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Evidence from a flexible empirical implementation

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A Democratic Dividend in Trade? Evidence from a Flexible Empirical Implementation*

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Abstract: We study the causal effect of country-specific democratic regime change on bilateral trade flows, extending standard structural gravity empirics to 'heterogeneous gravity' estimated at the country-pair level. Our original difference-in-differences implementation accounts for selection into regime change, multilateral resistance, globalisation effects, and spatial dependence. We initially find average effects of 46% higher exports for countries after thirty years in democracy, but demonstrate that these effects are driven by the democratic dividend for income: the causal chain runs from democracy to economic prosperity to trade, and democracy appears to have a limited 'direct' effect on trade flows.

Keywords: trade gravity model, democratic regime change, monadic variables, heterogeneity, panel data, interactive fixed effects

JEL codes: P16, F13, F14, C23

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1 Introduction

Economists generally agree on the importance of good institutions for economic prosperity and the causal effect of becoming a democracy — a 'bundle' of institutions — on long-run income per capita is suggested to be on the order of 20-30% (Acemoglu et al. 2019, Boese-Schlosser & Eberhardt 2024).[1] Similarly, the relevance of trade for economic development is widely recognised among academics and policymakers alike: trade flows are associated with efficiency gains, technology transfer, and increased innovation activities (Costinot & Rodríguez-Clare 2014, Feyrer 2019). To the uninitiated, it may therefore come as a surprise that the literature on democratic institutions in their effect on bilateral trade flows is quite sparse. Furthermore, the effects of other important *country-specific* policies or characteristics (e.g. MFN tariffs, exchange rate arrangements) on bilateral trade are also largely unexplored.

Counter-intuitively, the solution to this puzzle relates to the significant progress made in the literature: the structural gravity model[2] has been considerably strengthened by theoretical work deriving it from a wide class of microeconomic principles (Arkolakis et al. 2012, Costinot & Rodríguez-Clare 2014, Allen et al. 2020) and by contributions to the empirical methodology (Anderson & Van Wincoop 2003, Santos Silva & Tenreyro 2006, Baier & Bergstrand 2007).[3] The empirical path taken by the gravity literature to achieve credibility and rigour is also its demonstrable weakness, an empirical straitjacket of sorts: identification can be achieved by focusing on the narrow set of *dyadic* variables, with the analysis of country-specific (*monadic*) ones largely ignored due to adherence to the state-of-the-art of the structural gravity model. Exceptions include Heid et al. (2021) and Beverelli et al. (2024), who seek alternative means to identify monadic variables within the structural gravity model, involving intra-national sales. The merit of their analysis hinges on the assumption that regime change affects domestic and international sales differentially and that quantifying this difference is economically meaningful, rather than establishing whether 'democracy causes higher trade' (like we do). Other work has constructed dyadic democracy variables to get around the identification problem (e.g. Álvarez et al. 2018) but cannot speak to the impact of *country-specific* regime change. A substantial number of papers simply ignore the 'prescribed' set of fixed effects in their analysis of monadic variables (e.g. Dutt & Traca 2010, Yu 2010). Finally, some studies (e.g. Eaton & Kortum 2002, Head & Ries 2008) argue that country-specific effects could be extracted in

[1]These studies counter widespread scepticism about a positive 'democratic dividend': allowing voters to remove an incumbent government through the power of the electoral process could drive up consumption and reduce the investment rate to the detriment of economic growth (e.g. Baum & Lake 2003, 334f). Studying China or the East Asian 'Tigers' suggests that democracy is not a necessary condition for development, but this form of cherry-picking of autocratic success stories ignores the established link between autocracy and volatility of economic performance (Besley & Kudamatsu 2008, Persson & Tabellini 2009, Imam & Temple 2024).

[2]For recent surveys see Head & Mayer (2014), Yotov et al. (2016), and Yotov (2024).

[3]These in effect prescribe what the gravity specification should look like, and how it should be estimated to address a range of econometric challenges, including the global network aspect of trade.

a second-step regression from the fixed effects of a structural gravity model. This approach has been undermined by the finding that in the Poisson Pseudo Maximum Likelihood (PPML) model favoured in the literature these fixed effects have a clear theoretical interpretation which rules out any role for monadic variables in second-step analysis (Fally 2015, Beverelli et al. 2024).[4]

Within the political theory of trade, a 'direct democracy' model suggests that democratic regimes should act, under majority rule, in agreement with the trade preferences of the median voter (Mayer 1984, Dutt & Mitra 2002). In a standard Hecksher-Ohlin model, assuming that countries undergoing democratisation are labour-abundant, capital-poor workers gain from trade liberalisation. Extension of the franchise implies that the median voter is a worker and therefore greater trade openness is adopted by the newly-elected government. A 'political support' or 'protection for sale' approach (Grossman & Helpman 1994, Rodrik 1995) would also allow existing interest groups (e.g. protectionist old-guard political and economic elites) to lobby for their preferred trade policies but their political influence is likely to be substantially eroded in the newly-democratised regime (Milner & Mukherjee 2009). In addition, democratisation can have indirect economic effects by promoting legal and economic reforms which support all businesses, including trading firms (Rode & Gwartney 2012, Giuliano et al. 2013, Yue & Zhou 2018). The empirical literature on the effects of democratisation on trade is relatively scarce (and deviates from the 'prescribed' model specification of structural gravity) but, overall, finds a positive impact of 'more' democracy on the imports and exports of (capital-poor) developing countries (Milner & Kubota 2005, Eichengreen & Leblang 2008, Kono 2008, Tavares 2008, Aidt & Gassebner 2010, Yu 2010).

In this paper, we propose an estimation strategy to break the empirical straitjacket and study the trade effects of democratic regime change: we investigate the gravity relationship at the country-pair level, accounting for the manifestation of the trade network as well as regime change endogeneity.[5] Our implementation enables us to investigate the effect of binary *monadic* variables on bilateral trade flows. We adopt a common factor framework to capture multilateral resistance and other unobserved time-varying heterogeneity, treating the factors as nuisance terms in the same way the pooled gravity model employs a myriad of fixed effects.[6] Following Boneva & Linton (2017) we use cross-section averages (CA) to proxy common factors (an approach pioneered by Pesaran 2006), in combination with the spirit of a heterogeneous treatment effects estimator by Chan & Kwok (2022): we construct

[4]Freeman et al. (2025) recently suggested a two-step approach relying on the theoretical properties of Fally (2015) to calculate the multilateral resistances. Their approach relies on assumptions about the relationship between the country-specific variables and output/expenditure in the structural gravity model.

[5]Democratic regime change is treated as endogenous given the link between economic integration and institutional change established in the literature (e.g. Acemoglu et al. 2005, Nunn & Trefler 2014, Puga & Trefler 2014).

[6]The common factor framework is popular in the cross-country empirical literature to capture total factor productivity and other unobserved heterogeneity (e.g. Eberhardt et al. 2013, Chirinko & Mallick 2017, De Visscher et al. 2020). Our implementation relates to existing work modelling multilateral resistance in the gravity equation using common factors (e.g. Serlenga & Shin 2007, Chen et al. 2021), though these implementations cannot capture the effect of monadic covariates.

the CAs from 'economic mass' (GDP and population) in the samples of 'never-treated' exporters and importers, respectively, and conduct a diagnostic test whether the 'information' they capture is equally relevant for the treated sample.[7] We estimate CA-augmented treatment equations for the bilateral trade flows of countries that experienced regime change. We add a further correction to address the practical problem that in some country-pairs where the exporter or importer *did* experience regime change no trade *ever* takes place between the countries.[8] Implementation is via PPML at the pair-level to address concerns over consistency and truncation. Our approach enables us to causally identify binary monadic variables since we capture time-varying unobservables with the CA-augmentation.[9]

The economic interpretation of our estimates crucially depends on our treatment of exporter and importer income as additional covariates: (i) if we exclude these, our model identifies the 'total' effect of democracy on trade, but we cannot determine whether this is a direct effect or the outcome of increased income following regime change (an 'indirect effect' on trade); (ii) if, in line with structural gravity theory, we include these terms or use insights from theory to impose the income terms on the bilateral flow dependent variable, we identify the (controlled) 'direct effect' on trade (Imai et al. 2010, Celli 2022).[10]

We study the export flows of 84 countries which during 1950-2014 experienced 111 democratic regime changes and 55 reversals to autocracy.[11] Presenting our results in 5-year treatment intervals, we find that the 'total' effect of regime change on trade is substantial: the average treatment effect after thirty years in democracy amounts to a 46% increase in exports (65% for countries which experienced just a single regime change episode). In line with recent results in the democracy-growth literature, export effects are lacklustre for countries with fewer than a decade and a half in democracy: insignificant or even negative-significant.[12] Our results further show differences across *importer* regime types, whereby the variance of the effect for *autocratic* destinations is substantially higher. However, once we re-specify our model in line with structural gravity theory, the large longer-term effects effectively disappear, whether we include income terms or account for these by adjusting the dependent variable: the causal chain is suggested to run from democracy to higher

[7]Pooled difference-in-differences estimators rely on the assumption of parallel trends before treatment. The Chan & Kwok (2022) estimator allows for non-parallel trends but requires the weaker assumption of equal average factor loadings on the common factors between treated and control samples — see Section 2.

[8]We cannot estimate our gravity regression for such pairs, yet simply ignoring them would induce selection bias.

[9]Like in a fixed effects model where the variable of interest is endogenous to time-invariant country effects, the solution to the identification problem is to include proxies for the unobserved heterogeneity: multiple common factors with country pair-specific coefficients.

[10]The model with GDP *imposed* on trade flows can alternatively be motivated to address endogeneity concerns about trade influencing income (Irwin 2025) or income acting as a 'bad control' by being correlated with an omitted variable itself correlated with trade (e.g. Cinelli et al. 2024, Figure 9).

[11]These are countries which switched between autocracy to democracy at least once. 53 other countries remained autocratic throughout the sample period (control samples), while 28 countries were democratic throughout.

[12]These patterns can be rationalised by the upheaval following regime change and/or 'democratic overload' (see Gerring et al. 2005, Cervellati & Sunde 2014, Boese-Schlosser & Eberhardt 2024).

income to increased exports, with direct effects from democracy to exports quantitatively limited. The same pattern of results prevails if we study the effects of regime change on imports. We conduct a range of robustness checks including alternative cross-section average augmentations, alternative definitions of regime change, and adopting an alternative dataset which distinguishes between foreign and domestic trade, but our findings remain qualitatively unchanged.

Our study makes two important contributions to the literature: first, we introduce a simple but powerful empirical implementation of the gravity model which allows for the identification of binary monadic variables. Second, we study the relationship between country-specific democratic regime change and bilateral trade flows, contributing to our understanding of the mechanisms and causal chain of effects underlying the sizeable democratic dividend for long-run economic prosperity. We do not find evidence for democracies exhibiting political preferences for trade openness. Our channel of influence is purely economic: a country adopting democracy becomes richer and therefore also trades more.

2 Methodology

In this section, we introduce our 'PPML-CCE-DID' estimator. We begin by detailing its important elements and the literatures these originate from. We then introduce the estimation equations, assumptions, implementation and inference. A more detailed exposition is provided in Appendix D. The final section introduces a specification test adapted from Chan & Kwok (2022).

2.1 A Multi-Faceted Implementation

Our difference-in-differences implementation to capture binary monadic variables in a factor-augmented heterogeneous gravity model has the following conceptual ingredients: (i) We build on the literature for heterogeneous parameter models (Pesaran & Smith 1995) and estimate the gravity model at the country pair-level. (ii) We can do so because we treat multilateral resistance as unobserved time-varying heterogeneity and capture this in a common factor model (Pesaran 2006, Bai 2009). (iii) We follow the gravity literature in estimating a generalised linear (PPML) model to address concerns over heteroskedastic errors and zero trade flows (Santos Silva & Tenreyro 2006), but introduce a variant estimated at the pair-level and embedded in a common factor framework using cross-section averages (CAs) as factor proxies (Boneva & Linton 2017). (iv) Specifying democracy as a binary 'treatment' variable, we borrow from the treatment effects literature adopting common factors (Gobillon & Magnac 2016, Chan & Kwok 2022) and estimate the gravity model at the pair-level for exporters i or destinations j which experienced regime change during the sample period (treated sample). We use the 'never-treated' exporters and importers (control sample) to construct the factor proxies (cross-section averages) for causal identification of endogenous regime change, mimicking the Chan & Kwok (2022) strategy.[13] We further adapt their 'weak parallel trend test' to the three-way panel to test the identifying assumption of expected factor-loading equality between treated and control samples. (v) We cannot estimate a pairwise regression for treated exporters or importers if the trade flow remains zero in all time periods as the PPML estimator does not exist in this case (Santos Silva & Tenreyro 2010). We address this selection problem by including factor proxies from the zero-trade pairs in the treatment regressions. (vi) Finally, the inclusion or exclusion of exporter and destination income poses econometric difficulties (if included) or concerns over the theory-consistency and hence interpretation of our empirical model (if excluded) — we address this issue by providing an alternative specification where the income terms are imposed with unit elasticity (as motivated by the theoretical literature) and hence they do not pose a challenge to identification of the treatment effect.

2.2 Estimation and Inference

Using PPML, we estimate the following equation at the pair-level for exporters i and destinations j, either or both of which experienced democratic regime change:

$$\begin{aligned} \mu_{ijt} \quad = \quad & exp\Big[\alpha_{ij} + \gamma^1_{ij}\,\text{FTA}_{ijt} + \gamma^2_{ij}\,\text{Common Currency}_{ijt} + \theta_{ij}D_{it} + \eta_{ij}D_{jt} \\ & +\delta^i_{ij}\,\overline{ln(Y)}^i_t + \kappa^i_{ij}\,\overline{ln(Pop)}^i_t + \delta^j_{ij}\,\overline{ln(Y)}^j_t + \kappa^j_{ij}\,\overline{ln(Pop)}^j_t \\ & +\delta^{i0}_{ij}\,\overline{ln(Y)}^{i0}_t + \kappa^{i0}_{ij}\,\overline{ln(Pop)}^{i0}_t + \delta^{j0}_{ij}\,\overline{ln(Y)}^{j0}_t + \kappa^{j0}_{ij}\,\overline{ln(Pop)}^{j0}_t \\ & +\psi^i_{ij} ln(Y/L)_{it} + \psi^j_{ij} ln(Y/L)_{jt} + \epsilon_{ijt}\Big] \qquad \forall\, ij,\, j \neq i. \end{aligned} \tag{1}$$

The dependent variable represents the exports from i to j at time t. Our parameters of interest are θ_{ij} and η_{ij} which relate to the binary regime change indicators for exporters D_{it} and destinations D_{jt}.[14] They capture the individual treatment effects averaged over the treatment period (Chan & Kwok 2022, individual treatment effects on the treated). FTA and Common Currency are dyadic controls. The second line represents the cross-section averages at time t ($\overline{ln(X)}^i_t = N^{-1}\sum_i ln(X)_{it}$) of the 'economic mass' variables employed in the gravity model (GDP, population; in logs). These averages are computed over all exporters i (first two terms) and all destinations j (second two terms) which remained autocratic, respectively — since we do not observe bilateral trade relations for *all* dyads (e.g. A exports to B, but B not to A), averages over i and j differ from each other. This setup

[13] Unlike in pooled difference-in-differences models, years for countries which are 'not yet' treated *never* enter the control sample. This also bypasses concerns over negative weights in pooled difference-in-differences models (De Chaisemartin & d'Haultfœuille 2020). More broadly, our methodology shares many features with the estimator proposed by Wooldridge (2023) for staggered difference-in-differences settings.

[14] The recent literature on democracy and growth prefers *binary* democracy indicators (Papaioannou & Siourounis 2008, Acemoglu et al. 2019, Boese-Schlosser & Eberhardt 2024).

controls, *inter alia*, for the multilateral resistance terms, which are functions of output/expenditures (captured by the CAs) and bilateral trade costs (captured by the bilateral factor loadings).[15] The third line includes more cross-section averages, computed from GDP and population for exporters i which experienced regime change but for which trade flows to *some* destinations remained zero in all years and similarly for destinations j.[16] In the construction of these CAs we average the GDP and population data of exporters i (importers j) over the zero-trade partners.[17]

The last line of equation (1) refers to the exporter and destination income terms: if we exclude these, we study the 'total' effect of regime change on trade but cannot determine whether democracy causes trade or trade increases because democracy causes higher income. If we include them, we condition on income and hence identify the 'direct' effect of democracy on trade — we include GDP or per capita GDP terms, with results qualitatively identical. Alternatively, we adjust the dependent variable accounting for the unit elasticity of these income terms (see Head & Mayer 2014): $\mu^*_{ijt} \equiv \mu_{ijt}/(Y_{jt}Y_{it})$. ϵ_{ijt} is the error term.

Assumptions Since, by construction, the cross-section averages can have different coefficients across country-pairs, the 'parallel trend' assumption of standard pooled Difference-in-Differences models does not apply — however, a weaker assumption of common expected factor loadings between treated and control samples is required for identification (see next section). The above setup can accommodate endogeneity of treatment D_{it} in terms of selection into treatment and the timing of treatment: D_{it} can be correlated, *inter alia*, with any parameters on the CA terms or with the dyadic controls. The main assumption we require for consistency of $\hat{\theta}_{ij}$ is that we capture all unobserved time-varying heterogeneity with our low-dimensional factor structure (Pesaran 2006, Bai 2009, Athey et al. 2021), hence, that ϵ_{ijt} is orthogonal to all elements of equation (1).[18] Remaining threats to identification arise from idiosyncratic country 'shocks' which affect trade and may simultaneously trigger democratic regime change: for instance, a financial crisis or a natural resource discovery. Existing research suggests that financial crises have sizeable international dimensions (Cesa-Bianchi et al. 2019), while oil exploration is driven by global prices (Arellano et al. 2017) — both examples of common factors.

Post-Estimation and Inference We average treatment estimates $\hat{\theta}_{ij}$ to yield ATETs (Appendix Table B-1) or relate them to treatment length (Figure 1), using an M-estimator (Rousseeuw & Leroy

[15] With a perfectly balanced panel of bilateral trade between all dyads, the averages over exporters and importers would be identical, consistent with symmetric multilateral resistances for exports and imports if trade costs are symmetric and expenditures equal output for each country (Anderson & Van Wincoop 2003).

[16] In Appendix Figure C-1, Panels (e) and (f), we illustrate the upward bias of our results if we ignore this selection problem by omitting the third row of terms in equation (1).

[17] Say Country A experiences regime change and has 100 trade partners. For 95 we observe non-zero export flows during the sample period, but for 5 exports remains zero throughout. The CA terms with superscript $i0$ then include five instances of Country A's GDP and population.

[18] In robustness checks we consider alternative CA-augmentations with qualitatively identical results.

2005) to reduce the impact of outliers. These are Mean Group estimates: $\hat{\theta}^{MG} = N^{-1} \sum_i \omega_{ij} \hat{\theta}_{ij}$ (Pesaran & Smith 1995) with granular weights ω_{ij} and N the number of pairwise estimates. Our nonparametric variance estimator is $\widehat{\text{var}}(\hat{\theta}^{MG}) = [N(N-1)]^{-1} \sum_{i=1}^{N} (\hat{\theta}_{ij} - \hat{\theta}^{MG})^2$ (Pesaran 2006).

2.3 Testing the weak parallel trend assumption

The reliability of standard difference-in-differences estimators lives and dies by the validity of the 'parallel trend' assumption: if the soon-to-be-treated units were already on a different trajectory to the control units before treatment, then the standard difference-in-differences estimates are biased. TheChan & Kwok (2022) PCDID estimator allows for non-parallel trends across panel units, most importantly between countries in treated and control samples. Nevertheless, it requires a weaker form of the parallel trend test for validity: the factor loadings on the common factors in the treatment equation *on average* have to be equal those in the control equation. In practice, this implies that the estimated parameters on the factor proxies in the treatment equation statistically should not differ from 1. The intuition of this 'Alpha test' is to ask whether the information we extract from the control sample is sufficiently relevant in the treatment sample: imagine a world in which democratic regime change only happens in rich countries, whereas poor countries always stay autocratic. The 'information' contained in the factor proxies constructed from the poor-country control sample is then likely to be quite uninformative about the unobservables driving outcomes in the rich-country treated sample: countries at different ends of the income scale have different economic structures, financial systems, etc. — we would be proxying apples with oranges. Passing the Alpha test assures us that the unobservables are *on average the same* across treatment and control samples, that we are adopting apples to proxy for apples, or oranges for oranges.

In our setup using CAs we cannot use Chan & Kwok's 2022 testing strategy, which employs the cross-section average of the residuals from auxiliary regressions in the control sample. We devise an alternative Alpha test for the heterogeneous trade flow regressions augmented with cross-section averages by testing whether the averaged coefficients on the CAs, $\bar{\hat{\delta}}^i$, $\bar{\hat{\kappa}}^i$ and $\bar{\hat{\delta}}^j$, $\bar{\hat{\kappa}}^j$ from equation (1) for exporters and destinations, respectively sum to 1. If the null of equal average factor loadings between treatment and control samples is rejected, the treatment regression may be misspecified and hence deliver biased All Alpha test results are reported in Appendix Table B-2.

3 Data

We use bilateral trade flows alongside exporter and importer GDP and Population from TRADHIST (Fouquin & Hugot 2016, version 4), combined with CEPII data for FTAs and currency unions.[19] For the export flow data we code the 'plausible zeros' as zero. Our panel covers 1950-2014. We also use

[19]All monetary values are in nominal British Pounds.

data from Head & Mayer (2021) for 1960-2018 in robustness checks.

Data on democracy is taken from the Varieties of Democracy (V-Dem) project (Coppedge et al. 2021): motivated by Mukand & Rodrik (2020) we focus on the liberal democracy index. In a robustness check, we adopt the democracy definition of Acemoglu et al. (2019). We discard country pairs with fewer than 14 observations (0.1%) — this is by the necessity of the demands of our heterogeneous panel estimator.

We adopt the full sample mean for liberal democracy over 1950-2014 (0.34) as the threshold for democratic regime change. In robustness checks, we add/subtract a quarter or half a sample standard deviation (0.28) to this mean index value to adopt a tighter/looser definition of regime change. The baseline liberal democracy sample includes 822,946 observations for 17,291 country pairs (average $T = 48$ years).[20] Of these, 8,577 pairs are for regime changes in the exporter country, relating to 84 countries which trade with up to 164 destinations.[21] For our baseline specification, we provide detailed distributions of regime change events and time spent in democracy in Appendix A. These suggest that the thousands of treatment estimates are relatively uniformly distributed across time spent in democracy.

4 Results

4.1 Main Results

Baseline Results: Export Effects Figure 1 presents the treatment effects by years spent in democracy (5-year intervals) and Appendix Tables B-1 and B-2 the equivalent ATETs alongside Alpha test results — all specifications (unless discussed) satisfy the Alpha specification test. The results presented are average treatment estimates for exporters, using the specification including dyadic controls.[22] In each plot, we report robust mean estimates (diamonds), associated 95% confidence intervals (CI, coloured bars), and the number of country pairs. We plot full sample results and distinguish by destination regime status (always autocratic or always democratic).[23]

Focusing on the full sample results (with blue CI) in Figure 1 panel (a) we can see volatile effects in the first 15 years, followed by treatment effects of .12, .20 and .37 (13%, 22%, and 46%

[20] Our control sample for exporters (destinations) includes 343,186 (346,567) observations for 8,461 (8,474) pairs of 62 countries which never democratised. Finally, a total of 2,200 country pairs (74,561 observations) have zero trade flows in all years even if exporter or destination experienced regime change.

[21] Our results below only include 81 exporters (3 only reverted to autocracy) and 7,420 pairs (we exclude those pairwise estimates with zero weights in our M-estimator). For the analysis of regime change *in a destination* the results are presented for 7,387 pairs in 81 countries.

[22] Excluding dyadic controls for FTAs and common currency unions yields qualitatively identical results (Appendix Figure C-4). For the specification in panel (a) the outlier-robust mean coefficient on FTA is 0.15 ($t = 6.23$, $N = 1,346$) for democratising exporters, suggesting 16% higher exports. The robust mean for common currency unions is -0.14 ($t = -1.32$, $N = 220$) — most of these estimates (note the small sample size) are for regional African monetary unions.

[23] We exclude estimates for destinations which themselves experienced regime change for the conceptual reason that these conflate the effects when destinations are autocracies and democracies in a non-trivial manner and for the practical reason that their variability hampers ease of presentation. Their ATETs are reported in Appendix Table B-1.

higher exports) for countries which spent 16-20, 21-25 and 26-30 years in democracy, respectively. The effect for countries which have been democratic for over 30 years is attenuated, just over .2 (22%). Mean estimates for the subsample of results where destinations are always democratic (teal-coloured CI) closely match the pattern described, whereas for the subsample of autocratic destinations (see-through CI) we observe much larger confidence intervals, which lead to mostly statistically insignificant results. In panel (b) we limit results to exporters which experienced regime change *once*, which leads to rising trade effects with more time spent in democracy (average of .5 or 65% higher exports after 30 years). The patterns of subsample results follow those described above.

These results indicate that democratic regime change *ultimately* leads to an increase in exports of 46-65% after 30 years in democracy. But these 'total effects' cannot speak to the causal chain of effects from democracy to trade, which we investigate next.

Theory-Consistent Specifications The theory-consistent structural gravity model includes income terms for exporter and importer countries. We first discuss our results including income terms (here, GDP pc, in an Appendix, GDP) and then impose GDP on trade flows with a unit coefficient.

Panel (c) of Figure 1 reports the treatment effects for all exporters which democratised. We can see that compared with the results in panel (a) the overall effects are substantially attenuated for all treatment lengths: treatment effects for all country pairs are mostly statistically insignificant or of modest magnitude if significantly different from zero. With minor deviations, this finding holds for the results in panel (d) where we again look at exporters which only experienced democratic regime change once. Conditioning on economic development, we can suggest that the direct effect of democratic regime change is small and that the causal chain runs from democracy to higher income to higher trade. Our results are virtually identical if we condition on exporter and destination GDP rather than *per capita* GDP (Appendix Figure C-4).

Alternatively, structural gravity theory tells us that the income terms in a structural gravity model are theoretically predicted to have unit elasticities. We therefore proceed to adjust our trade flow dependent variable by accounting for economic mass, $\mu_{ijt}/(Y_{jt}Y_{it})$, and drop the income terms from the estimation equation. Panels (e) and (f) present the full results and those for countries with just a single regime change event for this specification. In line with the conclusion for specifications in panels (c) and (d) we find substantially attenuated and largely statistically insignificant treatment effects of regime change on trade flows.[24] It should be noted that the cross-section averages capturing the unobserved time-varying heterogeneity in *destination* countries which never democratised fail the Alpha test in this specification ($p = .019$) whereas those for *exporter* countries do not ($p = .579$). We suggest that limited country coverage in the early years of the control sample[25] may be the

[24] In Appendix Table B-1 we report the ATET *per year in democracy* using robust regression.

[25] The ratio of control to treated observations in each year is as low as .38 in the 1950s but from the 1960s never drops below .5.

cause of this. Analysis of a reduced sample from 1960-2014 provides qualitatively identical results (Appendix Figure C-6), but the specification with economic mass imposed now also 'passes' the Alpha test ($p = .473$ and $p = .315$ for exporter and destination CAs, respectively).

Our findings thus support the argument of a causal chain from democracy to economic prosperity to trade, whereas direct trade effects of democratic regime change appear to be limited.

Import Effects Our model allows us to quantify how export volumes are affected if a *destination* country experiences regime change (i.e. the import effect of regime change). In Figure 2 we present result plots for the same specifications as in our previous analysis. Full sample results for *all exporters* are presented with CI in marigold, the subsample results are now for *exporters* which are always democratic (CI in pink) or always autocratic (CI in grey). Baseline results in panels (a) and (b) are qualitatively very similar to the export results in Figure 1, including for destination sub-samples, albeit with commonly larger magnitudes and the absence of a negative effect for countries with 11-15 years in democracy. Crucially, the results speaking to theory-consistent gravity estimation in panels (c) to (f) again display substantially attenuated treatment effects.

4.2 Robustness Checks and Variations

For the robustness checks we limit presentation to the specifications including the income terms, results for those with income *imposed* on trade are qualitatively identical (available on request).

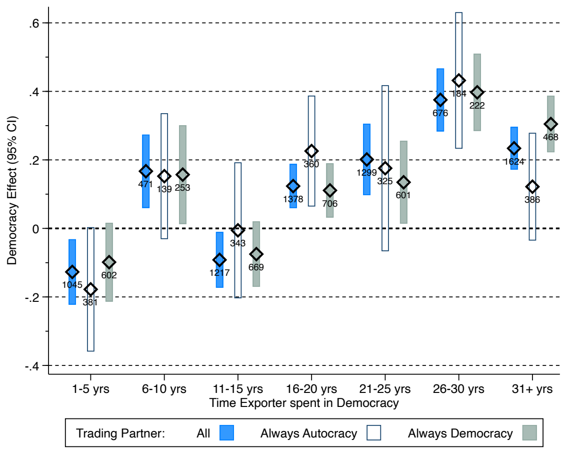
Alternative CA specifications Above results are based on empirical specifications incorporating cross-section averages (CAs) of GDP and Population for exporters and for destinations. In Appendix Figure C-1 we adopt alternative models (i) limiting CAs to those for GDP only in panels (a) and (b), and (ii) adding CAs for country total exports to those for GDP and population in panels (c) and (d).[26] Note that the specification with income and extended CA in panel (d) fails the Alpha test. Result patterns for baseline and theory-consistent models are qualitatively very similar to those in panels (a) and (c) of Figure 1.

Alternative Definitions of Democracy All of the above results are based on taking an arbitrary mean cut-off of the liberal democracy index to dichotomise regimes. As a robustness check, we still use the 1950-2014 V-Dem index mean but add or subtract $1/4$ or $1/2$ of its standard deviation to define regime change. Results in Appendix Figure C-2 panels (c), (e), (g) and (i) present baseline results for more liberal or more conservative definitions of democracy. Across all specifications, trade effects are (eventually) rising with years in democracy. The accompanying panels (d), (f), (h), and (j) present results for theory-consistent specifications including the income terms. Across all

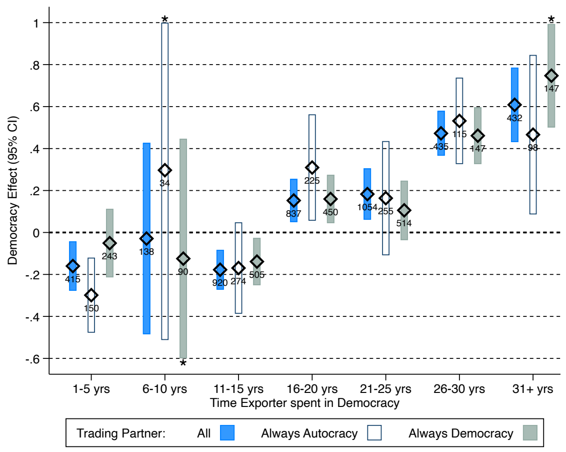
[26] These setups are motivated by (i) the assumption that the span of the CAs encompasses the unobserved factors (Boneva & Linton 2017), and (ii) the insight that too many CAs may be harmful (Juodis 2022).

Figure 1: Exporter Democratic Regime Change and Trade Flows — PPML-CCE-DID Results

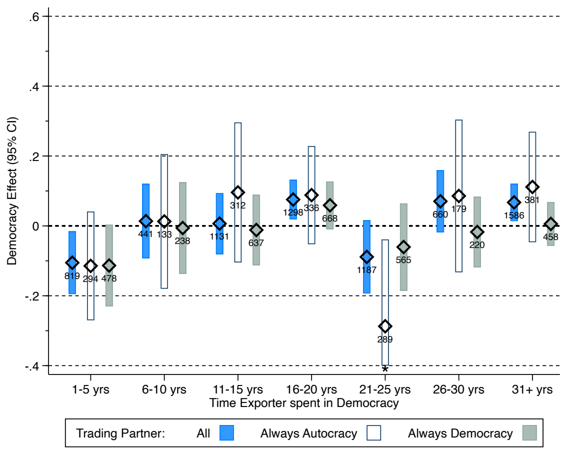
(a) Baseline including FTA and Comcur



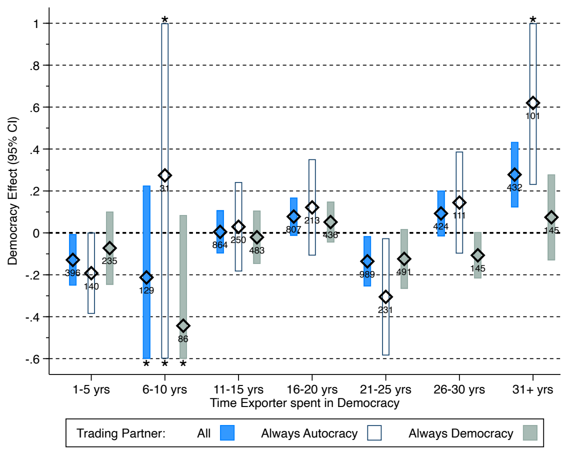
(b) dto, Single Regime Change



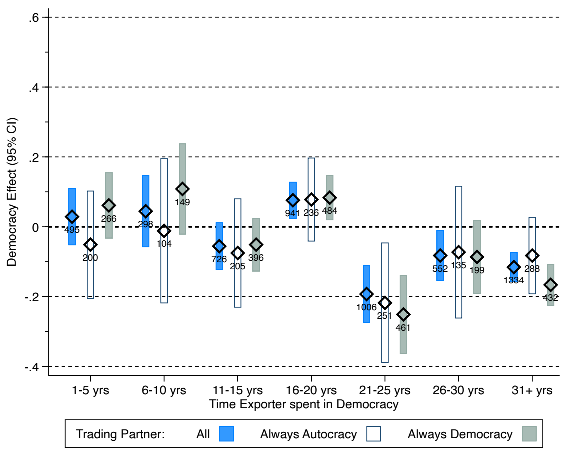
(c) Add Economic Mass Variables



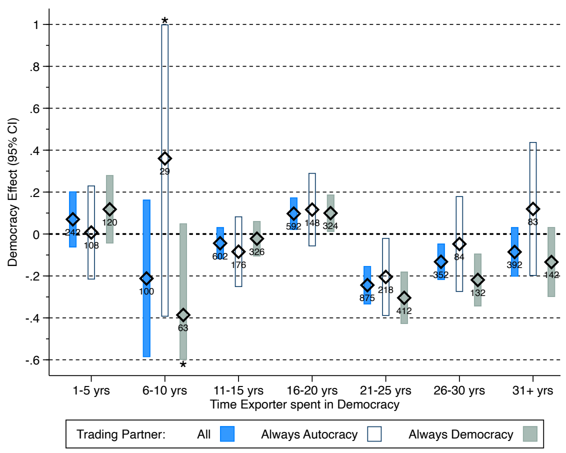
(d) dto, Single Regime Change



(e) Impose Unit Mass Coefficients Instead

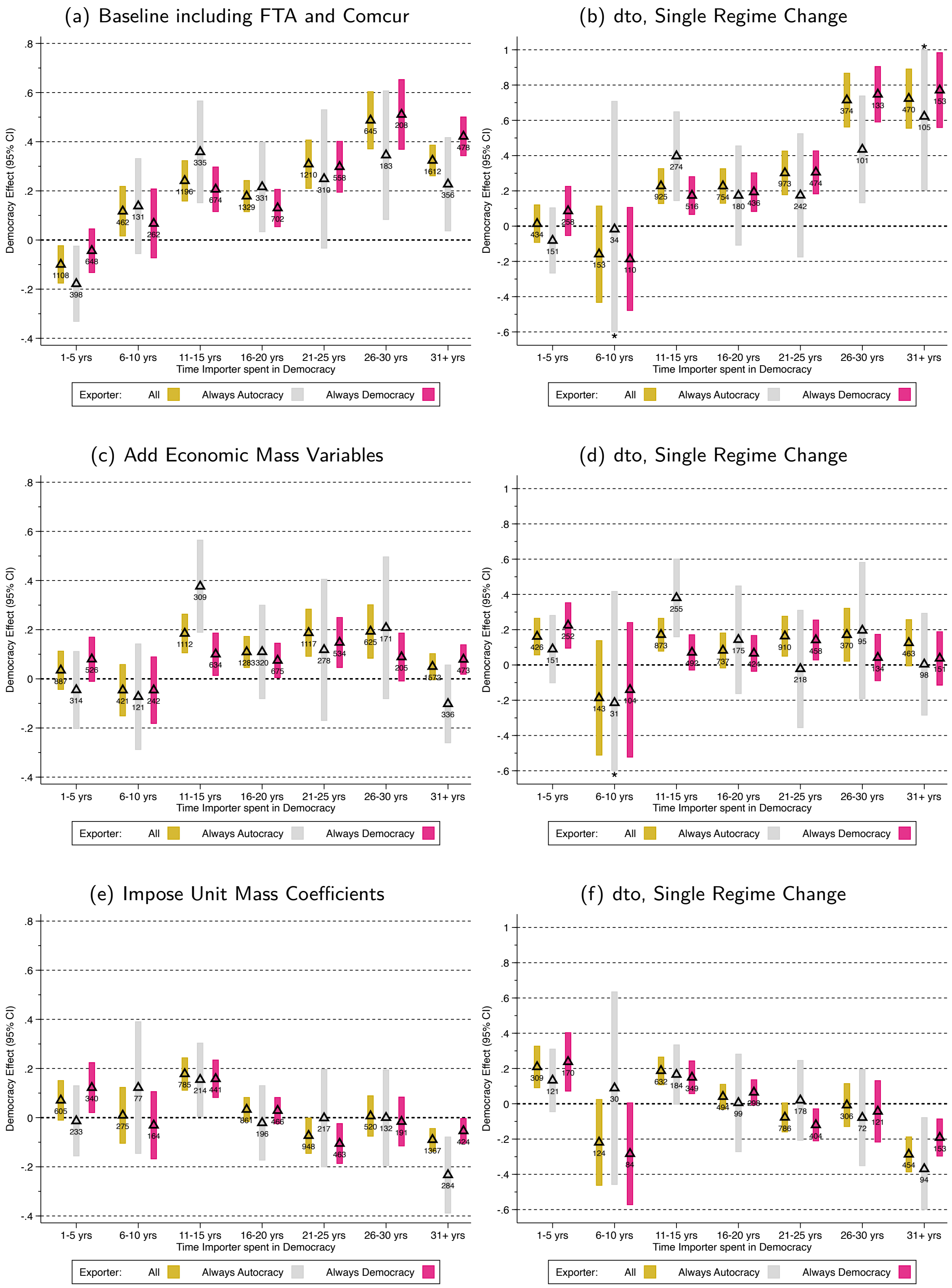


(f) dto, Single Regime Change



Notes: These plots present ATETs (using robust mean estimates) by time spent in democracy using hollow diamond markers. The bars show the 95% confidence interval (* indicates we curtailed these to aid presentation) and we report the number of underlying pairwise estimates. These are not event plots: e.g. exporters with 23 years in democracy only appear in the ATET for 21-25 years. A value of 0.2 suggests that the exporter's switch from autocracy to democracy caused a 22% ($exp(.2) - 1$) increase in exports. Median length in democracy is 20 years. Results are also disaggregated by destination regime status (autocracy, democracy). Panels (a) and (b) are based on the benchmark model including FTA and ComCur dummies — results are virtually identical if we exclude the latter two. In panels (c) and (d) the specifications additionally include exporter and destination per capita GDP (in logs). In panels (e) and (f) we instead impose unit mass on the trade flow variable. If an exporter experienced repeated regime change we sum the years in democracy as 'time spent in Democracy'. To avoid any distortion by this practice, panels (b), (d) and (f) restrict the sample to exporters which shifted from autocracy to democracy once (and did not revert to autocracy).

Figure 2: Destination Democratic Regime Change and Trade Flows — PPML-CCE-DID Results



Notes: These plots present ATETs (using robust mean estimates) by time spent in democracy using hollow triangle markers. The bars show the 95% confidence interval and the underlying number of pairwise estimates is indicated. These plots are the equivalent of those in Figure 1 but from the importer's perspective (destination) and provide insight into the causal effect of democratic regime change on *imports*. Sub-sample results are therefore by exporter regime.

specifications, trade effects are substantially attenuated and frequently insignificantly different from zero. Note that the models in panels (g) and (h) (Mean $+1/4$ SD) fail the Alpha test.

We also adopt the democracy definition of Acemoglu et al. (2019) for 1960-2010, which is based on Polity IV and Freedom House, in addition to detailed case analysis. Results in panels (k) and (l) of Appendix Figure C-2 again share the familiar patterns for the baseline specification and the model with income terms.

Average Trajectory of Treatment Effects All our results relate the treatment effect to the time spent in democracy. The estimates for a country with, say, 23 years in democracy are *only* contained in the robust mean for '21-25 years'. In panel (a) of Appendix Figure C-3 we follow the strategy in Boese-Schlosser & Eberhardt (2024) and add dummies for the first fifteen years in democracy to our treatment regressions and present outlier-robust means for each of these first 15 years in democracy as well as for all years over 15 ('Long Run'). This mimics the 'average trajectory' of an event study analysis, at least for the initial periods, within the confines of our heterogeneous gravity model.

The export effects of regime change rise to over 22% in the first dozen years and then increase further to 35% by year 14. The average country with 16-62 years in democracy enjoys 22% higher exports. These findings suggest that our earlier results of an inverted U-pattern in the first 15 years are not representative of the *trajectory* of the average democratiser, but reflect the characteristics of the countries *with only a few years in democracy* (a sample composition effect). These two results are not contradictory, they simply measure different things. In any case, our estimates are again substantially attenuated in panel (b) where we add the income terms.

Alternative Bilateral Data and Domestic Trade Recent research on the structural gravity model has emphasised the importance of including 'domestic trade' in the analysis (e.g. Yotov 2012, Yotov et al. 2016). However, the availability of data for 'trade with self' (production in goods industries less total exports) was patchy until recently (Head & Mayer 2021, Borchert et al. 2021, 2022). We adopt the data of Head & Mayer (2021) covering 1960-2018[27] for two purposes: first, we replicate our analysis for international bilateral trade in specifications with and without income terms; second, we analyse the effect of democracy on *domestic* trade for a sample of 64 exporters, again in specifications with and without income terms.[28]

Results for the first exercise in Appendix Figure C-5, panels (c) and (d), provide evidence of very strong 'total' effects of democracy on trade (ATET of .7 for over 30 years in democracy, equivalent to 100% higher trade), but once the income terms are added these are substantially attenuated.

[27] 78 exporters experience regime change. The bilateral analysis is based on 547,713 observations for 14,426 trade pairs. The share of countries experiencing regime change with permanent zero trade flows with some partners is negligible (3,683 observations for 93 pairs), so we do not include our CA correction in these specifications.

[28] These regressions only feature a constant, a democracy dummy, and CA terms for exporter control sample GDP and population.

Analysing domestic trade we observe in panel (e) initially minimal effects for a decade and a half, followed by some modest rise over the following decade of treatment (albeit statistically insignificant) before treatment effects turn significant and rise further (note that the increase beyond 30 years in democracy is based on a handful of country estimates). Once we include the income terms in panel (f) the domestic trade effect is U-shaped, negative significant for some of the earlier periods and subsequently converging to zero.

These results demonstrate the robustness of our estimates for bilateral trade flows and show that the findings extend to domestic trade, underscoring the notion of a causal chain running from regime change to growth in income to higher trade.

5 Concluding Remarks

In this paper, we introduced an empirical approach which seeks to satisfy the rigour of the structural gravity model but enables the analysis of binary country-specific variables within a 'heterogeneous' gravity model. This opens up the opportunity to study a wide array of trade determinants which the current literature had to ignore in its adherence to the empirical straitjacket of the pooled gravity model. We employ this new machinery to investigate one of the most pertinent questions in development economics, namely that of the impact of institutional change on economic prosperity: does democracy cause greater trade flows? Our first set of results for our sample from 1950 to 2014 indicate that the average country with three decades of democratic experience can expect high economic benefits in the form of a 46% increase in exports. This effect is driven by increased exports to other democracies, whereas the magnitude of change in exports to autocracies is a lot more varied. However, this finding only captures the end of a causal chain, since our subsequent gravity theory-consistent specifications reveal that democratic regime change does not have a direct causal effect on trade flows: our results suggest that instead, democracy causes higher income, which in turn causes increased trade. Hence, the main effect of democracy on trade is related to economic scale and not to political trade preferences.

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Online Appendix – Not Intended for Publication

A Data, Sample Makeup, and Descriptives

Sample makeup 61 of 84 exporters (73%) had a single regime change from autocracy to democracy (but 13 subsequently reverted to autocracy again), 12 had two, 6 three, and 2 four — 3 countries only reverted from autocracy to democracy. The mean (median) time in democracy is 23 (21.5) years. For details see Appendix Table A-1. The baseline specification (liberal democracy) has 8,693 estimates for regime change. In Appendix Table B-1 panel (a) column (1) the robust mean ATET of 0.125 is computed using an M-estimator which weights each estimate (0 to 1). In our ATET results and graphical presentation in the main text, those estimates with zero weight are not counted/included (neither are the three countries which only reverted to autocracy), our total therefore is 7,420 pairwise estimates for 81 exporters (and up to 164 destinations, though the median is 85).

Table A-1: Sample Makeup: Exporters

	ISO	Exporter	Start	Pairs	Demo	Reversal	Years
1	ALB	Albania	1950	59	1	0	23
2	ARG	Argentina	1950	127	4	3	38
3	ARM♯	Armenia	1992	37	0	1	3
4	BEN	Benin	1959	61	1	0	24
5	BFA	Burkina Faso	1954	74	1	0	16
6	BGR	Bulgaria	1950	103	1	0	25
7	BIH	Bosnia & Herzegovina	1994	52	1	0	18
8	BLR♯	Belarus	1992	109	0	1	5
9	BOL	Bolivia	1950	67	1	0	30
10	BRA	Brazil	1950	130	1	0	29
11	BTN	Bhutan	1980	37	1	0	7
12	CHL	Chile	1950	91	2	2	46
13	CIV	Cote d'Ivoire	1960	137	1	0	13
14	COG	Republic of Congo	1950	61	2	2	2
15	COL	Colombia	1950	98	2	1	35
16	COM	Comores	1980	42	1	0	1
17	CPV	Cabo Verde	1975	26	1	0	24
18	CUB♯	Cuba	1950	33	0	1	1
19	CYP	Cyprus	1960	35	1	0	54
20	DOM	Dominican Republic	1950	93	2	1	22
21	ECU	Ecuador	1950	122	1	1	33
22	ESP	Spain	1950	130	1	0	37
23	FJI	Fiji	1960	81	3	3	32
24	GEO	Georgia	1992	85	1	0	11
25	GHA	Ghana	1950	101	4	3	27
26	GMB	The Gambia	1965	49	1	1	27
27	GRC	Greece	1950	111	2	1	51
28	GTM	Guatemala	1950	99	1	0	16
29	GUY	Guyana	1960	64	1	0	22
30	HKG	Hong Kong	1960	138	1	0	23

(Continued Overleaf)

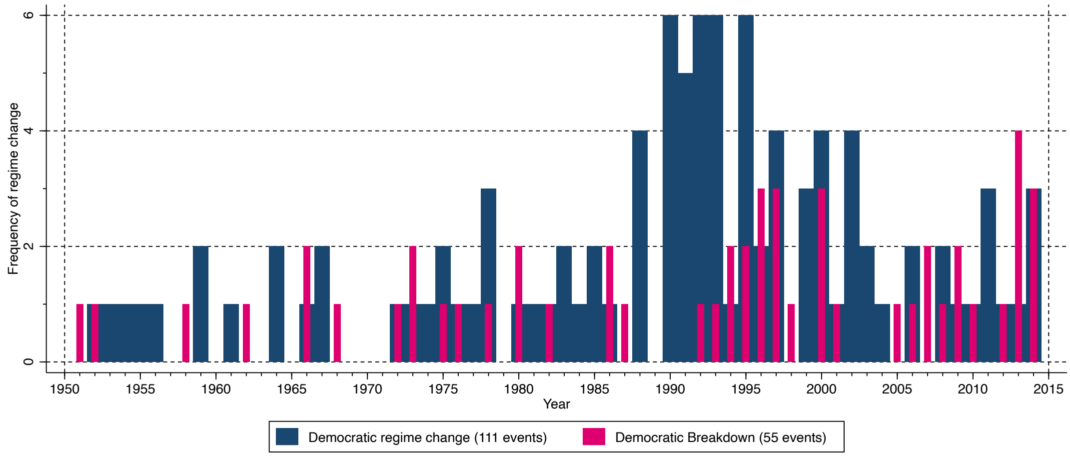
Table A-1: Sample Makeup: Exporters (continued)

	ISO	Exporter	Start	Pairs	Demo	Reversal	Years
31	HND	Honduras	1950	113	1	1	14
32	HRV	Croatia	1995	102	1	0	15
33	HUN	Hungary	1950	120	1	0	25
34	IDN	Indonesia	1950	159	2	1	18
35	IND	India	1950	123	2	1	61
36	JAM	Jamaica	1950	15	1	0	62
37	KEN	Kenya	1955	150	2	1	10
38	KGZ	Kirgistan	1992	66	1	0	4
39	KOR	South Korea	1950	131	1	0	27
40	LBN	Lebanon	1950	148	1	0	4
41	LBR	Liberia	1950	81	1	0	9
42	LBY	Libya	1960	68	1	1	1
43	LKA	Sri Lanka	1950	150	2	3	45
44	LSO	Eswatini	1960	57	2	1	15
45	MDG	Madagascar	1950	122	1	1	3
46	MDV	Maldives	1980	62	1	1	4
47	MEX	Mexico	1950	121	1	0	18
48	MKD	North Macedonia	1993	93	1	1	18
49	MLI	Mali	1960	90	2	1	21
50	MNG	Mongolia	1950	31	1	0	24
51	MOZ	Mozambique	1975	82	1	0	20
52	MWI	Malawi	1954	83	1	0	20
53	NAM	Namibia	1980	21	1	0	25
54	NER	Niger	1960	90	3	2	16
55	NGA	Nigeria	1950	104	1	0	15
56	NIC	Nicaragua	1950	97	1	1	17
57	NPL	Nepal	1952	98	3	2	15
58	PAN	Panama	1950	85	1	0	24
59	PER	Peru	1950	132	3	2	29
60	PHL	Philippines	1950	122	1	0	27
61	PNG	Papua New Guinea	1960	33	1	0	43
62	POL	Poland	1950	128	1	0	25
63	PRT	Portugal	1950	120	1	0	39
64	PRY	Paraguay	1950	74	1	0	23
65	RUS	Russia	1992	132	1	2	2
66	SEN	Senegal	1959	74	1	0	37
67	SGP	Singapore	1950	153	1	0	15
68	SLB	Solomon Islands	1967	39	2	1	35
69	SLE	Sierra Leone	1960	80	1	0	12
70	SLV	El Salvador	1950	86	1	0	16
71	STP	Sao Tome & Principe	1975	17	1	0	16
72	SUR	Suriname	1960	43	1	1	47
73	SYC	Sychelles	1960	66	1	0	12
74	THA	Thailand	1950	162	3	3	18
75	TTO	Trinidad & Tobago	1951	38	1	0	59
76	TUN	Tunisia	1960	134	1	0	3
77	TUR	Turkey	1950	163	3	3	39
78	TWN	Taiwan	1951	151	1	0	14
79	TZA	Tanzania	1955	95	1	0	23
80	UKR	Ukraine	1992	151	1	2	10
81	URY	Uruguay	1950	93	1	1	53
82	VEN	Venezuela	1950	104	1	1	42
83	ZAF	South Africa	1950	130	1	0	20
84	ZMB	Zambia	1950	83	1	0	23
	Total			7,717	111	55	

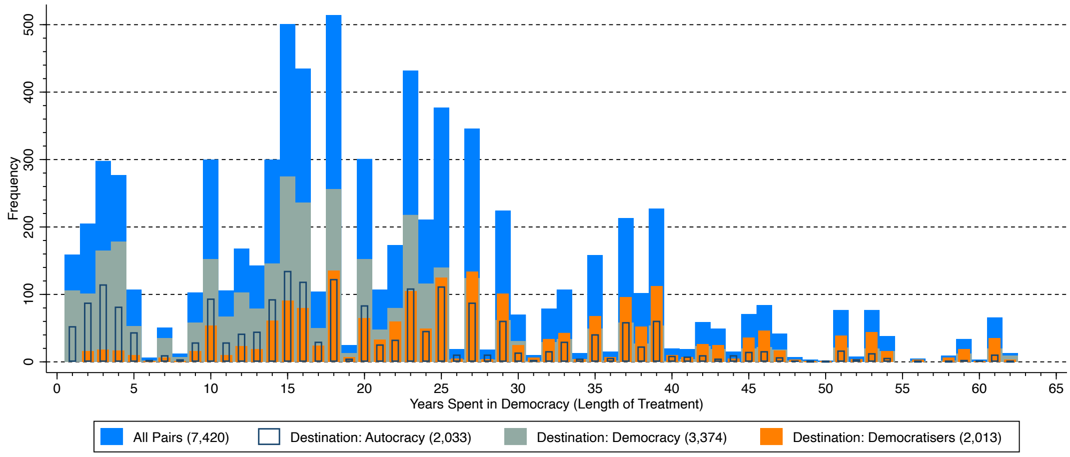
Notes: We present details of the 84 exporters which experienced regime change during 1950-2015 based on our baseline Liberal Democracy definition. 'Start' indicates the year in which the country enters the dataset, 'Pairs' the number of trade partners (with positive trade flows in at least one year), 'Demo' the number of democratic regime changes, 'Reversal' the number of autocratic reversals, and 'Years' the total number of years in democracy. We only report these statistics for countries which did not get assigned a zero weight by our M-estimator. ARM, BLR, and CUB (marked with ♯) are excluded from the results since these only experienced democratic breakdown.

Figure A-1: Democratic Regime Change — Years (countries) and Length of Treatment (dyadic)

(a) Year of Regime Change (Liberal Democracy)



(b) Distribution — Years Spent in Democracy (Liberal Democracy)



Notes: In panel (a) we present the frequency of regime change by year for the 81 (exporter) countries which experienced regime change (we dropped the 3 who only reverted from democracy to autocracy). A total of 111 democratic regime changes and 55 democratic breakdowns occurred during the 1950-2014 sample period. Panel (b) uses the dyadic data for these 81 countries (7,420 pairs) and indicates the frequency of total number of years spent in democracy on the x-axis. We present results using 5-year intervals, and for the full sample of estimates the distribution of 'years spent in democracy' is as follows: 1-5 years – 13.5% of all pairwise treatment estimates, 6-10 years – 6.1%, 11-15 years – 15.8%, 16-20 years – 17.9%, 21-25 years – 16.8%, 25-30 years – 8.8%, and over 30 years – 21.1%.

Descriptives Panel (a) of Appendix Figure A-1 provides the distribution of regime change events (111 democratisations, 55 autocratisations) in the exporter countries of our sample.

Panel (b) indicates the distribution of years spent in democracy (using the dyadic data) for four different samples, defined by the destination: (i) all pairs, (ii) pairs where the importer is autocratic throughout the sample period, (iii) pairs where the importer is a democracy throughout, and (iv) pairs where the importer experiences regime change during the sample period. In this and all results plots by construction the country experiencing regime change is always the exporter i, and on the x-axis we can read off the total number of years the exporter country has spent in democracy. With exception of the democratiser sub-sample,[29] these distributions suggest that the thousands of democracy estimates making up the results we present below are relatively uniformly distributed across these different periods in democracy,[30].

[29] Here the youngest democracies only make up 7% of the sample, 24% of estimates are for countries with 11-20 years of experience, and 35% each for 21-30 and over 30 years, respectively.

[30] Around one-quarter of estimates are for exporters with up to 10 years experience of democracy, 30-35% for those with 11 to 20 years, 24-28% for those with 21 to 30 years and 13-20% for those with more than 30 years in democracy.

B Full Results: ATET Estimates

Table B-1: Average Treatment Effects and Alpha Test Statistics (Main Text)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	All	Auto	Demo	Aut-Dem	All	Auto	Demo	Aut-Dem
Panel A Results for exporters in the main paper, Figure 1								
	(a) Baseline including FTA and Comcur				(b) dto, Single Regime Change			
ATET	0.131***	0.102**	0.115***	0.184***	0.135***	0.119*	0.111***	0.229***
	[0.016]	[0.037]	[0.020]	[0.035]	[0.026]	[0.055]	[0.031]	[0.065]
TE (in %)	13.95***	10.73**	12.21***	20.22***	14.46***	12.65*	11.72***	25.78***
TE pa (in %)	0.739***	0.761**	0.882***	0.384	2.225***	2.428***	2.056***	2.549***
Pairs	7,420	2,033	3,374	2,013	4,238	1,158	2,103	977
Alpha Origin	0.317							
Alpha Destination	0.601							
Alpha Joint	0.529							
	(c) Add Economic Mass Variables (GDPpc)				(d) dto, Single Regime Change			
ATET	0.045*	0.040	0.044	0.065	0.048	0.122	-0.007	0.157
	[0.018]	[0.044]	[0.023]	[0.038]	[0.037]	[0.088]	[0.043]	[0.085]
TE (in %)	4.59*	4.11	4.54	6.67	4.96	13.00	-0.66	16.97
TE pa (in %)	0.143	0.500	-0.073	0.184	1.027*	0.883	0.655	1.767
Pairs	6,657	1,591	3,204	1,862	2,952	681	1,600	671
Alpha Origin	0.276							
Alpha Destination	0.412							
Alpha Joint	0.330							
	(c)' Add Economic Mass Variables (GDP)				(d)' dto, Single Regime Change			
ATET	0.009	0.021	-0.015	0.047	-0.015	-0.004	-0.058*	0.112
	[0.015]	[0.036]	[0.019]	[0.032]	[0.024]	[0.055]	[0.029]	[0.061]
TE (in %)	0.91	2.16	-1.44	4.82	-1.50	-0.42	-5.63*	11.81
TE pa (in %)	0.166	0.206	0.180	-0.138	0.569*	0.939	0.141	0.696
Pairs	7,132	1,932	3,267	1,933	4,066	1,094	2,026	946
Alpha Origin	0.317							
Alpha Destination	0.316							
Alpha Joint	0.367							
	(e) Impose Unit Mass Coefficients Instead				(f) dto, Single Regime Change			
ATET	-0.054***	-0.057	-0.051**	-0.045	-0.064**	-0.025	-0.070**	-0.075
	[0.013]	[0.029]	[0.017]	[0.025]	[0.020]	[0.043]	[0.025]	[0.044]
TE (in %)	-5.21***	-5.51	-4.93**	-4.44	-6.24**	-2.49	-6.77**	-7.27
TE pa (in %)	-0.389***	-0.145	-0.572***	-0.493**	-0.602***	-0.173	-0.753**	-0.834*
Pairs	5,359	1,426	2,394	1,539	3,162	853	1,526	783
Alpha Origin	0.579							
Alpha Destination	0.019							
Alpha Joint	0.000							

(Continued Overleaf)

Table B-1: Average Treatment Effects... (Main Text) cont'd

Sample	(1) All	(2) Auto	(3) Demo	(4) Aut-Dem	(5) All	(6) Auto	(7) Demo	(8) Aut-Dem
Panel B Results for destinations in the main paper, Figure 2								
	(a) Baseline including FTA and Comcur				(b) dto, Single Regime Change			
ATET	0.236***	0.189***	0.225***	0.320***	0.300***	0.238***	0.260***	0.545***
	[0.016]	[0.041]	[0.019]	[0.036]	[0.026]	[0.063]	[0.028]	[0.069]
TE (in %)	26.62***	20.80***	25.22***	37.67***	34.96***	26.93***	29.66***	72.39***
TE pa (in %)	0.821***	0.727*	1.063***	0.248	1.713***	1.433*	1.727***	1.465*
Pairs	7,293	1,965	3,387	1,941	4,090	1,094	2,087	909
Alpha Origin	0.317							
Alpha Destination	0.601							
Alpha Joint	0.529							
	(c) Add Economic Mass Variables (GDPpc)				(d) dto, Single Regime Change			
ATET	0.102***	0.130*	0.070***	0.173***	0.204***	0.381***	0.161***	0.218*
	[0.018]	[0.052]	[0.019]	[0.039]	[0.036]	[0.103]	[0.038]	[0.087]
TE (in %)	10.71***	13.84*	7.21***	18.93***	22.64***	46.39***	17.48***	24.34*
TE pa (in %)	0.611***	1.415**	0.409*	0.530	0.979*	2.597*	0.105	2.463*
Pairs	6,355	1,425	3,135	1,795	2,704	564	1,534	606
Alpha Origin	0.276							
Alpha Destination	0.412							
Alpha Joint	0.330							
	(c)' Add Economic Mass Variables (GDP)				(d)' dto, Single Regime Change			
ATET	0.088***	0.085*	0.075***	0.147***	0.108***	0.147*	0.069**	0.249***
	[0.015]	[0.041]	[0.017]	[0.035]	[0.024]	[0.062]	[0.025]	[0.064]
TE (in %)	9.25***	8.91*	7.81***	15.79***	11.44***	15.89*	7.14**	28.33***
TE pa (in %)	0.035	-0.150	0.121	-0.361	-0.142	-0.399	-0.270	-0.050
Pairs	7,036	1,858	3,297	1,881	3,938	1,034	2,027	877
Alpha Origin	0.317							
Alpha Destination	0.316							
Alpha Joint	0.367							
	(e) Impose Unit Mass Coefficients Instead				(f) dto, Single Regime Change			
ATET	0.004	-0.019	0.011	0.000	-0.003	0.014	-0.001	-0.017
	[0.013]	[0.033]	[0.015]	[0.028]	[0.020]	[0.048]	[0.022]	[0.048]
TE (in %)	0.43	-1.87	1.11	0.01	-0.26	1.41	-0.09	-1.71
TE pa (in %)	-0.443***	-0.700**	-0.376**	-0.480*	-1.133***	-1.440***	-0.902***	-1.397***
Pairs	5,375	1,367	2,496	1,512	3,115	788	1,586	741
Alpha Origin	0.601							
Alpha Destination	0.019							
Alpha Joint	0.000							

Notes: The table presents robust mean estimates (using robust regression) for all specifications and subsamples considered in the main text of the paper: Figures 1 and 2. Columns indicate the destination (in Panel B exporter) regime status, including the sub-sample of 'democratisers' (Aut-Dem). The ATET is the robust mean estimate from each model (standard errors in square brackets), TE translates this into a percentage treatment effect, while TE pa reports the treatment effect per year in democracy (assuming a linear relationship and estimated from a robust regression of the pairwise treatment effect on the years the exporter or importer spent in democracy after regime change) — all indicate statistical significance at the 1, 5 and 10% level using ***, **, and *. The Alpha test is for the null hypothesis that the coefficients on the cross-section averages sum to 1. We compute this for the origin/exporter cross-section averages, the destination cross-section averages, and the joint test, reporting the respective p-value.

Table B-2: Alpha Test Results

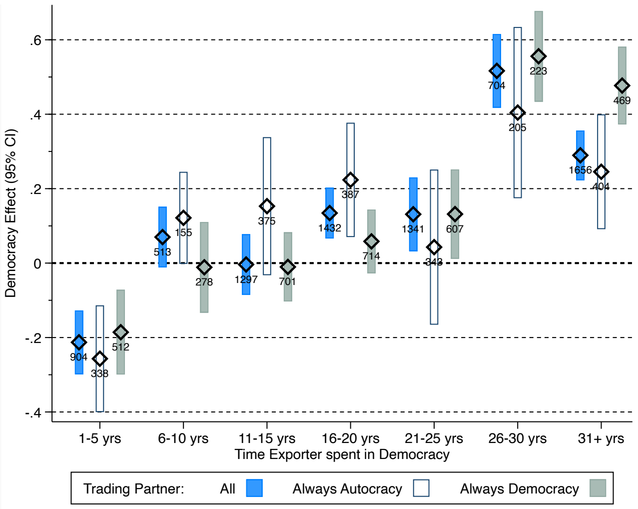
Model	origin	destination	Figure
Vanilla model (without FTA and Comcur)	0.317	0.614	C-4 (a), (b)
Add FTA and Comcur (Baseline Model)	0.317	0.601	1 (a), (b)
Robustness Sample start 1960	0.313	0.330	C-6 (a), (b)
Robustness Reduced CA	0.318	0.339	C-1 (a)
Robustness Extended CA	0.318	0.320	C-1 (c)
Ignore Zero Flow Pairs	0.380	0.348	C-1 (e)
Robustness LD mean - 0.5sd	0.800	0.283	C-2 (c)
Robustness LD mean - 0.25sd	0.360	0.303	C-2 (e)
Robustness LD mean + 0.25sd	0.000	0.000	C-2 (g)
Robustness LD mean + 0.5sd	0.206	0.992	C-2 (i)
Robustness ANRR	0.661	0.163	C-2 (k)
Event-Study Style Implementation	0.317	0.353	C-3 (a)
Extension Head & Mayer data: foreign trade	0.978	0.000	C-6 (c)
Extension Head & Mayer data: domestic trade	0.288	n/a	C-6 (c)
Full model with exp and dest GDP pc as mass	0.419	0.322	1 (c), (d)
Robustness Sample start 1960	0.314	0.331	C-6 (c), (d)
Robustness Reduced CA	0.240	0.416	C-1 (b)
Robustness Extended CA	0.315	0.036	C-1 (d)
Ignore Zero Flow Pairs	0.236	0.248	C-1 (f)
Robustness LD mean - 0.5sd	0.290	0.345	C-2 (d)
Robustness LD mean - 0.25sd	0.167	0.317	C-2 (f)
Robustness LD mean + 0.25sd	0.018	0.016	C-2 (h)
Robustness LD mean + 0.5sd	0.273	0.371	C-2 (j)
Robustness ANRR	0.276	0.412	C-2 (l)
Event-Study Style Implementation	0.318	0.317	C-3 (b)
Alternative Mass Terms (GDP)	0.317	0.316	C-4 (c), (d)
Extension Head & Mayer data: foreign trade	0.208	0.002	C-6 (c)
Extension Head & Mayer data: domestic trade	0.000	n/a	C-6 (c)
Full model with GDP deflation instead	0.579	0.019	1 (e), (f)
Robustness Sample start 1960	0.473	0.315	C-6 (e), (f)
Robustness LD mean - 0.5sd	0.156	0.454	aor
Robustness LD mean - 0.25sd	0.164	0.000	aor
Robustness LD mean + 0.25sd	0.032	0.086	aor
Robustness LD mean + 0.5sd	0.630	0.679	aor
Robustness ANRR	0.276	0.412	aor
Extension Polyarchy	0.170	0.026	aor
Extension Liberal Component	0.810	0.000	aor

Notes: The table presents the p-values for Alpha tests for exporter and destination cross-section averages, indicating the Figure in which the equivalent results are presented. aor – results available on request.

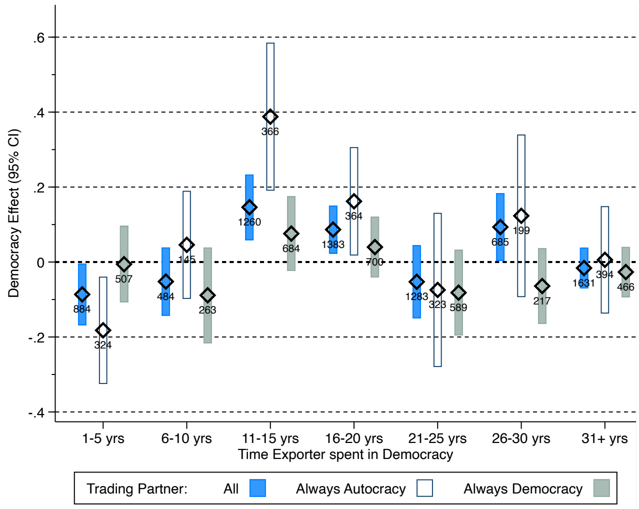
C Results: Robustness Checks and Extensions

Figure C-1: Democratic Regime Change and Trade Flows — Various Specifications

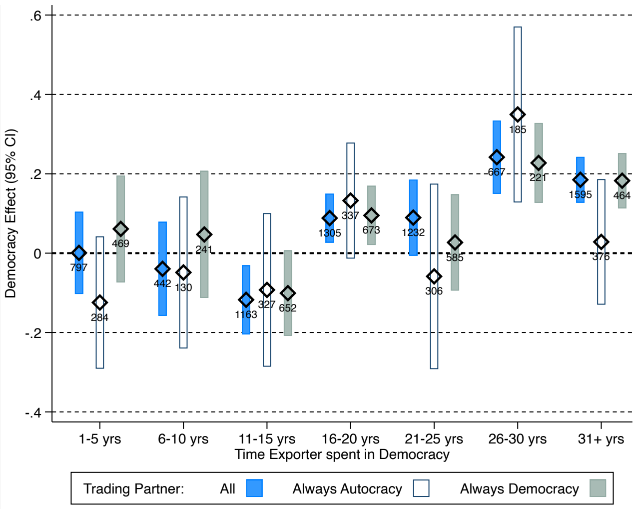
(a) Benchmark: Reduce Set of CA



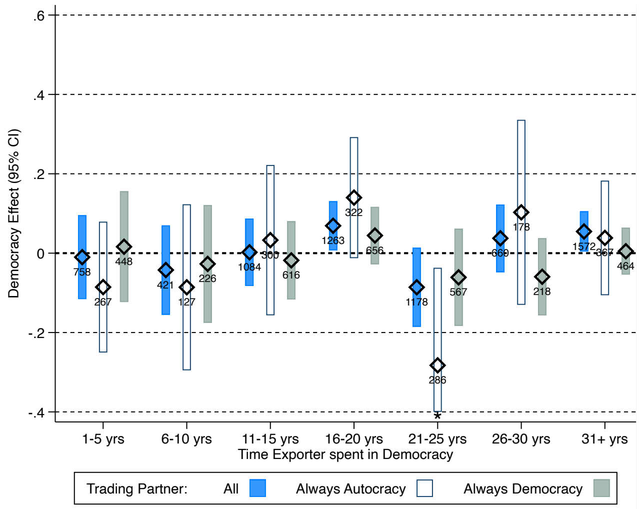
(b) dto, adding Mass terms



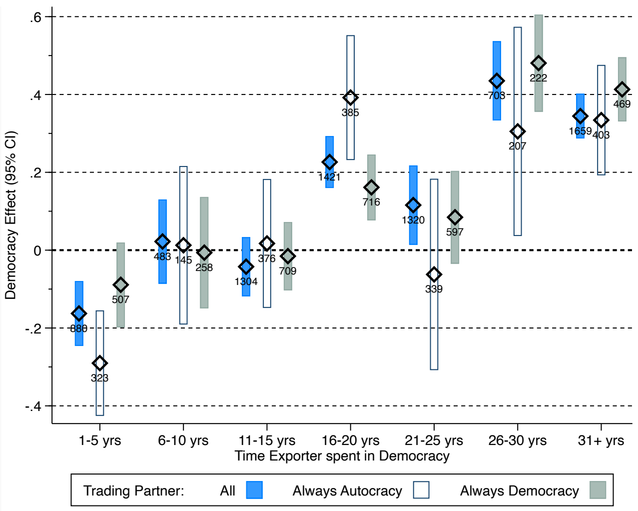
(c) Benchmark: Add Total Exports CA



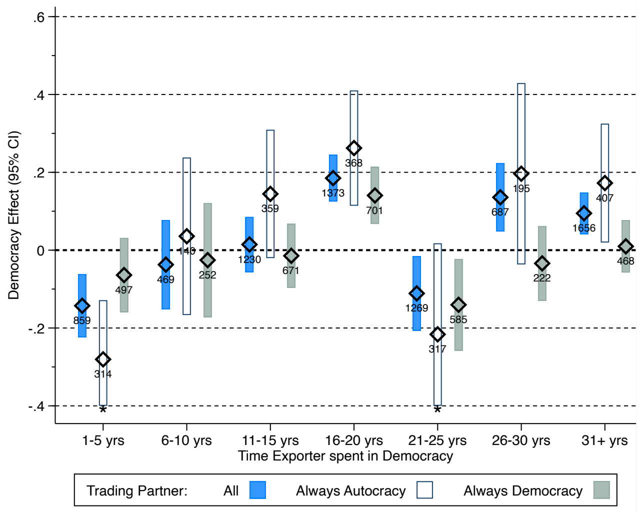
(d) dto, adding Mass terms



(e) Benchmark: Ignore Zero Trade



(f) dto, adding Mass terms



Notes: These plots present robust mean estimates for time spent in democracy (5-year averages). We distinguish trade partners which are always democracies and always autocracies. The bars are the 95% confidence intervals, and the markers are the outlier-robust means. We indicate the number of country pairs included in each estimate.

Figure C-2: Democratic Regime Change and Trade Flows — Alternative Democracy Dummies

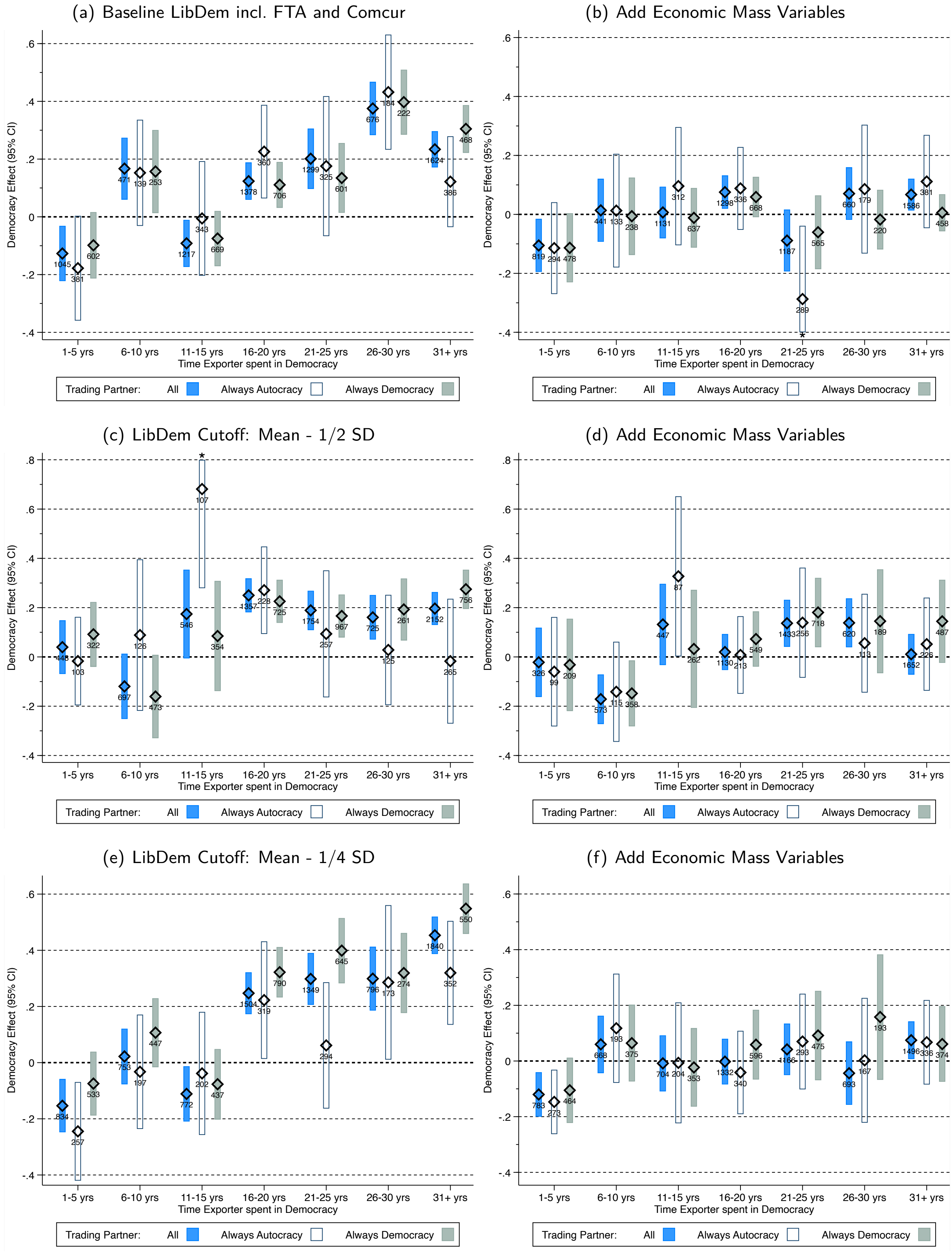


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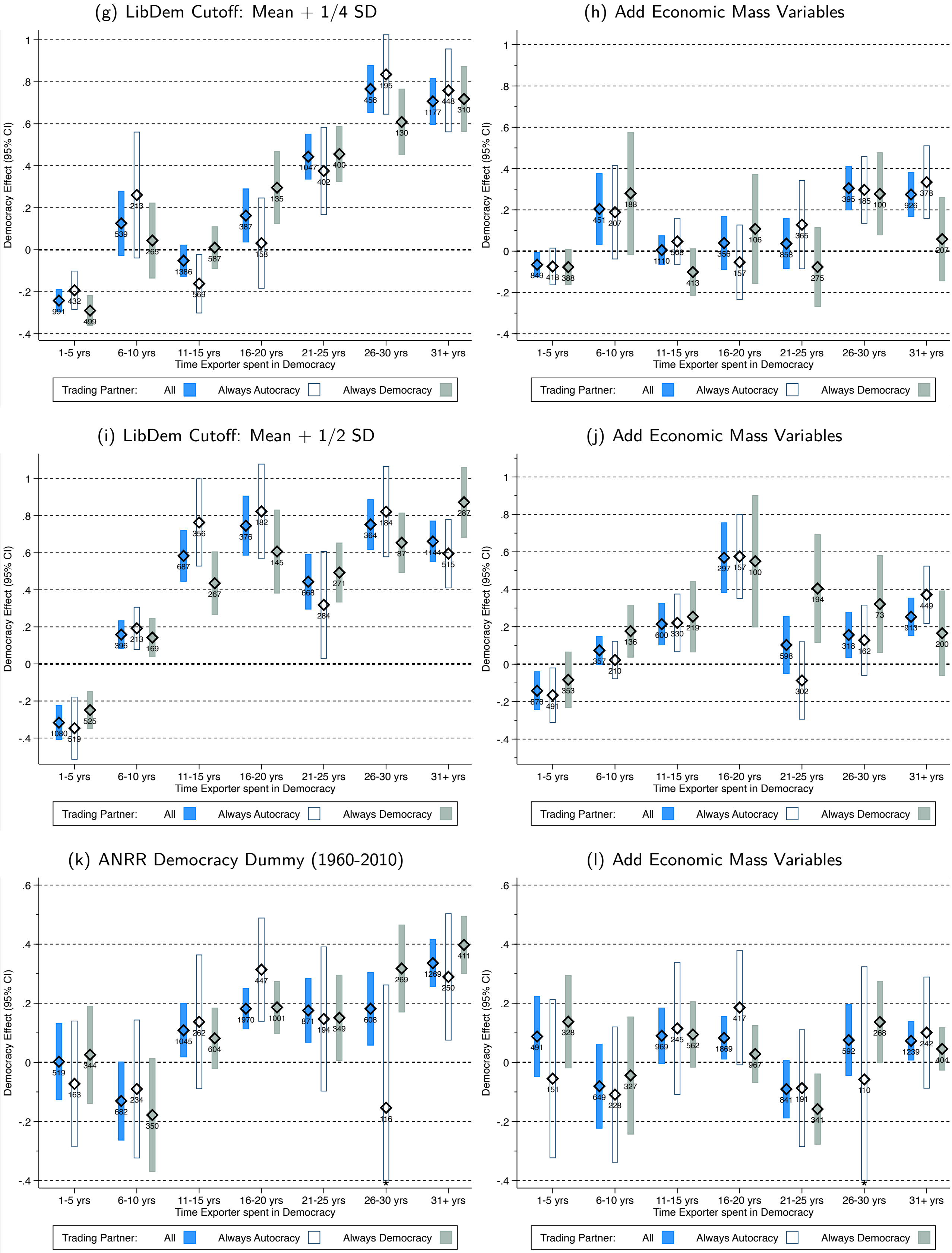
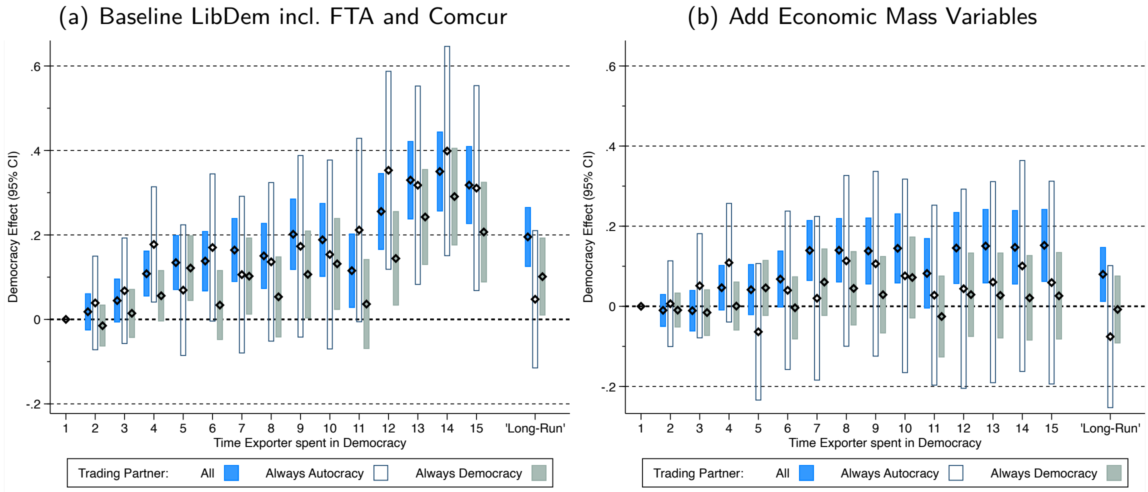
Figure C-2: Democratic Regime Change and Trade Flows — Alternative Democracy Dummies (cont'd)



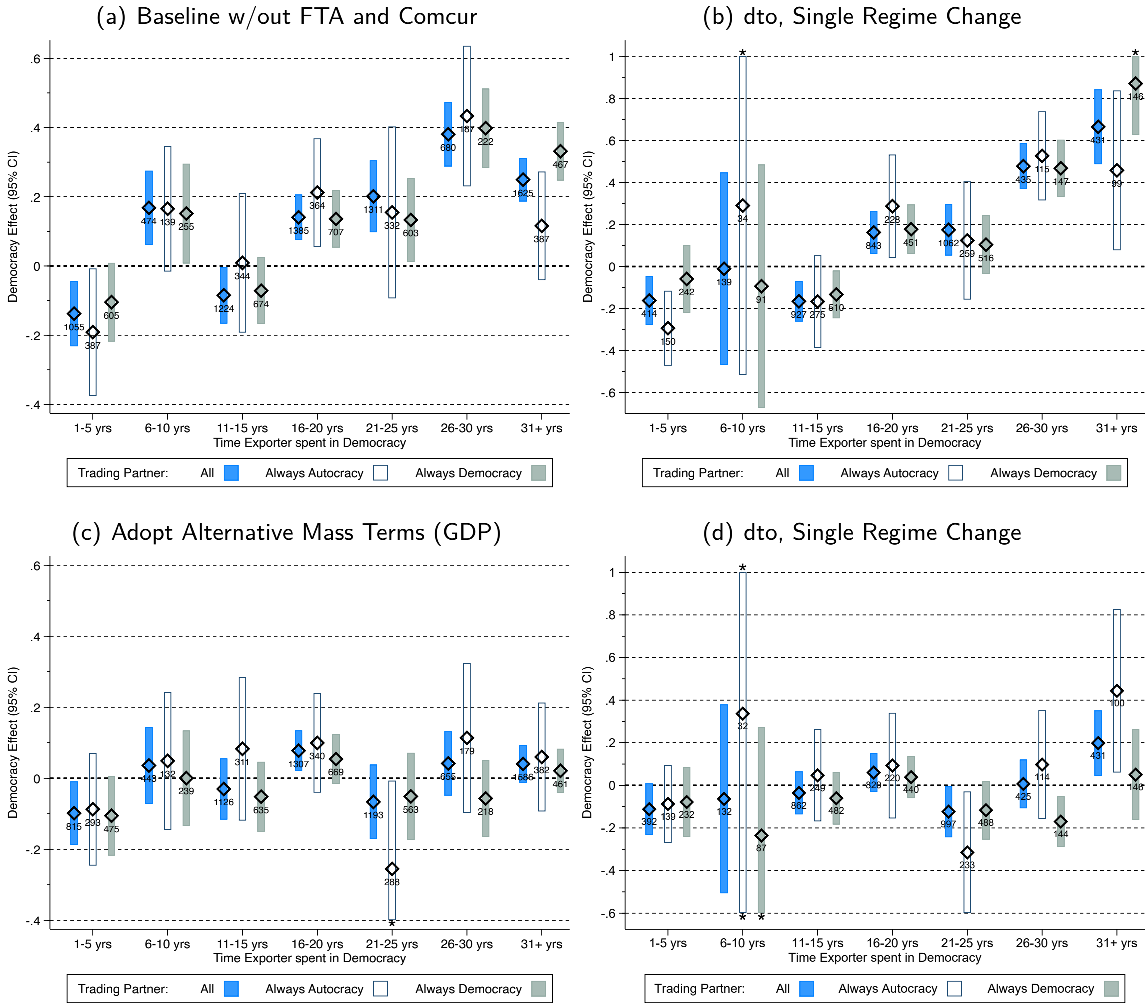
Notes: We use different definitions for our results adopting benchmark Liberal Democracy regime change (mean of the libdem index) — reprinted in panels (a) and (b) for the benchmark and model with mass variables, respectively: four versions, in panels (c) to (j) where we still use the Mean for the V-Dem LibDem index but subtract (add) 1/4 or 1/2 of a standard deviation to establish the democracy cutoff. In panels (k) and (l) we adopt the definition of Acemoglu et al (2019) which covers 1960-2010 (rather than 1950-2014). For all other details see Figure C-1.

Figure C-3: Democratic Regime Change and Trade Flows — Event Study-Style Implementation



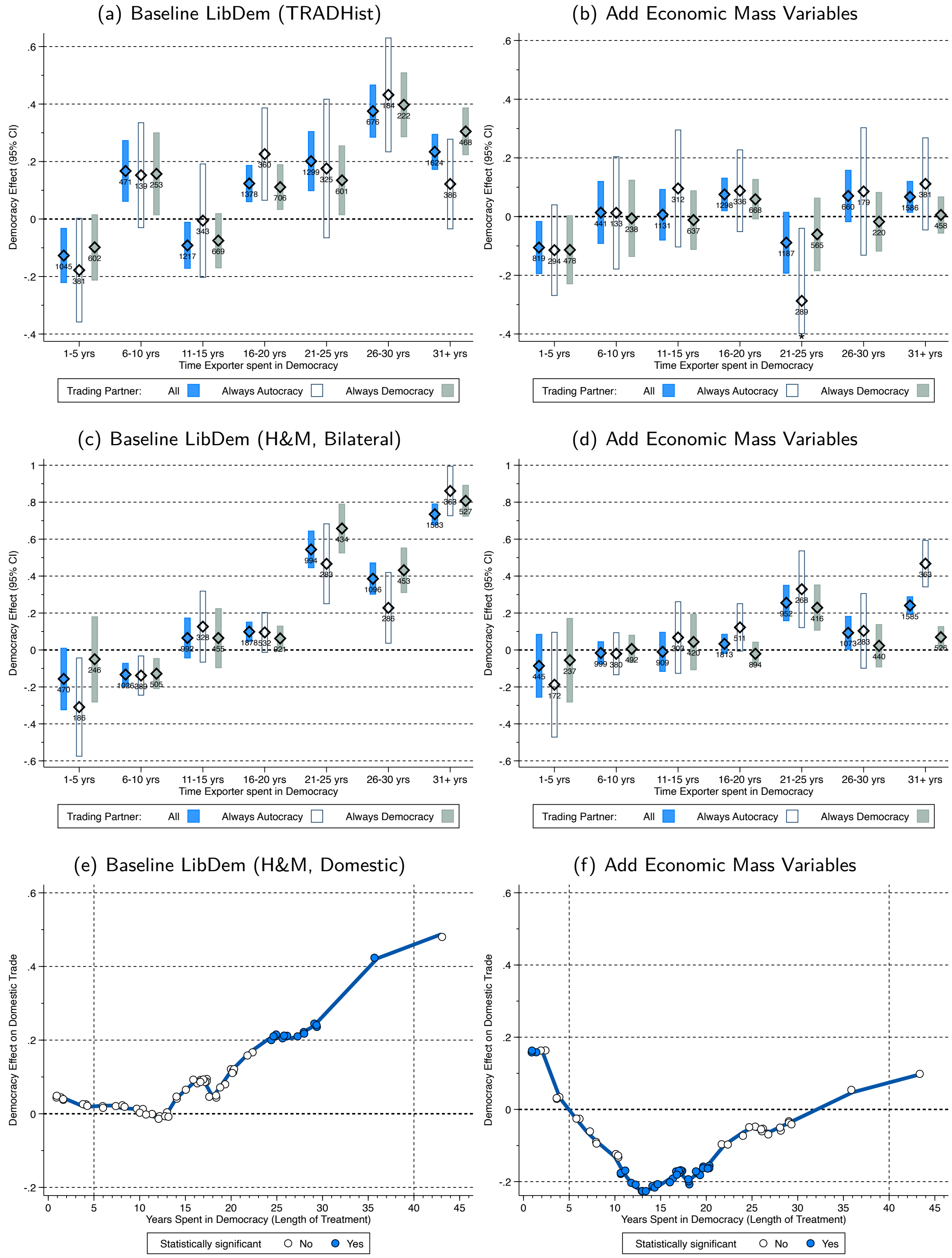
Notes: We present results from PPML-CCE-DID specifications augmented with year of treatment dummies for years 2 to 15. In contrast to the results in the maintext, this mimicks an event study approach within the confines of our empirical framework: the estimates for the early treatment dummies capture the average effect across all exporters which experienced democracy.

Figure C-4: Democratic Regime Change and Trade Flows — More Alternative Specifications



Notes: We present results from PPML-CCE-DID specifications where in panels (a) and (b) we drop the two dyadic controls (FTA, Comcur) and in panels (c) and (d) instead of including importer and exporter GDP per capita (in logs) as additional controls we include respective GDP (in logs) to capture economic mass. Markers indicate the ATET, bars the 95% confidence intervals (* indicates we curtailed these to aid presentation).

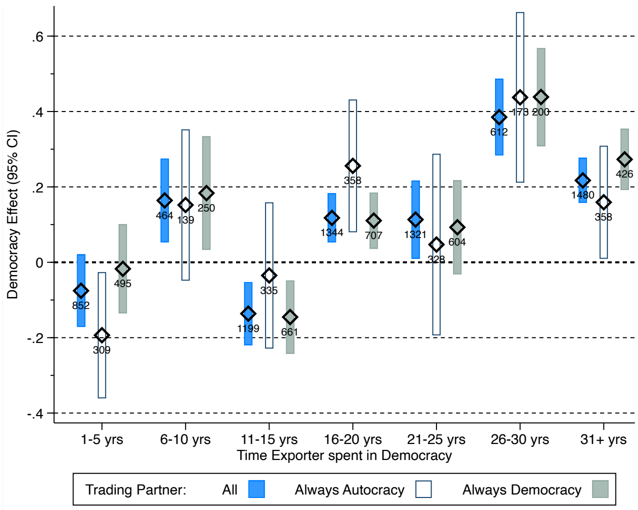
Figure C-5: Democratic Regime Change and Trade Flows — Bilateral/Domestic, 1960-2018 (Head & Mayer)



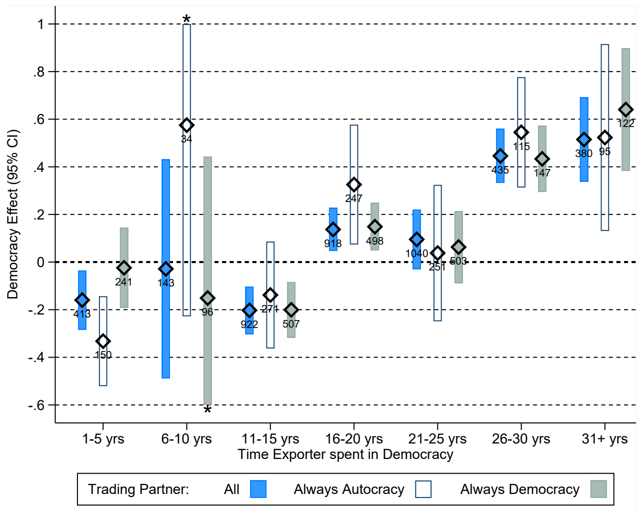
Notes: We present results from PPML-CCE-DID specifications using the data from Head and Mayer (H&M, 2021) which covers 1960-2018 and crucially provides 'domestic trade' figures. In panels (a) and (b) we reprint the results from our analysis (1950-2014) using the TRADHist dataset, with (a) providing the baseline results and (b) the specification including income per capita. Panels (c) and (d) use equivalent specifications for the Head and Mayer (2021) dataset. The models for all four international trade flow regressions include dyadic dummies for FTA and Comcur. In panels (e) and (f) we present results for the domestic trade regressions, where the model only includes the exporter democracy dummy, the CAs from the exporter control sample and, in (f) the exporter's income per capita. The latter two plots are predictions from multivariate running line regressions of the democracy treatment effect estimates and years in democracy, following Boese-Schlosser and Eberhardt (2024), which additionally control for the number of regime switches in a country. For comparison we add our main results using the 1950-2014 bilateral data in panels (a) and (b).

Figure C-6: Democratic Regime Change and Trade Flows — Start Year 1960

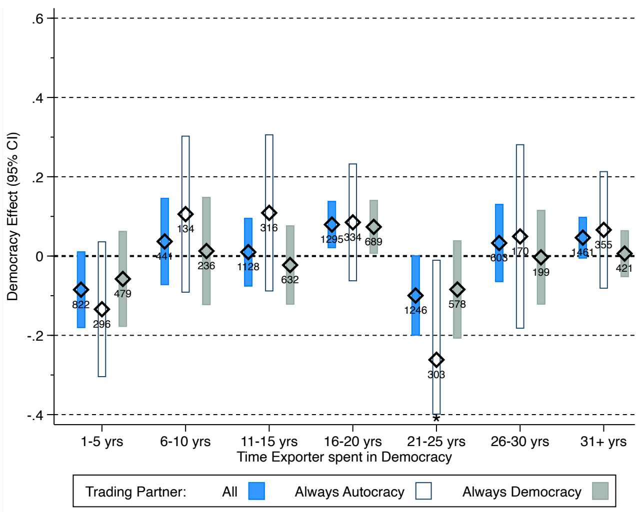
(a) Baseline LibDem incl. FTA and Comcur



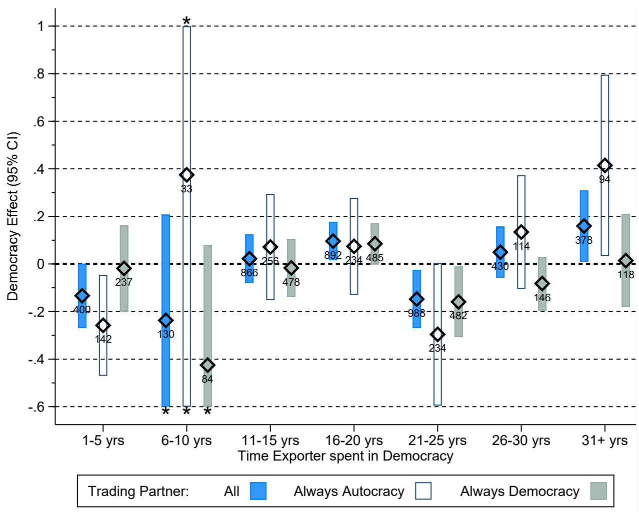
(b) Dto., Single Regime Change



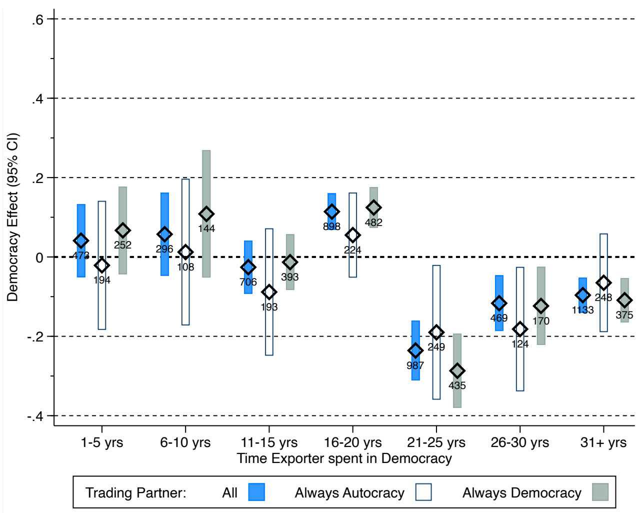
(c) Add Economic Mass Variables



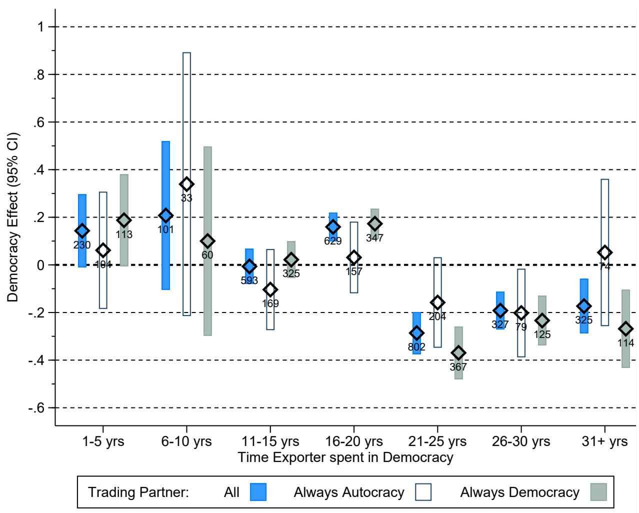
(d) Dto., Single Regime Change



(e) Imposed Unit Mass Coefficients Instead



(f) Dto., Single Regime Change



Notes: We present results from PPML-CCE-DID specifications when the sample is curtailed to start in 1960. For all other details see Figure 1 in the main text. As can be seen in Appendix Table B-2 the specification with unit mass coefficients imposed now passes the Alpha test for exporter *and* destination CAs.

D The PPML-CCE-DID Estimator

In this Appendix Section, we motivate and develop our empirical implementation to capture monadic variables in a factor-augmented heterogeneous gravity model. The first two sub-sections are a recap of the modern structural gravity empirics, before section D.3 introduces heterogeneous gravity. Our combination of the PPML-CCE and recent heterogeneous difference-in-differences estimators is presented thereafter.

D.1 The Gravity Equation

We assume a gravity relationship in the panel in which bilateral trade of exporter i to destination market j at time t is given by[1]

$$\mu_{ijt} = \frac{Y_{it}}{\Omega_{it}} \frac{X_{jt}}{\Phi_{jt}} \phi_{jit} \qquad \text{where} \quad 0 \leq \phi_{ijt} \leq 1. \tag{D-1}$$

μ_{ijt} is a trade flow from an exporter to a destination market, Y_{it} is the value of production for the exporter and X_{jt} the value of expenditure in the destination market j on all source countries — the latter two are typically proxied by GDP in the exporter and destination markets, respectively.[2] ϕ_{ijt} captures the 'bilateral accessibility' for destination j and exporter i: this contains trade costs between the two markets and any variable which may affect these, including time-variant and invariant, observed and unobserved factors.

A major development in gravity modelling over the past two decades following the seminal contribution by Anderson and Van Wincoop (2003) is the recognition that the conditional trade between destination j and exporter i (the conditions being the 'bilateral accessibility') cannot be viewed in isolation from the set of opportunities open to importer j in sourcing goods from exporters *other than* i and the relative access exporter i has to destinations other than j.[3] The multilateral resistance variables for each actor in the exchange of goods are defined in terms of the bilateral accessibility-weighted exporter capabilities and importer characteristics respectively: for exporter $i = 1, \ldots, N$ and for importer $j = 1, \ldots, N$ let

$$\Omega_{it} = \sum_{\ell} \frac{\phi_{i\ell t} X_{\ell t}}{\Phi_{\ell t}} \qquad \Phi_{jt} = \sum_{\ell} \frac{\phi_{\ell jt} Y_{\ell t}}{\Omega_{\ell t}}. \tag{D-2}$$

For our derivation of the empirical gravity model, we assume a stochastic version of equation (D-1)

$$\mu_{ijt} \quad = \quad \frac{Y_{it}}{\Omega_{it}} \frac{X_{jt}}{\Phi_{jt}} \phi_{ijt} \eta_{ijt} \tag{D-3}$$

where η_{ijt} is an error factor with $E[\eta_{ijt}|Y_{it}, \Omega_{it}, X_{jt}, \Phi_{jt}, \phi_{ijt}] = 1$.

A very general empirical equivalent to equation (D-3) allows for flexible unknown parameters on the observable mass and accessibility variables:

$$\begin{aligned} \mu_{ijt} \quad &= \quad exp[\beta^{i}_{ijt}\, ln(Y)_{it} + \beta^{j}_{ijt}\, ln(X)_{jt} + \gamma_{ijt}\, ln(\phi)_{ijt} \\ &\qquad + ln(\Omega)_{it} + ln(\Phi)_{jt}]\, \eta_{ijt}, \end{aligned} \tag{D-4}$$

where superscripts are used to identify the coefficient of exporter versus importer GDP/expenditure. This specification is the most general empirical model possible where all unknown parameters on observable variables (β^i, β^j, γ) vary at the pair level. We demonstrate how this relates to the models employed in the existing literature by adopting parameter restrictions.

[1] This exposition builds on Santos Silva and Tenreyro (2006), Yotov et al. (2016), and Cameron and Trivedi (1998).

[2] If X_{ij} is merchandise trade then theory-consistency dictates Y_i to be gross production of traded goods (not simply value-added/GDP) and X_j the apparent consumption of goods, production plus imports minus exports (Head and Mayer, 2014).

[3] Econometricians study this network of dependencies as a violation of the assumption of weak cross-sectional dependence (Andrews, 2005; Chudik and Pesaran, 2015b).

D.2 Model Restrictions and Pooled Estimation

A first restriction, adopted in the vast majority of studies, is to assume the gravity model estimates are fixed *across time*:

$$\begin{aligned}\mu_{ijt} &= exp[\beta^i_{ij}\, ln(Y)_{it} + \beta^j_{ij}\, ln(X)_{jt} + \gamma_{ij}\, ln(\phi)_{ijt} \\ &\quad + ln(\Omega)_{it} + ln(\Phi)_{jt}]\, \eta_{ijt},\end{aligned} \tag{D-5}$$

Conventionally, further restrictions in the panel gravity literature are to assume common parameters on the observable variables ($\beta^i_{ij} = \beta^i$, $\beta^j_{ij} = \beta^j$, $\gamma_{ij} = \gamma$). Pairwise fixed effects (δ_{ij}) are added to capture trade policy endogeneity (Baier and Bergstrand, 2007). Additionally including exporter-time (ω_{it}) and importer-time (ψ_{jt}) fixed effects can capture the MRTs (Hummels, 2001; Anderson and van Wincoop, 2003; Feenstra, 2004) — with implications for β^i and β^j (see below).

$$\begin{aligned}\mu_{ijt} &= exp[\beta^i\, ln(Y)_{it} + \beta^j\, ln(X)_{jt} + \gamma\, ln(\phi)_{ijt} + \delta_{ij} \\ &\quad + \omega_{it} + \psi_{jt}]\, \eta_{ijt}.\end{aligned} \tag{D-6}$$

Thus δ_{ij}, ϕ_{jt} and ω_{it} are the unknown parameters estimated on the various fixed effects in the reduced-form panel gravity model.

D.3 Heterogeneous Parameter Estimation

A practical difficulty arises if a more flexible specification for the observable variables such as that laid out in equation (D-4) on the one hand (β^i_{ij}, β^j_{ij}, γ_{ij}), and the aforementioned recommended practice to capture MRTs on the other are to be combined: for pairwise heterogeneity in the economic mass and policy variable parameters it is most convenient to estimate equation (D-4) *separately* for each country pair. However, we cannot include fixed effects for *all exporters and all importers* in an equation for a single importer-exporter pair, let alone interacted with time dummies. Existing studies in the literature which allow for trade policy heterogeneity employ interaction effects (e.g. Baier, Bergstrand and Clance, 2018) or maintain a set of fixed effects in a PPML model but use pair-specific trade policy dummies (Baier, Yotov and Zylkin, 2019). An alternative approach is to draw on recent insights from the panel time series literature (e.g. Pesaran, 2006; Bai, 2009) and to employ a multi-factor error structure to capture the unobserved MRTs.

First, we bring the error factor on the inside of the exponential function to capture all unobservables u_{ijt}:

$$\begin{aligned}u_{ijt} &= \delta_{ij} + \omega_{it} + \psi_{jt} + ln(\eta)_{ijt} \\ &\equiv \delta_{ij} + \omega_{it} + \psi_{jt} + \varepsilon_{ijt}.\end{aligned} \tag{D-7}$$

Next, the dimensionality problem of dealing with a large number of unknown parameters (the number of ω_{it} and ϕ_{jt} swiftly add up to thousands of directional-time dummies) in a country-pair equation with up to 65 post-WWII time series observations can be solved by imposing more structure on these unobservables. In the macro panel econometric literature it is widely acknowledged that a small number of common factors with heterogeneous factor loadings can represent large datasets of macro variables. For instance, in the forecasting literature Stock and Watson (2002) have shown that 149 macroeconomic time series can be reduced to half a dozen principal components. Furthermore, Bai (2009) discusses several macro-, microeconometric and finance applications where common factors can be employed to model unobserved time-varying heterogeneity in a tractable way.[4]

[4] The multifactor error structure has been applied to capture country-specific time-varying total factor productivity (Eberhardt

In the case at hand, we posit that the economic mass and accessibility variables along with the trade flows and MRTs are all driven by a small number of common factors with heterogeneous factor loadings across country pairs. Thus, we argue that a small number of unobserved common factors $\boldsymbol{f}_t$, each with country pair-specific factor loadings $\boldsymbol{\varphi}_{ij}$, can account for the evolution of trade flows, GDP, etc. In the above notation:

$$
\begin{aligned}
u_{ijt} &= \delta_{ij} + \omega_{it} + \psi_{jt} + \varepsilon_{ijt} && \text{(D-8)}\\
&\approx \delta_{ij} + \boldsymbol{\varphi}'_{ij}\boldsymbol{f}_t + \varepsilon_{ijt}, && \text{(D-9)}
\end{aligned}
$$

where an 'approximate factor structure' is represented by a set of common factors $\boldsymbol{f}$, and the associated factor loadings φ.

The insight gained in the recent panel time series literature from this setup in the *linear* regression case is that the unobserved common factors can be captured by observables, either via principal component analysis (Bai, 2009) or using cross-section averages of the dependent and independent variables (Pesaran, 2006). We briefly develop the latter approach (the 'common correlated effects' or CCE estimator) and provide a mathematical indication of the intuition at play — this is for ease of illustration since it will only be a small step in terms of implementation from the linear model to a generalised linear one (Boneva and Linton, 2017).

For simplicity we assume a double-index of t for time and i for the cross-section — we can think of the latter as a placeholder for the country pair as the unit of analysis like in the gravity model, i.e. $i \equiv ij$. Let

$$
\begin{aligned}
y_{it} &= \alpha_i + \beta_i x_{it} + u_{it} \qquad u_{it} = \lambda_i f_t + \varepsilon_{it} && \text{(D-10)}\\
x_{it} &= \delta_i + \phi_i f_t + \varrho_i g_t + e_{it}, && \text{(D-11)}
\end{aligned}
$$

where ε and e are white noise processes. This setup indicates that the regressor x is driven by the same common factor f as the dependent variable y, albeit with different parameters.[5] In addition there are some factors $\boldsymbol{g}$ which only drive x but not y. This setup is standard in the macro panel literature and we refer to the studies in footnote 4 for details on factor evolution, parameter distributions, etc. It is clear from equations (D-10) and (D-11) that x is endogenous and that failing to account for the presence of the unobserved common factors will lead to omitted variable bias.[6]

Pesaran's (2006) approach posits that the unobserved common factor f can be captured by the cross-section averages of y and x provided the cross-section dimension of the panel is not too small.[7] A simple algebraic derivation can provide intuition for the mechanism at work: take the cross-section average of equation (D-10) and solve it for the common factor f:

$$
\bar{y}_t = \bar{\alpha} + \bar{\beta}\bar{x}_t + \bar{\lambda} f_t \quad \Leftrightarrow \quad f_t = \bar{\lambda}^{-1}(\bar{y}_t - \bar{\alpha} - \bar{\beta}\bar{x}_t), \qquad \text{(D-14)}
$$

where the bars indicate cross-section averages and the error term disappears since $\bar{\varepsilon} = 0$ by assumption. Next,

and Presbitero, 2015), time-varying absorptive capacity (De Visscher et al, 2020), or knowledge spillovers in the analysis of sector-level production functions augmented with sectoral R&D stock (Eberhardt, Helmers, and Strauss, 2013).

[5] In a setup with multiple factors we can instead assume that only a subset of $\boldsymbol{f}$ 'overlaps' between the two equations.

[6] Solving the x equation for f and plugging this into the y equation yields

$$
\begin{aligned}
y_{it} &= \alpha_i + \beta_i x_{it} + \lambda_i \phi_i^{-1}(x_{it} - \delta_i - \psi_i g_t - e_{it}) + \varepsilon_{it} && \text{(D-12)}\\
&= \alpha_i - \lambda_i \phi^{-1}\delta_i + (\beta_i + \lambda_i \phi_i^{-1})\, x_{it} - \lambda_i \phi_i^{-1}\psi_i g_t - \lambda_i \phi_i^{-1} e_{it} + \varepsilon_{it} && \\
&= \eta_i + \theta_i x_{it} + \nu_{it}, && \text{(D-13)}
\end{aligned}
$$

where in the final line we reparameterise. Crucially, unless $\lambda_i \phi_i^{-1} = 0$ we can see that β_i is unidentified. The asymptotic bias will be a function of the (relative) 'strength' of the factors in their impact on y and x in panel member i.

[7] Additional robustness of this approach to nonstationary factors, structural breaks, and additional spatial dependence, among other aspects, is discussed in Kapetanios, Pesaran, and Yamagata (2011), Chudik, Pesaran and Tosetti (2011), Chudik and Pesaran (2015a) and Westerlund (2019).

plug the expression for f back into the original equation (D-10)

$$\begin{aligned} y_{it} &= \alpha_i + \beta_i x_{it} + \lambda_i \bar{\lambda}^{-1}(\bar{y}_t - \bar{\alpha} - \bar{\beta}\bar{x}_t) + \varepsilon_{it} && \text{(D-15)} \\ &= \alpha_i - \lambda_i \bar{\lambda}^{-1}\bar{\alpha} + \beta_i x_{it} + \lambda_i \bar{\lambda}^{-1}\bar{y}_t - \lambda_i \bar{\lambda}^{-1}\bar{\beta}\bar{x}_t + \varepsilon_{it}, \\ y_{it} &= \varpi_i + \beta_i x_{it} + \zeta_i \bar{y}_t + \vartheta_i \bar{x}_t + \varepsilon_{it}, && \text{(D-16)} \end{aligned}$$

where we reparameterize in the last line. Thus the unobserved common factor f can be captured by the cross-section averages of y and x, while the heterogeneous impact of f across i can be captured by estimating equation (D-16) separately for each panel member — the principle extends to multiple factors. A Mean Group estimator following Pesaran and Smith (1995) captures the central tendency of the panel and provides a convenient comparison with alternative pooled empirical models:

$$\hat{\beta}^{MG} = N^{-1} \sum_{i=1}^{N} \hat{\beta}_i. \tag{D-17}$$

Inference for the Mean Group estimates is based on a simple nonparametric variance estimator (Pesaran and Smith, 1995; Pesaran, 2006):[8]

$$\widehat{\Omega}^{MG} = \frac{1}{N-1} \sum_{i=1}^{N} (\hat{\beta}_i - \hat{\beta}^{MG})(\hat{\beta}_i - \hat{\beta}^{MG})'. \tag{D-18}$$

Boneva and Linton (2017) extend the above setup from the linear model to a setting where the outcome variable is discrete. They show that the CCE approach can be applied to a probit model by including only the cross-section averages of the observed regressors: under the assumption that the unobserved factors are contained in the span of the cross-section averages of the regressors they derive asymptotic results for the large T, large N case as well as the consistency and asymptotic normality of the Mean Group estimator of the individual-specific estimates.

The same principle can be applied to a generalised linear model: in our case, we begin by assuming the exponential mean function incorporating a multi-factor error structure[9]

$$E[y_{it}|x_{it}, \boldsymbol{f}_t] = \mu_{it} = exp[\alpha_i + \beta_i x_{it} + \boldsymbol{\lambda}_i' \boldsymbol{f}_t]. \tag{D-19}$$

The Poisson Maximum Likelihood (PML) estimator assumes that the distribution of y given x (and the factors) is Poisson (i.e. a count variable), but it is widely recognised that the data generating process is not required to be Poisson for this estimator to be consistent (Cameron and Trivedi, 1998; Santos Silva and Tenreyro, 2006), in which case it is referred to as a Poisson Pseudo Maximum Likelihood (PPML) estimator. Our implementation uses the estimator in the single time series, namely of the pair-wise gravity equation with exponential mean function, where the common factors are replaced by cross-section averages of the regressors:

$$\begin{aligned} \mu_{ijt} &= exp[\alpha_{ij} + \beta_{ij}^{i}\, ln(Y)_{it} + \beta_{ij}^{j}\, ln(X)_{jt} + \gamma_{ij}\, ln(\phi)_{ijt} && \text{(D-20)} \\ &\quad + \delta_{ij}^{Y}\, \overline{ln(Y)}_t + \delta_{ij}^{X}\, \overline{ln(X)}_t + \kappa_{ij}\, \overline{ln(\phi)}_t] + \epsilon_{ijt} \qquad \forall\, ij,\ j \neq i, \end{aligned}$$

where the CA terms are constructed from *monadic* data. The accessibility term (any variable that may affect bilateral trade costs) is replaced by dummies for democratic regime change in i and j respectively along with dyadic dummies for free trade agreements and currency unions (in robustness checks we omit the latter two).[10]

[8] In practice we follow the standard in the literature and employ robust means (Hamilton, 1992) to reduce the effect of outliers.

[9] We thank Lena Boneva and Oliver Linton for sharing the rough derivations for a CCE-augmentation in this more general case.

[10] We know that the dependent variable, conditional on the regressors, can be heteroskedastic, and serially correlated and that

In practice, where exporter and destination mass are proxied using their respective GDP (we use Y_{it} and X_{jt} to avoid notational confusion), the cross-section averages are identical, $\overline{ln(Y)}_t = \overline{ln(X)}_t$. In our difference-in-differences implementation these come from distinct control samples and are thus separately identified.

Empirical estimates are based on cross-section average-augmented time series PPML regressions at the country-pair level which are subsequently averaged following the Mean Group principle; inferential statistics for the PPML-CCE Mean Group estimate are computed using the variance estimator in (D-18) (Boneva & Linton 2017).

Estimation of the PPML-CCE model can be seen as an improvement on current practice for two reasons. First, the presence of common factors, with heterogeneous loadings across country pairs, offers a flexible way to account not only for the MRTs, but also so-called globalisation effects, and other forms of spatial dependence. Second, using common factors as proxies for spatial dependence allows the estimation of country-specific (monadic) variables — thus in addition to dyadic determinants such as those contained in $ln(\phi)_{ijt}$, we can also introduce country-specific variables of interest.

D.4 A Difference-in-Differences Gravity Model

Our exposition so far has focused on heterogeneous gravity without giving too much thought to the empirical practices in the democracy-growth literature, where economists favour using *binary* indicators for democracy versus autocracy (Acemoglu et al, 2019; Eberhardt, 2022; Boese-Schlosser and Eberhardt, 2024). We now introduce a combination of the PPML-CCE model with a factor-augmented difference-in-differences estimator following Chan and Kwok (2022): let D_{it} be a dummy for democratic regime change equal to 0 when country i is in autocracy and 1 if they are in democracy — we refer to the shift from 0 to 1 as 'treatment'. The Chan and Kwok (2022) principal component DID (PCDID) estimator estimates the causal effect of a treatment in country-specific regressions of treated countries only. While this appears to capture only the first of the difference-in-differences, the comparison with the control sample (in our case those countries which never experienced democratic regime change) is achieved by the inclusion of unobserved common factor proxies extracted from the control sample of never-democratisers. Crucially, this heterogeneous difference-in-differences estimator does not require the 'parallel trends' assumption to hold, but relies on a much weaker assumption of 'weak parallel trends' between treated and control samples.

In the context of democratic regime change and the PPML-CCE gravity model of trade flows, we make some adjustments to the way the Chan and Kwok (2022) PCDID is implemented: first, we do not *estimate* the unobserved common factors from auxiliary regressions in the control sample of never-democratisers. Extracting several principal components via PCA from the residuals of a PPML regression is conceptually difficult (given that these are not linear models) and in practice raises concerns over how many estimated factors to include. Instead, we use cross-section averages[11] of GDP and population in line with Boneva and Linton (2017). Second, these cross-section averages are constructed from the respective control samples of (i) exporters and (ii) destinations: since the trade flow panel is not symmetric (i may be exporting to j but j does not export to i) these two cross-section averages are not identical.

In this exposition we do not consider the economic and econometric implications of including (or excluding) income terms (GDP or per capita GDP) in the gravity model, which is discussed in the main paper.

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the PPML estimator maintains its consistency in the presence of serial correlation if the exponential conditional mean is correctly specified (Cameron and Trivedi, 1998: 226) — the latter is the requirement for any PML or PPML estimator.

[11] Eberhardt (2022) adopts the same strategy in a standard (monadic) panel dataset to study democracy and growth.

Our PPML-CCE-DID estimator is then implemented as follows:

$$\mu_{ijt} = exp[\alpha_{ij} + \gamma^1_{ij}\,\text{FTA}_{ijt} + \gamma^2_{ij}\,\text{Common Currency}_{ijt} + \theta_{ij}D_{it} + \eta_{ij}D_{jt} + \delta^i_{ij}\,\overline{ln(Y)}^i_t + \kappa^i_{ij}\,\overline{ln(Pop)}^i_t + \delta^j_{ij}\,\overline{ln(X)}^j_t + \kappa^j_{ij}\,\overline{ln(Pop)}^j_t + \epsilon_{ijt}] \qquad \forall\, ij,\ j \neq i. \tag{D-21}$$

We have pair fixed effect (α_{ij}) and the familiar dyadic variables for FTA and common currency union. The two democracy dummies (D_{it} for exporter i and D_{jt} for destination j) are accompanied by two sets of cross-section averages, which are constructed from the exporter i and destination j control samples of countries which never democratised, respectively. We do not include cross-section averages of the indicator variables in the first line of equation (D-21) since this practice is likely to induce severe bias in the estimation equation (Juodis et al, 2021). Our coefficient of interest is θ_{ij}, which is estimated (separately) in each trade flow equation of country i with each of its trade partners j — as is standard in the heterogeneous DID literature, we estimate equation (D-21) only for equations where either country i or country j (or both) experienced regime change.

Finally, we cannot estimate the democracy effect in country-pairs where trade flows remained zero throughout the sample period, whether they experienced regime change or not: this is necessitated by our heterogenous regression implementation. We do however include the GDP and population variables for these country pairs in the computation of the cross-section averages. More worryingly, excluding country pairs where the exporter or importer did experience regime change but nevertheless, trade between the pair never materialised, will likely induce an upward (selection) bias in our results. We therefore adjust the model to include cross-section averages from those country pairs where the exporter or importer country did experience regime change but where trade flows remained zero. Our final empirical implementation is thus:

$$\mu_{ijt} = exp[\alpha_{ij} + \gamma^1_{ij}\,\text{FTA}_{ijt} + \gamma^2_{ij}\,\text{Common Currency}_{ijt} + \theta_{ij}D_{it} + \eta_{ij}D_{jt} + \delta^i_{ij}\,\overline{ln(Y)}^i_t + \kappa^i_{ij}\,\overline{ln(Pop)}^i_t + \delta^j_{ij}\,\overline{ln(X)}^j_t + \kappa^j_{ij}\,\overline{ln(Pop)}^j_t + \delta^{i0}_{ij}\,\overline{ln(Y)}^{i0}_t + \kappa^{i0}_{ij}\,\overline{ln(Pop)}^{i0}_t + \delta^{j0}_{ij}\,\overline{ln(X)}^{j0}_t + \kappa^{j0}_{ij}\,\overline{ln(Pop)}^{j0}_t + \epsilon_{ijt}] \qquad \forall\, ij,\ j \neq i, \tag{D-22}$$

where the superscript $i0$ ($j0$) indicates the cross-section averages or associated parameters from exporter (destination) countries which did experience regime change but where trade flows between i and j remained zero throughout the sample period. These are constructed from the *dyadic* data, in contrast to the CA terms in the second line of equation (D-22), since we want to give more prominence/weight to treated countries which had many 'always-zero' trade partnerships.

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