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The erosion of colonial trade linkages after independence

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ABSTRACT

Most independent nations today were part of empires in 1945. Using bilateral trade data from 1948 to 2006, we examine the effect of independence on post-colonial trade. While there is little short-run effect on trade, after four decades trade with the metropole (colonizer) has contracted by about 65%. Hostile separations lead to large, immediate reductions in trade. We also find that trade between former colonies of the same empire erodes as much as trade with the metropole, whereas trade with third countries decreases about 20%. The gradual trade deterioration following independence suggests the depreciation of some form of trading capital.

1. Introduction

The dismantling of European empires after World War II led to sweeping changes in the governance of developing countries in Africa and Asia. Recent research in economics has investigated the long-run consequences of colonial rule. La Porta et al. (1998) argue that the British endowed their colonies with a legal system that produces superior economic outcomes. Acemoglu et al. (2001, 2002) find that colonizers were more likely to establish pro-growth institutions in sparsely populated areas with lower settler mortality. Banerjee and Iyer (2005) find that 50 years after India abolished land revenue systems that the British imposed in the mid-19th century, their "institutional overhang" manifests itself in agricultural productivity differences. Huillery (2009) shows that uneven colonial investment partly explains current income inequalities within former French West Africa. In this paper, we investigate a different legacy of colonial rule: the bias in post-colonial bilateral trade patterns.

Algeria's trade with France offers *prima facie* evidence of large post-colonial trade erosion. In 1962, the year of independence, Algeria accounted for 8.8% of French imports, a share that had been stable over the preceding 14-year period. The share fell by two thirds over the next two decades (to 2.7% in 1984) and another two thirds over the succeeding two decades, reaching 1.0% in 2006. A variety of potential explanations for this fact suggest themselves. First, it might reflect poor economic performance over the last four decades by Algeria, which may have reduced its exports to all markets. Second, Algeria's abandonment of the Franc in 1964 may have raised currency transaction costs. Third, France's participation in GATT and the European Community probably redirected its import purchasing patterns, lowering the share taken by any absolute level of imports from Algeria. Fourth, deterioration of business networks and trade-creating institutions may have raised bilateral trade costs.

Utilizing data encompassing almost every country in the world from 1948 and 2006, we identify the impact of independence based on within variation in bilateral trade. In a non-parametric specification, we estimate the effect of years since independence. Unlike the work cited in the opening paragraph, we will take as given any changes in per capita incomes caused by changing *internal* institutions. We also control for *formal external* institutions (membership in regional trade agreements, GATT, and currency unions). This allows us to focus on the effects of unobserved *informal external* institutions as well as the business networks emphasized by Rauch (1999).

Countries in colonial empires choose if and when to separate, raising the concern of endogeneity bias. As we discuss in Section 2, historical accounts suggest a significant random component to independence events. Nevertheless, systematic determinants of independence are a possible source of bias. The political and economic attributes of the colonizer (metropole) and colony, as well as the strength of their bilateral association, may affect the likelihood of independence. We remove these factors, however, in specifications that eliminate time-varying country effects and non-time varying bilateral effects.

We find that four decades after independence, trade between colony and metropole had fallen by about 65%. Our results are supported by a falsification exercise where we randomly create false colonial links (with random dates of independence) and find no evidence of independence effects for the countries in these false

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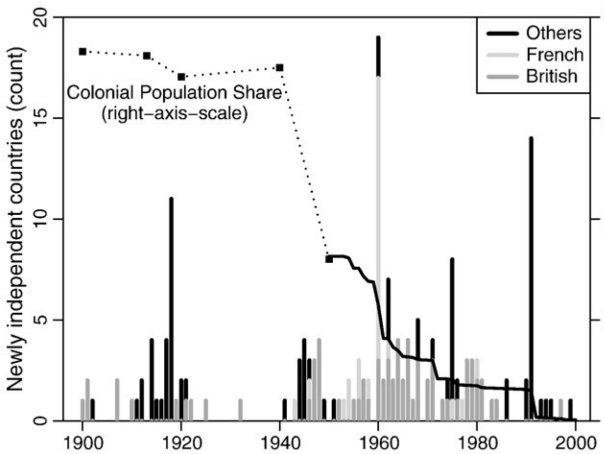

Fig. 1. Independence events since 1900.

colonial relationships. We categorize independence events into amicable and hostile separations, and find that, while the latter are more immediately destructive to trade, both generate similar levels of trade erosion in the long-run.

We also investigate potential trade redirection by examining the effects of independence on trade with siblings (other colonies in the same empire) and trade with rest-of-world (ROW). Trade erosion with siblings is comparable to that of trade with metropoles. Trade also decreases with ROW. Finally, we examine the impact of independence on the extensive margin of trade. We find that independence has a strong, but gradual, negative influence on the probability of positive trade flows between the colony and metropole. However, we see small positive increases in the propensity to trade with siblings and ROW.

The paper proceeds as follows. In the next section, we describe our panel of independence events and bilateral trade data. Section 3 specifies a gravity model employing country-pair (dyad) fixed effects. Due to the computational difficulties of estimating country-year fixed effects to capture multilateral resistance terms, we eliminate them by implementing a method of "tetrads" that takes the ratio of ratios of trade flows. Estimates of the impact of independence on bilateral trade are presented in Section 4. The concluding section summarizes and interprets our results.

2. Data on independence and trade

The principal variable of interest is the timing of independence events. We define independence as arising at the end of a *colonial* period involving long-term, civilian administration that usually includes significant settlement. The end of a military occupation is not a sufficient condition for an independence event. Information on colonial relationships comes from a variety of sources but we used the CIA World Factbook as the primary authority for independence dates.

There are 255 country pairs with colonial histories, of which 34 remain current. Fig. 1 displays the number of countries that gained independence since 1900, a total of 174.[1] The two main colonizers in this sample, the UK and France, are shown in dark and light gray, respectively, with all others grouped and represented as black bars. The two highest black spikes correspond to the possessions lost by the defeated nations after World War I and the dissolution of the Soviet Union in 1991.

The timing of the independences shown in Fig. 1 reflects a variety of political and economic forces. Historical accounts point to an important role for idiosyncratic events. For example, France's President De Gaulle first threatened to cut ties (and aid) to African colonies that voted to leave the "French Community." However, after Guinea declared sovereignty in 1958, De Gaulle reversed position and offered economic cooperation agreements to all countries that voted for independence. Fourteen colonies promptly gained independence in 1960. Rothermund (2006, p. 153) remarks that "in 1960 the French almost had to impose independence on a reluctant Gabon" because De Gaulle "did not tolerate exceptions to the granting of independence in 1960." This was despite oil and uranium resources that "the French were interested in keeping under their control." In contrast to the wave of independence for French colonies in the 1960s, Portugal adamantly clung to its five "overseas provinces" in Africa until after the Salazar dictatorship was replaced with a democratic and pro-decolonization government in 1974.

To estimate the influence of the independence events on bilateral trade, we use the International Monetary Fund's *Direction of Trade Statistics* (DOTS). It covers the 1948–2006 period, which is of crucial importance, since this includes pre-independence trade for many countries, as well as the immediate years following independence. While DOTS lacks data on trade for individual goods, it is the only data set containing a panel of worldwide bilateral trade that goes back far enough to study the main independence events of the twentieth century. Our typical regression includes around 600,000 observations.

3. Specification

In order to estimate the effects of independence, we need a benchmark for the amount of trade expected had independence not occurred. We will follow the common practice of modeling "expected" bilateral trade using a specification based on the gravity equation.

All the well-known empirical and theoretical formulations of the gravity equation can be represented in the following equation for the value of x_{ijt}, the exports from exporting country i to importing country j in year t:[2]

$$x_{ijt} = G_t M_{it}^{\mathrm{ex}} M_{jt}^{\mathrm{im}} \phi_{ijt}. \tag{1}$$

In this equation, M_{it}^{ex} and M_{jt}^{im} are indexes of the attributes of exporter i and importer j in a specific year, and G_t is a common year-specific factor determining trade. Variation in bilateral trade intensity enters through ϕ_{ijt}. We refer to M_{it}^{ex} and M_{jt}^{im} as monadic effects and ϕ_{ijt} as the dyadic effect. Following Eaton and Kortum (2002), we approximate the log of the dyadic term ϕ_{ijt} as a linear combination of factors that affect trade costs between i and j:

$$\ln \phi_{ijt} = \delta \mathbf{D}_{ijt} + u_{ijt}. \tag{2}$$

The $\mathbf{D}_{ijt}$ and u_{ijt} in this equation represent respectively observed and unobserved bilateral trade cost determinants.

The conventional approach to estimation is to take logs of Eq. (1) and substitute in Eq. (2) to obtain

$$\ln x_{ijt} = \ln G_t + \ln M_{it}^{\mathrm{ex}} + \ln M_{jt}^{\mathrm{im}} + \delta \mathbf{D}_{ijt} + u_{ijt}. \tag{3}$$

Alternatively, we can re-express Eq. (3) as

$$x_{ijt} = exp\Big(\ln G_t + \ln M_{it}^{\mathrm{ex}} + \ln M_{jt}^{\mathrm{im}} + \delta \mathbf{D}_{ijt}\Big)\eta_{ijt}, \tag{4}$$

where $\eta_{ijt} \equiv exp(u_{ijt})$. Under the assumption that the expectation of η conditional on the covariates equals one, the parameters can be

[1] Table A.4 in the Appendix lists independence events since 1900 as well as the continuing colonial relationships for which we have trade data.

[2] See Anderson and van Wincoop (2003), Eaton and Kortum (2002), and Chaney (2008) for three theoretical foundations of the gravity equation relying on very different modeling assumptions.

estimated consistently using Poisson pseudo-maximum likelihood estimation (PMLE).[3]

We use year dummies to capture $\ln G_t$. The next two subsections explain how we model the monadic ($\ln M_{it}^{\text{ex}}$ and $\ln M_{jt}^{\text{im}}$) and dyadic ($\mathbf{D}_{ijt}$ and u_{ijt}) effects.

3.1. Monadic issues

In many empirical applications the exporter and importer attributes are assumed to be determined by GDP and GDP per capita. We prefer to separate size and development effects and therefore express the monadic terms as $M_{it}^{\text{ex}}=N_{it}^{\alpha_1}y_{it}^{\alpha_2}$ and $M_{jt}^{\text{im}}=N_{jt}^{\alpha_3}y_{jt}^{\alpha_4}$, where N represents population and y is GDP per capita.[4] Plugging in these monadic effects, we re-express Eq. (3) as

$$\ln x_{ijt} = \ln G_t + \alpha_1 \ln N_{it} + \alpha_2 \ln y_{it} + \alpha_3 \ln N_{jt} + \alpha_4 \ln y_{jt} + \delta \mathbf{D}_{ijt} + u_{ijt}. \tag{5}$$

Variants on Eq. (5) are referred to as gravity equations and have been used in hundreds of papers to estimate the determinants of bilateral trade patterns. They suffer from a serious flaw that has become well-known due to the work of Anderson and van Wincoop (2003). Standard gravity equations omit "multilateral resistance" terms that are functions of the whole set of ϕ_{ijt}. Feenstra (2004, pp. 153–163) discusses different approaches to estimating gravity equations that take into account multilateral resistance. The preferred method for most applications (such as papers like ours that focus on estimating parts of $\mathbf{D}_{ijt}$) uses fixed effects for each exporter-year and importer-year to "absorb" the monadic effects ($\ln M_{it}^{\text{ex}}$ and $\ln M_{jt}^{\text{im}}$) in Eq. (3). With a balanced panel of bilateral exports, a *within* transformation could be used to remove the two sets of monadic effects. Due to missing data, zeros, and variation in the number of partners for each reporting country, actual bilateral data sets are almost never balanced. Baltagi (1995, p. 160) points out that the within transformation does not work with unbalanced two-way panels. One should therefore use the least squares dummy variable (LSDV) method. Since DOTS has close to 200 trade entities and over 50 years of trade, the LSDV approach would involve about 20,000 dummies. Estimation requires a massive matrix inversion that is beyond the capability of commonly used statistical software.

We apply a different approach to estimation. It takes advantage of the multiplicative structure of Eq. (1) and then takes the ratio of ratios to eliminate the monadic effects (including the multilateral resistance terms). This requires a set of *four* trading partners. For that reason, we call it the method of *tetrads*.

Consider four countries indexed i, j, k, and ℓ. Using Eq. (1), the ratio of i's exports to j over its exports to importer k is given by

$$R_{i\{jk\}t} = \frac{x_{ijt}}{x_{ikt}} = \frac{M_{jt}^{\text{im}}\phi_{ijt}}{M_{kt}^{\text{im}}\phi_{ikt}}. \tag{6}$$

We have canceled out G_t, and more importantly, M_{it}^{ex}, the exporter fixed effect. The $M_{jt}^{\text{im}}/M_{kt}^{\text{im}}$ ratio remains problematic for estimation however, and we now need an expression parallel to Eq. (6) containing $M_{jt}^{\text{im}}/M_{kt}^{\text{im}}$ that we can divide $R_{i\{jk\}t}$ by in order to cancel out these remaining monadic terms. This can be achieved by picking a reference exporter ℓ and calculating the corresponding ratio to the same pair of importers:

$$R_{\ell\{jk\}t} = \frac{x_{\ell jt}}{x_{\ell kt}} = \frac{M_{jt}^{\text{im}}\phi_{\ell jt}}{M_{kt}^{\text{im}}\phi_{\ell kt}}. \tag{7}$$

Taking the ratio of ratios we can define the tetradic term

$$r_{\{i\ell\}\{jk\}t} \equiv \frac{R_{i\{jk\}t}}{R_{\ell\{jk\}t}} = \frac{x_{ijt}\,/\,x_{ikt}}{x_{\ell jt}\,/\,x_{\ell kt}} = \frac{\phi_{ijt}\,/\,\phi_{ikt}}{\phi_{\ell jt}\,/\,\phi_{\ell kt}}, \tag{8}$$

where the tetrad comprises two exporters, $\{i\ell\}$, and two importers, $\{jk\}$. Taking logs, we have

$$\ln r_{\{i\ell\}\{jk\}t} = \ln \phi_{ijt} - \ln \phi_{ikt} - \ln \phi_{\ell jt} + \ln \phi_{\ell kt}. \tag{9}$$

Plugging Eq. (2) into the four $\ln \phi$ in Eq. (9), we obtain a second estimating equation:

$$\ln r_{\{i\ell\}\{jk\}t} = \delta \tilde{\mathbf{D}}_{ijt} + \tilde{u}_{ijt}, \tag{10}$$

where $\tilde{\mathbf{D}}_{ijt} \equiv \mathbf{D}_{ijt} - \mathbf{D}_{ikt} - \mathbf{D}_{\ell jt} + \mathbf{D}_{\ell kt}$ and $\tilde{u}_{ijt} \equiv u_{ijt} - \mu_{ikt} - u_{\ell jt} + u_{\ell kt}$. Each variable in $\tilde{\mathbf{D}}_{ijt}$ can take five possible values: 2, 1, 0, −1 and −2, depending on the pattern of linkages within the tetrad.[5]

The tetrad approach can be seen as an extension of existing ratio approaches that take advantage of the multiplicative functional form of the gravity equation to eliminate *either* the exporters' (Anderson and Marcouiller, 2002) or importers' (Head and Mayer, 2000; Martin et al., 2008) fixed effects. Combining the two approaches yields a specification *free of any monadic term*.[6] Two recent papers also employ the ratio of ratios to eliminate the monadic terms. Romalis (2007) estimates the response of US imports from Canada and Mexico to NAFTA tariff reductions. Hallak (2006) uses the approach to quantify the economic magnitude of coefficients obtained from fixed effects gravity equations.

The tetrad method presents two special issues. First, one needs to select the reference countries k and ℓ in order to do the tetrad calculations. In their single-ratio methods, Anderson and Marcouiller (2002) and Martin et al. (2008) take the United States as the reference country. The EU is the reference importer and the rest of the world is the reference exporter in Romalis (2007). Generating all possible tetrad combinations is infeasible since it would involve dealing with billions of observations in our case. Instead, we estimate results using the six countries with the most extensive trade partner coverage as our reference countries. While we find that the choice of reference countries has some effect on results, the basic shape and magnitude of independence effects are robust.

A second issue with tetrads concerns the independence of the observations. As represented in Eq. (10), the error terms $\mu_{\ell kt}$, μ_{ikt}, and $\mu_{\ell jt}$, appear repeatedly across observations. Indeed, $\mu_{\ell kt}$ is contained in each observation for year t. Year dummies can account for $\mu_{\ell kt}$ but correlated errors remain as a consequence of μ_{ikt}, and $\mu_{\ell jt}$. The appropriate form of clustering is more complex than usual here, since the repeated presences of μ_{ikt} and $\mu_{\ell jt}$ call for both exporter-year and importer-year clusters, which are non-nested. We therefore use three-way clustering—it, jt, and ij—employing the method of Cameron et al. (forthcoming).[7]

3.2. Dyadic issues

We divide the set of dyadic variables, $\mathbf{D}_{ijt}$, into two groups: a set of control variables typically used in gravity regressions and a set of indicators that represent current and past colonial ties. Some of the dyadic controls are time-invariant and therefore drop out in specifications based on within-dyad variation. The time-invariant

[3] See Santos Silva and Tenreyro (2006) for full development of the rationale for Poisson PMLE.

[4] Using GDP instead of population results in different coefficient estimates for ln y but an otherwise identical fit.

[5] For example the tetrad-transformed indicator variable would equal 2 if a link exists between i and j as well as between k and ℓ but not between i and k nor between j and ℓ.

[6] The computational benefits of the tetrads approach would be even greater for commodity level trade since monadic terms are presumed to be good-specific.

[7] Stata code for tetrad estimation and a link to the multi-way clustering code are available at http://strategy.sauder.ubc.ca/head/sup/.

controls are distance, shared border, shared language, and shared legal origins. The time-varying controls include belonging to a common regional trade arrangement (RTA), belonging jointly to GATT/WTO, and sharing a currency. To capture preferential tariffs conferred to former colonies by European metropoles, we add an indicator for Asia–Caribbean–Pacific (ACP) treatment of imports into the European Union (or preceding associations). The time-varying list of ACP countries is provided in Appendix A.

The colonial linkage variables identify the effects of being in a current or former colonial relationship. We use a comprehensive set of indicators to capture the presence and type of colonial relationship between two trading partners. The variable ColHist_{ij} indicates that country i and j were once, or are still, in a colonial relationship. ColAlways_{ij} turns on for trade between the countries in ongoing colonial relationships and their metropole.[8] Our focus is on colony–metropole trade in the years subsequent to independence. To avoid imposing any functional form on the evolution of bilateral trade following independence, we estimate the independence effects with indicator variables that turn on for each number of years since independence, up to a cap which we set at 60. The independence dummies (Indep1_{ijt} to Indep60_{ijt}) indicate trade between countries with independence events in all years other than the year of independence and years preceding independence.[9] Thus, trade between a colony and its metropole up to and including the independence year is reflected by the coefficient on the ColHist variable.

The vector of dyadic variables, while containing all the "usual suspects," remains incomplete. Unobserved dyadic linkages end up in the error term (u_{ijt}). The concern is that there may be unobserved bilateral influences on both trade and the decision to become independent. We employ two econometric techniques to deal with this potential source of bias. First, we introduce a lagged dependent variable to control for unobserved influences on trade that evolve gradually over time. Unfortunately, estimates are not consistent if there is a fixed component of u_{ijt} that is correlated with the control variables. The second method controls for unobserved, but fixed, component of bilateral linkages using dyadic fixed effects. This specification identifies the effect of independence based on temporal (within-dyad) variation.[10]

3.3. Treatment of "zero" and "small" observations

DOTS data include reports of trade from both the exporter and importer and we explain how we utilize both sources of information in Appendix A. Appendix A also details important data inaccuracies—incorrect zeros and implausibly small values of trade—that influence the regression method employed.

In the data set generated from the DOTS CD, 1% of the positive trade observations are valued at less than \$500 and there are 42 cases of trade of one cent. These numbers seem implausibly low and have the potential to distort results when taking logs.[11] The IMF documentation states that trade is recorded in millions with accuracy out to one or two decimal places, depending on the reporting country. Two decimal places would make the smallest value of trade \$10,000. Accordingly, we round the data to the nearest \$0.01 million; trade below 0.005 becomes zero.

After rounding, the data set has 1,204,671 total observations of which 529,663 correspond to zero trade.[12] A linear-in-logs specification converts the zeros to missing and these observations drop out of the sample, potentially introducing selection bias. The Poisson PMLE is an appealing alternative because it incorporates the zeros and delivers consistent estimates as long as η_{ijt} in Eq. (4) has an expectation of one conditional on the covariates.[13] Monte Carlo results of Martin and Pham (2009), however, show that Poisson PMLE yields "severely biased estimates" when large numbers of zeros are generated by a limited-dependent variable process. The natural method to handle data generated by a limited-dependent variable process is Tobit. While, like Poisson PMLE, Tobit incorporates the zeros, it makes strong parametric assumptions on the error term: log normality and homoskedasticity.

Techniques that incorporate zeros may generate biased estimates if some trade flows are incorrectly reported as zeros. As we discuss in Appendix A, there are instances of reported zeros in colony–metropole trade before or just after the year of independence that should be coded as missing. For example, French exports to Vietnam are erroneously recorded as zero between 1948 and 1954. In DOTS they appear to jump from zero to \$132.9 million in 1955 (1954 is the year of independence). Russian exports to Ukraine jump from 0 to \$6 billion from 1993 to 1994. Such incorrect zero trade observations can lead to bias in the estimated independence effects in either Tobit or Poisson PMLE.

Another problem for Tobit estimation is what to use as the left censor value. Cameron and Trivedi (2009, p. 531) recommend using the observed minimum value of logged exports. We do not want to use the actual minimum value of one cent but the minimum value implied by our rounding suggests a left censor of ln(0.005). However, it is not certain that 0.01 million is the correct rounding point for all trade flows. Felbermayr and Kohler (2006) add one to actual trade in order to include the zero trade observations. This method generates different results depending on the units of actual trade (i.e., dollars or millions of dollars). We ran Tobit regressions using four different ways of handling zeros: coding zeros to be \$5000, coding them as \$500, adding one to exports in dollars, and adding one to exports in millions of dollars. In results available at http://strategy.sauder.ubc.ca/head/sup/, we find that the independence estimates are highly sensitive to the treatment of the zeros. Depending on the specification, we find both Tobit and marginal effects (Tobit estimates multiplied by the probability of non-zero trade) that are smaller and larger than those obtained in OLS regressions where the zeros are dropped. The same holds for coefficients on other gravity covariates such as GDP and distance.

In light of the problems associated with incorrect zeros and the sensitivity of Tobit estimation to the value assigned to zero trade, we do not use this method of estimation. Instead, we follow the conventional method of taking the log of actual trade and dropping observations where trade is recorded as zero. We also report estimates for Poisson PMLE to verify that our results are robust to this specification.

4. Results

Before presenting regression results, we begin this section by providing evidence of large independence effects using two instructive cases. Our main econometric results are discussed in Section 4.2 where we report estimates of the control variables and independence effects for six alternative specifications. In the following subsection, we conduct a falsification exercise to test whether the results are driven by spurious dynamics. Section 4.4 categorizes independence events as amicable or hostile and examines differences in trade erosion between the two. We extend the analysis to investigate the effects of independence on trade between colonies with a common metropole and trade with the rest-of-world in the ensuing subsection. Finally, in Section 4.6, we consider the effect of independence on the extensive margin of trade.

[8] We define ongoing as existing in 2006, the last year of our sample.

[9] There are only 1474 positive trade values for colonial trade prior to independence.

[10] Baier and Bergstrand (2007) and Glick and Rose (2002) find that dyad fixed effects can lead to substantially different results for regional trade agreements and currency unions.

[11] The log of 1.0e−8 million is −18, which is more than 5 standard deviations away from the mean of log exports.

[12] The standard gravity and Poisson PMLE regressions lose 82,085 and 258,798 observations, respectively, due to missing GDP and population data. The tetrad specification loses 45,008 observations due to zeros in the reference country trade flows.

[13] Efficiency requires that the variance be proportional to the conditional mean.

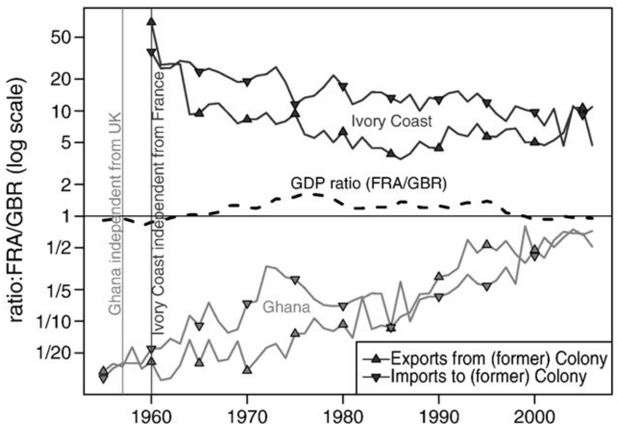

Fig. 2. Trade of Ivory Coast and Ghana with their respective metropoles.

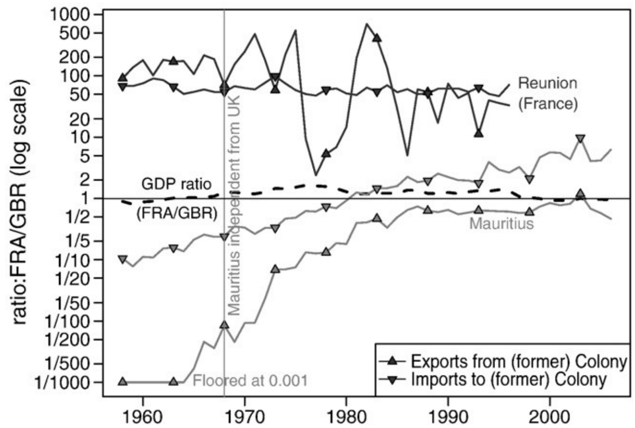

Fig. 3. Trade of Reunion and Mauritius islands with their respective metropoles.

4.1. Graphic examples of independence effects

Fig. 2 displays Ivorian (*I*) and Ghanaian (*G*) trade patterns with France (*F*) and the United Kingdom (*U*). The figure reports the ratio of the two countries' trade flows to and from France divided by the corresponding flow with the UK (x_{IF}/x_{IU}, x_{FI}/x_{UI}, and x_{GF}/x_{GU}, x_{FG}/x_{UG}). The Ivory Coast was a colony of France until 1960 and Ghana a colony of the UK until 1957. Ghana and the Ivory Coast make a useful case study since they are adjacent, comparable in size, and yet had different colonizers. Differences in distances between colonies and metropoles seem negligible. Furthermore, changes in multilateral resistance indices should be fairly similar.[14] If colonial ties did not influence trade, we would expect that the ratio of exports to France over exports to the UK (shown with up-pointing triangles) to be approximately equal to the relative size of their markets. Similarly, relative imports from the two sources (down-pointing triangles) would be equal to their relative production. Using GDP as the measure of market and production size, all four trade lines would be expected to be close to the France-to-UK GDP ratio (dashed line) if colonial history did not matter. Instead, we see large gaps on both sides.

France's former colony Ivory Coast trades much more with its former metropole than France's relative size would imply. The ratio of export ratios to GDP ratios is 79 in the year it became independent. By 2006, the ratio had fallen to 6. Its imports also begin heavily biased towards France (ratio of 39) and, while the import bias also declines, it persists at 12 in 2006. On the other hand, Ghanaian trade exhibits bias towards the UK. The ratios of relative trade to relative GDP are 13.4 (exports) and 23.1 (imports) in 1957. Their decline in recent years has been remarkable and the bias has fallen to 1.9 (exports) and 1.3 (imports) in 2006. Even these numbers should be seen as impressive: Forty-six years after independence Ghana still exports about 90% more to its former ruler than a simple gravity model would predict. From our within-dyad regression estimates in Table 2, this is larger than if Ghana and the UK belonged to a regional trade agreement or a currency union.[15]

Another interesting illustration can be made using two comparable countries, where one gained independence, while the other remained part of the national territory of the colonial power. The two islands of Reunion and Mauritius are particularly good examples, featured in Fig. 3, which uses the same graphical devices as Fig. 2. The two islands are only 250 km away, and were both under the control of France from the early 18th century until the United Kingdom took over both islands in 1810. By historical accident, the Congress of Vienna in 1815 gave Reunion back to France, while Mauritius remained a British colony (until the peaceful 1968 independence). The difference in the trade patterns of the two islands is quite striking. For Reunion, both relative exports and imports seem to fluctuate around an equilibrium stable level of 50, comparable to the level of Ivory Coast at the time of independence in Fig. 2, but around 50 times higher than the expected level. By contrast, Mauritius has a very different trade pattern—independence marks a sharp change in the ratio of relative exports to France and UK. While the "metropole premia" was close to a factor of 200 in 1968, it falls gradually over time, so that Mauritian exports to UK and France since 2000 are roughly the same as the GDP ratio. Figs. 2 and 3 both portray an erosion of colonial trade subsequent to independence. We show several other versions of these figures for different country pairs at http://strategy.sauder.ubc.ca/head/sup/. To estimate the average effects of years since independence on all post-colonial relationships we now turn to regressions.

4.2. Independence effects estimates

Table 2 and Fig. 4 contain estimation results. We report results for six regressions and present estimates of the control variables in the first table and graphs of independence effects in a six-panel figure. Table 1 shows the five different specifications we employ.

In the first four specifications, monadic effects are captured in exporter and importer population and per capita income. The first column presents coefficients for OLS, the typical way gravity models have been estimated while the second column contains the Poisson PMLE results. The ensuing two columns employ alternative methods of dealing with unobserved dyadic effects: OLS with a lagged dependent variable and dyadic fixed effects. Standard errors of estimates in each of the first four columns are robust to heteroskedasticity and correlation of error terms within *ij* pairs.

The last two columns follow Eq. (10) in using tetradic ratios of trade flow ratios to eliminate the monadic effects. This requires choosing reference countries (designated k and ℓ in the previous section). In column (5) we choose France as the reference importer (k) and the UK as the reference exporter (ℓ). They are the two main colonizers in our sample and have relatively complete data. Standard errors in column (5) are calculated using three-way clustering: *it*, *jt*, and *ij*. We then find average results of tetrad regressions run for all 30 possible combinations of the six countries with the largest number of partners (France, UK, Germany, USA, Italy, and Netherlands) as the reference importers and exporters. Column (6) summarizes the results of the 30 regressions by reporting the mean and standard deviation of each variable's coefficient.

The first specification pools data in a simple linear regression, allowing us to compare results for our large panel to those in the large gravity equation literature. The results, listed in column (1), show that increases in exporter- and importer-country per capita income and

[14] A surge in Nigerian GDP would have approximately the same effect on Ghana and Ivory Coast, whereas a surge in German GDP would have similar effects on the UK and France.

[15] Column (4) of Table 2 implies that RTAs and currency unions increase trade by *exp*(0.435) − 1 = 54% and *exp*(0.416) − 1 = 52%.

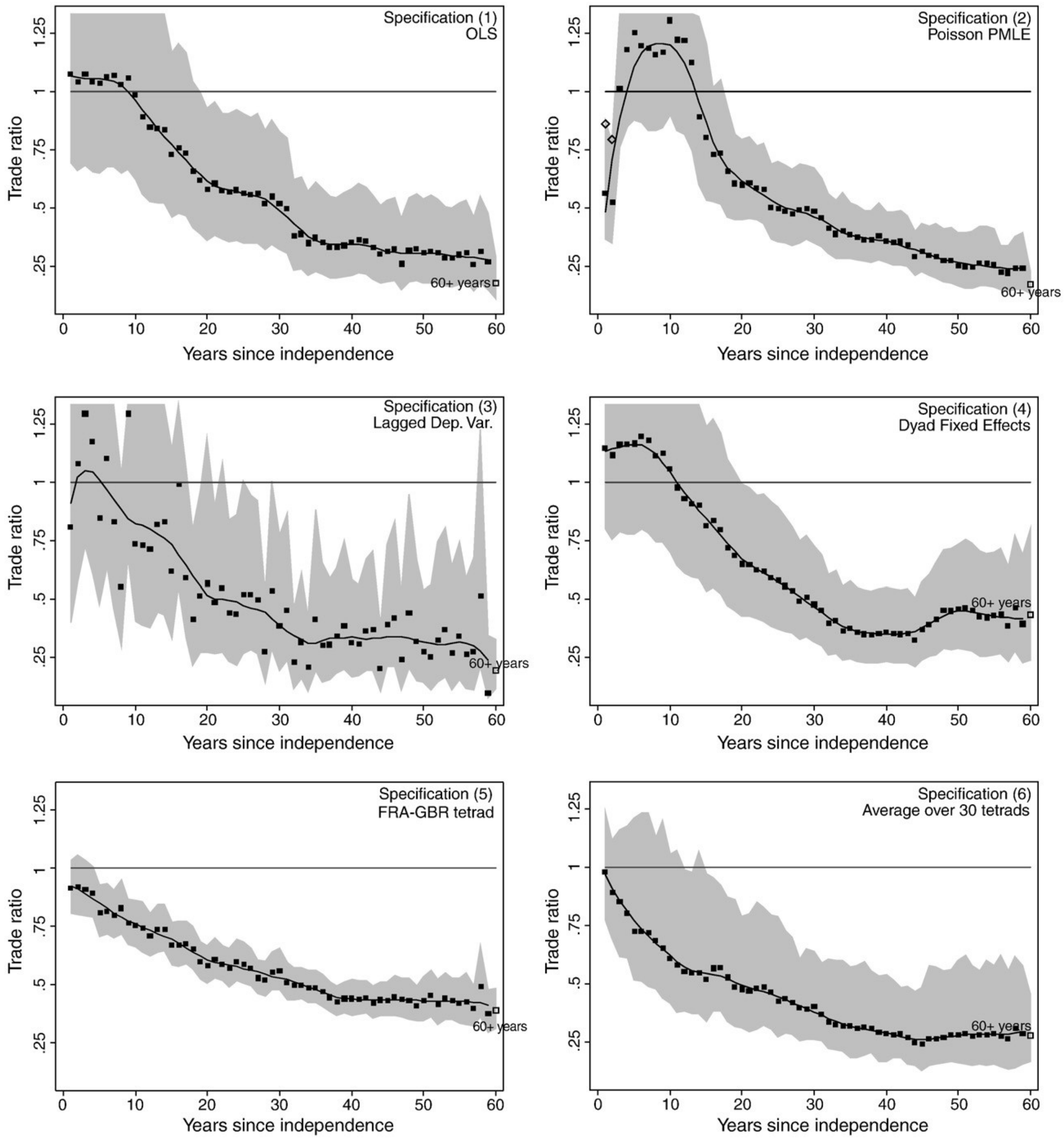

Fig. 4. Non-parametric independence effects.

population promote bilateral trade with elasticities that vary between 0.768 and 1.026. Distance between partners reduces trade and the estimated elasticity is very close to one (the typical finding). The other dyadic control variables—shared border, shared language, shared legal origins, RTA and GATT membership, and currency union—increase trade as expected and all estimates are highly statistically significant. We also observe that ACP customs treatment is associated with significantly higher trade.

Since post-independence trade between a colony and its metropole is captured by the 60 independence dummies, ColHist reflects colony–metropole trade up to and including the year of independence. Column (1) reveals that, prior to independence, colony–metropole trade was 7.4 ($=exp(1.995)$) times higher than trade between countries that are not in colonial relationships. Trade between the countries that are in ongoing colonial relationships and their metropole is measured by the sum of ColAlways and ColHist. This sum equals 1.035, indicating almost three times more trade than countries never in a colonial relationship. Since the coefficient on ColAlways is imprecisely estimated, the data do not reject the hypothesis that countries that remained colonies have the same tendency to trade with the metropole as the ones that ultimately became independent.[16]

Results shown in column (2) are estimated using Poisson PMLE to incorporate observations with zero trade. Coefficients on the monadic and time-fixed dyadic variables are similar to the OLS estimates, generally having the same signs and levels of significance. The distance and ColHist coefficients fall by one-third. Exponentiating the estimate

[16] While there were 33 current colonies in 2006, DOTS only provides trade data for 16 of them.

Table 1
Regression specifications used in Table 2 and Fig. 4.

Abbreviation	Dep. Var.	Monadic Vars.	Dyadic-observed	Dyadic-unobserved
(1) OLS	$\ln x_{ijt}$	$\ln N_i$, $\ln N_j$, $\ln y_i$, $\ln y_j$	All $\mathbf{D}_{ijt}$	None
(2) Poisson PMLE	x_{ijt}	$\ln N_i$, $\ln N_j$, $\ln y_i$, $\ln y_j$	All $\mathbf{D}_{ijt}$	None
(3) LagDV	$\ln x_{ijt}$	$\ln N_i$, $\ln N_j$, $\ln y_i$, $\ln y_j$	All $\mathbf{D}_{ijt}$	$\ln x_{ij,t-1}$
(4) DyadFE	$\ln x_{ijt}$	$\ln N_i$, $\ln N_j$, $\ln y_i$, $\ln y_j$	Time-varying $\mathbf{D}_{ijt}$	Fixed effects
(5) Tetrad	$\ln r_{\{i\ell\}\{jk\}t}$	N/A	Time-varying $\tilde{\mathrm{D}}_{ijt}$	Fixed effects

Note: $\mathbf{D}_{ijt}$ comprises log distance; ij indicators for sharing a border, a language, legal origins, colonial history, ongoing colonial relationship; ijt indicators for regional trade agreements, common currency, both i and j in GATT, i in ACP and j in EU, and Indep1 to Indep60. All specifications include year dummies.

for ColHist, 1.317, implies that a colonial relationship magnifies trade by a factor of 3.73. In Santos Silva and Tenreyro (2006), Poisson PMLE estimates on distance and colonial history (column 6 of their Table 3) also fall relative to the OLS estimates (column 1). In common with our results, the distance coefficient declines by one-third. Their estimates for colonial history plunge from 0.397 (OLS) to 0.024 (Poisson PMLE), with the latter insignificantly different from zero. Turning to the time-varying dyadic variables, we find that they generally become insignificant in our Poisson PMLE specification. The exception being ACP that switches signs, becomes negative, and is significant at the 10% level.

We introduce a lagged dependent variable to the OLS specification and report results in column (3). The rationale for including a lagged term is that trade patterns tend to show persistence over time and shocks (like independence) take time to become fully reflected in trade flows. Furthermore, the lagged dependent variable can be seen as a control for slow-moving unobserved influences on trade. A drawback of this specification is that we lose early observations that often coincide with the year of independence. The lagged dependent variable enters with a coefficient of $\hat{\rho}=0.84$. The short-run effects of changes in the covariates are reflected in the coefficients shown in column (3). Multiplying the coefficients by $1/(1-\hat{\rho})=6.25$ leads to estimates of the long-run effects of changes in each covariate.[17] With the exception of GATT and ACP, re-scaling the column (3) coefficients by 6.25 generates estimates quite similar to those in column (1). In the case of ACP, controlling for slow-moving unobservables causes the estimate to flip signs relative to column (1).

Column (4) reports results based on within-dyad variation in trade. Linkage variables that do not vary over time (distance, shared language, shared legal origins, ColHist, and ColAlways) are captured by the dyadic fixed effects and drop out of the specification. In comparison to the column (1) pooled OLS estimates, the coefficients fall but remain statistically significant. The GATT effect of 0.18 is close to the 0.15 estimate that Rose (2004) obtains when he employs dyadic fixed effects. The RTA estimate of 0.45 is somewhat smaller than Baier and Bergstrand's comparable estimate of 0.68.[18] The effect of currency unions, 0.42, is lower than the 0.65 found in Glick and Rose (2002) using the same method, but a somewhat smaller sample. As with the coefficient obtained in the LagDV specification, using within-dyad variation results in a negative estimated effect for ACP treatment.

In the final two specifications, the tetrad method removes all (time-varying) monadic effects (e.g., population, per capita income, and multilateral resistance terms). We also employ dyadic fixed effects. Looking across the final two columns, regressions that use France and the UK as reference countries (column 5) or an average of 30 tetrad combinations (column 6), we find that the signs of the estimated coefficients on RTA, GATT, and currency union are the same as those listed in column (4) but have lower magnitudes. The ACP coefficient reverses sign and becomes positive again. It appears that the perverse negative effects found in the previous three specifications derive from unobserved changes in the monadic effects of either the ACP or EU members. After removing such effects, the tetrad regressions lead to estimates of ACP effects that are similar in magnitude to the GATT and currency union effects.

Fig. 4 displays our estimates of the 60 years-since-independence dummy variables. The six panels correspond to the specifications in Table 2. The squares represent exponentiated coefficients of the variables indicating 1, 2, 3,..., 60+ years since independence.[19] The empty square at 60 gives the average reduction in trade for 60 or more years of independence. In the lagged dependent variable specification, independence effects are scaled (prior to exponentiating) by $1/(1-\hat{\rho})$ so that they reflect long-run effects. We display a LOWESS smoothing line through the estimates. The first five panels shade the 95% confidence intervals for each estimate based on the standard error of each coefficient. The squares in the bottom-right panel represent averages of coefficients for 30 reference country combinations. The shading in this panel corresponds to the region between the 10th and 90th percentiles. The reference group in all six panels is the trade during the year of independence and the years prior to independence (given by ColHist in the first three columns, but normalized to zero in the specifications with dyadic fixed effects). For example, at 30 years of independence, the OLS specification graph (top left) tells us that trade between former metropole and colony is about 50% of what it was during the reference years (all else equal).

Non-parametric estimates of independence effects in the first four specifications, shown in the top four panels in Fig. 4, depict a common time-profile for colony–metropole trade. Trade tends to be slightly higher relative to the base year in the first ten years but this difference is not statistically significant. Subsequently, trade erodes steadily to about 35% of pre-independence trade 40 years after independence and then remains fairly steady. Since the LagDV specification drops initial observations for each dyad and the lagged dependent variable captures much of the variation in exports, the standard errors of the estimated independence effects are very large, as revealed by the wide confidence intervals in the middle-left panel.

The Poisson PMLE estimates (upper, right panel) differ from those obtained in the other specifications in indicating sharp declines in trade in the first two years after independence. We find that this discrepancy mainly results from the incorrect zeros discussed in Section 3.3. We re-estimate with a restricted sample where we try to systematically eliminate incorrect zeros. Our primary criterion for keeping observations corresponding to zero is that the value is corroborated by reports from both the exporter and importer. This filter eliminates most of the egregious incorrect zeros that occur during the colonial period. For example, the zero trade observations between France and Vietnam from 1948 to 1954 are dropped. We also drop the suspect zero trade observations between former USSR countries and all other countries in the years 1992 and 1993.[20] For the full sample, there are 592,923 positive trade flows and 352,950 zeros. With the reduced sample, the number of zeros falls to 288,456. The estimated independence effects for the first two years after independence for the reduced sample are shown with hollow diamonds in the upper, right panel. They are substantially less negative than the corresponding estimates based on the full sample and, in one case, not

[17] A permanent one unit rise in $\mathbf{D}$ for a pair ij at time t increases contemporaneous trade (x_{ijt}) by a direct effect of δ (in t as well as for all following years). There is also the indirect effect through lagged trade. In $t+1$ for instance, the shock on $\mathbf{D}_{ijt}$ further raises $x_{ij,t+1}$ by $\delta\rho$. In period T the total accumulated effect of the change in D is given by $\delta(1+\sum_{\tau=1}^{T}\rho^{\tau})$. As $T\rightarrow\infty$ the series converges to $\delta/(1-\rho)$.

[18] Their estimate falls to 0.46 when monadic fixed effects are introduced.

[19] Exponentiating makes the results easier to interpret since the y-axis expresses an estimate of the ratio of trade after x years of independence relative to the pre-independence levels.

[20] We kept zeros corresponding to Russian trade with countries other than those that were members of the Soviet Union.

Table 2
Gravity regression control variables.

Specification	(1) OLS	(2) Poisson PMLE	(3) LagDV	(4) DyadFE	(5) Tetrad FRA, GBR	(6) Tetrad 30 Avg.
Monadic variables						
ln Pop, origin	0.882[a]	0.805[a]	0.142[a]	0.223[a]		
	(0.006)	(0.025)	(0.002)	(0.045)		
ln Pop, dest	0.767[a]	0.811[a]	0.124[a]	0.886[a]		
	(0.006)	(0.025)	(0.002)	(0.039)		
ln GDP/Pop, origin	1.030[a]	0.784[a]	0.162[a]	0.659[a]		
	(0.007)	(0.029)	(0.002)	(0.015)		
ln GDP/Pop, dest	0.868[a]	0.825[a]	0.138[a]	0.634[a]		
	(0.007)	(0.027)	(0.002)	(0.014)		
Time-fixed dyadic variables						
ln Dist (avg)	−0.906[a]	−0.641[a]	−0.144[a]			
	(0.014)	(0.040)	(0.003)			
Shared border	0.598[a]	0.548[a]	0.086[a]			
	(0.062)	(0.110)	(0.011)			
Shared language	0.434[a]	0.524[a]	0.055[a]			
	(0.032)	(0.111)	(0.006)			
Shared legal	0.306[a]	0.134	0.054[a]			
	(0.024)	(0.087)	(0.004)			
ColHist	1.995[a]	1.317[a]	0.300[a]			
	(0.233)	(0.141)	(0.041)			
ColAlways	−0.960	−0.610	−0.173			
	(0.643)	(0.418)	(0.111)			
Time-varying dyadic variables						
RTA	0.868[a]	−0.054	0.136[a]	0.435[a]	0.420[a]	0.383[a]
	(0.038)	(0.102)	(0.007)	(0.025)	(0.028)	(0.062)
Both GATT	0.120[a]	0.060	0.003	0.181[a]	0.102[a]	0.118[c]
	(0.018)	(0.059)	(0.003)	(0.015)	(0.037)	(0.082)
Shared currency	0.638[a]	−0.008	0.091[a]	0.416[a]	0.125[a]	0.290[c]
	(0.078)	(0.086)	(0.014)	(0.065)	(0.038)	(0.156)
ACP	0.156[a]	−0.199[c]	−0.032[a]	−0.402[a]	0.256[a]	0.097
	(0.057)	(0.115)	(0.010)	(0.051)	(0.067)	(0.186)
Lagged exports			0.840[a]			
			(0.001)			
Observations	592,923	945,873	533,359	592,923	630,317	624,855.9
R^2	.627	.743	.891	.843	n/a	n/a
RMSE	1.888	1.903	0.974	1.225	1.465	1.481

Note: Standard errors in parentheses with [a], [b] and [c] respectively denoting significance at the 1%, 5% and 10% levels. Standard errors are robust to correlation of errors within dyads in columns (1) to (4). Column (5) clusters by *ij*, *it*, and *jt*. Column (6) shows mean and standard deviation across 30 tetrad regressions.[a]Means no negative coefficients, [b]less than 5% negative, and [c]less than 10% negative.

statistically different from zero. The other independence effects do not differ perceptibly across samples.

The tetrad specifications, shown in the bottom two panels, also display substantial trade erosion. The average of the tetrad regressions (bottom-right panel) portrays a 70% contraction in long-run trade (after 40 years). One difference from the above panels is that tetrad specifications estimate trade erosion to begin immediately after independence. The tetrad method eliminates unobserved monadic effects. It appears that in the first decade following independence, either former colonies or their metropoles had higher multilateral trade propensities than before. We infer that in specifications (1)–(4) these higher average trade propensities are reflected in positive coefficients for the initial set of independence dummies. However, one should not make too much of these differences given the wide confidence intervals. The main finding that all six specifications agree on is that post-independence trade does not exhibit immediate significant changes, but that after several decades, the accumulated erosion is large and statistically significant.

To measure colony–metropole trade after *n* years of independence relative to trade between countries that never had a colonial relationship, the coefficient on the relevant independence variable should be added to the coefficient on ColHist. In the case of the OLS estimates, the sum of ColHist and the coefficient for being independent 60 or more years is $1.995-1.722=0.273$. Exponentiating and subtracting one reveals that, on average, colony–metropole trade remains 31% higher than trade of countries that were never in colonial relationships. The LagDV results are remarkably similar: Adding the 60+ coefficient to the colonial history effect ($0.300-0.262=0.038$), scaling by 6.25, and exponentiating suggests that a 27% trade boost persists after 60 years. Long-run effects of this magnitude would not be surprising in light of the fact that we have not controlled for all conceivable long-run legacies of the colonial relationship. For example, overlap in ethnic populations is a long-run consequence of colonization that is known to be positively associated with trade.[21]

It is not clear, however, that a colonial history remains a positive influence on trade after 60 years. In the Poisson PMLE, the sum of the corresponding estimates is negative, $1.317-1.740=-0.423$, indicating 34% *less* trade. We prefer the DyadFE and Tetrad specifications to the first three specifications because they control for unobserved dyadic effects (and monadic effects in the Tetrad specifications) that are correlated with independence. However, ColHist cannot be estimated in these specifications because it does not vary over time. Thus, while all the specifications show substantial trade erosion subsequent to independence, the evidence is inconclusive on whether trade between metropoles and former colonies remains permanently

[21] See Rauch and Trindade (2002) for evidence.

higher than the level of trade for countries that were never in a colonial relationship.

Three possible explanations for reductions in trade with the metropole after independence are (1) reverse causation, (2) the termination of trade arrangements imposed by the metropole, and (3) the deterioration of trade-promoting capital such as common institutions and business networks. Reverse causation would arise if metropoles relinquish control of colonies once they have exploited all of the trading opportunities (e.g., extracted the natural resources). Under this story, independence is the consequence, not the cause, of lower trade. In the second story, the metropole has also colonized countries in order to exploit its natural resources. Presumably, this would have distorted trade patterns of the colony to be over-specialized in trading with the metropole. Following independence, this abnormally high level of trade with the former metropole would be abandoned by the newly autonomous authorities of the colony. In cases (1) and (2) we would therefore expect an immediate shift to a lower level of trade than what prevailed prior to independence. Thereafter trade would be expected to remain constant. Depreciation of trade-creating capital over a 40-year period could occur as a result of the gradual retirement of business people who facilitated trade within the empire. Thus, the continuous trade erosion depicted in Fig. 4 most closely conforms with explanation (3).

4.3. Falsification exercise

Since the vast majority of independence cases involve a European country, there is the concern that our results are driven by a tendency for metropoles to reorient trade towards other European countries and away from poor and remote countries (some of them being ex-colonies, some not). Several factors might explain this general trend, including the coincidence of European integration with decolonization, and the growing importance of trade in manufactured goods (as opposed to primary goods) over that period.

In order to address this concern, we conduct a falsification exercise. We first identify a control group of countries that were never colonized and have characteristics similar to the colonized group. It turns out that this set of countries is a quite restricted one. It includes Bhutan, China, Ethiopia, Iran, Liberia, Nepal, Oman, Saudi Arabia and Thailand.[22]

We assign the never-colonized countries randomly to potential colonizers from Europe other than the UK and France, the two principal European metropoles of the last century. We generate random independence dates between 1950 and 1975. Table 3 shows the resulting false history of decolonization.

We add a set of years-since-independence dummies for these false independence events. Because the first false independence date is 1962 and our sample ends in 2006, there are 44 independence dummy variables. We focus on specifications (4) and (5), DyadFE and Tetrads (using France and Britain as the reference countries), because they remove unobserved dyadic effects that may be correlated with the independence variables.

Fig. 5 portrays independence effects for the "false" colonial relationships. The left panel shows dyadic FE estimates and reveals *positive* independence effects, many of which are significant. Because the dyadic FE estimates do not include monadic effects, the positive results we observe might reflect increasing multilateral trading propensities (beyond what is captured by the unreported year dummies) of countries in the false colonial relationships. The tetrad method factors out these monadic effects. The right panel displays the tetrad estimates and we find independence effects that are about equally divided between positive and negative coefficients and are never statistically significant. These results certainly do not suggest that the trade erosion we find for actual colonial relationships is driven by spurious dynamics.

[22] Andorra, Norway, San Marino, and Switzerland were also never colonized, but are clearly too dissimilar to be used in the control group.

Table 3
False colonial relationships and independence dates.

False colony	False metropole	False indep. date
Bhutan	Belgium	1967
China	Italy	1973
Ethiopia	Spain	1962
Iran	Spain	1962
Liberia	Germany	1974
Nepal	Austria	1964
Oman	Switzerland	1970
Saudi Arabia	Sweden	1971
Thailand	Spain	1965

4.4. Amicable versus hostile separations

The circumstances of the dissolution of colonial ties varied greatly. For example, Algeria's war for independence from France involved a protracted (1954–1962) and bloody conflict, whereas Senegal's 1960 independence occurred peacefully. We would expect hostile independence events to cause more trade disruption than amicable ones. Indeed, it seems possible that amicable separations do not depress trade at all and that the results we have obtained so far are averages of negative consequences of hostile separations and zero effects for amicable ones. We test this proposition by categorizing independence events as peaceful or hostile. Of the 220 independence events in our data set, we categorized 154 as amicable and 66 as hostile.[23] However, limiting the sample to events that provide times series information in our period of study, i.e. those occurring after 1900, we have 131 amicable and 43 hostile separations.

Fig. 6 presents estimated independence effects for each type of separation. The left panel portrays results using the DyadFE specification whereas the right panel reflects the Tetrad specification (again with France and Britain as the reference countries). Since we now have two lines per figure, we use shading (amicable) and brackets (hostile) to identify the 95% confidence intervals of the estimated trade reduction ratios. Both panels indicate that trade eroded after independence for hostile and amicable separations. However, hostile separations were followed by abrupt collapses in trade. As before, the DyadFE specification yields positive effects in the early years after independence and the tetrad method shows immediate trade declines. Throughout almost all of the post-independence years, hostile separations are associated with larger reductions in trade but the differences become smaller over time. After about 55 years of separation the point estimates are quite similar and the differences are not statistically significant.

4.5. Trade with siblings and ROW

We observe that independence reduces colony trade with the metropole. But what happens to colonial trade with other countries in the colonial empire (siblings) as well as rest-of-world (ROW) countries? As is the case for trade with the metropole, trade between siblings may decline suddenly due to termination of trade arrangements imposed on members of the empire or gradually due to depreciation of colonial capital. There are a couple of reasons to expect that trade might *increase* with ROW countries. First, rising trading costs with the metropole and siblings could redirect trade to other countries. Second, the metropole might have constrained the ability of colonies to trade with ROW countries prior to independence.[24]

[23] We started with information listed in the "Territorial Change" database (Tir et al., 1998) from the Correlates of Wars project and used internet sources (the CIA Factbook, BBC country briefs, and Wikipedia) to complete the classification, shown in Table A4.

[24] Bonfatti (2008) develops a Heckscher–Ohlin model of trade between a colony, a metropole, and a third country which predicts that independence is more likely for colonies with good trading opportunities with the rest of world. An implication of the analysis is that independence should be accompanied by increased trade with the third country (ROW).

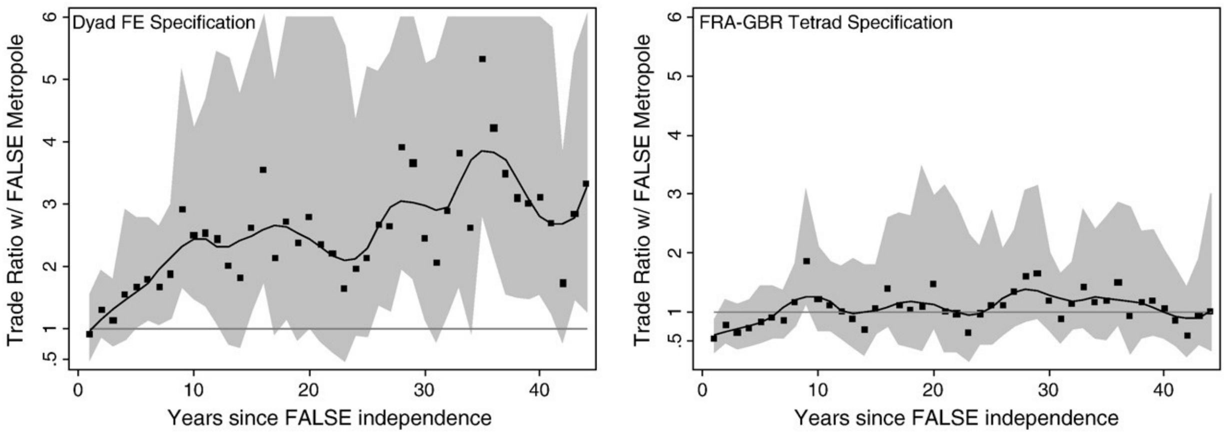

Fig. 5. Estimated independence effects for false colonial relationships.

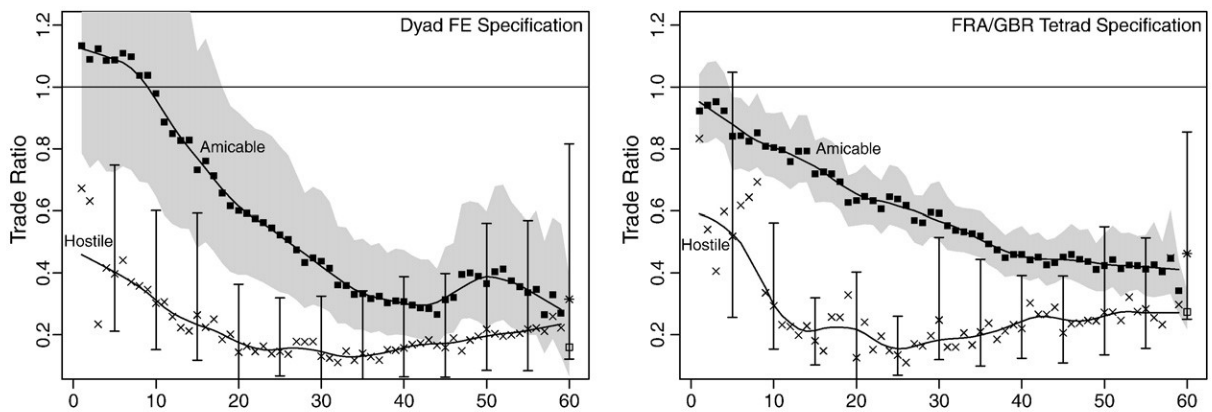

Fig. 6. Independence effects depend on type of separation.

To measure the impact of years of independence on trade with each type of country, we estimate distinct independence effects for a colony's trade with metropole, siblings, and ROW countries. For siblings, we allow trade deterioration to occur as soon as one sibling gains independence from its metropole and this deterioration is augmented when the other sibling separates. To illustrate, consider Senegal's and Algeria's separation from the French empire in 1960 and 1962, respectively. For the observation corresponding to Senegal–Algeria trade in 1964, the sibling dummy variable for four years since independence turns on (Senegal has been independent for four years) as does the sibling dummy variable for two years since independence (Algeria has been independent for two years). A similar procedure is applied to ROW trade. Consider the trade between Ghana and the Ivory Coast in 1965. This is ROW trade because the two countries are neither siblings nor colonies of one another. In 1965, Ghana had been independent for nine years and the Ivory Coast for five years and we code the two corresponding ROW dummy variables to turn on for that observation. In the case of colony trade with a metropole other than its own or a never-colonized country, only one ROW dummy variable turns on (the one corresponding to years since independence of the colony). The coding is complicated in cases where a country was colonized by more than one metropole. A complete description of the coding procedure is available at http://strategy.sauder.ubc.ca/head/sup/.

We cannot use the tetrad method to obtain separate effects for metropole, sibling, and ROW. This is because the sum of these dummy variables is collinear with the monadic effects. Thus, we confine the estimates to those generated in the DyadFE specification and the results are portrayed in Fig. 7. Adding the additional dummy variables steepens and deepens the profile of trade erosion with the metropole that we observed in the comparable specification of Fig. 4 (specification 4). For siblings we observe strong trade erosion as well. The decline in sibling trade occurs immediately but levels off a bit sooner than does metropole trade. The estimates for 60+ years post-independence are nearly identical for metropole and siblings, at

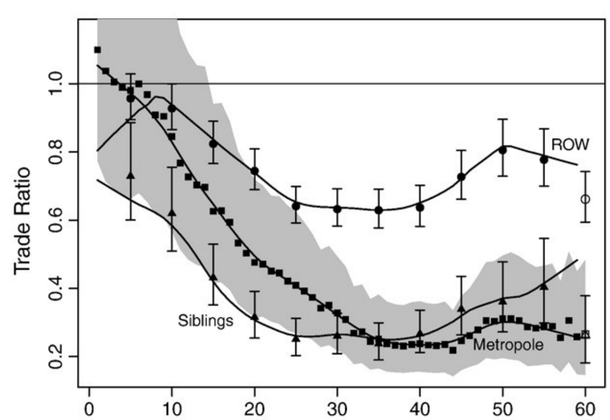

Fig. 7. Trade with metropole, "siblings", and rest-of-world after independence.

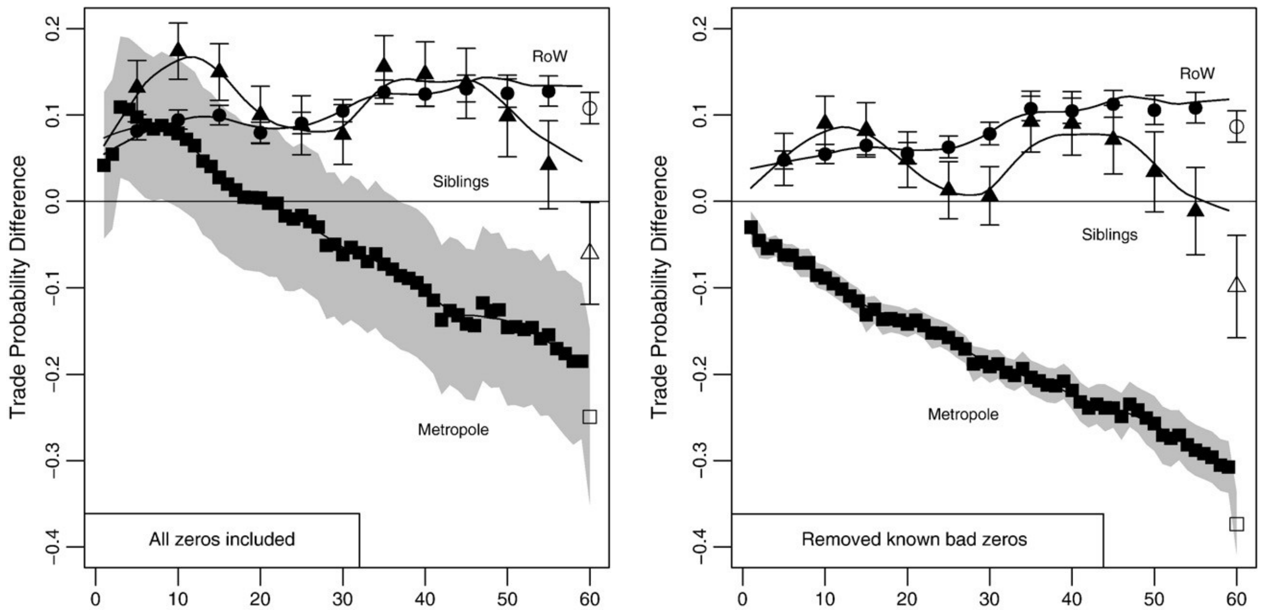

Fig. 8. Linear probability model estimates of independence effects on the extensive margin.

about 27% of pre-independence trade. None of the differences between the metropole and sibling effects throughout the years are statistically significant.

Interestingly, during the first 30 years after independence, colony trade with ROW declines by 20–30%. One potential explanation is that many former colonies adopted import-substitution measures.[25] After four decades, ROW trade rebounds slightly but remains significantly lower than prior to independence. In unreported regressions we find that amicable separations lead to gradual reductions in trade with ROW for 30–40 years before flattening out. The slight increase in ROW trade after 40 years seen in Fig. 7 is driven mainly by increasing, positive ROW effects associated with hostile separations.[26]

We interpret the gradual trade erosion observed between siblings as further evidence that the trade-enhancing “capital” (networks and institutions) associated with empires encouraged inter-sibling trade and that this capital depreciates after independence. Contrary to the hypothesis that empires acted as constraints on pre-independence trade diversification of colonies, we find that on average former colonies did not redirect trade to ROW countries. Indeed, countries that become independent on average trade *less with all countries*, with the decline most pronounced for trade with the former metropole and former siblings.

4.6. Changes on the extensive margin

With the exception of the Poisson PMLE estimates, we have examined effects of independence on exports conditional on flows being positive. These results can be interpreted as a decline in trade on the intensive margin. As discussed in Section 3.3, problems with incorrect zeros and choosing the left censor value make Tobit estimates extremely sensitive to assumptions. Despite the problematic zeros, we believe that it is instructive to see how independence affected the extensive margin of trade, i.e., the likelihood of a former colony realizing positive trade with its metropole, siblings, and ROW after independence. To estimate this probability, we code the positive trade flows as one and evaluate a binary dependent variable.

We estimate a linear probability model (LPM) where the dependent variable equals one if exports are positive. The LPM has the advantage of directly estimating the marginal effects of years since independence on the probability of positive trade.[27] LPM can be estimated using the within transformation to remove dyadic fixed effects, which is not possible in a probit model. Fixed effects logit estimation can accommodate dyadic fixed effects but it discards dyads where observations are either all positive or all zero. This could cause selection bias. Suppose colony–metropole trade flows are continuously positive before and after independence. This would suggest that independence had little effect on the extensive margin for this dyad. LPM takes this into account but fixed effects logit ignores this information.

We estimate with the full sample as well as with a restricted sample that eliminates incorrect zeros using the criteria described in Section 4.2. The estimates are graphed in Fig. 8. The left panel shows results for the full sample. We observe an initial increase in the probability of positive trade with the metropole, siblings, and ROW of about 10 percentage points. Because we are estimating with dyadic fixed effects, this probability is relative to the likelihood of positive trade flows in the colonial period. The increased probability of positive flows stays fairly steady for siblings and ROW but decreases steadily for metropole, becoming negative after about 20 years and continuing to decline thereafter. Estimates for the reduced sample, shown in the right panel, display metropole and sibling effects that are shifted downward. Now the independence effect for trade with metropole is always negative and the sibling effect becomes negative late in the sample. The reason for the shift is that our screening method disproportionately drops reported (and presumably false) zero trade flows for the pre-independence period. With fewer zeros initially, the relative likelihood of positive trade flows will fall.

These estimated independence effects on the extensive margin of trade should be interpreted with caution. As we discuss in detail in Appendix A, there are many reasons to distrust the many zero flows reported in DOTS. Unless the zeros are valid, the estimates are

[25] See Bruton (1998) for a survey and reconsideration of these policies.

[26] The estimated effects of independence on ROW trade for hostile separations are positive during the first 20 years of independence, negative for the next 15 years, and thereafter are positive. They are mostly insignificantly different from zero due to large standard errors. Figures showing independence effects on metropole, sibling, and ROW trade for amicable and hostile separations are available at http://strategy.sauder.ubc.ca/head/sup/.

[27] See Angrist and Pischke (2009, pp. 102–107, 197) for a full exposition of the arguments for using LPM instead of probit or logit.

unreliable. Nonetheless, these results suggest that trade with metropole eroded on both the intensive and extensive margin. However, trade with siblings and ROW, while decreasing on the intensive margin, exhibits small increases on the extensive margin. One explanation is that the metropole dictated trade with certain siblings and ROW countries but not others. Once free from colonial rule, colonies became began to trade with new countries, while, at the same time, reducing trade with traditional trading partners.

5. Conclusion

We find that independence erodes colonial trade with the metropole and other countries in the colonial empire. On average, trade between a colony and its metropole declines by about 65% during the first 40 years of independence. Trade between siblings falls by a similar amount. Hostile separations lead to more immediate negative reductions in trade than amicable separations but long-run trade deterioration is similar for both. Trade erosion is not confined to the colonial empire—we also find that trade with third countries falls after independence by about 20%. Our analysis of the extensive margin reveals large decreases in the likelihood of positive trade flows with the metropole and small increases in the propensity to trade with siblings and third countries.

In addition to controlling for a large number of covariates commonly used in gravity equations, our preferred specifications account for unobserved country and country-pair influences. The tetrad method we develop removes time-varying importer and exporter effects and dyadic effects to account for time-invariant bilateral influences. Our falsification exercise supports the hypothesis that declines in trade between colonizers and metropoles were caused by independence, rather than historical trends that happened to coincide with independence.

Non-parametric estimates portray a steady erosion of trade for four decades after independence. This time-profile is not what one would expect if former colonies used their independence to immediately terminate patterns of trade that had been imposed by the colonial power. It also does not support a reverse causation story whereby metropoles free colonies once colonial resources have been fully exploited. The most plausible interpretation of the pattern of trade reduction observed in the data is that it arises from the depreciation of trade-promoting capital embodied in institutions and networks of individuals with knowledge of trading opportunities.

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Appendix A. Data

A.1. Compiling trade flows from DOTS

The DOTS database often reports two values for the same flow from country *A* to *B*. This is because country *A* may report its imports from *B* and country *B* reports its exports to *A*. While some researchers simply take the average of those two values, it seems preferable to try and infer what is the most reliable source of data and drop information from the other source. Given the problematic zeros reported in the database (discussed in detail below), we consider as most reliable the larger value reported by the two countries. When using exporter reported trade, we adjust for the fact that exports are reported FOB while imports are reported CIF, with a 10% difference in value, which is the actual mean margin revealed by countries reporting imports in both CIF and FOB values.

In the compiled data set, 1% of the trade observations are valued at less than $500 and there are 42 cases of trade of one cent. The IMF documentation states that trade is recorded in millions with accuracy out to one or two decimal places, depending on the reporting country. Two decimal places would make the smallest value of trade $10,000. Accordingly, we round the data to the nearest $10,000—trade below $5000 becomes zero.

Scrutiny of the reported zeros in DOTs reveals many cases were reported zero trade are actually cases where the true trade value is likely positive. Evidence of incorrect zeros include:

- Trade between France and Vietnam, Laos, and Cambodia is recorded as zero between 1948 and 1953 or 1954 and then becomes positive thereafter. French exports to Vietnam jump from 0 to $132.9 million from 1954 to 1955 (1954 is the year of independence). United Nations (1955) reports that French exports to and imports from Indochina (Vietnam, Laos, and Cambodia) for the January to September period were $200.2 million and $21.7 million in 1953 and $164.4 million and $16.0 million in 1954.
- DOTS records zero trade between Russia and Ukraine, Kazakhstan, and Uzbekistan in 1992/3 and positive flows thereafter. Russian exports to the Ukraine jump from 0 to $5997.8 million from 1993 to 1994. The independence date is 1991 for this empire.
- We predicted trade using the OLS estimates corresponding to specification (1) in Table 2 and examined predicted trade for cases where actual trade was recorded as zero. 13% of the zeros were predicted to have trade in excess of $1 million. While some of these cases corresponded to special circumstances (the war between Iran and Iraq may have eliminated trade between them[28]), other cases, such as Iran's zero exports to Russia from 1981 to 1987 (exports for this dyad were missing from 1969 to 1980 and jumped to $106 million in 1988) suggest incorrect data.
- There are over 1200 cases where an exporting country or an importing country records all zero trade flows with every trading partner in a given year. These "no imports" or "no exports" represent over 70,000 observations in the data set. In about half of the no-trade cases, DOTS records zero trade with at least 70 trading partners.
- Gleditsch (2002) closely investigates DOTS data and states "On closer inspection, many of the trade flows of exactly zero in the DOT data seem problematic. To maintain a rectangular data structure, many missing observations appear to have been substituted with zeros. These structural zeros are probably better treated as missing observations rather than true zeros."

Falsely reporting missings as zeros biases analysis that utilizes information on zeros. In the case of French trade with Indochina, independence would correspond to a huge amount of trade creation. In the case of Russian trade with former Soviet block countries, trade would increase dramatically a few years after independence. Moreover, the incorrect zeros for trade within empires introduce positive bias in regressions estimating the effect of independence on the probability of realizing non-zero trade.

A.2. Gravity controls

GDPs and populations come from the World Bank's World Development Indicators (WDI). Note that in accordance to trade flows, GDPs are not deflated. Since the WDI excludes Taiwan, we use national data sources. WDI also starts in 1960 and sometimes does not keep track of countries that ceased to exist, or changed definitions.

[28] See Martin et al. (2008) for a quantification of the trade disrupting effects of military conflicts.

Typically, WDI has Russian GDP starting in 1989. In order to correct both problems, we complement WDI with population estimates provided by Angus Maddison (http://www.ggdc.net/maddison/Historical_Statistics/horizontal-file_10-2006.xls). Furthermore, we also use the 1948–1992 GDP estimates collected by Katherine Barbieri and made available by the Correlates of Warproject (http://www.correlatesofwar.org/).

RTAs are mainly constructed from three main sources: Table 3 of Baier and Bergstrand (2007) supplemented with the WTO web site (http://www.wto.org/english/tratop_e/region_e/summary_e.xls) and qualitative information contained in Frankel (1997). GATT/WTO membership of different countries over time comes from the WTO website. The data on currency unions are an updated and extended version of the list provided by Glick and Rose (2002). Data on common legal origins of the two countries are vailable from Andrei Shleifer at http://post.economics.harvard.edu/faculty/shleifer/Data/qgov_web.xls. Bilateral distances and common (official) language come from the CEPII distance database (http://www.cepii.fr/anglaisgraph/bdd/distances.htm). We use the population-weighted great circle distance between large cities of the two countries.

The ACP variable refers to a sequence of agreements conferring preferential treatment of imports from former colonies and some other developing countries (e.g. Liberia). Our ACP dummy is coded as one when an ACP country is included in the agreement and it exports to a member of the EC/EU. Both the ACP and EC/EU memberships grow over time, as shown below (obtained from http://ec.europa.eu/development/geographical/cotonou/lomegen/lomeevolution_en.cfm):

- Yaoundé I (1963): Benin, Burkina Faso, Burundi, Cameroon, Central African Republic, Chad, Congo (Brazzaville), Congo (Kinshasa), Côte d'Ivoire, Gabon, Madagascar, Mali, Mauritania, Niger, Rwanda, Senegal, Somalia, Togo
- Yaoundé II (1969): Kenya, Tanzania, Uganda
- Lomé I (1975): The Bahamas, Barbados, Botswana, Ethiopia, Fiji, Gambia, Ghana, Grenada, Guinea, Guinea-Bissau, Guyana, Jamaica, Lesotho, Liberia, Malawi, Mauritius, Nigeria, Samoa, Sierra Leone, Sudan, Swaziland, Tonga, Trinidad and Tobago, Zambia
- Lomé II (1979): Cape Verde, Comoros, Djibouti, Dominica, Kiribati, Papua New Guinea, Saint Lucia, Sao Tome and Principe, Seychelles, Solomon Islands, Suriname, Tuvalu
- Lomé III (1984): Angola, Antigua and Barbuda, Belize, Dominican Republic, Mozambique, Saint Kitts and Nevis, Saint Vincent and the Grenadines, Vanuatu, Zimbabwe
- Lomé IV (1990): Equatorial Guinea, Haiti
- Lomé IV revised (1995): Eritrea, Namibia, South Africa
- Cotonou (2000): Cook Islands, Marshall Islands, Federated States of Micronesia, Nauru, Niue, Palau

The chronology of EC/EU membership:

- EC6 (1957): Belgium, France, Germany, Italy, Luxembourg, The Netherlands
- EC9 (1973): Denmark, Ireland, United Kingdom
- EC10 (1981): Greece
- EC12 (1986): Portugal, Spain
- EU15 (1995): Austria, Finland, Sweden
- EU25 (2004): Czech Republic, Cyprus, Estonia, Latvia, Lithuania, Hungary, Malta, Poland, Slovenia, Slovakia

A.3. Independence events

Table A.4 lists the independence dates for each empire, denoting hostile separations with a †.

Table A.4
Metropoles, colonies, and independence events since 1900.

UK		Ghana	1957	Cambodia	1953	S Africa	1902†
Bermuda	–	Malaysia	1957	Syria	1946†	**Austria**	
Falklands	–	Sudan	1956	Lebanon	1943	Bosnia	1918†
Gibraltar	–	Eritrea	1952	**Russia**		Croatia	1918†
St Helena	–	Israel	1948	Armenia	1991	Czechia	1918†
Hong Kong	1997	Burma	1948	Azerbaijan	1991	Slovenia	1918†
Brunei	1984	Sri Lanka	1948	Belarus	1991	Slovakia	1918†
St Kitts	1983	Bangladesh	1947	Estonia	1991	**Japan**	
Antigua	1981	India	1947	Georgia	1991	Korea, N	1945†
Belize	1981	Pakistan	1947	Kazakhstan	1991	Korea, S	1945†
Vanuatu	1980	Jordan	1946	Kyrgyzstan	1991	Palau	1945†
Zimbabwe	1980	Iraq	1932	Latvia	1991	Taiwan	1945†
Kiribati	1979	Egypt	1922	Moldova	1991	**Belgium**	
St Vincent	1979	Ireland	1921†	Tajikistan	1991	Burundi	1962
St. Lucia	1979	Afghanistan	1919†	Turkmenistan	1991	Rwanda	1962
Dominica	1978	S Africa	1910	Ukraine	1991	Zaire	1960
Solomon Is.	1978	N Zealand	1907	Uzbekistan	1991	**Denmark**	
Tuvalu	1978	Australia	1901	Lithuania	1990	Faroe Is	–
Seychelles	1976	Papua	1901	Finland	1917	Greenland	–
Grenada	1974	**France**		**Turkey**		Iceland	1944
Bahamas	1973	F Guiana	–	Cyprus	–	**Italy**	
Bahrain	1971	F Polynesia	–	Armenia	1920†	Somalia	1960
Qatar	1971	Guadeloupe	–	Lebanon	1920†	Libya	1951
UAE	1971	Martinique	–	Yemen	1918	Eritrea	1941†
Fiji	1970	N Caledonia	–	Syria	1917†	**Australia**	
Tonga	1970	Reunion	–	Iraq	1916†	Papua	1975
Mauritius	1968	St Pierre	–	Albania	1912	Nauru	1968
Nauru	1968	Vanuatu	1980	Macedonia	1912†	**USA**	
Swaziland	1968	Djibouti	1977	Libya	1911†	Palau	1994
Yemen	1967	Comoros	1975	**Germany**		Philippines	1946
Barbados	1966	Algeria	1962†	Burundi	1918†	Marshall Is.	1986
Botswana	1966	Benin	1960	Namibia	1918†	**Yugoslavia**	
Guyana	1966	Burkina Faso	1960	Poland	1918†	Bosnia	1995†
Lesotho	1966	Cameroon	1960	Rwanda	1918†	Slovenia	1991†
Gambia	1965	C African Rep	1960	Papua	1915†	**China**	
Maldives	1965	Chad	1960	Nauru	1914†	Mongolia	1921†
Malawi	1964	Congo	1960	Palau	1914†	**Ethiopia**	
Malta	1964	Cote D'Ivoire	1960	Samoa	1914	Eritrea	1993†
Tanzania	1964	Gabon	1960	**Portugal**		**Greece**	
Zambia	1964	Madagascar	1960	Macao	1999	Cyprus	–
Kenya	1963	Mali	1960	Angola	1975†	**N Zealand**	
Singapore	1963	Mauritania	1960	Cape Verde	1975	Samoa	1962
Jamaica	1962	Niger	1960	Mozambique	1975	**Pakistan**	
Trinidad	1962	Senegal	1960	Sao Tome	1975	Bangladesh	1971†
Uganda	1962	Togo	1960	Guinea-Bissau	1974	**S Africa**	
Kuwait	1961	Guinea	1958	**Netherlands**		Namibia	1990†
Sierra Leone	1961	Morocco	1956	Aruba	–	**Spain**	
Cyprus	1960	Tunisia	1956	N Antilles	–	Eq Guinea	1968
Nigeria	1960	Laos	1954†	Suriname	1975		
Somalia	1960	Viet Nam	1954†	Indonesia	1949†		

Note: **Metropole** = colonizer, – = current colony, **†** = hostile separation.

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