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Tariffs and economic growth in the first era of globalization

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Abstract Are liberal trade policies good for growth? Sceptics often point to the late nineteenth century as a period when protectionist policies promoted economic development. This apparent blueprint for the benefits of protectionism paradoxically comes from a period that is often described as the first era of globalization. In this paper we reassess the empirical evidence about the relationship between tariffs and growth between 1870 and 1914. Our key findings challenge the idea that in the nineteenth century countries that raised tariffs thereby increased their own growth rate. Using new and improved data and employing a whole portfolio of econometric tests we do not find evidence that increased protectionism raised the rate of individual countries' growth. While some positive cross-sectional correlation exists between tariffs and growth, this may reflect unobserved country traits rather than a causal relation. There is equally little evidence that other external factors, such as real exchange rates and terms of trade changes, were key drivers of economic performance. A paradox of this first era of globalization is not that free trade was bad for growth; it is that international economic policies seem to have mattered little to countries' growth trajectories.

Keywords Economic growth · International trade · Economic history · Growth econometrics · Globalization

JEL Classification F10 · F13 · N10 · O11

The empirical analysis of the relationship between trade openness and economic growth has generated mixed results. Disagreement persists among economists on how a country's international economic policies affect its rate of economic growth. Early empirical studies generally supported the idea that openness is positively related to economic growth (Dollar 1992; Ben-David 1993; Sachs and Warner 1995; Edwards 1998; Frankel and Romer 1999; Wacziarg 2001; Wacziarg and Welch 2008). Yet other contributions have elicited doubts as

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to whether these results reflect causal influences of trade on growth (Rodríguez and Rodrik 2001; Acemoglu et al. 2001; Yanikkaya 2003). In particular, the study by Rodríguez and Rodrik (2001) has been very influential as it demonstrated the empirical fragility of the widely held belief that trade openness was closely associated with better growth performance. In this paper, we aim at a similarly iconoclastic challenge to conventional wisdom. We assess the widely held belief that protectionism had positive effects on economic development in the first era of globalization: does history really provide a blueprint for successful protectionism in early phases of industrialization?

For scholars who believe in the growth benefits of free trade, some of the most disturbing empirical evidence comes indeed from the literature on the economic history of the late nineteenth century—curiously from a period that is often seen as the first period when global market integration and rapid falls in transaction costs led to booms in trade and capital flows (Sachs and Warner 1995; Schularick 2006; Jacks et al. 2010). Studies of this period have emphasized the existence of a "tariff-growth paradox", describing how protectionist tariff policy was associated with higher rates of economic growth before 1914 (Bairoch 1989; O'Rourke 2000; Clemens and Williamson 2004; Jacks 2006), in contrast to the negative relationship observed in many studies of the post-WWII era.

In this paper we reassess the empirical evidence on the relationship between tariffs and growth during the period 1870–1914. Our key findings challenge the empirical validity of the nineteenth century tariff-growth paradox. We can reproduce earlier findings, but changes in the specification and improved data lead to different conclusions. A central observation that we highlight is that the co-movement of average growth rates and average tariff during the severe 1875–1879 depression is a key factor behind the positive correlation between tariffs and growth found in previous studies. Countries entered the depression in the late 1870s with relatively low tariff rates following the move to free trade after the Cobden-Chevalier treaty. During the 1870s depression, protectionism made a comeback and many countries raised tariffs. When the cyclical recovery came in the 1880s, average tariff levels were higher. Time-effects are needed to control for such common movements in the sample average. Countries recovered irrespective of their tariff policies. Higher protection did not lead to faster growth.

This general result does not rule out that there could have been winners and losers from trade policy. Nor does it rule out that specific forms of tariff policy could have had some positive growth effects as recent studies have suggested (Lehmann and O'Rourke 2008). We also don't find strong evidence to suggest that high tariffs were harmful to growth. But our critical assessment of the relationship between tariff policy and economic growth stands in contrast to much of the previous literature in that we find no robust evidence for a tariff-growth paradox.

Our restatement of the tariff-growth hypothesis in the late nineteenth century is based on three major contributions. First, we improve on the quality of the historical data used in the panel regressions (a detailed Data Appendix discusses the data used). Second, we specify a neoclassical growth model, allowing for a more comprehensive picture than in the previous literature, in which various proxies were used for the capital stock and potentially influential variables, such as population growth, were omitted from the analysis. Third, this paper addresses a number of econometric issues that make the robustness of previous results uncertain. These issues relate to controls for common trends in the panel, the dynamic nature of the panel model, and potential endogeneity of regressors. We use, *inter alia*, generalized method of moments (GMM) estimation (Bond et al. 2001), and attempt to gain a fuller picture of short-run adjustments as well as potential parameter heterogeneity by employing the pooled mean group estimation method (Pesaran et al. 1999). While the validity and strength of internal instruments in the context of GMM estimation have recently come under increased scrutiny, we think these estimations are still useful as part of a whole portfolio of statistical tests.

The result across these various statistical approaches is that changes in tariffs were not closely linked to changes in individual countries' growth performance. Including time effects to account for variation in growth rates that are common to all countries and using an improved set of control variables, the relationship between tariffs and growth turns insignificant. The pooled-mean group estimations using annual data confirm the absence of a statistically meaningful relationship between average tariff levels and economic growth. Yet it should also be stressed that there is no compelling evidence for a statistically significant *negative* tariff-growth relationship in this era—although some of our results imply that higher tariffs actually resulted in lower growth. All this suggests that the openness-growth relationship is complex, time-varying and may display significant heterogeneity, which is of relevance to recent textbooks on economic growth that have posited a positive openness-growth relationship as a stylised fact of modern economic growth (Jones 2001).

Furthermore, controlling for other external factors in the growth process—such as real effective exchange rates and terms of trade movements—does not impact the tariff-growth relationship, nor does it reveal strong linkages between these factors and economic growth at the time. If external factors such as tariffs, price competitiveness, or terms of trade movements played an important role in the late nineteenth century growth process, the effects are not easily identifiable—at least in standard empirical growth models focusing on changes in growth within countries over time, i.e., using fixed effects. It is possible that such links will become visible through more complex models or interactions with other variables.[1] Trade integration and globalization might also have shaped comparative economic performance through other channels than trade policy (Galor and Mountford 2008). But a qualified description of international factors as drivers of economic development in the first era of globalization in fact seems warranted. We find that history does not provide a blueprint for the successful use of tariff policy to accelerate economic development. There is little in the data to suggest that countries increased their growth paths by raising tariffs. A positive relationship between protectionist changes of tariff policy and higher rates of economic growth is not a feature of economic growth during the late nineteenth and early twentieth centuries.

1 Introducing a new dataset

Starting from Bairoch's (1989) observation that the tariff hikes of the 1870s had positive growth effects for the countries that applied this policy, a number of studies have linked growth performance to protectionist tariff policy (O'Rourke 2000; Vamvakidis 2002; Clemens and Williamson 2004). Irwin (2002) has challenged the causal interpretation of the tariff-growth link, but in general the empirical fact of a significant positive correlation between tariffs and growth seems widely accepted in the literature. In this study, we will examine the robustness of this relationship in the face of three modifications, which we consider necessary.

First, while relying on the pioneering datasets assembled for previous studies, we have improved the underlying tariff data in a number of ways. Most importantly, we have integrated new tariff series for the USA, Australia, India and the Netherlands. We have also

[1] Capital market integration and migration might have played a role; see Schularick and Steger (2010) and Taylor and Williamson (1997). Madsen (2009) has found some evidence that openness may impact on economic growth positively via the learning effects arising from openness and foreign knowledge transfer.

collected import price series which will allow us to deal with the (well-recognised) problem that tariff rates have been mostly measured by time series for revenue tariffs, i.e. yearly tariff revenues were divided by the total value of imports. But as many tariffs during this period were set in 'specific' terms, the measured tariff rates will be strongly affected by price movements (O'Rourke 2000). Inflationary periods would show up as tariff rate reductions in the data while there was no active change in trade policy. The impact of price fluctuations on the tariff measure has been addressed by using the GDP deflator to make price adjustments (O'Rourke 2000). However, since tariffs are import weighted we have opted to use import price deflators, which is likely to lead to more reliable adjustments (see Data Appendix for data sources). To distinguish the policy component from the price component in tariff movements we constructed an adjusted tariff variable by regressing revenue tariffs on import price trends and used the residual as an adjusted tariff measure.[2]

Second, we have constructed a panel dataset consisting of control variables that allow us to estimate a standard neoclassical growth model, in line with contemporary studies on the determinants of economic growth (Mankiw et al. 1992; Frankel and Romer 1999; Rodríguez and Rodrik 2001). We did so in a deliberate attempt to make our findings comparable to the recent openness-growth literature, and improve over more pragmatic empirical models that were used in previous historical studies. For example, previous studies worked with rough proxies for the investment ratio—such as coal consumption per capita.[3] For this study, we have assembled investment data for a panel of 20 countries from the available historical national accounts data. Our dataset covers: Argentina, Australia, Brazil, Canada, Chile, Denmark, France, Germany, Japan, Italy, India, Mexico, Netherlands, Norway, Russia, Spain, Sweden, Switzerland, United Kingdom, USA. A summary table and further details on our sources can be found in the Data Appendix.

The main constraint with regard to the number of countries was the availability of reliable macroeconomic time series for GDP and other control variables at the annual frequency. Given the reliability of current historical data, considering a selection of 20 countries represents the maximum number to our best knowledge. Clemens and Williamson (2004) were able to work with a data set covering up to 35 countries; however, this broad sample also comes at a cost. In some cases it entailed linear interpolation over missing periods—which could be particularly problematic in regressions focusing on the within-country dimension—and the use of data of widely differing quality. Taking a closer look at our sample compared to the 35 country sample in Clemens and Williamson (2004), it is obvious that the main difference lies in our coverage of less developed, peripheral economies. The key difference is that we can only include 4 out of the 14 less developed economies from the Clemens and Williamson sample. In Asia, we are missing data for Burma, Ceylon/Sri Lanka, China, Indonesia, Philippines, Siam/Thailand; in Latin America, we have to omit Colombia, Cuba, Peru and Uruguay. Both samples exclude the majority of African (mostly colonial) economies.

We attempted to extend our selection, but encountered data availability and reliability problems. Reliability issues do not only exist with respect to various right-hand-side control variables, but also with regard to the dependent variable itself, growth of real GDP per capita. For many peripheral countries GDP estimates actually begin very late in the period studied here—for example in 1905 for Colombia and 1902 for the Philippines. In the case of China, all that exists are benchmark estimates for GDP in a handful of years, the rest is

[2] We use both the unadjusted and the adjusted tariff measure in our regressions. This allows us to evaluate separately the policy component from the price-induced changes in tariff movements.

[3] Coal consumption per capita is a very poor proxy of the investment ratio in this period. In cases where both data sets exist, the correlation is insignificant, with coal consumption per capita failing to capture both the trends and cyclical movements of the investment ratio.

interpolation. If GDP data are typically based on rough estimates for agricultural output in a few benchmark years, it is not surprising that most other right-hand side variables such as the investment ratio or measures of human capital are simply not available for a bigger sample. Even in cases where some annual data exists, there is doubt as to whether the data is reliable at the annual frequency—as in the case of Indonesia. In terms of share of world GDP, only China and to a lesser extent Indonesia are likely to influence the coverage meaningfully. Using the Maddison data, the combined share of these two countries in world GDP was about 10% in 1913. Our 20-country selection represents about 65% of world GDP in this period based on Maddison's data for 1913, compared to about 78% for the 35-country Clemens and Williamson sample.

This then implies a trade-off between a potential sample selection bias in favour of rich countries (for which more reliable data are available) and a potential measurement error bias when working with data series of doubtful reliability and based on interpolation. We ran a sample selection regression (probit) with inclusion in our sample being the binary dependent variable, and GDP per capita levels and tariff levels the explanatory variables. Income level emerges as the key variable for sample inclusion with tariff rates playing no role. While the inferences we draw need to be qualified in the light of this, we ran two checks that yielded rather reassuring insights. First, we weighted observations with the inverse of the probability of being part of the sample, using GDP per capita as the weighting variable. Second, we restricted the regressions to less developed countries in our sample to check whether poorer countries exhibited meaningfully different dynamics. Both regressions are reported in the Appendix. The results were very similar to the regressions reported later on as our benchmark estimations.

Measurement errors are also a potential problem when working with the historical data for our 20-country selection. This also applies to the dependent variable, growth in real GDP per capita. To date most economists have relied on the seminal work of Angus Maddison for historical GDP data. Barro and Ursua (2008) have noted a number of problems with Maddison's database and compiled a new database integrating recent revisions to the national accounts of a number of countries. In this study we use the Barro and Ursua (2008) database together with some national data revisions that have been omitted by them.[4]

We have also added a number of additional control variables that the historical literature considers important for understanding the link between tariffs and economic growth. For instance, a substantial body of literature suggests that other international economic policy variables need to be considered. Haber (2005) argues that tariffs and real exchange rates need to be considered together to explain the late nineteenth century performance of Latin American countries; policy makers were aware of the links between the two policies and reacted appropriately—periods of exchange rate appreciation resulted in higher tariff rates. Eichengreen and Irwin (2009) have also noted a strong relationship between the exchange rate and tariff movements for the interwar period—countries that became most overvalued in the 1930s raised tariff rates the most. The real exchange rate has also been identified as an important variable determining comparative growth performance in the period 1873–1895. Nugent (1973) found that the depreciation of the nominal and real exchange rate of the silver countries (India, Mexico and Japan) gave these countries an identifiable positive growth stimulus, relative to the gold standard countries. Such evidence could suggest that the real exchange rate may be a significant omitted variable in growth equations for the period that focus only on tariffs as a policy variable.

[4] In fact, we also estimated all the models using Maddison's GDP series and obtained similar results.

To deal with this we have extended the Catao and Solomou (2005) data on real effective exchange rates (covering 16 countries) to include Denmark, Norway, Sweden and Switzerland. Another external variable that could be important as an omitted variable is change in the terms of trade. The literature linking terms of trade to comparative development in the nineteenth century is substantial and well-known (Prebisch 1950; Hadass and Williamson 2003).[5] Finally, we believe that a third area—that of econometric modelling—also necessitates modifications to previous studies. We will discuss this in greater detail in the following section.

2 Estimating the growth effects of tariff policy

The econometric approaches taken to estimate the tariff-growth paradox have differed substantially across studies, and particular caution seems warranted in specifying an appropriate model to identify the growth effects of tariff policy. Most previous estimations included country-specific effects. This is uncontroversial as country dummies are needed to capture cross-sectional heterogeneity through different intercepts (Durlauf et al. 2005, p. 4; Baltagi 2006, p. 363). Only if country dummies are included, will omitted variables that are constant over time not bias the estimates, even if they are correlated with the explanatory variables. Given the heterogeneity of our sample and large differences in time-invariant growth drivers between countries—such as institutional quality, climate and geography—the inclusion of country dummies would seem essential to identifying the growth impact of trade policy changes. Theory also leads us to expect temporary growth effects from free trade, not permanent effects (Rodríguez and Rodrik 2001). We are thus interested in changes relative to country means from fixed effect regression, not in the level effects apparent in long-run cross-sections.

However, not all previous studies have included period-specific intercepts.[6] This can be problematic because time effects would seem necessary to capture growth changes that are common to all countries in a specific sub-period.[7] Their inclusion has essentially the same effect as would transforming the variables into deviations from period means, which is particularly important for the estimation of convergence models as the mean of output increases over time due to productivity growth (Bond et al. 2001, p.15). Our strong prior therefore is that time effects should be included in the regressions.

Also the dynamic structure of the panel model has not always been addressed explicitly.[8] Studies on the tariff-growth relationship have been based on the hypothesis that countries display conditional convergence. An implication of such models is that current period growth contains some dynamics in lagged output. The problem then is that the fixed-effect models used in the literature generate biased estimates when the time dimension of the panel is small. Correlation between the lagged dependent variable and the disturbances plagues the analysis (Judson and Owen 1999). A related problem is the potential endogeneity of the tariff variable.

[5] Jeffrey Williamson generously shared the data from his pioneering quantitative work (Hadass and Williamson 2003; Williamson 2006, 2008).

[6] An exception here is Jacks (2006). O'Rourke (2000) noted that time effects affected his results but did not allow this to affect his interpretation of the Tariff-Growth relationship.

[7] Such time dummies may also capture period-specific components of measurement errors.

[8] An exception here is the work of Clemens and Williamson (2004), in which they employed the instrumental variable estimator developed by Andersen-Hsiao. Judson and Owen (1999) have compared different estimators in the presence of small T dynamic fixed effects panels. The efficiency of the Anderson-Hsiao indicator increases when the number of time periods approaches 20, but GMM should be the estimator of choice if the number of time observations is only about 10.

It is entirely possible that trade policy itself could be a function of economic growth as well as impacting on growth.[9] Both issues call for caution in the interpretation of previous results showing a positive growth impact of higher tariffs.

Corroborating the OLS fixed effects panel estimation within a GMM approach allows us to test the tariff-growth hypothesis while accounting for the dynamic nature of the model and potential endogeneity of some regressors. Arellano and Bond (1991) have shown that in generalized methods of moment (GMM) estimation lagged values of endogenous variables can serve as instruments for first-difference equations. However, such a first-differenced GMM estimator can have poor properties in short dynamic panels if the lagged levels of the variable are weak instruments for the first differences (Blundell and Bond 1998). The system GMM estimator, introduced by Arellano and Bover (1995) and Blundell and Bond (1998), can be superior to difference GMM in such situations as it combines the standard set of equations in first differences with suitably lagged levels as instruments with an additional set of equations in levels with suitable lagged first differences as instruments (Bond et al. 2001).[10] More recent studies have questioned the validity and strength of the internal instruments used in system GMM. Weak instruments could be as problematic as invalid instruments for testing hypotheses about growth and its causes. Especially, if the levels are weak instruments for differences, and differences are weak instruments for levels, the identification ability of GMM estimation would decrease meaningfully (Bazzi and Clemens 2009). Bun and Windmeijer (2010) show that if the variance of country effects is high relative to the variance of transitory shocks, a weak instrument problem can arise for the system GMM estimator. We will come back to these issues in the discussion below.

The starting point for our panel estimations is the following growth regression:

$$y_{i,t} - y_{i,t-1} = (\alpha - 1)y_{i,t-1} + \beta T_{i,t} + \boldsymbol{\gamma}'\mathbf{X}_{i,t} + \eta_i + \tau_t + \varepsilon_{i,t}, \tag{1}$$

Minor reformulation of Eq. 1 leads to a dynamic panel regression model of first order:

$$y_{i,t} = \alpha y_{i,t-1} + \beta T_{i,t} + \boldsymbol{\gamma}'\mathbf{X}_{i,t} + \eta_i + \tau_t + \varepsilon_{i,t}. \tag{2}$$

where $y_{i,t}$ is the logarithm of per capita income, $T_{i,t}$ is the logarithm of (1+ tariff rate) at the beginning of the period, η_i is a (time-invariant) country-specific effect, and ε_i represents an i.i.d. stochastic term. Subscript i indicates countries and subscript t the time periods under consideration. As noted above, we also include strictly exogenous time-dummies τ_t. In the conditional convergence model, $\mathbf{X}_{i,t}$ represents a vector of economic control variables: for the neoclassical growth model these include the logarithm of the investment ratio, the logarithm of the primary school enrolment rate, the logarithm of the growth rate of the population (plus a constant for depreciation and exogenous technological progress) as well as land endowment. In alternative specifications, reported below, we also consider other variables that have been discussed in connection with the nineteenth century growth experience.

It is standard in panel studies to use 5-year observation periods on the assumption that the averaging over 5-year periods will eliminate business cycle effects. However, this could prove a problem in studies of the pre-1914 period if business cycles were longer as studies

9 For example, the historical analysis of trade policy suggests that the transition to a period of slow growth following the depression of the early 1870s resulted in a defining moment in trade policy leading to a shift towards protection.

10 We use the Stata "xtabond2" routine implemented by Roodman (2005) with the one-step robust estimator. Following Blundell and Bond (1998) we opted for the one-step estimators as the two-step standard errors can exhibit a severe downward bias in finite samples so that inference becomes difficult. The one-step GMM estimator on the other hand produces standard errors that are robust to heteroskedasticity and more reliable for finite sample inference (Blundell and Bond 1998; Bond et al. 2001, p. 16).

of the Juglar cycle suggest. Alert to the potential problems of data averaging, we use non-overlapping 5-year periods (1870–1874, 1875–1879…). But we also run identical regressions across different 5-year sub-periods (1872–1877, 1878–1883…), and longer 10-year intervals (1870–1879, 1880–1889…) as robustness checks.

In a second specification we deal with this problem by employing an estimator that allows us to exploit the annual data in our dataset and avoid the information loss induced by averaging over periods. We have three key motivations. First, averaging always involves a loss of potentially important information while it is not entirely clear that the procedure effectively cleans the data of business cycle fluctuations. Second, averaging also eliminates the possibility of identifying different dynamic relationships between tariffs and growth in the short- and long run. Finally, the models discussed above impose homogeneity of all slope coefficients, allowing only the intercepts to vary across countries. Pesaran and Smith (1995) have argued that estimates could suffer from heterogeneity bias in a relatively small sample if the assumptions of a common growth rate of technology and a common convergence parameter are not fulfilled (see Lee et al. 1997).

We use three different estimators that allow us to exploit the annual frequency of the data: the Dynamic Fixed Effects Estimator (DFE), the Mean Group (MG) and finally the Pooled Mean Group (PMG) estimator introduced by Pesaran et al. (1999). The latter is an intermediate choice between imposing homogeneity on all slope coefficients (DFE) and imposing no restrictions (MG). The PMG estimator allows intercepts, the convergence parameter, short-run coefficients and error variances to differ freely across countries, but imposes homogeneity on long-run coefficients. The PMG method is a panel error-correction model, where short- and long-run effects are estimated jointly from a general autoregressive distributed-lag (1, 1, 1) model and where short-run effects are allowed to vary across countries.

We estimated the following restricted version of the growth equation on annual data using mean group, pooled mean group and dynamic fixed effect estimation (in the DFE model the speed of adjustment coefficient and the short-run coefficients are restricted to be equal and panel-specific intercepts are allowed for):

$$\begin{aligned}\Delta \ln y_{i,t} = & -\phi_i(\ln y_{i,t-1} - \theta_1 \ln I_{i,t} - \theta_2 \ln H_{i,t} + \theta_3 P_{i,t} - \theta_4 T_{i,t} - \theta_5 L_{i,t} - \alpha_{6,t} t \\ & -\theta_{0,i}) + \beta_{1,i}\Delta \ln I_{i,t} + \beta_{2,i}\Delta \ln H_{i,t} + \beta_{3,i}\Delta^2 \ln P_{i,t} + \beta_{4,i}\Delta \ln T_{i,t} \\ & +\beta_{5,i}\Delta \ln L_{i,t} + \varepsilon_{i,t}\end{aligned} \tag{3}$$

where $\mathrm{y_{i,t}}$ is the natural logarithm of per capita income, $\mathrm{I_{i,t}}$ the investment ratio, $\mathrm{H_{i,t}}$ a proxy for the human capital stock, $\mathrm{P_{i,t}}$ the log of rate of population growth (plus deprecation and a common growth rate of technology), $\mathrm{L_{i,t}}$ the land endowment, and $\mathrm{T_{i,t}}$ the logarithm of (1+tariff rate), and *t* a time trend; the coefficient on lagged income $\mathrm{y_{i,t-1}}$ can be interpreted as a convergence parameter.

By using such a wide range of methods to evaluate the tariff-growth relationship, we are in a good position to test the robustness of any results that arise from different estimation methods. Given the nature of the issue being addressed, the quality of the historical data, and the fact that different data transformations focus on different aspects of the data, we see a portfolio of checks as essential to making robust inferences.

3 Reproducing the tariff-growth paradox

Our empirical strategy proceeds in three steps. We shall first aim to reproduce the results of previous studies before moving on to our preferred model and then test the robustness of our results by working with annual data and adding additional control variables. In Table 1 we

Table 1 Reproduction of previous results

Growth of real GDP	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Our sample	Our sample	Our sample	Clemens/ Williamson	Clemens/ Williamson	Clemens/ Williamson
Time period	5 y	5 y	5 y	5 y	5 y	5 y
Country effects	No	Yes	Yes	No	Yes	Yes
Time effects	No	No	Yes	No	No	Yes
log (1 + tariff rate)	0.134**	0.268*	0.0331	0.0742**	0.243***	0.0702
	(0.0563)	(0.146)	(0.144)	(0.0370)	(0.0900)	(0.0951)
95% confidence interval	0.022–0.245	−0.0200–0.555	−0.251–0.317	0.001–0.147	−0.0711–0.350	−0.117–0.257
log (real GDP per capita)	0.0139	−0.00358	−0.179***	0.0170***	−0.0319*	−0.136***
	(0.00849)	(0.0250)	(0.0436)	(0.00505)	(0.0168)	(0.0282)
Constant	−0.0729	0.0830	1.429***	−0.0916**	0.278**	1.088***
	(0.0662)	(0.202)	(0.338)	(0.0363)	(0.130)	(0.218)
Observations	177	177	177	327	327	327
R-squared	0.040	0.158	0.332	0.050	0.228	0.322
r2_a	0.029	0.043	0.194	0.044	0.132	0.214
Test for country effects = 0	–	1.142	2.397	–	1.959	2.755
Prob.	–	0.315	0.002	–	0.002	0.000
Test for time effects = 0	–	–	4.222	–	–	4.357
Prob.	–	–	0.000	–	–	0.000

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

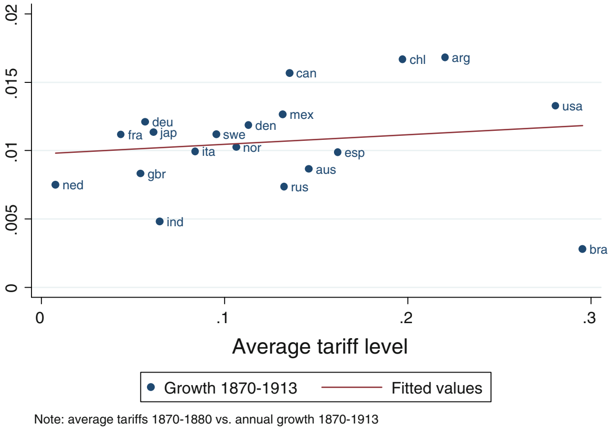

Fig. 1 Tariff levels and annual growth 1870–1913

start by reproducing the results of previous studies of the tariff-growth paradox.[11] In regressions (1–3) we employ our new data for a sample of 20 countries, in regressions (4)–(6) we use the data from the Clemens and Williamson (2004) study for their sample of 35 countries. Both for our sample and the Clemens and Williamson sample we start with a simple pooled model and then subsequently add country effects and time effects. We first look to a simple unconditional convergence model, i.e. initially we control only for income levels and then add additional control variables.

Some interesting insights emerge from Table 1. First, we can reproduce the tariff growth paradox with our sample and our data in regression (1). The tariff-growth paradox is also robust to the inclusion of country effects (2). Yet it is weakened considerably in regression (3) when both country and time effects are included. Statistical significance is strongly reduced, but the coefficient estimate remains positive. Second, this result is not driven by sample selection. We get the identical result—a positive tariff-growth relationship that fades when time effects are included—for the Clemens and Williamson dataset or using GDP per capita weights. Finally, our new data also seem to play some role as some coefficients are meaningfully different.

The results presented in Table 1 reveal a number of interesting dynamics. First, both the pooled OLS model and the fixed-effects model display a significantly positive effect of tariffs on growth. The tariff-growth paradox therefore does not seem to be purely driven by the cross-sectional country dimension. The between-groups estimator shows a positive (albeit insignificant) relationship between tariffs and growth. In Fig. 1 we show the correlation between average tariffs and growth over the 1870–1913 period. In both samples the cross-section shows a positive correlation between average tariffs and average growth rates.

However, Irwin (2002) has demonstrated the fragility of such cross-sectional correlations. A key reason is shown in Fig. 2. Big countries (in terms of land area) tended to have higher tariffs. In particular, the fast growing settler economies in the New World tended to have higher tariffs as they were the most reliable way to raise public revenues in such countries. Yet we can obviously say very little about causality. Large economies could have grown faster than the rest for many reasons other than tariffs—including a greater potential to capture gains

[11] For comparability with Clemens and Williamson (2004) for this part of the estimation we use the sample period 1865–1913.

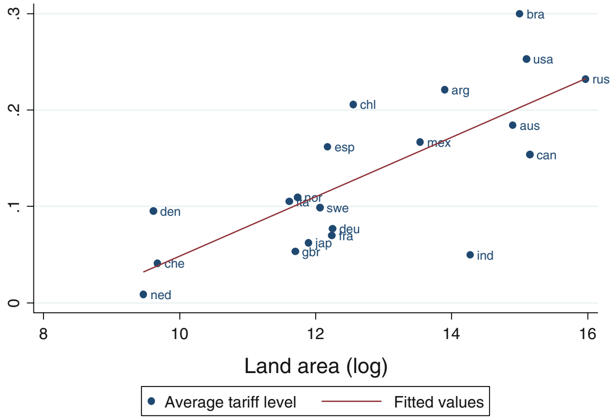

Fig. 2 Tariff level and land area

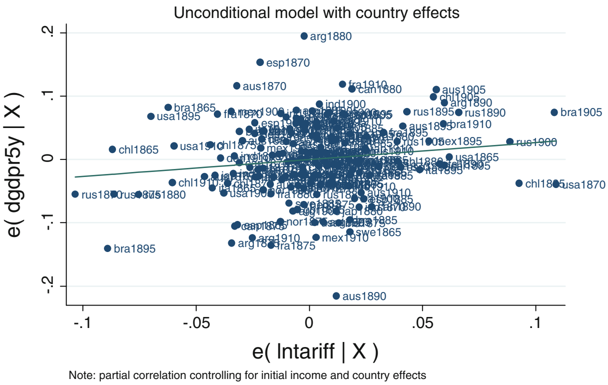

Fig. 3 Partial correlation between tariffs and growth

from internal trade, different institutional endowments, or the effects of immigration. Such heterogeneity cannot be captured in a cross-section with a low number of observations.

However, the tariff-growth paradox remains robust to the inclusion of country effects as shown in regressions (2) and (5). We can shed some light on the role played by country effects by visually inspecting the partial correlation plots between tariffs and growth. Figure 3 shows the partial correlation between tariffs and growth after controlling for initial income and country specific effects, but leaving out time effects. The message seems clear: as countries' tariffs moved up relative to their (country-specific) means, growth rates increased. Both in the cross-section and in the time dimension there is some evidence supporting the tariff growth paradox. However, we do not yet control for period-specific effects. As it turns out, these matter.

Figure 4 finally displays the partial correlation between tariffs and growth from an unconditional model that includes period-specific intercepts. As is clearly visible, the relationship between tariffs and growth is much weakened after the inclusion of time effects. What does this mean? It demonstrates that the positive correlation between tariffs and growth that was apparent in Fig. 3 was in fact only derived from nine observations for the time-means in the panel, not from the full set of individual country-time observations. The period-specific

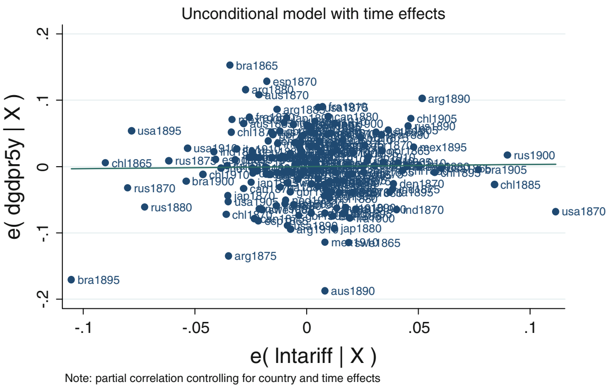

Fig. 4 Partial correlation between tariffs and growth

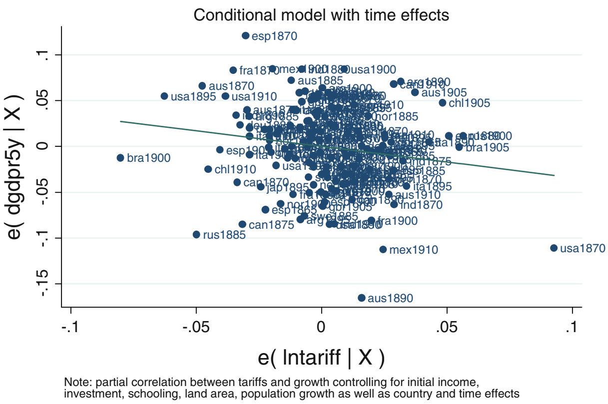

Fig. 5 Partial correlation between tariffs and growth

intercepts capture this variation in growth rates that is common to all countries. By including time effects, the variables at each t are transformed into deviations from the mean of the variable across all panels at time t. Such a procedure purges the data of time trends in the variables and avoids falsely attributing variation of the endogenous variable to common shocks hitting all countries at the time. Are time-effects needed to control for cross-panel shocks? In light of the different correlation patterns apparent from Figs. 3 and 4, it comes as no surprise that a Wald-test for the significance of the time effects strongly suggests that their inclusion is essential. The null hypothesis that the time effects are not jointly significant is rejected with a p-value smaller than 1%—as can be seen in the lower half of Table 1 Fig. (5).

4 Tariffs and the "Long Depression"

The econometric case for including time effects in the models has been made above. An intuitive explanation for the observed phenomena is the following. Why do time-effects have such an impact on the tariff-growth relationship? There could be two reasons: firstly, the sample average of tariffs and the sample average of growth rates could share an underlying common movement over time, i.e. as average tariffs go up, world growth goes up. Such cross-country shifts in tariff rates are sometimes referred to as changes in the "world tariff environment". Clearly, such a finding that shifts to more or less protectionism on a global level were positively correlated with average global growth rates would be an interesting result per se (albeit little could be said about causality on an individual country level). Secondly, it is also possible that no such general relationship between average growth rates and average tariff rates exists in the data, but that one or two particular observations drive the result. Remember that there are only about eight observations for the 5-year sample means of growth and tariffs so that a single observation could shift the regression of average tariffs on average growth line quite strongly. What is driving this particular result here? We look at this issue in the Appendix and show that the overall correlation between average tariff rates and average growth rates is driven by one influential observation that coincides with the depression period of the late 1870s. The co-movement of average growth rates and average tariffs during and after the severe 1875–1879 depression plays an important role.

Countries entered the 1870s depression with relatively low tariff rates following the move to free trade after the Cobden-Chevalier treaty. During the 1870s depression, protectionism made a comeback and many countries raised tariffs. The "Long Depression" of the 1870s was a period of synchronised recession and deflation across many countries.[12] The depression represents a low point in economic growth across countries and happened at a time of relatively low tariff rates—without there being any obvious causal relationship between the two. From the late 1870s tariffs were raised, often in response to the economic downturn, so that in the 1880s, when most economies recovered from the depression of the 1870s, tariff levels were considerably higher. The result is a common step movement in the levels of both variables. The positive correlation between tariffs and growth—the so-called tariff-growth paradox—relied strongly on this co-movement of sample average tariffs and sample average growth in and after the 1870s depression.

However, such a common movement suggests that cyclical, rather than a structural effects were at play. Low growth in the cyclical depression of the late 1870s—the result of the common effects of financial crises, sudden stops of capital flow affecting some of the periphery and adverse weather conditions in much of Europe (Solomou and Wu 2002)—was correlated with low tariffs in the post Cobden-Chevalier world. When the cyclical recovery came in the 1880s, average tariff levels were higher. But did countries that raised tariffs recover more strongly than those that did not?

In order to evaluate this more closely we ask whether a growth differential between hikers and non-hikers opened up in the 1880s. In other words, we ask whether the rate of growth accelerated more in the increasingly protectionist countries (treatment group) as compared to the other countries?[13] This results in a small difference-in-difference set-up. We divide the sample into countries that hiked tariffs (Argentina, Australia, Brazil, Canada, Chile, France, Germany, Italy, Japan, Norway, Russia, Spain, Switzerland) and those that failed to do so (Denmark, India, USA, UK, Mexico, Netherlands, Sweden). We use two periods (1865–1875 and 1880–1890) and our dependent variable is the difference in the growth rates of per capita GDP between these two periods. Such an estimation of growth rates in differences is not affected by omitted variable bias arising from unobserved country characteristics as the

12 The long depression of the 1870s is part of a particular feature of late nineteenth century growth that the historical literature describes as "long swings of economic growth" entailing quasi-cycles of approximately 20-year periods. Although these were national growth swings the period of the 1870s represented an episode of synchrony across a wide set of countries (Solomou 1998).

13 For a similar approach see Estevadeordal and Taylor (2008).

country effects are eliminated by differencing. The resulting differenced regression can be written as:

$$\Delta g_i = \alpha(\Delta tariff_i) + \beta \Delta X_i + v_i, \quad (4)$$

where Δg_i is the change in the per capita growth rate in the two periods, $\Delta tariff_i$ an indicator for the change in the tariff policy during the depression, and, depending on the specification, ΔX_i denotes the change in other relevant control variables.

Table 2 shows these difference-in-difference regressions using both a discrete variable—a simple dummy variable indicating whether a county raised tariffs or not—and a continuous treatment variable, namely the change in (log) tariffs over 1875–1879. A pure discrete indicator could raise concerns that too much information about policy stances is lost by reducing the variance to a 0/1 effect. In regressions (2) and (4) we also add lagged growth as a convergence term. The estimations show that countries that raised tariffs in the late 1870s did not recover more strongly in the subsequent period. If anything, the point estimates suggest that protectionist countries' growth was about 80 bp lower than in the control group that weathered the depression without succumbing to tariff hikes.

What about potential endogeneity of the tariff response—were countries that raised tariffs driven to do so by bad growth performance? Some insights can be gained by a probit model with tariff hikes (0/1) in the depression period as the dependent variable and lagged growth as the regressor. It is at least reassuring that in regression (7) we do not discover that countries raising tariffs were systematically different in this regard. If anything, it seems that higher growth countries were more likely to raise tariffs, but these coefficients are nowhere close to significance. We also repeat these estimations with the countries in the Clemens and Williamson sample in order to make sure that these effects are not driven by sample selection issues (regressions 5 and 6).

Period controls are so central because they control for the step change in growth that happened *across* countries after the 1870s depression. The sample average of tariffs increased by about 20% between 1875 and 1885; at the same time average growth in the sample increased from below zero during the late 1870s to close to 1.5% in the early 1880s as countries recovered from the depression. The positive tariff-growth correlation found in the time-dimension of the panel data stemmed from these cyclical shifts across countries in the late 1870s. But countries recovered irrespective of their tariff policies. Without period-effects one could falsely interpret this co-movement as a causal impact of tariffs on growth.

5 Tariffs and economic growth before 1914: a reassessment

In the previous section we have raised doubts about the robustness of the tariff-growth paradox. The latter seems to be highly sensitive to specification and data choices. Our next step to disentangle the historical openness-growth relationship is to employ a fully specified neoclassical growth model. In addition to a full set of control variables, including country effects and period effects, we will also test the sensitivity across various frequencies and address endogeneity issues through GMM estimation.. Our empirical strategy involves a portfolio of tests: we first look at regressions over 5-year averages and evaluate the sensitivity of the findings to averaging over 10 years. We then move on to look at the results from GMM estimation using internal instruments and pooled mean group estimation using annual data, before running a number of additional sensitivity checks and discussing the broader implications of our results.

Table 2 Growth effects of protectionist response in the 1870s depression

Dependent variable: In regressions (1) and (2) difference in GDP per capita growth rate 1865–1875 v. 1880–1890. In (3)–(5) dichotomous variable for tariff hikes in the late 1870s

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Estimation	OLS	OLS	OLS	OLS	OLS	OLS	Probit
Sample	Our sample	Our sample	Our sample	Our sample	CW	CW	Our sample
Protectionist response (0/1)	−0.00810	−0.00194			−0.143***		
	(0.0561)	(0.0528)			(0.0504)		
Change in log tariff			−0.700	−0.209		−0.446	
			(1.098)	(1.111)		(0.588)	
Real GDP per capita growth (L.1)		−0.447*		−0.395	−0.646***	−0.720***	0.889
		(0.242)		(0.268)	(0.175)	(0.212)	(3.004)
Constant	−0.0303	0.0373	−0.0313	0.0236		0.0924**	0.245
	(0.0453)	(0.0562)	(0.0334)	(0.0492)		(0.0377)	(0.549)
Observations	20	18	20	18	25	24	20
R-squared	0.001	0.167	0.025	0.149	0.512	0.354	

Standard errors in parentheses, *** $p < 0.01$, * $p < 0.05$, * $p < 0.1$

Table 3 Baseline neoclassical growth model

Growth of GDP per capita Estimation Time period Price-adjusted tariffs	(1) OLS 5 y No	(2) OLS 5 y Yes	(3) OLS 5 y2 No	(4) OLS 5 y2 Yes	(5) OLS 10 y No	(6) OLS 10 y Yes
log (1 + tariff rate)	−0.342*	−0.197	−0.188	0.0910	−1.302***	−0.477
	(0.183)	(0.197)	(0.257)	(0.257)	(0.462)	(0.559)
log (real GDP per capita)	−0.295***	−0.299***	−0.263***	−0.189**	−0.581***	−0.517***
	(0.0623)	(0.0637)	(0.0884)	(0.0897)	(0.158)	(0.178)
log (school enrollment rate)	−0.0562	−0.0484	−0.0357	−0.0262	−0.181	−0.144
	(0.0401)	(0.0412)	(0.0559)	(0.0558)	(0.115)	(0.132)
log (investment rate)	0.0114	0.0173	0.0567	0.0610	−0.0892	−0.0387
	(0.0292)	(0.0296)	(0.0378)	(0.0373)	(0.0947)	(0.105)
log (land area)	0.129**	0.120**	−0.0477	−0.0424	0.436***	0.290**
	(0.0546)	(0.0564)	(0.0778)	(0.0774)	(0.139)	(0.142)
log (n+g+d)	−0.00603	0.00709	0.0662	0.0354	0.0682	0.0589
	(0.0298)	(0.0329)	(0.0519)	(0.0535)	(0.0601)	(0.0676)
Constant	2.978***	2.949***	2.555***	1.843**	6.746***	5.585***
	(0.611)	(0.611)	(0.813)	(0.799)	(1.649)	(1.886)
Observations	152	146	133	127	62	61
R-squared	0.459	0.449	0.416	0.412	0.706	0.644
Test for time effects = 0	6.57	6.524	3.053	2.350	9.455	5.613
Prob	0.000	0.000	0.004	0.0295	0.000	0.00330
Test for country effects = 0	2.724	2.634	1.317	1.146	2.302	1.709
Prob > F	0.001	0.001	0.190	0.321	0.017	0.0881
Number of countries	20	20	20	20	20	20

Standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

5.1 Baseline OLS estimations

Table 3 displays the results of our benchmark regressions for the tariff-growth relationship over the period 1870–1913.[14] We estimate a standard neoclassical growth model by adding variables for human capital, investment in physical capital as well as the logarithm of population growth plus a constant for depreciation (d) and exogenous technological change (g). We assume the latter two to be identical across countries. As an additional regressor, we add the per capita endowment with land which is typically held constant in contemporary studies, but might have an impact in the late nineteenth century when the frontier was still pushed outwards in some countries, as argued in Taylor (1999).[15]

[14] In light of the availability of control variables we focus on the period 1870–1913, hence on the first era of globalisation coinciding with the emergence of the classical gold standard.

[15] Taylor (1999) develops and estimates a factor accumulation model that includes the change in land endowment as a regressor in his sample of convergence economies. In our fixed-effects estimations, we include land endowment in levels to capture potential effects linked to greater per capita land endowment such as immigration potential or internal gains from trade that are likely to have raised growth rates over a longer period. Quantitatively, using the log level change had no effect on the overall results.

We start our analysis with a standard fixed effects estimation over 5-year non-overlapping sub periods and then shift the frequencies as well as the tariff measure. In the Appendix (see Table A4), we also show how the results develop adding the variable step by step. There is one main message that emerges from Table 3 across the different specifications. Controlling for country and time effects, tariffs were by and large uncorrelated with growth rates in the 1870–1913 era. Some regressions would suggest a negative relationship, but this result is not particularly robust either.[16] Admittedly, the inclusion of country and time effects increase the noise-to-signal ratio, but as we argue above this is hurdle that we think the tariff-growth paradox needs to pass.

Yet we also have to be clear about the economic interpretation of these results. What we show is that typically the growth rate of a country (relative to its long-run mean) did not increase when tariffs were moved up. For a policy-maker, this is a central question. However, we cannot say much about the underlying differences in growth rates between countries. These might or might not be related to tariff levels.

What mattered for growth if not tariffs? First and foremost, there is evidence of conditional convergence. As countries grew richer, their growth rates slowed down. In addition to the convergence term, we find some evidence that land endowment per capita mattered. Also investment frequently enters with the right sign, and sometime borders statistical significance. But overall the performance of the neoclassical model is mixed. It could be that data quality is poor and measurement errors are compounded by the country effects. This being said, fixed and period effects are clearly needed as demonstrated by the tests for joint significance. Given such clear evidence of heterogeneity, evidence derived from pooled regressions would not seem particularly robust.

In regressions (2), (4) and (6) we consider the problem that revenue tariffs could be distorted by price movements (O'Rourke 2000). Yet one could also argue that the cause of tariff rate change does not matter. Whether tariff policy actually changes or price variation induces a change in tariff rates, the economic effects on trade flows and import substitution could be identical.[17] However, ex-ante we could expect policy and prices to have differential effects. For instance, policy changes may lead to expectations of a permanent shift in the tariff environment whereas price movements could easily reverse in the next period. Hence, in terms of possible effects on forward looking variables, such as investment, the effects of the two components may differ. We therefore followed the previous literature and constructed a price-adjusted tariff rate by regressing revenue tariffs on import prices for each country. We then used the residual as a proxy of tariff changes driven by policy shifts. The price-adjusted tariffs enter the growth equation mostly with a negative sign, albeit there is only limited evidence of a statistically significant relationship.

We also vary the data frequency. In (3) and (4) we average the data over alternative 5-year periods (1872–1877, 1878–1882…), and in (5) and (6) we look at 10-year intervals (1870–1879, 1880–1889…). The frequency shift broadly confirms the results from the baseline regressions. Except for regression (5) which shows a significantly negative effect of tariffs on growth, the tariff ratio remains insignificant and the coefficient estimate negative in three out of the four regressions. Again, we cannot find strong evidence for a positive openness-growth relationship pre-1914, but we certainly cannot find evidence for a tariff-growth "paradox".

[16] Both our new data and the inclusion of time effects drive this result. With the full set of controls but without time-effects the coefficient estimate is positive but insignificant with a p-value 0.49.

[17] We are grateful to a referee for clarifying our thinking on this point.

As discussed above, we also looked at weighted regressions to address the sample selection issues discussed above. We weighted observations with the inverse of the probability of being part of the sample, using GDP per capita as the weighting variable. In an additional step, we restricted the regressions to less developed countries to check whether poorer countries exhibited meaningfully different dynamics. Both regressions are reported in the Appendix. The results were very similar to the regressions reported later on as our benchmark estimations. We also studied different regional subsamples (the results are reported in the Appendix). The idea of a differential impact of tariff protection between core and periphery has received considerable attention in the previous literature (Clemens and Williamson 2004). To test whether tariff growth-effects differed by region, we ran individual regressions (again including a full-set of country and period-dummies) for different groups. We report results for two sub-samples: core economies and peripheral economies. We also looked at the European economies only and commodity exporters versus countries with a more diversified export product mix.[18] No major differences in the tariff-growth link are visible between the core and the periphery. The coefficient on the tariff rate is significant in the core sample and in the European subsample—but again with a negative sign, not a positive one.

Figure 4 visualizes our key result in the same way as above. We plot the partial correlation between tariffs and growth from a regression that includes the full set of control variables from our benchmark regressions—schooling, investment, population growth, land area—in addition to initial income, country effects and time effects. The erstwhile positive correlation now turns negative. A good part of the differences between our results and those of previous studies can be traced back to the role of time dummies as discussed above, but our expanded set of economic control variables also matters. Accounting for these, there is no evidence for a tariff-growth paradox in the late nineteenth century.

5.2 GMM estimation

In Table 3, we use difference (DGMM) and the system GMM estimators (SGMM) to take the dynamic nature of the panel into account and instrument potentially endogenous variables using their own lags. In particular system GMM has received considerable attention in growth research in the past decade.[19] The GMM estimator addresses potential endogeneity of the regressors by exploiting the panel nature of the data. The GMM method produces valid instruments under the assumption that current period shocks do not impact on past values of the regressors and that past values of the regressors do not affect output directly. But a crucial assumption for the validity of GMM estimates is of course that the instruments are exogenous. This is not easily tested. The Hansen test of overidentifying restrictions is typically used to check instrument validity, but it is highly sensitive to instrument proliferation (Bowsher 2002). The Sargan test does not suffer from instrument proliferation problems but requires homoskedastic errors, which are not present in our context. This then leaves the researcher in a difficult situation as the exclusion restriction becomes uncertain.

The problem of potentially weak internal instruments in small samples has been identified by the literature (Hayakawa 2007; Hauk and Wacziarg 2009). While internal instruments are attractive to deal with endogeneity, the weak instrument problem in system GMM has attracted considerable attention in the recent literature (Ashley 2009; Kiviet 2009; Newey

[18] We followed Clemens and Williamson (2004) for the distinction between core and periphery. We classified countries as commodity exporters if more than 90% of their exports in 1900 consisted of commodities. Data come from Clemens and Williamson (2004).

[19] A comprehensive discussion can be found in Bazzi and Clemens (2009).

and Windmeijer 2009; Bun and Windmeijer 2010). If instruments are weak, the confidence intervals will be wide. A standard test for instrument strength in GMM is not available. Not only in difference GMM, but also in system GMM weak instruments can lead to finite sample bias (Bun and Windmeijer 2010). Bazzi and Clemens 2009 show that weak instrument bias can be highly problematic in typical growth applications.

Clearly, if instruments are weak, insignificant results in GMM estimation can not be read as a strong proof for that tariffs and growth were unrelated. We therefore have to take these results with a grain of salt. Yet while we might not be able to prove that tariffs and growth were unrelated, we remain committed to our strategy of applying a portfolio of methods to test the tariff growth paradox. At the very least, the GMM estimations can show that it is equally hard to prove the existence of a positive relationship as it is to disprove it. For the instrumentation, we treat tariffs, investment and population growth as potentially endogenous variables, all others as predetermined. In order to deal with instrument proliferation, we keep the number of instruments close to the number of countries in our panel by combining the instruments into smaller sets (Roodman 2008).

With respect to tariffs, the GMM results are indeed by and large inconclusive. In the difference GMM regressions, the coefficient tends to be negative, while the system GMM coefficients tend to be positive—and insignificant in both cases. Instrument proliferation will bias the results in direction of OLS estimates (Roodman 2008). Despite our efforts to limit the number of instruments by collapsing the instrument matrix, our instrument count is above the number of groups. A bias of the results in the direction of pooled OLS would therefore not come as a surprise. As for the other variables, The GMM estimates presented in Table 4 yield slightly different convergence parameters. The human capital proxy—school enrolment rates—tends to be significant in the system GMM estimations. Yet overall, as in the OLS results above, we find that the performance of the neoclassical growth model is mixed. Besides instrument strength and data quality, also the assumption of homogenous coefficients across countries might be problematic. We will address this issue in the following section.

5.3 Pooled mean group estimation

Our next step is to use annual data for mean group, pooled mean group and dynamic fixed effects estimations. There are two key advantages of the different estimation method. First, we don't need to average data but can use all available information. Second, we can loosen the strong assumptions made before about homogeneity of the coefficients across countries. As detailed above, the mean group approach entails estimating separate regressions for each country and averaging the coefficients. The dynamic fixed effect estimator forces homogeneity on all slope coefficients while the pooled mean group estimator only imposes homogeneity on long-run coefficients. Short-term heterogeneity in the growth processes between countries would not come as a surprise to many economic historians, but the problem has also attracted considerable attention in the more recent literature. Recent applications of the mean group and pooled mean group estimators in the growth context include Lee et al. (1998), Bassanini and Scarpetta (2002), Loayza and Ranciere (2006) as well as Tan (2009). In our context, pooled mean group estimation carries an additional advantage as it enables us to distinguish between short-run and long-run effects of tariff protection.

We first estimate a standard neoclassical growth model with nominal average tariff rates before turning to import price adjusted tariffs. Table 5 clearly shows the much debated differences in convergence speed, ranging from 1.9% (DFE) to 7.7% (MG) reflecting different assumptions on shared growth rates of technology and convergence parameters. Physical

Table 4 Difference and System GMM estimation

Growth of GDP per capita	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Estimation	DGMM	SGMM	DGMM	SGMM	DGMM	SGMM	DGMM	SGMM
Time period	5 y	5 y	5 y	5 y	5 y(b)	5 y(b)	10 y	10 y
Price-adjusted tariffs	No	No	Yes	Yes	No	No	No	No
log (1 + tariff rate)	−0.782	0.532	−0.472	0.307	−0.538	0.396	−1.378	0.285
	(0.540)	(0.765)	(0.436)	(0.709)	(0.920)	(0.720)	(0.979)	(0.230)
log (real GDP per capita)	−0.660**	−0.239**	−0.726**	−0.276*	−0.831***	−0.263	−0.647**	−0.139**
	(0.248)	(0.102)	(0.273)	(0.136)	(0.253)	(0.161)	(0.233)	(0.0563)
log (school enrollment rate)	−0.0709	0.158***	−0.0354	0.156**	0.00887	0.143**	−0.191	0.0873**
	(0.101)	(0.0442)	(0.0989)	(0.0562)	(0.0727)	(0.0637)	(0.130)	(0.0313)
log (investment rate)	0.0914	−0.0611	−0.0120	−0.0272	0.115	0.0567	−0.143	−0.0274
	(0.0865)	(0.0637)	(0.124)	(0.0761)	(0.0856)	(0.0829)	(0.143)	(0.0422)
log (land area)	0.143	−0.00932	0.189	0.00600	−0.235	−0.0131	0.333	0.00271
	(0.107)	(0.0312)	(0.148)	(0.0213)	(0.141)	(0.0248)	(0.279)	(0.00612)
log (n + g + d)	−0.0322	0.176	−0.124	0.168	0.160	0.153	0.162**	0.102*
	(0.0824)	(0.127)	(0.0857)	(0.138)	(0.134)	(0.120)	(0.0734)	(0.0594)
Observations	132	152	126	146	113	133	42	62
Test for time effects = 0	5.317	2.237	5.878	3.078	2.054	4.841	3.378	5.368
Prob	0.001	0.0601	0.001	0.0174	0.092	0.002	0.043	0.004
Number of countries	20	20	20	20	20	20	17	20
AR2 test (p-value)	0.955	0.541	0.567	0.364	0.131	0.117	0.705	0.654
Hansen test (p-value)	0.969	0.996	0.925	0.961	0.999	0.999	0.985	0.998
Observations per group	6.60	7.60	6.30	7.30	5.65	6.65	2.47	3.10
j (instrument count)	31	32	30	31	30	31	26	38

Std. errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 5 Mean group, dynamic fixed effects and pooled mean group estimation

Growth of real GDP per capita	(1)	(2)	(3)	(4)	(5)	(6)
Estimation	MG	DFE	PMG	MG	DFE	PMG
Time period	1 y	1 y	1 y	1 y	1 y	1 y
Price-adjusted tariffs	No	No	No	Yes	Yes	Yes
Convergence coefficient	−0.776***	−0.193***	−0.355***	−0.763***	−0.165***	−0.324***
	(0.112)	(0.0233)	(0.0926)	(0.101)	(0.0226)	(0.0857)
log (1+tariff rate)	−0.135	−1.672***	−0.177	0.130	−0.901*	−0.346**
	(0.583)	(0.367)	(0.138)	(0.549)	(0.472)	(0.154)
log (school enrollment rate)	−0.965	−0.0662	−0.0408	−1.010	0.00354	0.293***
	(0.767)	(0.0775)	(0.0260)	(0.820)	(0.0916)	(0.0565)
log (investment rate)	0.0943***	0.0597	0.117***	0.0944***	0.0815	0.0879***
	(0.0280)	(0.0430)	(0.0112)	(0.0332)	(0.0505)	(0.0161)
log (n + g + d)	−3.476	0.218*	0.323***	−3.536	0.198	0.465***
	(2.296)	(0.125)	(0.0315)	(2.393)	(0.150)	(0.0369)
log (land area)	4.115	2.674***	−3.406***	5.279	2.623***	1.198***
	(5.657)	(0.656)	(1.256)	(5.743)	(0.785)	(0.436)
D.log (1 + tariff rate)	−0.0307	0.138	−0.0117	0.0347	0.108	0.202
	(0.296)	(0.103)	(0.375)	(0.262)	(0.0872)	(0.280)
D.log (school enrollment rate)	0.0713	−0.0769	0.129	0.108	−0.137	−0.603**
	(0.827)	(0.134)	(0.297)	(0.745)	(0.135)	(0.290)
D.log (investment rate)	0.0385	0.0190**	0.0246	0.0338	0.0184*	0.0289
	(0.0331)	(0.00925)	(0.0228)	(0.0294)	(0.00947)	(0.0211)
D.log (n + g + d)	−0.581	−8.08e-05	−0.0365	−0.670	0.00317	−0.235
	(0.593)	(0.0264)	(0.0881)	(0.661)	(0.0268)	(0.165)
D.log (land area)	0.349	−0.0644	0.731	0.353	−0.0274	0.736
	(1.895)	(0.235)	(0.649)	(1.535)	(0.238)	(0.624)
Constant	31.13	−5.798***	16.83***	12.07	−4.975***	−4.537***
	(64.29)	(1.643)	(4.572)	(61.39)	(1.659)	(1.219)
Observations	705	705	705	694	694	694
Countries	20	20	20	20	20	20
Av. observations per country	35.25	35.25	35.25	34.70	34.70	34.70
Log likelihood			1580			1548

Standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

capital accumulation was positively correlated with long run income, as was the change in land endowment and, at least in our preferred specification with price adjusted-tariffs (6), also our human capital proxy. We find again that population growth was associated with higher long run growth rates in the nineteenth century, potentially reflecting the effects of large scale migration to the New World. In the short-run, however, there is some evidence that population growth tended to depress per capita growth rates whereas investment shows a high short-run correlation with growth rates.

What about tariffs? Our results again add to the doubts about the presence of a tariff-growth paradox before 1914. In the dynamic fixed effects and pooled mean group estimations with annual data a statistically significant negative long-run relationship emerges. In the short-run, higher tariffs do not seem to have been particularly harmful to growth but they clearly did not help much either. In sum, including short-run dynamics and loosening the homogeneity assumptions of the fixed effects estimations strengthen rather than weaken our case that the hypothesis of a generalized tariff-growth paradox is not borne out by late nineteenth century data.

6 External factors in nineteenth century growth

In this last part of our empirical analysis we ask whether additional controls for terms of trade movements and real effective exchange rates influence our results on the tariff-growth paradox. A key motivation is that such factors have been discussed in the literature ever since Prebisch and Singer linked terms of trade developments to comparative development (Prebisch 1950). More recently, there has been renewed interest linking changes in export and import prices to nineteenth century growth (Hadass and Williamson 2003; Williamson 2006; Blattman et al. 2007; Williamson 2008).[20] In addition, Nugent (1973) and Haber (2005) have discussed the role of real exchange rates and their interaction with tariff policy in driving growth in the periphery, not unlike the recent literature linking real exchange rates and growth in developing countries (Roodman 2008; Eichengreen 2008). In other words, we pose the question if other variables that have been discussed in the literature as important drivers of late nineteenth century growth outcomes—such as real effective exchange rate movements and terms of trade fluctuations—share the same fate as the presumed tariff-growth paradox. Is there robust evidence that these external factors mattered much for growth trajectories?

Again, a number of disclaimers are needed. Just as our previous results do not preclude the possibility that particular forms of tariff protection could have had positive or negative effects on individual growth performance, we do not test whether real effective exchange rate and terms of trade movements can under no circumstances influence individual countries' growth paths. All we do is test whether there is evidence that changes in the terms of trade and real exchange rates were correlated with changes in the growth rate. We are posing two questions: first, do additional controls for variation of the real effective exchange rate or terms of trade change our previous results of a tariff-growth non-paradox? Second, how strong is the evidence that external factors in general were closely related with growth during the first era of globalization?

Table 6 demonstrates that our results concerning tariff policy are robust to controlling for changes in the terms of trade (1) and the real effective exchange rate (3). Controlling for these other external factors, tariffs and growth remain by and large unrelated. The coefficient estimates are sometimes negative and significant as in (1), but the sign switches in (3) and (5). Interactions between the variables also had no impact on the overall tariff-growth relationship, as shown in regressions (2) and (4). Another sobering result emerges from this exercise: there is not much evidence that real effective exchange rates and terms of trade were significant drivers of late nineteenth century growth.

In Levine and Renelt (1992) seminal robustness analysis of much of the post-1960 empirical growth literature, they conclude that almost all the results were fragile and susceptible to small changes in specification or different conditioning variables. Despite the attention that the literature has shown such factors, we are tempted to conclude that a similar phenomenon might be observable in the case of external factors in late nineteenth century growth. If they mattered for growth outcomes, these effects are not easily detected in standard empirical

[20] Jeffrey Williamson generously shared the data from his pioneering quantitative work (Hadass and Williamson 2003; Williamson 2006, 2008).

Table 6 External factors in late 19th century growth

Growth of real GDP per capita	(1)	(2)	(3)	(4)	(5)
Estimation	OLS	OLS	OLS	OLS	OLS
Time period	5 y	5 y	5 y	5 y	5 y
Price-adjusted tariffs	No	No	No	No	No
log (1 + tariff rate)	−0.369*	−2.863	0.229	−0.149	0.249
	(0.192)	(2.778)	(0.270)	(0.436)	(0.278)
log (terms of trade)	0.0471	−0.0169			0.0830
	(0.0461)	(0.0847)			(0.0703)
log(1 + tariff) * log(tot)		0.530 (0.589)			
log (reer)			0.00822	−0.0153	0.0109
			(0.0108)	(0.0238)	(0.0114)
log(1 + tariff) * log(reer)				0.179	
				(0.162)	
log (real GDP per capita)	−0.303***	−0.304***	−0.298**	−0.302**	−0.302**
	(0.0678)	(0.0678)	(0.118)	(0.118)	(0.122)
log (school enrollment)	−0.0523	−0.0504	0.0317	0.0356	0.0483
	(0.0420)	(0.0421)	(0.0591)	(0.0590)	(0.0622)
log (investment ratio)	0.00815	0.00587	0.0188	0.0278	−0.00140
	(0.0309)	(0.0311)	(0.0413)	(0.0420)	(0.0458)
log (land area)	0.156**	0.176**	0.227*	0.184	0.248*
	(0.0637)	(0.0674)	(0.134)	(0.139)	(0.139)
log (n + d + g)	−0.00822	−0.0135	0.0136	0.0412	0.00837
	(0.0319)	(0.0325)	(0.0517)	(0.0573)	(0.0534)
Constant	2.825***	3.133***	2.418**	2.473**	1.898*
	(0.692)	(0.772)	(0.991)	(0.990)	(1.095)
Observations	137	137	73	73	69
R-squared	0.463	0.468	0.549	0.562	0.535
Test for time effects = 0	6.157	6.206	2.792	2.220	2.639
Prob	0.000	0.000	0.0139	0.0450	0.0205
Test for country effects = 0	2.837	2.876	1.989	2.029	1.894
Prob > F	0.0006	0.0005	0.0426	0.0390	0.062

Standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

models. Controlling for other external growth drivers does not impact the tariff-growth relationship, nor does it reveal strong linkages between these external factors and economic growth.

While it is not possible to draw strong conclusions, it would seem that domestic factors remain central for our understanding of growth during this period. It is true that some of the most dynamic economies of the time were relatively closed as measured by trade shares. The US and Canada, among others, owed much of their economic dynamism to the development of internal markets and the exploitation of the gains from trade within their economies. Also France, Germany, the UK and some of the larger Latin American countries did not make big gains in trade openness over the period. External trade grew at the same pace or slower than the domestic economy. For the world economy as a whole, the gains in trade openness between 1870 and 1914 were rather modest. International trade grew rapidly, but domestic development proceeded at about the same pace. In our 20-country sample, on a GDP weighted basis, trade shares rose by a few percentage points only from the early 1870s to 1910.[21]

However, it is entirely possible that trade integration shaped the countries' growth trajectories in other ways. The economic forces of the growing international division of labor might have operated through other channels. For instance, Galor and Mountford (2008) have argued that trade has played a key role in the Great Divergence by driving the demand for human capital and the demographic transition. Our empirical approach focused on the policy dimension. Important cross-country trends such as a differential impact of trade openness on demography in the core and the periphery remain necessarily in the dark. Country effects capture all time invariant differences between countries that might impact on the growth process—such as legal and political institutions and geography.

However, we think that such an analysis is important as it represents a necessary corroboration of some of the findings in the previous literature. At the very least, our results suggest that the conclusions in much of the previous literature are not robust to the inclusion of country and period controls. Some generalizations about the role of external factors in the first era of globalization need qualification. The openness-growth relationship is complex, time-varying and displays significant heterogeneity.

7 Conclusion

Bairoch's hypothesis that tariff policy in the late nineteenth century stimulated economic growth has been widely accepted by much of the recent research as a stylised fact of economic growth during the late nineteenth century industrialization process. Using improved data series and more advanced panel econometric techniques, we fail to observe that within-country changes in tariffs caused within-country growth to rise in the late 19th century. On a country level, we do not find evidence for a tariff-growth paradox in the late nineteenth century. With respect to the cross-country dimension, our data also show that a number of high tariff countries were high growth countries. But this positive association could well reflect unobserved country traits rather than a uniform causal relationship between tariffs and growth. Moreover, we also used period-effects that control for shifts of the average growth rate in the sample—in particular to filter out the cyclical swings in the "Long Depression" of the 1870s—which turned out to be influential. However, we think that a presumed paradox showing the benefits of protectionism in an age of globalization has to take such high hurdles to become an accepted stylised fact of late nineteenth and early twentieth century economic history.

The portfolio of tests that we have performed suggests that the relationship between tariffs and economic growth during the period 1870–1914 was not a close one. Tariff policy changes did not go hand in hand with changes in growth. In light of more recent data as well as new panel econometric methods, the idea of a generalized tariff-growth paradox in the late nineteenth century turns out not to be robust. Our results for this era complement the findings of Rodríguez and Rodrik (2001) for the post-war period; in the late nineteenth century the openness-growth relation was complex and showed substantial heterogeneity, making generalizations difficult.

[21] The increase in openness reported here is smaller than the rise in trade shares reported in Estevadeordal et al. (2003). Our 20-country sample is not as comprehensive, but also includes all the major economies in terms of world income (accounting for 65% of world GDP) that dominate the global trends. The differences are likely to be due to calculation, not sampling: our trade ratio data is based on export, import and GDP data in current local currency from the reconstructed national accounts data. To reconstruct trade ratios for their sample Estevadeordal et al. applied the US GDP deflator to all 56 countries.

Although this paper has focused on the tariff-growth relationship, an interesting broader issue has arisen from this research. In our reading, the paradox of this era of globalization is not that free trade was bad for growth; it is that changes in international economic policies seems to have mattered little to countries' growth trajectories. At least in the light of our regressions, changes in tariff policy, real effective exchange rates and terms of trade did not correlate closely with changes in growth rates.

We recognize that these are somewhat surprising results given the importance that has been attributed to these factors in the literature. To the chagrin of international economists, trade policies might be less central to the understanding of economic performance during that crucial period of modern growth. However, trade integration and globalization might have shaped comparative economic performance through other channels than trade policy. This calls for further dedicated research on the importance of the international economy for individual countries' growth trajectories in the first era of globalization.

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A Statistical appendix

Table A1 Summary statistics

		Mean	Std. Dev.	Min	Max	Observations
Tariff rate	Overall	14.92	10.18	0.68	58.17	$N = 919$
	Between		9.46	0.86	35.13	$n = 20$
	Within		4.34	−0.05	37.96	T-bar = 45.95
Growth	Overall	0.01	0.04	−0.25	0.21	$N = 910$
	Between		0.01	0.00	0.02	$n = 20$
	Within		0.04	−0.26	0.19	T-bar = 45.5
Initial income	Overall	2259.04	1130.40	455.48	5300.73	$N = 930$
	Between		1026.50	568.28	4074.02	$n = 20$
	Within		501.67	896.50	4099.73	T-bar = 46.5
Investment/GDP	Overall	0.14	0.06	0.03	0.38	$N = 786$
	Between		0.06	0.06	0.28	$n = 20$
	Within		0.03	0.02	0.31	T-bar = 39.3
School enrollment rate	Overall	2967.00	1769.96	250.15	6755.37	$N = 975$
	Between		1773.13	284.79	5633.96	$n = 20$
	Within		404.83	1730.57	5080.46	T-bar = 48.75
Population growth	Overall	0.01	0.01	−0.01	0.05	$N = 960$
	Between		0.01	0.00	0.03	$n = 20$
	Within		0.00	−0.01	0.04	$T = 48$
Area per capita	Overall	0.14	0.30	0.00	2.13	$N = 975$
	Between		0.29	0.00	1.14	$n = 20$
	Within		0.11	−0.37	1.14	T-bar = 48.75

Table A1 continued

Import price index	Overall	0.87	0.20	0.40	1.92	$N = 870$
	Between		0.11	0.64	1.02	$n = 20$
	Within		0.17	0.44	1.96	T-bar = 43.50
Real effective exchange rate	overall	94.96	14.21	42.52	151.64	$N = 711$
	Between		10.46	69.08	111.28	$n = 17$
	Within		9.84	58.69	138.85	T-bar = 41.82
Terms of trade index	Overall	103.22	22.10	47.20	203.54	$N = 877$
	Between		15.90	80.15	148.97	$n = 18$
	Within		15.77	54.25	191.72	T-bar = 48.72

Table A2 Weighted regressions and developing countries subsample

Growth of GDP per capita Estimation Time period Sample Price-adjusted tariffs	(1) Weighted 5 y All No	(2) Weighted 5 y All No	(3) OLS 5 y LDC No	(4) OLS 5 y (b) LDC No
log (1 + tariff rate)	0.147	−0.235	0.246	−0.0850
	(0.204)	(0.178)	(0.169)	(0.194)
log (real GDP per capita)	−0.131**	−0.347***	−0.121*	−0.668***
	(0.0593)	(0.0751)	(0.0648)	(0.112)
log (school enrollment rate)		−0.0345		−0.0482
		(0.0437)		(0.0466)
log (investment rate)		0.00922		0.0161
		(0.0283)		(0.0366)
log (land area)		0.0260		−0.723***
		(0.0785)		(0.171)
log (n + g + d)		0.0220		0.139***
		(0.0366)		(0.0407)
Constant	1.101**	3.090***	0.736*	1.089
	(0.489)	(0.671)	(0.398)	(0.844)
Observations	177	152	80	59
R-squared	0.347	0.513	0.367	0.760

Standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A3 Regional variation

Growth of GDP per capita Estimation Time period Region Price-adjusted tariffs	(1) OLS 5 y Core No	(2) OLS 5 y Core Yes	(3) OLS 5 y Periphery No	(4) OLS 5 y Periphery Yes	(5) OLS 5 y Europe No	(6) OLS 5 y Europe Yes
log (1 + tariff rate)	−0.716**	−0.308	−0.181	−0.101	−0.487*	−0.304
	(0.324)	(0.308)	(0.260)	(0.247)	(0.275)	(0.331)
log (real GDP per capita)	−0.177**	−0.171*	−0.591***	−0.612***	−0.250***	−0.270***
	(0.0800)	(0.0874)	(0.109)	(0.108)	(0.0836)	(0.0879)

Table A3 Continued

log (school enrollment rate)	−0.0651	−0.0611	0.0168	0.0285	−0.127**	−0.127**
	(0.0669)	(0.0732)	(0.0573)	(0.0565)	(0.0595)	(0.0630)
log (investment rate)	0.0312	0.0287	0.0216	0.0218	0.0208	0.0203
	(0.0481)	(0.0521)	(0.0442)	(0.0429)	(0.0387)	(0.0414)
log (land area)	0.247**	0.127	0.0813	0.0845	0.229**	0.188
	(0.108)	(0.0928)	(0.0737)	(0.0720)	(0.110)	(0.114)
log (n+g+d)	−0.0333	−0.0359	0.0139	0.0129	−0.0726*	−0.0558
	(0.0453)	(0.0591)	(0.0439)	(0.0429)	(0.0420)	(0.0579)
Constant	3.377***	2.596**	4.661***	4.903***	3.352***	4.225***
	(1.072)	(1.087)	(0.909)	(0.888)	(0.983)	(1.276)
Observations	87	83	65	63	89	83
R-squared	0.505	0.469	0.668	0.676	0.555	0.546
Test for time effects = 0	3.913	3.345	4.615	4.251	5.558	5.123
Prob	0.001	0.003	0.000	0.001	0.000	0.000
Test for country effects = 0	1.728	1.305	5.224	5.960	2.661	2.371
Prob > F	0.101	0.254	0.000	0.000	0.011	0.023
Number of countries	10	10	10	10	10	10

Std. errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A4 Effects of additional control variables

Growth of GDP per capita	(1)	(2)	(3)	(4)	(5)
Estimation	OLS	OLS	OLS	OLS	OLS
Time period	5 y	5 y	5 y	5 y	5 y
Price-adjusted tariffs	No	No	No	No	No
log (1 + tariff rate)	0.0331	0.0409	−0.271	−0.341*	−0.342*
	(0.144)	(0.144)	(0.184)	(0.183)	(0.183)
log (real GDP per capita)	−0 179***	−0.183***	−0.317***	−0.299***	−0.295***
	(0.0436)	(0.0437)	(0.0595)	(0.0588)	(0.0623)
log (school enrollment rate)		−0.0423	−0.0478	−0.0574	−0.0562
		(0.0389)	(0.0401)	(0.0394)	(0.0401)
log (investment rate)			0.0166	0.0111	0.0114
			(0.0296)	(0.0291)	(0.0292)
log (land area)				0.125**	0.129**
				(0.0511)	(0.0546)
log (n + g + d)					−0.00603
					(0.0298)
Constant	1 429***	1.751***	2.941***	3.068***	2.978***
	(0.338)	(0.449)	(0.577)	(0.563)	(0.611)
Observations	177	177	152	152	152
R-squared	0.332	0.337	0.431	0.459	0.459
Test for time effects = 0	4.222	4.096	5.829	6.620	6.570
Prob	7.13e-05	0.000104	1.05e-06	1.28e-07	1.50e-07
Test for country effects = 0	2.397	2.099	2.494	2.748	2.724
Prob > F	0.00185	0.00740	0.00146	0.000463	0.000524

Std. errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

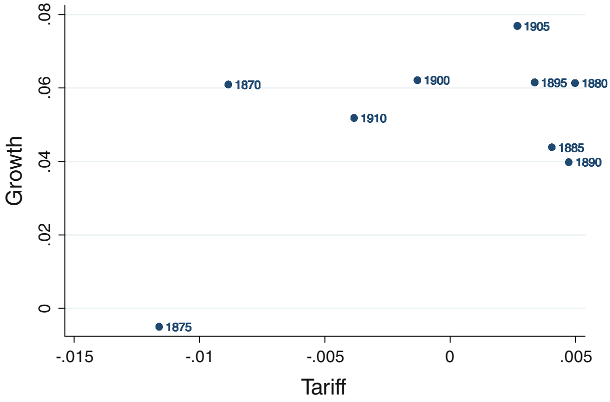

Fig. A1 Sample mean growth and tariffs

B Time-effects and the tariff-growth paradox

In this part of the Appendix, we illustrate how the inclusion of time-effects controls for common movements in growth rates across countries. In the chart below we plot the average growth rate in our 20-country sample against the average tariff rate. Figure A1 suggests a positive correlation—at times of high average tariffs, average growth rates were also high. But the positive relationship in Fig. A1 is driven by one influential observation for the depression years in the late 1870s when cross-country growth rates were extremely low at a time when tariffs were also low. As discussed in the main text, there is no evidence that countries that raised tariffs in the late 1870s recovered more or less strongly from the Long Depression of the 1870s.

C Data appendix: sources

1. Tariff rates (total import duties as per cent of imports)

Argentina

Clemens and Williamson (2004) data set. For the period 1865–1900 the tariff data are taken from the *Anuario de la Dirección General de Estadística Correspondiente al Año 1900*, Volume 1 (Buenos Aires: Compañía Sud-Americana de Billetes de Banco, 1901), p. 357, while figures for 1910–1913 come from the 1915 edition of the same publication (pp. 798 and 815).

Australia

Vamplew, W. (ed.) (1987), *Australians: historical statistics*, Fairfax, Syme and Weldon Associates, Sydney. The tariff rate is calculated as the ratio of customs revenue net of excise taxes, to the value of merchandise imports all in current prices. Customs duties are from Vamplew (1987 pp. 283–284) and merchandise imports from Vamplew (1987, pp. 282–284).

Brazil

Clemens and Williamson (2004) data set. The data is derived from Laura Randall, *A Comparative Economic History of Latin America: 1500–1914, Volume 3: Brazil* (New York: Institute for Latin American Studies, Columbia University, 1977), pp. 219–249.

Canada

Brian R. Mitchell, (1993), *International Historical Statistics: The Americas 1750–1988*, Second Edition, New York, Macmillan.

Chile

Clemens and Williamson (2004) data set. The original data is from Jose Diaz and Gert Wagner, "Importaciones, Aranceles y Otros Instrumentos de Politica Comercial. Antecedentes Siglos XIX y XX", *Documento de Trabajo del Instituto de Economia de la Pontificia Universidad Catolica de Chile, No. 223,* Santiago (2002).

Denmark

Brian R. Mitchell, 1992, *International Historical Statistics: Europe 1750–1988*. London, Macmillan.

France

Brian R. Mitchell, 1992, *International Historical Statistics: Europe 1750–1988*. London, Macmillan.

Germany

Brian R. Mitchell, 1992, *International Historical Statistics: Europe 1750–1988*. London, Macmillan.

India

B. R. Mitchell, 1995, *International Historical Statistics: Africa, Asia & Oceania 1750–1988*, New York, Macmillan.

Japan

Clemens and Williamson (2004) data set. Figures for 1870–1891 are from Brian R. Mitchell, 1998, *International Historical Statistics: The Americas and Australasia*. London, Macmillan. Figures for 1892–1914 taken from *Japan Statistical Yearbook* (Tokyo: Sorifu, Tokeikyoku, 1949), p. 471. Figures from 1893–1896 are obtained through geometric interpolation.

Mexico

Clemens and Williamson (2004) data set.

Netherlands

Jan-Pieter Smits, Edwin Horlings, and Jan Luiten van Zanden, *Dutch GNP and Its Components, 1800–1913*, Groningen, 2000. http://nationalaccounts.niwi.knaw.nl/start.htm.

Norway

Brian R. Mitchell, 1992, *International Historical Statistics: Europe 1750–1988*. London, Macmillan.

Portugal

Lains, P. (2006): "Growth in a protected environment: Portugal, 1850–1950, *Research in Economic History*, *Research in Economic History*, 2007, vol. 24, pp. 121–163.

Russia

Forrest Capie, "Tariff Protection and Economic Performance in the Nineteenth Century", in Black, J and Winters, L. A., *Policy and Performance in International Trade*, London and Basingstoke, 1983, pp. 20–21.

Spain

Clemens and Williamson (2004) data set. Current price imports are taken from Leandro Prados de la Escosura, *El Progreso economico de Espana, 1850–2000* (Madrid: 2002), and customs revenue is taken from F. Comin, *Fuentes cuantitativas para el estudio del sector publico en Espana* (Madrid: 1985).

Sweden

Brian R. Mitchell, 1992, *International Historical Statistics: Europe 1750–1988*. London, Macmillan.

Switzerland

Siegenthaler, HJ. and H. Ritzmann-Blickenstorfer (eds.) (1996) *Historische Statistik der Schweiz*, Chronos, Zürich. The data is available online at The Swiss Economic and Social History Online Database: http://www.eso.uzh.ch/modul4_en.print.html.

United Kingdom

B. R. Mitchell, *British Historical Statistics* (Cambridge: Cambridge University Press, 1988).

United States

Irwin, Douglas A., "Merchandise imports and duties: 1790–2000." Table Ee424–430 in *Historical Statistics of the United States, Earliest Times to the Present: Millennial Edition,* edited by Susan B. Carter, Scott Sigmund Gartner, Michael R. Haines, Alan L. Olmstead, Richard Sutch, and Gavin Wright. New York: Cambridge University Press, 2006.

2. Import prices

Argentina

della Paolera, G. and A. M. Taylor, 2001, *Straining at the Anchor: The Argentine Currency Board and the Search for Macroeconomic Stability, 1880–1935.* NBER, Chicago.

Australia

Vamplew, W. (ed.) (1987), *Australians: historical statistics*, Fairfax, Syme and Weldon Associates, Sydney, p.190, Table ITFC81-83 for the period 1870–1900 and Butlin M. (1977), 'A preliminary annual database 1900/01 to 1973/74', Reserve Bank of Australia Research Discussion Paper No 7701 for the period 1901–1913.

Brazil

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Canada

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We tested two different series for the US Investment ratio: (1) the Jones and Obstfeld (2001) data which is based on the Kuznets-Kendrick data sets; and (2) Gallman's investment and income series as reported in *Historical Statistics of the United States, Earliest Times to the Present: Millennial Edition,* edited by Susan B. Carter, Scott Sigmund Gartner, Michael R. Haines, Alan L. Olmstead, Richard Sutch, and Gavin Wright. New York: Cambridge University Press, 2006. The regressions presented in the paper are based on the second series by Gallman.

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