13

Notes: Each value in columns 1, 2, 4 and 5 is a mean over a sub-sample of episodes in our data, columns 3 and 6 present the *p*-value of the difference between the values in columns 1 and 2 (for column 3) and 4 and 5 (for column 6). The sample in the first panel is all episodes that occur in developing countries since 1970 (columns 1–3) and all episodes that occur pre 1970 (columns 4–6), columns 1 and 4 include all episodes for which there is a fiscal cost, columns 2 and 5 all episodes for which there is no fiscal cost. The sample in the second panel is restricted to episodes for which there is a fiscal cost and at least 10 years in the data after the start of the episode. Columns 1 and 3 include all such episodes for which there is no fiscal recovery after 10 years, columns 2 and 4 all such episodes for which there is recovery after 10 years. See the text for a description of the variables used.

a fiscal cost and compares countries that recovered under 10 years and those that did not. We use data on when countries received IMF loans from [Barro and Lee \(2005\)](#); these data are only available from 1975.³² We find that in the recent period countries that experienced no fiscal cost and countries that recovered under 10 years are slightly less likely to have been recipients of IMF loans, in line with the idea that these countries were pushed to lower tariffs “too early” by the loans’ conditions, but the differences are not statistically significant.

An alternative explanation is that what we observe is the consequence of an optimal policy change: governments in developing countries may have chosen to simultaneously open up to trade and lower their tax ratios. Indeed, one argument sometimes made at the time many developing countries entered structural stabilization plans was that governments intervened “too much” in these countries’ economies, including through taxation (see for example [Brune et al., 2004](#)). We find in [Table 8](#) that developing countries that experienced a fiscal cost since 1970 had (marginally) significantly higher tax revenues at the start of the episode but were not richer, unlike countries in which episodes occurred pre-1970, suggesting their revenues may indeed have been thought of as ‘too high’ for their level of development. There is no evidence however that countries with higher tax revenues are also less likely to recover, conditional on experiencing a fiscal cost.

Finally, differences in the political economic determinants of trade liberalization across historical periods could also explain the different levels of fiscal recovery. Governments set trade policy partly in response to the pressures applied by special interest (see [Grossman and Helpman, 1994](#), for a formal version of this argument), once in place protectionist policies create vested interests opposed to reform. Trade liberalization thus requires the emergence of particular political coalitions; these coalitions could be different from the ones needed to create political support for increasing domestic sources of taxation. One important political determinant of the timing of trade liberalization in the 19th and early 20th century - democratization - has also been found to be relevant in the post 1970s period.³³ There is substantial evidence that democracies were more likely to enter trade agreements in the 19th century ([Lampe, 2011](#)), though more so in labor-rich economies ([O’Rourke and Taylor, 2006](#)); similarly democratic transitions often led to free-trade agreements in developing countries since 1970 ([Milner and Kubota, 2005](#)). Democratization is also a key determinant of growth of governments revenues; [Aidt et al. \(2006\)](#) for example find evidence that democratization in Europe led to higher government spending in the 1830–1938 period. This suggests that trade liberalization episodes that were contemporary to democratization reforms could have led to a lower fiscal cost than those that took place in more autocratic countries. We test this idea in [Table 8](#) in which

³² We create an indicator equal to 1 if the country was the recipient of an IMF loan on the year the episode started. We exclude the 2 HICs with an episode since 1970 from this Table as they do not experience a fiscal cost and look very different from the rest of the sample on all dimensions.

³³ Because protectionism is typically more favorable to the capital-rich segments of the population, democratization, by shifting power away from these groups and towards the labor-rich working class, could in theory lead to more trade openness.

we use the democracy index from the Polity IV dataset, available from 1800.³⁴ The evidence does not contradict this hypothesis as countries that experience no fiscal cost or recover faster are indeed more democratic in both the contemporary and historical samples of episodes, but again the differences are typically not statistically significant.

Could differences in the types of democratic institutions in place in developing countries today relative to those adopted by early developers explain the differences in fiscal cost? Most trade liberalization episodes in 19th century Europe occurred in periods of limited franchise, on the contrary in democratic developing countries universal franchise has been the norm since the 1970s. It may be that in the 19th century limited government responsiveness to the working class segments of the population made increasing domestic taxes, and in particular non-progressive taxes on consumption (the main source of domestic tax revenues in early developers and in developing countries today), politically easier.

The literature on the political determinants of trade liberalization in developing countries also points out that, unlike early developers, developing countries typically abandoned protectionist regimes in periods of economic and often political crisis (see for example Rodrik, 1995). These crises enabled free-trade coalitions to emerge by relegating distribution consequences behind economy-wide concerns, or ending on-going ‘wars of attrition’ between competing interest groups (Drazen and Grilli, 1993; Rodrik, 1994). The case of Mexico in the run-up to the country joining GATT in 1986, well-documented in Tornell (1995) and in Tornell and Hernandez (1997), is a good example. Up to the late 1970s, capitalist elites had successfully blocked trade liberalization but the debt and oil crisis of the early 1980s left them with a choice between wide-ranging liberalization reforms to attract revenues from abroad, and expropriation – an option put forward by the competing statist elites. We find an episode in Mexico starting in 1981, around the time the coalition in power agreed to start lowering tariffs on manufactured goods. Similarly, in the case of Chile, the 1975 episode we discuss above followed the major political upheavals of the early 1970s; Velasco (1994) argues that amidst political turmoil business groups and trade unions were no longer able to oppose the government’s reform policies which included trade liberalization measures. This particularity of the developing country context could potentially explain the higher fiscal cost we observe if these crisis coalitions lacked the legitimacy to simultaneously under-go the wide-ranging fiscal reforms required to increase tax revenues.

10. Conclusion

This paper shows that trade liberalization sometimes comes at a fiscal cost. Using a new panel dataset of tax revenues covering 130 developed and developing countries from 1792 to 2006, we characterize 99 episodes of decrease in trade tax revenues and consider the contemporary evolution of total tax revenues to investigate the potential fiscal cost of trade liberalization. We show that in the period since 1970 developing countries are more likely than rich countries to experience a fall in total tax revenues as they decrease trade taxes and less likely to recover the lost tax revenues through other sources of taxation. They are also more likely to experience a contemporaneous fall in total government expenditures. We observe similar episodes of decreases in trade tax revenues in today’s rich countries when they were at earlier level of development in the 19th and early 20th centuries but find that they were less likely to experience a simultaneous decrease in total tax revenues than today’s developing countries, and that when they did this decrease was smaller and shorter-lived. Trade liberalization, defined here by a decrease in trade tax revenues, seems to have come at a larger fiscal cost in today’s developing countries; this may be because they decreased taxes on trade before having developed tax administrations capable of taxing domestic transactions on a large scale. Our results imply that equilibrium trade models that consider the impact of trade policy independently from tax policy may be over-estimating the welfare impact of trade liberalization (see for example Alvarez and Lucas, 2007); taking into account the fiscal consequences of trade policy in general equilibrium theories of trade is a potential fruitful avenue for further research.

The fiscal cost of opening up to trade experienced by developing countries could be eroding support for further trade liberalization. Trade taxes still represent nearly one-fourth of total tax revenues in 2000–2006 in low-income countries. These are precisely the countries for which the international community calls for increases in domestic revenue mobilization (Gupta and Tareq, 2008; OECD, 2010). Our findings suggest that increasing these countries’ capacity to tax could weaken one of the reasons they are reluctant to embrace free trade by making governments less dependent on taxes on trade for public revenues.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at [10.1016/j.eeh.2018.07.004](https://doi.org/10.1016/j.eeh.2018.07.004).

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³⁴ This index takes values going from –10 to +10, with higher values indicating more democratic institutions.

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